Price (to Non-Fellows), 15/-

ROYAL HORTICULTURAL SOCIETY.

FOUNDED A.D. 1804. INCORPORATED A.D. 1809.

VINCENT SQUARE, LONDON, S.W.

REPORT

OF THE

THIRD INTERNATIONAL CONFERENCE 1906

ON

GENETICS;

HYBRIDISATION
(THE CROSS-BREEDING OF GENERA OR SPECIES),
THE CROSS-BREEDING OF VARIETIES,
AND GENERAL PLANT-BREEDING.

EDITED BY THE

REV. W. WILKS, M.A.

SECRETARY.

LONDON:
Printed for the Royal Horticultural Society
BY
SPOTTISWOODE & CO. LTD., NEW-STREET SQUARE, E.C.

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1906

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PREFATORY NOTE.

To apologise for the delay in the issue of this Report would be useless. It has been carried through with all the speed possible compatible with accuracy of workmanship and the many other necessary calls on the time, not only of the Editor, but of each one of the many contributors. The Editor has no fear of being judged harshly by anyone who knows anything at all of the labour involved in the passing of such very highly technical and scientific matter through the Press.

The Editor wishes to acknowledge the always courtesy, if not always promptness, of his many correspondents and contributors. His thanks are also due to Mr. Bateson, F.R.S., V.M.H., to whom much of the excellence and none of the faults of the present issue are due. But above all he wishes to express his vast indebtedness to the assistance rendered him by Mr. R. H. Hutchinson, the Society's Librarian, without whose careful reading of the sheets every error now discoverable would have been multiplied ten times.

W. W.

Vincent Square, London.
February 21, 1907.

Gregor Johann Mendel.

Born 1822, Died 1884.

From a photograph taken in 1862.

Royal Horticultural Society.

ESTABLISHED 1804. INCORPORATED 1809.

COUNCIL AND OFFICERS, 1906.

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HIS MOST GRACIOUS MAJESTY THE KING.
HER MOST GRACIOUS MAJESTY QUEEN ALEXANDRA.

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Thos. E. Sedgwick.

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Royal Horticultural Society.

REPORT OF THE CONFERENCE ON GENETICS AND ALLIED SCIENCES.

The Council of the Royal Horticultural Society having decided in the winter of 1904 to hold a further International Conference in 1906 on Genetics in continuation of the two previously held, in London and at New York, a preliminary circular was issued in April 1905 inviting co-operation. This was followed in February 1906 by a letter of definite invitation together with an outline of the programme as far as it had then been settled. In May 1906 a final and complete programme was sent out. These and other official and formal documents are reproduced here (even such trivial ones as the menus) as being of some historical interest and also because they may prove suggestive and possibly helpful for future similar meetings. A notice was also sent out to British amateurs and nurserymen drawing attention to the Conference and requesting them to exhibit hybrid or cross-bred plants (whether in bloom or not) with their parents.

* * *

ROYAL HORTICULTURAL SOCIETY,

VINCENT SQUARE, WESTMINSTER,

LONDRES.

Février, 1906.

Monsieur,

Deux Conférences Internationales sur **L'HYBRIDATION des PLANTES** ont été déjà tenues, dont l'une à Londres en 1899, sous les auspices de la Royal Horticultural Society, et l'autre aux Etats-Unis en 1902, sur l'invitation de la Horticultural Society de New-York.

En vue du progrès considérable accompli récemment dans cette science, la Royal Horticultural Society de Grande-Bretagne se propose de tenir la troisième Conférence Internationale à Londres, ayant trait aux mêmes questions, commençant Lundi le 30 Juillet 1906. Il est sûr qu'une telle Conférence offre aux personnes scientifiques et pratiques des occasions exceptionnelles pour l'échange des méthodes et des idées, et il est à espérer que les représentants des deux classes d'investigateurs répondront à cette invitation de participer à la Conférence. Le Président et le Conseil de la Royal Horticultural Society me chargent donc de vous inviter de venir personnellement, ou, au cas qu'il vous serait impossible de venir, d'envoyer une communication à lire à la Conférence. Je vous serais aussi obligé, si vous vouliez bien me donner les noms et les adresses des personnes que vous connaissez de s'intéresser à ces sujets, et qui voudraient recevoir une invitation.

Vous m'obligerez beaucoup en répondant à cette invitation par un prochain courrier et en me faisant savoir en même temps le sujet que vous auriez choisi pour la communication que vous avez l'intention de soumettre à la Conférence.

Les billets d'invitation définitifs seront envoyés dès qu'on saura le nombre de personnes qui pourraient venir en personne.

Le programme provisoire est le suivant :

Lundi, le 30 Juillet.

9 h. du soir. Réception dans la Grande Salle de la Société, Vincent Square, Westminster.

9.30. Adresse de bienvenue par le Président de a Société, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., etc.

10. Démonstration à la lanterne magique de différentes hybrides. On serait très obligé pour le prêt de toutes plaques intéressantes.

Rafraîchissements seront servis pendant la soirée.

Mardi, le 31 Juillet.

10.30. du matin. Discours de réception par M. W. Bateson, F.R.S., V.M.H. La première séance de la Conférence.

1.15. Déjeuner.

2.30 à 5. La seconde séance de la Conférence.

6.30. Dîner à l'Hôtel Windsor, sur l'aimable invitation du Horticultural Club.

Mercredi, le 1er Août.

10.30 du matin à 12.45. La troisième séance de la Conférence.

1.30. Déjeuner à Burford, sur l'aimable invitation du Président de la Société, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H.

Jeudi, le 2 Août.

10.30 du matin à 1h. La quatrième séance de la Conférence.

1.15. Déjeuner.

2.30 à 5. La cinquième séance de la Conferénce.

7. Le Banquet dans la Grande Salle de la Société.

Vendredi, le 3 Août.

10.30 à 11.30. Visite au Musée de l'Histoire Naturelle.

12. Visite aux Jardins à Gunnersbury.

1.30. Déjeuner à Gunnersbury, sur l'aimable invitation de M. Léopold de Rothschild.

3 à 5. Visite aux Jardins de Kew.

En renouvelant ma prière de répondre le plus tôt possible à cette invitation préliminaire, je vous prie, Monsieur, de vouloir bien agréer mes salutations empressées.

W. WILKS,
Secrétaire de la Royal Horticultural Society.

* * *   * * *   * * *

ROYAL HORTICULTURAL SOCIETY,

Vincent Square, Westminster, S.W.

ELEGRAMS: "HORTENSIA, LONDON."
ELEPHONE: No. 5363 WESTMINSTER.
SECRETARY, REV. W. WILKS, M.A.

May, 1906.

INTERNATIONAL CONFERENCE

ON

HYBRIDISATION AND PLANT BREEDING.

July 30th to August 3rd, 1906.

MY DEAR SIR,

I have now the pleasure to forward you a further programme, though it cannot even yet be considered complete.

In order that all the Papers and other Communications to be laid before the Conference may be more widely appreciated, we propose to have them all previously translated into English. If, therefore, you intend to use any other language, may I ask you to be so kind as to forward your Communications to me *at your earliest convenience*, in order that the translations may be ready before the Conference opens.

It will probably be necessary to limit the delivery of each address to fifteen or twenty minutes, but the written Communications handed in to be printed in the "Report of the Conference," need not be so limited, but may be given *in extenso*.

The Lecture Room in which the Sessions of the Conference will be held is fitted with an admirable Lantern for the Exhibition of illustrations.

As a Guest of the Society a book of Tickets has been prepared for you, admitting you to the several meetings, &c. May I ask you to be so good as to give me an exact address to which I may send it by post on or about July 1, so that it may be sure to reach you safely, as from the number attending the Conference it is absolutely necessary that everyone should be provided with his own tickets. *Additional* Tickets for the

Conversazione and for the Banquet can be obtained from my Office—for the Conversazione, 2*s.* 6*d.*; for the Banquet, Ladies £1 1*s.*, Gentlemen, 25*s.*

Faithfully yours,
W. Wilks, *Secretary.*

The programme is as follows:—

Monday, July 30.

9 P.M. to 10.30. Conversazione in the Society's Great Hall.

9.30. Address of Welcome by the President of the Society, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., &c.

Exhibits of Japanese Waltzing Mice crossed with albinos, shown by A. D. Darbishire, Esq., of the Royal College of Science, London, and of various other Hybrids.

The Band of the Royal Artillery will play in the Musicians' Gallery.

Refreshments will be served during the evening.

Tuesday, July 31.

[*Nota Bene.—Only the holders of the special Conference tickets can be admitted to the Sessions of the Conference as the space is limited and the seats already engaged.*]

10.30 to 1. First Session of the Conference.

Opening address by W. Bateson, Esq., F.R.S., V.M.H., President of the Conference.

Professor W. Johannsen, of Copenhagen, on "Does Hybridisation increase fluctuating Variability?"

C. C. Hurst, Esq., of Hinckley, England, on "Mendelian Characters in Plants and Animals."

A. D. Darbishire, Esq., of the Royal College of Science, London, on "Recent advances in animal breeding and their bearing on our knowledge of Heredity."

Professor Davenport, Station for Experimental Evolution, Cold Spring Harbour, Long Island, U.S.A., on "Dominance of Characteristics in Poultry."

G. Udny Yule, Esq., of University College, London, on "The theory of inheritance of quantitative compound characters on the basis of Mendel's laws."

1.15. Light Refreshments.

2.30. Second Session of the Conference.

Monsieur E. G. Camus, Lauréat de l'Institut de France, on "A Contribution to the Study of Spontaneous Hybrids in the European Flora."

Monsieur E. Malinvaud, President of the Société Botanique of France, on "Phenomena of Hybridisation in the Genus *Mentha*."

John H. Wilson, Esq., D.Sc., F.R.S.E., of the University of St. Andrews, Scotland, on "Infertile Hybrids," with Lantern Illustrations.

R. A. Rolfe, Esq., of Kew, England, on "Natural Hybrids of the *Cattleya* Group."

Herr C. L. W. Noorduijn, of Groningen, Holland, on "The Hereditary Transmission of Colour in Cross-Breeding."

F. J. Chittenden, Esq., of the Biological Laboratory, Chelmsford, England, on "The Influence of the Parents on the Colour of the Hybrid."

Professor E. Pfitzer, of the University of Heidelberg, on "Hybridisation and the Systematic Arrangement of Orchids."

de Barri Crawshay, Esq., of Sevenoaks, England, on "Hybrid *Odontoglossa*."

Professor John Macfarlane, of Philadelphia, U.S.A., on "The Occurrence of Natural Hybrids in the genus *Sarracenia*."

C. T. Druery, Esq., F.L.S., V.M.H., of Acton, England, on "Fern Breeding."

6.30. Dinner to the Foreign Members of the Conference at the Hotel Windsor, Victoria Street, at the kind invitation of the Horticultural Club. —Morning Dress.

Wednesday, August 1.

10.30 to 12.15. Third Session of the Conference.

Dr. E. Tschermak, of the Hochschule für Bodencultur, Vienna, on "The importance of Hybridisation in the Study of Descent."

Prof. Rosenberg, of the University, Stockholm, on "Cytological Investigations on Plant Hybrids."

Prof. C. H. Ostenfeld, of Copenhagen, on "Castration and Hybridisation in the genus *Hieracium*."

Monsieur Noël Bernard, of the University, Caen, France, on "The Germination of Orchids": with Lantern Slides.

E. A. Bunyard, Esq., of Maidstone, England, on "Xenia."

12.30. Special Train leaves Victoria Station.

1.30. Luncheon at Burford, at the kind invitation of Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., President of the Society, and Lady Lawrence.

Thursday, August 2.

10.30 a.m. to 1. Fourth Session of the Conference.

Miss Saunders, of Newnham College, Cambridge, England, on "Certain Complications arising in the Cross-Breeding of Stocks."

Dr. Erwin Smith, of the Department of Agriculture, Washington, U.S.A., on "The work of the U.S.A. Bureau of Plant Breeding."

Sir Daniel Morris, K.C.M.G., V.M.H., of Barbados, West Indies, on "The Improvement of the Sugar-Cane by Selection and Hybridisation."

Monsieur Phillipe de Vilmorin, of Paris, on "Hybrid Wheats."

Prof. C. A. Zavitz, of Guelph, Canada, on "The Breeding of Oats, Barley and Wheat."

Charles E. Saunders, Esq., Ph.D., of Ottawa, on "The Inheritance of Awns in Wheat."

Dr. L. Wittmack, of the Royal Agricultural College, Berlin, on "*Solanum Commersonii*, the Swamp-Potato."

H.-H. B. Bradley, Esq., of Sydney, Australia, on "Hybridising at the Antipodes."

George Kerslake, Esq., of Sydney, Australia, on "Some practical experiments in Cross-Fertilisation in New South Wales."

R. H. Biffen, Esq., of Cambridge, on "Experiments on the Breeding of Wheats for English Conditions": with Lantern Slides.

E. S. Salmon, Esq., F.L.S., of the College, Wye, Kent, on "Raising Strains of Plants resistant to Fungus Disease."

1.15. Light Refreshments.

2.30 to 5. FIFTH Session of the Conference.

C. Willis Ward, Esq., of New York, on "Carnation Breeding in America."

John H. Troy, Esq., of New York, on "American Florists' Ideals."

James Douglas, Esq., V.M.H., of Great Bookham, Surrey, England, on "Cross Fertilisation of the Auricula, and of the Carnation."

Herr C. G. Van Tubergen, Jun., of Haarlem, Holland, on "Hybrids and Hybridisation among Bulbous Plants."

H. F. Groff, Esq., of Ontario, on "Practical Plant-Breeding."

A. Worsley, Esq., of Isleworth, England, on "Hybrids among the *Amaryllidæ* and *Cactaceæ*, with some notes on variation in the *Gesneraceæ* and the Genus *Senecio*."

G. Yeld, M.A., Esq., of York, England, on "Hybrids of *Hemerocallis*."

Herr Max Bürger, of Halberstadt, Germany, on the "Hybrid *Pelargonium grandiflorum nanum*."

A. W. Paul, Esq., of Waltham Cross, England, on "The Derivation of some recent Varieties of Roses."

Prof. N. E. Hansen, of the South Dakota Agricultural College, U.S.A., on "The Breeding of Cold-Resistant Fruits."

H. Somers Rivers, Esq., of Sawbridgeworth, England, on "The Cross-Breeding of Peaches and Nectarines."

W. Laxton, Esq., on "The Cross-Breeding and Hybridisation of Peas and of Hardy Fruits."

7. Banquet in the Society's Great Hall. Evening Dress.

Friday, August 3.

10.30 A.M. Carriages leave Vincent Square.

10.30 to 11.30. Visit to the Natural History Museum.

12. Visit the gardens at Gunnersbury.

1.30. Luncheon at Gunnersbury, at the kind invitation of Mr. Leopold de Rothschild.

3 to 5. Visit Kew Gardens, where Lieut.-Colonel D. Prain, F.R.S., will kindly entertain the members of the Conference at tea.

N.B.—Ladies are cordially invited to both the Conversazione and the Banquet, to both of which they will be most gladly welcomed. Additional Tickets may be obtained from the Secretary: for the Conversazione, 2*s*. 6*d*.; for the Banquet, Ladies £1 1*s*., Gentlemen 25*s*.

In view of its being helpful in the future, an exact reprint is given of the books of tickets referred to in the circular letters of invitations. Each page was perforated at the side, with face and back printed in English and French respectively, and was on different coloured paper, for ease of tearing out and to reduce the possibility of error to a minimum; each page also bore in the right hand corner the number 1, 2, 3, or whatever it might be under which the invited guest to whom it was sent was enrolled on the Conference list, his name having to be written on the cover only. Every invited member of the Conference received a book of tickets as the Society's guest. No. 197 is selected for reprint for no other reason than that it chanced to come first to hand.

COVER.] [No. 197.

ROYAL HORTICULTURAL SOCIETY,

VINCENT SQUARE, WESTMINSTER.

Established A.D. 1804. Incorporated A.D. 1809.

INTERNATIONAL CONFERENCE

ON

HYBRIDISATION AND CROSS-BREEDING,

July 30th to Aug. 3rd, 1906.

Name..

NOTA BENE.—**NONE** of the following Tickets are transferable or may be used by any one but the person whose name is written above.

BACK OF COVER.]

Si les Hôtes étrangers de la "Royal Horticultural Society" désirent visiter dans la semaine de la Conférence (29 Juillet au 5 Août) les Jardins de la "Zoological Society" à Regent's Park, cette Société a bien voulu prendre les arrangements nécessaires pour leur entrée gratuite avec ce livret.

W. WILKS, *Secretary*.

PAGE 1.] [No. 197.

All the Guests of the Society are provided with the following Tickets for their own convenience and in order that they may meet with no inconvenience or hindrance at the doors or elsewhere.

The holder of this book of Tickets is requested always to carry it with him during the proceedings of the Conference and to be so kind as to show it whenever applied to.

NOTA BENE.—These Tickets are not any of them transferable but are only for the use of the person whose name is written on the cover. Should any of the tickets not be required for use the holder would greatly oblige by sending them at once to The Secretary, Royal Horticultural Society, Vincent Square, Westminster.

Note also that the Ticket herein for the Banquet does not itself admit, but can be exchanged for a Special Ticket before 2.30 p.m. on Thursday, August 2nd.

W. WILKS, *Secretary.*

BACK OF PAGE 1.]

Tous les hôtes de la Société sont pourvus de ces Billets afin de faciliter leur entrée aux portes ou ailleurs.

Le porteur de ce livret est prié de l'avoir toujours avec lui et de vouloir bien le montrer sur demande.

À NOTER.—Les billets sont personnels. Si l'invité ne peut pas s'en servir lui-même, il est prié de vouloir bien le renvoyer sur le champ au "Secretary, Royal Horticultural Society, Vincent Square, Westminster."

À NOTER.—Le billet ci-après n'admet pas au Banquet—il faut l'échanger contre un billet spécial avant 2.30 de l'après-midi du Jeudi 2 Août.

W. WILKS, *Secretary.*

PAGE 2.] [No. 197.

MONDAY, JULY 30th.

Ticket for the

CONVERSAZIONE

IN THE

Society's Great Hall, Vincent Square.

9 p.m. to 10.30 p.m.

9.30.—Address of Welcome by the President of the Society, Sir TREVOR LAWRENCE, Bart., K.C.V.O., V.M.H., &c.

10.—Display of various Hybrids, Lantern Slides, &c.

Light Refreshments will be served during the Evening.

Additional Tickets can be obtained on application at 2/6 each.

W. WILKS, *Secretary.*

BACK OF PAGE 2.]

LUNDI, 30 JUILLET.

Billet pour

LA RÉUNION

DANS LA

"Society's Great Hall, Vincent Square,"
9 heures soir à 10.30.

9.30.—Adresse de Bienvenue par le Président, Sir TREVOR LAWRENCE, Bart., K.C.V.O., V.M.H.

10.—Étalage de plusieurs Hybrides et de plaques photographiques, etc.

ON SERVIRA DES RAFRAÎCHISSEMENTS PENDANT LA SOIRÉE.

W. WILKS, *Secretary.*

PAGE 3.] [No. 197.

TUESDAY, JULY 31st.

Ticket for the

First Session of the Conference

IN THE

LECTURE ROOM, VINCENT SQUARE,

10.30 a.m. to 1 p.m.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 3.]

MARDI, 31 JUILLET.

Billet pour

LA PREMIÈRE SÉANCE DE LA CONFÉRENCE

DANS LA

"LECTURE ROOM," VINCENT SQUARE,

10 heures 30, matin, à 1 heure.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 4.] [No. 197.

TUESDAY, JULY 31st.

Ticket for

LIGHT REFRESHMENTS,

Which will be served at 1.15 in the Committee Rooms on the First Floor.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 4.]

MARDI, 31 JUILLET.

Billet pour

LES RAFRAÎCHISSEMENTS,

Qui seront servis à 1.15 dans les "Committee Rooms," au premier étage.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 5.] [No. 197.

TUESDAY, JULY 31st.

Ticket for the

Second Session of the Conference

IN THE

LECTURE ROOM, VINCENT SQUARE,

2.30 p.m. to 5 p.m.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 5.]

MARDI, 31 JUILLET.

Billet pour

LA SECONDE SÉANCE DE LA CONFÉRENCE

DANS LA

"LECTURE ROOM," VINCENT SQUARE,

2.30 de l'après-midi à 5 heures.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 6.] [No. 197.

TUESDAY, JULY 31st.

Ticket for the

DINNER

AT THE

"Hotel Windsor," Victoria Street, Westminster,

At 6 for 6.30 p.m.

At the kind invitation of the Horticultural Club.

MORNING DRESS.

W. WILKS, *Secretary.*

BACK OF PAGE 6.]

MARDI, 31 JUILLET.

Billet pour

LE DÎNER

À l'Hôtel Windsor, Victoria St., Westminster,

À 6 heures pour 6.30,

Offert par le "Horticultural Club."

TOILETTE DE MATIN.

W. WILKS, *Secretary.*

PAGE 7.] [No. 197.

WEDNESDAY, AUGUST 1st.

Ticket for the

Third Session of the Conference

IN THE

LECTURE ROOM, VINCENT SQUARE,

10.30 a.m. to 12.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 7.]

MERCREDI, 1 AOÛT.

Billet pour

LA TROISIÈME CONFÉRENCE

DANS LA

"LECTURE ROOM," VINCENT SQUARE,

10.30, matin, à midi.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 8.] [No. 197.

WEDNESDAY, AUGUST 1st.

Ticket for the

Royal Horticultural Society's Special Train to Burford.

Leaving Victoria Station, L. B. & S. C. R. at 12.30 precisely.

W. WILKS, *Secretary.*

BACK OF PAGE 8.]

MERCREDI, 1 AOÛT.

Billet pour le

TRAIN EXTRA DE LA "ROYAL HORTICULTURAL SOCIETY" À BURFORD,

Départ de "Victoria Station," L. B. & S. C. Ry. à midi 30 précis.

W. WILKS, *Secretary.*

PAGE 9.] [No. 197.

WEDNESDAY, AUGUST 1st.

Ticket for the

LUNCHEON,

At 1.30.

At BURFORD,

At the kind invitation of Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., President of the Society, and Lady Lawrence.

W. WILKS, *Secretary.*

BACK OF PAGE 9.]

MERCREDI, 1 AOÛT.

Billet pour

LE DÉJEUNER,

À 1 heure et demie,

A BURFORD,

Offert par Sir TREVOR LAWRENCE, Bart., K.C.V.O., Président de la Société.

W. WILKS, *Secretary*

PAGE 10.] [No. 197.

WEDNESDAY, AUGUST 1st.

Ticket for the

ROYAL HORTICULTURAL SOCIETY'S SPECIAL TRAIN TO LONDON,

Leaving Burford Station, L. B. & S. C. Rly., at 5.23 precisely.

W. WILKS, *Secretary.*

BACK OF PAGE 10.]

MERCREDI, 1 AOÛT.

Billet pour le

TRAIN EXTRA DE LA ROYAL HORTICULTURAL SOCIETY. BILLET DE RETOUR À LONDRES.

Départ de Burford Station, L. B. & S. C. Rly., à 5.23 du soir.

W. WILKS, *Secretary.*

PAGE 11.] [No. 197.

THURSDAY, AUGUST 2nd.

Ticket for the

Fourth Session of the Conference,

IN THE

LECTURE ROOM, VINCENT SQUARE,

10.30 a.m. to 1 p.m.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 11.]

JEUDI, 2 AOÛT.

Billet pour

LA QUATRIÈME SÉANCE DE LA CONFERENCE

DANS LA

"LECTURE ROOM," VINCENT SQUARE,

10.30 matin à 1 heure.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 12.] [No. 197.

THURSDAY, AUGUST 2nd.

Ticket for

LIGHT REFRESHMENTS,

Which will be served at 1.15 in the Committee Rooms on the First Floor.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 12.]

JEUDI, 2 AOÛT.

Billet pour

LES RAFRAÎCHISSEMENTS,

Qui seront servis à 1.15 dans les "Committee Rooms," au premier étage.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 13.] [No. 197.

THURSDAY, AUGUST 2nd.

Ticket for the

Fifth Session of the Conference,

IN THE

LECTURE ROOM, VINCENT SQUARE,

2.30 p.m. to 5.

Cloak Room and Lavatory Accommodation in the Basement.

W. WILKS, *Secretary.*

BACK OF PAGE 13.]

JEUDI, 2 AOÛT.

Billet pour

LA CINQUIÈME SÉANCE DE LA CONFÉRENCE

DANS LA

"LECTURE ROOM," VINCENT SQUARE,

2.30 de l'après-midi à 5 heures.

Garde-robe et Toilette au Sous-sol.

W. WILKS, *Secretary.*

PAGE 14.] [No. 197.

THURSDAY, AUGUST 2nd.

This Ticket should be exchanged before 2.30 p.m. on Thursday, August 2nd, at the Secretary's Office, Vincent Square, for a Special Ticket admitting to the

GRAND BANQUET

IN THE

SOCIETY'S GREAT HALL,

At 7 for 7.30 punctually.

Additional Tickets can be obtained on application before Wednesday, August 1st.—Ladies, £1 1*s*. Gentlemen, 25/-

W. WILKS, *Secretary.*

BACK OF PAGE 14.]

JEUDI, 2 AOÛT.

Il faut échanger ce Billet avant 2.30 de l'après-midi Jeudi, 2 Août, au Bureau du Secrétaire à Vincent Square, contre un Billet spécial qui admettra

AU GRAND BANQUET

DANS LE

"GRAND HALL" de la SOCIÉTÉ,

Á 7 heures pour 7.30 précises.

On peut obtenir des billets extra sur demande avant Mercredi 1 Août.

Dames, 21/–. Messieurs, 25/–.

W. WILKS, *Secretary.*

PAGE 15.] [No. 197.

FRIDAY, AUGUST 3rd.

Ticket for a

CONVEYANCE to the Natural History Museum, Gunnersbury, Kew, and return.

Carriages will leave Vincent Square at 10.30 a.m.

W. WILKS, *Secretary.*

BACK OF PAGE 15.]

VENDREDI, 3 AOÛT.

Billet pour

VOITURE, aller et retour, au "Natural History Museum," à Gunnersbury, et à Kew.

Les Voitures partiront de Vincent Square à 10.30 du matin.

W. WILKS, *Secretary.*

PAGE 16.] [No. 197.

FRIDAY, AUGUST 3rd.

Ticket for the

LUNCHEON at Gunnersbury,

At 1.30 p.m.

At the kind invitation of Mr. LEOPOLD DE ROTHSCHILD.

W. WILKS, *Secretary.*

BACK OF PAGE 16.]

VENDREDI, 3 AOÛT.

Billet pour

LE DÉJEUNER à Gunnersbury,

À une heure 30,

Offert par Monsieur LEOPOLD DE ROTHSCHILD.

W. WILKS, *Secretary.*

LIST OF INVITED GUESTS.

The following is the list of those who were specially invited to the Conference by the Council of the Society. Some were unfortunately unable to be present, but their names are included below as serving as a guide for any future similar Conference.

Agar, W. E., Natural History Department, The University, Glasgow.
Allen, Prof. G. M., Cambridge, Massachusetts.
André, Edouard, 30 rue Chaptal, Paris.
Baden-Powell, Miss, 32 Prince's Gate, London, S.W.
Bailey, Prof. L. H., Cornell University, Ithaca, New York, U.S.A.
Baker, W. G., Botanic Garden, Oxford.
Balfour of Burleigh, the Rt. Hon. Lord, K.T., Kennet House, Alloa, N.B.
Balfour, Prof. I. B., D.Sc., F.R.S., V.M.H., Royal Botanic Garden, Edinburgh.
Balls, Prof. W. L., Khedival Agricultural Society Laboratories, Cairo.
Barbey, Prof. W., Valleyres, Vaud, France.
Barr, J. W., Long Ditton, Surrey.
Bartleet, H. S., Severndroog, Shooter's Hill, Kent.
Bateson, W., M.A., V.M.H., F.R.S., Merton House, Grantchester, Cambs.
Bateson, Mrs., Merton House, Grantchester, Cambs.
Batten, Dr. F. E., 33 Harley Street, London, W.
Bauer, Dr. E., Bot. Inst. der Universität, Berlin.
Beach, S. A., Experiment Station, Ames, Iowa, U.S.A.
Benary, Ernest, Erfurt, Germany.
Bennett-Poë, John T., M.A., V.M.H., 29 Ashley Place, Westminster, S.W.
Bernard, Noël, Maître de Conférences à l'Université, 10 rue Caponière, Caen, France.
Biffen, R. H., The Gables, Histon, Cambs.
Bilney, W. A., Fir Grange, The Heath, Weybridge.
Bitter, Prof., Director, Botanisches Garten, Bremen, Germany.
Blackman, F. F., St. John's College, Cambridge.
Blackman, V. H., Natural History Museum, London, S.W.
Bleu, A., Avenue d'Italie, Paris.
Bois, D., 36 Boulevard Thiers, Fontainebleau, Paris.
Bonhotë, J. L., Gade Spring, Hemel Hempsted.
Bonnier, Prof. G., Sorbonne, Paris.
Booth, Sir Josslyn Gore-, Bart., Lissadell, Sligo.
Boscawen, Hon. John, Tregye, Perranwell, Cornwall.
Boulger, Prof. G. S., F.L.S., 11 Onslow Road, Richmond, Surrey.
Boullet, Eugene Banquier, Corbie, Somme, France.
Boveri, Prof., Würzburg, Germany.
Bowles, E. A., M.A., F.L.S., Myddleton House, Waltham Cross.
Bradley, H. H. B., 60 Margaret Street, Sydney, N.S.W.
Bridgman, F. J., Kenmore, Avenue Road, Highgate, N.
Browne, R. Staples-, 22 Park Road, Cambridge.
Bruant, Georges, Boulevard Pont-Neuf 23, Poitiers, Vienne.
Bunyard, E. A., The Royal Nurseries, Maidstone.

Bunyard, George, V.M.H., The Royal Nurseries, Maidstone.
Burbank, Luther, Santa Rosa, California.
Bürger, Max, Schützenstrasse 7, Halberstadt, Germany.
Cahuzac, R. Martin-, Chateau de Sybirol, Floirac, Gironde, France.
Camus, E. G., lauréat de l'Institut de France, 199 rue Lecourbe, Paris.
Carruthers, W., F.R.S., 44 Central Hill, Norwood, London, S.E.
Castle, W. E., Harvard University, Cambridge, Massachusetts, U.S.A.
Chapman, H. J., The Gardens, Oakwood, Wylam-on-Tyne.
Chappelier, P., Faubourg Poissonière, Paris.
Charlesworth, J., Heaton, Bradford, Yorkshire.
Cheal, Joseph, Lowfield, Crawley, Sussex.
Chittenden, F. J., Biological Laboratory, Chelmsford
Cobb, Dr., Experimental Farms, Wagga Wagga, N.S.W.
Colman, Jeremiah, Gatton Park, Reigate, Surrey.
Cook, E. T., 20 Tavistock Street, Covent Garden, London.
Cook, O. F., Bureau of Plant Industry, Department of Agriculture Washington, U.S.A.
Cookson, Norman C., Oakwood, Wylam-on-Tyne.
Corbett, Prof. L., Bureau of Plant Industry, Department of Agriculture, Washington.
Correns, Prof. C., Leipzig, Germany.
Costantin, Prof. J., 61 rue de Buffon, Paris.
Crawshay, de Barri, Rosefield, Sevenoaks, Kent.
Crozy, M., rue de la Guillotière, Paris.
Cuboni, Prof. G., Director of the Royal Institute of Vegetable Pathology, Rome.
Cuenot, Prof. L., l'Université, Nancy, France.
Dammer, Prof. Udo, Dahlem, bei Gross Lichterfelde III., Altensteinstrasse 37.
Darbishire, A. D., Royal College of Science, South Kensington.
Darbishire, H. C., 68 Brooklyn Street, Crewe, Cheshire.
Darwin, Francis, F.R.S., 13 Madingley Road, Cambridge.
Daveau, M., Jardin Botanique de l'Université, Montpellier, Hérault.
Davenport, Prof., Carnegie Experiment Station, Cold Spring Harbour, New York, U.S.A.
Defosse, H., 23 Route d'Olivet, Orleans.
Demilly, J., Ecole de Pharmacie, Paris.
Denis, F., Balaruc les Bains, Hérault, France.
De Vries, Prof. Hugo, Parklaan 9, and the University, Amsterdam.
Dickson, A., senior, Newtownards, Scotland.
Dixon, Prof. H. H., Trinity College, Dublin.
Doncaster, L., King's College, Cambridge.
D'Orves, Comte d'Estienne, Quai de la Megisserie 4, Paris.
Douglas, James, V.M.H., Edenside, Gt. Bookham, Surrey.
Druery, Charles T., V.M.H., 11 Shaa Road, Acton, London, W.
Duchartre, Monsieur, Ferrières, en Gatinais, Loiret.
Durham, Miss, 83 Tenison Road, Cambridge.
Duval, L., 8 rue de l'Ermitage, Versailles, Paris.
Dykes, W. R., Charterhouse, Godalming, Surrey.
East, E. M., Agricultural Experiment Station, New Haven, Connecticut.

Elwes, H. J., F.R.S., V.M.H., Colesborne, Cheltenham, Glos.
Emerson, R. A., Agricultural Experiment Station, Lincoln, Nebraska.
Engleheart, Rev. G. H., V.M.H., Dinton, Salisbury.
Engler, Dr. A., Dahlem, Berlin.
Ewart, J. Cossar, M.D., F.R.S., The University, Edinburgh.
Fairchild, David, Bureau of Plant Industry, Department of Agriculture, Washington, U.S.A.
Farmer, Prof. J.B., D.Sc., F.R.S., Royal College of Science, South Kensington, S.W.
Fawcett, William, Director, Department of Public Gardens, Kingston, Jamaica.
Fenn, Robert, V.M.H., Holmwood, Sulhamstead, Reading.
Focke, Dr. W. O., Stein-Kreuz 5, Bremen, Germany,
Foster, Sir Michael, K.C.B., F.R.S., V.M.H., Nine Wells, Great Shelford, Cambs.
Fowler, J. Gurney, Glebe Lands, South Woodford, Essex.
Fraser, Samuel, Cornell University, Ithaca, New York, U.S.A.
Froebel, Otto, Zurich, Switzerland.
Fruwirth, Prof. C., Königliche Landwirtschaftliche Akademie, Hohenheim, Württemberg, Germany.
Galpin, G., 61 rue de la Boche, Paris.
Galton, Francis, F.R.S., D.C.L., 42 Rutland Gate, London, S.W.
Gardiner, J. S., Caius College, Cambridge.
Giard, Prof. A., rue Stanislas 14, Paris.
Giltay, Dr., Wageningen, Holland.
Godman, F. du C., F.R.S., D.C.L., 10 Chandos Street, W.
Gordon, George, V.M.H., Endsleigh, Priory Park, Kew.
Gregory, R. P., St. John's College, Cambridge, England.
Groff, H. H., Simcoe, Ontario.
Grosvenor, New College, Oxford.
Guaita, Dr. Georg von, Maria Theresiastr. 11, Freiburg-in-B.
Gunn, A. R., 13 Roseneath Terrace, Edinburgh.
Güssow, H. T., 44 Central Hill, Norwood, S.E.
Haacke, Prof. W., Lingen, Hannover.
Hanson, Prof. N. E., Agricultural College, Brookings, S. Dakota, U.S.A.
Hartley, Dr. C. P., Bureau of Plant Industry, Department of Agriculture, Washington.
Hartog, Prof. M., M.A., D.Sc., F.L.S., Queen's College, Cork.
Hays, Prof. Willet M., Department of Agriculture, Washington, U.S.A.
Heape, Walter, Heyroun, Chaucer Road, Cambridge.
Hemsley, W. Botting, F.R.S., The Green, Kew.
Henriques, Dr. J. A., Coimbra, Portugal.
Henry, Dr. A., V.M.H., Royal Gardens, Kew.
Henry, Prof. Louis, École Nationale d'Horticulture, Versailles, Paris.
Henslow, Rev. Prof., M.A., V.M.H., Drayton House, Kenilworth Road, Leamington.
Hildebrand, Prof., Freiburg-in-B., Germany.
Hillebrand, Frau Professor, Vienna.
Hincks, Miss A. K., Barons Down, Dulverton.
Hindmarsh, Wm. T. Alnbank, Alnwick, Northumberland.

Hogg, R. Milligan, 13 Whitefriars Street, London, E.C.
Holford, Major George, C.I.E., C.V.O., Westonbirt, Gloucestershire.
Hooker, Sir Joseph D., K.C.S.I., F.R.S., V.M.H., Sunningdale, Ascot.
Howard, F. H., Los Angeles, California, U.S.A.
Hudson, James, V.M.H., The Gardens, Gunnersbury House, Acton, W.
Hurst, C. C., F.L.S., Burbage, Leicestershire.
Johannsen, Prof. W., Universitetets plantefysiologiske Laboratorium, Copenhagen.
Juel, Prof. O., Upsala, Sweden.
Keeble, Prof. Frederick, University College, Reading.
Kerr, R. Wilson, Basnett Street, Liverpool.
Kerslake, G. H., Pott's Hill, Rookwood, Sydney, N.S.W.
Killby, Miss, Glen Villa, Cherryhinton Road, Cambridge, England.
Koernicke, Prof. M., Bonn, Germany.
Krelage, E. H., Haarlem, Holland.
Latour-Marliac, B., Temple-sur-Lot, France.
Lawrence, Sir Trevor, Bart., K.C.V.O., V.M.H., Burford, Dorking.
Laxton, William, Bedford.
Lees, A. H., King's College, Cambridge.
Lees, James G., Sydney Villa, Stannergait, Dundee.
Lehmann, Prof. C., Royal Agricultural High School, Berlin.
Leichtlin, Max, Baden-Baden.
Lemoine, Emile, Nancy.
Lemoine, Victor, 134 rue de Montet, Nancy.
Llewelyn, Sir J. T. Dillwyn-, Bart., J.P., D.L., Penllergaer, Swansea.
Loader, John, Edinglassie, N.S.W., Australia.
Lock, R. H., Caius College, Cambridge, England.
Loisel, Dr., École des Hautes Études, 18 rue de l'Ecole de Medecin, Paris
Lotsy, Dr. J. P., Rijn en Schiekade 113, Leiden.
Ludwig, Prof. F., Greiz, Austria.
Lynch, R. Irwin, V.M.H., Botanic Garden, Cambridge.
MacDougal, D. T., Carnegie Institution, New York, U.S.A.
Macfarlane, Prof. John M., Director of the Botanic Garden, The University, Philadelphia.
Malinvaud, E., rue Linné 8, Paris.
Maron, C., 3 rue de Montgeron, Brunoy, Seine-et-Oise, France.
Marryat, Miss D., Newnham College, Cambridge.
Marshall, William, V.M.H., Auchinraith, Bexley, Kent.
Massee, George, V.M.H., F.L.S., Gateacre, Sandycombe Road, Kew.
Masters, Dr. Maxwell T., F.R.S., 41 Wellington Street, London, W.C.
May, H. B., Stanmore, High Street, Chingford, Essex.
Meehan, Thomas, Germantown, Philadelphia, U.S.A.
Meredith, Venerable Archdeacon, Tibberton Rectory, Newport, Salop.
Micheli, Marc, Chateau du Crest, Jussy, Geneva.
Middleton, Prof. T. H., South House, Barton Road, Cambridge.
Mollison, J., Inspector-General of Agriculture in India, Calcutta.
Moore, F. W., V.M.H., Royal Botanic Gardens, Glasnevin, Dublin.
Morel, F., rue de Souvenir, Lyon Vaise, France.
Morris, Sir Daniel, K.C.M.G., V.M.H., Imperial Department of Agriculture, Barbados, W. Indies.

Moser, J. J., rue S. Symphorien, Versailles, Paris, France.
Mountmorres, Viscount, The Municipal Museums, Liverpool.
Munson, F., Vinita Home, Denison, Texas.
Murbeck, Prof. S., Lund, Sweden.
Murrill, W. A., Botanical Garden, New York.
Nettleship, E., Nutcombe Hall, Hindhead, Haslemere.
Nicholson, George, V.M.H., 37 Larkfield Road, Richmond, Surrey.
Nilsson, Prof. M., Svalöf, Sweden.
Nobbe, Dr. F., Tharandt.
Nonin, Auguste, 20 Avenue de Paris, Chatillon-sous-Bagneux, France.
Noorduijn, C. L. W., Ontvanger van Rijksbelastigen, Groningen, Holland.
Norton, Jesse B., Washington.
Oberthür, René, Rennes, France.
O'Brien, J., V.M.H., Marian, Harrow, England.
Odell, J. W., Grove Farm, Stanmore, Middlesex.
Oliver, Prof. F. W., F.R.S., University College, Gower Street, London, W.C.
Orton, W. A., Bureau of Plant Industry, Department of Agriculture, Washington.
Ostenfeld, Dr. C. H., Aabenraa 31, Copenhagen.
Pacherot, Jules, 12 rue Carnot, Billancourt, France.
Paton, Rev. J. Aikman, B.Sc., Soulseat, Castle Kennedy, N.B.
Paul, A. W., Waltham Cross, Hertfordshire.
Paul, George, V.M.H., Cheshunt, Hertfordshire.
Pearson, A. H., The Hut, Lowdham, Nottingham.
Pearson, Prof. R., University College, Gower Street, London, W.C.
Peeters-Carter, A. A., 72 Chaussée de Forest, St. Gilles, Brussels.
Percival, Prof. John, M.A., F.L.S., University College, Reading, Berkshire.
Perkins, Miss Janet, Ph.D., Royal Botanical Museum, Berlin.
Pfitzer, Prof. E., Bergheimer Strasse 1, Heidelberg.
Pfitzer, Wilhelm, Militärstrasse 74, Stuttgart, Germany.
Pickering, Spencer, M.A., F.R.S., Harpenden, Hertfordshire.
Plate, Prof. L., Royal Agricultural High School, Berlin.
Prain, Lieut.-Col. D., F.R.S., Director Royal Gardens, Kew.
Przibram, Prof. Hans, Biologische-Versuchs-Anstalt, Vienna.
Punnett, R. C., Caius College, Cambridge, England.
Pynaert, E., Ghent.
Quetard, Maurice, Corbie, Somme, France.
Ragot, Jules, Villenoy, Meaux, S. et M., France.
Raunkiaer, Dr., Botanic Garden, Copenhagen.
Rendle, Dr. A. B., M.A., D.Sc., Natural History Museum, South Kensington.
Rivers, H. Somers, The Nurseries, Sawbridgeworth.
Roberts, H. F., Experiment Station, Manhattan, Kansas.
Robertson, Miss Agnes, D.Sc., University College, Gower Street, London, W.C.
Rolfe, R. Allan, A.L.S., Royal Gardens, Kew.
Rollit, Sir Albert, LL.D., D.C.L., 45 Belgrave Square, S.W.
Rosenberg, Prof. Otto, Botaniska Institutet, Högskda, Stockholm.
Rümker, Prof. von, Matthiasplatz 20, Breslau.
Salaman, Dr. R. N., Homestall, Barley, Royston, Herts.

Sallier, Joanni, rue Delaizemen, Neuilly.
Salmon, E. S., F.L.S., The College, Wye, Kent.
Sander, F., V.M.H., The Camp, St. Albans.
Sargent, Prof. C. S., The Arnold Arboretum, Jamaica Plain, Massachusetts.
Saunders, C. E., Department of Agriculture, Ottawa, Canada.
Saunders, George, F.L.S., 20 Dent's Road, Wandsworth Common, S.W.
Saunders, Miss E., Newnham College, Cambridge.
Saunders, Dr. W., Department of Agriculture, Ottawa, Canada.
Schmidt, Carl, Erfurt, Germany.
Schröder, Baron Sir J. Henry, Bart., V.M.H., The Dell, Englefield Green.
Schuster, Edgar, 110 Banbury Road, Oxford.
Scott, Dr. D. H., Ph.D., F.R.S., Old Palace, Richmond, Surrey.
Scott-Elliot, Prof. G., Newton, Dumfries, N.B.
Scrase-Dickins, C. R., Coolhurst, Horsham, Sussex.
Shull, Dr. G. H., Carnegie Station of Experimental Evolution, Cold Spring Harbour, New York.
Skalweit, Dr. B., Ph.D., Arran, Bampton Road, Forest Hill, London, S.E.
Smith, Dr. Erwin, Bureau of Plant Industry, Department of Agriculture, Washington.
Smith, Geoffery W., Ivy Bank, Beckenham, Kent.
Smith, Martin R., Warren House, Hayes, Kent.
Smith, Dr. W. G., Ph.D., The University, Leeds.
Smith, Worthington G., 121 High Street, Dunstable.
Sollar, Miss I., Newnham College, Cambridge.
Somerville, W., D.Sc., Plantations, Warlingham.
Spillman, W. J., Bureau of Plant Industry, Department of Agriculture, Washington.
Stansfield, Dr. F. W., 120 Oxford Road, Reading.
Stapf, Dr. O., F.L.S., Hanover House, Kew.
Sutton, A. W., V.M.H., 6 Abbott's Walk, Reading.
Sutton, Leonard, Hillside, Reading.
Sutton, Prof. Walter S., Columbia University, New York.
Swingle, Walter, Bureau of Plant Industry, Department of Agriculture, Washington, U.S.A.
Tankerville, Earl of, Thornington House, Mindrum, R.S.O., Northumberland.
Tedin, Dr. H., landwirtschaftliche Versuchsstation, Svalöf.
Thays, Charles, Botanic Garden, Buenos Ayres, South America.
Thiselton-Dyer, Sir William, K.C.M.G., F.R.S., The Ferns, Witcombe, Glos.
Thomas, Miss E. N., University College, Gower Street, London, W.C.
Thomson, P. Murray, 5 York Place, Edinburgh.
Tischler, Dr. Geo., Bot. Inst. der Universitat, Heidelberg, Germany.
Trabut, Dr. Louis, rue des Fontaines, Algiers.
Troy, John H., New Rochelle, New York.
Truffaut, A., 40 rue de Chantiers, Versailles, Paris.
Truffaut, G., avenue de Picardie, Paris.
Tschermak, Dr E., Hochschule für Bodencultur, Vienna.
Vacherot, Jules, rue Carnot 12, Billancourt, France.

van Tubergen, C. C., jun., Zwanenburg Nurseries, Haarlem, Holland.
Veitch, Harry J., V.M.H., 34 Redcliffe Gardens, South Kensington.
Veitch, Peter, Royal Nurseries, Exeter.
Vilmorin, Maurice de, 13 quai d'Orsay, Paris.
Vilmorin, Phillippe de, 23 quai d'Orsay, Paris.
Voelcker, Dr. J. A., M.A., F.I.C., 20 Upper Phillimore Garndes, London, S.W.
Von Dippe, King's Road, Reading.
Waite, M. B., Bureau of Plant Industry, Department of Agriculture, Washington, U.S.A.
Wallace, Prof. R., F.R.S.E., The University, Edinburgh.
Ward, C. Willis, The Cottage Gardens, Queens, New York, U.S.A.
Ward (the late) Prof. Marshall, F.R.S., Botanical Laboratory, Cambridge.
Watrous, C. L., Des Moines, Iowa, U.S.A.
Watson, William, A.L.S., Royal Gardens, Kew.
Webber, Prof. H. J., Bureau of Plant Industry, Department of Agriculture, Washington.
Wheldale, Miss M., Sidgwick Hall, Newnham, Cambridge, England.
Whitman, Prof. C. O., The University, Chicago, U.S.A.
Wigan, A. L., Forest Park, Windsor, England.
Wildeman, E. D., Jardin Botanique, Brussels.
Williams, P. D., Lanarth, St. Keverne, R.S.O., Cornwall.
Willmott, Miss, V.M.H., Warley Place, Great Warley, Essex.
Wilson, John H., D.Sc., F.R.S.E., The University, Edinburgh.
Witte, E. T., Jardin Botanique de l'Université, Leide, Pays Bas.
Wittmack, Prof. Dr. L., Royal Agricultural High School, Berlin.
Worsley, A., Mandeville House, Isleworth, England.
Yeld, George, Clifton Cottage, York.
Yule, G. Udny, 50 St. James's Court, Westminster.
Zavitz, A. C., Ontaria Agricultural College, Guelph, Canada.

Wilks, Rev. W., M.A. (Secretary, Royal Horticultural Society), Shirley Vicarage, Croydon.
Sedgwick, Thos. E. (Assistant Secretary, Royal Horticultural Society), Vincent Square, Westminster, S.W.
Howe, J. W. (Official Shorthand Writer), 84 Broomwood Road, Wandsworth Common, S.W.

The Rev. William Wilks, M.A., Secretary of the Royal Horticultural Society, Vicar of Shirley, Surrey.

THE CONVERSAZIONE.

The Conference opened with a Conversazione in the Society's Great Hall on Monday evening, July 30, when the foreign guests of the Society were received by Sir Trevor Lawrence, Bart., V.M.H., K.C.V.O., President of the Royal Horticultural Society, and were introduced to their British *confrères*.

Tables were set all round the hall (the centre being left open save for trophies of palms and flowers), and on them were numerous most interesting exhibits bearing on the subject of the Conference. A description of the most remarkable of these exhibits will be found a few pages further on.

* * *

Refreshments were served in the two annexes.

BILL OF FARE.

Sandwiches.

Ham. Chicken. Foie Gras.
Cress. Anchovy. Tongue.

Pastries.
Mirletons à la Favorite.
Chocolat Eclairs. Tartlets.
"Surprise" Cutlets. Maids of Honour.

Currant Cakes. Plain Cakes.
Madeira Cakes.
Seed Cakes.
Chocolate Cakes. Sponge Cakes.
Fancy Biscuits.

Tea, Coffee, Hock Cup, Claret Cup, Lemonade.

Strawberry, Lemon, and Vanilla Ices.

* * *

At intervals during the evening the band of His Majesty's Royal Artillery, under the conductorship of Cavaliere L. Zavertal, M.V.O. R.A., performed the following programme of music:—

1.	March	"Pomp and Circumstance"	*Elgar*
2.	Overture	"Ruy Blas"	*Mendelssohn*
3.	Menuet and Farandole (L'Arlésienne)		*Bizet*
4.	Selection	"Lohengrin"	*Wagner*
5.	Pizzicati	"Sylvia"	*Delibes*
6.	"Bolero"		*Moszkowski*
7.	"Berceuse de Jocelyn"		*Godard*
8.	Masque	"As you like it"	*Ed. German*
9.	(*a*) "In a Trance" (*b*) "Murmurs of the Nile"		*L. Zavertal*
10.	"Danza delle Ore" *from* "La Gioconda"		*Ponchielli*
11.	Selection	"La Traviata"	*Verdi*
12.	"Toréador et Andalouse"		*Rubinstein*
13.	Song (Cornet Solo)	"Adieu"	*Schubert*
14.	Selection	"Dinórah"	*Meyerbeer*

The guests of the Society having all arrived, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., addressed them as follows:—Ladies and Gentlemen,—If there is one thing in the world about which there can be no possible doubt, it is that the Royal Horticultural Society extends a most hearty welcome to all the guests who are interested in the Conference on Plant-breeding, and who have been kind enough to gather round us on this occasion. Especially do we extend our hearty thanks to our foreign guests, many of whom have travelled from distant countries to be here—men whose opinions and views we shall hear and receive with the greatest possible regard and satisfaction. And I think I may say that not only the Royal Horticultural Society extends a most hearty welcome to them, but the welcome is also given by all those who are interested in gardening and in the pursuit of the art and science of Horticulture in this country. They also extend to you all a most hearty welcome. The present occasion marks an era when scientific men are present from all nationalities, and I am sure, whether it be in this country or elsewhere, they will always receive a hearty welcome.

With regard to the Royal Horticultural Society I should like to say a few words, because there may be a good many here who are not very well acquainted with the history of the Society. The Society is now in the 103rd year of its existence, and during the period—the long period—it has been established, it has naturally met with the usual ups and downs of fortune. A good many years ago it was part of the work of the Royal Horticultural Society to send collectors abroad for the purpose of gathering together such shrubs, plants, trees, seeds, bulbs, and other things as it might be thought desirable to introduce into this country. That business has now come to an end. Private enterprise has taken the place of the Society in that respect, and I think it is a subject for congratulation, because it has enabled the Society to devote its energies to fostering, guiding, aiding, and protecting the horticultural interests of this country rather than to sending collectors round the world, which, after all, is more properly the work of professional horticulturists.

I dare say you all know, as is most certainly the case, that the Gardening Industry of this country is a very important industry. I do not think I should be exaggerating at all if I were to say that perhaps it is almost, if not quite, at the head of the minor industries of this country, and that it is closely associated with the great agricultural interests of the land, and may possibly be regarded as being in some respects even of greater importance than that industry.

I need scarcely say that in dealing with the subjects we are to discuss at the Conference I should be the very last person to claim any intimate acquaintance with the specially scientific aspects of those questions; but I know enough to enable me to say that we owe a great deal to the intelligence, enterprise, industry, and sagacity which have been at the root of all the hybridisation and plant-breeding which have been going on for many years in this country.

If we carry our minds back a few years we shall see what enormous strides have been made. I was very much struck, for instance, when we

From a photograph.

Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H. &c., President of the Royal Horticultural Society.

held our recent great Show at Holland House, by noticing the extraordinary development which has taken place in modern times in the genus *Begonia*. We can see at a glance what has been done in regard to that one genus of plants, and I only mention it because it is so almost immediately perceptible. But the same thing is being done in the case of many other genera of flowers, fruits, and vegetables; and I am sure that the public generally have never fully recognised, as it is desirable they should, the very great obligation they are under to plant-breeders and to plant-raisers in every way.

I have often wondered what position we in this country should be in but for introductions from abroad, and for the intelligence, sagacity, and industry which have been devoted to their improvement by horticulturists, British as well as foreign? Why, if all these exotics were suddenly banished from the scene, we should be left with the comparatively few trees, shrubs, and flowers which are indigenous; whereas now there is scarcely a garden in the land, however humble, that has not in it some plants which it owes to the patient intelligence of the plant-breeder. Therefore I say we are under very great obligations to them.

Speaking of our own country, I do not think that, up to the present time, we have, as a nation, allowed our minds to grasp the great importance of the industry of Horticulture and of the incalculable influence of science in relation to it, and indeed to all other aspects of our daily life. I think we are, to some extent, behind foreign nations, even in such matters as gardening; and I am sure that in agriculture we have not followed the guidance of science as closely, or as obediently, as we ought to have done. I dare say you noticed, only a day or two ago, that a dinner was given to a very eminent man of science, Dr. Perkin, who has made some very striking and remarkable discoveries under circumstances with which many of you are doubtless fully cognisant. We know that other countries reaped the benefit of his discoveries. Why was not the importance of his discoveries grasped by Englishmen? I lay no claim to being a man of science myself, but I have some small smattering of the scientific spirit, and I venture to remind you of what Huxley said—that the scientific spirit is more valuable than any of its results. What we in this country want is the scientific spirit. It is a remarkable circumstance that in the present House of Commons, elected after a very arduous fight and after many speeches of great force and power, there is not a single person whose opinion on any scientific subject is worth regarding. That is a rather remarkable fact, but I believe it is perfectly true, and if it is so, I must say we are still sadly lagging behind foreign countries in regard to scientific matters.

I referred just now to plant-breeding, and, as most of you are aware, I have devoted a certain amount of attention to the cultivation of orchids, and I can recommend the cultivation of orchids to people who wish to have a very interesting as well as a very instructive pursuit. I have sometimes been asked whether I think orchids are more beautiful than roses. I think that that is such an absurd question that I always endeavour to hear some other question that may be asked at the same time with a view to answering it instead of the other; the truth being that no one who has ever studied the whole natural order of orchids has failed to get the greatest possible interest from it. At the present time—I believe before very long—the collection of orchids from their native habitats will almost come to an end, and we shall be entirely dependent on those of our own raising. There is scarcely a single orchid grower who has not got his houses full of seedlings. We are all of us, I think, in that position, and we are very often disappointed. I refer to this matter, not to magnify or to lay any great emphasis on the cultivation of orchids in particular, but to show how greatly we shall have to depend upon plant-breeding for our future supplies of these very popular and useful plants.

We are promised many important and interesting papers, and the Royal Horticultural Society begs beforehand to tender its hearty thanks to those who have been kind enough to undertake to prepare them. We know of what a high value they must be by the eminent names attached to them. It is, of course, the intention of the Society to publish all the papers that will be read in a separate volume, distinct from its usual *Journal* and *Transactions*. Our very able Secretary, Mr. Wilks, will be the editor of the volume, carrying on the work that he did so admirably at the close of our first Conference on Plant-breeding in the summer of 1899.

I thank you for listening so patiently to what I have had to say, and I extend to you the most hearty greeting and good wishes from the Council of our old Society. I thank you, too, Ladies and Gentlemen, for being present this evening to grace the opening ceremony of our Conference, but most especially I thank our foreign guests for leaving their far-distant homes and coming over here, at no trifling inconvenience and expense, to assist us in our joint deliberations on Hybridisation and Plant-breeding.

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The following were some of the most interesting exhibits shown at the conversazione :—

Plants exhibited by Miss E. R. Saunders, Newnham College, Cambridge.

1. *Lychnis vespertina*, type and var. *glabra* ; crossbreds (F_1), and their offspring (F_2).

To show a simple Mendelian case where the parents differ in respect of *one* character (surface character).

2. *Datura Tatula*, type and var. *inermis ; D. Stramonium*, type and var. *inermis* ; crossbreds (F_1), and their offspring (F_2).

To show a simple Mendelian case where the parents differ in respect of *two independent* characters (flower-colour, and fruit character).

3. *Salvia Horminum* (*a*) *violacea*, (*b*) *rosea*, (*c*) *alba* ; crossbreds (F_1), and their offspring (F_2).

To show a simple Mendelian case where the parents differ in respect of *one* character (flower-colour), which is determined by three distinct factors.

4. Stocks, *Matthiola incana*, *M. sinuata*, wallflower-leaved ten-week varieties ; crossbreds (F_1), and their offspring (F_2).

Some of the Guests at the Conversazione.

To show results obtained in a case where the parents differ as regards two features (surface character and flower-colour), which are dependent on the inter-relationships of *several* factors.

5. *Ranunculus arvensis* (*a*) type, (*b*) *tuberculatus*, (*c*) *inermis*.

A case where prickliness in the fruit is dominant over the unarmed or partially armed condition.

6. *Mercurialis annua*.

A case where parthenogenetically developed individuals are all female.

* * *

Exhibit of Experiments in Cross-fertilisation of Varieties of Antirrhinum majus by Miss M. Wheldale, Newnham College, Cambridge.

The experiments were made with a view to investigating the laws of inheritance of the flower-colour. Specimens were shown of the types of the original parents and of those occurring in the first and second generations from some dozen or more crosses.

The various colours exhibited by the corolla are due to various combinations of several pairs of Mendelian characters, and the proportions in which the types appear experimentally have been found to agree closely with the proportions calculated theoretically.

* * *

Plants and Seeds exhibited by R. H. Lock, Caius College, Cambridge.

1. Pea plants illustrating the characters used by Mendel in the original experiments which led to the discovery of his law.

2. Specimens illustrating the inheritance of the colour of the testa of peas. Three pairs of allelomorphs are concerned :

The presence and absence of a grey colour, C and c
The presence and absence of purple spots, P and p
The presence and absence of "maple" marks, M and m

P and M only lead to the development of a visible character when they occur in the same zygote with C.

3. Seeds of *Canavalia ensiformis*. A large number of new types appeared in F_2 from the cross red × white. The inheritance of these has not yet been worked out.

4. Specimens of maize illustrating Mendel's laws of inheritance. The majority of specimens show the result of crossing the crossbred form (heterozygote) with one of the parents (recessive). In most cases the two parental types of grain make their appearance in equal numbers under these circumstances.

* * *

Exhibit of Hybrid Wheats and Barleys by R. H. Biffen.

Beardless × Bearded Wheat.

F_1. Beardless.
F_2. Beardless : Bearded : : 3 : 1.
F_3. Beardless breeds true once in three times, bearded always.

Lax Ears × Dense Ears.

A series of hybrids between Club wheats and various lax-eared varieties.

The hybrid is in each case intermediate between its parents in respect to the denseness of the ear.

Where the ears are partially bearded such awns may be found in beardless parent.

Club Wheat × New Era.

The essential difference between the parents is that one has a dense and the other a lax ear.

The F_1 plants are intermediate in this respect, and in the following generation dense, intermediate, and lax forms are produced in the proportion of 1 : 2 : 1.

Amongst these are individuals which are denser than the parent.

Rough Chaff × Golden Drop.

Rough chaff, white with a felted chaff. Golden drop, red with a smooth chaff.

F_1, chaff felted, colour red. Segregation in F_2 normal, giving nine rough red, three rough white, three smooth red, and one smooth white.

Triticum polonicum × T. turgidum.

The F_1 has a rough chaff with a shade of grey colour. The glumes are intermediate in size between the parents, and the time of ripening is again intermediate. The ears are about as lax as those of *T. polonicum.*

F_2 forms with long, intermediate, and short glumes in the proportion of 1 : 2 : 1. Time of ripening same ratio. Rough chaff 3 : 1, but confined to the intermediate and short-glumed individuals. All plants of the F_2 are white in colour.

Club Wheat × 'Tasmanian.'

Club wheat, bearded, dense and smooth chaffed; Tasmanian lax, beardless, and rough-chaffed.

F_1, intermediate in denseness, rough-chaffed, and beardless.

F_2, three rough to one smooth; beardless to bearded in the same proportion; dense, intermediate to lax as 1 : 2 : 1.

dense
intermediate } rough or smooth, beardless or bearded.
lax

T. turgidum × T. Spelta dicoccum.

T. turgidum, chaff rough, grey, rachis tough; plants six feet high.

T. dicoccum, chaff smooth, white, rachis brittle, and spikelets closed; plants less than three feet high.

F_1, similar to *T. turgidum* except that the rachis is more brittle and the spikelets closed.

The F_2 generation has still to be examined statistically. The specimens shown represent the types occurring.

T. turgidum × *T. vulgare* (vars.).

T. turgidum, ears dense, bearded, felted, grey, glumes keeled to the base; *T. vulgare*, lax, beardless or with traces of awns, smooth, red, glumes rounded at the base.

F_1 shows dominance of lax, beardless, felting, keeling of glumes. The colour is not so pronounced a grey as that of the parent.

F_2 shows the 3 : 1 ratio for the characters given above and occasionally an intensification of such characters as laxness and denseness, colour, and certain ill-defined straw characters. In addition, types very similar to *T. Spelta* occur. These latter breed true to the spelt characters.

Inheritance of Immunity and Susceptibility to the Attacks of Yellow Rust (Puccinia Glumarum).

POT CULTURES.

Susceptible and immune parents in the same pot. In the next, grown under the same conditions, a number of F_2 plants, some of which have become diseased, others which are disease-free. In the field trials F_1 was susceptible, and in the F_2 the ratio of susceptible to immune was as 3 : 1.

Inheritance of Susceptibility and Immunity to the Attacks of Yellow Rust (Puccinia Glumarum).

Specimens taken from a field culture of the first generation from the hybrid Susceptible × Immune.

Parents: Immune variety with dense ears, susceptible with lax ears.

Immune and susceptible types of both varieties as they appear in the F_2.

On the right, plants for most part nearly killed. Left, vigorous plants with rust-free straw.

Hordeum deficiens × *H. Steudelii.*

The parents differ simply in colour, one being white the other black.

F_1 shows dominance of the black colour.

F_2, segregation into black and white in the proportion of 3 : 1.

No intermediate shades occur.

H. abyssinicum × *H. Steudelii.*

The chief difference between the parents is that one has broad, the other narrow glumes.

The narrow type is the dominant one.

H. nutans × *H. distichon.*

The one parent has lax ears, the other dense (narrow or wide).

The F_1 has lax ears, very similar to those of *H. nutans*.

F_2. This generation consists of obviously lax and dense ears, together with a series which cannot be placed in either group. By matching against the parents it is impossible to separate the individuals into lax, intermediate, and dense. In the F_3 the extreme lax or dense ears are found to breed true, whilst the intermediates break up into the three forms again. A series of sowings shows that these three types probably occur in the ratio of 1 : 2 : 1.

H. nutans × *H. deficiens.*

In *H. nutans* the lateral florets are staminate, and in *H. deficiens* sexless.

F_1. Lateral florets small and sexless, but more developed than in *H. deficiens.*

F_2. Segregation into three groups with staminate, small and rudimentary lateral florets in the proportion of 1 : 2 : 1. The forms with the small laterals again split in the next generation.

H. parallelum × *H. decipiens.*

In *H. parallelum* the lateral florets of each triplet are hermaphrodite and set grain. In *H. decipiens* the median floret only is hermaphrodite and the lateral florets are rudimentary.

F_1. The lateral florets are staminate, as in the group *H. distichon.*

F_2. Fertile lateral florets, staminate and rudimentary occur in the proportion of 1 : 2 : 1.

F_3. The forms with the fully fertile and those with rudimentary florets breed true, whilst those with staminate laterals give the same types as before.

H. vulgare × *H. Steudelii.*

Parents: White, lateral florets fertile. Black, lateral florets sterile.

F_2:—

Black, with fertile laterals, 24
White, with fertile laterals, 6
Black, with sterile laterals, 71
White, with sterile laterals, 19

H. nigrescens × *H. æthiops.*

H. nigrescens, lateral florets male only; bearded. *H. æthiops,* lateral florets hermaphrodite and fertile; hooded.

F_1. Lateral florets larger than those of *H. nigrescens* and frequently fertile (dependent on supply of nutriment?); hoods on short awns.

F_2. Hooded and awned six and two row types with a series of individuals with occasionally fertile lateral florets. The extent to which these set grain is very variable. In the next generation these all prove to be heterozygotes. Six row, heterozygote, two row as 1 : 2 : 1.

H. ianthinium × *H. utriculatum.*

H. ianthinium, awned, purple; *H. utriculatum,* paleæ trifurcate (hood), white.

F_1. Purple; the colour is most marked before the ears are dead ripe: hooded, the hoods sessile.

F_2. Segregation into the expected types :

Hooded, purple.
Awned, purple.
Hooded, white.
Awned, white.

H. Schimperianum × *H. nutans.*

H. Schimperianum, lateral florets hermaphrodite, paleæ black.

H. nutans, laterals staminate, paleæ white.

The black colour shows simple dominance. The F_1 has a few fertile laterals. In the F_2 there were forty-nine individuals with fertile laterals, forty-six with staminate laterals, whilst the remainder, ninety-nine in number, bore an occasional fertile lateral floret. Twelve sowings of this last type have all proved heterozygotes.

H. decorticatum × *H. densifurcatum.*

H. decorticatum, lateral florets rudimentary with no sexual organs.

H. densifurcatum, lateral florets hermaphrodite, paleæ hooded.

F_1. Lateral florets staminate, hoods more or less sessile.

F_2. Hermaphrodite : staminate : sexless laterals :: 1 : 2 : 1.

Hooded and awned types of all three in the proportion of 3 : 1.

H. spontaneum × *H. hexastico-furcatum.*

In *H. spontaneum* the rachis is brittle : laterals staminate, paleæ awned. F_1 with staminate laterals, hooded paleæ, and a brittle rachis.

In the F_2 tough and brittle rachises on six rowed or two rowed ears with awns or hoods.

(The rachis is so brittle that only isolated spikelets could be shown.)

* * *

SPECIMENS EXHIBITED BY DR. JOHN H. WILSON, St. Andrews University, Scotland.

1. A series of mounted specimens of crossed wheats to show Mendelian segregation. The seed-parent was Red King (Garton's) and the pollen-parent Rood Koren. The former is awnless, the latter awned. The specimens illustrated the established fact that the awned character is recessive.

2. An ear of a new hybrid oat Goldfinder × Potato Oat, to show enhanced vigour characteristic of many hybrids.

3. Pods of peas, viz. Gradus, Sugar Pea, and reciprocal hybrids of the first generation. The hybrid pods, still green, were dissected to demonstrate the fact that the inner membranous lining present in the pod of Gradus and absent in the Sugar Pea, was present in both hybrids.

4. *Digitalis lutea* × *D. purpurea*, *D. lutea* × *D. purpurea alba*, and *D. purpurea alba* × *D. lutea*. These specimens showed that the hybrids having the white foxglove as a parent were more vigorous than those having the purple foxglove, and further that the reciprocal crosses between the white foxglove and *D. lutea* differed in respect of form and colour of the flower.

5. A red garden variety of tuberous begonia × *B. coccinea*, the plant eight years old, showing the habit of growth partaking of both tuberous and shrubby characters. This hybrid has never flowered.

B. foliosa × *B. fuchsioides.*—The reciprocal hybrids are alike. The progeny of this cross to show that Mendelian segregation does not occur.

B. vitifolia × *B. sanguinea* and the reciprocal cross to show the great similarity of the reciprocals.

B. Dregei × *B. heracleifolia.*—This hybrid refuses to flower.

B. heracleifolia × *B. coccinea.*—An infertile hybrid.

B. hydrocotylifolia × *B. coccinea*, also infertile.

6. *Centaurea ragusina candidissima* × *C. Scabiosa.*—Flowers freely, but is infertile.

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Exhibit of Hybrid Pineapples by W. Fawcett, Director of Public Gardens, Jamaica.

Coloured drawings of several hybrids were shown. These drawings had been made by Miss H. A. Wood at Hope Gardens, Jamaica, where the pineapples were grown.

This exhibit was shown as an indication of one of the experiments in hybridisation and plant-breeding which are being carried out in the Botanic Gardens, Jamaica. The object of this special experiment was to get an improved variety of pineapple. The drawings showed crosses between the 'Ripley' and the 'Smooth Cayenne.' The 'Ripley' is a small fruit with an insignificant top, but of very fine flavour. The 'Smooth Cayenne' is a larger fruit with a fine top and no prickles on the edges of the leaves, but the flavour is not nearly so good as that of 'Ripley.' The drawings showed some of the results attained. The flavour of all the hybrid fruits was excellent. The leaves and tops showed various intermediate stages between the two parents, and one drawing was very remarkable, showing *that the fruit had neither top, nor slips, nor suckers*, but it is said to have had a very fine flavour.

Fig. 5 illustrates the method of cross-pollination.

No. 1. Flower-head with a few open flowers (nat. size). The corolla has been removed from one of these, and the stamens pinched off with the foreceps. The same thing is done to the variety with which it is to be crossed, but the stamens of the flowers of *this* plant are held with the forceps and the anthers brushed against the stigma of the first so that the pollen adheres to it. (A shows this process more clearly).

No. 3. Enlarged flower with surrounding bracts.

No. 4. Petal, pistil, and stamen—natural size.

No. 5. Pistil and stamen with vertical section of ovary—much enlarged.

No. 6. Transverse section of ovary—much enlarged.

No. 7. Vertical section of ovary, greatly enlarged, showing attachment of ovules.

A. The process of cross-pollination.

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Fig. 5.—The Cross-pollination of Pineapples.

EXHIBIT OF ROSA FOLIOLOSA BY MAURICE DE VILMORIN, Paris.

Rosa foliolosa of Nuttall is a wild species in Arkansas, Texas, &c., and has been too much neglected by horticulturists. It presents very distinct features—a small size, very thin wood, very elegant little foliage, with numerous and glossy leaflets. It is very late flowering and continues

FIG. 6.—ROSA FOLIOLOSA, NUTT.

to bloom all through the autumn, showing bright rosy-white flowers, bearing at the same time both green and red fruits with fine fimbriated persistent sepals. The defect of its sending out so many suckers may be corrected by budding it on the collar of some other rose. (Fig. 6.)

But it is chiefly for hybridising purposes that *Rosa foliolosa* ought to be interesting. Crossed with some dark-coloured variety of *Rosa indica* it would probably give some very good results. I successfully tried its hybridisation with the red *Rosa rugosa*, and the result is a bush some three feet high, broadly pyramidal, with plenty of blossoms of a light yellowish-pink, produced from July to the frosts. Many other combinations might be tried.

* * *

SOME DETAILS IN THE HISTORY OF THE CROSS-BREEDING OF THE POTATO FROM THE YEAR 1876 UP TO THE PRESENT TIME WITH THE WILD "SOLANUM FENDLERI" (?), SENT TO MR. FENN FROM GUADALJARA IN NEW MEXICO BY MR. C. G. PRINGLE IN 1874. By ROBERT FENN, V.M.H., SULHAMSTEAD, READING.

In 1874 Lord Cathcart's potato 'Magna' was fertilised with pollen from 'Magnum Bonum.' A seedling was obtained from this cross and was called 'Antagonist' on account of its being at once put into a competitive trial with the Scotch 'Champion,' which it beat in respect of both crop and disease-resisting power.

In 1876 *S. Fendleri* was fertilised with pollen from 'Antagonist.' This experiment was repeated for no less than seventeen years and a seedling at last obtained.

This seedling was fertilised with pollen from 'Rector of Woodstock,' which had received a First-class Certificate from the R.H.S. The result of this cross (S_2) was very encouraging, though not yet considered sufficiently good for commerce.

In 1896 S_2 was fertilised with pollen from 'International Kidney.' This again succeeded, and in August 1897 a number of resultant tubers (S_3) were lifted, showing a distinctly progressive result.

S_3, however, still throws out too wild-long stolons, bearing the tubers too far away from the parent plant. This tendency has still to be corrected by crossing them again, and perhaps again, and perhaps yet still again.

[Mr. Fenn, though now over ninety years of age, still hopes to live to do this work, as he considers that the crossing of potatos from the North American continent with those from South America ought to prove very valuable by giving us a new strain—new blood, as it were—from a new latitude.

The *Solanum Fendleri* referred to by Mr. Fenn is almost certainly not the true *S. Fendleri* Heurck and Muell. Arg. from the region of Panama, but *S. tuberosum* var. *boreale* Gray (once published by Dr. Asa Gray as *S. Fendleri*). But Mr. Fenn's plant, although thus reduced to a simple northward variety-extension of *S. tuberosum*, and not a distinct species, would bring in "new blood" all the same, from the mere fact of its having been established for unknown ages in its northern habitat.—ED.]

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EXHIBIT OF PEAS BY ARTHUR W. SUTTON, V.M.H., READING.

1 Seeds of the pure wild pea collected in Palestine.
2. A most interesting plant raised from one of the seeds.
3. Plants of hybrid peas.

* * *

Exhibit of Plants by William Laxton, Bedford.

1. Japanese plum × 'Moorpark' apricot.
2. Japanese plum × 'Sea Eagle' peach.
3. *Clematis Jackmanni* × *C. Flammula.*
4. The Loganberry × Raspberry.

These plants are described and some of them figured in a paper by Mr. Laxton, which will be found further on.

* * *

Photographs of Sheep, showing the Inheritance of Horns and Face-colour. Exhibited by T. B. Wood, M.A., Cambridge University Department of Agriculture.

The photographs illustrated the following points observed in cross-breeding sheep at the Cambridge University Farm.

Breeds under experiment:—

Dorset.—White faces, and horned in both sexes.

Suffolk.—Black faces, and hornless in both sexes.

These two breeds were crossed both ways, and the reciprocals were found to be identical.

Horns.—In F_1 all the males were horned, all the females hornless.

In F_2, males, the large majority were horned, a few were hornless, and a number possessed rudimentary horns.

Females, the majority were hornless, a few were horned, and one showed rudimentary horns.

Face-colour.—In F_1 all the sheep of both sexes had faces evenly speckled with black and white.

In F_2 a few had pure-white faces, a few pure-black faces, and the large majority speckled faces. Most of these latter had the two colours evenly distributed, as in F_1, but in some there was a marked tendency for the black colour to confine itself to the tip of the nose, to rings round the eyes, or to both these regions.

Both the characters appear to be inherited according to Mendel's laws. Horns appear to be dominant in the male, recessive in the female.

The face-colour of the first generation is intermediate between that of the parents, but in the second generation small numbers of the two pure colours split out, the majority being intermediate, as would be expected. The occurrence of special "patterns" seems to indicate that face-colour is not a simple character.

* * *

Exhibit of Breeding Experiments with Lepidoptera and Rats by L. Doncaster, M.A., King's College, Cambridge.

Lepidoptera: In *Angerona primaria* and its variety *sordiata*, the variety *sordiata* is dominant over the type, but the heterozygous form is distinguishable from the pure dominant. The later generations exhibit ordinary Mendelian segregation.

In *Abraxas grossulariata* the type is dominant over the variety *lacticolor*, but there is a coupling of the recessive variety with the female sex, so that among the offspring of first crosses all the recessive are female.

Rats: Grey colour is dominant over black, and colour over albino. "Self-colour" is partially dominant over piebald. The determinants for grey or black, and for "self" or piebald, may be borne by albino, so that an albino bearing "self" and grey, paired with a black piebald gives offspring which are grey and self-coloured.

* * *

Hybrid Ducks bred and exhibited by J. L. Bonhote, M.A., F.L.S., Hemel Hempstead.

One of the objects of these experiments has been to test the fertility of the hybrids of pure species, and it has been found that crosses between as many as five different species are all fertile.

Two *main* points may be noticed:

(1) That the various crosses tend to split into two well-marked forms, a light and a dark.

(2) That the second and third generations become much lighter in coloration, and the drakes tend to lose their bright colours.

Other points of interest are:

(1) That resemblances are shown to the plumage of one parent in the winter dress, and to the other parent in the eclipse plumage.

(2) That all variations seem to follow certain definite lines or tracts known as "pœcilomeres," thereby showing resemblance to species other than their parents or to no known species.

A *résumé* of an exhibition to the Zoological Society was laid on the table for those interested, and a full account will shortly appear in the *Proceedings of the Fourth International Ornithological Congress.*

Pœcilomeres have been dealt with in a paper to the Linnean Society (*Proc. Linn. Soc. Zool.* 1904, p. 95) and more fully in *Knowledge* (Dec. 1905, Jan.–April 1906).

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Specimen showing Results of Experiments on Heredity in Pigeons by Richard Staples-Browne, Cambridge.

Blue colour as found in *Columba livia* is shown in the F_2 generation, both in crosses between black and white birds, and also in the cross between a white fantail and a white tumbler.

Series I.

skins showing results of crossing white fantail and white tumbler.

Type I.—*Pure-bred white fantail.*

Type II.—*Pure-bred white tumbler.*—The breeder of this bird has had a strain of white tumblers in his possession for twenty years. About fifteen years ago he introduced two white hens with red splashes, and since then has never used any but white birds, and only pure-white birds have been selected for stock. The strain occasionally throws birds showing coloured splashes, the coloured feathers being red, brown, or black.

Type III.—White cross-bred tumbler fantail. No coloured feathers.

Type IV.—White bird splashed with red feathers on the neck and back.

Type V.—Another splashed bird showing a black feather at the root of the tail.

Type VI.—Bird with much red on neck, breast, back, and wing coverts. Also bluish tinge on the back. Patches of blue feathers very clearly seen at the root of the tail and on the right thigh.

Type VII.—Much colour as in Type VI., but many of the red and blue feathers, especially on the back, are heavily chequered with black.

Type VIII.—White bird splashed with reddish feathers chequered with black on the back. Also shows one blue tail feather with indications of a terminal black bar.

Table showing Results of the Matings.

(Types of Offspring.)

Generation of parent	Type of parent	Generation of offspring	Type III	Types IV and V	Type VI	Type VII	Type VIII
Pure	I. and II.	F_1	6	3	0	0	0
F_1	IV. and V.	F_2	4	4	2	0	0
F_2	V.	F_3	3	0	4	1	0
F_2	IV. and V.	F_3	4	4	0	0	1

Series II.

SKINS SHOWING RESULTS OF CROSSING BLACK BARBS WITH WHITE FANTAILS (EXPERIMENTS STILL IN PROGRESS).

Original birds used :

White Fantails (see skin in Series I.).

Black Barbs.—Plumage black, no white feathers. (No skin available, as birds still being used in experiments.)

Original Matings.—(*a*) White fantail ♀ × black barb ♂ ; (*b*) black barb ♀ × white fantail ♂. Both the barbs and the fantails used in original crosses were obtained from different sources.

F_1 Generation.—From mating (*a*) five birds. Black with few white feathers (Skin I.). From mating (*b*) four birds. Black with few white feathers (Skin II.).

Matings of F_1 Birds together.—(*c*) ♀ and ♂ from mating (*a*) ; (*d*) ♀ and ♂ from mating (*b*) (two years) ; (*e*) ♀ from mating (*a*) ♂ from mating (*b*).

F_2 Generation.

Matings	Blacks	Blacks with few white feathers	"Blues"	"Blues" with few white feathers	Reds	Whites with few coloured feathers	Whites
c	0	0	1	2	0	0	1
d	4	6	0	0	5	2	3
e	1	4	1	0	0	2	2
Totals	5	10	2	2	5	4	6

NOTES ON BIRDS OF F_2 GENERATION.

Blacks (no skin shown, as birds still being used in experiments).—Similar extracted blacks in F_2 from a cross between an F_1 barb fantail and F_1 barb nun have bred true.

Blacks with few White Feathers (Skin III.).—A pair of these birds from mating (*d*) gave :—

Black.	*Black with white feathers.*	*Blue with white feathers.*	*White.*
1	3	1	1

The blue with white feathers is shown (Skin VII.).

"*Blues*" (Skin IV.).—The blue is seen chiefly in the tail, and the black tail-bar is well marked. These birds have been shown by experiment not to contain white.

"*Blues*" *with few White Feathers* (Skin V.).—These birds have been shown by experiment to contain white. Mating together the blues of the two types, gave blues with no white feathers, and blues showing several white feathers (Skin VIII.) in equal numbers.

Reds.—Red was contained in one of the black barbs used. These birds are being dealt with in a separate experiment.

Whites with few Coloured Feathers.—The coloured feathers may be black or red. Experiments are being made with them.

Whites (Skin VI.).—A white in F_2 mated with a white fantail gave all offspring white.

Skin IX. shows a blue containing white of the F_4 generation.

To test whether a bird contains white it is mated to a white fantail, and is found to give all coloured offspring if it contains no white, or coloured and white offspring in equal numbers if white is being carried.

Skins X., XI., and XII show the coloured offspring from the matings of the birds of the F_1, F_2, and F_3 generations respectively, with white fantails.

Similar experiments are now in progress on the mating of the blue crossbreds with blacks.

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Exhibits by C. C. Hurst, Burbage.

Horses.—Coloured drawings of bay, brown, and chestnut thoroughbreds to illustrate Mendelian dominance of bay and brown over chestnut, segregation of chestnut from bay and brown, and the purity of the extracted chestnuts (for details see *Proc. Roy. Soc.* B. vol. lxxvii. 1906, pp. 388–394).

Rabbits.—Specimen coat-skins of "Belgian Hare" and "White Angora" rabbits and their hybrid forms showing Mendelian dominance in F_1 of short over Angora, coloured over white, grey over black; segregation of these characters in F_2 in Mendelian proportions; purity of recessives, purity and impurity of dominants in F_3; also illustrating the fact that certain albinos may carry factors for coat colour and coat pattern, which characters only become visible when the albinos are mated with coloured animals (for details see *Journ. Linn. Soc.* [Zool.] vol. xxix. 1905, pp. 283–324). A photograph of the above exhibit is published with the Hybrid Conference paper.

ORCHIDS.—Photographs of flowers of forms of *Paphiopedilum* × *Hera* showing segregation of spotted sap pattern and striped sap pattern (for details see JOURN. R. H. S. 1903, vol. xxvii. pp. 614–624). These photographs are reproduced with the Hybrid Conference paper.

ANTIRRHINUMS.—Specimen flowers of the dwarf races, 'Crimson King,' 'Yellow Prince,' and 'White Queen,' and their hybrid forms, showing Mendelian dominance in F_1 of red over yellow, red over white, and white over yellow corolla segments ; segregation of these characters in F_2 in Mendelian proportions ; also illustrating the compound nature of the red colour of 'Crimson King' *i.e.* red based on yellow (for details see Hybrid Conference paper).

TOMATOS.—Specimens of tomato fruits showing Mendelian dominance in F_1 of red over yellow flesh, and yellow over white skin ; Mendelian segregation in F_2 into 9 red flesh in yellow skin : 3 red flesh in white skin : 3 yellow flesh in yellow skin : 1 yellow flesh in white skin ; also illustrating the compound nature of the red colour of the 'Fireball' tomato, *i.e.* red flesh in a yellow skin (for details and photograph of the exhibit see the Hybrid Conference paper).

SWEET PEAS.—Specimen flowers of 'Black Knight,' 'Sadie Burpee,' 'Pink Cupid,' 'White Cupid,' 'Salopian,' 'Dorothy,' and their hybrid forms, showing Mendelian dominance in F_1 of red over white, purple over red, tall over cupid, long over round pollen ; segregation in F_2 in Mendelian proportions and purity of recessives in F_3.

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EXHIBIT OF PLANTS BY R. IRWIN LYNCH, V.M.H., Botanic Garden, Cambridge.

This was an exhibit of great general interest and suggestiveness, but was not intended to illustrate Mendelian laws. Among the plants shown were two remarkable hybrid ferns, one, *Polypodium Schneideri*, between *P. vulgare elegantissimum* and *P. aureum*, the other, *Scolopendrium hybridum*, between *S. vulgare* and *Asplenium Ceterach*, the first being undoubtedly hybrid and easily obtained by making a mixed sowing of the spores, and the second certainly showed strong evidence of hybrid character; see also p. 50. A very good *Nepenthes* hybrid was shown in *N. Allardi*, raised between *N. Veitchii* and *N. Curtisii* by Mr. Allard, foreman in the Cambridge Botanic Garden, which, though very near to one or two similar hybrids, is still quite distinct.

Mr. Lynch also showed the very remarkable *Kalanchoë kewensis*, raised at Kew between *K. flammea* and *K. Bentii*. A hybrid in the same genus, but of less importance, raised in the Cambridge Botanic Garden between *K. grandiflora* and *K. Kirkii*, was shown as *K. cantabrigiensis*. A hybrid *Sarracenia*, also raised in the same garden, by the Curator, between *S. Drummondii* and *S. variolaris* was of horticultural interest because of its fine size and colour, resembling a gigantic *S. variolaris*, with the fine coloration of a good form of *S. Drummondii*. From Professor Macfarlane we learn that some at least of these garden hybrids are found wild in nature. *Begonia weltoniensis*, raised many years ago by the late Colonal Trevor Clarke, was shown as having a colour apparently not possessed by either parent. The explanation, however, is simple, if we may understand that the yellow of the orange *B. Sutherlandi* is dropped by the influence of the white *B. Dregei*, leaving the red. Another instance occurs in *Rhododendron*.

Showing how readily some quite distinct species may combine in one, a plant of Cineraria was exhibited in which, by means of different crossings, were combined the common garden Cineraria and all the allied species cultivated at different times in the Cambridge Botanic Garden. The last Cineraria cross, recently made, bringing an unnamed species into the combination, will probably be as fertile as the previous crosses have been.

One of the most interesting plants botanically was a cross between *Senecio vulgaris* and *S. squalidus*, which is now established as a weed in the Cambridge Botanic Garden. It came from near Cork, where it is found wild. The "graft hybrid" *Cytisus Adami* was shown with two of the three forms on one branch. It is understood to have been produced by grafting *Cytisus purpureus* on the common Laburnum. Both these species break out separately on the tree, apparently pure, with a third form regarded as the "graft hybrid." Specimens of Laburnum and *C. purpureus* were shown pure, raised from seed gathered from corresponding parts of the tree. Other plants shown were *Aloe Lynchii*, a bigeneric hybrid between *Gasteria verrucosa* and *Aloe stricta*, and also a cross made both ways between *A. somaliensis* and *A. oligospila*, no difference being evident between the plants whichever way the cross was made. Also in this group were *Hypericum Moserianum* (*H. calycinum* × *patulum*) and hybrids of *Clematis coccinea* with one of the older Clematis hybrids, viz. 'Star of India.'

* * *

Exhibit of Heredity in Snails by Professor Lang, of the University of Zürich.

Professor Lang, of Zürich, very kindly sent over the results of his breeding experiments with *Helix hortensis* and *Helix nemoralis*.

The examples sent illustrated two chief experiments. First, the result of crossing banded with unbanded individuals of *Helix hortensis*; and secondly, of a cross between *Helix hortensis* and *Helix nemoralis*.

The results of the first experiment could be described in terms of a Mendelian formula, inasmuch as they exhibited the phenomena of dominance and segregation.

The results of the second experiment were exceedingly interesting, partly because they consisted in the production of a hybrid between two forms that are universally recognised as distinct species and partly because the knowledge obtained by the experiments has a strong bearing on the interpretation offered by Contagne of a certain state of affairs that he observed in the field.

If the reader desires further information on this case the two following references, the first to Prof. Lang's work and the second to an abstract in English and a review of it, will be useful to him:—

Lang. A. "Ueber Vorversuche zu Untersuchungen über die Varietätenbildungen von *Helix hortensis* Müller und *Helix nemoralis L.*," *Festschrift zum siebzigsten Geburtstage von Ernst Haeckel*, Jena, 1904, p. 439.

Darbishire, A. D. "Professor Lang's Breeding Experiments with *Helix hortensis* and *H. nemoralis*: an Abstract and Review." Journal of Conchology. July 1905.

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EXHIBIT OF WALTZING MICE BY A. DARBISHIRE, of the Royal College of Science. This is referred to in his paper, which will be found further on.

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EXHIBITS OF PRIMULAS, BRYONY, SWEET PEAS, AND THE COMBS OF FOWLS BY W. BATESON, F.R.S., R. P. GREGORY, AND R. C. PUNNETT.

Some seedling Primulas were exhibited by Mr. Bateson and Mr. Gregory, illustrating that in one dozen plants there are nine palm-leaved (dominant) and three fern-leaved (recessive). This proportion appeared in the F_2 (second generation) from a cross effected between the types. It may be remarked that the fern-leaved type (recessive) breeds true.

On the subject of inheritance of sex some plants of *Bryonia alba* and *B. dioica* were shown to illustrate and confirm the experiments of Correns. *Bryonia alba* is monœcious and *B. dioica* is diœcious. Between these species reciprocal crosses give dissimilar results. *B. alba* ♀ × *B. dioica* ♂ gives ♂'s and ♀'s in equal numbers, but *B. dioica* ♀ × *B. alba* ♂ gives only plants which are ♀ except for rare ♂ flowers at the bases of the stems. All these hybrids so far have been totally sterile.

The rules of heredity were further illustrated by Mr. Bateson and Mr. Punnett in an elaborate series of experiments on the sweet pea. White sweet peas when "selfed" breed true. When, however, a cross is made between certain strains of whites, all the offspring are purple; such purples on "selfing" give purples, reds, and whites in the proportions of 27 : 9 : 28. On raising a subsequent generation from these F_2 plants, the twenty-seven purples are found to consist of four different kinds, namely—

(*a*) Those giving purples, reds, and whites, sixteen in number.
(*b*) Those giving purples and whites, eight in number.
(*c*) Those giving purples and red, two in number.
(*d*) Those giving purples only, one in number.

Similarly among the nine reds there were :—

(*a*) Eight giving red and whites.
(*b*) One giving reds only.

(These reds never give a purple, and the whites always breed true.)

The chance of a pure purple coming in the F_2 (second) generation is therefore 1 in 27, and of a pure red is 1 in 9. Moreover the composition of each plant is shown by its offspring. Consequently, by saving from individuals which are thus proved to be pure, these types may at once be fixed.

A curious case of reversion was illustrated in a cross between 'White Cupid' (round pollen), a dwarf plant of procumbent habit, and 'White Bush' (long pollen), a variety growing 3 feet high. The hybrid was very much taller than the taller parent and had purple flowers, being, therefore, a reversion both in height and colour.

Dominant and recessive characteristics in fowls' combs were brought out in a series of crosses made by Mr. Bateson and Mr. Punnett, and

FIG. 7.—PRIMULA DEORUM.

the Mendelian principles of inheritance were further illustrated by experiments upon the plumage colour of Rosecomb bantams and Andalusian fowls, as well as upon the colour of the down in game bantams.

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Exhibit of Rare Plants by W. T. Hindmarsh, F.L.S., Alnwick.

Mr. Hindmarsh sent magnificent illustrations of the four following plants, which though not hybrids excited great interest amongst all who saw them :—

1. *Primula deorum*, a very rare and beautiful primrose from Bulgaria, the blossoms of a very rich purple-violet and leaves of deep bluish-green. (Fig. 7.)

2. *Shortia uniflora*, a rare plant from Northern Japan with wax-like blush-coloured flowers and dull green leathery leaves having very

Fig. 8.—Shortia uniflora.

prominent veins of a lighter shade. The leaves from August onwards turn a most brilliant and beautiful red, veined and shaded. It must not be confused with the more often met with *S. galacifolia*, which, although it resembles *uniflora* closely, is a less beautiful plant. (Fig. 8.)

3. *Rhodothamnus Chamæcistus*, a plant very closely related to *Rhododendron* and not essentially rare, being a native of the Tyrol, but exceedingly rare in the wonderfully floriferous condition which fig. 9 shows, more than 1,000 blossoms, of a rosy-pink colour, being open at one time. (Fig. 9.)

4. *Eremurus Elwesii*, probably the finest of all the *Eremurus*, and apparently about midway between *E. robustus* and *E. himalaicus*, but without any sign of being actually a hybrid. It is of a beautiful flesh-

pink colour, deeper than those of *E. robustus*, and, as may be seen from fig. 10, of wonderful vigour and strength when it gets into a suitable

FIG. 9.—RHODOTHAMNUS CHAMÆCISTUS.

spot. In 1906 Mr. Hindmarsh's plant sent up no less than seventeen of its huge spikes of blossom.

Exhibit of a Hybrid Fern by Charles T. Druery, V.M.H.

Mr. Druery exhibited *Scolopendrium hybridum*, presumed to be a natural hybrid between *S. hemionitis* or *S. vulgare*, and *Ceterach officinarum*, as it partakes of the characteristics of both the suggested

Fig. 10.—Eremurus Elwesii.

parents. Only one plant was found on an old wall in a vineyard near Porto Zigale, in the island of Lossin (see "Britten's European Ferns," p. 137). Its spores, however, are perfectly fertile and reproduce the type exactly.

It is a peculiarly interesting plant. The fronds, six or seven inches long, are leathery and pinnatifid, with broad rounded lobes, merging into an almost smooth-edged, tapering terminal, strongly resembling *S. vulgare*, the pinnatifid portion equally resembling *C. officinarum*. The back of the fronds is slightly scaly, and the fructification, which is profuse, is sometimes single, of the Asplenium type, and sometimes in associated pairs exactly resembling *Scolopendrium*. The plants raised from its spores are freely produced, and despite its origin in the Adriatic have proved quite hardy under glass. Assuming it to be a hybrid, which can hardly be doubted, this fertility and constancy to type are very remarkable.

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EXHIBIT OF HYBRID HEMEROCALLIS BY G. YELD, Clifton Cottage, York.

Flowers of *Hemerocallis Thumbergii* (seed parent) and *H. aurantiaca* (pollen parent) were shown, with a number of seedlings from this cross. The flowers varied much both in shape and colour. Many of them showed a sort of halo round the interior of the blossom. It was with the intention of producing a flower of this appearance that the cross was originally made. A close observation of the blossom of *H. aurantiaca* reveals a suspicion of such a halo. This halo does not confine itself to the dark-coloured flowers, but appears in many of the lightest coloured. One dark-coloured self was effective, but perhaps the best, certainly the largest of all, was a bloom with more or less of the shape of *aurantiaca*, and a colour but little darker than that of *Thumbergii*.

THE CONFERENCE.

The First Session of the Conference took place under the presidency of Mr. W. Bateson, F.R.S., V.M.H., in the Society's Lecture-room, at 10.30 A.M., on Tuesday, July 31, and at 1 o'clock an adjournment took place, light refreshments being served by Messrs. Ring & Brymer both on Tuesday and Thursday in Committee Rooms No. I. and No. II.

LUNCHEON MENU.

TUESDAY, *July* 31. THURSDAY, *August* 2.

Salmon mayonnaise.
Lobster Patties.
Chicken Patties.
Torpilles aux pistaches.
Foie-gras Sandwiches.
Sandwiches of Ham, Tongue, Chicken, and Anchovy.
Fancy Biscuits.
Rout Cakes.
Fruit Salads and Cream.

Wines: Hock, Moselle, and Claret.

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The Second Session of the Conference assembled at 2.30 and adjourned at 5.30 P.M.

The Third Session of the Conference assembled at 10.30 on Wednesday, August 1, and adjourned at 12.15, the members of the Conference being conveyed by special train to the President's country seat at Burford, near Dorking.

The Fourth Session of the Conference assembled at 10.30 on Thursday, August 2, and adjourned at 1 o'clock for light refreshments, which were served in Committee Rooms Nos. I. and II.

The Fifth and last Session of the Conference assembled at 2.30 on Thursday, August 2, and adjourned at 5 P.M., the Society's Banquet taking place in the Great Hall at 7 o'clock.

DINNER GIVEN BY THE HORTICULTURAL CLUB.

The Horticultural Club most kindly invited all the foreign guests of the Society and as many of the British ones as their rooms would accommodate to a grand dinner at the Hotel Windsor, Victoria Street, at 7 P.M. on Tuesday, July 31, under the kind and genial presidency of Sir John T. Dillwyn Llewelyn, Bart., President of the Club.

TUESDAY, *July* 31, 1906.

MENU AND PROGRAMME.

Hors-d'œuvres variés.

Soup.
Thick Mock Turtle.
Clear Ox-tail.

Fish.
Turbot, Sauce Hollandaise.
Whitebait.

Roast.
Roast Sirloin of Beef.
Saddle of Mutton.

Game.
Roast Duck and Green Peas.
Salad.

Sweets.
Apple Tart.
Fruit Salad and Clotted Cream.

Ice.
Ice Pudding.

Savoury.
Haddock on Toast.

Dessert.

TOAST "His Majesty the King."

NATIONAL ANTHEM . . . . . . "God Save the King."

TOAST { Her Majesty the Queen, T.R.H. the Prince and Princess of Wales, and other Members of the Royal Family.

DUET . . . . "All's Well" (Braham)

TOAST The Royal Horticultural Society
Sir Daniel Morris, K.C.M.G., V.M.H.

SONG . . "Here's a health unto His Majesty" (Old English Song)

RESPONSE Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H.
President of the Society.

TOAST Our Guests
The Chairman.

SONG . . . . "Tom Bowling" (Dibdin)

RESPONSE W. Bateson, Esq., F.R.S., V.M.H., President of the Conference; M. Ph. de Vilmorin, Paris; Professor Wittmack, Berlin; Professor Johannsen, Copenhagen.

TOAST The Chairman
Lt.-Col. D. Prain, F.R.S.

DUET . . . "The moon hath raised her lamp above" (Benedict)

RESPONSE The Chairman

SONG . . . . "Simon the Cellarer" (Hatton)

SONG . . . . . . "Alice, where art thou" (Asher)

The tables were most beautifully decorated with superb flowers most tastefully arranged, and the quantity and beauty of the fruit were quite beyond description.

Dinner being ended, the Chairman, Sir John Llewelyn, Bart., rose to propose the first toast of the evening, "His Majesty the King." He said:—His Majesty is a great patron of horticulture and a man whom all Englishmen, and I fancy not a few foreigners, delight to honour. I give you "His Majesty the King."

The toast having been drunk with the greatest enthusiasm,

The Chairman again rose to propose the toast of "Her gracious Majesty the Queen, T.R.H. the Prince and Princess of Wales, and other members of the Royal Family." He said Englishmen always drank that toast with pride, for there was not a single member of the Royal Family who did not count it not only his duty but his pleasure as well, to do all in his power to help forward the happiness and prosperity of the whole empire.

Sir Daniel Morris, K.C.M.G., then proposed "The Royal Horticultural Society." He said:—Mr. Chairman and Gentlemen,—I feel it a very great honour that has been conferred upon me to propose the toast of the Royal Horticultural Society. Many years ago I was very closely connected with the Society, and I can assure you that to-night it is one of the greatest pleasures of my life to rise and speak on behalf of such a grand old Society and to congratulate the Fellows on the very great success that has been attained. It is many years since—I think it was in February 1888 that a meeting of the Royal Horticultural Society was held at the South Kensington buildings. At that meeting an election of officers took place, and our distinguished President, our indefatigable and worthy Secretary, and I myself were elected to be officers of the Society in possibly the very darkest period of its whole history—a period so dark and unpromising that I can assure you it required no little personal courage for any of us to accept the posts to which we were then elected. At that time we had no new Fellows coming in, only old Fellows leaving us at the rate of some hundreds every year. I well remember when I took over the office of Treasurer, the Society was in debt to the tune of not much less than £1,500. Our annual expenditure was something like £3,500, and that upon an income which was barely £2,000 a year! At that time, too, the Society really had no home whatever. It was being turned out of the home it had long had at South Kensington, and we did not know where in the wide world we were to go. I need not enlarge very much upon those trying years. I will only say this, that the history of the Society at that time of utmost depression and anxiety is an exact illustration of the truth of the old saying, that it is always darkest towards morning. New friends seemed literally to spring out of the ground, and came forward to help the old Society. We at once started in new offices—somewhere. For, in fact, at that time we had to give as our address—and our only address—"somewhere near the Army and Navy Stores." Very quickly afterwards we were fortunate enough to get a hall, "The Drill Hall"; and with our offices in Victoria Street, we gradually worked up the Society and began to improve the balance at the bank. I mention all this because from that fresh start in early 1888 we made up our minds most resolutely to devote all the energies of the reorganised

Society to purely horticultural matters. We determined to stand or fall by that principle; we refused even to consider the renewal of the Badminton and tennis courts and garden parties of South Kensington. We said to the Fellows in effect, that if they wished us to guide the destinies of the Society it must go back to its real aim and object—the promotion of true horticulture. You can easily understand that during the years when I was Treasurer of the Society, with such a decided change of policy we had many difficulties to contend with. Retrenchment was the order of the day. The Treasurer was obliged to call on his Finance Committee, consisting of Baron Schröder, our old friend Mr. Veitch, and others. We had to fight every step we took. But the real friends of horticulture—friends consequently of the new policy of the Society—combined in supporting us so heartily that in a very short time we began to see the old Society mend; and the prosperity and progress so founded and set going has continued, until at the present day the Society is fast approaching to 10,000 Fellows. I am also very glad to see that the income of the Society—I wish I were Treasurer now!—is something like £18,000 a year. The Society is entirely free from debt, and has nearly £20,000 invested. I remember well how that at one meeting of the Council we had a great discussion over the extravagance of spending half a guinea! and we really did not know what was to be done, as we felt we could not afford it. But that ever kind friend of the Society, Baron Schröder, said: "I will pay the ten-and-six myself so that the business of the Council can go on." Well, the alteration between then and now is really so great that I can hardly recognise the Royal Horticultural Society in its present magnificent condition. And when I mention the Hall, I can never do so without thinking again of Baron Schröder. He never ceased to urge upon us that we must have a Hall of our own. Gentlemen, we have got that Hall to-day, and I think we are all proud of it. I must also connect with any mention of the Hall the name of our worthy President, Sir Trevor Lawrence, who has never ceased to work for the Society, and has happily been able to continue to hold office all through this long period of growth. Nor can I pass on without paying a tribute of great affection, regard, and esteem for my worthy friend, the Secretary, who also began the reorganising work with me, and has never for a moment left it. No Society in the world has been so faithfully served as this Society has been by Mr. Wilks. We have got Chairmen and members of Committee, we have got Judges, we have got, in fact, one of the finest Societies existing in the whole world. I mentioned the officers, as the success of the Society is so largely attributable to their judgment, and upon the splendid work which they have done. I am glad to say there are many other features of the Royal Horticultural Society which deserve to be mentioned to-night. We have got our beautiful Garden at Wisley, and we have got the JOURNAL. I receive it regularly in the West Indies, and it is a journal any Society would be proud of. I do not know of any other journal connected with horticulture that has such a volume of information so carefully put together, and so attractive in its reading matter. I think the Royal Horticultural Society has done an excellent work in organising this Conference, which is sure to do so much good. In 1899 our Society held the first Hybrid Conference ever

Dinner given by the Horticultural Club to the Foreign Members of the Conference.

held in any part of the world. Unfortunately for myself, I was unable to be present, but I was at the Second Conference in New York in 1902, when I was glad to see that Mr. Bateson communicated to the people of the States the Mendelian theories. This Society sent over Mr. Bateson and Mr. Nicholson, and the people of the States were very pleased indeed, and very proud, in fact, that you sent over Mr. Bateson, because he did such an immense amount of good out there. They have now got a great American Society, and they attribute it to the Royal Horticultural Society, and to the presence of Mr. Bateson at that Second Conference. I give you the toast of the Royal Horticultural Society, and couple with it the name of its worthy President, Sir Trevor Lawrence.

Sir Trevor Lawrence, in responding, said :—Mr. Chairman, Sir Daniel Morris, and Gentlemen,—I can assure you, speaking on behalf of the Royal Horticultural Society, I have been very much interested in the reminiscences which Sir Daniel has brought to your notice. I am sorry to say that I have the advantage of Sir Daniel Morris by a good many years' remembrance, and my memory goes back more years in connection with the Royal Horticultural Society than his does. I remember when, as a boy at school, I was allowed to go up by an early train as a great treat, for behaving properly, to see the Show at Chiswick. I remember very well what lively Shows they were. I was not present when the final disaster took place in connection with those Shows when, with the Society's usual misfortune in regard to the weather at Chiswick, the tents collapsed in a tremendous blizzard, obliterating plants and visitors alike, and when the garments of ladies and gentlemen were scattered to the winds and had to be picked up in bushel-baskets. I believe that that occasion was almost the final disaster of the Chiswick Shows, which at one time were the most beautiful Shows ever conceived of. Then, as everybody knows, we were planted down in South Kensington—a curious example of the way in which British institutions possess the habit of endurance, notwithstanding mistaken management and the abandonment of the fundamental principle of horticulture pure and simple. Sir Daniel Morris has referred to his own efforts, for which the Society must be ever grateful to him. He says he was not at all times very popular as Treasurer, but who that does his duty under trying and painful circumstances, and cuts off extravagances in this direction and in that, can hope to be very popular? No one likes what he considers as his privileges to be curtailed. But it is better to succeed than to be popular, and the fact will remain that Sir Daniel was Treasurer when the finances of the Society were checked in their downward career and set moving on that upward progress which they have pursued ever since. We were deeply sorry to lose his services so soon, but we are proud to think that it was from the treasurership and reorganisation of the Royal Horticultural Society that Sir Daniel went out to the West Indies, where he has been able to do such invaluable work for the British Empire in our Colonies. Then as to our old friend Baron Schröder, it is impossible for anyone to speak too highly of the magnificent work which he has done for the Society and of the great enthusiasm with which he always inspired his colleagues on the Council, and I am quite sure that had it not been for the burden of more than eighty years that has fallen upon him he would have been here with us to-night.

I am quite sure, gentlemen, that you will allow me to express a word of thanks on behalf of the Society and of the Conference to the Horticultural Club for their sumptuous entertainment to-night.

I find myself between two old friends, Sir John Llewelyn and the President of the Conference, and anyone who heard Mr. Bateson's opening address this morning must have been struck with the exceedingly lucid manner in which he dealt with his most difficult subject. Science makes possible to-day what was impossible yesterday, and when the results of investigations are placed before us with such extreme lucidity I am sure we are deeply grateful to him. Our Secretary gave me a paper seme little while ago, which shows that anyone can become a member of the Royal Horticultural Society, the fee being one guinea. I have always said you could scarcely have a better investment for your money, because every guinea produces three guineas at the least! Mr. Wilks has drawn out a very careful statement, and from it he makes out that every subscriber of one guinea gets in return £6 14*s*.

Well, I have got a little corroboration from an entirely independent source. I have the misfortune to be a Unionist, and, therefore, I am in a pitiful minority. I sometimes speak to gentlemen in the oppesite camp, and one of them is our present Foreign Secretary, Sir Edward Grey. He said to me the other day, "I am a Fellow of your Society, because it is the only Society from which I ever get anything." The present position of the Society has been referred to. Its financial position is eminently satisfactory. The Hall you have met in to-day is, on the whole, a very satisfactory building, and it is absolutely paid for. It has cest somewhere about £45,000. We have also a Garden which was given us by Sir Thomas Hanbury, to whom we are greatly indebted. It is exactly the sort of Garden we ought to have.

Before Mr. Wilson died, greatly regretted by all horticulturists, the Garden, owing to his advancing years, had not been as carefully looked after as during his more active period. For our purposes it required a good deal of money spent upon it. We have spent that money, and we hope in the not remote future to establish there a scientific laboratory both for the training of students and also for undertaking independent research work. I think when that is accemplished, if we do not rival Rothamsted we may do—not the same, but similar—work of an equally useful kind.

We still have an investment of £20,000 which is put on one side for the future. I do net know that there is much more I desire to say, and when I am speaking on this subject I am afraid of saying the same things over and over again. Our Fellows now number nearly 10,000, and I am sometimes inclined to ask whether the time has not come for us to do what clubs have done and clese our lists! I therefore venture to suggest that anybody who desires to become a Fellow of the Royal Horticultural Society should make haste, because in a short time it may become necessary to close the list from the impossibility ef accommodating more Fellows in our grand new Hall. As was said years ago, "it may confidently be asserted that the Society has done and is doing a great work."

The Chairman proposed "Our Guests." He said:—It is due to our guests that our thanks should be well and heartily expressed, as they have responded to our invitation to attend this most important and valuable Conference—this International Conference on Hybridisation. Our guests are all men who are experts in either or in both scientific and practical horticulture, and they have come from all parts of the world; and it is a great privilege to the members of this Club to invite these distinguished gentlemen from other countries, and also those representatives from the United Kingdom whom the space at our command permits, to give us the honour and the pleasure of their company at this banquet here to-night. It seems to me, however, that there is another motive in our breasts in welcoming such a gathering as we see here to-night, a motive which I may call the brotherhood of kindred tastes. We are all aiming at higher standards for the future, for which we hardly at the present moment know how to aspire. These tastes have been recognised by the different counties of England, and their County Councils have decided that in future, in the education of the people, a knowledge of horticulture shall form a part, which will, I am sure, raise the whole tone of the tastes and lives of the young people of this country. Horticulture is useful, elevating, pure, healthy, and progressive, and it is on the behalf of progress in horticulture that you gentlemen have come together on this occasion. If you look back three or four hundred years there are things that were then quite luxuries, but which at the present day are the necessaries of life, and there are things which were quite unknown at that time. Take, for instance, tobacco: it is not a necessity of life for me, because I do not smoke; but it has become a necessity to many, and there are hosts of similar things, such as the potato, tea and coffee, and cocoa, which have now become necessities for men, women, and children. And as a few hundred years ago these things were unknown, but have now become necessaries in the lives of the people, so it is for the welfare of the rising generation that the knowledge of such subjects as hybridisation and plant breeding should be studied, and the probable result that may be obtained considered. At the conclusion of this Conference you will all, I have no doubt, go away to your distant homes and work for the benefit of the community at large, and may we all in the future come back and enjoy many such useful and pleasant reunions such as we have here to-night.

Mr. Bateson, F.R.S., V.M.H., was the first to respond. He said:—I wish I could express some part of what I feel on rising to return thanks for the guests to-night. This is a very different occasion from that in 1899, when I had the honour of being invited to a somewhat similar dinner by the Horticultural Club, when we assembled in a little company numbering about thirty guests in all, members of the first Hybridisation Conference. I think one has only to look round this large room to feel how wonderfully the field has developed since those days, how much the interest has been increased. I am returning thanks for the guests for an entertainment of no common order. The entertainment I may describe as princely, and we have only to look at the wonderful decorations of the tables to help us to feel what this entertainment is, and in what it is we are taking part. I have to return thanks also for those who are not here. I think of one who was with us on the former occasion, Professor de Vries, whose marvellous discoveries, published soon afterwards, have thrilled the scientific world. He ought to have been here to-night, but, like works of art which are acquired by San Francisco and Boston, Professor de Vries has been acquired by the American nation. Happily, however, unlike the works of art, he will return, not the worse, but I hope the better, for his sojourn across the water. There is another professor absent to-night, Professor Correns, whose name will be remembered as long as this branch of science is pursued. We are exceedingly sorry that it is ill-health which keeps him away. But even without these two "giants in the land" I am happy to say there are many present this evening who will make this gathering distinguished. When I ask myself "What is the reason for this princely hospitality; why should we be taken under these generous auspices?" I really have no answer to give. I wonder sometimes whether we really have a valid position; but I feel, after all, that Science and Practice in horticulture should go hand-in-hand, and tbat Science by her discoveries can give useful instruction which can be used and appropriated to the practice of the art, and we are here to-night to declare our belief that the promises made on behalf of that union will yet be fulfilled, and that there is a solidarity in our international union which will last for many a day to come. To the question, "Will those promises be fulfilled?" sometimes I say "Yes," and sometimes I say "No." To-night I am going to say "Yes."

The great thing is not to promise too fast or too much, and I trust that no one will suppose that Science is going to do the impossible, and produce new forms of plants and animals out of nothing. You cannot do it! You cannot get a yellow pea from a white, or a red from a yellow, if the yellow or the red is non-existent; but if the colours are latent there, Science will get at them. At present we know only the mere rudiments of our profession, but I believe that our knowledge, combined with yours, will one day produce something very remarkable. On an occasion seven years ago at the Royal Horticultural Society I expressed a doubt whether Science had anything to contribute to Practice, looked at from the trader's point of view. But seven years have gone by since then, and I now know that we have something. The scientific and the practical have gone to form a perfect and fertile hybrid. I think segregation will occur, and that Science will ultimately separate from Practice; but that date is remote, and it is quite enough for us to rest on the absolute fact that for many years to come Science and Practice will go hand-in-hand and assist each other. There will come a time when Science will have learned all it can from Practice, and possibly there will come a time when Practice will have learnt all it can from Science, and, as in the profession of electricity, Practice will develop into Science. The practical electrician of to-day is at the same time the scientific electrician, but in horticulture I expect a century must elapse before the same complete union of Science and Practice will be achieved. But one thing is certain, we shall between us succeed in producing very good results in the meantime. There is one thing one learns by being a hybridist. We have all produced hybrids, although some of them have been worthless and sterile. But there is one thing we learn, and that is "patience." I am not referring to the patience necessary for watching a seed come up; I am alluding to patience of a different character—the patience that is wanted, and which we ask you to show towards us, when we are speaking to you on matters which to many are absolutely unintelligible. Yes, the practical hybridist learns patience when he attends the deliberation of a Conference of this kind. I suppose those who are exclusively practical gained small illumination from what was said this morning. But we on our side also are learning patience. For example, I ask myself when I look round these beautifully decorated tables—I ask what could we do to increase the beauty of these objects by the aid of those mysterious symbols written on the blackboard, and as yet I have to reply, Absolutely nothing! Our union is of the most bizarre character. We are each getting something from the other, and we have to show the greatest patience with each other as matters develop. I feel every confidence, however, that that union will last for many years to come, and that it will be extraordinarily prolific. The day has passed when our subjects will suffer for want of interest. There will always be sufficient interest in our work to take us over the dead points, for our work will become a living reality. I have only to thank you for helping us towards this success.

M. Philippe de Vilmorin: Mr. Chairman and Gentlemen,—Mr. Bateson has spoken of the union between the scientific man and the practical

Fig. 12.—M. Philippe de Vilmorin.

man. He said that the differences between Practice and Science are very great, and that few practical men know very much about Science, and that, on the other hand, scientific men often lack Practice. I can only answer and say that if we practical men lack Science we only want to get it, and what we especially want to do is, to help others engaged in like work with ourselves. Many years ago, and before the theories of Mendel were published, or his ideas of heredity known, we were working on the old theory of evolution, which still stands good for many cases. Even at that time some of the people whom you call practical were beginning to work, and I think contributed very important elements to Science. Speaking only of what I know,—my grandfather said that variation and selection in wild plants would ultimately bring them to the same types as cultivated plants. At that time this opinion of his seemed almost paradoxical. But to test it he started with the wild carrot, and in only a very few generations produced something very like a garden variety. That is why my father, in his turn, began his experiments, starting from a plant of the same family. Later on he started his experiments in wheat, to prove the unity of the species, and to find out the variation in the F_2, or second generation. In this, he was not of course working from a practical point of view, but from a scientific ; and what I want to point out is this, that some of the practical men (to whose number I am proud to belong) have already done something to assist Science. The only thing we claim is that we have helped scientific men, and we ask them to give us in return the results of their experiments. I ask you to join me in thanking the Royal Horticultural Society for organising this Conference for the improvement of knowledge, and the Horticultural Club for inviting us to this magnificent banquet.

Professor Wittmack : Mr. Chairman and Gentlemen,—We have seen all that you have done on the present occasion, and we congratulate you on having called together men of science and men of practice. The man of practice can hybridise plants, and working with the man of science they together can hybridise nations. Your programme shows how universal your Society is, and we shall all be anxious to read your Report of the Conference, which we hope will be published as soon as possible. I have so enjoyed your Society and am so struck with the work you are doing, that I cannot say how glad I am to be among you.

Professor Johannsen : Mr. Chairman and Gentlemen,—I must say, what all others have said, I am quite surprised at the way in which you have entertained us. I am only a man of science, and I once thought with Liebig that there was *no* common ground of action between the practical and the scientific. Now I know that Science and Practice can and do go hand in-hand together, especially in horticultural matters. It is marvellous to see what you English men of science are doing, and in particular what the Cambridge school is doing ; it is marvellous, it is excellent. All present, Germans and Frenchmen, Austrians and Italians Swedes and Norwegians, Belgians, Danes, and Dutch, all of us beg to thank you.

Lieut.-Colonel Prain, F.R.S., next proposed "The Chairman." He said :—Gentlemen,—The part our Chairman has taken in public affairs is a matter of contemporary history. We know what he has done in the world of sport. Has he not hunted and shot? Has he not been captain of a redoubtable cricket team? Is he not now President of a still more redoubtable Football League? Has he not taken a share in the Congress of the Auricula Society? We know what his work has been for the Horticultural Club, and I ask you to drink the health of a good gardener, a great sportsman, and an all-round English gentleman—"Our Chairman."

The toast was drunk with the utmost enthusiasm.

Sir John Llewelyn, in replying to the toast, said:—I thank you, gentlemen, very heartily for the exceedingly kind way in which my name has been proposed to you and for the way it has been received by this distinguished company. My friend Colonel Prain, you may know, is a Fellow of the Royal Society, and is also the distinguished head of the great national garden at Kew—a garden of which we are all exceedingly proud. Many a time, I have been there, and many a time have I seen hundreds, I might say thousands, of people, one and all enjoying the beautiful gardens of our national possession. Long may Colonel Prain be there to superintend its destinies.

And now as to our dinner. I should like to give credit where credit is due. The organisation of this dinner has been very well managed by our excellent Secretary, Mr. Cook; and I can assure you that the honour of having had you as our guests will ever make your visit to the Club memorable when you shall have left this little island of ours and returned to the different parts of the world from which you have so kindly come to visit us. And I hope that our little convivial meeting here to-night may also linger in your memories and recall to you occasionally, as it will constantly to us, a very pleasant and enjoyable evening at the Third Conference on Genetics.

VISIT TO BURFORD.

On the afternoon of Wednesday, August 1, the members of the Conference, with many ladies, went on a visit to the beautiful country residence of Sir Trevor Lawrence, Bart., the President of the Society, at Burford, near Dorking, in Surrey.

They travelled by special train from Victoria Station, and on arrival were warmly welcomed by Sir Trevor and Lady Lawrence. Luncheon was served in a marquee most charmingly decorated to harmonise with the surrounding foliage.

DÉJEUNER DU 1er AOÛT, 1906.

Darnes de Saumon. Sauce Remoulade.
Côtelettes de Mouton à la Norvégienne.
Pâtés de Pigeons à la Française.
Poulets et Langues au Cresson.
Salade de Laitues. Salade jardinière.

Jambon à l'Aspic.
Rond de Bœuf à l'Anglaise.
Quartier d'Agneau. Sauce Menthe.
Roast Beef à la broche.

Entremets.
Gelée aux liqueurs.
Riz à l'Impératrice.
Tartes aux fruits.
Macédoine de fruits.
Glaces panachées.

Luncheon ended, Dr. Professor Wittmack, of Berlin, rose to propose the health of the President. He said :—I am sure, ladies and gentlemen, that I speak in your name, as well as my own, when I offer our heartiest thanks to Sir Trevor and Lady Lawrence for the abounding kindness and hospitality with which we have been received here to-day. Is not everything here lovely! We from Germany have not often had occasion to see an English country mansion-house, or the English gardens and parks. Therefore I am more astonished at the beauty and the calmness of this delightful situation. When we return to our own lands we shall always think with pleasure of the mansion of the President of the Royal Horticultural Society. The Royal Horticultural Society is a venerable Society. It is now more than a hundred years old. But I am sure that in all those hundred years it has never had a better President than now. He has now been President for a long series of years—twenty-one years. You heard something about the Society in his address on Monday evening but he did not tell you all. When he began his reign the Society was very, very much down. Now it is flourishing, it is very, very much up; and to whom is that due? It is indebted for that to a great extent to its excellent President, Sir Trevor Lawrence. Sir Trevor is a man who loves flowers, and who not only loves flowers, but who *studies* flowers, especially orchids, and he hybridises his love for flowers with his love for welcoming his guests. I propose that we drink the health of Sir Trevor and Lady Lawrence and their family.

Sir Trevor Lawrence said:—Professor Wittmack, Ladies, and Gentlemen,—I do not remember how many years it is, but it is some considerable time since I had the pleasure of a visit from the Medical Congress, which at that time met in London on very much such a day as this. I think I may say that to-day we have been fortunate. I am afraid, owing to the hangings of the tent, you have not all of you had quite as much of the delightful breeze that is blowing as you could have desired; still, it is a beautiful day. I am not going to wish for what happened on the occasion of the visit of the Medical Congress, when it began to rain the next day, and rained vigorously for about a fortnight. I hope the rain, having now kept off so long, will postpone its advent for yet some few days longer—at any rate until this Conference is over. It has been a very, very great pleasure to my wife and myself to receive so many distinguished men as we see around us here to-day, and we are the more pleased because the weather has enabled you to see this part of Surrey to the best advantage. Surrey is an exceedingly pretty county. Well, gentlemen, you have heard what Professor Wittmack said about the Royal Horticultural Society. I have had the good fortune to be more or less a figure-head, and you know the people to whom our success is really due. I should like it to be understood that I am the very last to take any credit for the resuscitation of the Society. We have had the advantage of the help of the best gardeners in the kingdom. We have had with us men who worked for horticulture and all that belonged to it, because in this country it is a very big industry. They have done all they could to support the Society, and to them very greatly is due the present position of the Royal Horticultural Society, as well as to the fact that we, that is to say, the President, the Treasurer (Sir Daniel Morris), the Secretary (Mr. Wilks), and the rest of the Council, decided to devote ourselves and the Society, absolutely and solely to the promotion of horticulture. Some reference has been made to the plants grown here. When my friends say to me, "I think you grow orchids, don't you?" I always say, "Yes, it is perfectly true," but I always like them to understand that I take a warm interest in every class of flower and fruit and vegetable, in fact in every branch of horticulture. I may know a little bit more about this branch or that, but I should be unwilling for my friends to suppose that I am entirely absorbed by orchids and do not take an interest in other plants as well. I have lived among horticulturists all my life, and the little that I know I have found to be of great use under all circumstances, and if horticulture has been my foster-mother she has been a very genial foster-mother indeed. There may be some plants that may interest you out of doors. I hope you will go wherever you like to wander. I would only repeat that it has given my wife and myself the greatest possible pleasure to see you all here to-day, and I hope we may all still be alive when the next Hybrid Conference takes place in London, and that you will again favour us with the pleasure of your company at Burford. I thank you, Professor, very much indeed, for your most kind words, and you, ladies and gentlemen, for the kind way in which you received them.

The guests then rambled about the beautiful park and visited the gardens and plant houses. Everything was greatly admired. As has been said, the day was perfect, and one of the ladies of the party observed, "Everything seems so happy"—a remark absolutely descriptive of the peaceful surroundings, as well as of all the visitors. During the afternoon the band of his Majesty's Royal Artillery played a selection of music. At half-past four tea was served on the lawn, and shortly afterwards the members of the Conference and their friends left by special train and reached London at a quarter past six, having enjoyed one of the most delightful excursions imaginable.

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At the Fifth Session of the Conference, held on Thursday, August 2, the two following resolutions were passed unanimously:—

I. Proposed by Professor Wittmack, of Berlin, seconded by Dr. Erwin Smith, of U.S.A. Department of Agriculture:

That the following message be sent to Baron Schröder, V.M.H.:—The Foreign Members of the Third International Conference on Hybridisation and Plant-breeding, gathered from all quarters of the world, and now sitting in the Lecture Room of the Royal Horticultural Society, wish to congratulate Baron Sir Henry Schröder, Baronet, Victoria Medal of Honour, and his fellow-helpers, on the magnificent Hall and Buildings which their efforts have raised in celebration of the Centenary of the Society.

II. Proposed by H. J. Elwes, Esq., F.R.S., V.M.H., seconded by Sir Daniel Morris, K.C.M.G., V.M.H.:

That the members of the International Conference on Hybridisation and Plant-breeding, gathered from all parts of the world, and assembled in the Hall of the Royal Horticultural Society of Great Britain, desire to express to the President of the United States of America and to the Minister of the Department of Agriculture at Washington, their hearty appreciation of and thanks for the invaluable assistance which has been given to farmers, horticulturists, planters, and scientific men throughout the whole world, by the liberal distribution of American research publications.

American Embassy, London, August 4, 1906.

Sir,—I have the honour to acknowledge the receipt of the resolution passed on August 2 by the members of the International Conference on Hybridisation and Plant-breeding expressing to the President of the United States and to the Minister of the Department of Agriculture appreciation and thanks for the assistance given farmers, horticulturists, planters, and scientific men by the liberal distribution of American research publications.

In accordance with the request of the President and Council of the Royal Horticultural Society, which you have been kind enough to communicate, I shall take pleasure in forwarding this resolution to my Government.

I am, Sir,
Very respectfully,
Your obedient servant,
WHITELAW REID.

The Rev. W. Wilks, M.A.,
Secretary, Royal Horticultural Society,
Vincent Square, Westminster, S.W.

Department of Agriculture, Office of the Secretary, Washington, D.C.,
September 4, 1906.

Dear Sir,—I am in receipt of the resolution passed by the members of the International Conference on Hybridisation and Plant-breeding relative to the publications of this Department. We appreciate very highly the estimate that the Conference places on the publications of this Department, and we are glad that they are useful to the agricultural interests outside of the United States.

Thanking the Conference in behalf of the Department, I am,

Very respectfully,
JAMES WILSON,
Secretary.

The Rev. W. Wilks,
Secretary, Royal Horticultural Society,
Vincent Square, Westminster, S.W., London, England.

American Embassy, London, September 8, 1906.

Sir,—With reference to your letter of the 2nd ultimo inclosing a resolution passed by the members of the International Conference on Hybridisation and Plant-breeding assembled in the Hall of the Royal Horticultural Society of Great Britain, expressing to the President and the Secretary of Agriculture the appreciation of the members of the Conference of the aid given to agriculture and kindred sciences by the research publications of the Department of Agriculture, I am instructed to express, in the name of the President and the Secretary of Agriculture, their high appreciation of the honour you have done them in forwarding the resolution.

I am, Sir,
Your obedient Servant,
J. R. CARTER,
The Secretary of the Embassy.

Rev. W. Wilks,
Royal Horticultural Society,
Vincent Square, Westminster, S.W.

THE SOCIETY'S BANQUET.

On Thursday, August 2, the guests were entertained at a banquet in the Great Hall of the Society. The President, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., presided over a large company, including, besides other specially invited guests, all the members of the Conference.

Dîner du 2 Août, 1906.

Potages.

Purée St. Germain. Consommé Sévigné.

Poissons.

Suprême de Soles. Saumon. Sauce Vert-pré.

Entrées.

Vol-au-vent à la Toulouse. Petite Galantine de Cailles.

Rôt.

Selle de Mouton.

Relevés.

Pintade en Casserole. Jambon de York.
Petits Pois.

Entremets.

Gelée à la Maltaise. Compote de Fruits à la Crème.
Bombe à l'Ananas.

Fin.

Petite Mousse au Parmesan.

Dessert.

THE TOASTS.

1. His Gracious Majesty the King.
Proposed by the President.

2. Her Gracious Majesty Queen Alexandra and the rest of the Royal Family.
Proposed by the President.

3. The Foreign and British Members of the Conference.
Proposed by Sir John T. Dillwyn-Llewelyn, Bart.
Responded to by
(*a*) Professor Hansen
U.S.A. Department of Agriculture.
(*b*) Professor Dr. Tschermak, Vienna.
(*c*) M. Philippe de Vilmorin, Paris.
(*d*) Sir Michael Foster, K.C.B., F.R.S.

4. The Board of Agriculture, Horticulture, and Fisheries.
Proposed by W. Bateson, Esq., M.A., F.R.S.
President of the Conference.
Responded to by Sir Thomas Elliott, K.C.B.
Secretary to the Board.

5. Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., President of the Royal Horticultural Society.
Proposed by the Rt. Hon. Viscount Mountmorres.

God Save the King.

After dinner Monsieur Tivadar Nachèz performed on the violin.

1. Reverie . . . . *Vieuxtemps*
2. { a. Preislied . . . . . . *Wagner*
   { b. Humoreske . . . . . *Dvorak*
Poëme de la Puszta . . . . . *Nachèz*

Pianist, Mr. S. Liddle.

The Loyal Toasts.

The President, rising to propose the toast of " His Gracious Majesty the King," said :—My Lords, Ladies, and Gentlemen,—I ask you to drink to the health of His Gracious Majesty King Edward VII. His Majesty has always taken a warm interest in all that belongs to gardeners and gardens, and he was graciously pleased to come to this Hall two years ago and open it on the occasion of the first gathering we held in it, and His Majesty was so good as to describe it as " this magnificent Hall " ; but I do not think I have ever seen it look so magnificent as it does to-night when so many of the most eminent scientific men of Europe and of America are assembled in it. I ask you to drink the health of His Gracious Majesty King Edward VII.—a toast which appeals to us on all occasions when Englishmen gather together, and in which, I am confident, all our foreign guests will join as cordially as we ourselves who have the privilege and happiness to be His Majesty's most loyal subjects.

The toast having been enthusiastically honoured, the President rose to give the toast of " Her Gracious Majesty Queen Alexandra and the rest of the Royal Family." He said :—My Lords, Ladies, and Gentlemen,—The Queen has endeared herself to the hearts of all Englishmen, but especially to all gardeners, for I think I may say that Her Majesty is herself one of Nature's own flowers. She lives also in the hearts of all of us, she is the type of everything that is gracious and delightful, and I am sure that she and all the members of the Royal Family will always so live in our hearts. I have been asked, in addition to proposing the toast of our own Royal Family, to mention the fact that this day is the birthday of Her Majesty Queen Emma of Holland. We have the advantage of the presence here this evening of several eminent Dutch men of science, and I am sure that you will join with me in saying that, after our own Royal Family, there is hardly anyone for whom we entertain a greater admiration than for Queen Emma. I ask you to couple with the toast the health of Queen Emma of the Netherlands.

The double toast was cordially honoured.

Presentation of Medals.

The President :—My Lords, Ladies, and Gentlemen,—Before we proceed further with the toast list I have the very pleasant task of presenting some medals which I am bold to say have been earned many, many times over by those who are to receive them. In the first place, there are four Veitchean medals (founded to perpetuate the memory of the late Mr. James Veitch, of Chelsea, and in recognition of his many benefits to horticulture) : these medals are awarded from time to time to gentlemen who stand out pre-eminent for their services in the advancement of scientific or practical horticulture. The first medal which I have to give to-night, if he will honour us by receiving it, is awarded to the President of our International Conference on Genetics, Mr. Bateson. It is most gratifying to all the members of the Royal Horticultural Society to greet this announcement with enthusiasm. It is impossible for any Conference to have had a better Chairman, and if it happens that the Society should ever hold another such Conference, we shall be exceedingly fortunate if we secure as good a President as Mr. Bateson has been.

The next medal—and I may here remark that they are gold medals, and not silver-gilt—goes to Professor Johannsen, of Copenhagen, and is awarded, among other claims, for his discovery of the effect of ether in hastening the inflorescence of flowers. Professor Johannsen, by the paper he read at this Conference, has earned our admiration and thanks, and in regard to his scientific acumen and ability we here in England can stand in no doubt whatever.

The next medal goes to Professor Wittmack, of Berlin. Professor Wittmack has for many years devoted the whole of his weighty learning to the study and the exposition of systematic and practical botany, and his services to horticulture are so many that unless the twenty-four hours of the day could be extended to thirty I could not enumerate them.

The next medal, and the last of this series, I have the honour to offer to Monsieur Maurice de Vilmorin. I do not suppose anybody who has the slightest interest in horticulture is unacquainted with the name of the eminent firm of which Monsieur de Vilmorin is a prominent member. I can bear personal testimony that it is a firm which for many, many years past has rendered most signal service to European, indeed to the whole world's horticulture.

It is so seldom that we have such an International Conference that I must ask leave to be allowed to present three further medals: One is to a lady, Miss E. R. Saunders, Lecturer on Botany at Newnham College, Cambridge. Miss Saunders has conducted the most intricate and difficult researches on the basis of Mendel's laws—researches demanding the utmost exercise of patience, coupled with the keenest observation. A silver-gilt "Banksian" medal is awarded to her for the value and extent of her researches in the physiology of inheritance in plants.

A similar medal to the last is awarded to Mr. R. H. Biffen, M.A., for his researches and discoveries in the heredity of cereals. Working on Mendel's principles, Mr. Biffen has shown that new varieties of wheat may be produced combining in one the high quality of the best foreign wheats with the productiveness of our standard English varieties. He has also made a most important contribution to our knowledge of the inheritance of disease, by proving that certain common diseases in wheat are transmitted to the offspring in strict accordance with Mendel's laws, so that they can be controlled, and in fact bred out; and in both these ways his work holds out the brightest hopes to farmers and landowners in these days of gloomy agricultural prospects.

The last medal is presented to Mr. C. C. Hurst for his researches into Mendel's laws of inheritance. As is well known to all, Mr. Hurst has been for many years conducting these researches, beginning, I believe,

Fig. 13.—Professor Johannsen.

Fig. 14. Professor Wittmack.

Fig. 15.—M. Maurice de Vilmorin.

Fig. 16.—Miss E. R. Saunders.

Fig. 17.—Mr. C. C. Hurst.

The Society's Banquet in the Great Hall.

with Cypripediums, and following up his plant discoveries into the animal kingdom, and dealing especially with pigeons, rabbits, sheep, and even racehorses.

As the recipients came forward to receive their medals at the hands of the President they were cheered again and again.

Sir John T. Dillwyn-Llewelyn, Bart., then proposed "The Foreign and British Members of the Conference." He said:—My Lords, Ladies, and Gentlemen,—The duty that has been placed upon me is a pleasant one at all times—that of proposing the health of our visitors—but it is especially pleasant on the present occasion. We were delighted at the exceedingly pleasant ceremony, through which we have just passed, at the hands of our excellent President. It was not upon the list—it was interpolated—but it is perfectly clear, by your applause, that you appreciated that ceremony very much indeed.

The toast of "The Foreign and British Members of the Conference" brings to my mind the hope that we entertained from the commencement—a hope which, I believe, has been fully realised—that we might be permitted to allow our visitors to carry with them, on their return to their homes, pleasant memories of their visit to England—memories that their visit has been a useful one, useful to science and pleasant to themselves; and I think I may say that the kindly way in which you, Sir Trevor, and Lady Lawrence received them at Burford yesterday will be perhaps almost the brightest and pleasantest part of their memories when they return home again. It has been said that the man is a benefactor to his country when he makes two blades of grass grow where only one grew before; and I think the efforts of our Society in bringing together scientific men on the one side and practical men on the other, have justified our efforts, because it is impossible for any horticulturist to see what is going on at the present day without being able to recognise in all branches of horticulture a justification for bringing together the hybridist, the botanist, and the man of science—men who have joined together in giving effect to their work, in giving to their seedling plants a larger yield, a longer period of usefulness, and greater marketable value—who have given their plants improved size and quality, and greater immunity from disease. These are points which, to my mind, justify the work of this Conference; and if I might refer to one other fact to illustrate the usefulness of what we are doing to-day it is this: this is the Third Conference on Plant-breeding within seven years. You met once before in London in 1899, and again in New York in 1902; but so much has been done since then, so much progress has been made, so much new knowledge has been attained, that you now find it necessary to meet together again in 1906; and I challenge anyone to say that the meetings of this Conference have been without fruit and practical value under the able superintendence of Mr. Bateson. It is with the very greatest of pleasure that I ask you to drink the toast, and I am privileged to ask you to join with that toast the names of those eminent and able men Professor Hansen, of the Department of Agriculture of the United States of America; Professor Tschermak, of the University of Vienna; M. Philippe de Vilmorin, of Paris; and Sir Michael Foster, K.C.B., F.R.S. I give you the toast with all my heart.

Professor Noorduijn, of Groningen, Holland, speaking in Dutch, said :—I rise with the permission of the President to say a few words. Sir Trevor Lawrence, my Lords, Ladies, and Gentlemen,—Everyone in Holland speaks with great sympathy of Queen Emma, who, on account of what she did for our dearly beloved Queen Wilhelmina, and because of the great interest she continues to show in everything partaking of national concern, implanted in all Dutch hearts a feeling of lasting love and gratitude. Sir Trevor Lawrence, in the name of our Dutch people I thank you most heartily for your sympathetic words, and you all, ladies and gentlemen, as the scientific delegates of so many different nationalities, for your sympathy. Long live the Queen-Mother of the Netherlands!

Professor N. E. Hansen :—Mr. President, my Lords, Ladies, and Gentlemen,—On behalf of the United States Department of Agriculture and my brethren across the seas, I thank you for this most cordial greeting, and for the opportunity we have had of learning some of the latest and best things in the development of plant life. What does this Conference mean? It means that the development of plants is going to be an exact science. What was formerly a chaos of empiricism is now becoming one of the exact sciences due to the recent discoveries in heredity. No longer is heredity a jungle. Owing to the discovery of Mendel's law, a clear path has been blazed through the jungle of heredity. New and valuable forms of plants may spring, like Minerva, full-fledged from the head of Jupiter, and we now go forward in hope that in the next twenty years we shall have many new varieties of flowers and of plants of great economic value.

The policy of the present U.S.A. Department of Agriculture is to search the world for some new plant life better adapted to the various parts of the United States from Alaska to Southern Florida. I was the first agricultural explorer who was sent out on behalf of our department in 1898–99 to Russia, Transcaucasia, Turkestan, China, and Siberia, and I am now again on my way to Russia, intending to pass through Siberia and Japan. Since 1897 fourteen or fifteen other explorers have been sent out to various parts of the world. We are searching the world over for new plants, and we are also making new plants at home; we are determined to find, or to develop, new plants that will endure under all conditions. This means, further, that we intend to have plants that will be completely immune to the many diseases of plant life. And so it goes on. If I had more time I could tell you something of the perils and dangers which agricultural explorers undergo in a two thousand miles' journey by waggon and sleigh in Turkestan, China, and Western Siberia, but there is hardly time to do that now. Suffice it to say that the world is being searched by the United States for forms of plant life that are better adapted to our manifold conditions, which are so very varied. As fast as this material is secured we resort to crossing and selection, and thus carry the improvement still further.

[During the delivery of the Professor's speech one of the heaviest thunderstorms within living memory raged, almost drowning all sound but its own.]

Dr. Tschermak responded in German. He expressed on behalf of the German and Austrian delegates their deep gratitude for the cordial

Fig. 19.—Dr. E. Tschermak.

reception given them by the Society. As an Austrian he had gladly accepted the invitation extended to his country. He was convinced that the latest developments in the study of hybridisation had received the greatest impetus from that Conference. In 1900 he had, contemporaneously with Professor de Vries and Professor Correns, been able to rediscover and re-prove the Mendelian laws which had so long been lying unnoticed, unregarded, in the library at Brunn. Since then the conditions of the study had been changed by a long series of memorable discoveries. Dr. Nägeli had published materials written by Mendel's own hand on the subject of hybrids, which had been of the greatest value to science; and in England Mr. Bateson had rendered the greatest possible services to the furtherance of the science of genetics. The Royal Horticultural Society did indeed deserve the warmest, heartiest thanks of all European men of science, both for summoning the Conference and also for the princely hospitality with which it had entertained its visitors.

M. Philippe de Vilmorin, speaking in English, said :—Mr. President, my Lords, Ladies, and Gentlemen,—Two days ago, as many of you can remember, I was asked to answer to a toast at the dinner of the Horticultural Club. I then said I was not prepared and complained of it; but Sir Daniel Morris told me it was all right because "you would enjoy your dinner much better than if you had known beforehand that you were to speak." To-day I knew beforehand that I would have to respond for my part of the toast of the guests. I say I knew it before, but I must tell you that all the same I have enjoyed my most excellent dinner. First of all, I enjoyed it because it was so very good; and in the second place because I knew you would give me all your indulgence, and also because it is always easy to return thanks for things you have really enjoyed. I knew that I should have to ask for your indulgence for my bad English, because on the other side of the Channel we are not accustomed to after-dinner speaking as you are in this country. I think, to use a terminology to which you all are accustomed, the talent for after-dinner speeches is with us of a "recessive" character. I think, perhaps, after a few years in England, you would find us using "cryptomères" in our speeches. But speeches after dinner are generally a banality. I could do it very easily. I could say, for instance, that horticulture is at the same time a trade, an art, and a science; that horticulture is the most difficult and the most abstract of all the sciences. But I do not think it is to those points that we owe our great popularity. I think it is only because we have, first of all, the good luck to deal with things that are, ladies excepted, the most fascinating gifts of creation, and it is, besides, because we every day of our lives come in close contact with Nature. By doing that we better ourselves, and we also try to better others by the contemplation of these beauties of Nature. We cannot live close to them without trying to know the laws of Nature, and the great problems of heredity and of the origin of life, and that is why our calling is so popular and why we like it so much. I have already told you that I knew it would be easier for me to thank you for what you have done for us. I have spoken already of this excellent banquet. You must not think that the dinner has been our only enjoyment, because there have been the fruits of the Conference. When I received a fortnight ago this most convenient little volume of tickets, I thought it would last for ever, but every day we had to give up a few, and now I am sorry to say the book is almost empty. I speak for myself when I tell you that when I came to this Conference to hear of Mendelian theories I was rather doubtful; but now that I have been so much with you, and have heard all that has been said, especially (if I may be allowed to say so) this morning, by Mr. Biffen, some of whose characters at first sight seemed to be strangers to Mendel's laws, I am and will ever be an apostle of the theory.

And now, ladies and gentlemen, I should like, representing as I do the National Horticultural Society and the Botanical Society of France, to invite you, if I may be allowed to do so, to come and hold your next Conference on Genetics in Paris. During the past few days we have heard about, and we have seen, the greatness and the power of your century-old Society. We in France are much smaller and fewer, much poorer and younger; but all the same we will do our best to please you and to give you something, if not an equivalent, for what we have received, something that will give you a good impression of our country. I hope you will all come with papers. I cannot tell you exactly when that Conference will meet, but perhaps four years would give time to all the workers in heredity and hybridisation to make fresh experiments, to find new laws, and to make interesting communications. But as the International Botanical Congress also takes place in 1910, I do not think it possible for both events to take place in the same year. I will communicate with the organisers of that Conference as to their views, and try to fix a date in the year which will suit everybody. I ask you now to rise and drink the health of the Royal Horticultural Society, who have given us such a splendid reception during the past week.

Sir Michael Foster said:—Sir Trevor Lawrence, Ladies, and Gentlemen,—I obey your summons, Mr. President, to rise to respond to the toast on behalf of the British guests, but I feel ashamed in doing so. I had hoped to be a diligent member of the Conference, but circumstances, public and private, made my attendance but very fitful. But this has its reward: it enables me to dissociate myself from the rest of the delegates, all of whom, I may say, fully deserve all the praise that has been bestowed upon them by Sir John Llewelyn.

This Conference has been in many ways a remarkable one. It is a Hybrid Conference. It has represented the crossing of the efforts of many nations. While, however, many hybrids are sterile, this hybrid has been remarkably fertile, and indeed in so doing it has defeated its own purpose. The good results, the progressive results, became dominant in the first generation on Monday; they remained dominant all through to the fifth generation this afternoon; and the mixed results, the misleading results, did not make their appearance at all. The bad results, the recessive results, gave the lie even to our friend Mr. Bateson, falsified his statement, and made his three-to-one, or whatever his ratio is, disappear altogether. May I take it that this happy result is due to the fact that there has been unfettered activity on the part of each nation; that this Conference has not attempted to interfere or in any way to lay down lines as to what this observer or that observer, or this nation or that nation, should do? I myself do not believe in central international committees which mark out the work to be done by this or that person or nation. I believe that the development of differences is the true basis of unity, and that we shall arrive at complete unity, as we have already arrived at partial unity, by each going his own way. We talk of nations, but we here are all men and women of science, and science is cosmopolitan. Science bursts the narrow bounds of nations and tongues; it knows that what is for the good of all is for the good of each. It wants no formal regulations, treaties, tribunals of the Hague. We have Nature, that is to say Truth, as our great and our only arbiter; and she leads us, and is leading us, to that unity—the greatest unity—which in the good times to come shall embrace the whole world. I speak on behalf of the British delegates, and I use the expression "British" in no narrow sense. I do not confine it to England, to that country whose fickle climate makes gardening, as it were, a pious occupation, for it is said that when the gods see good men and women struggling against adversity their hearts are delighted; and that is what English gardeners are always doing. I include Scotland where, if they get better results, it must be because they use greater skill. I include Ireland where the blooms burst forth with Celtic expansiveness. I include Wales, but it is never necessary to welcome Wales, because (turning to Sir John Llewelyn) Wales is always to the front. I include modest Canada. Their native flowers riot in the freedom of Nature. I include Australia, I include the Cape, I include our great tropical and colonial possessions, so rich in great beauty and practical results. On behalf of all these I thank you for the toast which you have so cordially drunk.

THE BOARD OF AGRICULTURE, HORTICULTURE, AND FISHERIES.

Mr. W. Bateson, F.R.S., V.M.H. (President of the Conference), then rose to propose the toast of "The Board of Agriculture, Horticulture, and Fisheries." He said:—Sir Trevor Lawrence, my Lords, Ladies, and Gentlemen,—I have to propose a toast, but before doing that I must speak something of the thankfulness and the pride in my heart to-night. I have received at your hands, Sir Trevor, an honour which is to me quite overwhelming and utterly unexpected, to which I feel I have truly no right whatever. This is an honour given to those who have done benefit to horticulture. I cannot think that, in any way, what little we have done in Cambridge has yet benefited horticulture. In the future we hope to do so; but at present there is nothing, nothing I fear, which we can claim as having as yet been of benefit to horticulture. And then as to the grounds for our hopes for the future—what are they? How many of them have been annihilated in the past! Had it not been for the work that has been done by my friends and pupils—first of all by my colleague, Miss Saunders, whose name has been so deservedly honoured to-night—there would have been nothing at all to justify me in speaking of the significance of the work of inheritance; but for that vast reservoir of work they have piled together, I could never have dared, without that force behind me, to have asserted that Mendelian research has been and is of the importance that we now know it must possess.

I am h re to propose the toast of "The Board of Agriculture, Horticulture, and Fisheries," and to couple it with the name of Sir Thomas

Elliott. As I came into the hall to-night Sir Thomas Elliott said to me, "I hope you are not going to say ditto to Ray Lankester. Lankester has asked for ten millions annually to fight disease." Well, I *am* going to say "ditto" to Professor Lankester, though, perhaps, with some modifications. I suppose that in the spending of ten millions of money we should not altogether agree at once to the first suggestions made! Well, we might welcome or consider amendments. But if we could have ten millions—and I foresee that the day cannot be very long deferred when it will be recognised that a work like this is worth some millions—then I should ask that some part of that should be devoted to inquiries in heredity. Those who listened to Mr. Biffen's paper this morning must have felt that here we had one of the first solid facts that has ever been discovered respecting the inheritance of disease. That fact stands alone at present, but I am certain that if the work that is devoted to some commercial questions were devoted to the study of the inheritance of disease, we should very soon have recognised that inheritance was a science which would be amenable to experimental inquiry, and that the results of that inquiry would be of the highest possible value to the human race. I wonder if those around me know what we feel in our hearts when we talk of research and heredity. We believe it, and we feel it, when we say, that there is something that will come out of that science that will equal, if not exceed, in direct consequence, anything that any other branch of science has ever discovered. A knowledge—a precise knowledge—of the laws of heredity will give man a power over his future that no other science has ever yet endowed him with. I am not going to say that that knowledge is going to create the millennium of the human race; I only say it will change man's destinies profoundly—whether for good or evil the future alone will show! When man has discovered a power he always turns that power on. Man is a curious animal. He sees a machine, with all its taps, and he turns them on. I am confident that the results of this knowledge will be such as we in this room literally do not dream of. They shall change the human race in a way beyond what Mr. Wells in his wildest imaginations has conceived. That will happen when the whole nation wakes up to a knowledge of the laws of heredity. I am going to propose the health of the Board of Agriculture, Horticulture, and Fisheries. At this Conference the Department of Agriculture in Washington is represented by many distinguished delegates. I wish they would talk to Sir Thomas Elliott and tell him what the U.S.A. Board of Agriculture is doing. I have visited one of their sixty stations, and that one station alone is endowed with at least £20,000 a year. What are they doing there? Benefiting the farmers, no doubt; but here let me ask you to think for a moment how that amount of money could be spent here—how it might be spent to the advantage not only of agriculture but of every sociological interest, if its employment were not fettered, as unfortunately it is in America. There, all money spent on experimental stations is strictly devoted to economical objects. I fully realise the difficulties. The Board of Agriculture is not able to pursue any line of inquiry which does not get the farmers on their side, and which is not likely to be immediately convertible into bushels of wheat per acre or into thousands

Fig. 20.—William Bateson, F.R.S., V.M.H.,
President of the Conference.

of dollars! It is an utterly wrong use of opportunity to pursue such inquiries only. I am not going to advise the Board not to look for those thousands of dollars or those bushels of wheat; but you will get those, and more also, if you realise that science must come first, and the application of science afterwards. In this large hall I am not sure that I can make my voice heard, even so far as Sir Thomas Elliott; but I trust there are some who can hear what I say and will take what I say to heart. Science must come first, application afterwards. The science of heredity must be pursued in the same spirit that astronomers pursue their science. What is the economic use of knowing the orbit of, say, Halley's Comet, or the component gases of the Great Nebula in Orion, or the proper motion of the Pleiades *inter se*? What economic truth does astronomy teach us? Why, that the sun never sets on the British Empire! Any other? No! Yet who would dream of curtailing the resources of our great astronomical institutes and those all over the civilised world? Their inquiries are not pursued in the hope that they may be converted into thousands of pounds. We look forward to the future. I am confident that the time will come when the people of this country, and every other civilised country, will know, as we know, that vast achievements can be attained if only the money is given. And now, in conclusion, as to the Conference. We have had every imaginable festivity; we have made vast experiments not only in hybridisation but in digestion also, and, as M. Vilmorin has said, the book of tickets is nearly exhausted. But I believe, joking apart, that there have been seeds sown here that will bring forth good fruit in the future. I do not expect that I can live to see the days I am speaking of, but the younger generation will live to see those days; and in wishing prosperity to the Board of Agriculture we look to Sir Thomas Elliott, and to the head of his department, and to his subordinates, to help to bring that time to pass, and that quickly.

Sir Thomas Elliott, K.C.B., said:—Mr. President, Ladies, and Gentlemen,—I confess that it was with some trepidation that I, a mere Civil Servant, accepted your hospitable invitation to dine with so many men of science this evening. My trepidation would have been increased if I had known beforehand that the toast of the Board of Agriculture and Horticulture was to be discussed and proposed by so distinguished a man as Mr. Bateson. But I think I can say for the Department I represent that we do fully recognise the great work that Science has in front of her. Indeed, I would gladly associate the Board with every word that fell from Mr. Bateson himself. I think I may also say that the Board's officials have done something in the direction that he has indicated. We have no concern, you may say, with men and women, but we have concern with other races of animals; and I can say this, and I would ask critics of the Department to remember it, that we took steps in years gone by, first of all to discover the truths of animal pathology, and then we set to work to apply those truths in practice. And what has been the result? We have absolutely stamped out from this country some of the most serious diseases which had decimated our flocks and herds. Mr. Bateson has referred to the time when we should arrive at the millennium of the human race. At any rate I can claim for the Board that we have brought the millennium of the bovine race much nearer than it was. The diseases which used to carry off our livestock and other animals in this country—literally in their hundreds and thousands—are no more existent among us, and as one single example I would only refer to one disease stamped out altogether from this country, a disease which affected not only the canine race but the human race—the disease of rabies. I believe that the work done in extirpating that disease is a feather in the cap of the Board.

But although I feel tempted on this occasion to speak at great length on the work of the Department, I would much prefer to dwell on the work that has been done by this Conference. I wish to say on behalf of the noble President of the Board of Agriculture—the Earl Carrington—and on behalf of every member of his Department, that we value most highly the scientific work which has been done at this Conference. We naturally look to the economic side, and we firmly believe that you have it in your power to confer increased prosperity—increased economic prosperity—on all those who are engaged in the cultivation of plants of every kind in this country. Therefore we wish you most heartily God-speed in your work. We believe your work is capable of bestowing the greatest benefits, and that it will promote the happiness, prosperity, and welfare of mankind. I thank you for the way in which you have drunk the toast, and I assure you of our appreciation of the benefits which your researches have already conferred upon us, and of our good wishes for your further success and prosperity at all times.

The Right Hon. Viscount Mountmorres rose to propose the toast of "Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., President of the Royal Horticultural Society." He said: Ladies and Gentlemen,—It needs no words of mine to commend this toast to the enthusiasm of this or any gathering in this magnificent hall. When I came in Sir John Llewelyn reminded me of the proposer to a toast at the end of the evening who explained that he could say nothing because everything he had intended to say had already been said. That, I am thankful to say, is not my position. I could, if it were necessary, expatiate—even if, as our President has already said to-night, the twenty-four hours of the day were prolonged to thirty—on the merits of the subject of my toast, and on the reasons why, to-night especially, he is deserving of a most cordial and hearty reception at your hands.

You are aware—everyone in this hall, I am certain, is aware—of the magnificent work Sir Trevor Lawrence has done for the Royal Horticultural Society. Twenty-one years ago, when he took over the presidency of the Society, it had only 550 paying members; it was a Society without a home, and at that time with no very definite objects. But it was a Society of honourable and noble traditions; it had been one of the most energetic Societies devoted to the advancement of science; it dated from the earliest days of the nineteenth century; it had been active in sending out its collectors into foreign lands in the interest of that branch of science in which it was more particularly interested; it had had many honourable and distinguished men occupying its presidential chair. But it had at that time, about twenty-one years ago, fallen on evil days, and I think I am not exaggerating, speaking as a layman—I am not exaggerating in saying that the world recognises that it is very largely due to the energy and the ability, and to the untiring zeal of your President to-night—the

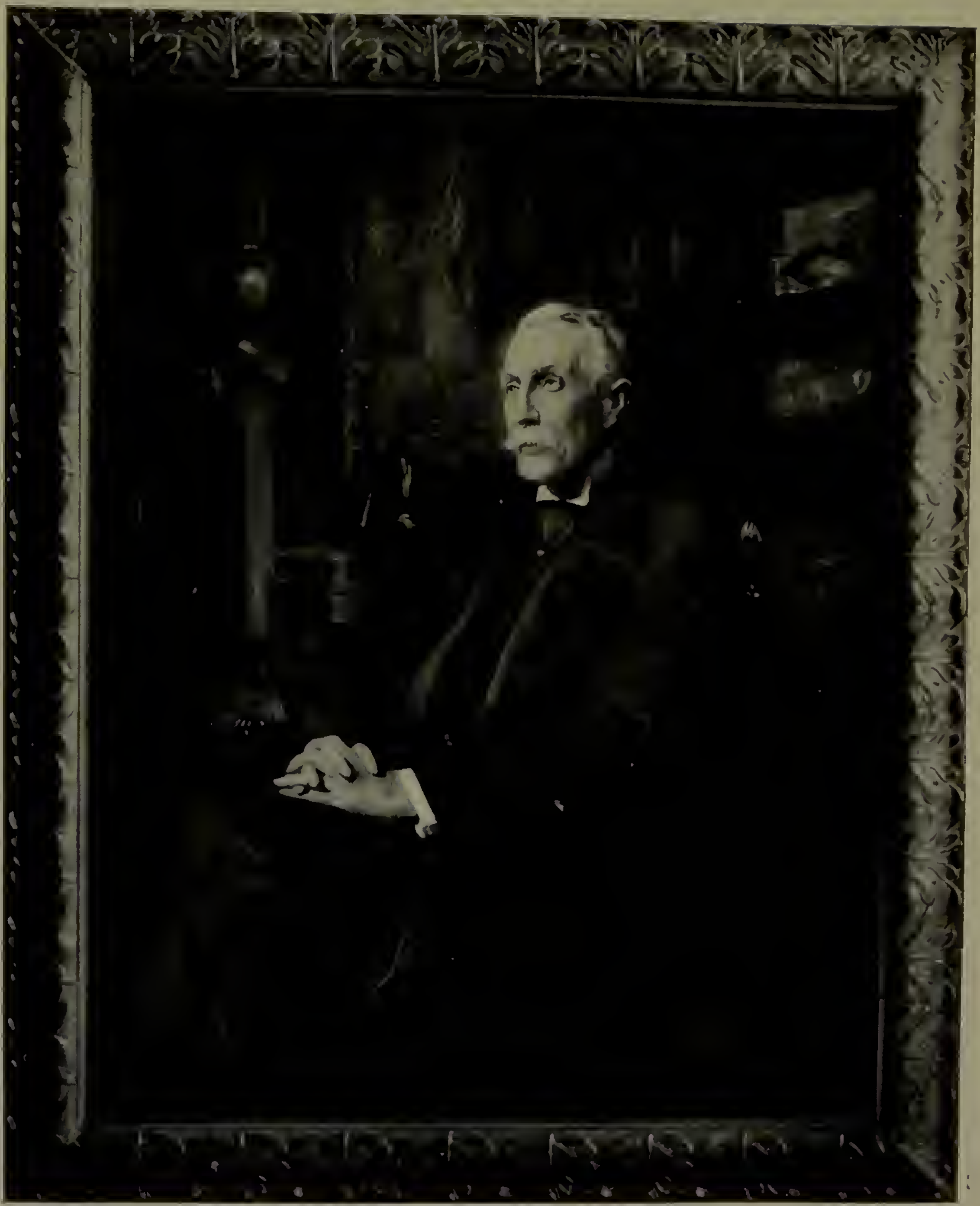

FIG. 21.—SIR TREVOR LAWRENCE, BARONET, K.C.V.O., V.M.H., PRESIDENT OF THE ROYAL HORTICULTURAL SOCIETY.

After a painting by Professor Hubert von Herkomer, C.V.O., R.A.

President of the Royal Horticultural Society—that the membership of that Society to-day runs, if I mistake not, into five figures, and that its income, I believe, is envied by nearly every other learned Society in this and in every other country to-day.

I hope—I am perfectly certain—that Sir Trevor will forgive me if I may be allowed to say in proposing this toast how delightful it is to see the whole of the triumvirate who were instrumental in bringing about the revivification of the Society gathered here to-night. I need scarcely refer to the presence of Sir Daniel Morris, the Hon. Treasurer of the reform movement of twenty years ago, and of Mr. Wilks, the perpetual and energetic Secretary. But to-night I am particularly glad in being allowed to propose this toast, because on this occasion the members of the Royal Horticultural Society have an opportunity of seeing the portrait of our President, which has been painted by Prof. von Herkomer, and presented to the Society in honour of your President's twenty-first anniversary of the occupation of the presidential chair. I feel that, in full sight of that portrait which looks down upon us with so vivid and so clear a representation of its original, I need say nothing further; for I am perfectly certain that it will prove sufficient for the expression of your heartiest goodwill and for your most cordial acceptance of the toast I now give you —the toast of your President, Sir Trevor Lawrence, Bart.

The President, who rose to respond amid constantly renewed bursts of cheering, said:—Lord Mountmorres, Ladies, and Gentlemen,—It has unfortunately been necessary for me during the sitting of the Congress to inflict something in the nature of a speech upon several previous occasions, and therefore I think you will probably forgive me for being very brief now. In the first place I must venture to disclaim that very large amount of merit for the resuscitation of the Society which Lord Mountmorres has been kind enough to ascribe to me. The fact of the matter is this. The love of the British race, both in the Home Country and the Colonies, for flowers and for gardens has of recent years grown very largely. Wherever you go—I do not mind where, it may be in town or country districts—you will find clear and distinct evidence that the British people are really heart and soul devoted to the innocent, delightful, and charming pursuit of gardening; and really to that is due very much of the success of the Society, which long, long ago gave up such matters as Badminton, bowls, and lawn tennis, healthful to body and mind as they may be, and devoted itself entirely, to do all that it could, to advance horticulture, both scientific and practical.

Well now, I think you will all agree that we owe a very great debt of gratitude to the foreign visitors who have come here, many of them at great inconvenience, to accept our invitation and to take part in the Conference; and all I can say is that more pleasant, more capable, or more charming guests in every way no Society could possibly have to entertain. And when M. de Vilmorin was kind enough to suggest that in the future our friends and neighbours across the Channel might be disposed to reciprocate the reception that we have been able to give them here, I am quite sure every one of us hopes he may be able to accept the kindness which the National Society of Horticulture of France would be certain to extend to us.

There are one or two absentees to-night whose absence we greatly—deeply—regret. I am not referring to the gentlemen who have been beguiled from the innocent pleasures of horticulture to Goodwood. I think there are some who would have been here had it not been for the gathering of the British Association at York. I think it is beginning to be clearly understood on all sides that unless we in this country take a little more interest in science we shall certainly be left behind in the world. Why are two of the absentees away to-night—Lord Balfour of Burleigh, an excellent gardener, and an active member of our Society's Council, and the noble Earl at the head of the Department of Agriculture and Horticulture, Lord Carrington? They are in the House of Lords discussing what is considered to be a most important measure. I do not know whether you ever heard of what a very distinguished philosopher—Herbert Spencer—said about the laws that our legislators pass. He said that of every hundred laws passed in this country 90 per cent. were absolutely injurious: of the remaining 10 per cent. six were neither harmful nor advantageous, and that the residue of four were very advantageous indeed because they were devoted exclusively to repealing Acts that had been passed by some previous Parliament. Therefore I venture to think that if those two noble lords had been able to tear themselves away from the House of Lords to-night, they perhaps might have been no worse, and we should have profited.

Some advances we have undoubtedly made in horticulture in the last twenty years, and I think the more we are able to take advantage of the great achievements Science is making, the more shall we prosper, and sooner or later I think we shall come to believe in this country, what I have so many times ventured to say, that the future is really in the hands of science. I beg to thank you most sincerely for listening to what I have had to say.

God Save the King.

THE VISIT TO GUNNERSBURY AND KEW, &c.

On Friday, August 3, the members of the Conference with their lady friends went on a visit to the country house of Mr. Leopold de Rothschild at Gunnersbury, and later in the day visited Kew Gardens. Conveyances for the accommodation of the guests left the Society's Hall in Vincent Square at 10.30. The guests broke their journey at the Natural History Museum, South Kensington, where they were most courteously received by Dr. Rendle and Dr. Smith Woodward. These gentlemen acted as guides—the former to the Botanical Section and the latter to the Geological Section. The foreign guests were greatly interested in the collections of the great Sir Joseph Banks, which are here stored in a gallery beautifully fitted up for the purpose. After a most pleasant leave-taking the guests entered their carriages and proceeded to Gunnersbury. Mr. Leopold de Rothschild himself was absent in Switzerland, but the guests were received by his son, Mr. Lionel de Rothschild. They then strolled through the beautiful gardens and were particularly struck with the quaint Japanese garden where there was everything to delight the botanist. The professors constantly made notes of what they saw. They then returned to the mansion, where luncheon was served in the double drawing-rooms.

Déjeuner.

Darnes de Saumon froid. Sauce Romaine.

Chaud.

Côtelettes de Mouton aux Champignons.
Poulets rôtis aux Cressons.

Froid.

Mayonnaise de Volaille.
Langue de Bœuf à la Gelée.
Salade.

Macédoine de Fruits.
Tartes de Fruits.
Gâteaux.

Glaces.
Café.

After luncheon Dr. Erwin Smith, of Washington, D.C., U.S.A., rose and said:—Mr. Lionel de Rothschild, will you allow me in the name of the foreign delegates of this great Conference to thank you, Sir, and your world-renowned father for the delightful courtesy and generosity which you have extended to us to-day, and which, great as indeed it is, is only a continuation of the wonderful hospitality we have enjoyed during our visit to England? I think many of us, at least those who have not been in this country before, have been very much surprised at the geniality and warmth and delicacy of it all. We have always heard much about your English homes, and we have been delighted to see them. We have also heard much about your landscapes, and now that we have seen them we shall carry away with us very pleasant memories of this gathering.

At the close of that delightful evening to which we were invited by the Horticultural Club, a scientist of European renown, and whose fame extends even to the States, was heard to exclaim, "I had no idea the English were so pleasant! I am *very* glad I did come!" That is a sentiment which I am sure all of us who hail from a distance cordially re-echo :—"I am very glad I did come."

Sir, we all trust that your kind and hospitable father and yourself and all the family will live long to enjoy this lovely home.

Sir Albert Rollit, LL.D., said :—I have just been asked, as a member of the Council of the Royal Horticultural Society, and in the absence of our President, Sir Trevor Lawrence, to say a word on behalf of the British guests of Mr. Leopold de Rothschild, Dr. Erwin Smith having eloquently expressed those of our foreign visitors. We Englishmen really owe our host our twofold thanks—not only for the entertainment of ourselves, but for the splendid reception and English home-like hospitality which he has extended to all the members of this Conference, which the Society has organised, and to the success of which Mr. de Rothschild has so greatly contributed, thus making even more fruitful the most able and untiring efforts of our Secretary, who has done so much to assure success in both the scientific work and also in the pleasurable recreation of the members of the Conference.

The Lucullan feast of ortolans and the flow of champagne has been splendid, and we are not like the Scotchman who, on his first visit to France, after tasting champagne, exclaimed, "Saundie, I do-ant ca-are a-bout these French min-e-ral waters!" But this is the lesser part of the service Mr. de Rothschild has done to the Society and its guests, though it is to be hoped the day is far distant when, for the sake of good feeling and good fellowship, dining will cease to be a fine art, and hospitality become only an ancient virtue. What is even more valued, however, is such a courteous and kindly reception and welcome of Britishers and foreigners alike at one of our great English homes, and the opportunity of seeing its most beautiful gardens—surpassing even those of Damascus—its artistic treasures, and its princely hospitality. This is not, on such an occasion, a merely personal or national service ; it is an international obligation. Such intercourse brings closer together the hearts and minds of mankind ; it broadens knowledge, thought, and feeling ; it awakens gratitude—which is the memory of the heart. More, it gives hope of that blessed time when men shall realise that they may do anything with bayonets—except sit upon them ; when the force of right shall supplant the right of force ; when the animosities shall perish, and the humanities only be eternal ; when the barriers shall fall down between nation and nation and be set up only between right and wrong ; when it may be said :

> The sheathed sword falls,
> And Peace, an Angel, folds her golden wings,
> And Commerce, smiling, calls.

May such be the new race which time and hybridisation shall produce for the world! Then, indeed, the "sports" of Conferences will not have been in vain, and for a day in this his *rus in urbe* we, and especially we of the Royal Horticultural Society and our foreign friends—with all whose many nations we Britons wish a fair and frank friendship for ever—are grateful to Mr. Leopold de Rothschild, whose absence we regret, while we appreciate Mr. Lionel's reception of us on his father's behalf; and, in the poverty of my own language to express fully my own feelings, and the feelings of all of us, I borrow from the wealth of our greatest poet—who is the poet of all civilised and cultured nations—to teach me to say all in a single verse:

> We can no other answer make but Thanks,
> And Thanks, and ever Thanks.

Mr. Lionel de Rothschild, in replying, said:—I thank you, Dr. Erwin Smith, and you, Sir Albert Rollit, for your kind speeches, and you, ladies and gentlemen, for the way in which you have received the toast of the health of my father and mother. My father is, as you know, quite unavoidably absent in Switzerland, but he asked me to tell you how sorry he is at not being here, and how pleased he would have been to have been able himself to welcome you and to show you the gardens, which he will be very glad to hear you have all enjoyed so much.

*** *** ***

In the other room the guests had speeches to themselves.

Professor Johannsen, of Copenhagen, said he expressed the feelings of all present when he thanked their host most heartily for the princely hospitality shown to them that day; indeed during the whole week they had had no feeling in their hearts but those of gratitude and pleasure. He then called upon those present to empty their glasses to the health of Mr. Leopold de Rothschild and his family.

Professor E. N. Hansen, of the United States, said:—Here, amidst these beautiful grounds, a masterpiece of the landscape gardener's art, we may say that the flower of hospitality is again in full bloom to-day. Landscape gardening is one of the fine arts: it is our soul's ideal of beauty expressed in terms of trees, shrubs, and flowers. In like manner we may say that true hospitality, as shown to us this week in so many ways, is brotherly affection in full bloom. The representatives of the civilised nations of the world have gathered together to compare notes on their efforts to make the world better by making the plants better, and with better plants we help to make firmer the foundations of a civilisation ever growing more complex. We feel, then, that we are doing a grand work for the advancement of mankind; and it is truly refreshing to us, as a body of faithful workers, to find such cordial welcome and appreciation as we have done all this week. Mr. Leopold de Rothschild, in honouring this Conference with such magnificent hospitality, shows that he, in the midst of a busy life, finds time to realise that our work deals with things fundamental, and that he himself is a true lover of the "Art that doth mend Nature." We thank our host, and in drinking to his health we hope that he, with his family, may live many years to enjoy this beautiful home and garden.

Mr. Robert Fenn, V.M.H., also joined in the vote of thanks to Mr. Leopold de Rothschild and his family. Although over ninety years of age, he said he hoped to see the Conference assemble in London next time. Only one thing he regretted, and that was that he could not address the Congress in all the languages of Europe. He had frequently had the advantage of visiting Gunnersbury. but he had never seen the gardens looking more lovely or more luxuriant. In conclusion he expressed the hope that Mr. Leopold de Rothschild and his family would long enjoy that beautiful home and gardens.

After luncheon the guests visited the adjoining gardens of Gunnersbury Park, also belonging to Mr. Leopold de Rothschild, and were then driven to the Royal Gardens, Kew, where they were most kindly received by Colonel Prain, F.R.S., the Director, and took tea with him in the grounds before visiting the gardens, which, with the magnificent plant-houses, excited the utmost admiration. After recording their thanks to Colonel Prain for so kindly providing them with tea and escorting them about the gardens, the guests returned to London, bringing to a close a very memorable Conference.

Fig. 22.—Gregor Johann Mendel.

From a photograph taken about 1880.

GREGOR JOHANN MENDEL.

By The Editor.

As this Report of the third Conference on Genetics will be read by many who either were not Fellows of our Society seven years ago, when we first published a translation of Mendel's remarkable "Experiments in Plant Hybridisation," or, if Fellows then, took but little interest in his work, which up to that time had attracted so little notice among even scientific circles as to be practically unknown, it may be as well to preface the Report of this Conference with a very brief account of Mendel and his work. And of his work I wish to speak in such popular language as may enable even an unscientific mind like my own to grasp the fundamental law which he discovered and used as a basis on which to build up the other conclusions at which he arrived.

Gregor Johann Mendel was born on July 22, 1822, at Heizendorf bei Odrau, in Austrian Silesia, his father being one of the smaller peasant-farmers of the district. In 1843 Mendel became a novice in an Augustinian foundation known as the Königinkloster, at Alt-Brünn, and four years later was ordained a priest. In 1851 he moved to Vienna, and spent three years there, studying physics and natural science. In 1853 he returned to the Cloister in Brünn, and was appointed to a mastership in the Realschule of that town. It was probably during his sojourn at Vienna that he became interested in the problems of hybridisation, for on his return to Brünn he at once began, in the gardens of the Cloister, that remarkable series of experiments with the common edible Pea (*Pisum sativum*) for which his name has now become world-famous. His work was difficult and, popularly speaking, most uninteresting, except for the fact that he possessed the almost prophetic perception that a general fundamental law (or laws) governing the results of cross-breeding might be thereby discovered. For more than ten long years he worked at his "dry" subject, and in 1865 he announced the results of it to the Society of Naturalists at Brünn, and his "Communication" was published in that society's Journal the next year. He does not seem to have rested content with this, but renewed his investigations, using this time *Hieracium*, *Lychnis*, and some of the common thistles. In 1869 he communicated to the Brünn Society a preliminary notice of his *Hieracium* experiments, and, being about this time appointed Abbot of his Cloister, he hoped, as he says in a letter to Nägeli (of which we give a facsimile with translation on pp. 88–89), to have had more time to give to his experiments, but for some reason or other this hope does not appear to have been realised, and he does not seem to have had opportunity to finish and elaborate his later investigations and discoveries.

It must not, however, for a moment be supposed that Mendel was a man of only one idea. On the contrary, his interests were very wide and varied. Meteorology was a favourite study, the various theories of sun-spots interesting him greatly. He is also known to have carried on experiments with bees, but their record, if ever written, has been lost; so that besides these two "Communications" and some "Meteorological Observations," Mendel (as far as is known) only published two other brief Notes, one on *Scopolia margaritalis*, and the other on *Bruchus Pisi*. He died at Brünn on January 6, 1884, practically unknown to the world, and it was not till 1900—sixteen years after his death—that his brilliant discoveries and patient work came to be known to science, and as soon as known appreciated. From 1866 to 1900 there is, as far as can be discovered, only one single passing mention of Mendel's work, and that without any suspicion, apparently, of its enormous value and importance. This total lack of appreciation is not easy to account for. Mendel's work was known to Carl Nägeli (to whom he wrote a series of letters) and to Focke, but they seem to have been unable to perceive the magnitude and far-reaching results of his discoveries. And this is the more surprising because during the latter half of the nineteenth century biologists, aroused by Darwin's work, were putting forth a multitude of theories to account for the observed facts of Heredity and Variation, and, as Mr. Hurst has so well expressed it, "Theories there were in plenty, but not one law"; and yet, had they only known it, the "law" which they one and all so sorely needed for their direction and guidance had been long discovered, and lay unutilised in the Journal of the Natural History Society of Brünn. It is almost a commonplace to say that patient workers in science, and benefactors who are in advance of the times in which they live, must not expect much recognition; but seldom, if ever, in the world's history has there been so striking an example of its truth as in the case of Gregor Johann Mendel. And the story of the independent and almost simultaneous discovery and experimental confirmation of his work, by De Vries in Holland, Correns in Germany, Tschermak in Austria, and the publication of a translation of his original "Communication" by our Society, reads like a romance.

It was the very simplicity of his experiments that brought him success. He confined himself to one single plant, the edible Pea, but used large numbers of that plant, and followed their behaviour through many generations, thus reducing his liability to error to a minimum. And not only did he confine himself to one single plant, but he followed and kept exact record of its simplest characters—such as "round or wrinkled seeds," "yellow or green seed leaves," "purple or white flowers," "tall or dwarf stems," and so on—and the observation and record of each of these pairs of simple characters was kept singly—independently of all the others. He cross-fertilised his peas once, and then, generation after generation, noted the result separately for each pair of characters.

For example, he took yellow-seeded peas which in previous generations had come true to type, and, similarly pure, green-seeded; sowed them, and fertilised the green with pollen from the yellow, and the yellow with pollen from the green. And he found that it mattered not which was the pollen-bearing plant or which the seed-bearer, the resulting seeds were in either case all yellow. He therefore called the colour yellow the **dominant** colour and green the **recessive**—because it receded from sight for one generation at least.

These yellow seeds were sown the following year and the resulting plants were allowed to self-fertilise, and it was found that all the plants in this generation bore seeds of *both* colours—green and yellow being often found in the same pod—proving that though in the first generation the green colour had *receded* from sight, it had still been present potentially in the offspring and came into sight again in the second generation; and not only came into sight again, but did so in an almost exact ratio, there being in this second generation a fairly constant proportion of one green seed to three yellow ones.

Continuing with these seeds he found that next year in the third generation all the green ones of the second generation, when self-fertilised, produced *only* green ones in the third, and they again only green, and so

FIG. 23.—GREGOR JOHANN MENDEL.

From a photographic group taken about 1866.

on until six generations had been proved with no trace of any reappearance of the dominant yellow ancestry: in other words, *the Recessive character bred true.*

But it was not so with the *yellow* seeds of the second generation; some of these when self-fertilised were found to produce both yellow and green seeds (often in the same pod) just as their parents had done in the first *

* Owing to the fact that the cotyledons (or seed leaves) in peas are embryonic (*i.e.* the result of fertilisation), the seeds (except the outer skin or coat of each seed which is purely maternal) borne by the plant belong to a different generation from that of the plant itself; *e.g.* a pure parent plant crossed bears hybrid *seeds* of the first generation, these when sown produce the hybrid *plants* of the first generation, which in their turn bear seeds of the second generation, and so on, the seeds always exhibiting the effects of hybridisation a generation in front of the plants. (In scientific terminology P_1 plants bear F_1 seeds; F_1 plants bear F_2 seeds; F_2 plants bear F_3 seeds, and so on.) In view of this I have ignored the plant generations (which are not essential in this particular case) and referred only to the seed generations (which are all-important).

generation from the cross. Of the yellow seeds of this second generation it was found that some bred true to yellow, and some not; the former, therefore, he called "pure" yellow, and the latter "hybrid" yellow; and he proved the pure yellows through several generations and found them to keep true, showing no reappearance of the green recessive ancestry. But the others—the hybrid yellows—produced, pure yellow and hybrid yellow and pure green, just as their parents had. It should, however, be borne in mind that the pure yellow and the hybrid yellow seeds are exactly similar to the eye, and only by sowing them and noticing their progeny can it be discovered which individual seeds are *pure* yellow and which *hybrid* yellow; but Mendel discovered by long experiment that though they are indistinguishable to the eye, yet the pure and the hybrid yellows bear to one another the proportion of one pure to two hybrid or impure. So that we get the following law of inheritance:—

Yellow × Green (or *vice versa*)

1st Generation	all yellow							
2nd Generation	3 yellow to							1 green
3rd Generation	1 pure yellow to	2 impure yellow to					1 green	green
4th Generation	pure yellow	1 pure yellow to	2 impure yellow to		1 green		green	green
5th Generation	pure yellow	pure yellow	1 pure yellow	2 impure yellow	1 green	green	green	green

and so on in a continuing series, the impure or *hybrid* yellow seeds, as they are called, producing with fair constancy a proportion of one seed in every four which will prove (on being sown) to be a pure yellow and one which will be seen to be green, the two remaining seeds being impure yellow, which will in turn repeat the same result as their immediate parent.

In other words, if you cross pure yellow and pure green peas either way—it matters not which is seed-bearer and which pollen-bearer—you will get all yellow seeds. If you sow these hybrid seeds, each will, if it germinates, produce a plant which will bear, say, forty seeds, thirty of which will on the average be yellow, and ten green. The green, if sown and sown and sown for countless generations, will always bear green seeds, true to the original green parents (barring the always possible intervention of insects). Not so the thirty yellow. These when sown will on the average produce ten plants bearing all pure yellow seeds, which will be constant and true to the original yellow parent for countless generations. The remaining twenty plants will be impure yellows, each plant producing on the average one quarter of their seeds pure yellow, one quarter pure green, and one half impure yellow, which last will repeat the process and proportion practically for ever.

Fig. 24.—Augustinian Cloister at Brünn, with the Chapel. On the left the Abbot's Lodgings, where Mendel lived.

FACSIMILE OF A LETTER ADDRESSED BY MENDEL TO NÄGELI.

The Original, which has been published by Professor Correns, was kindly lent by him for exhibition at the Conference.

Hochverehrter Herr

Empfangen Sie meinen herzlichsten Dank für die glücklich angekommenen Hieracien-Samen. Wie sehr bin ich Ihnen für diese freundliche Zusendung verbunden, und wie hoch muß ich außerdem noch Ihre Güte schätzen, da Sie auch eine Sendung lebender Pflanzen in Aussicht stellen! Ich werde alle Mühe anwenden, um die zwischen den verschiedenen Species möglichen Hybridformen zu Stande zu bringen, und falls diese fruchtbar sind, sollen auch ihre Nachkommen durch mehrere Generationen beobachtet werden. Nun eins muß ich Sie freundlichst bitten, nämlich die Ankaufs- und Transportkosten, so wie andere weitere Expensen auf meine Rechnung setzen zu wollen.

Mit lebhaftem Bedauern habe ich die Nachricht von dem Unfalle vernommen, der Sie hochgeschätzter Herr am 1 März betroffen hat, und freue mich aufrichtig, daß das Unglück nicht von den schlimmsten Folgen begleitet war.

In meinen Verhältnissen ist in der letzten Zeit ganz unerwartet eine vollständige Änderung eingetreten, meine Wenigkeit wurde nämlich am 30 März von dem Capitel des Stiftes, dessen Mitglied ich bin, zum lebenslänglichen Vorstande gewählt. Aus meiner bisherigen ganz bescheidenen Stellung als Lehrer der Experimental-Physik sehe ich mich mit einem Male in eine Sphäre versetzt, in welcher mir so manches fremd erscheint und es wird wohl noch einige Zeit und Mühe kosten, bis ich mich darin heimisch fühlen kann. Das soll mich indessen nicht abhalten, die mir so lieb gewordenen Bastardirungs-Versuche fortzusetzen, ich hoffe sogar, denselben mehr Zeit und Aufmerksamkeit zuwenden zu können, bis ich nun in meine neue Stellung eingearbeitet bin.

In meinem Versuchsgarten haben die Pflanzen im Durchschnitte gut überwintert, auch ist die Entwicklung

Sämlinge ziemlich weit vorgeschritten, die meisten
Piloselloiden und auch ein Theil der Archieracien
zeigen schon ihre Blattrosetten. Als gelungen sind
bis jetzt zu erkennen die Bastarde H Auricula
+ H Pilosella, H praealtum (Bauhini) + H aurantiacum
und wahrscheinlich auch H Pilosella + H. Auricula.
Von den Samenpflanzen des vorigjährigen Bastar
des H praealtum + H stoloniflorum (Autor.) haben
circa 100 überwintert. Bis jetzt sind diese (allerdings
noch kleinen) Pflanzen in Form und Behaarung
der Blätter von einander <u>nicht verschieden</u> und
stimmen mit der Bastard Mutterpflanze überein.
Ihrer weiteren Entwicklung sehe ich mit einiger
Spannung entgegen.

Mit der Versicherung der vorzüglichsten Hoch
schätzung zeichnet sich

Ihr ergebenster Freund
Gregor Mendel
Abt und Prälat des
Stiftes St Thomas

Brünn am 4 Mai 868

The same "law"—*i.e.* the same ratio of results—obtains also with other pairs of simple characters and with other plants (or animals), always premising that the original parents which you cross-fertilise are themselves pure strains, in respect of each of the different characters under consideration, and that after the first cross the resulting plants are self-fertilised in future (inbred in animals), and not re-crossed with a third strain—which obviously would bring in very difficult and altering complications.

This law of inheritance is by no means all that Mendel discovered, but it is the basis and foundation from which he pursued his further investigations, and which has enabled modern biological students to feel that their work rests on no mere theory, however ingenious or plausible, but on the unshakable premiss of undoubted natural law.

Translation of Mendel's letter to Carl Nägeli.

Dear Sir,—Accept my most hearty thanks for the *Hieracium* seeds safely received. I feel very grateful to you for this kind gift, and I highly appreciate your generous intention to send me living plants also. I shall make every endeavour to raise the various possible hybrid forms between these species, and if they prove fertile their posterity shall be studied for several generations.

It was with great regret that I received the news of your accident on March 1, and I heartily rejoice that it was not followed by very serious consequences.

A complete and most unexpected change has lately come to pass in my circumstances. My unworthy self was chosen on March 30 by the Chapter of the institution to which I belong to be its head for life. From my hitherto humble position as a teacher of experimental physics, I thus find myself suddenly translated into a sphere where everything is so strange that it will be only after much time and effort that I shall be able to feel myself at home. This, however, shall not prevent me from continuing the experiments in hybridisation which have now become so dear to me, and I even hope when I have got used to my new position to be able to devote more time and attention to them.

In my experimental plot the plants have got through the winter well on the whole, and they are now fairly forward; most of the *Pilosclloidca* and the *Archieracia* are already showing their flower-buds. So far the following crosses can be seen to have succeeded:—*H. Auricula* × *H. Pilosella*, *H. prœaltum* (*Bauhini*) × *H. aurantiacum*, and probably *H. Pilosella* × *H. Auricula*. Of the autumn seedlings of the hybrid *H. prœaltum* × *H. stoloniferum* (Autor) which was raised last year, about 100 have overwintered. Thus far these plants (still of course small) in both the structure and the hairyness of the leaves are *indistinguishable* from each other and agree with the hybrid mother-plant. I look forward to their further development with some eagerness.

Your devoted friend,
GREGOR MENDEL,
Abt und Prälat des Stiftes St. Thomas.

Brünn, May 4, 1868.

THE CONFERENCE.

When the Conference assembled on the first morning, Sir Trevor Lawrence, Bart., K.C.V.O., V.M.H., President of the Royal Horticultural Society, again welcomed the delegates. He said:—Gentlemen, it is scarcely necessary for me to make any remarks by way of introducing to you your President of the Conference. We all know him to be an eminent man of science, a Fellow of the Royal Society, and a distinguished member of the University of Cambridge. He from the very first made a study into the laws of natural inheritance his peculiar province, and in response to an invitation from our Society he has kindly undertaken to preside over this Conference, and has, I understand, prepared an opening address to which we are all now looking forward with the greatest possible anticipation.

Mr. Bateson : I am exceedingly sorry to have to announce that Dr. Camus, Laureate of the Institute of France, whom we had hoped to have present with us in person, is not able to attend our meetings ; but I am glad to be able to add that he has kindly sent his paper for our consideration. Dr. Camus has also sent besides his paper a very complete and voluminous list of European natural hybrids ; but so complete is it, and so voluminous, that I fear that even this great Society will not feel able to undertake the expense of printing it. It will, however, be here for reference, and it is most kind of Dr. Camus to have sent it.

One other preliminary remark I wish to make. It will have been noticed by many that unhappily our date coincides with that of the British Association. The proceedings of the British Association are taking place at York this and next week, and that, of course, is most unfortunate. Our date was chosen long before theirs was fixed, but they had to accept the date assigned to them by the city they were invited to visit. However, the goodly gathering we had last night and have again to-day shows, I hope, that our proceedings will not be seriously affected even by such mighty competition as that of the British Association.

THE PROGRESS OF GENETIC RESEARCH.

An Inaugural Address to the Third Conference on Hybridisation and Plant-Breeding.

By W. Bateson, M.A., F.R.S., V.M.H.

It is just seven years since, on the hottest day of a very hot summer, the first Conference devoted to Hybridisation and Plant-breeding assembled at Chiswick. Looking back on that occasion we realise what some of us even then suspected, that we were concerned in a remarkable enterprise. No such conference had taken place before, and our proceedings were of the nature of experiment. That definite results might come from that beginning we naturally hoped, but of those who endured the heat of that stifling marquee, or inspected the plants exhibited in that tropical vinery, not one, I suppose, anticipated that in less than a decade we should have such extraordinary progress to record. The predominant note of our deliberations in 1899 was mystery. In 1906 we speak less of mystery than of order.

When formerly we looked at a series of plants produced by hybridisation we perceived little but bewildering complexity. We knew well enough that behind that complexity order and system were concealed. Glimpses indeed of pervading order were from time to time obtained, but they were transient and uncertain. As casual prospectors we picked up occasional stray nuggets in the sand, but we had not located the reef, nor had we any machinery for working it if discovered.

Then came the revelation of Mendel's clue, with all the manifold advances in knowledge to which it has led. The most Protean assemblage of hybrid derivatives no longer menaces us as a hopeless enigma. We are sure that even the multitudinous shapes of the cucurbits, or the polychromatic hues of orchids—though they may range from one end of the spectrum to the other—would yield to our analysis. Methods for grappling even with these higher problems have been devised. The immediate difficulties are chiefly of extension and application. Thus the study of hybridisation and plant-breeding, from being a speculative pastime to be pursued without apparatus or technical equipment in the hope that something would turn up, has become a developed science, destined, as we believe, not merely to add new regions to man's knowledge and power, but also to absorb and modify profoundly large tracts of the older sciences.

Like other new crafts, we have been compelled to adopt a terminology, which, if somewhat deterrent to the novice, is so necessary a tool to the craftsman that it must be endured. But though these attributes of scientific activity are in evidence, the science itself is still nameless, and we can only describe our pursuit by cumbrous and often misleading periphrasis. To meet this difficulty I suggest for the consideration of this Congress the term *Genetics*, which sufficiently indicates that our labours are devoted to the elucidation of the phenomena of heredity and variation: in other words, to the physiology of Descent, with implied bearing on the theoretical problems of the evolutionist and the systematist, and application to the practical problems of breeders, whether of animals or plants. After more or less undirected wanderings we have thus a definite aim in view.

The suggestive impulse to which this great progress is due came from without, but we take pleasure in the thought that the London Conference, and no less the second gathering at New York in 1902, did much to ensure the vigorous response which that long-awaited stimulus received. Of those who have taken a chief part in the advancement of Genetics several were with us then, and to the interchange of ideas which ensued may be ascribed much of the keenness and solidarity of purpose with which the Mendelian clue was followed out.

Conferences, like other stimulants, are, I believe, beneficial if not indulged in to excess. There are, however, special considerations which make it desirable that people with our particular interests should occasionally confer. Genetics constitute a subject of vast range. Each worker can have experience only of some small part. Nevertheless the various phenomena are so closely interrelated that the centre of progress may shift rapidly from one part of the field to another. No one, therefore, can safely neglect the advances made in his neighbour's territory. Sciences follow the plan of developing organisms in that they pass through stages of little differentiation, when parts are still doing the work of the whole. In these early stages inquiry must be comprehensive. The worker must be wary of narrowness. While he is engrossed and perhaps lost in the idiosyncrasies of orchids a discovery may be made in regard to peas, or it may even be mice or lepidoptera, which is just what the orchidist requires to clear away his own obstacles. Not even the time-honoured distinction between things botanical and things zoological is valid in Genetics, and I notice with satisfaction that though we meet as guests of the Royal Horticultural Society, and though by the nature of the case plants figure most in the bill, yet animals are by no means excluded.

Now Conferences, especially those informal gatherings which are to make so pleasant a feature of our present programme, offer exceptionally good opportunities for the acquisition of knowledge of this comprehensive character. In the course of these meetings we shall gain information and suggestions that would not be attainable by months of search in the best ordered library.

There is another reason why the subject of Genetics is particularly appropriate to the deliberations of a Conference. I find this reason in the fact that practical and scientific workers here have equal need of each other's aid. I hesitate to add that they have equal prospects of benefiting from the partnership; for while it is clear that the mind of the practical breeder is stored with the experience that the physiologist requires, it is less certain that the practical man would recognise that the scientific experimenter had much of great value to impart to him yet. To this question of the practical evaluation of genetic discovery I will again refer, merely for the present noting the fact that two quite distinct classes of workers are interested in this one class of facts, and that such meetings give a capital opportunity for them to compare experiences and take stock of each other's progress. For the success of our meetings it is essential that neither the practical nor the more strictly scientific aspect should unduly prevail to the exclusion of the other. There is then abundant reason for our coming together, and it is not without due sense of the importance of the occasion that I have accepted the great honour of presiding over your deliberations.

In the few moments which I can now claim it is impossible to enumerate, and much more so to demonstrate, the genetic discoveries made by various workers, here and abroad, since last we met. Much of this information will be given in the papers communicated to the meeting. We have with us to-day several distinguished pioneers of these inquiries. We are looking forward to hearing them speak for themselves.

It seems to me, therefore, that I shall most fitly inaugurate these proceedings by attempting with the utmost brevity to state the position which genetic inquiry has now reached. The difference between the present and the former standpoint is well illustrated by taking two of the common ideas current among breeders and considering how each has gained in precision. The ideas I shall speak of are those conveyed by the terms "pure-bred" and "reversion." We have at last a critical appreciation of the physiological meaning of the term "pure-bred" as applied to a plant or animal. In a general way every breeder is familiar with the notion that some animals and plants are pure while others are not. We have long been accustomed to distinguish the two conditions in various ways—estimating purity sometimes by truth to parental type, sometimes by the uniformity of the offspring. Neither of these tests, as we now know, is valid. An individual may be impure though not sensibly different from the accepted type of its breed; and though continued breeding from an impure individual will probably in the end reveal impurity, yet several generations may be produced in succession without any such indication appearing. For example, if in a rose-combed breed of fowls that had bred true for generations, a single-combed bird were to appear, we might formerly have supposed either that one of the parents was impure, or that a new variation had occurred. We now realise that the introduction of the single comb may have taken place in some generation indefinitely remote, and the appearance of that feature in a perceptible form is due simply to the fortuitous meeting of two germ-cells bearing the recessive character.

An individual is *pure-bred* when the two cells, male and female, from which it develops, are *alike in composition*, containing identical elements or characters. No long line of like progenitors is needed to produce a pure-bred plant. A purple sweet-pea may, as we now know, have been bred from white grandparents exclusively, and yet be pure to the purple character. Conversely, a white sweet-pea may be a seedling produced by the self-fertilisation of a purple-flowered plant, and yet be pure-bred in respect of whiteness. It matters not how the parents are bred. They may be mongrels, as heterogeneous in composition as packs of cards; but if from the two packs *similar* cards happen to be dealt, the product of these two cards is pure. And as in the cards we may consider their attributes of colour, suit, and number as distinct, so in the living thing we know that the several features or physiological characteristics may be treated as distinct in the cell-divisions by which the germ-cells are formed.

From this separability or distinctness of the characters it follows that an organism may be pure-bred in one respect and cross-bred in another. I need not remind my present audience that this conception of the unity and distinctness of characters provides the solid foundation which makes the science of Genetics possible. Instead of regarding genetic purity as a vague and problematical state which might or might not be attainable by a long course of selection and fixation, we now know exactly what it is and how it is produced.

It is evident that this is a piece of knowledge which the practical breeder can turn to account. In future he will work with individuals of tested composition and avoid masses, thereby greatly simplifying the work of selection and fixation. It is no exaggeration to say that in this branch of industry the breeder can now perform in four years what formerly he could scarcely have effected in twelve.

Take similarly the idea of Reversion, which was formerly invoked to account for the unexpected or the unwelcome, much as our ancestors appealed to the powers of evil. Reversion, as usually met with, is one of two very definite but quite distinct things. Commonly these recurrences of characters the breeder supposed he had bred out are merely due to the reappearance of a recessive character. Like the single comb spoken of above, these recessives never get the chance of appearing until they are introduced into the organism simultaneously from both sides of its parentage. A proof that any given reversionary character is merely a recessive can be got at once by observing that the reverting individuals, on being fertilised with themselves or with their like, will breed true, and at least will not reproduce the types from which they were extracted.

But in addition to this very simple sort of reversion there is another of a more complex and much more instructive kind—that which is generally known as *reversion on crossing.* The most familiar illustrations have been seen in pigeons, fowls, sweet-peas, and stocks. This reversion to an ancestral form, which may be indefinitely distant, can occur even when types of absolute purity are crossed together. Such reversionary forms, unlike those first considered, *never* breed true in the first generation—the F_1 generation, as we call it—but in the F_2 generation there must in all ordinary cases be a small but definite percentage of reversionary individuals which are then pure-bred and thenceforth able to breed true. As we now can prove, the reappearance of the ancient characteristic is caused by the meeting together of distinct elements, long parted. In some unknown way these two factors "let each other off." Both factors must be present together in order that the feature in question may be developed.

The most complex illustration yet known of the effects of interaction between factors is provided by the ten-week stocks investigated by Miss Saunders, where, as we now know, an independent factor must be present in the plant to produce hoariness in the leaves ; but even if this factor is present, the leaves are still glabrous unless it is also associated with the two other factors which are concerned with the production of flower-colour. How much further such analysis can be carried it is impossible to surmise. We see, as yet, no reason for supposing that the rules of inheritance now perceived in the case of the simpler properties or structures of animals and plants, are not applicable also to the features we regard as higher.

There is also a special kind of reversion on crossing made famous by Darwin's experiments on pigeons. Here the reversionary type is often not perceptible in F_1—the first cross-bred generation—but appears first in F_2 when the F_1 birds are bred together. Such a phenomenon has been made the subject of experiment by Mr. Staples-Browne, and, as his results clearly indicate, the reason why the reversionary character, viz. the black barring on a blue ground, does not appear in F_1 is that this feature is obscured by the dominant blackness introduced by one of the parents. When the factors which produced the blue meet in F_2 birds, which do *not* also contain black, the Blue Rock colouring is then evident.

Such a case as this last is only an apparent difficulty. Nevertheless I should warn you that there is a large class of alleged reversions, of a kind more economically important than these, arising in ways not yet properly understood. I allude to the appearance of reversionary "rogues" among seed-crops, where circumstances preclude the idea that we have to deal with mere recessives, and make it *prima facie* unlikely that crossing is the provocative cause. For example, in the case of peas, such reversionary and wild-looking "rogues" with *round* seeds have been shown me by my friend Mr. Arthur Sutton amongst crops of highly bred wrinkled peas. They are regarded as indications of that general degradation or degeneration which it is supposed would permeate all highly bred stock if selection were suspended. Now, though it is certain that in practice if the crops were neglected these hardy and productive "rogues" would soon prevail and overwhelm the pure and more delicate strain, we are no longer content to regard their presence as inevitable. In order to cope with them we must find out exactly what they are. By the strict method of breeding from individuals under proper precautions we have now the means of doing this, and not till such investigations have been made need it be regarded as the inevitable property of any high-class variety to produce "rogues." Though as to this special case I make no prophecy, modern observations strongly suggest the paradoxical conclusion that there is no such thing as general degradation or degeneration. These phenomena are due to specific causes, most commonly to nothing more obscure than insect-crossing, or to unsuspected mixture with an unrecognised variety. I mention these things simply to illustrate the fact that though the precise physiological nature of reversion may seem a matter remote from practical life, it is not remote at all, but closely bound up with very important industrial considerations.

I have said that reversion on crossing is due to the meeting of long-parted factors. Conversely, variation is often due to the separation or elimination of factors. In other cases it is almost certainly, though perhaps not quite certainly, due to the addition of *new* factors. Genetic research has thus provided the first indication of the physiological process which results in the birth of a variation. The consequences of this knowledge to the systematist and to the science of evolution I will not now pursue. By following the clue which the discovery of unit-characters has provided, the long range of phenomena first grouped in an orderly fashion by Darwin in "Animals and Plants under Domestication" can at last be subjected to precise inquiry. The proximate significance of many of these mysteries is indeed already made out. Only those to whom that treatise has long been a kind of "De Occultis Naturæ Miraculis" are able to appreciate what the new knowledge means to biological science.

Now once more as to the practical importance of all this. The breeder has two main objects in view: he wants to *create* novelties and to *fix* them. In the second of these objects he can, as we have already seen, expect help from Genetics. As regards the creation of new forms I must not speak so confidently. Nevertheless, there is a valuable class of novelties which are really novel only in so far as they recombine pre-existing characters of known types. Such recombinations, say of hardiness with desirable qualities of colour or shape, or of size or free-flowering habit with brilliancy, or of colours such as red and cream-yellow, belonging to distinct physiological systems, may be of great value to the breeder. If in the majority of such cases no infallible prescription can yet be given to produce the desired result, Mendelian knowledge often indicates the course which is most likely to succeed. I am not straining the truth when I say that the right course in numbers of instances is one which an operator guided by common-sense alone would have studiously avoided.

But, apart from any specific claim as to the immediate economic value of genetic research, let me again take more general ground, and without fear of contradiction I will insist on the truth that with a critical knowledge of the meaning of "pure-bred" and "reversion" a new era begins. To confusion and guesswork, knowledge and orderly experiment succeed.

The conclusions I have named and others like them have been arrived at by statistical observations of a somewhat arduous kind. An account of these technical proceedings scarcely falls within the scope of this address. I must, however, dwell for a moment on the fact that the processes of segregation which bring about the outward and visible facts of heredity are in essence *symmetrical* processes. It has long been known, ever since the beginning of microscopical research, that cell-division often appears to be a symmetrical process. We have now learnt that this visible symmetry is in the main a true representation of the qualitative symmetry by which the qualities or characters are distributed among the developing germ-cells. No one can yet declare that plans of distribution following some higher order of complexity do not exist; but analysis of the simple cases will keep us employed for many a year, and not till the symmetrical phenomena of heredity have been exhaustively explored can we contemplate a further expedition into the unknown. Of one thing at least we may be sure: that heredity is a regular phenomenon, in many of its manifestations simple and amenable to experimental methods of research. To have said as much in 1899 would have been only to make a pious ejaculation of personal faith. Before our meeting in 1902 the change had begun. We could deal with simple cases involving only two types of individuals. When a family contained on an average three of one type to one of another, or equal numbers of both, we knew what the fact meant. Now we can deal with much more difficult cases. The number of types does not trouble us. We understand the ratios 9 : 7 and 9 : 3 : 4 and 27 : 9 : 28, with many variations on these simpler themes. All these can be shown to be produced by the chance combinations of germ-cells or gametes produced by symmetrical divisions.

But ever in our thoughts the question rings, what *are* these units that bring all this to pass, which in their orderly distributions decide so many and perhaps all of the attributes or faculties of each creature before it is launched into separate existence? Colour, shape, habit, power of resistance to disease, and many another property that might be named, have one by one been analysed and shown to be alike in the laws of their transmission, owing their excitation or extinction to the presence or absence of such units or factors. Upon them the success or failure of every living thing depends. How the pack is shuffled and dealt we begin to perceive: but what are they—the cards? Wild and inscrutable the question sounds, but genetic research may answer it yet. Substances which excite disease or confer resistance, which preserve health or produce deformity, have been extracted, and it may not be more difficult to determine the nature of those critical factors which excite hoariness or colour in a plant.

It is not only the breeder of animals or plants who is concerned to know the truth about heredity. The results of genetic research affect every branch of physiological or sociological inquiry. Too long has science been content to explore the specialised and outlying functions of the body and to neglect the primary, central, and all-controlling function of heredity, on which the rest depend. Such neglect manifestly arose from the curious delusion that the laws of breeding were untraceable. With the Mendelian renascence that dark age has ended.

As a hopeful sign it may be noticed that in the United States the Carnegie Institution has lately equipped a station for the experimental study of evolution. In England, where through Darwin's genius the study of evolution first became a reality, the country in which the art of breeding has for ages occupied a place unequalled in other lands, no such opportunities exist. The pursuit of these objects demands facilities of a special kind, such as neither technical colleges nor the laboratories of the Universities are able to supply. Sooner or later, perhaps, an effort will be made to provide equipment of this kind in England. Whenever such an institution as that I contemplate comes to be designed, let it not be tied down to the pursuit of directly economical results. When someone says to me, "But can't you breed a Derby winner, or do something useful?" the reproach does not break my heart. In parenthesis let me remark that though, in the attempt to discriminate among animals all good enough to win, science may be as much at fault as common-sense, yet it would not surprise me if science were to devise a way of breeding even race-horses which would not produce about a hundred wasters for one fit to win—and yet I understand that common-sense remains content with that rather modest attainment after two centuries and a half of steady trying.

The great advances in the application of science have generally become possible through discoveries made in the search for pure knowledge. Mendel's incomparable achievement, with all that it imports both to science and to practice, was brought about by the resolute determination to get to the bottom of one particular problem in hybridisation, a problem, too, without any very obvious practical concern, and we may rest assured that in no other spirit can natural knowledge be more profitably pursued.

DOES HYBRIDISATION INCREASE FLUCTUATING VARIABILITY?

By PROFESSOR W. JOHANNSEN, University of Copenhagen.

THE problem of heredity is the subject of very diligent study at the present time. Two different methods of investigation have been followed by workers, viz. the statistical method and the experimental method, but the results of these two methods do not always seem to agree. And yet in reality agreement must be found.

Pure statistics in this matter provide a dangerous and uncertain method, not only because the special data are very seldom controllable, but especially because (granting their inherent correctness) a scientific biological analysis of such data cannot be made: it remains quite uncertain whether the numbers in question contain a multitude, or a few, or only one single "sort" of organisms—"biotypes" as I have called them (1).*

In many organisms heredity can only be investigated by the statistical method, as for example in the human race, where experiments are impossible, and in many of the larger animals, such as horses, &c. In all such cases the research is limited to the indications of genealogical tables, stud-books, &c. But such materials are not at all qualified to form a basis for an exact inquiry in heredity. For this purpose data are required which can be controlled, and which are sufficiently specialised to enable them to be separated into different groups from various points of view and in such ways that a true biological analysis may be made in each special case.

The imposing display of mathematical knowledge and refinement with which the "Biometrical School" has dazzled our eyes really proves ineffectual for the true understanding of the physiological laws of heredity, when the mathematical treatment is not based upon an accomplished sorting of the special facts and a biological setting-out of the premises which are to be treated. The most prominent biometrician, Professor Karl Pearson, has in all his work in this biological domain proceeded as if his motto were: "There are no premises; all is treatment!" Indeed this very expression was once flung out against me in a private discussion with a biometrician. Neglect of premises—in a degree quite inconceivable to the experimenting biologists—is the Achilles-heel of biometry, and the whole Biometrical School is therefore standing on very unsafe ground as to the biological value of its results in heredity.

What mistakes and absurdities the neglect of the premises has introduced into the literature of heredity must be known by all who have taken notice of Mr. Bateson's criticisms on "Homotyposis" (2), or—not to go further into polemics—who have seen the recent

* The numbers refer to the list of literature at the end of the paper.

important little paper by Mr. Hurst (3) concerning the heredity of coat-colour in horses.

Darbishire's (4) change of front with regard to the interpretation of the Mendelian laws shows the awakening of a better understanding as to the value of pure statistics for biology.

It is beyond all doubt that statistical methods have very great importance in many points of research in heredity, but the *conditio sine qua non* is, as always, a previous competent sifting and arrangement of the data to be used. The questions which interest us in heredity must be formulated biologically, if an answer, biologically applicable, is to be given. But this point has been almost totally neglected by the biometricians.

If anybody makes a study as to the speed of railway-cars, he will of course regard every train or every type of train separately: express trains, local trains, goods trains, and so on. He can then collect details and statistics needful for understanding the traffic as a whole, the train-types, &c. But what would be said of an inquirer who, for solving the problem, collected statistics as to the speed of the different carriage-classes, first, second, and third class, and by this method found out that the average speed of the first-class car was much greater than the average speed of the third-class car—for in the express trains (on the Continent at least) there are only, or almost always only, first and second class cars, while in the local trains the third-class car is in the majority. The result of these statistics would certainly be a truth also, but it would be without any real interest: indeed it would be quite misleading as to matters of railway traffic. I must confess that the main part of biometrical work in questions of heredity somewhat resembles such preposterous statistics.

The mathematical terms in which biometricians have tried to express the "ancestral influence" may in reality be a true result of statistics; but in these statistics the data have mostly not been analysed in a biologically reasonable manner. It is much to be regretted that biometricians, although fairly compelled by the force of argument to see the faults of their premises, still persevere in their "antibiological" proceedings. They seem to confound the statistics serviceable for insurance purposes and also possessing great scientific interest for social questions with the exploration of fundamental laws of biology or physiology.

In the science of biology the rediscovery of Mendel's laws, and the highly important development of Mendelian researches in the last few years, have entirely displaced the general biometrical conception of ancestral influence: it is now evident that in Mendelian cases not the personal qualities of the ancestors but the nature of the zygotes is the essential factor in heredity—and the nature of the zygote is not a mere function of ancestral qualities. Statements of averages are here, of course, without value for the experimenting biologist.

The inadequacy of the assumed "ancestral influence" is now granted by all biologists who in their breeding experiments are operating with "traits" which are characterised qualitatively. All the famous Mendelian examples from peas, the results of Correns', Miss Saunders', Tschermak's and De Vries's experiments (with plants), Bateson's, Darbishire's, Guaita's, Hurst's, Lang's and others' experiments (with animals) are so plain and clear just because the characters in question are "qualities."

The problem whether the Mendelian segregation is absolutely pure is a matter of a special nature, giving no loophole for the biometrical view of ancestral influence. By impure segregation, when a small quantity of "substance which ought to have been cleaned out" is carried over with the gamete, and finds conditions in the zygote for increasing, impurity may be increasingly augmented. And at last it will become manifest in some individuals in a generation possibly very far removed from the ancestor in question. I have made some experiments on this point, but this is not the place for discussing such matters more closely.

Now I come to the domain in which the stronghold of biometry is situated: the traits which are characterised quantitatively, the types that manifest themselves as differences in degree. Here we meet with the greatest difficulties; here we cannot by simple inspection of any individual decide its type. Here we meet with the "transgressive variability," which makes it quite impossible to judge by inspection whether an individual specimen is a plus-variant of a "little" type or a minus-variant of a "large" type, and so on.

The most important and conspicuous results of the Mendelian experiments relate to traits that do not blend, and with regard to which every simple individual can be grouped in the right class immediately. The results are therefore very striking and well fitted for popular demonstration. In De Vries's celebrated studies of mutations it is almost always such qualities which are regarded; the same may also be said about the extensive and important experiments carried out at Svalöf, in Sweden (5). Here "botanical" characters are almost exclusively regarded, *i.e.* unmistakable morphological characters, which—as De Vries has said—are "traits not of fluctuating but of mutative nature." These morphological characters are constant except when mutations suddenly give rise to new types. That the pure "pedigrees" of Svalöf in reality have constant types in respect of the quantitatively characterised traits—which give the crops their value—is for me a matter of course, and is also asserted by Professor Hj. Nilsson, of Svalöf. But conclusive scientific researches about all such highly fluctuating characters have not yet been made at Svalöf, where the excellent special workers with good reason have taken "botanical" characters as starting-points for their isolation of types.

The study of heredity as to characters, which by inspection can only be estimated as differences in intensity of the same quality, and which blend in hybridisation, requires special methods. The hybrids with such characters have not yet been examined in a satisfactory manner. In my experiments with "pure lines" (6) I particularly tried to isolate quantitatively different types from the population in question, and in that way I —as the first, I believe—found out that the Galtonian law of filial regression, declaring that fluctuations are to a certain considerable degree hereditary, is quite wrong and only depends on the presence of several different types in the populations. In a population containing only one single type the selection of fluctuations has no action at all! The just-mentioned famous Galtonian law should hereafter—if my view has a general bearing—only be the statistical expression for the circumstance that populations mostly are mixtures, containing different "biotypes." Galton's law is then only a statistical law, but not at all a true biological law. My researches, which have been of no short duration, have given me a very considerable stock of facts in full accordance with this view, thus forming a supplement to the Mendelian and Svalöf experiments as to the appreciation of the effects of selection. And as to my researches we stand upon that ground—quantitative studies—on which the still prevalent conception is based: *that selection is able to shift a type in the same direction as that in which the selection of its fluctuations is carried on.*

This conception, which I regard as absolutely erroneous, involves the idea that evolution proceeds through continuous variation.

Biological study of the behaviour of the traits that are qualitatively characterised, as in the classical examples of Mendel, does not usually require special mathematical treatment beyond some little calculation of probabilities. But when we attempt researches respecting quantitatively characterised traits, or, it may be, the fluctuations of qualitative traits, we must use the armoury of collective-measuring statistics. Here we find that a long series of prominent mathematicians have worked out methods of computation and other devices. From Gauss and Laplace through Fechner, Quetelet and Galton to Thiele, Lipps, Pearson, Bruns, Kapteyn, Udny Yule, Charlier and Davenport in modern times, the theory of exact observation has been developed and enriched with instructions for the treatment of collective series of measures. As to the finer methods the mathematicians are not at all in accord, and the biologist eager to learn from them is too often a witness to very sharp discussions between mathematicians as to the finer fitting of the mathematical implements which are offered to us. I cannot say that the nature of these discussions gives special reasons to regret that most of the biologists are not able to follow those finer methods in question. And, indeed, even the five or six special equations and formulas for different types of frequency-curves elaborated by Pearson are not of much use for biological students. Here I suppose that Charlier's (7) simplification of the computation, giving only room for two different types of curves, represents a real progress. But also these formulas and equations are too complicated for general biological use; and perhaps future mathematical speculation will give us simpler proceedings.

After having tried to understand the fundamental principles in the publications of Thiele (8) and Charlier, and after studying Davenport's "Statistical Methods" (based especially on Pearson's important work) (9), I suppose that the biologist can satisfy the claim to exactitude without too much trouble in all those cases where the different characters are to be regarded independently. In the case of correlated variability some greater complication is needed. When only one character is to be regarded at a time it is sufficient—and may be said to be necessary—to compute the mean value (average) of the variants, the standard deviation, and, as expressions for the total shape of the frequency curve, two coefficients, the one giving the asymmetry or skewness of the curve, the other giving what Pearson calls the "excess," *i.e.* indicating whether, and in what manner, the curve surpasses the limits of a binomial curve, skewness not regarded. Of course the total number of observations, n, must also be given. With these five indications the fluctuating variability of a stock as to the traits in question should mostly be sufficiently characterised.

The computation of the mean (average, A) and the standard deviations (σ) are well known. The skewness will be determined by the average value of the third powers of deviations from the mean ($\mu_3 = \Sigma\,(x^3 f) : n$; see Davenport, "Statistical Methods," 2nd edit. p. 116. By complete symmetry $\mu_3 = 0$). As the simplest coefficient of skewness the relation $\mu_3 : \sigma^3$ may be regarded; this expression being absolutely independent of any theory of variation. As empirical skewness, therefore, can be indicated $S = \mu_3 : \sigma^3$. As to the "excess," it must be remembered that the average value of the fourth powers of deviations from the mean ($\mu_4 = \Sigma(x^4 f) : n$) shall in case of the normal binomial frequency curve be $\mu_4 = 3\sigma^4$. Hence $\mu_4 : \sigma^4 = 3$ indicates that there is no excess. Therefore the formula $E = (\mu_4 : \sigma^4) - 3$ gives the value and the sign of excess. This value E is, like S, an abstract number and also absolutely independent of any theoretical view of variability.

As to the method of computation, I must refer to the highly practical computation scheme of Charlier, with its excellent controlling system. For the suggestion to limit the computation to the estimation of E and S without following any hypothesis of different types of variation-curves I am indebted to Thiele.

When the biologist in this way is content to use these simple mathematical methods, the legitimacy of which is granted by all authorities, he is able to characterise his series of variation in a manner which gives a very good description of the variability. It is still a desideratum to determine how good the accordance—as to S and E—may be between different series of the same organisms, *e.g.* the sections of the same pure line in culture in the same garden, &c. The variations in the environments may here give greater disturbances than in respect to the standard deviation. Special researches on this question have been commenced.

As to biological questions concerning heredity and fluctuating variability, it must again and again be emphasised that to procure the facts is the most important but most difficult point in the whole matter. To gather materials from forests, fields and gardens, or—as to man—to send inquiry papers to families, schools and other institutions, may be good for many purposes of social statistics, but it is a quite fallacious method for biological research into heredity questions. And it is a fundamental error to believe that the inspection of variation-curves and correlation-tables can give any certainty to conclusions as to heredity in the true biological or physiological sense of this word. Pearson has, not only in working out his ideas of homotyposis, but perhaps still more by his recent researches (10) in the mental character of school-children, totally omitted to analyse the causes which may be the condition of greater resemblance between brothers and sisters than between children in general. In my own materials of beans I have observed a much greater resemblance between sister-beans than between other beans of the same pure line, and yet all these different individuals (or homotypical organs) have the same value when judged by the offspring's qualities. Here the special resemblance between brothers and sisters has nothing at all to do with heredity as defined by the characters of the zygotes or gametes. I cannot here enter into this matter, but it was necessary to point out this recent biological fault (confusion of social problems with biological—as always by Pearson) because it is important to demonstrate how this very excellent mathematician errs when dealing with biological questions of heredity. Nay, heredity can only be studied in an exact manner by breeding experiments, and here in two ways—analysis and synthesis. The analytical experiment is in its clearest and purest form carried out by working with "pure lines," *i.e.* individuals descending from one single homozygotic individual. Pure lines are only to be had in organisms with self-fertilisation (or parthenogenesis); multiplication by graftings, cuttings and other forms of vegetative propagation can here be left out of sight.

"Pure line" is a mere genealogical term; different authors have unfortunately misconceived this meaning, and confounded "pure lines" with "types," "small species," and other such things. I must energetically protest against this misrepresentation of my term "pure line." It indicates nothing more than the warranted purity of descent. By mutation or segregation new types of gametes can be formed within pure lines as well as in genealogical hybrids—the line remains notwithstanding as pure as before in the genealogical sense. Pure lines, therefore, can be monotypical or bi- and polytypical. When we only have quantitatively determined types in view, we may express the fact by the words mono-, bi- and polymodal pure lines. Hitherto, I have only published a few of my researches in monomodal pure lines. As my work is proceeding I hope to publish the results of experiments with bi- and polymodal "lines," the behaviour of which in some points may have resemblance with the segregation in genealogical hybrids. Such occurrences having been found in pure lines seem to me to have a special and peculiar interest, affecting also the cytological problems of heredity. The time at my disposal does not allow me to enter into this matter here.

Experiments with monomodal pure lines have shown me that Galton's law of filial regression (in all those cases where this law has been analysed by means of pure lines) is only a consequence of the fact that the populations in question contain different types of organisms. And this composite character of a population cannot be recognised by inspection or any computation of the variations! Selection acts in all such cases apparently as a type-displacing factor; in reality, selection has no altering influence as to the nature of the existing biotypes. Selections act only as sorting factors, more or less perfectly isolating that type or those types which differ most from the average of the population.

The continued researches which I have carried out during the last four years have only confirmed this view, and it will be seen that this is in accordance with the practical experiences from Svalöf. I have tried to find special cases where an effect of selection could be recognised, but in vain. Thus it might be supposed that special selection of the very smallest seeds would give weakly plants, the seeds of which in their turn would be badly nourished, and therefore small; but even this reaction (which must not at all be confounded with heredity) has not been observed with any degree of certainty in my experiments. Small plants gave a less number of seeds—that was all. I hope to be successful in finding such action of selection in pure monomodal lines—in being able to demonstrate the difference between such secondary effect and a veritable type-alteration. It may be that such "spurious" type-alterations are more frequently to be found in breeding experiments with animals. I never heard about them; but they perhaps may have been present in some of De Vries' (11) cases of selection experiments combined with over-nutrition. Unfortunately De Vries' materials have not been homogeneous in my sense of the word.

As to the conception of Galton's (12) law of filial regression, Pearson (13) has the merit of taking in the clearest manner the consequences of that law when he maintains that continued selection is not checked by regression, and must therefore produce an alteration of the type ("Grammar of Science," p. 483). Nevertheless we meet quite erroneous conceptions as to the significance of the above-mentioned Galton's law, so—to take one example only—in the recent book of Lotsy (14), who gives an exposition of these matters without understanding their bearings. It is of course another matter that the often-mentioned law is not a biological law at all, but only the statistical expression of the compound character of the population.

Still more confusion is found as to the celebrated question whether the ambient conditions may be able to produce transmissible alterations in the characters of organisms—*i.e.* whether exterior conditions may be able to produce an alteration of types. We see here, in place of sober experiments, speculations of a very audacious nature, mostly based upon the confusion of individual adaptative reactions with a supposed alteration of the veritable types (qualities of gametes and zygotes). Most of the "Neo-Lamarckian" literature demonstrates the necessity of exact experiments in all these matters.

It is a pleasure to emphasise the exact experiments of E. Chr. Hansen with yeast-cells (15), cultivated in different ways. Mr. Hansen has operated with "pure lines"; his celebrated studies in fermentations were founded, as is well known, in an exact analysis of yeast-populations—just the same principle that Vilmorin introduced into his heredity experiments more than fifty years ago, the principle which has also been followed in Svalöf and in my own researches.

The influence of the ambient conditions upon the types of organisms can only be studied in reality by means of "pure lines"—if we are to have some warrant as to the meaning of the results: the presumed type-alteration may be nothing but the effect of an unconscious selection in impure, mixed populations. But even in pure lines we have the possibility of mutation, and perhaps extreme conditions may be able to set mutability in action. The whole theory of type-altering by means of altered conditions and direct adaptation is still so vague and floating that it seems unjustifiable to teach it as a sort of semi-scientific creed. As to the evidence from observations in Nature, I cannot omit the striking remarks of Bateson (16), that the differences in ambient nature are gradual, but the differences in organisms from the same locality are specific.

In the domain of hybridology Mendelian analysis has cleared away very much of the obscurity which until recent years was reigning here. It has been the easily appreciable qualitatively characterised traits which here have been the objects of research, and hence in cross-fertilising it has mostly not been necessary to use individuals of which the type-characters in other respects have been determined by special experiments in several generations. Perhaps the neglect of this point may have given to some series of hybrid descendants a greater heterogeneity than would have been encountered by intercrossing individuals belonging to the same pure line of one variety with similarly constituted individuals of another variety (or species).

However it may be in this question; when we proceed to researches in the hybridisation of types that are quantitatively characterised, the highest degree of purity in the two intercrossing varieties or species is required. The material for such hybridisation experiments—to be of scientific value—must be pure lines, the constancy (or, if it may be, the mutability, segregative capacity, and so on) of which has been previously studied in a sufficient number of generations.

We here again touch the fundamental problem as to selection and continuous variability, but now *with the complication of intercrossing.* Here general scientific opinion sticks to the very popular idea that selection—continued again and again—is able to displace the type of the organisms in question. As to the qualitatively characterised types, Mendelism has shown the inadequacy of selection (17), but as to the quantitatively characterised types the conception is still alive that selection will be able to displace the types in the same direction as the selection is made.

Here I may give some remarks about some criticisms of my paper on heredity in pure lines (6). Professor Plate (18) has quite misinterpreted my views. I maintain that in (monomedal) pure lines no effect of selection has been proved; I never spoke of an effect which goes back when selection is stopped—here Plate has confounded me with De Vries, who has not worked with pure lines (19). One of the chief points in my little work is that I regard selection of fluctuations as quite ineffective, and hence must emphasise an absolute difference between fluctuation and mutation—at least as to their perceptible manifestations. Here I must see more than "difference of degree." When recently, besides the biometricians and Plate, also an eminent experimenter, Professor Lang (20), basing his views upon very interesting breeding experiments with snails, declares that mutation and fluctuation only are different in degree, then we are at a point of irreconcilable opposition. We are here concerned with one of the most important fundamental problems in heredity—even the very conception of the meaning of "heredity" is affected. This is manifested by such expressions of Lang as "different degrees in the heredity of recurring unaltered characters" ("verschiedene Grade der Erblichkeit unverändert wieder auftauchender Merkmale"), and that heredity may be augmented or diminished in the course of generations ("dass sich die Erblichkeit im Verlaufe der Generationen steigern oder vermindern kann"). All these expressions recall Vilmorin's (21) idea as to a greater or smaller hereditary power ("force héréditaire"). But this idea seems to me not only quite superfluous but also wrong, the pretended different degrees of heredity being—in the cases hitherto analysed—the simple consequences of different types existing in the population erroneously regarded as homogeneous, but in reality containing individuals which are fluctuating about a plurality of types.

I anticipate that the results of Lang's researches will eventually prove to be quite reconcilable with my views. As to the experiments which have been fully carried through (with little-fluctuating types) he is a convinced Mendelian. But as to his experiments concerning snail populations with great fluctuations, experiments which are still only in their beginning, Lang seems to have been overpowered by the fluctuations. If the analysis can be carried to an end I cannot doubt that Lang will find distinct types as centres for transgressing fluctuations. The idea of "degrees in heredity" was an advance in Vilmorin's time, but now it only implies that the analysis has not been quite completed. In fact, wherever the essential difference between fluctuation and deviation of type (mutation included) is not conspicuous, we may be sure that a biological analysis has not been performed; it may be that such analysis cannot be effected, or simply that the experimenters have neglected it. At all events I must again say emphatically that results as to which the analysis has not been fully performed, or cannot be effected, must never be used as a basis for fundamental biological theories. We have always to elucidate the unanalysed from the analysed facts; the converse proceeding is wrong.

The most interesting objection against my use of the principle of pure lines is made by Plate. It is that the variability will be diminished when intercrossing is excluded. Lotsy says something similar, if I have understood his somewhat ambiguous remarks. Plate, in his usual clear and sharp manner, expresses his thoughts about my little work. It seems to him that I have proved that self-fertilisation in few generations considerably diminishes the tendency to variation, and that a sort of fixed type is arising in the descent ("dass die Selbstbefruchtung die Neigung zum Variiren nach wenigen Generationen sehr erheblich nachlässt und sich gleichsam ein fester Typus der betreffenden Deszendenten heranbildet"). And Plate says further that the main result of my paper is an indirect proof that intercrossing is a natural means for procuring variations ("Das wichtigste Resultat, dass freilich in der ganzen Arbeit nirgends erwähnt wird, scheint mir darin zu liegen, dass sie indirekt beweist, dass Wechselbefruchtung ein natürliches Mittel zur Erzeugung von Variationen ist").

But Plate is here caught by misconceptions and prejudices, which he shares with others; zoologists being very often not familiar with the circumstances of natural self-fertilisation in plants. (The idea that self-fertilisation is something abnormal is very wide-spread; so a prominent anthropologist in a private letter expressed his opinion that my beans in pure lines must soon die out! In nature self-fertilisation may perhaps be more common in plants than cross-fertilisation, and Galton's (22) own experiments stating his law of filial regression were carried out with self-fertilising sweet peas.) In reality there is no trace of indication as to diminution of variability in the course of generations by cultivation in pure lines. There is also no suggestion as to any successive formation of new "fixed" types: the given types have been present from the beginning—they were found and isolated, and the fluctuations about them have not in the least been diminished. How should such marvellous effects of cultivation in pure lines be possible? The self-fertilising plants remain self-fertilisers, whether they are cultivated in numbered places or without numbers. To control this I have made a special research as to the variability in succeeding years—of course there is no alteration, the standard deviation, skewness, and so on, are the same for the same pure line year after year, oscillating to and fro, as all such measures may do.

Hence there is no talk about diminishing variability in pure lines. But should not intercrossing augment variability? We all know that hybridisation gives augmented variability in so far as, by intercrossing of individuals producing different gametes, the different "traits" enter into new combinations, and so on. But this truism is not in question here. Here we have to find out whether intercrossing augments the range of fluctuation or not. Intercrossing of individuals belonging to the same pure line should hardly give any result of interest—and there is no criterion for the success of such an intercrossing experiment, the gametes being of the same nature. But it might *a priori* be probable—in this respect I can agree with Plate—that intercrossing of individuals belonging to different pure lines would augment the fluctuation in respect of such quantitatively estimated characters which (at least in the first generation of hybrids) blend in hybridisation. Where we have dominant and recessive traits the question is quite different.

For the study of the problem here in question we must first possess well-characterised pure lines, the types and the variability of which have been measured and controlled for several generations. I have chosen four such pure lines for my hybridisation experiments. Three of these pure lines were brown beans (*Phaseolus vulgaris*, 'Princess beans').

Line E: seeds broad and rather large (petals pure white and yellow).
Line MM: seeds narrow and rather long (petals with trace of purple).
Line BB: seeds broad and small (petals with trace of reddish-purple).

The fourth was black (dark-blue) beans (*Phaseolus vulgaris*; Belgian *haricot vert hâtif*).

Line SE: seeds very narrow and long (petals purple).

The dimensions and weight of the beans, being the subjects of the research, will be mentioned more concisely below. Other differences between the four lines will not here be mentioned. The black beans were chosen because the conspicuous difference in colour made it easy to ascertain whether the intercrossing was accomplished or not. *A priori* it was to be expected that all the hybrids here in question would show the same general behaviour as to the dimensions of the seeds (length, L, breadth, B, and breadth-index, $J = 100 B : L$). Hence the behaviour of the guaranteed hybrids could be used as a criterion of the hybrid nature of intercrosses between the brown beans.

The hybridisations were performed in the summer 1904 during a visit at Svalöf. My friend Dr. Tedin, the excellent scientific assistant at Svalöf, who is specially trained in the technical difficulties as to intercrossing the leguminous plants, has been so kind as to make all the intercrossings for me. I most heartily thank him for his great amiability on that occasion.

The following hybridisations succeeded :

MM × BB
E × MM
E × SE and SE × E.

A germ produced by intercrossing is developed in a testa belonging to the mother-plant. The germ is "fused" in the "forms" of the mother-plants, and here it was quite impossible to recognise in any case whether the hybridisation is realised or not. But when the seeds germinated the hybrids of E × SE were easily recognisable by the purple colour-stripes on the stem—a character belonging to SE. The seeds of these guaranteed hybrids were characterised by dimensions (L, B, and J) the average values of which—each plant regarded separately—were intermediate between the dimensions of the two parent-lines, and the same was found as to the weight of the beans. These characters evidently blend in the hybrid first generation (F_1) and are therefore well suited for our studies. It was now a very easy matter to find out the real hybrids of the brown lines, giving also for each plant intermediate values as to the weight and dimensions of seed. Only in one single case I have been in doubt, because the plant in question (of the cross MM × BB) had only two seeds—a number too small for estimating with any certainty.

The question now to be elucidated is whether or not the hybrids have an increased variability as to the weight and dimensions of the beans. The ripe beans were weighed and measured in the same manner as indicated in my paper on "pure lines." Here we shall only regard the results as to the weight, the absolute length and breadth. The correlations between length and breadth are too complicated to be treated here; but in reality the breadth indices of the hybrids are—as we shall see—intermediate between the indices of the relative pure lines.

All hybrid beans have been weighed; but in the crop of 1905 I have weighed some portions taken at random from the pure lines. The results are tabulated in Table I., in which the hybrids are placed between their parent lines.

All indications relate to the crop of 1905.

The heading letters in Table I. signify :—

n the number of individuals.
A the average weight in centigrammes.
σ the standard deviation in centigrammes.
V the coefficient of variability ($100\ \sigma : A$).
S the coefficient of skewness (see p. 102).
E the coefficient of excess (see p. 102).

TABLE I.—WEIGHTS OF BEANS FROM FOUR "PURE LINES" AND THREE OF THEIR HYBRIDS (1905).

	n	A	σ	V	S	E
Pure line SE (black) .	414	36·9	6·47	17·5	−0·28	−0·22
Hybrids . . .	902	*46·6*	*7·34*	*15·7*	*−0·38*	*+0·24*
Pure line E (brown) .	446	59·7	6·25	10·5	−0·19	+0·69
Hybrids . . .	421	*54·8*	*7·14*	*13·0*	*−0·21*	*−0·11*
Pure line MM (brown) .	722	50·6	6·08	12·0	−0·32	+0·61
Hybrids . . .	375	*45·3*	*5·97*	*13·2*	*−0·31*	*+0·07*
Pure line BB (brown) .	612	42·1	6·17	14·7	−0·72	+0·84

These numbers do not demonstrate any considerable difference between the variation of hybrids and pure lines. The hybrids have in most of the cases intermediate values between the values of their parent lines, but as to the "excess" the pure lines evidently have much larger deviation from the "normal" curve than the hybrids. It is to be seen in all cases; and with exception of line SE, deviating negatively, the pure lines have a much higher excess than the hybrids.

The same is to be seen in the variation of the dimensions. These are presented in the two following tables, giving respectively the measures of length and of breadth. Here all individuals of the pure lines have been measured; the characteristics of the pure lines are therefore very true. In Table II. A and σ are expressed in millimetres, the rest of the heading letters have the same significance as in Table I.

TABLE II.—THE LENGTH OF BEANS FROM FOUR "PURE LINES" AND THREE OF THEIR HYBRIDS (1905).

	n	A	σ	V	S	E
Pure line SE (black) .	414	14·53	0·92	6·4	−0·62	+0·66
Hybrids . . .	902	*13·92*	*0·87*	*6·2*	*−0·49*	*+0·96*
Pure line E (brown) .	6004	12·63	0·61	4·8	−0·59	+2·84
Hybrids . . .	421	*13·53*	*0·68*	*5·0*	*0·00*	*+0·02*
Pure line MM (brown) .	5546	14·01	0·70	5·0	−0·79	+3·08
Hybrids . . .	375	*12·76*	*0·67*	*5·2*	*−0·72*	*+1·35*
Pure line BB (brown) .	6663	11·25	0·53	4·7	−0·68	+4·01

In Table III., giving the measures of breadth, A and σ are also expressed in millimetres. In the column headed J the relative breadths are indicated, *i.e.* average breadth indices, J = 100 breadth : length.

TABLE III.—THE BREADTH OF BEANS FROM FOUR "PURE LINES" AND THREE OF THEIR HYBRIDS (1905).

	n	A	σ	V	S	E	J
Pure line SE (black) .	414	6·93	0·36	5·2	−0·57	+0·45	47·7
Hybrids . . .	902	*7·81*	*0·42*	*5·4*	*−0·35*	*+0·13*	*56·1*
Pure line E (brown) .	6004	9·01	0·71	4·6	−0·60	+1·33	71·3
Hybrids . . .	421	*8·39*	*0·40*	*4·7*	*−0·12*	*−0·42*	*62·0*
Pure line MM (brown) .	5546	7·72	0·31	4·1	−0·45	+1·07	55·1
Hybrids . . .	375	*7·86*	*0·34*	*4·3*	*−0·25*	*+0·11*	*61·6*
Pure line BB (brown) .	6663	7·97	0·41	5·2	−0·61	+1·01	70·7

As to the dimensions also we cannot find any greater variability in hybrids. But they have always shown a much smaller coefficient of excess than the pure lines. The standard deviation or coefficient of variability being almost identical, this means clearly that the greatest deviations from the mean are relatively more numerous in our pure lines than in their hybrids. These fluctuate more in accordance with the "normal frequency curve" than their pure parent lines. These also have a greater skewness in their curves than the hybrids.

Resuming these experiments, it may be said that the fluctuations as to weight and dimensions in the pure lines were not less than in their hybrids; here was no increased amplitude of variability, offering any better material for selection. The contrary was rather the case as expressed by the higher "excesses" in the pure lines. These results may also be regarded as an answer to the criticism which maintained that my pure lines should present diminished fluctuations!

It is now my task to observe the progeny of the hybrids through a series of generations in the same manner as I have observed several pure lines. To judge from some few breeding experiments in the greenhouse, there will be found Mendelian segregations as to dimensions and weights. This matter will be observed more closely, and the isolation of the new type-combinations shall be carried out. In this manner what may be called "unit-characters" as to length, breadth, indices, weight and so on will be elucidated. I hope to find some quantitatively estimated traits that not only blend in the first generation of hybrids, but do not segregate at all. The exact quantitative study of such hybrids is still to be performed.

At all events it seems to me now that we have no reason to suppose that an augmented fluctuation will be found in the new types which here may be formed by segregations and new combinations. Further research will, I have every conviction, give greater clearness as to the fundamental distinction of true *type differences* and *fluctuations*. The way out of the confusion in the struggling theories of heredity and evolution is by exact biological analysis; mathematics may here be a good and indispensable servant, but not the commander! "Treatment"—mathematical, philosophical, and fantastical—may be disputable; what we want—in much higher degree than commonly admitted—are well analysed pure and clear elementary premises.

Continuity of evolution is the most beautiful idea of modern biological philosophy; we all may love this idea and have some hope of its being true, but in reality not one indisputable fact as yet proves it. And are not the results of modern chemistry speaking loudly of discontinuity as a fundamental fact in nature?

APPENDIX.

After writing this paper, I received, by the kindness of Mr. Darbishire, his very interesting pamphlet "On the Differences between Physiological and Statistical Laws of Heredity" (from Memoirs and Proceedings of the Manchester Lit. and Phil. Soc., vol. l. Part III., 1906). The author attacks his problem on another ground than that upon which my criticisms as to statistical treatment of heredity are based; so far we supplement each other. It will be evident to an intelligent reader, that one of the tendencies of my present paper is to emphasise the fact that the biometrician's methods of measuring the "intensity of heredity" are fallacious not only when applied in "predicable cases" (Darbishire p. 37), but also—from a biological point of view—when applied in "non-predicable cases." These comprise all the non-analysed cases (including what may be non-analysable), concerning especially the quantitatively characterised highly fluctuating traits. Here biometry has given us stones for bread, *e.g.* as to the understanding of the action of selection, as to the problem of discontinuous or continuous evolution, and so on. I am quite in accordance with Mr. Darbishire when he says that "the true function of the biometrician is to give us statistics of average conduct where we cannot predict individual conduct." And this may perhaps suffice for many important problems of sociology (with pleasure I will say "biological sociology" if desired), but it has no value at all for the biology of heredity and evolution, the aim of which is *to elucidate the origin and conduct of the veritable types of organisms*, the "biotypes." Here biology must try to make the "non-predicable" predicable, by a sound analysis avoiding statistics of heterogeneous impure masses. The biometrical "truths" as to such masses may be able to confuse the views of biologists just so much as Weismann's speculations on the "All-sufficiency of Natural Selection"—both operating with false premises: impure masses regarded as homogeneous aggregations.

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17. It is here sufficient to refer to Bateson, Presidential Address to Section D, British Association's Report, Cambridge, 1904, p. 574.

18. Plate.—Review of my "Erblichkeit in Populationen," &c. in Archiv für

Rassen- und Gesellschafts-Biologie, I., p. 137, 1904. See also Plate's article "Die Mutationstheorie im Lichte zoologischer Tatsachen." (Comptes rendus du 6me Congrès intern. de Zoologie, 1904–1905.)

19. MacDougal (and Vail, Shull, and Small).—Mutants and Hybrids of the Œnotheras (Papers of Station for Experimental Evolution at Cold Spring Harbour, New York, Nr. 2 Washington, 1905), emphasises and exemplifies the necessity of analysing the species before entering on the question of mutation. This is quite in accordance with my own views.

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DISCUSSION.

The President: Professor Johannsen has been dealing with a problem of extreme difficulty. He analyses types according to their quantitative relations, and he shows that what we call one type is in reality a great number of types which are each true to a certain definite average weight. His experiments go to indicate that these averages are in themselves pure factors. What happens when these pure types differentiated by small fluctuations are crossed, we do not know, but there is a suggestion that segregation occurs.

Professor Plate, of Berlin: If you take a pure type which is always self-fertilised, you cannot expect variation; but as soon as you change the outward conditions of the pure type—I do not say that the variations would not be small, but on the one side or the other there will be continuous variation. If the continued conditions are fixed, there would not be any change, either to the one side or the other, and that would be what Darwin calls "continuous variation." Therefore Professor Johannsen has not convinced me that continuous variation does not exist. If we look into nature we can always get continuous variation. For instance, I have studied snails which are to be found in the Bahama Islands, and although there were the greatest variations they were continuous.

Mr. G. U. Yule, University College, London: I am afraid I have not yet been won entirely to Professor Johannsen's views. It is quite true that he has not been able to observe any differentiation, even though he has selected the weight or width of his beans throughout five generations; but, as I suggested in a short review of Professor Johannsen's work, it would be quite possible that that should happen if the variations due to environment were large compared with the variations in the germinal types, and I think it will be found that the somatic variation in these beans is very large indeed compared with the germinal variation. If, under such circumstances, you select according to somatic character, there will be only a very slight selection of germinal types, and this may well be masked by somatic fluctuations. I judge, from other things we know, that the germinal variation cannot be absolutely zero. To justify this statement I fear I must mention a quantity which I shall have to mention again later on, and that is the correlation-coefficient of the biometrical school. We know few such correlation coefficients for cases of self-fertilisation or vegetative reproduction, but the coefficients that have been determined exhibit one common characteristic—the coefficients of the offspring with the higher ancestry are always less than the correlation with the parent. If Professor Johannsen's view, as I understand it, were true, and the germinal type were absolutely and rigidly fixed, then in the mass of the population, the correlation between the offspring and the grandparent would be identical with that between the offspring and the parent. We have not many data, as I have said, but such as exist seem against Professor Johannsen's view, and accordingly I feel inclined to hold my judgment in suspense until the question has been further studied.

The President: We must expect the answer to come from later generations. Pending further tests we are bound to suspend our judgment.

Mr. C. C. Hurst, Hinckley, England: In view of the discussion I might remark that I have also been carrying out some experiments of the same nature as those Professor Johannsen has been engaged upon. I have chosen the Dutch rabbit, which is very fluctuating, and I might say that up to now the results are of a purely negative nature. I hope to be able to report shortly, but at present the continuous variations are hereditable. Professor Johannsen also stated that Mendel's experiments did not touch the question of continuous variation. That is true in a sense; but I should like to point out that before Mendel's experiments were begun, our general ideas of variation were that continuity was the rule and discontinuity almost the exception. I think the solitary person who recognised the great value of discontinuity was our worthy President, Mr. Bateson, long before Mendel was known, and when the rest of us were sticking to Darwin's continuity. When we made an experiment with sweet peas, before Mendel was known, and we found in the F_2 generation purples, reds, and whites, and all the different gradations of colour, we should at first sight have said that that was the effect of variation. Now we know, from the experiments of Bateson and Miss Saunders, that discontinuity is the rule with sweet peas. Therefore I think it is only fair to point out that the supposed continuous variations are really discontinuous. We shall find that almost all the hereditable characters are discontinuous in nature, and that the continuous variation is merely somatic and altogether apart from heredity.

MENDELIAN CHARACTERS IN PLANTS AND ANIMALS.

By C. C. Hurst, F.L.S., F.R.H.S.

Recent experiments with many kinds of plants and animals have largely extended the application of Mendel's law of heredity.

In my own experiments and observations, for instance, the Mendelian principles have been evident in such widely different organisms as peas and rabbits, sweet peas and horses, tomatos and poultry, orchids and man.

The main object of the experimenter has been to discover the Mendelian characters in each type of plant and animal by means of Mendel's methods.

In many cases this has been apparently easy, while in others it has been more difficult.

In Mendel's classical experiments with peas, the characters were patent, consisting simply of pairs of contrasts, *e.g.* round and wrinkled seeds, yellow and green cotyledons. Many similar cases have been found in other plants and in animals, and these usually follow the simple rules of dominance, segregation, and purity.

In these simple cases the outward or zygotic character of a pure plant or animal is presumably represented in the germ-cells or gametes by a single factor or determiner. In other cases, however, the zygotic character, though apparently simple, is really compound, being represented in the gametes by more than one factor.

Thus Mr. Bateson, Miss Saunders, and Mr. Punnett have recently demonstrated that red flower-colour in sweet peas and stocks is due to the association of two gametic factors, purple colour to three factors, while hoariness in stocks has been shown by them to be due to no less than four distinct gametic factors.

These compound characters are not often to be detected at sight, and for the most part have to be subjected to a Mendelian analysis ere their true nature is discovered.

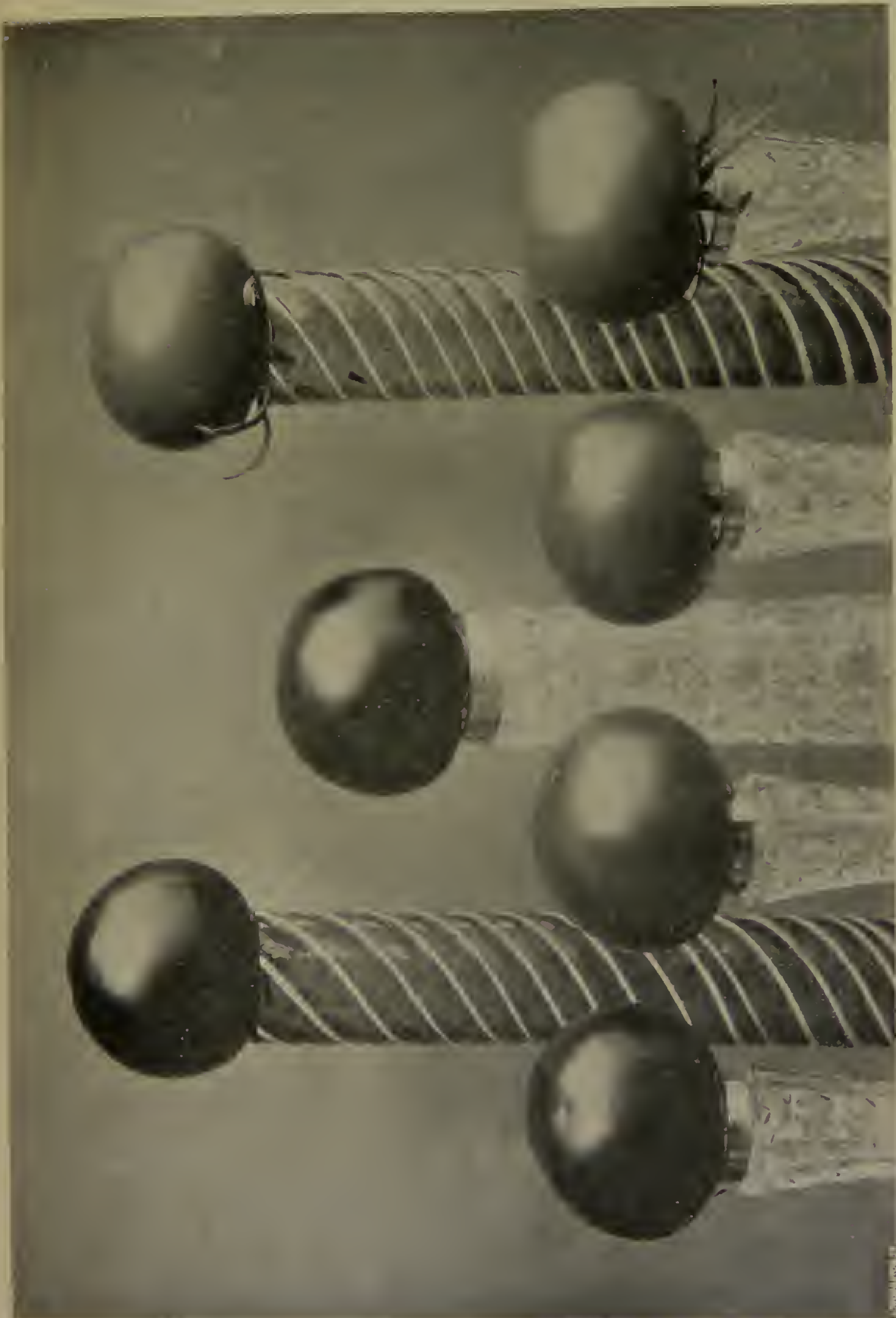
What could appear more simple, for instance, than such characters as the red colour of the fruits of the 'Fireball' tomato, the red colour of the flowers of the 'Crimson King' Antirrhinum, the yellow-grey coat of the 'Belgian Hare' rabbit, or the rose comb of the 'Black Hamburgh' fowl?

All these characters breed true to themselves, and have done so for many generations; yet, as my experiments show, all are really compound characters, each being represented in the gametes by more than one factor.

The Red Colour of the 'Fireball' Tomato.

The 'Fireball' tomato is a pure race, bearing red fruits: it has bred true to colour with me for at least ten generations. The shade of colour of the ripe fruits corresponds with that of 'Rouge Tomate' (Tomato

Red) in "Répertoire de Couleurs," t. lxxxi. (tone 1): the peculiar fiery-red colour is due to the red flesh showing through a bright yellow skin.



'Fireball' — F_1 hybrid 'Fireball' — 'Golden Queen'

F_2 'Fireball' — F_2 Carmine — F_2 Gamboge — F_2 'Golden Queen'

FIG. 26.—TOMATOS.

Showing dominance of red over yellow flesh, and yellow over white skin in F_1 and segregation of these characters in F_2.

In my experiments 'Fireball' was crossed with 'Golden Queen,' a race bearing yellow fruits. In F_1 all the cross-breds were indistinguishable from 'Fireball,' red colour being dominant and yellow recessive (see fig. 26) In F_2 segregation into three red to one yellow took place (42 : 13). In this generation, however, there were four distinct and discontinuous types, two reds, and two yellows. One type of red was like the original 'Fireball,' while the other was quite different, corresponding with 'Rouge Caroubier' (Carmine Red) in "Rép. de Coul." t. cxiii. (tone 4); this shade is due to the red flesh showing through a white or colourless skin.

One type of yellow was quite different from the original 'Golden Queen,' corresponding with 'Jaune Gomme Gutte' (Gamboge Yellow) in "Rép. de Coul." t. xxv. (tone 4); this shade is due to the yellow flesh showing through a bright yellow skin. The other type of yellow was like the original 'Golden Queen' corresponding with 'Jaune Soleil' (Sunflower Yellow) in "Rép. de Coul." t. xxiii. (tone 3); this shade is due to the yellow flesh showing through a white skin (see fig. 26).

These four types occurred in F_2 approximately in the ratio of 9 : 3 : 3 : 1 respectively (31 : 11 : 10 : 3), suggesting that the Mendelian characters in this case are two pairs, which are apparently (1) red and yellow flesh, red being dominant and yellow recessive; (2) yellow and white skin, yellow being dominant and white recessive. It is probable, however, that the unit-characters are really simply (1) presence (R) and absence (r) of red in the flesh; (2) presence (Y) and absence (y) of yellow in the skin; presence being dominant and absence recessive in both.

In that case the gametic formula of 'Fireball' would be (R+Y), 'Golden Queen' would be (r+y), the Carmine Red would be (R+y), and the Gamboge Yellow would be (r+Y).

It is evident, therefore, that the red colour of the 'Fireball' tomato is due to at least two gametic factors.

Broadly speaking, red and yellow tomatos may be said to behave as Mendelian characters, red being dominant and yellow recessive: at the same time Mendelian analysis shows that there are at least two types of red tomatos, viz. red flesh in a yellow skin and red flesh in a white skin: similarly there are at least two types of yellow tomatos, viz. yellow flesh in a yellow skin and yellow flesh in a white skin.

The practical importance of these results to the breeder lies in the fact that any of these four colour types can be quickly fixed (no matter how they may have arisen) by anyone possessing a knowledge of the Mendelian principles, and their various behaviours when crossed can be faithfully predicted for any number of generations.

The Red Colour of 'Crimson King' Antirrhinum.

The race of dwarf *Antirrhinum majus* grown in gardens as 'Crimson King' has red corolla segments corresponding with the shade 'Rouge Caroubier' (carmine red) of "Rép. de Coul." t. cxiii. (tone 4).

In my experiments this form was crossed with 'Yellow Prince,' a dwarf race with yellow segments corresponding with the shade 'Jaune Soufre' (sulphur yellow) in "Rép. de Coul." t. xviii. (tones 1 and 2).

All the F_1 cross-breds were indistinguishable from 'Crimson King,' red colour being dominant and yellow recessive. In F_2 segregation took place, there being approximately three 'Crimson King' to one 'Yellow Prince' (116 : 41).

Similarly 'Yellow Prince' crossed with 'White Queen,' a dwarf race with white segments, gave in F_1 cross-breds which were indistinguishable from 'White Queen,' white being dominant and yellow recessive. In F_2 segregation took place, there being approximately three 'White Queen' to one 'Yellow Prince' (37 : 13).

When, however, 'Crimson King' was crossed with 'White Queen,' all the cross-breds in F_1 were reds, but a different shade from that of 'Crimson King'; the shade of the cross-breds corresponded with 'Rouge Amarante' (amaranth red) in "Rép. de Coul." t. clxviii. (tones 3 and 4). In F_2 segregation into four types took place, there being approximately nine Amaranth Red, three 'Crimson King,' three 'White Queen,' one 'Yellow Prince' (81 : 22 : 26 : 8).

This result suggests that the red colour of 'Crimson King' is really a compound of red and yellow represented in the gametes by two distinct factors, R and Y, which, when associated, produce the zygotic red of 'Crimson King'; in the zygote the red colour predominates over the yellow, and so long as the two colours are associated 'Crimson King' breeds true colour, but when dissociation takes place through crossing, the compound red-yellow of 'Crimson King' is segregated into amaranth-red and the sulphur-yellow of 'Yellow Prince.' *

In these three colour forms of antirrhinum, red, yellow, and white, the Mendelian character pairs are evidently not red and yellow, red and white, and yellow and white, as might be supposed, but are apparently two pairs only, viz. (1) presence (R) and absence (r) of red, presence being dominant and absence recessive; (2) presence (Y) and absence (y) of yellow, absence being dominant and presence recessive.

The gametic formula of 'Crimson King' may therefore be regarded as (R + Y), amaranth-red as (R + y), 'Yellow Prince' as (r + Y), and 'White Queen' as (r + y). On this basis the whole of the results are clear, and the inheritance is strictly in accordance with the Mendelian principles.

One interesting and curious feature of these experiments with antirrhinum is the demonstration of recessiveness of presence of yellow sap and the apparent dominance of a negative quality.

As in the case of the tomatos, the practical importance of these results to the breeder lies in the fact that any of the four colour types can be quickly fixed, no matter how they may have arisen, by anyone possessing a knowledge of the Mendelian principles, and their various behaviours when crossed can be faithfully predicted for any number of generations.†

* Both the red and yellow colours concerned are apparently sap-colours, being soluble in water, and each solution gives a distinct chemical reaction. A suggestive demonstration of the compound nature of the red colour of 'Crimson King' may be seen when a solution of the amaranth-red is added to a solution of 'Yellow Prince'; this gives a similar solution to that obtained from 'Crimson King,' and both solutions give the same chemical reactions.

† At the Conference Miss Wheldale suggested that 'White Queen' was not a white but a cream.

It is true that 'White Queen' is not an absolute albino, seeing that it has a yellow palate and two rows of yellow hairs within the corolla tube, but the five corolla segments are pure white when mature, and no trace of the red or yellow sap-colours could be found when the white segments were tested chemically.

For the sake of simplicity the account of my results was confined to the white, yellow, and red corolla segments of the three races. With regard to the remainder

Grey Coat Colour in the 'Belgian Hare' Rabbit.

In my experiments the pure-bred 'Belgian Hare' with a grey coat was crossed with the pure-bred 'White Angora' with a white coat. In F_1 all the offspring had grey coats (see fig. 27). In F_2 there were approximately nine grey: three black: four white (127 : 44 : 53) (see fig. 28).

The colour ratio in F_2 suggests that at least two pairs of Mendelian characters are concerned which appear to be (1) presence (C) and absence (c) of colour, presence being dominant and absence recessive; (2) grey (G) and black (B) colour, grey being dominant and black recessive.

In that case the gametic formula of the 'Belgian Hare' would be (C + G), and that of the 'White Angora' (c + B); the new character 'black' which appeared in F_2 being thus introduced by the Albino Angora.

From this it would appear that the coloured coat of a rabbit is due to the meeting of two distinct gametic factors, one of which may determine the presence of the pigment, while the other determines the colour of that pigment. If, for instance, C be present, the animal will be coloured, if absent it will be white; if both C and G be present it will be coloured grey, while if C and B be present it will be coloured black.

These results with rabbits confirm Prof. Cuénot's experiments and conclusions with similar coat-colours in mice.

Since my results were published, Mr. Bateson has suggested that in such cases the coat-colours may be due to at least three pairs of gametic factors, viz. (1) presence (C) and absence (c) of colour; (2) presence (G) and absence (g) of grey; (3) presence (B) and absence (b) of black; presence being dominant and absence recessive in each case. In that case the gametic formula of the 'Belgian Hare' would be (C + G + B), and that of the 'White Angora' would be (c + g + B), both being homozygous in B.

Either interpretation covers the known facts, and further experiments are necessary to determine which is correct. In any case, however, it is clear that the pure-breeding grey coat-colour of the 'Belgian Hare' is a compound character represented in the gametes by at least two distinct factors.

The Rose Comb of the 'Black Hamburgh' Fowl.

In my experiments the pure-bred 'Black Hamburgh' with a rose comb, crossed with the pure-bred Houdan with a leaf comb, gave in F_1 cross-breds with modified rose combs. In F_2 there appeared a small proportion of true *single* combs (7 in 70). These results suggest that the homozygous rose and leaf combs are not simple Mendelian characters, but are probably compounds of rose on single, and leaf on single, respectively. In rose crossed with leaf combs, the two pairs of Mendelian

of the flower, the yellow palate and the yellow hairs within the corolla tube are common to all the three races and evidently belong to an independent Mendelian character, all three forms being homozygous in that character.

With regard to the remainder of the corolla tube, this is white in 'White Queen' and 'Yellow Prince,' and amaranth-red in 'Crimson King.' When 'Crimson King' was crossed with 'White Queen' and with 'Yellow Prince' all the cross-breds had red corolla tubes in F_1, while in F_2 segregation into red tubes and white tubes took place *among the reds only*, all the whites and yellows having white tubes. Further experiments are necessary to determine the precise nature of these 'Delila' forms.

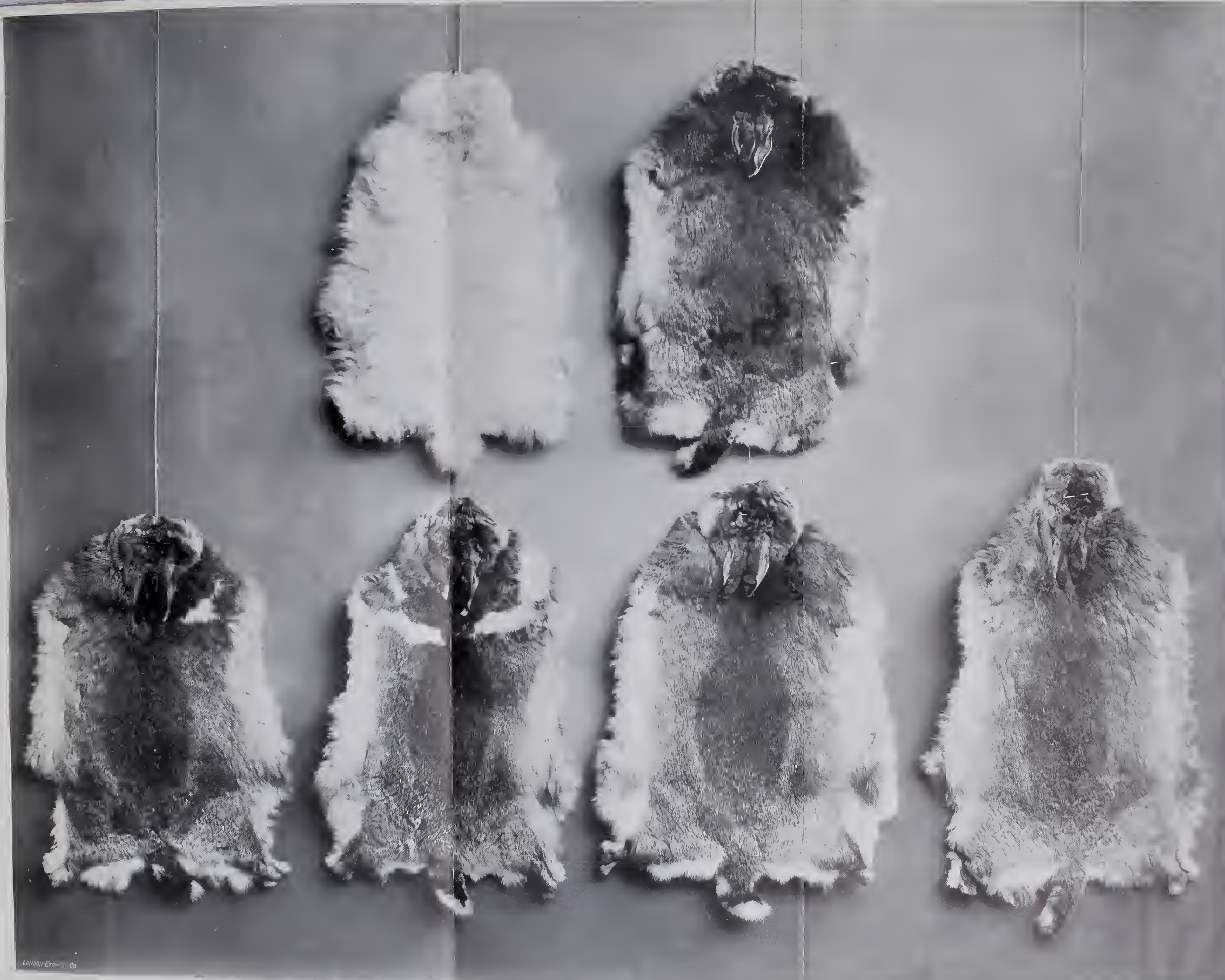


FIG. 27.—RABBITS. Coat-skins of 'White Angora' and 'Belgian Hare'

Fig. 28.—Rabbits. Coat-skins of F_2 forms, raised from the four F_1 forms in fig. 27, showing segregation of coloured and white coat, and short and Angora coat. Illustrating also the appearance in F_2 of the black and Dutch-marked coat characters introduced by the Albino Angora in fig. 27.

characters may well be: (1) presence (R) and absence (r) of rose; (2) presence (L) and absence (l) of leaf; presence being dominant to absence in both cases, single (S) being common to both. In that case the gametic formula of rose comb may be regarded as (R + l + S), and that of leaf comb as (L + r + S).

These results confirm the similar experiments and conclusions of Messrs. Bateson and Punnett with rose and *pea* combs.

It seems clear, therefore, that the pure-breeding rose comb of the 'Black Hamburgh' fowl is a compound character represented in the gametes by at least two distinct factors.

The Nature of Mendelian Characters.

The foregoing illustrations show some of the difficulties encountered by the experimenter in the determination of Mendelian characters in plants and animals. It is evident that the precise determination of unit-characters can only be secured by means of careful and exhaustive experiments.

When we find such apparently simple zygotic characters as those noted above giving a simple Mendelian result in certain crosses and yet in others proving to be gametically compound, the question naturally arises whether many other of the apparently simple Mendelian characters are not also compound in their gametic constitution.

Is it not possible, for instance, that some of the original Mendelian characters in peas may be due to more than one gametic factor?

For example, in cotyledon colours in peas, might not the character pairs be really presence and absence of yellow on a basis of green, rather than the contrasting yellow and green? Is it not possible that many of the so-called contrasting pairs of Mendelian characters are really compound, and that the true unit-characters are simply presence and absence?

Review of Experiments.

In view of the possible reduction of Mendelian characters in plants and animals to the simple presence and absence of unit-characters, it may be useful to review briefly the Mendelian characters found in my own experiments.

Peas.

In my experiments with peas, the Mendelian characters met with are the same as those discovered by Mendel, viz. yellow and green cotyledons, round and wrinkled seeds, tall and dwarf stems—the first-named of the pair being dominant over the other, which is recessive. As suggested above, the Mendelian contrasting pair, yellow and green, might be regarded as presence and absence of yellow on a basis of green. On this view, the characters yellow and green would belong to two distinct pairs, instead of one as Mendel supposed, and these would be presence (Y) and absence (y) of yellow, and presence (G) and absence (g) of green, presence being dominant over absence. The gametic formula of the pure-breeding yellow pea based on green would, on this view be (Y + G), and the zygote yellow owing to dominance.

The gametic formula of the green pea would be (y + G) and the zygote green. It will be observed that both the yellow and the green peas are homozygous in G.

The wild pea has yellow cotyledons, and on the view that its yellow is based on green, the evolution of the green pea from the yellow might be explained by the mutational absence of the yellow factor in a certain gamete.

With regard to the pair of Mendelian contrasts, round and wrinkled seeds, the precise physiological nature of the irregular shrinking of wrinkled peas is hardly yet understood. Mr. R. P. Gregory found that in round peas the starch grains were large and oval in shape, while in wrinkled peas they were very small, rounded, and frequently compound.* Professor de Vries apparently regards the wrinkling of peas as similar in nature to the wrinkling of the sugar maize, *i.e.* due to lack of starch. He states that "the sugar is only accumulated as a result of an incapacity of changing it into starch." † If this is so, then the Mendelian pair, round and wrinkled, might be regarded as presence and absence of roundness (starchiness) on a wrinkled basis (sugariness), presence being dominant over absence. Similarly, the Mendelian contrasting pair, tall and dwarf stems, might be regarded as presence and absence of tallness on a dwarf basis, presence being dominant over absence.

Sweet Peas.

In my experiments with Sweet Peas, four pairs of Mendelian characters have, so far, been met with, viz. coloured and white flowers, purple and red flowers, tall and dwarf habit, long and round pollen grains—the first-named of the pair being dominant over the other, which is recessive.

With regard to the first two pairs, it has been clearly demonstrated by Mr. Bateson, Miss Saunders, and Mr. Punnett ‡ that three pairs of unit-characters are concerned in the sap colours of sweet pea flowers, viz. presence (C) and absence (c) of first colour factor (probably a colour-forming stuff), presence (R) and absence (r) of second colour factor (probably an enzyme), presence (B) and absence (b) of blueness.

A coloured flower depends on the simultaneous presence in the zygote of the two colour factors C and R. If both the factors are present, the flower will be red; if either C or R is absent, the flower will be white. If the three factors C, R, and B are present, the flower will be purple; if B is absent, the flower will be red; if either C or R is absent, the flower will be white. From this it is evident that the two pairs of characters—coloured and white flowers, purple and red flowers, which in my experiments behaved as simple Mendelian pairs—are really due to three pairs of unit-characters, each pair being presence and absence of a certain unit, presence being dominant over absence.

With regard to the remaining characters met with in my experiments, tall and dwarf habit might be regarded as presence and absence of

* *The New Phytologist*, vol. ii. (1903), p. 226.
† *Species and Varieties* (1904), pp. 283, 289.
‡ Report III. *Evol. Com. Roy. Soc.* 1906.

tallness on a dwarf basis, as in the peas, while long and round pollen might be regarded as presence and absence of longness on a round basis, presence being dominant in both cases.

Poppies.

In my experiments with *Papaver somniferum*, three pairs of Mendelian characters were met with, viz. coloured and white flowers, purple and red flowers, black and white basal area of petals—the first-named of the pair being dominant over the other, which is recessive. The first two pairs are probably, like the sweet pea, due to three pairs of unit-characters—presence and absence of C, presence and absence of R, presence and absence of B—presence being dominant. The remaining pair, black and white basal area of petals, may be regarded as presence and absence of black pigment, presence being dominant.

Antirrhinums.

In my experiments with *Antirrhinum majus*, as we have already seen, two pairs of Mendelian characters have been found, viz. white and yellow corolla segments, red and yellow corolla segments, the first of the pair being dominant over the other, which is recessive. The unit-characters in these two cases seem to be absence and presence of yellow sap, presence and absence of red sap, respectively, presence being recessive in the yellow, and dominant in the red.

Tomatos.

In my experiments with tomatos, as already seen, two pairs of Mendelian characters have been found, viz. red and yellow flesh, yellow and white skin, the first-named of the pair being dominant over the other, which is recessive. The unit-characters in these two cases seem to be presence and absence of red in the flesh, and presence and absence of yellow in the skin, respectively, presence being dominant in both cases (see fig. 26).

Primulas.

In my experiments with *Primula*, three pairs of Mendelian characters were met with, viz. red and green stems, palm and fern leaves, thrum and pin eyes, the first-named of the pair being dominant over the other, which is recessive.

Red and green stems may be regarded as presence and absence of red on a green basis, presence being dominant and absence recessive.

The application of the presence and absence hypothesis to palm and fern leaves is not so obvious.

If we regard this as presence and absence of palm on a fern basis, we are at once met by the difficulty that the fern leaf appears to be an elongated palm leaf. If, on the other hand, we regard it as presence and absence of fern on a palm basis, this would imply dominance of fern over palm in the zygote, while experiments show, on the contrary, that palm leaf is dominant over fern leaf. Similarly with thrum and pin eye, if we regard it as presence and absence of thrum on a pin basis, we are met by the difficulty that pin eye or long style seems to be an elongated thrum or short style; while on the other hand, if we regard it as presence and absence of pin on a thrum basis, this would imply dominance of pin over

Fig. 29.—Orchids.
Forms of *Paphiopedilum* × *Hera* showing segregation of spotted and striped sap patterns.

thrum in the zygote, and experiments show, on the contrary, that thrum eye is dominant over pin eye.

Orchids.

In my experiments and observations with orchid hybrids of various genera and species, three pairs of Mendelian characters have been found,

Fig. 30.—Orchids.
Forms of *Paphiopedilum* × *Hera* showing segregation of spotted and striped sap patterns.

viz. purple sap colour and albino, spotted sap and striped sap patterns (see figs. 29, 30, and 31), white and green areas, the first-named of the pair being dominant over the other, which is recessive. The first pair may be regarded as presence and absence of purple sap, presence being dominant.

The others may be regarded respectively as absence and presence of sap areas, and absence and presence of green plastids, presence being apparently recessive.

Many other characters in orchids, both pigmental and structural, are obviously Mendelian, but more evidence and much careful investigation

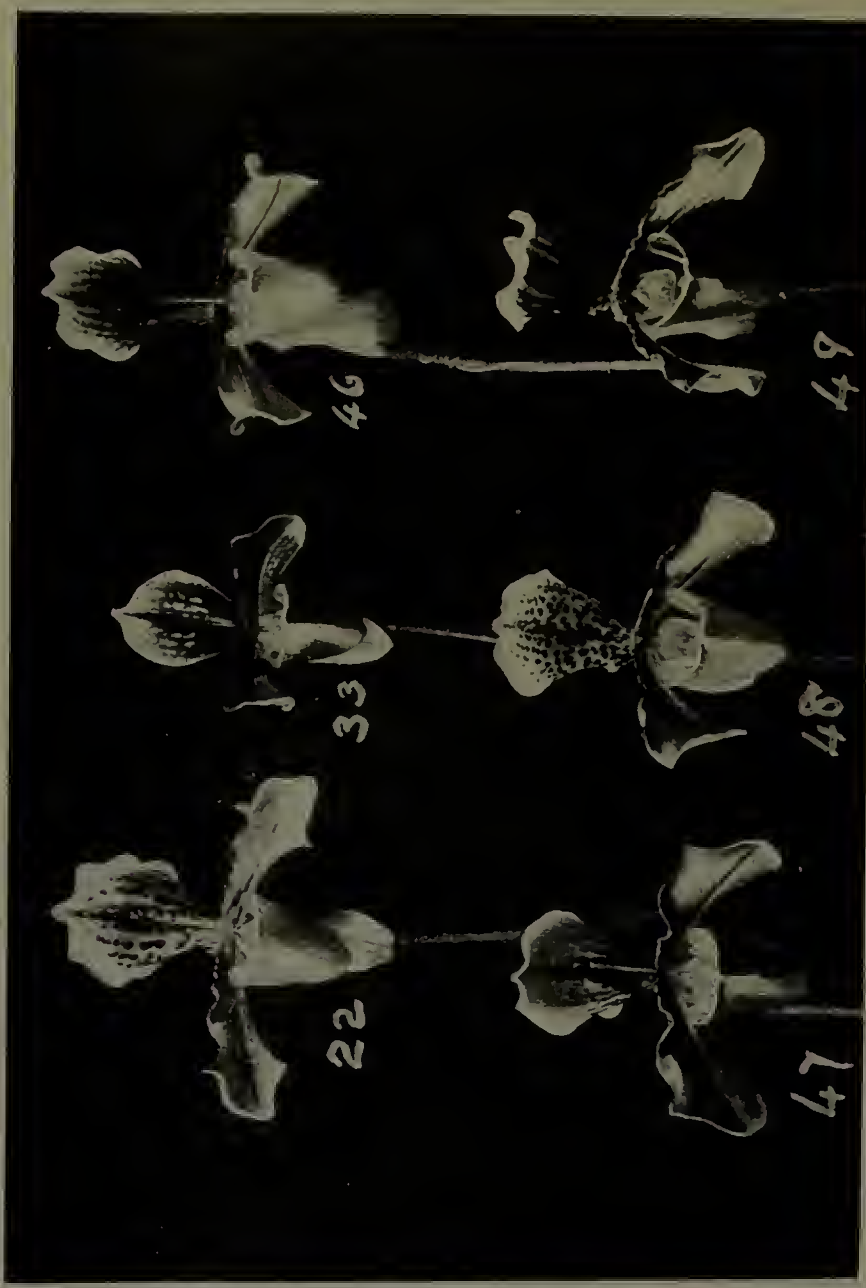


Fig. 31.—Orchids.
Forms of *Paphiopedilum* × *Hera* showing segregation of spotted and striped sap patterns.

are necessary before one can venture to deal confidently with such complicated material.

Rabbits.

In my experiments with rabbits, fourteen pairs of Mendelian characters have so far been found, which may be classified as follows, the first-named of the pair being dominant over the other, which is recessive:

(*a*) Coat colour, five pairs, viz. coloured and white (see figs. 27 and 28), grey and black, grey and yellow, black and yellow, full and dilute (full colours are grey, black, and yellow, and their respective dilutions blue-grey, blue, and fawn).

(*b*) Coat patterns, eight pairs, viz. Himalayan white and clear white, tortoise-yellow and clear yellow, self-coloured and Himalayan white, English-marked and self-coloured, tan-marked and self-coloured, Dutch-marked and self-coloured (the heterozygote of this is variably marked), English-marked and Dutch-marked, plain and silvered.

(*c*) Coat length and texture, one pair, viz. short and angora (see figs. 27 and 28).

With regard to the five Mendelian pairs of coat-colours, the first four might be regarded, as Mr. Bateson suggests, as presence and absence of a specific colour, presence being dominant. The original grey might be regarded as based on black and yellow, black being absence of grey, and yellow being absence of black and grey. The remaining pair, full and dilute, may be regarded as presence and absence of full on a dilute basis, presence being dominant.

With regard to the eight Mendelian pairs of coat patterns, all may be regarded as presence and absence of colour in certain areas, presence being dominant, except in the English and tan patterns, where presence is apparently recessive.

With regard to the one pair of Mendelian characters for coat length and texture, short and angora, the application of the presence and absence hypothesis is not so obvious. If we regard this as presence and absence of short on an angora basis, we have the difficulty that the long coat of the angora appears to be a lengthened short coat. If, on the other hand, we regard it as presence and absence of angora on a short basis, this would imply dominance of angora over short in the zygote, while experiments show, on the contrary, that short coat is dominant over angora coat.

POULTRY.

In my experiments with poultry, eight pairs of Mendelian characters were met with, viz. rose and single comb, leaf and single comb, extra toe and normal foot, crested and plain head, feathered and clear shanks, white and yellow shanks, white and black plumage, white and buff plumage; the first-named of the pair being dominant over the other, which is recessive.

With regard to the combs, we have already seen that the unit-characters may be regarded as presence and absence of rose, presence and absence of leaf, presence being dominant, absence of either being single comb on which the rose and pea are presumably based. (It is possible also that the large single comb of the Mediterranean races is a separate unit-character based on the original small single comb.)

With regard to the foot characters, the dominance of extra toe over normal foot was found to be both incomplete and irregular, but segregation and gametic purity were evident, the unit-characters being apparently presence and absence of extra toe, presence being usually dominant.

With regard to shank feathering, dominance of feathered over clear shanks was found to be incomplete but regular, while segregation was irregular, but gametic purity evident, the unit-characters being apparently presence and absence of shank feathers.

The remaining pairs of Mendelian characters may be respectively regarded as presence and absence of crest, absence and presence of yellow pigment in the shanks, absence and presence of black pigment in the plumage, absence and presence of yellow pigment in the plumage. It will be noted that presence is dominant over absence in all cases except the black and yellow pigments, in which presence is apparently recessive though incompletely.

Horses.

In my investigations in coat-colour in thoroughbred horses, I have found that chestnut colour is a Mendelian character, recessive to both bay and brown, which are dominant characters.

The unit-characters in this case are apparently presence and absence of black pigment in the points on a basis of red.

Summary of Experiments.

From the brief survey given above, it will be seen that out of a total of 44 pairs of Mendelian characters met with in my experiments with plants and animals, no less than 41—or rather more than 93 per cent.—may be regarded as favourable to the hypothesis of presence and absence of unit-characters ; the remaining three—or rather less than 7 per cent.—being more favourable to Mendel's original view of contrasting characters. Further experiments and the observations of others are necessary to determine the question, but in the meantime it may be interesting to inquire into the possible behaviour of the factors of these unit-characters in the processes of fertilisation and gameto-genesis.

The Behaviour of Unit-characters in Fertilisation.

In pure breeding, Mendel presumed that two like factors pair in the process of fertilisation—one factor from the male parent and one from the female parent—and when the resulting zygote produces gametes the two factors segregate, one factor going into one gamete and the other into another. In cross-breeding, Mendel presumed that two unlike but contrasting factors pair in fertilisation, and when the resulting hybrid zygote produces gametes, the two contrasting factors segregate, one going into one gamete and the other into another: *e.g.* a pea with green cotyledons produces gametes carrying the factor G, and a pea with yellow cotyledons produces gametes carrying the factor Y. Then the green pea (G) self-fertilised, or fertilised with another green pea (G), produces a zygote (GG) which is green, and this produces gametes carrying G.

Similarly a yellow pea self-fertilised produces a yellow zygote (YY), which produces gametes carrying Y. When a green pea is cross-fertilised by a yellow pea, the hybrid yellow zygote is YG, and this produces two kinds of gametes, Y and G.

This is Mendel's view of the process—presuming, as he did, that the unit-characters consist of pairs of contrasting characters. The hypothesis of presence and absence of unit-characters, however, necessitates a somewhat different view of the process.

On this view, a yellow pea is based on green, and produces gametes carrying two factors—one for yellowness (Y) and one for greenness (G). When self-fertilised, or fertilised with another yellow pea, the corresponding factors pair and the resulting zygote is yellow based on green (YY + GG). When the zygote produces gametes the corresponding factors segregate and each gamete has the constitution (Y + G).

Similarly a green pea produces gametes carrying two factors—one for greenness (G) and one for absence of yellowness (y). When self-fertilised, or fertilised with another green pea, the corresponding factors pair, and the resulting zygote is green with the constitution (yy + GG). When the zygote produces gametes the corresponding factors segregate, and each gamete has the constitution of (y + G).

So much for the process of pure breeding.

In cross-breeding the process would be as follows:—A green pea, producing gametes (y + G), crossed with a yellow pea producing gametes (Y + G), would produce a hybrid yellow zygote of the constitution (Yy + GG). The hybrid zygote would produce two kinds of gametes (Y + G) and (y + G), one representing yellow based on green, and the other absence of yellow based on green. The practical result is, of course, the same on Mendel's view and on the hypothesis of presence and absence; it is the interpretation of the process that is fundamentally different.

The Nature of the 'Absence' Factor.

Presuming that the presence and absence hypothesis is the correct interpretation, the question arises:—

What is the nature of the presumed gametic factor for absence? A factor for presence is concrete and tangible, but a factor for absence is not so easily comprehended.

With regard to the possible nature of the 'absence' factor, three distinct views present themselves.

(1) There may be a concrete factor literally representing 'absence.'

(2) The factor for 'absence' may represent simply 'presence' in a dormant or latent state.

(3) There may be no factor at all, the presumed factor for 'absence' being simply nothing.

The first view is perhaps the simplest in the abstract, yet it is difficult to comprehend, and also to understand how such a negative factor could have originated.

The second view is, perhaps, the most plausible, but it is open to the serious objection that it implies that 'absence' is not real but only apparent; there is also the further objection that many cases are now known where the 'presence' factor itself exists in a dormant or latent state.

The third view is, perhaps, the most practical, inasmuch as 'absence' is not represented by anything, but this implies a non-pairing of factors in cross-breeding, and the question arises as to how segregation takes place in such cases. In pure breeding, segregation of the pair of factors for 'presence' would be normal, and in the case of two 'absences' neither pairing nor segregation would be necessary; the only difficulty, therefore, is as to how segregation takes place in the gamete of a hybrid carrying some non-paired factors.

Such a process, however, is not inconceivable, for in most cases there would be both paired and unpaired factors together, and where segregation of the paired factors takes place it is conceivable that the same process might also segregate the unpaired factors, the 'presence' factor going into one gamete, nothing corresponding with it going into the other.

All three views are possible, and all are open to some objection; in the present state of knowledge it is difficult to say which of the three is the most reasonable. On the whole, the last view, that 'absence' is simply nothing, certainly appeals to the practical mind, and is perhaps, of the three, the one least open to objection.

On this view mutational variations may consist simply of the addition of new unit-characters and the subtraction of old ones.

The evolution of races, of plants under cultivation and animals under domestication, has most certainly been made possible by these mutational variations.

The precise determination of the unit-characters in plants and animals—by Mendelian analysis or otherwise—is therefore of the utmost importance both to the biologist and the practical breeder.

The biological problem of the future will be not so much the origin of species as the origin of unit-characters.

DISCUSSION.

Miss Wheldale, Cambridge, referring to Mr. Hurst's 'white' Antirrhinums, said there were true albino Antirrhinums existing, but those shown by Mr. Hurst were cream, a colour which contained some yellow. Some creams by self-fertilisation would produce creams, yellows, and whites.

Sir Michael Foster: It would be much more valuable if we called things according to their spectrum nomenclature. We should not then have any confusion.

The President asked if any work giving the spectrum names of colours were published which ordinary people could understand.

Professor Plate said there was such a publication called *Jordan, Farbentafellen*, to be obtained from Jüsfel & Göttel, booksellers, Leipzig.

The "Répertoire de Couleurs," published under the auspices of the French Chrysanthemum Society, a copy of which was procured from the Society's Library, was here produced and handed round the Conference.

Sir Michael Foster: Sir Philip Magnus has shown that there is no difficulty in the matter, and everyone has seen the rainbow.

The President remarked that the difficulty was not in the nomenclature. It was a question of how many factors went to make up these colours.

Professor Wittmack, of Berlin: We have heard that in this white there is yellow. It might be possible to ascertain what we want to know by microscopical investigation. That would be a means of smoothing the way. The great horticultural societies in Germany have endeavoured to give coloured plates, and the German Rose Society have tried to make out tables. But they had to give it up, it was too difficult; and no one knew what the others meant.

Dr. Lotsy, of Leiden: It is important to express the different colours, but that is not the most important thing. I do not care so much whether you express a colour as crimson or magenta. The more serious thing is that where you have two investigators they shall not be calling the same thing by two names.

The President: The facts we have been discussing are very interesting and very important to us, and the question is to what extent we are right in regarding dominance and recessiveness as denoting the presence and absence, respectively, of a factor in those cases where the negative character apparently dominates. That is what is in all our minds in this discussion. Here we have the deeper yellow dominated by the lighter yellow, and we have to find out what it means. Until the problem is settled we shall be in constant difficulties. When we say a character, such as albinism, is pure, it is only that it is pure to its whiteness. There may be other characteristics, cryptomeres, underneath, which we cannot see. We are greatly obliged to Mr. Hurst for bringing these Antirrhinums before the Conference.

RECENT ADVANCES IN ANIMAL BREEDING AND THEIR BEARING ON OUR KNOWLEDGE OF HEREDITY.

By A. D. Darbishire, M.A., Royal College of Science, London.

Curious results are obtained by crossing albino with the so-called Japanese waltzing mice. It is perhaps not necessary to say that an albino mouse is one with an absolutely white coat and with pink eyes, the pink colour in them being due, not to a special pigment, but to the colour of the blood in the vessels at the back of the eye.

The colour of the waltzing mice used in this experiment is best described by saying that were it not for a patch of fawn on the shoulders, and sometimes on the rump, they would be albinos. Their curious movements, inaccurately denoted by the term "waltzing," are not likely to be forgotten by those who have seen them. The animals appear to have no control of the movements of their heads, nor of the direction in which they themselves proceed; and, when they are awake, they spend most of their time in twirling round and round, apparently mad, in a very small circle.

When these two are mated the result is a mouse hardly distinguishable from our common house mouse (when the albino parent is pure bred).

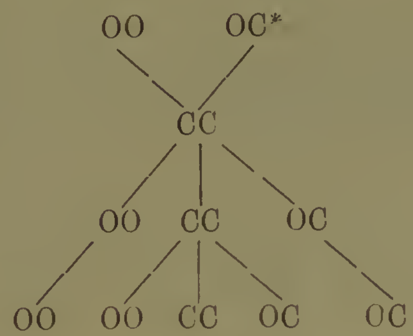
The hybrid, therefore, has a grey-brown coat and coal-black eyes.

We start with a pink-eyed mouse with a colourless coat (which we may denote for brevity's sake by the formula OO)—the albino, and mate it with a mouse which is also pink-eyed, but has a partially-coloured coat (which we may call OC), and get as a result a black-eyed mouse, with a fully-coloured coat (which we may call CC). So much for the nature of the hybrids as far as colour is concerned. Now for their progression. *The hybrids never waltz.* This is true of the hundreds that I have raised.

Let us now consider the result of mating these hybrids together. First with regard to *colour.* The offspring produced by the union of these hybrids fall into the three categories OO, OC, and CC, in the proportions 25, 25, and 50 per cent. respectively. That is to say, *in point of colour,* on the average one mouse in every four is like its albino grandparent; one in every four like its waltzing grandparent; and two in every four like their parents the hybrids. It should be mentioned that all mice falling into the category OC, for example, are not exactly like the Japanese waltzer in colour. For example, the fawn colour may extend over the whole body; or, again, a new colour, lilac, may arise, associated with pink eyes, in this generation. So long as a mouse has pink eyes and some colour in its coat it is reckoned as belonging to the category OC. But the number of colours that can co-exist in a mouse with pink eyes is limited; for example, neither a dark grey nor a black mouse ever has pink eyes.

Now let us look at the posterity of the three kinds of mice denoted by the formulæ OO, OC, and CC. OOs, *i.e.* albinos, when mated together produce only albinos; OCs also when paired breed true with very rare exceptions; while the offspring of CC × CC fall as before into the three categories OO, OC, and CC in proportions which I have not yet determined.

The inheritance of colour in this case is shown at a glance in the following table:—



Now let us consider the generation, produced by mating the hybrids, from the point of view of its progression. Less than a quarter of them waltz. But the deficiency is probably due to the fact that waltzers are constitutionally weak creatures and are more likely to die before they reach the age at which their characters can be recorded than other mice are. What is of interest is that the waltzing habit is not necessarily associated with that colour character OC with which it is associated in the pure strain, but is distributed at random over the three categories, OO, OC, and CC.

We have so far confined ourselves to the description of phenomena. Now let us consider two theories which have been put forward to account for them; first, one by Von Guaita associated with the name of Weismann, and secondly, one by Bateson associated with the name of Mendel.

The theory suggested by Von Guaita was intended to account for the results of a hybridisation experiment, similar to mine except for the fact that his waltzers had black eyes, carried out by himself.

He suggested that there were two kinds of factors in a germ-cell that gives rise to an albino; one, which we may call M, which determines that the organism which develops from the germ-cell shall be a mouse, and another, A, which makes that mouse an albino. And similarly in the case of the waltzer, its germ-cell contains an M similar to that of the albino, and a factor, W, which makes it what it is, a waltzer with pink eyes and fawn-and-white coat. Now M and M are supposed to be the same; but A and W different and antagonistic. So that when an albino and a waltzer are mated it is a question which of the two factors which are antagonistic, W or A, will be manifested in the offspring. What

* It may be objected that I have introduced an element of confusion by representing one homozygote by two similar letters, the other by two different ones, and the heterozygote by two similar ones. But this objection is successfully met by saying that my formulæ can only lead to confusion among those students who have not been taught that such formulæ are nothing more than conceptual descriptions of features of the germ-cells which we have not yet perceived. And such beginners will be confused anyhow.

happens according to Von Guaita's theory is that the two factors W and A struggle, and neutralise each other so that neither of them is manifested in the offspring, leaving the two Ms, which are similar and compatible, in sole possession of the field. This theory accounts in a very ingenious way for the character of the hybrid : and doubtless some elaboration of it could be suggested which would account for the reproduction of the three categories OO, OC, and CC in the next generation.

The theory put forward by Bateson two years ago to account for these phenomena is that there is in the germ-cell of an albino a factor determining albinism, which he calls g, and similarly that there is a factor g' in the waltzer, determining its colour characters, which we have already called OC.

The result of the union of g and g' is a hybrid $g'g$, whose character we have already denoted by CC. The result of the union of g' and g—the production of a form more different from either of them than they are from each other—is said to be a specific result in the sense that the production of water is said to be the specific result of the union of H_2 and O. But the most striking part of this theory is that which refers to the germ-cells of the hybrid. According to it, 50 per cent. of the germ-cells of the hybrid bear the factor g' and 50 per cent. g. Now the result of the union of two hybrids each containing (50 per cent. g') + (50 per cent. g) germ-cells is the production of offspring falling into the following categories in the proportions indicated by the numbers prefixed to them—25 $g'g'$ (or g'), 25 $g'g$, 25 gg', and 25 gg, or ($g'g$ and gg' being the same) 25 g', 50 $g'g$, and 25 g—which, the reader will remember, is the actual result.

This proportion is simply the result of the random union of the gametes of the hybrids, and can be illustrated by making pairs of counters by taking one of the pair at random from a hat containing red (R) and white (W) counters in equal numbers, and the other of them from another hat with similar contents. The result of a large number of trials will be in percentage 25RR, 25RW, 25WR, and 25WW, or 25RR, 50RW, 25WW.

The difference between the above-outlined Mendelian and Weismannian theories is that while the former tries to account for the segregation and not for the reversion, the latter tries to account for the reversion and not for the segregation. It is when we fix our attention on that part of the Mendelian theory which refers to the nature of the gametes of the hybrid that we see what the doctrine of gametic purity really means, how profoundly new and definite a thing that theory is, and how widely it differs from any other theory of heredity whatsoever.

Let us imagine that we have one of our hybrids, with its rich brown coat and black bead-like eyes, before us ; a mouse that we might easily mistake for a wild one caught in a trap, if we did not know its parentage. According to the particular Mendelian theory we have been discussing, *none of the gametes of this mouse contain an element representing the character of the animal which bears it*, namely, $g'g$. But half of the gametes bear the element, g' and half g. The fact that 50 per cent. of the children of such hybrids are like their parents is not due to the presence in the germ-cells of their parents of any elements representing their own characters, but to the chance union of g' and g, borne by different parents. A hybrid in this generation—F_2—is, therefore, never produced by the union of similar, but always by the union of dissimilar gametes; and if an F_1 hybrid could multiply parthenogenetically, none of its offspring would be like itself. This theory as to the nature of the character-representing elements borne by the hybrids is so remarkable that one requires very strong evidence for it, to believe it. The only evidence so far adduced is that the proportions in which the various kinds of young occur are those demanded by the theory; but this does not prove the theory to be true.

The question we have to ask ourselves in considering the value of the evidence for an hypothesis is not "How many cases are there which are consonant with its truth?" but "Is there a single case which is not?" The list of cases in which the proportion 1 : 2 : 1 obtains in the F_2 generation is lengthening every day, and it is imagined that the value of the evidence for this particular theory becomes greater as the list becomes longer. The simple truth, that I have stated in the form of two questions above, is often forgotten.

We are too apt to think that it is sufficient to rest content with the many that are with us; and too ready to forget that we ought to be up and seeking out one that may be against us.

Are there any facts which render the above-outlined Mendelian theory untenable? I have at my disposal two, to only one of which will I refer now. I may say by way of preface that I do not wish my remarks to be construed as antagonistic to Mendelian theory as a whole, but merely critical of a particular hypothesis bearing that name, which was put forward two years ago: an hypothesis in which Mr. Bateson ceased to believe before I did.

At a time when I still thought that it was a useful subject for investigation to try to find out which of the two theories, Galton's or Mendel's, fitted the result of my experiment best, I obtained a result that was apparently conclusive in favour of the former.

The result, which I have described before, but which I may briefly recapitulate here, was obtained by tabulating the difference between the results of making two kinds of hybrids differing not in any visible character but in their pedigree. The two kinds of hybrids that were used were (i) a hybrid produced by the union of two hybrids, and (ii) a hybrid produced by crossing a hybrid with an albino; we may call the former HH and the latter HA. Three kinds of matings can be made with this material; namely, HH × HH, HH × HA, and HA × HA. In each of these types of union a hybrid is mated with a hybrid. So that I argued that according to the Mendelian theory a quarter of the population produced in each of the three cases should be albino; but that according to the commonly accepted view of heredity, known as the law of contribution, one would expect the proportion of albinos to be greater in proportion as the number of albinos in the pedigree of the hybrid parents was greater. This was found to be the case. But it was pointed out to me that this result was not evidence against Mendel's theory, unless I had established the hybrid nature of every individual used in the experiment. "Have you done this?" I was asked. "Are there any cases of families in unions of type (i) where no albinos have been produced?"

"Yes," was my reply. "Then one or both the parents of these families were really dominants," was the answer to me.

I did not think that this theory was true, because it seemed to me that to say, that, unless a quarter of the family produced by an animal consisted of recessives it was not a hybrid, was a very easy way of establishing the fact that hybrids always produce recessives in the proportion of one in every four. It seemed that an argument of that kind was not likely to be based on anything having any existence in nature, but it is a strong warning not to be led away by appearances that when I tested this theory I found it to be true. The animals falling into the category CC in F_2 are sharply distinguished into two kinds: (i) hybrids that will produce albinos in the proportion of one in every four, and (ii) dominants which when mated *inter se* produce no albinos at all, and when mated with albinos are dominant over them. I have proved these two kinds to exist. But the existence of the second of them is fatal to the suggested Mendelian theory outlined above. For how could individuals whose germ-cells appear to bear a new unit $g'g$ arise, from a hybrid in which the germ-cells, either carried an element representing g' or g, and never both? The suggestion that neighbouring ova, or spermatozoa, fuse is not likely to meet with general approval, yet it is the only one which will account for their appearance if our Mendelian theory is true. We have to choose between the two improbabilities: (i) that neighbouring germ-cells fuse, and (ii) that none of the germ-cells of the hybrids bear elements representing animals like them; and two probabilities: (i) that neighbouring germ-cells do not fuse, and (ii) that some of the germ-cells of the hybrid contain elements representing animals like them. I choose the probabilities. But, in claiming to have demonstrated the falsity of the Mendelian theory described above, I do not wish to be credited with having "discovered an exception to Mendel's Law." On the contrary, the best measure of the progress which Mendelian inquiry has made in these two years is the fact that while at the beginning of them the existence of hybrids that breed true would have been regarded as a difficulty, to-day a reasonable explanation of their occurrence has been given.

Progress in knowledge is made by the suggestion of hypotheses, and their rejection when found to be false; by this means the Mendelian has been able to account for some very complicated cases of segregation, and for reversion in some cases as well.

It is natural to inquire how much experiments of this kind tell us about heredity. We are told that Mendel's law only applies to a very limited class of facts, that there is only a certain very-limited and definite set of characters to which it applies, or that it only deals with the phenomena of hybridisation.

Let us consider these objections one by one. With regard to the first, I would say, what I have said before, that the service which Mendelian theory has done to progress in the study of heredity lies partly in the facts which it has accounted for, and partly in the method which it has introduced; and that even if Mendel's law has a limited application, his method has a great future.

The second objection is merely a detailed expression of the first; it states in what the limitation lies. Mendel's law is said to apply to a very few characters, of which colour stands out pre-eminently among the rest. And although it is true that the list of characters whose inheritance can be described in terms of Mendel's law comprises many other characters than colour, *e.g.* the shape of the comb in fowls, the waltzing habit in mice, and even, lately, resistance to disease in plants, it is nevertheless true that the number of characters to which Mendel's law can be said to apply is very small indeed when compared with the number of characters which go to make up an organism. And it can be said with some truth that the characters with which the hybridiser can deal are in a sense superficial. When we cross an albino and a waltzing mouse the result is to our eyes remarkably different from either parent; it is like a wild mouse, but it is a mouse. The features in which it differs from its parents are its colour, its progression—it never waltzes like one of its parents—and to a certain extent its vigour and temperament, for it is healthier and wilder than either parent: but, it is still a mouse. The charge is brought against the hybridiser that he can only stir up the surface, but that he cannot disturb the depths. My answer to this objection is that it is entirely well founded; that there certainly are two sets of characters, one which can be affected by hybridisation, and another, a much larger one, which cannot, and that it is legitimate to regard the former as upper and the latter as lower. By saying this I do not mean to subscribe to the view that recently arisen characters have less tendency to be transmitted than old ones. The case of snails will illustrate my meaning. In *Helix nemoralis* the unbanded condition is dominant over the banded. Now, it is probable that some form of banding is more ancient than colourlessness, and still more probable that some form of colouring at any rate is more ancient than colourlessness, yet absence of colour is dominant over colour. But my point is that in crossing these snails the only thing affected is colour; this is almost true when *H. nemoralis* is crossed with another species, *H. hortensis*: it is not quite true because the cast of the spire of the shell is also altered, but the main thing which is affected is the colour. The animal was a snail, a *Helix*, before it had any definite colour; and, even after it had become stamped as *Helix*, probably underwent many alterations in coloration. I hold that it cannot be denied that the characters with which one deals are in this sense superficial. But I do not think that this need be regarded as a damaging admission by the hybridiser. On the contrary, I hold that the recognition of a limit between the two sets of characters—alterable and unalterable—is desirable, and that the discovery of the difference between the kinds of characters which it separates would be intensely interesting.

The objection that the Mendelian only deals with hybridisation phenomena—doubtless very interesting and important phenomena, it is often perhaps semi-ironically granted—must be met. Those who urge it complain, "It is all very well to tell us about hybridisation—about the result of the union of unlike; we want to know about the union of like. Hybridisation seldom occurs in nature, and when it does the results are more perplexing than in the case of crossing domesticated breeds. What we want to know is, 'What is the mechanism by which the similarity between parent and son is brought about?' The existence of a form *Triton Blasii*, which is a cross between *T. marmoratus* and *T. cristatus*, is undoubtedly interesting, but it is an anomaly. I want to know how it is that the offspring of the crested newt is like its parent. And this you can't tell me. I want to know about normal heredity; you give me nothing but information about abnormal. I ask for bread, and you give me what is to me a stone; interesting and curious, but still a stone."

I should answer objections of this kind by asking, "But why abnormal?" Why should we regard the disintegration of biological units as more "abnormal" than that of chemical ones? It is only by experiment with "abnormal" phenomena that the chemist has progressed. If he had stuck as rigidly to the observation of "normal" water as those who bring this objection against the hybridiser would have him do, he would know as little about the chemistry of water as the biologist did about heredity before he began to experiment with it.

But this answer, though it sounds plausible enough at first hearing, can only be thoroughly satisfactory to those who urge this objection if we can show them that the appearance of abnormality is merely due to the fact that we are dealing with *normal* units in an "abnormal" condition (the result of disturbance by cross-breeding), and if we can show them that we really are not dealing with an abnormal hereditary phenomenon.

Now what are we to understand by abnormal? The most definite formulation of what is meant by abnormality in heredity is that of Dr. Archdall Reid. According to him alternative inheritance has been evolved as a means of keeping the sexes separate, or, to put it in a teleological way, of ensuring that an individual shall be either a male or a female. When the alternative mode of inheritance first became differentiated it was only sex which was inherited in this way. But just as sex, so to speak, sometimes makes a mistake, and trespasses on forms of heredity which do not belong to it, and *blends* in inheritance, with the result that a hermaphrodite is produced, so sometimes not-sexual characters, albinism for example, trespass on the mode of inheritance reserved for sex and are inherited alternatively. Mendelians, says Dr. Reid, have lately suggested that the inheritance of sexual characters may be Mendelian. We shall be much nearer the truth, he thinks, if we say that the inheritance of Mendelian characters is sexual.

There is undoubtedly a parallel between the manner in which Mendelian and that in which sexual characters are inherited. The Mendelian view is that Mendel's work has provided us with conceptions which will enable us to account for the mass of hereditary phenomena; the latest extension of the method being an attempt to account for the phenomena of the inheritance even of sex by it. Dr. Reid's view is that Mendelian phenomena are merely anomalies which are the result of the accidental association of certain varietal characters with a mode of inheritance primarily evolved to ensure bisexuality. This view may or may not be right; but it deserves careful consideration because one of the most deep-rooted weaknesses of the mind is the tendency to regard that with which we have been acquainted for the longest time as the starting-point from which we must proceed to other things. For example, the most hopeless confusion characterised the attempts that were made to homologise the body cavities in leeches so long as zoologists persisted in regarding the cœlom of that member of the *Hirudinea* with which they had been longest acquainted—the medicinal leech—as their starting-point, and in interpreting the state of affairs in other leeches in terms of this one. And the question remained in darkness until a few years ago, when Asajiro Oka showed that the cœlom of the medicinal leech, far from being the starting-point, formed the very last term of a series of gradual modifications of the body cavity; and in fact that the anatomy of this leech could not be understood without a knowledge of the series of which it was the culmination.

References to recent literature on this subject will be found in a paper by me entitled "On the Difference between Physiological and Statistical Laws of Heredity;" "Manchester Memoirs," vol. 1. (1906), No. 11.

DOMINANCE OF CHARACTERISTICS IN POULTRY.

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According to Mendel's first principle, when two opposed characteristics (allelomorphs, Bateson) meet in hybridisation, one only appears in the offspring; this character is called dominant, while the occluded characteristic is called recessive. The question is: What determines which of the two characteristics shall dominate? Is there any general law of dominance?

Three hypotheses have been formulated. First, it has been suggested by de Vries and others that the allelomorph belonging to the older species dominates. But this cannot be a general law, for it implies that all of the characteristics of the one species shall dominate over all those of the other species, and this is certainly not usually true. Second, Correns has concluded that, in general, the phylogenetically more advanced characteristic—the later originated, younger characteristic—dominates. Third, there is an hypothesis proposed by de Vries, and based upon his dictum that a variety differs from the parent species in that at least one characteristic of the species has become latent in the variety. Then, when an individual having a certain characteristic patent is crossed with one in which that characteristic is latent, the patent characteristic is dominant; the latent, recessive.

The two latter hypotheses have been tested on poultry, a group that shows a great number of allelomorphs. To test the Correns hypothesis the older and newer allelomorphs are placed opposite each other in parallel columns and the dominant characteristics are italicised.

Old Characteristics	New Characteristics
1. Single comb.	*Pea comb.*
2. Single comb.	*Rose comb.*
3. *Low nostril.*	High nostril.
4. *Plain skull.*	Cerebral hernia.
5. Plain head.	*Crest.*
6. No muffling.	*Muffling.*
7. Plain feathers.	*Frizzled* feathers.
8. *Non-silkiness.*	Silkiness.
9. White skin.	*Black skin.*
10. Red iris.	*Black iris.*
11. Black plumage.	*White plumage.*
12. Red plumage.	*Black; no red.*
13. *Shafting*	No shafting.
14. *Pencilling.*	No pencilling.

This table shows that, out of fourteen characteristics, five old ones are dominant and nine new ones. Clearly, dominance in poultry is not determined by the age of the characteristic.

The second table is arranged to test de Vries's hypothesis of dominance of patent over latent characteristics. As not all of the preceding characters can be placed in these two categories, this list differs from the last.

Characteristic	Patent Condition	Latent Condition
1. Nasal process of premaxillary	*Narrow nostril*	High nostril
2. Cerebral closure	*Plain skull*	Cerebral hernia
3. Crest	*Present*	Absent
4. Complete development of feather	*Non-silkiness*	Silkiness
5. Plumage pigment	Black and red	*White* (usually)
6. Shafting	*Present*	Absent
7. Pencilling	*Present*	Absent

This table shows that of the foregoing seven characters six are dominant in the patent condition. The exceptional case of white pigment is not universally dominant. The result indicates that de Vries's law is a valid one where the allelomorphs can be classified as patent and latent respectively. The law has, however, this plain limit to its applicability.

A more general expression of the law of dominance in poultry is this: a progressive variation, one which means a further stage in ontogeny (whether novel or ancient, and without reference to latency or patency), will be dominant; a variation that is due to abbreviation of the ontogenic process, which depends on something having dropped out, will be recessive. The following table shows this relation :

Characteristic	Progressive Condition	Arrested Condition
1. } Comb	*Pea*	Single
2. } Comb	*Rose*	Single
3. Nasal process of premaxillary	Developed; *narrow nostril*	Undeveloped; wide nostril
4. Cerebral closure	Perfect; *plain skull*	Imperfect; hernia
5. Crest	*Present*	Absent
6. } Feather-form	Typical; *plain*	Embryonic; silky
7. } Feather-form	*Frizzled*	Plain
8. Muffling	*Present*	Absent
9. Skin colour	Pigmented; *black*	White
10. Iris colour	Pigmented; *black*	Red
11. Plumage colour	Pigmented	*White* (usually)
12. Melanic pigmentation	Melanism; *wholly black*	Red and black pigmented
13. Shafting	*Present*	Absent
14. Pencilling	*Present*	Absent

Of the foregoing fourteen characters thirteen have the more progressive condition of the characteristic dominant. The exception is again plumage colour, which is, as stated, not always an exception.

To sum up, I think the evidence warrants the conclusion that, in poultry, dominance of a characteristic in hybridisation is usually determined by the same causes as determine the appearance in the race of a progressive variation.

ON THE THEORY OF INHERITANCE OF QUANTITATIVE COMPOUND CHARACTERS ON THE BASIS OF MENDEL'S LAWS—A PRELIMINARY NOTE.

By G. Udny Yule, University College, London.

In his memoir of 1904 "On a generalised theory of Alternative Inheritance with especial reference to Mendel's laws" (Phil. Trans. Roy. Soc. A, vol. 203) Professor Pearson laid the foundation of the theory of inheritance of a quantitative character determined by n allelomorphic pairs, in a race of which the individuals mate at random. Distinguishing the three types of couplet that can occur as "protogenic," "heterogenic," and "allogenic," he discussed only the theory of inheritance of the number of pairs of the first or last type. Parents containing, say, m allogenic couplets will, he showed, give rise to offspring containing on the average only $\frac{1}{3}$ of m such couplets; that is to say, as the variability of the two successive generations is the same, the coefficient of correlation between parents and offspring is, for this character, $\frac{1}{3}$. The similar coefficients between grandparents and grandchildren, great-grandparents and great-grandchildren were found to be $\frac{1}{6}$, $\frac{1}{12}$ and so on, all these values being quite independent of the total number of couplets by which the character was determined. But the coefficients of correlation between parents and offspring that have been determined from actual data are for the most part greater than $\frac{1}{3}$, and moreover appear to exhibit significant differences as compared with one another. Professor Pearson concluded, accordingly, that the theory was "not sufficiently elastic to cover the observed facts" (p. 73); that "when we come to the actual numerical values for the coefficients of heredity deducible from such a theory of the pure gamete, they do not accord with observation. They diverge in two ways. First, they give a rigid value for these coefficients for all races and characters—a result not in reasonable accordance with observation. Secondly, they give values distinctly too small, as compared with the average values, or with the modal values of large series of population observations." (p. 85).

There does not appear to be any justification in the memoir, however, for the very wide statement in the second passage cited regarding "all races and characters." The only character there dealt with is the number of allogenic or protogenic couplets, and no reason is shown for supposing that this is typical of all characters. There did not appear to me, moreover, to be any obvious reason for making such a supposition, and I accordingly endeavoured to work out a slightly more general, though still quite limited case. Imagine a *length* to be made up of a number of distinct segments, the length of each of which is determined by an independent allelomorphic pair. Let each segment take the length a, b, or c, according as the corresponding protozygote, heterozygote, or allozygote is present; then the total length L is related to the number of proto-, hetero-, and allogenic pairs determining it, m_1, m_2, and m_3 by a relation of the form:

$$L = am_1 + bm_2 + cm_3 \qquad (1)$$

Using methods which are relatively much simpler than those employed by Professor Pearson, the value found for the coefficient of correlation between parent and offspring for such a character was:

$$R = \frac{(a - c)^2}{2 (a - c)^2 + (a - 2b + c)^2} \qquad (2)$$

If, now, either $a = b$ or $b = c$, the case reduces to that of dominance, one of the homozygotes giving rise to the same somatic character as the heterozygote: this is virtually the case discussed by Pearson, and accordingly the value of R is the same as that found by him, viz. $\frac{1}{3}$. If, however, the heterozygote give rise in every case to a length exactly intermediate between those due to the respective homozygotes, we must have $b = (a + c)/2$, whence $R = \frac{1}{2}$. This is the greatest value that the above expression for R can attain, and consequently a character of the kind considered may exhibit coefficients of heredity lying anywhere between the limits $\frac{1}{3}$ and $\frac{1}{2}$, for random mating of the parents. With homogamy, higher values could, no doubt, be obtained. There is therefore no difficulty in accounting for a coefficient of 0·5 on the theory of segregation, but such a value probably indicates an absence of the somatic phenomenon of dominance. In the case of characters like stature, span, &c. in man this does not seem very improbable.

As regards the coefficients of correlation with the higher ancestry, the theory leads to results which are still rather limited, for the ratio of successive coefficients appears to be always $\frac{1}{2}$; *i.e.* in the case of dominance or Pearson's case we obtain his series $\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{12}$ &c., and in the case of perfect blending the series $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$ &c. This second series implies, it should be noted, a complete absence of "ancestral inheritance" in the proper sense of the term, the partial coefficients of correlation between the offspring and the higher ancestry being all zero.

A complete theory of heredity should take into account, besides germinal processes, the effect of the environment in modifying the soma obtained from any given type of germ-cell—an effect which is hardly likely to be negligible in the case of such a character as stature. This may be done without much difficulty for the limited case discussed. Let us suppose that the protozygote determines segments which have not all the same length a, but, owing to the varying effect of environment, a mean length a and a standard deviation u. Similarly, let the mean lengths and standard deviations of segments determined by the heterozygote and allozygote be b and v, c and w respectively. Then the value of R as given in equation (2) is reduced by the addition of a term

$$3u^2 + 4v^2 + 3w^2$$

to the denominator. The common ratio of the ancestral coefficients remains, however, unaltered at its former value of $\frac{1}{2}$. So far as the coefficients of correlation alone are concerned, it is accordingly impossible to distinguish between the effect of the heterozygote giving rise to forms that are not strictly intermediate, and the effect of the environment in causing somatic variations which are not heritable.

The case taken is a limited one, but the results are sufficient to show that the theory of the pure gamete, as applied to compound characters, is much more flexible than would appear from Professor Pearson's work, and can hardly be summarily dismissed as inapplicable to cases in which the coefficients of correlation approximate to 0·5.

Remarks by the President.

In opening the Second Session of the Conference Mr. Bateson said :—Before we begin the formal business I should like you to look at the three Antirrhinums I hold in my hand. There was some discussion this morning as to whether there was a whiter form than the one then shown. We have come to the conclusion that there is a full yellow form, a less yellow form (white with yellow lips), and a third form which is whiter still. I think Miss Wheldale says there is yet something whiter. But whether it is desirable to talk of one as being cream colour and the other as white is a question, and it is not until we have a physical terminology, as suggested by Sir Michael Foster, that we shall get out of these difficulties of accurate description.

* * *

CERTAIN COMPLICATIONS ARISING IN THE CROSS-BREEDING OF STOCKS.

By Miss E. R. Saunders, F.R.H.S., of Newnham College, Cambridge.

For a statistical investigation of the laws of inheritance, garden stocks offer particularly favourable material, for the forms in cultivation differ in respect of several characters which are sharply marked and easily determined, as, *e.g.*, surface character (whether hoary or glabrous) and flower colour (whether due to coloured or uncoloured sap, and to coloured or uncoloured plastids). The method of cross-breeding has revealed many points of interest in regard to the inheritance of these characters, some of which it is the object of the present paper briefly to describe.

Mendel's idea that to study the inheritance of each character separately was the most likely means of advancing our knowledge of heredity served him as a guiding principle in his now well-known experiments with peas. By a fortunate circumstance his choice fell upon an extremely simple case—a case in which the alternative characters (allelomorphs) are determined by a single factor which was present in one of the two forms crossed together and absent in the other, the relation of the two being that of dominant to recessive; in which, further, each several factor behaved independently of the others. From the results obtained with peas the correctness of the supposition and the value of the method were at once apparent. But it was soon evident that all cases could not be fitted into a simple scheme based upon the presence or absence of so many independent factors. In some cases the results of cross-breeding are extremely complex, and in stocks we have an excellent illustration of a case of this kind. Here the complicated inter-relations existing between the several factors determining the flower colour and surface character often lead to curious results, requiring careful analysis for their elucidation.

A cross between a hoary and a glabrous stock, whatever the flower colour, gives a simple Mendelian result. F_1 is all hoary; F_2 shows a mixture of hoary and glabrous in the ratio of 3 hoary : 1 glabrous or 1 hoary : 1 glabrous, according as F_1 is self-fertilised or crossed back with the original glabrous form. Taken by themselves these results might lead us to suppose that the surface character is determined by a single factor, which when present produces hoariness; when absent, glabrousness. But unsuspected complexities lie behind this apparently simple Mendelian result, and further investigation shows that it cannot be represented by so simple an expression. For though we find that any hoary × any glabrous gives all hoary, and that such hoary crossbreds × self give 3 hoary : 1 glabrous, yet certain *glabrous* strains when crossed together also give all hoary. This latter fact would be inexplicable did glabrousness merely consist in the absence of a hoary factor. But before treating further of the results obtained from the interbreeding of glabrous strains a word must be said about flower colour. The flower colour in stocks may be due to one or both of two distinct causes, viz. colour in the cell-sap and colour in the special cell constituents—the plastids. Non-sap-coloured forms are white or cream according as the plastids are uncoloured or of a pale yellow. Sap-coloured forms are of various shades of red and blue; those with colourless plastids may be termed *self* sap-colours in contradistinction to the *bicolours* in which both sap and plastids are coloured. In both forms the character of the plastids is masked by the sap-colour except in the centre of the flower where the ground is white in the selfs, cream-coloured in the bicolours. Now when glabrous strains (" wallflower-leaved " forms of florists) are bred together the results, briefly stated, are as follows:

1. Any glabrous sap-colour × any glabrous sap-colour
   |
   F_1 = All glabrous sap-coloured
   |
   F_2 = All glabrous sap-coloured

2. Any glabrous sap-colour × a glabrous non-sap-colour (*i.e.* white or cream)
   |
   F_1 = All hoary sap-coloured
   |
   F_2 = 9 hoary sap-coloured : 3 glabrous sap-coloured : 4 glabrous non-sap-coloured.

3. Glabrous white × glabrous cream
   |
   F_1 = All hoary sap-coloured
   |
   F_2 = 9 hoary sap-coloured : 7 glabrous non-sap-coloured

It has already been shown that the idea of a single factor, which, if present produces hoariness, if absent glabrousness, cannot apply here, for on such a view it should be impossible to obtain hoary offspring when both parents are glabrous. Neither can it be that in *each* glabrous strain *one or other* of two complementary factors is present, and that in certain combinations the two factors meet and produce hoariness. For, *in whatever way* we pair forms such as white and cream and red, the offspring in F_1 are in every case hoary. We are driven to conclude that in some way the surface character is dependent upon the flower colour, and, in fact, it is only when both these characters are considered in conjunction, that the system of inter-relationships underlying the above results becomes intelligible.

The proportion of 9 sap-coloured to 7 non-sap-coloured in F_2 from white × cream, and of 3 sap-coloured to 1 non-sap-coloured in F_2 from white or cream × a sap-colour is at once explained if we assume that two factors are necessary for the production of sap-colour, and that one of these is present in white and the other in cream. Red or any other sap-colour, *ex hypothesi*, contains both. If we indicate these factors by C and R, and their absence by c and r, we may write Cr and cR respectively for white and cream, CR for any sap-colour. From the cross Cr × cR, where only individuals containing both C and R have coloured sap, we should expect in F_2 9 coloured to 7 uncoloured; from the cross CR × cR or CR × Cr we should, on the other hand, expect 3 coloured to 1 uncoloured in F_2. This, as previously stated, is precisely what occurs; by the above supposition we can therefore satisfactorily explain the general facts as regards sap-colour. We may now return to the consideration of surface character. Though the facts given under 2 and 3 point to the existence of two complementary factors which on meeting produce hoariness, the supposition that *only one or other* is present in each glabrous strain will not account for the results observed. A noticeable feature in both cases is the absence of non-sap-coloured hoary plants in F_2. This fact furnishes the required clue. Hoariness, it appears, cannot be manifested unless C and R are both present together with H and K.

If this be so, it follows that sap-coloured glabrous types must lack one or both of these factors; but on this view non-sap-coloured forms may contain both, or, again, may lack one or both. Hence if we denote the presence of these factors by H and K, their absence by h and k, glabrous sap-colours must be either CRHk * or CRhk in composition, non-sap-colours Cr (or cR) HK, Cr (or cR) Hk, or Cr (or cR) hk. On working out the results of these several alternatives it will be found that the expressions CrHK and cRHK for the two non-sap-colours, and CRHk for all glabrous sap-colours alone yield the results given above. The accompanying figures will serve to make this clear.

The foregoing scheme provides an explanation of the complications underlying the results obtained with regard to surface character and presence or absence of sap-colour. The diversity of F_2 forms appearing in certain crosses in the sap-coloured class are evidently due to other additional factors, which are merely superposed upon those already considered, and call for little remark. Thus in F_2 from white glabrous × cream glabrous we can distinguish by inspection, without further breeding, ten distinct forms, viz. four self sap-colours—purple, red, plum, copper—

* We are unable to distinguish between H and K; hence the choice of H rather than K in these expressions is purely arbitrary. The same applies to C and R in the case of white and cream.

the four corresponding bi-colours, white, and cream. Plastid colour behaves as a recessive character. Hence white and self sap-colours are dominant over cream and bi-colours respectively. The sap-colours occur in the proportion of 9 purple : 3 red : 3 plum : 1 copper; and among the non-sap-colours the white are to the cream as 3 : 1.* The appearance of this sap-colour series is due to the presence in the white parent of two independent factors, neither of which can be detected unless C and R are both present, viz. (1) a factor (B) which turns the colour produced when C meets R blue (in the absence of B the colour is red); and (2) a factor which produces the modified shades, copper and plum, in place of the full red and purple. Both these factors are absent in cream, for cream glabrous × red glabrous gives red F_1 and only the full colours in F_2;

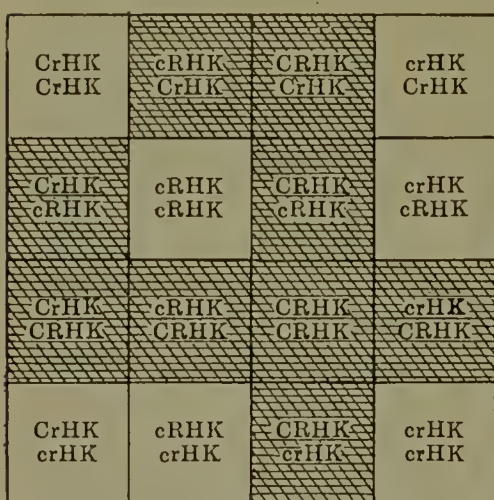

FIG. 1.—SCHEME OF F_2 FROM WHITE GLABROUS × CREAM GLABROUS (CrHK × cRHK).

Ratio of hoary to glabrous = 9 . 7.
Ratio of coloured to uncoloured = 9 : 7.

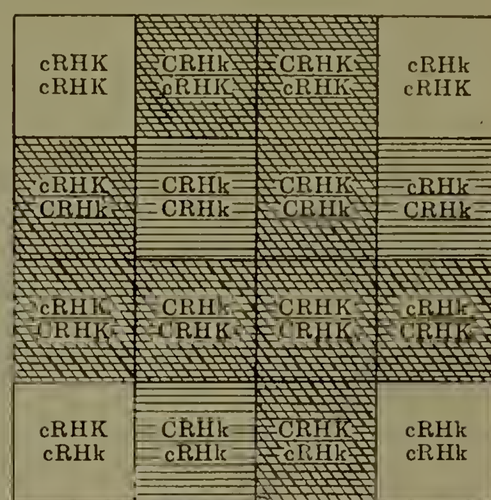

FIG. 2.—SCHEME OF F_2 FROM CREAM GLABROUS × SAP-COLOURED GLABROUS (cRHK × CRHk).

Ratio of hoary to glabrous = 9 : 7.
Ratio of coloured to uncoloured = 3 : 1.

Each square represents an individual, and the lettering shows its composition. Horizontal hatching shows sap colour in the flower. Oblique hatching indicates hoariness. When C and R are both present, the flower is coloured. When H and K are also present, the plant is hoary; it is glabrous when one or more of these four factors is absent.

whereas white glabrous × red glabrous gives purple F_1 and both the full and the modified colours in F_2.

The pale shades occurring in certain other glabrous types, such as flesh and pale purple, which are dominant over the full colours, are probably due to the presence of yet another factor.

It appears, however, that we have not even yet exhausted the list of factors which may affect the flower-colour in cases such as those which we have been considering. There is some evidence that when the parents are individuals which yield both single- and double-flowered offspring, there may be a certain amount of coupling between the character of the flower and some of the colour factors. One strain of ten-week stocks,

* In certain cases, however, a deficiency of creams was observed. Whether this is due merely to the late-flowering habit of the creams or to a further complication is not yet clear.

for example, has the peculiarity that the singles are white while the doubles are cream. Experiments are now in progress with a view to determining to what extent colour is affected by the occurrence of doubling. Apart from this possible complication the general scheme underlying these various results is now clear, and we can now perceive that many which at first sight appeared irregular and paradoxical are in reality orderly and consistent—due to the existence of definite relationships between factors which are typically Mendelian in their behaviour.

CORRIGENDUM.—Since this paper was printed Mr. Doncaster has called attention to an error in the account given here and in Report to the Evolution Committee of the Royal Society, III. A study of the figures and diagrams shows that the postulate of *two* hoariness factors (H and K) is unnecessary, and that *one* (K), in addition to the colour factors, is sufficient to represent the whole series of phenomena. This unfortunate mistake arose through a misinterpretation made in earlier stages of the analysis, which was carelessly retained after each of the results on which it was based had been otherwise elucidated.

DISCUSSION.

Professor Tschermak asked whether Miss Saunders began with uniform whites or whites extracted from other colours.

Miss Saunders said they were all pure whites belonging to the same strain. There were, of course, as she understood, different varieties of whites. For example, by crossing an ordinary white *Matthiola incana* with a glabrous red—a white hoary form with a glabrous red—they got in F_2 whites, which were different from the whites which they got if they crossed a white glabrous with a cream glabrous.

The President said that Brompton stocks did not behave exactly as did Ten-week stocks.

Miss Saunders added that there was one other point she had intended to mention, and that was that there appeared to be a curious connection between flower colour and the occurrence of doubling. There was a well-known strain of Ten-week stocks in which the singles were all-white and the doubles all-cream colour, and there appeared to be a curious coupling between the character of the flower and the colour factor. As to that, her experiments were still in progress.

The President said the matter was very complicated. The distribution of doubling was the most difficult of all cases they had heard of to bring under any kind of systematic Mendelian scheme. They had a single variety throwing off the double form, and yet the double form in stocks did not set seeds. They were given off by single stocks which did set seeds, and in the general run Miss Saunders's work showed that the double was recessive to the single. But another remarkable paradox was that some of the single varieties might give as much as 80 per cent. of doubles. How that was to be dealt with as a question of physiology was at present entirely unknown. They all hoped that Miss Saunders would succeed in solving the problem.

Mr. Robert Fenn, V.M.H.: I think that planting the single stock in conjunction with the double stock gives a predominance of the double stock from the single variety.

Mr. Arthur Sutton, V.M.H., said that the question of single stocks giving double flowers was an intensely interesting one. He would say no more about stocks then than that he did not think there was anything in the suggestion of Mr. Fenn. There must be something absolutely inherent in the strain. He very much hoped that Miss Saunders would be able to elucidate the mystery.

Mr. Alexander Dean, V.M.H., said that some years ago the matter was fought out in the pages of the "Gardiners' Chronicle." There was a belief, in regard to Ten-week and Pyramidal stocks, that if the roots were examined it would be found that the roots of the doubles were cramped or crosswise, and that the single flowers were borne upon the tap-rooted plants. That, again, he had reason to believe, was another fallacy. Some years ago he had a very fine strain of Scarlet Brompton stocks, but ultimately they both became so recessive as to be absolutely white, and though he tried to get back the double form he failed to do so, and to-day the true old Brompton stock was not commonly to be met with. Mr. Sutton might have told them whether Ten-week and Pyramidal stocks formed a large proportion of doubles. He believed that it was the case that the seeds were produced under glass-house cultivation, and that the seeds were starved.

Mr. Sutton said that to produce the largest possible percentage of doubles the plants must be grown in pots. Why, he could not tell.

M. Maurice de Vilmorin observed that the only thing he could say was that it seemed characteristic of the individual, which varies so much. If out of the same lot of double stocks they found a certain proportion of plants to give single flowers, and they grew the seeds of those plants, they would find the same proportion of doubles in all. The only way was to select the genealogy of the families which gave doubles.

Mr. Dippe said it was heredity in the families of the stocks that some plants were inclined to give a higher percentage of doubles, and so by a selection they could retain the plants which gave the highest percentage. He did not think by any possibility they could know it from the plant. It had been said that there should be a difference in the length of the pod. He did not know if that was right, and he had not yet finished his examination. It might be possible to tell by the pod. Stocks in Germany were grown in pots as well as in the open ground. That was done in case of failure through storms. The plants were stronger in the open ground than when they were protected from the weather under glass.

The President: It is most interesting to get information first hand on this subject. I do not know whether selecting seeds according to size gives any result.

M. de Vilmorin: No.

Mr. Fenn: It appears to me that the short pod gives more doubles, and that it would be better to use seeds from the shortest pods.

Mr. Dippe: The only way would appear to be to choose those flowers which produce doubles, and to throw the others away.

Miss Saunders: I quite agree with what has been said. We must regard the suggestion of planting double stocks in proximity to single ones as merely a fairy tale. The double stock is absolutely sterile and can have no effect whatever upon a neighbouring plant. If we are to get any further with this question we must breed from individuals. I have been breeding from individuals, and have kept families distinct for some time. I quite agree that there are certain individuals which give a large percentage of doubles, and certainly there are also individuals from which one cannot get a double. I cannot find a red glabrous Ten-week stock that does not throw a double. I have white and cream glabrous belonging to the same strain, and some of them will give doubles and some will not. It is undoubtedly a question of individuals. I have chiefly grown stocks in the open ground because I was aware of the belief that the production of doubles was increased by starvation. One fact is extremely interesting in regard to cross-breeding. If you cross a glabrous Ten-week stock with an individual which produces doubles, in F_2 you get the Mendelian proportions between singles and doubles. Whether or not the doubling is effected by environment, there appears to be a certain regularity. The general result of the crossing of two individuals which are proved to be self-fertilising and are not throwing doubles, will be that you will not get doubles. If neither is throwing doubles you cannot get doubles in a later generation.

Mr. Fenn: If the bee works in the double stock, what is it for? I think the double stock must give pollen.

Miss Saunders: He is not looking for pollen, because there is none.

The President: Perhaps the bee goes to try.

Mr. Fenn: I have seen bees work in the double and then work directly in the single. I think the bee must go for pollen.

Miss Saunders: That is an assumption which, in the face of all our observation, would require very definite proof. Has anyone ever found a single grain of pollen in a double stock?

A CONTRIBUTION TO THE STUDY OF SPONTANEOUS HYBRIDS IN THE EUROPEAN FLORA.

By Monsieur E. G. Camus.

General Observations.

It is only exceptionally that the older writers on systematic botany refer to hybrid plants. Hybridisation was regarded in their days as an exceptional event, and consequently of little importance. To-day it is admitted that hybrids are much less rare than had been supposed. In certain species (*Nasturtium*, *Viola*, *Cistus*, *Dianthus*, *Epilobium*, *Rubus*, *Rosa*, *Potentilla*, *Cirsium*, *Carduus*, *Centaurea*, *Hieracium*, *Verbascum*, *Mentha*, *Rumex*, *Potamogeton*, *Salix*, *Orchis*, *Ophrys*, *Serapias*, *Carex*, &c.) hybridisation is so ordinary an event that it becomes almost commonplace. One must recognise that it is necessary to study the forms tainted by irregular crossing and distinguish them from the species to which they belong, so as to fix the limits of the variation of such species. Teratological forms must further be eliminated, and then the species remain true within the limits of variation.

The botanists of to-day are therefore right in their works (other than in quite elementary ones) in according to the study of hybridisation the place which it should occupy.

I would ask permission to lay before you the general results of the researches and observations which have been devoted to this subject. Much is due to others, but the limits of this communication will not allow of my going into the historical side of the subject. My own observations on hybrids have extended over more than thirty years of herborisation. Devoted collaborators have aided me by their counsel and by the communication of most important documents. The curators of the large Parisian herbaria have also been of great assistance. I therefore hope that the observations of which I submit a *résumé* will prove of some value.

The study of spontaneous hybridisation shows that very considerable differences exist in the families and even in the genera which have been under observation.

Gaertner with reason has said that there are no general laws on the subject of fertility or sterility in hybrids.

The experiments of Naudin have shown that of forty different hybrids about thirty have produced seeds which germinated.

There is a distinct relation between the fertility and the proportion of pollen-grains which are normally formed.

Hybrids have a tendency to revert to specific forms by the operation of their own pollen, and also in forming quadroons as the result of a second crossing with one or other of their parents, which may thus be shown:—

$$(A \times B) \times (A \times B);\ (A \times B) \times A;\ \text{and}\ (A \times B) \times B.$$

Klotzsch propounded the principle that the sterility of hybrids is never due to imperfection in the ovary. But such a rule appears to me to be far from general.

On the other hand, the presence of seeds only indicates a probability of fertility, so long as the seed has not been subjected to a germination producing positive results.

When two very distinct species produce hybrids, one can divide the results into two classes: the one which favours one species, and the other which favours the second parent. These forms are usually intermediate, but without absolute fusion. In a general sense one may say that the vegetative organs approach more nearly to one species and the reproductive organs rather resemble those of the other species, but yet without either being identical.

The inversion of the *rôle* of pollination has no apparent effect on the different series.

In a nomenclature of spontaneous hybrids, there is no reason to indicate which is the pollen-bearing parent. It would be best to write A × B to indicate the series which is more like B, and B × A to represent the series more nearly allied to A. The form A × B, following the alphabetical order, has the advantage of by no means prejudging the hypothetical facts.

The disjunction of the parental characteristics described in Naudin's memoir is an exception.

Floral anomalies in certain species are very frequent, but much less rare in hybrids than in the species.

Certain families produce few or no hybrids. These are especially those in which the fertilisation is early, and has taken place almost as soon as, or even before, the opening of the corolla. Species with introrse anthers give equally few cross-bred products.

Hybridisation between two species may be observed in one locality, but be more or less absent in another. This can easily be explained if the insects which carry the pollen are rare or do not exist in the latter region.

Places where the foliage is so dense as to exclude the light are less visited by insects, and consequently have few hybrids. For this reason one place, which at one time might be favourable, might later be able, in the case of perennials, to preserve the hybrids for a series of years, and yet produce no more of them for a long time afterwards.

For the same reason the clearings in woods are favourable places.

For crossing, the flowers must occur at the same time, but it must not be forgotten that, in two species which do not usually flower simultaneously, the earlier species may have late flowers, and the later one early flowers, which would bring about the necessary contemporary flowering.

When the style is not as yet suitable for the anthers, the period of possible crossing is increased, and the probability consequently favoured.

Another favourable circumstance is the existence of the species mixed together, or at all events in close contiguity with each other, and especially diœcism not merely of the species but of the genus. Bright colours and the presence of nectaries are also factors of importance.

Special Observations on Some Families or Species.

I apply the term "cross" to the pollination of one species by another species, and "products" to the results of such "crosses."

Ranunculaceæ.—A considerable number of crosses. Products: individuals almost isolated. Carpels well formed, but not numerous.

Cruciferæ.—Many vernal species, provided with nectaries, much visited by insects. Forms of crosses numerous, and individuals often abundant (genera, *Arabis, Nasturtium, Cardamine, Draba*). Fruits often abortive.

Cistineæ.*—Forms of crosses very numerous, giving rise to numerous individuals. The cross products bear the impress of the varieties from which they have sprung.

Violaceæ.—Numerous crosses; individuals abundant. Capsules provided with seeds not very numerous.

Caryophylleæ. *Dianthus.*—Crosses easy; number of individuals very large. Capsules often malformed.

Papilionaceæ.—Crosses less numerous; individuals as a rule almost isolated.

Pomaceæ.—The genus *Sorbus* gives frequent hybrids, the species being very distinct; their products are rather variable, even in the same individual, especially in regard to the foliage. The sterile branches often differ very much from the fertile ones. Fructification *nil*, or impoverished.

Rosaceæ. *Rosa, Rubus.*—In these two genera the species are rather badly limited. Hybrids are formed between allied or very distant species. The forms of the crosses are extremely numerous, and their characteristics are ill-defined. The individuals resulting from crosses are relatively abundant. Sterility is frequent, but not absolute in these two families.

Potentilla.—Same observations as for *Rosa* and *Rubus*, but more often sterile.

Onagrarieæ. *Epilobium.*—One of the genera in which hybrids are very frequent. Most often the capsules contain well-formed seeds, of which it would be desirable to know the germinating capacity.

Umbelliferæ.—By reason of precocious fertilisation, hybridisation is an exception in this important order. In the whole order there are only seventeen hybrid forms recognised, and some of them are very doubtful.

Saxifragaceæ.—Crosses numerous; individuals abundant, and fairly stable in form.

Rubiaceæ.—Crosses numerous. Varieties abundant. Fertilisation relative.

Compositæ. *Inula.*—Crosses numerous; individuals abundant.

Senecio.—Crosses very numerous; individuals abundant, often provided with well-formed but not numerous seeds.

Cirsium.—Crosses very numerous; individuals very abundant. In certain hybrids, *C. rigens, C. hybridum*, &c., the root gives rise to several stalks or branches. The leaves of the central stalks frequently differ

* My eminent colleague Dr. Bornet, member of the Institute, has published, respecting this family, works of such importance that I could not attempt to give an analysis of them here.

considerably from those of the lateral stalks or of the lower branches. Fertility relative. In this genus it is easy to observe the mongrels of which I have already spoken above. In certain peat-bogs of the Jura, I have found it impossible to separate the abundant hybrids which I have collected, the forms of reversion being numerous, thus making it very difficult to distinguish the different forms pointed out by authors.

Carduus.—Crosses numerous; individuals abundant. Relative sterility.

Centaurea.—Crosses numerous ; number of individuals very variable ; sterility frequent.

Hieracium.—Hybrids and mongrels very frequent. Fertility relative. Individuals springing from crossings bear evidence of the forms from which they originate ; the same is true of the mongrels (*métis*).

GENTIANACEÆ.—Gentians are easily crossed, but the capsules bear few seeds, and the hybrids produced are not numerous.

PRIMULACEÆ.—The genus *Primula* is one of the most propitious for the study of hybridisation. Certain crosses are very abundant, × *P. digenea* (*vulgaris* × *elatior*), × *P. variabilis* (*officinalis* × *vulgaris*) ; others are much more rare, as × *P. media* (*elatior* × *officinalis*). Where *P. vulgaris*, *elatior*, and *officinalis* are crossed together there always results a great quantity of hybrids of the first two forms. *P. media* is on the contrary an exception, which may be explained by the fact that insects which visit *P. elatior* more rarely despoil *P. officinalis*. The small throat of these two species is perhaps also an obstacle to their visits. In *Primula* the capsules frequently prove abortive or enclose but few well-developed seeds. The calyx is the organ which provides the most definite characteristics by which to recognise traces of descent.

The genus *Soldanella* and *Androsace*, in proportion to the small number of species of which they are composed, provide a large number of hybrids.

SCROPHULARINEÆ. *Verbascum.*—With *Verbascum* as with *Primula* hybridisations are easy ; the results produced are numerous and remarkable for their height, which prevents their escaping observation. Sterility is almost universal in this genus.

Euphrasia and Pedicularis.—Cross-bred forms are numerous, with results varying in number, some very numerous ; others represented by almost isolated individuals.

LABIATÆ.—The genus *Mentha*, on which my eminent colleague M. Malinvaud, President of the Botanical Society of France, has contributed an article, is one of the most interesting from our point of view. Researches, especially those lately undertaken, have shown that hybridisation may often be observed in the following genera ; *Lavandula*, *Salvia*, *Thymus*, *Marrubium*, *Stachys*, *Galeopsis*, *Prunella*, *Teucrium*.

CHENOPODIACEÆ.—Hybridisation rare.

POLYGONACEÆ. *Rumex.*—Hybridisation frequent, results numerous ; fruit more or less abortive.

Polygonum.—Crosses frequent, results numerous, sterility less marked.

CUPULIFERÆ. *Quercus.*—Hybridisation frequent, results keeping the characteristics of the form from which they have originated.

SALICINEÆ. *Salix.*—Hybridisation very frequent, even between species differing much. The frequency of hybridisation is much favoured by the separation of the sexes and the presence of nectaries, and the often intense perfume, diffused by the catkins at the time of the ripening of the anthers.

The species of *Salix* whose flowering season is very early are visited by a great number of insects.

When hybridisation takes place between two quite distinct species, several intermediate, fairly definite, forms are produced. It is not unusual to find individuals of which the branches at the bottom and the sterile branches show by their foliage a very marked dimorphism. The capsules of the hybrids appear in many cases to be almost normal; they enclose numerous seeds which may possibly germinate. On the other hand, there are produced ternary hybrids which prove the fertility of the seeds, or at all events of some of them.

In dwarf willows or in certain forms derived from *Salix nigricans*, the style is often atrophied. The proportionate length of the style is not at all fixed. It is clear that two species having the style short or almost absent will never produce a species with a long style. Willows are frequently planted, consequently the parents must be lacking in the vicinity, if varieties do not appear spontaneously.

Populus.—My excellent friend M. Dode, who is preparing a monograph on the genus *Populus*, declares that hybridisation is almost as common in this genus as with willows.

Potameæ. *Potamogeton*.—Hybridisation fairly frequent. Results fairly numerous; relative sterility.

Orchideæ.—We know how orchids lend themselves to hybridisation. Crossings take place even between species of incontestably distinct genera, such as *Serapias* and *Orchis*.

I will not go into all the well-known causes which facilitate cross-fertilisation. I will simply mention that the organ which appears the most unchanging, and in consequence is frequently a sure guide in determining one of the parents, is the gynophores (gynostème). The form of the spur comes next in order of stability. The form of the lip (labellum), and above all the colour, are very variable points. Hybrids which have been formed in marshy fields have persisted for several years after a drainage which had dried up the locality and swept away one of the parents. The hybrids have not been reproduced on the spot, but they have been multiplied through their bulbs. For several years I have kept hybrids in pots, reproducing themselves by bulbs, and I have only observed variations of colour with a tendency to degenerate.

As to *Cyperaceæ*, *Gramineæ*, *Filicineæ*; recent observations relating to these have demonstrated the frequent action of hybridisation. It is, however, difficult to generalise with the limited materials at our disposal.

[Note by the Editor: Monsieur Camus laid before the Conference an *Analytical Catalogue of Spontaneous Hybrids of European Plants*, containing the original bibliography, the synonyms, the geographical distribution, and the herbarium notes relating to the hybrids—a work on which he has been engaged for upwards of thirty years. Whilst fully recognising the immense value of this catalogue and the really wonderful patience, investigation, and research which it displays, we are of opinion that it is too long to be embodied in a Report of the Conference and merits publication as a separate volume.—W.W.]

ON THE OCCURRENCE OF NATURAL HYBRIDS IN THE GENUS *SARRACENIA*.

By Professor J. M. MACFARLANE, Philadelphia, U.S.A.

EVERY cultivator of the genus *Sarracenia* is familiar with the fact, that many and even complex artificial crosses have been made between the seven species in cultivation, since the time when the late Dr. Moore exhibited his first hybrid plants in 1874 at the International Congress in Florence.

Large and complex though the flowers typically are, they are so perfectly adapted for cross-pollination that one might expect to find natural hybrids, where two or more parents grow together. But such will most likely occur, if the period of blooming for any two species is coeval or approximately so, and if the flowers are so coloured as to attract insects in common. Some details regarding these points have already been published by the writer,* who has had frequent opportunities for studying "the American pitcher-plants" in their native haunts during the past fifteen years.

It may at once be said that abundant and ever-increasing evidence is accumulating, to prove that the species of *Sarracenia* hybridise in their native haunts. Further, some hybridise so frequently, and develop so many hybrid plants, that at least one of these has been mistaken for, and described as, a new species. The first evidence in line with the above was secured by the writer in June of 1893, when in company with one of his students, Mr. W. Davis, he noticed two plants growing by a "branch" or stream in the savannas about three miles south of Wilmington, N.C. Their exact similarity to the artificial hybrid between *S. flava* and *S. purpurea* at once suggested such a parentage. The first-named parent was in striking evidence around, but slight examination of the surrounding ground revealed also a considerable number of *S. purpurea*, which is often obscured on first look by surrounding herbage. The two hybrid specimens were removed, and later on demonstrated their parentage by their floral structure and colour. Proof was thus secured that the artificial hybrid *Sarracenia Stevensi*, with the above parentage, had probably often been produced in nature.

If we may now continue the history of the same hybrid, it should next be said that Elliott described a supposed new species in 1824 under the name *S. Catesbæi*, from leaves which had been forwarded to him by Dr. Macbride of Chesterfield, S.C. Trusting to Elliott's short and defective description, the writer, as well as Small, considered this to be a type which is represented in most herbaria and botanic gardens, and which the writer has dealt with in the above-named article. But through the kind interest of Mr. Rea, Curator of the Charleston Museum, Elliott's original specimen was studied by me about eighteen months ago, and was at once recognised to be a rather large leaf of the same hybrid parentage

* *Bot. Contrib. Univ. Penn.* vol. ii. (1904), p. 426.

as the Wilmington specimens. This again is proof that it was found wild in South Carolina about seventy years ago, and therefore long antedating the period when our modern hybridisers first touched the group. Here it may be said that in February of 1904 I made an extended but rather hasty examination of the Chesterfield region. Though both parents were in great quantity, no traces of the hybrid were obtained. The season of the year was unfavourable for easy observation, but the locality deserves careful study.

It can now be definitely stated that Elliott's plant *S. Catesbæi* was a natural hybrid, not a species, and accordingly his name should be retained for the same hybrid combination, whether natural or artificial. It is thus necessary to secure a name for the very distinct species which centres round the middle Gulf States, and which the writer treated of in the article already quoted. He now proposes *Sarracenia Sledgei*, in honour of Dr. Sledge of Mobile, Ala., who first forwarded abundant material for culture and study about three years ago.

Last summer the writer botanised round the small village of Ponce de Leon in North-Central Florida. Everywhere *S. flava* and *S. purpurea* were abundant, but interspersed among them were so many hybrids, that in a single day's botanising 117 examples were counted. In some instances the plants were rather isolated, in others they were thickly dotted round, as if the product of a single seed-pod. Now both parent species, though very constant in general morphology, showed marked variations in size and particularly in colour. Needless to say, like variations were constantly observed in the hybrids. Nineteen plants were selected at random during the day's excursion, and forwarded to the Botanic Garden of the University of Pennsylvania. Most of these flowered in the *Sarracenia* house during the past spring, and it may well be emphasised that for delicacy and variety of colour combination, as well as for size and form of the flower, this is decidedly the finest hybrid of the genus.

Several specimens were collected that suggested their being second hybrids, in both generations of which *S. flava* was a parent. Two of these bloomed during the past spring, and demonstrated the correctness of the surmise.

But in the Ponce de Leon locality, *S. psittacina* was even more abundant than *S. flava* and *S. purpurea*, so much so, that one with regret trampled over it at every step in the swampy ground. Careful search failed to reveal a single hybrid between *S. psittacina* and the other species around. In slight part such may be due to the later blooming of this, as compared with the other two species, but such does not seem to be the entire explanation. More likely it is that, from differences in the size and structure of the flower, they are visited by different types of insect. That such a hybrid combination as that between *S. purpurea* and *S. psittacina* is possible, has been demonstrated by its first appearance as *S. Courtii* from Mr. Veitch's nursery, by its later production by Mr. Oliver, of the Washington Botanic Garden, and its recent production at our Botanic Garden.

The relative geographical distribution of *S. psittacina* and *S. minor* (*S. variolaris*) gives only a moderate opportunity for the crossing of these species, since the latter is confined to the Atlantic coastal plain, and the former mainly to the circum-Gulf region. Only over a limited area north and south of Waycross, Ga., do the two meet. In the spring of 1904 I received from the southern edge of the region a very typical cross between the two which is now growing healthily. In size, form, colour, pattern and mode of growth, it is about midway between the parents, though the rich red intensity of colour suggests an approach to *S. psittacina.* Both of the parent species are closely synchronous in blooming. Harper has recently * described the same hybrid from four different localities in Georgia.

The distribution of *S. minor* along the Southern Atlantic coastal plain largely limits its opportunities for crossing, to *S. flava* and *S. purpurea.* In 1903 † Mr. Roland Harper recorded the finding of an evident cross between *S. flava* and *S. minor.* In April of the same year, the writer secured three large patches about four miles west of Summerville, S.C., which have since been continuously watched under cultivation. In it the size, shape, and colour of the pitchers, as well as of the flowers, are fairly intermediate, but the typical white areolæ over the pitcher top and back of the *minor* parent are largely minimised in aspect, since the influence of the *flava* parent gives a rather thick dense character to the pitchers of the progeny. Harper has recorded the finding of it again at Douglas, Coffee Co., Georgia, ‡ so that it may be expected to occur occasionally where the two species grow together. A slight obstacle to frequent pollination in nature probably is that the climax of blooming in *S. minor* is ten to fourteen days later than that of *S. flava.*

Sarracenia Drummondii has proved an attractive species to the hybridiser, owing to the size and rich coloration of the leaves. In June of last year the writer visited the Bay Minette region in S.W. Alabama. Here he found great abundance of *S. psittacina* and *S. purpurea*, also considerable quantities of *S. flava* and *S. rubra.* But over the flat or undulating swampy savannas the most striking feature was the luxuriant growth of *S. Drummondii*, which coloured hundreds of acres with its tall reddish-white pitchers. Interspersed among these here and there were patches of *S. flava.* Soon a remarkable-looking patch was descried, which on closer inspection consisted of the large and finely coloured pitchers of a hybrid between the last-mentioned parents. The reticulated red veins and the white areolations over the pitcher top of the one parent were reproduced in reduced intensity, along with the crimson colour of the throat which is typical of the variety of *S. flava* most frequent in that region. Subsequent microscopic study revealed equally interesting details of morphological blending in the cells. Examples of this hybrid have since then flowered in our *Sarracenia* house, and while the blooms might be described as bizarre rather than beautiful, they exhibit like blending as do the pitchers. Three additional patches were observed.

One small area examined was of particular interest. Here grew considerable quantities of the pale greenish-white variety of *S. Drummondii* which horticulturists have named var. *alba.* Among these was a hybrid patch which accurately reproduced the colour condition of the pale variety, though with diminished intensity. Growing plants and herbarium

* *Bull. Torr. Bot. Club*, vol. xxxiii. (1906), p. 236. † *Ibid.* vol. xxx. (1903), p. 332.
‡ *Ibid.* vol. xxxi. (1904), p. 22.

specimens of all were secured, and illustrate well the parental and hybrid peculiarities.

What seems to be the same cross was also observed by R. M. Harper,* near Americus, Ga. As *S. rubra* and *S. Drummondii* were the two parents common around, it is possible that these gave rise to the hybrid progeny. An examination of the flowers, or microscopic study of the leaves, would alone determine this point with accuracy. Sheets of it were distributed by Harper under the name *S. Catesbæi.* He had some reason for such an identification, in view of the short and unsatisfactory diagnosis given originally by Elliott. But this shows that every new species cannot be too minutely described, and if possible illustrated. Historically it is worth noting that the above natural hybrid is identical with *Sarracenia Moorei*, raised by the late Dr. Moore and first exhibited by him at the Florence International Congress of 1874.

The above evidence clearly demonstrates that *S. purpurea*, *S. flava*, *S. minor*, *S. psittacina*, and *S. Drummondii* all hybridise more or less perfectly in the wild state, and even that second hybrids are not extremely rare. All of the hybrids seem to originate where masses of two parents are growing together, or in close proximity. Since the flowers are wholly adapted for insect attraction and pollination, and since the pollen is so discharged from the anthers that it gets wetted and glued together, the transfer of the pollen by insects is effected in a neat and efficient manner. Wherever opportunities occur for such transfer, natural hybrids evidently appear at times. The only two species of which we have as yet no exact hybridisation records are *S. rubra* and *S. Sledgei* (former *S. Catesbæi* of Small and the writer). As regards *S. rubra*, it seldom grows near the other species in conspicuous masses, but rather in scattered clumps, and in shady situations, where chances for insect visits are rarer than in the open. In favourable localities, however, we believe that it will yet be found to hybridise.

So far as accurate records show, *S. Sledgei* seems to be confined to the Gulf region between the Alabama River and Eastern Texas, over which area it may be at times extremely abundant. Here it is only associated with *S. psittacina* and *S. purpurea*, both of which flower from one to two weeks later than it. In spite of this there is every likelihood that hybrids will in time be reported, for the flowers of all the species last from fourteen to twenty-one days, and even at the close of that period the ovarian nectary may be secreting an attractive juice, after the petals have fallen.

Through the kindness of the Director of Kew Gardens some specimens of hybrid Sarracenias with similar parentage to the above were exhibited at the Conference. These were raised in the Botanic Garden of the University of Pennsylvania, and they demonstrate the close similarity of the artificial products to those gathered in the wild state. It is superfluous, in an article like the present, to treat of the microscopic details shown by the tissues of these plants. Suffice it to say that they confirm the conclusions already reached by the writer, that hybrids blend to a marvellously minute degree the details of both parents, while tending at times to sway toward one parent or the other.

* *Bull. Torr. Bot. Club*, vol. xxx. (1903), p. 334.

NATURAL HYBRIDS.

By R. Irwin Lynch, M.A., V.M.H., Curator of the Botanic Garden, Cambridge.

Few realise how frequently hybrids occur in nature or the extent to which they have been recognised by the systematic botanist. His interest in them is not usually that of the readily convinced hybridist; they force themselves upon his attention, and, unwillingly it may be, he is a witness to the reality of their existence. To the student of evolution they must always be of interest, but I think they must also be of importance to the plant-breeder and hybridist, because they belong to the subject of his investigation, and having taken an actual place in nature may be expected to afford valuable material for experiment and observation. In many cases they are actual "species," and none, indeed, know how many so-called pure species may not have originated wholly, or in part, by hybridisation. In parenthesis, and apart from the present subject, the speculation may be advanced that casual hybrids have sometimes a *rôle* in nature, as well as the permanent entity entitled species. Kerner was a strong believer in the origin or making of new species by hybridisation, and he estimated that the number of hybrids brought to light within forty years and recognised in the Flora of Europe could not be less than a thousand. All those certainly could not rank as species, but the time has passed when it could be doubted that hybridisation plays a very important part in evolution. My point is that the plant-breeder's work is nothing but an intellectually guided form of evolution, which must proceed under some of the laws that are concerned in the evolution of Nature. Nature attains its ends, it appears, by a circuitous route and through much waste, insomuch that few only of a large number of hybrids are permanently reproduced, while the plant-breeder, by understanding law, is learning how to work directly towards the results he desires, and he is limited only by the potentiality of the material at his command. In the study of law very much has to be done, and it is with the hope of assisting the experimentalist and the observer that I have prepared the following lists. They are by no means complete, but they include the majority of the genera which are likely to be useful; and as to the crossing of species, it is usual that hybridising habit is similar throughout a genus, though to this there are exceptions. All who study hybrids extensively will, no doubt, consult "Pflanzen-Mischlinge," by Focke, but this work was published in 1881 and is not available to all readers of this report, so that the following lists may be of service.

I have made a selection of British and Continental hybrids, because they are most within reach, and being usually hardy are suitable for experimental work, while to have found a limitation with wider scope would have been difficult. Reference I have given to a single work only, and I may point out that any of the plants may possibly be found in some of the others works I have quoted. Each reference refers to all the plants back to the preceding reference. It is a rule in writing down a hybrid by the names of its parents, with a cross between, that the name first written should be that of the female parent. Here, however, the rule does not apply, because in very few cases is it known how the crossing took place, and in many cases probably the crossing may have happened both ways. In artificial crosses the rule is valuable. I have indicated by means of an asterisk a few of the genera which I believe may best lend themselves either to observation or manipulation combined with easy cultivation and early result, and, by means of a dagger, I have pointed out some of the most remarkable and interesting of the hybrids. I have especially to thank the Rev. E. F. Linton for a list of the British hybrids of Willow, and the Rev. W. Moyle Rogers for an especially prepared list of hybrid *Rubi.* I have attempted no critical work or verification, and I can hope only that something may prove suggestive.

I. British.

Ranunculus Baudotii × Drouetii
× heterophyllus
peltatus × Lenormandi (R. Hiltoni, *H. & J. Groves*)
(See figure in "Journal of Botany," 1901, p. 121.)
× trichophyllus
"Babington's Manual," 9th ed. H. & J. Groves.

Viola canina × lactea
Chatteris, Cambs.
× stagnina
odorata × hirta (V. permixta, *Jord.*)
Riviniana × canina
sylvestris × Riviniana
In Surrey with parents.
"Babington's Manual," 9th ed. H. & J. Groves.
* tricolor—the forms and subspecies of this very variable species are probably deserving of attention for experimental work.—*R. I. L.*

Drosera anglica × rotundifolia (D. obovata, *Mert. & Koch*)
D. obovata is recognised as a hybrid in 9th ed. of the "London Catalogue of British Plants."

*Dianthus deltoides × Armeria
See Continental list of hybrids. This cross occurs in Germany, but is not recorded for Britain. Species of this genus are probably very suitable for experimental work.

Potentilla procumbens × reptans (P. mixta, *Nolte*)
× Tormentilla (P. suberecta, *Zimmeter*)
reptans × Tormentilla (P. italica, *Lehm.*)
"Journal of Botany," 1893, p. 325. "Some British Potentilla Hybrids," by the Rev. E. S. Marshall, M.A.
Dr. Focke remarks that, "as *P. procumbens* is usually associated with *P. Tormentilla*, the hybrid forms may be almost universally found where *P. procumbens* grows, often far more frequent than it."

	Localities known to me
Rubus rusticanus *Merc.* × R. affinis, var. Briggsianus, *Rogers*	Several: Dev., Cornw., Dors.
× cæsius, *Linn.*	Many: Dors., Somers., &c.
× corylifolius, *Sm.*	One: Dors. (certainly elsewhere)
× holerythros, *Focke*	One: Surrey
× argenteus, *Wh. & N.*	One: Somerset
× leucostachys, *Schleich.*	Many: Dors., Hants, Heref., Glam., Derby, Worc.
× mutabilis, *Gener.*	One: Surrey
× Lindleianus, *Lees*	One: Surrey
leucostachys, *Schleich.* × corylifolius, *Sm.*	One: Somers. (certainly frequent elsewhere)
× imbricatus, *Hort.*	One: Surrey
× pulcherrimus, *Neum.*	One: Surrey
× Sprengelii, *Weihe*	One: Derby
× Marshalli, *Focke & Rogers*	One: Surrey
× foliosus, *Wh. & N.*	One: Kent
cæsius, *L.* × Idæus, *L.*, R. Pseudo-idæus, *Lej.*	Four: Dors., Hants, Surrey, Staffs.
holerythros, *Focke* × Sprengelii, *Weihe*	One: Surrey
Lindleianus, *Lees*, × Radula, *Weihe*	One: Derby
mucronatus, *Blox.* × rosaceus, subsp. infecundus, *Rogers*	One: Herefordshire
anglo-saxonicus, *Gelert* × rudis, *Weihe*	One: Derby
× its subsp. setulosus, *Rogers*	One: Herefordshire

The above list of hybrid *Rubi* was kindly sent to me by the Rev. W. Moyle Rogers, an accepted authority on British brambles and author of a "Handbook of British Rubi." He writes: "The foregoing two lists (I have made them one) are certainly not exhaustive for British *Rubi* hybrids. They omit especially a considerable number of the bewildering forms of hybrid origin which are almost invariably frequent in districts where the group *Cæsii* is strongly represented." He also informs me that beyond all question they are fairly numerous in some neighbourhoods, though not nearly so frequent as some suppose, and he expresses the opinion that they are usually infrequent except on clay soils. All the above are represented in Mr. Moyle Rogers's herbarium, with the exception of the last under *R. rusticanus* and the last under *R. leucostachys*

†Geum urbanum × rivale (G. intermedium *Ehrh.*)

This plant has long been recognised as a "species," and its hybrid nature has been proved experimentally by Bell-Salter.

Rosa spinosissima × canina (R. hibernica, *Sm.*)

× villosa, tomentosa, &c. (R. involuta, *Sm.*)

"Babington's Manual," 9th ed. H. & J. Groves.

Pyrus Aucuparia × Aria ? (P. pinnatifida, *Ehrh.*)
Occurs in shrubberies.
"Babington's Manual," 9th ed. H. & J. Groves.
× intermedia (P. fennica, *Bab.*)
See "Journal of Botany," 1897, p. 99.

*Epilobium hirsutum, *L.* × lanceolatum (E. Surreyanum, *Marshall*)
× montanum
× obscurum (E. anglicum, *Marshall*)
× parviflorum

parviflorum, *Schreb.* × roseum

montanum, *L.* × obscurum
× palustre
× parviflorum
× roseum (E. heterocaule, *Borbas*)

lanceolatum, *Seb. & Mauri*, × montanum (E. neogradiense, *Borbas*)
× obscurum (E. Lamotteanum, *Hausskn.*)
× parviflorum
× roseum

adnatum, *Grisebach* × Lamyi (E. semiadnatum, *Borbas*)
× montanum (E. Beckhausii, *Hausskn.*)
× obscurum
× palustre
× parviflorum (E. weissenburgense, *F. Schultz*)

obscurum, *Schreb.* × palustre (E. Schmidtianum, *Rostk.*)
× parviflorum (E. dacicum, *Borbas*)
× roseum (E. brachiatum, *Celakovsky*)

Lamyi, *F. Schultz* × lanceolatum (E. ambigens, *Hausskn.*)
× montanum
× obscurum (E. semiobscurum, *Borbas*)
× parviflorum (E. palatinum, *F. Schultz*)

palustre, *L.* × parviflorum
× roseum, *Schreb.* (E. purpureum, *Fries*)

alsinifolium, *Vill.* × anagallidifolium (E. Boissieri, *Hausskn.*)
× montanum (E. salicifolium, *Facchini*)
× obscurum
× palustre (E. Haynaldianum, *Hausskn.*)

anagallidifolium, *Lam.* × obscurum (E. Marshallianum, *Hausskn.*)
× palustre (E. dasycarpum, *Fr.*)

The above list of *Epilobia* is from the ninth edition of the "London Catalogue," the names of the hybrids, where given, being added from Marshall.

See Rev. Edward S. Marshall, M.A., in "Journal of Botany," 1889, p. 143, and 1890, p. 2. In the latter article he gives the following triple hybrids :—

(montanum × roseum) × roseum
montanum × roseum × parviflorum
(obscurum × palustre) × obscurum

In "Epilobium Notes for 1890," "Journal of Botany," 1891, p. 9, I find—

E. (obscurum × palustre) × palustre

The Rev. E. S. Marshall has published numerous articles on the hybrids of this genus in the "Journal of Botany," extending over a series of years.

†Senecio squalidus × vulgaris

Found at Oxford and Cork. Introduced from the latter locality, it has been a weed in the Cambridge Botanic Garden for some years.

"Babington's Manual," 9th ed. H. & J. Groves.

† Jacobæa × Cineraria (S. albescens, *Burbidge & Colgan*, "Journ. Bot." 1902, p. 401)

Dalkey, co. Dublin.

Arctium minus × tomentosum (A. pubens)

Kerner's "Natural History of Plants," translated by Oliver, ii. p. 586.

Carduus pratensis × palustris (C. Forsteri, *Bab.*—"probably a hybrid")

acaulis × pratensis (C. Woodwardii, *Wats.*—"may be a hybrid")

× arvensis (C. dubius, *Willd.*—"perhaps a hybrid")

heterophyllus × palustris (C. Carolorum, *Jenn.*—"seems to be a hybrid")

crispus × nutans (C. Newbouldi, *H. C. Wats.*)

Egg Buckland, near Plymouth.

"Babington's Manual," 9th ed. H. & J. Groves.

Hieracium boreale × sciaphilum

Railway bank near Rhayader, Radnorshire.

See "Journal of Botany," 1894, p. 232.

anglicum × hypochæroides

See "Journal of Botany," 1893, p. 16.

These are the only two hybrids given in Mr. F. J. Hanbury's tentative list of Hieracia published in the "Journal of Botany" in 1894.

Erica ciliaris × Tetralix (E. Watsoni, *Benth.*)

"London Catalogue."

(*E. Mackaiana* is given by Kerner as a hybrid between these species.)

†Vaccinium Myrtillus × Vitis-Idæa (V. intermedium, *Ruthe*)

"London Catalogue."

*Verbascum Lychnitis × nigrum

× Thapsus

nigrum × pulverulentum

× Thapsus

Near Willey, Surrey.

"London Catalogue."

V. Thapso-Lychnitis, *V. Thapso-nigrum*, *V. nigro-pulverulentum*, and *V. nigro-Lychnitis* are figured in Sowerby's "English Botany," 3rd ed., vol. vi.

Linaria repens × vulgaris

"London Catalogue."

Euphrasia stricta × brevipila

brevipila × scotica

curta var. glabrescens × brevipila

Euphrasia gracilis × brevipila
scotica × gracilis
Rostkoviana × brevipila
× nemorosa
Are recognised as British by Mr. Townsend.
"Babington's Manual," 9th ed., H. & J. Groves.

Scutellaria galericulata × minor
"London Catalogue."

Stachys palustris × sylvatica (S. ambigua)
"London Catalogue."
"Journ. Bot." 1895, p. 167.

Mentha aquatica × M. sylvestris (M. nemorosa)
Kerner's "Natural History of Plants," vol. ii. p. 586.

Primula acaulis × elatior
† × veris (P. variabilis), False Oxlip, is found over almost the whole of Europe.
elatior × veris
"London Catalogue."

Limonium humile (Statice Limonium, sub-sp. rariflora) × L. vulgare (S. Limonium) = Limonium Neumani, *Salmon*
See description with plate in "Journal of Botany," 1904, p. 361.
Occurs at Basham in West Sussex.

Rumex conglomeratus × maritimus
× obtusifolius
× pulcher
pulcher × rupestris
obtusifolius × pulcher
crispus × domesticus (R. acutus)
× obtusifolius (R. acutus)
× pulcher
× sanguineus
domesticus × obtusifolius (R. conspersus, *Hartm.*)
"London Catalogue."
aquaticus × Hydrolapathum (R. maximus)
Kerner's "Natural History of Plants," translated by Oliver, vol. ii. p. 586.

Polygonum Hydropiper × P. Persicaria (Polygonum mite)
Kerner's "Natural History of Plants," translated by Oliver, vol. ii. p. 586.

Salix, *Linn.* (ordine, *E. F. Linton*)
triandra, *Linn.*
× viminalis (hippophæfolia, *Wimm.* et *Grah.*)
× alba (undulata, *Ehrh.*)
fragilis, *Linn.*
× pentandra (cuspidata, *Schultz*)
× triandra ? (decipiens, *Hoffm.*)
alba, *Linn.*
× fragilis (viridis, *Fries*).
× pentandra (hexandra, *Ehrh.*)

Salix purpurea, *Linn.*
× aurita (dichroa, *Döll.*)
× aurita × phylicifolia (sesquitertia, *F. B. W.*)
× cinerea (sordida, *Kern.*)
× phylicifolia (secerneta, *F. B. W.*)
× repens (Doniana, *Sm.*)
× viminalis (rubra, *Huds.*) *f.* Forbyana (*Sm.*)
viminalis, *Linn.*
× aurita (fruticosa, *Doell.*)
× Caprea (Smithiana, *Willd.* p. pte.)
× cinerea (Smithiana, *Willd.* p. pte.)
× Smithiana ? (stipularis, *Sm.*)
× ———— ? (acuminata, *Sm.*)
× repens
Lapponum, *Linn.*
× Myrsinites (phæophylla, *Anders.*)
× phylicifolia (Lapponum-phylicifolia, *Linton*)
× repens (Lapponum-repens, *Wimm.*)
× reticulata (Sibyllina, *F. B. White*)
Caprea, *Linn.*
× cinerea (Reichardtii, *A. Kern.*)
× Lapponum (Caprea-Lapponum, *Wimm.*)
× Myrsinites (Caprea-Myrsinites, *Linton*)
× nigricans (latifolia, *Forbes*)
× phylicifolia (?)
aurita, *Linn.*
× Caprea (capreola, *J. Kern.*)
× cinerea (lutescens, *A. Kern.*)
× cinerea × nigricans
× herbacea (Margarita, *F. B. White*)
× Lapponum (aurita-Lapponum, *Wimm.*)
× Myrsinites (saxetana, *F. B. White*)
× Myrsinites × nigricans
× nigricans (coriacea, *Forbes*)
× phylicifolia (ludificans, *F. B. White*)
× repens (ambigua, *Ehrh.*)
cinerea, *Linn.*
× Lapponum (cinerea-limosa, *Laestad.*)
× nigricans (strepida, *Forbes*)
× phylicifolia (laurina, *Sm.*)
× repens (cinerea-repens, *Wimm.*)
nigricans, *Sm.*
× phylicifolia (nigricans-phylicifolia, *Wimm.*)
× repens (nigricans-repens, *Heidenr.*)
× reticulata ? (semi-reticulata, *F. B. White*)
phylicifolia, *Linn.*
× repens (Schraderiana, *Willd.*)
Arbuscula, *Linn.*
× herbacea (simulatrix, *F. B. White*)
× Lapponum (spuria, *Willd.*)

Salix Arbuscula, *Linn.*
× Myrsinites (serta, *F. B. W.*)
× nigricans (Kraettliana, *Brügg.*)
× phylicifolia
lanata, *Linn.*
× Lapponum (lanata-Lapponum, *Linton*)
× reticulata (superata, *F. B. White*)
Myrsinites, *Linn.*
× phylicifolia (Normanni, *Anders.*)
× nigricans (Myrsinites-nigricans, *Wimm.*)
herbacea, *Linn.*
× lanata (Sadleri, *Boswell-Syme*)
× Lapponum (sobrina, *F. B. White*)
× Lapponum × Myrsinites? (eugenes, *Linton*)
× Myrsinites (Grahami, *Baker*)
× nigricans? (Moorei, *H. C. Watson*) ?
× repens (cernua, *Linton*)
× reticulata (onychiophylla, *Anders.*)
"London Catalogue."
See "The Salix lists in the 'London Catalogue,'" by E. F. Linton, M.A., "Journ. Bot." Nov. 1896, pp. 461–472.

Populus alba × tremula (P. canescens), Hook., "Student's Flora," p. 369 (supposed hybrid). In list of hybrids by Kerner.

Orchis maculata × latifolia
"Babington's Manual," 9th ed., H. & J. Groves.
× Gymnadenia conopsea
"Journ. Bot." 1899, p. 360.

Gymnadenia conopsea × albida
"Babington's Manual," 9th ed., H. & J. Groves.

Habenaria viridis × Orchis maculata
"Journ. Bot." 1893, p. 56.

Juncus effusus × glaucus (J. diffusus)
acutiflorus × lamprocarpus

Luzula Forsteri × vernalis (Borrer)
"London Catalogue."

Potamogeton natans × lucens (P. fluitans is probably this)
natans × Zizii (P. crassifolius)
"Journ. Bot.," with figure, 1890, p. 321.
polygonifolius × —? (P. Griffithii)
Drucei (possibly a hybrid)
coloratus × Zizii (P. Billupsii)
heterophyllus × pusillus (P. lanceolatus)
natans × polygonifolius (P. sparganifolius)
graminifolius × perfoliatus (under P. nitens)
heterophyllus × perfoliatus (under P. nitens)
Zizii × perfoliatus (under P. nitens)
Zizii × perfoliatus (P. involutus)
heterophyllus × Zizii (P. lucens var. coriaceus)
lucens × perfoliatus (P. decipiens)
× prælongus (P. longifolius Mr. Bennett believes to be this)

Potamogeton crispus × perfoliatus (P. Cooperi)
× obtusifolius (P. Bennettii)
"Babington's Manual," 9th ed., H. & J. Groves.
polygonifolius × rufescens (P. spathulatus)
"Kerner's Natural History of Plants," vol. ii. p. 586.
Carex canescens × lagopina (C. helvola)
paniculata × remota (C. Boenningbauseniana)
aquatilis × salina (C. Grantii)
× elata (C. hibernica)
"Babington's Manual," 9th ed., H. & J. Groves.
flava × fulva (C. xanthocarpa, *Degl.*)
fluitans × plicata ? (G. pedicellata, *Town.*)
"Journal of Botany," 1894, p. 168.
flava × fulva
"London Catalogue."
Alopecurus pratensis × geniculatus (A. hybridus)
"Journal of Botany," 1901, p. 232.
"Babington's Manual," 9th ed., H. & J. Groves.
Polypogon monspeliensis × Agrostis alba (Polypogon littoralis)
"Babington's Manual," 9th ed., H. & J. Groves.
Glyceria fluitans × plicata (G. pedicellata)
Sclerochloa distans × procumbens (S. distans, var. pseudo-procumbens)
"Babington's Manual," 9th ed., H. & J. Groves.
Festuca pratensis × Lolium perenne (Festuca pratensis, var. F. loliacea)
"Babington's Manual," 9th ed., H. & J. Groves.
elatior × Lolium perenne (F. loliacea, *Huds.*)
Above Ernsford.
"Journal of Botany," 1894, p. 149.
Asplenium Trichomanes × Ruta-muraria (A. Clermontæ)
"Babington's Manual," 9th ed., H. & J. Groves.
Ruta-muraria × septentrionale (A. germanicum)
Kerner, "Natural History of Plants," vol. ii. p. 586.

Mr. W. H. Beeby, writing on Natural Hybrids in the "Journal of Botany," 1892, p. 209, gives the following list of hybrids, selected from Dr. Focke's *Pflanzen-Mischlinge*, which have not yet been noted in this country, the parents being British or admitted to a place in British floras.

Ranunculus acris × bulbosus
sardous × sceleratus
Papaver dubium × somniferum
Rhœas × somniferum
dubium × Rhœas
Nasturtium amphibium × sylvestre
palustre × sylvestre
× amphibium
Cardamine amara × sylvatica
Helianthemum Chamæcistus × polifolium
Viola arenaria × canina
× sylvatica
lactea × Riviniana
Dianthus deltoides × Armeria

Lychnis diurna × alba
Stellaria graminea × uliginosa
Hypericum perforatum × dubium
× tetrapterum
dubium × tetrapterum
humifusum × perforatum
Medicago falcata × sativa
Potentilla argentea × Tormentilla
Alchemilla alpina × vulgaris
Pyrus communis × Aria
Malus × torminalis
× Aria
Aria × torminalis
Aucuparia × Aria
Erigeron acris × canadensis
Senecio vulgaris × sylvaticus
sylvaticus × viscosus
Carduus, numerous hybrids
Cirsium, numerous hybrids
Centaurea Cyanus × Scabiosa
Lactuca saligna × virosa
Gentiana campestris × germanica
Galeopsis ochroleuca × angustifolia
Ajuga reptans × pyramidalis
Rumex, numerous hybrids.
Polygonum, numerous hybrids.
Orchidaceæ, numerous hybrids.
Carex vulpina × remota
ovalis × remota
curta × remota
distans × Hornschuchiana
pallescens × punctata
hirta × vesicaria
riparia × vesicaria
filiformis × riparia (C. evoluta, Hartm.)
× paludosa (C. Kochiana, Schueb.)
flacca × paludosa
Scirpus lacustris × Tabernæmontani
Calamagrostis Epigeios × lanceolata
× arenaria
Avena fatua × sativa
Bromus mollis × racemosus
and various other grasses, &c.

II. Continental.

Anemone (Pulsatilla) vulgaris × A. montana (A. nutans)
Valleys of Planail and Plawen in mountains of the Oetzthal
Kerner's "Natural History of Plants," translated by Oliver
vol. ii. p. 592.

Anemone patens × pratensis (A. Hakelii)
Kerner, "Natural History of Plants," list, vol. ii. p. 586.
Ranunculus aconitifolius × glacialis (R. aconitoides)
× pyrenæus (R. lacerus)
alpestris × glacialis (R. gelidus)
Gremli's "Swiss Flora," translated by Paitson.
Nuphar luteum × pumilum (N. intermedium)
Black Forest and Vosges, North Germany, Central and Northern Russia, and Sweden.
Kerner, "Natural History of Plants," vol. ii. p. 589.
Dentaria digitata × pinnata (D. digenea)
× polyphylla (D. Kiliasii)
Gremli, "Swiss Flora," translated by Paitson.
Nasturtium palustre × N. sylvestre (N. stenocarpum)
Roripa, Kerner, list, vol. ii. p. 586.
Draba aizoides × tomentosa (D. setulosa)
× Johannis
Gremli, "Swiss Flora," translated by Paitson.
fladnizensis × carinthiaca (D. Hoppeana)
Kerner, "Natural History of Plants," translated by Oliver, list, vol. ii. p. 586.
Cistus albidus × crispus (C. pulverulentus)
Southern France, Spain, and garden hybrid.
hirsutus × salvifolius (C. obtusifolius)
Near Coimbra, Sierra de Cintra.
populifolius × salvifolius (C. corbariensis)
Southern France, especially in neighbourhood of Narbonne.
ladaniferus × villosus (C. purpureus)
Garden hybrid.
× monspeliensis
Garden hybrid.
laurifolius × monspeliensis (C. glaucus)
Southern France, especially in Provence, Languedoc, Narbonne, Montpellier, &c.
× salvifolius (C. Costei)
Southern France, dept. Aveyron, in neighbourhood of Belmont.
parvifolius × villosus (C. cymosus)
In cultivation only.
hirsutus × ladaniferus
Raised by Bornet, and found also in Portugal.
monspeliensis × parviflorus (C. Skanbergi)
Island of Lampedura.
crispus × monspeliensis (C. varius)
Southern France, Portugal, Spain.
monspeliensis × villosus
Albania, Thessaly.
hirsutus × monspeliensis (C. platysepalus)
Portugal, between Melides and S. Thrago de Cacem.

Cistus monspeliensis × salvifolius (C. florentinus)
Southern France, Corsica, Portugal, Algeria.
× populifolius (C. nigricans)
Southern France, neighbourhood of Narbonne.
albidus × salvifolius (C. albereensis)
Southern France, Portugal.
crispus × salvifolius (C. novus)
Southern France.
× laurifolius (C. Pourretii)
Southern Spain, Gibraltar.
albidus × hirsutus
Portugal.
ladaniferus × salvifolius
Portugal.
Engler's "Pflanzenreich," Cistaceæ, by W. Grosser, iv. 193. Leipzig: Engelmann.

Viola alba × hirta (V. badensis)
arenaria Riviniana (V. Burnati)
Beraudii × hirta
calcarata · tricolor (V. Christii)
hirta × odorata (V. permixta)
mirabilis × sylvatica (V. spuria)
× Riviniana
odorata × scotophylla
Gremli, "Swiss Flora," translated from fifth edition.

*Dianthus barbatus × superbus (D. Courtoisii)
Mem.—Species of this genus usually hybridise freely, and are easy to manipulate.
Gremli, "Swiss Flora," translated by Paitson.

Lychnis diurna × vespertina (L. dubia)
Gremli, "Swiss Flora," translated by Paitson from fifth edition.

Hypericum quadrangulum × tetrapterum
Gremli, "Swiss Flora," translated by Paitson.

Medicago falcata × M. sativa (M. media, "Pers. Syn." vol. ii. p. 356)
Kerner, ii. p. 579.

Rubus tomentosus × vestitus
× Radula
And a number of others.

Fragaria collina × elatior (F. sericea)
Near Binningen, canton Bâle.
collina × vesca (F. Hagenbachiana)

Potentilla alba × Fragariastrum (P. hybrida)
aurea × minima (P. semiternata)
Fragariastrum × micrantha (P. spuria)
frigida × grandiflora (P. valesiaca)
× multifida (P. pennina)
grandiflora × salisburgensis (P. rhætica)
minima × salisburgensis
multifida × salisburgensis (P. geranioides)
Gremli, "Swiss Flora," translated by Paitson from fifth edition.

Rosa gallica × rubiginosa (R. consanguinea)
arvensis × gallica (R. hybrida)
canina × gallica (R. depressa)
gallica × mollis
gallica × tomentosa (R. fimbriata)
micrantha × sepium
sepium × tomentosa
arvensis × canina?
dumetorum × gallica
cinnamomea × pomifera (R. anoplantha)
coriifolia × pomifera (R. semproniana)
ferruginea × pomifera (R. Franzonii)
glauca × pomifera (R. Murithii)
graveolens × pomifera (R. personata)
alpina × coriifolia (R. stenosepala)
alpina × pomifera (R. longicruris)
Alchemilla alpina × vulgaris (A. splendens)
Pyrus (Sorbus) Aria × torminalis (P. confusa)
Gremli, "Swiss Flora."
*Saxifraga aizoides × S. cæsia (S. patens)
× S. mutata (S. Hausmanni and S. Regelii)
Aizoon × S. Cotyledon
Simplon, Switzerland.
Androsacea × S. Seguieri
biflora × S. oppositifolia (S. hybrida)
Cotyledon × S. cuneifolia (S. Jaeggiana)
planifolia × S. stenopetala (S. Mureti)
× S. varians
Gremli, "Swiss Flora," translated by Paitson, p. 183.
aretioides × calyciflora (S. luteo-purpurea)
cæsia × mutata
Aizoon × cuneifolia
Lynch, "Evolution of Plants" in JOURN. R.H.S. vol. xxv. p. 30.
Sempervivum alpinum × arachnoideum
× montanum
arachnoideum × montanum (S. barbatulum)
× tectorum (S. Fontanæ)
montanum × Wulfeni (S. Huteri)
tectorum × Wulfeni (S. Comollii)
*Epilobium adnatum × Lamyi-montanum (E. Hausknechtianum)
× parviflorum
× montanum
hirsutum × parviflorum (E. intermedium)
montanum × obscurum (E. aggregatum)
× parviflorum (E. limosum)
× roseum (E. glanduligerum)
× trigonum (E. Freynii)
obscurum × palustre (E. ligulatum)
origanifolium × trigonum
palustre × parviflorum (E. rivulare)

Epilobium parviflorum × roseum (E. persicinum)
rosmarinifolium × spicatum
adnatum × palustre (E. semiadnatum)
anagallidifolium × origanifolium
Gremli, "Swiss Flora."
alsinifolium × palustre (E. scaturiginium)
Riesengebirge, Bihar Gebirge, Hochkamm.
Kerner, vol. ii. p. 591.
× montanum (E. salicifolium)
Kerner, vol. ii. p. 586.
Adenostyles leucophylla × albifrons (A. hybrida)
× alpina (A. eginensis)
Erigeron acris × Villarsii (E. Favrati)
alpinus × glabratus
× uniflorus
Gremli, "Swiss Flora."
canadensis × acris (E. Huelsenii)
Kerner, vol. ii. p. 585.
Inula hirta × salicina (I. spuria)
salicina × Vaillantii (I. semiamplexicaulis)
Achillea atrata × macrophylla (A. Thomasiana)
× moschata (A. impunctata)
× nana (A. Laggeri)
macrophylla × moschata (A. Lereschei)
× nana (A. valesiaca)
moschata × nana (A. intermedia)
Millefolium × tomentosum
Valley of Saas according to Schneider.
nobilis × setacea
Petasites albus × niveus
niveus × officinalis
Senecio incanus × uniflorus (S. oligocephalus)
abrotanifolius × incanus
Centaurea Jacea × nervosa
× rhætica
Cirsium (Cnicus)
1. Leaves with small spines on upper surface.
acaule × lanceolatum (C. sabaudum)
2. Leaves without spines on upper surface.
a. Leaves more or less decurrent.
bulbosum × palustre (C. semidecurrens)
oleraceum × palustre (C. hybridum)
b. Leaves not decurrent.
acaule × heterophyllum (C. alpestre)
× bulbosum (C. medium)
× oleraceum (C. rigens)
And others, the above being only a selection.
Gremli, "Swiss Flora."
canum × oleraceum (C. tataricum)
Kerner, list, vol. ii. p. 586.

Carduus crispus × nutans (C. Stangii)
defloratus × nutans (C. Brunneri)
× Personata (C. Bambergeri)
nutans × Personata (C. Grenieri)
Crepis alpestris × blattarioides
× grandiflora (C. longifolia)
blattarioides × grandiflora
hyoseridifolia × Jacquini (C. hybrida)
Gremli, "Swiss Flora."
Hieracium
Hybrids in this genus are so numerous that it would be impossible here to deal with them. For Mendel's account of his own experiments see "Principles of Heredity," by Bateson. p. 96.
Leontodon pyrenaicus × Taraxaci
Gremli, "Swiss Flora."
†Rhododendron ferrugineum × R. hirsutum (R. intermedium)
Tyrolese Alps, on the Hohe Burgstall, and Padaster Alp.
"More common than either parent"
Kerner, vol. ii. p. 588.
Primula officinalis × P. elatior (P. media)
† subacaulis × P. officinalis (P. brevistyla)
superacaulis × P. officinalis (P. flagellicaulis)
† elatior × P. vulgaris (P. digenea)
Common in upland meadows of Eastern Alps.
Clusiana × P. minima (P. intermedia)
subminima × P. spectabilis (P. Fachinii)
superminima × P. spectabilis (P. Dumoulini)
subintegrifolia × P. viscosa (P. Muretiana)
× (P. Dinyana)
hirsuta × P. integrifolia (P. Heerii)
Auricula × P. integrifolia (P. Escheri)
integrifolia × P. glutinosa (P. Huguenini)
superglutinosa × P. minima (P. Floerkeana)
Floerkeana × minima var. P. salisburgensis
× glutinosa (P. Huteri)
† subglutinosa × minima (P. salisburgensis)
Common on the Tyrolese Alps.
superminima × hirsuta (P. Forsteri)
subminima × hirsuta (P. Steinii)
minima × œnensis (P. pumila)
× villosa (P. truncata)
tyrolensis × Wulfeniana (P. Venzoi)
hirsuta × viscosa (P. Berninæ)
× œnensis (P. Plantæ)
sub-Auricula × hirsuta (P. Arctotis)
super-Auricula × villosa (P. Goebelii)
× (P. Kerneri)
× (P. alpina)
× viscosa (P. Peyritschii)
× œnensis (P. discolor)

Primula sub-Auricula × œnensis (P. Portæ)
super-Balbisii × Auricula (P. Obristii)
" × " (P. similis)
Auricula × carniolica (P. venusta)
From list of "Cultivated Primroses," by Mr. Stein, of the Breslau Botanic Gardens, printed in Report on the Primula Conference, JOURN. R.H.S. vol. vii. 1886. For some other hybrids see "Synonymic List," by Daniel Dewar, in same report.
super-Auricula × hirsuta (P. pubescens)
Kerner, list, vol. ii. p. 586.
Androsace glacialis × helvetica (A. Heeri)
× obtusifolia (A. Ebneri)
helvetica × pubescens (A. hybrida)
Soldanella alpina × pusilla (S. hybrida)
Anagallis arvensis × cærulea
Near Lostorp, Canton Solothurn.
Gremli, "Swiss Flora."
Gentiana campestris × germanica (G. chloræfolia)
Intermediate forms between *G. campestris* and *G. obtusifolia* are found, according to Nägeli, on the Piz Padella, in the Upper Engadine.
lutea × punctata (G. Charpentierii)
× purpurea (G. Thomasii)
punctata × purpurea (G. spuria)
Gremli, "Swiss Flora."
*Verbascum
I. Leaves shortly or half decurrent
a. Wool of the stamens white
α. Anthers equal
Lychnitis × Thapsus (V. spurium)
pulverulentum × thapsiforme (V. mosellanum)
β. Anthers of the two longer stamens distinctly decurrent
Lychnitis × thapsiforme (V. ramigerum)
b. Wool of the stamens entirely or partly violet
α. Anthers of the two longer stamens obliquely decurrent
pulverulentum × thapsiforme (V. nothum)
β. Anthers equal
nigrum × thapsiforme (V. adulterinum)
× Thapsus (V. collinum)
II. Leaves not decurrent
a. Flowers clustered
α. Wool of the stamens white
Lychnitis × pulverulentum (V. Regelianum)
β. Wool of the stamens violet
Lychnitis × nigrum (V. Schiedeanum)
nigrum × pulverulentum (V. mixtum)
b. Flowers solitary, or 2–4 together, raceme lax, habit of V. Blattarias
Blattarias × Lychnitis (V. blattarioides)
× thapsiforme (V. Bastardi)
V. montanum × nigrum (V. uriense)
Gremli, "Swiss Flora."

V. andriacum × phœniceum (V. rubiginosum)
Kerner, list, vol. ii. p. 586.
Linaria striata × L. vulgaris (L. stricta)
West of Europe, Montpellier.
Kerner, ii. p. 591.
*Digitalis ambigua × lutea (D. media)
Mem.—*D. grandiflora* and *D. purpurea* hybridise freely.
Gremli, "Swiss Flora."
Pedicularis gyroflexa × tuberosa
Monte Generoso
asplenifolia × Jacquini
incarnata × recutita (P. atrorubens)
× rostrata
recutita × tuberosa (P. marithiana)
Jacquini × tuberosa
rostrata × tuberosa
Euphrasia minima × salisburgensis
Mt. Pilatus.
ericetorum × salisburgensis
Chéseret, canton Vaud.
Gremli, "Swiss Flora."
Acanthus mollis × A. spinosissimus (A. spinulosus)
Kerner, list, vol. ii. p. 586.
Mentha nemorosa × rotundifolia (dumetorum)
rotundifolia × sylvestris (gratissima)
Gremli, "Swiss Flora."
Micromeria græca × M. Juliana (M. Kerneri)
Herzegovina.
Kerner, ii. p. 592.
†Salvia nemorosa × pratensis (S. sylvestris)
Low country south of Vienna.
Kerner, ii. p. 589.
†Prunella laciniata × P. vulgaris (P. hybrida)
Weinerwald district, Moravia, and Bohemia.
Kerner, ii. p. 591.
†Marrubium peregrinum × M. vulgare (M. remotum)
Flat country by the Theiss and Lower Danube, everywhere on the plains of S.E. Europe.
Kerner, ii. p. 585.
Stachys alpina × lanata
× sylvatica
Galeopsis angustifolia × dubia (G. Wirtgeni)
Lamium album × purpureum
According to Jäggi, near Rheinan.
Ajuga genevensis × reptans (A. hybrida)
Near Munich.
Gremli, "Swiss Flora."
Rumex aquaticus × crispus (R. Patientia)
Hungary and Bosnia.
Kerner, ii. p. 592.

Rumex crispus × nemorosus
On the Eschenberg, according to Siegfried.
conglomeratus × crispus
Near Walkenweier, according to Siegfried.
alpinus × obtusifolius
Enzeindaz and Pilatus, according to Haussknecht.
Polygonum lapathifolium × Persicaria
Papon, near Villeneuve.
Hydropiper × Persicaria
× mite
Bistorta × viviparum
Near Chur.
Gremli, "Swiss Flora."
†Betula alba × B. nana (B. alpestris)
Common in the Jura, in Scandinavia, and in the north of Russia.
Kerner, ii. p. 586.
A long list of the hybrids in this genus is given in Gremli, "Swiss Flora," English translation from fifth edition by Paitson, p. 354.
Salix grandifolia × S. purpurea (S. austriaca)
Kerner, list, vol. ii. p. 586.
incana × S. daphnoides
Island on Danube, near Dürenstein.
Kerner, ii. p. 577.
Orchis incarnata × palustris
laxiflora × Morio (O. alata)
× palustris
mascula × pallens
militaris × Aceras (O. spuria)
× purpurea (O. hybrida)
× tephrosanthos (O. Beyrichii)
globosa × Gymnadenia conopsea (O. valesiaca)
purpurea × tephrosanthos
tridentata × ustulata (O. Dietrichiana)
Morio × palustris
Gymnadenia odoratissima × Orchis maculata
× albida (G. Strampfii)
Gremli, "Swiss Flora."
† conopsea × Nigritella nigra (N. suaveolens)
Common in the Central Alps.
Kerner, ii. p. 586.
†Nigritella angustifolia × Gymnadenia conopsea (N. fragrans)
× odoratissima (N. suaveolens)
Ophrys aranifera × muscifera (O. apiculata)
fuciflora × muscifera (O. devenensis)
Gremli, "Swiss Flora."
†Epipactis rubiginosa × Cephalanthera alba (Epipactis speciosa)
Erlafthal of Lower Austria.
Kerner, ii. p. 583.
Polygonatum multiflorum × officinale

Gagea Liottardi-minima
Luzula angustifolia × nivea
Gremli, "Swiss Flora."
Scirpus lacustris × S. Pollichii (S. Duvalii)
Kerner, list, vol. ii. p. 586.
Calamagrostis arundinacea × C. epigeios (C. acutiflora)
Kerner, list, vol. ii. p. 586.
Festuca pratensis × Lolium perenne (F. loliacea, *Curt.*)
× Lolium italicum
Lolium italicum × perenne
Gremli, "Swiss Flora."
Ægilops ovata × Triticum sativum
Kerner, ii. p. 583.
†Pinus montana × sylvestris (P. Friesiana)
†Juniperus communis × J. sabinoides (J. Kanitzii)
Kerner, ii. p. 583, Index Kewensis, supp. 1, p. 230.
†Scolopendrium officinale × Asplenium Ceterach (Scolopendrium hybridum, *Milde*)
Istria.

Figured in "European Ferns," by Britten, p. 137. He says that one plant only appears to have been found near Porto Zigale.

Kerner, ii. p. 582.
†Equisetum arvense × E. limosum (E. inundatum)
"A rather common hybrid."
Kerner, ii. p. 582.

PHENOMENA OF HYBRIDISATION IN THE GENUS *MENTHA*.

RÉSUMÉ OF THE FACTS ACQUIRED

By M. Ernest Malinvaud.

I.—The Cardinal Species in the Genus *Mentha*.

We call "cardinal" * the five irreducible Linnean species, which constitute the group of *Eumenthæ* : *M. rotundifolia, sylvestris, viridis, aquatica,* and *arvensis.*

II.—The Signs of Hybridisation in the Genus *Mentha*.

The *Eumenthæ* present numerous phenomena of hybridisation, of which several exhibit remarkable persistence.

M. rotundifolia and *sylvestris* are hybridised regularly,† wherever they are to be found growing together, or in the vicinity of each other, except for very rare exceptions due to local circumstances. The individuals resulting from these crossings frequently appear in such abundance that we might be led to suppose that they are the legitimate dominant species, and some authors, Fries, Godron, &c., through a most unfortunate error, have referred the *M. sylvestris* type to *M. viridis,*‡ as var. *canescens.*

Mentha aquatica and *arvensis* cross with the same facility as the preceding, and produce the innumerable varieties of *M. sativa* L.§

M. viridis, rare in France in the wild state, but often cultivated or escaped from gardens, takes part in various hybridisations when it meets either with *M. rotundifolia* (*M. Lamyi* Mlvd.), *Nouletiana* Timb., *rubra* Sm., *gentilis* L. &c.,‖ or with *M. arvensis* or with a variety of garden origin, and in this case the results are more or less complex.

The *M. aquatica* crosses in our country rarely with *M. rotundifolia* (*M. Maximiliana* F. Schultz, *M. Schultzii* Bout.), and still more rarely with *M. sylvestris* (*M. pubescens* Willd., *nepetoides* Lej., *Ayassei* Mlvd.).¶

The products from the group *arvensi-rotundifolia,* without being frequent, are less rare than the preceding (*M. Muelleriana* and *Wohl-*

* See *Bull. Soc. bot. Fr.*, xxiv. (1877) ; *Rev. bibl.*, p. 43.

† Malinvaud, *Menthæ exsiccatæ, præsertim gallicæ,* Nos. 8, 10, 111, 113, 114, 115, &c.

‡ Grenier & Gordon, *Flora of France*, ii. p. 650.

§ Malinvaud, *Menthæ exs.*, Nos. 42–50, 53–55, 57, 59, 60, &c.

‖ The same, Nos. 13 (*M. sapida*), 15 (*Nouletiana*), 21 (*piperita*), 38 (*citrata*), 61 (*rubra*), 63 and 65 (*gentilis*), 178 (*cantalica*), &c.

¶ The same, Nos. 22 (*pubescens*), 23 (*nepetoides*), 24 and 25 (*pubescens*), 26 (*rotundifolio-hirsuta*), 27 and 28 (*Maximiliana*), 29 and 30 (*Schultzii*).

werthiana F. Schultz, *Malinvaldi* G. Camus), but generally have little fixity.*

Hitherto in France no authentic case has been known of spontaneous crossing between *M. arvensis* and *sylvestris*, but examples of it have been observed in other countries.

A powerful vegetative system (rhizomes, stolons and suckers, pseudo-rhizæ), supplementing the sexual organs which are habitually imperfect, favours the propagation of hybrid *Menthæ*, most often at the expense of their parents, which they eliminate, in some cases even completely, from the ground, which they occupy. A succession of individuals, continually derived from the same rhizomes for several years, may bring about the illusion that the form has the fixity of a species.

By reason of the almost infinite variety of forms resulting from the polymorphism of *Mentha* and from the mutability of their hybrids, the nomenclature of the genus has grown to hundreds of names, of which many are only applicable to vanished types.

I will briefly enumerate the principal signs of hybridisation :

Inflorescence.—The characteristics drawn from the manner of arrangement of the inflorescence, on which is founded the Linnean division into *spicata*, *capitata*, *verticillata*,† are invariable in the true *Menthæ* ; all mixed inflorescences, that is to say those which exhibit on the same individual plant a combination or mixture of the above forms, are a certain mark of hybridisation, and such mixture including three forms of inflorescence is frequently seen in five species. In the groups *aquatico-rotundifolia* and *aquatico-sylvestris*, the inflorescence is almost always spicate at the top of the principal stalk, and often a globular head on the secondary axes ; these also terminate in a capitate form in many varieties of the *sativa* group, while the primary axis is surmounted by a cluster of small leaves, &c.

Corolla.—The interior of this organ is glabrous in the legitimate forms of the three *spicatæ*, and more or less pilose in *aquatica* and *arvensis*.‡ All departure from this rule is an evidence of hybridisation. The glabrous or pilose state of the exterior has no significance in this respect.

* The same. Nos. 66 (*Wohlwerthiana*), 68 (*Muelleriana*), 69 (*arvensi-rotundifolia* Wirtg.), 70 (*Malinvaldi* G. Camus, = *arvensis* var. *micrantha* F. Sch.), 71 and 72 (*mollis* and *Scordiastrum* F. Schultz, &c.).

† The terms *spicate* and *verticillate* are expressive here of the appearance. In fact we have to do with false spikes (*spicastrum*) and with false whorls (*verticillastrum*), and it would be more correct to write *spicastræ* and *verticillastræ*.

‡ The Prussian botanist Wirtgen was the first to remark about half a century ago that the tube of the corolla was always glabrous in the interior (*Blumen-Kronenröhre innen kahl*) in the legitimate *Mentha spicata* and more or less pilose ("*Bl. innen behaart*") in the species belonging to the two other groups. I have confirmed the accuracy of this observation, which is often a great help towards the discrimination of hybrid forms. Nevertheless Wirtgen exaggerates the value of it in substituting the new characteristic which he had found in the manner of inflorescence in order to make it the basis of an artificial classification which manifestly violated the natural affinities. In his *Flora der preussischen Rhein-provinz* (Bonn, 1857) he formed two sub-genera : (A) *Mentha* properly so called with *M. piperita*, *viridis*, *gentilis*, *rotundifolia*, *sylvestris*, *pubescens*, and (B) *Trichomentha* with *M. aquatica*, *rubra*, and *arvensis*. These arbitrary and inconsistent classifications manifestly transgress the principle of the subordination of characteristics, and, in consequence of this error of judgment, Wirtgen reaps but a small benefit from his happy discovery.

Calyx.—This is always more or less hairy at the base in the true verticillate and capitate forms; a complete absence of hairs at this point, at least between the nerves (a very marked characteristic in *M. viridis*, and habitually coinciding with the glabrousness or glaucousness of the interior of the corolla), indicates a crossing between *M. arvensis* and *viridis* (*M. gentilis* and its varieties), or between *aquatica* and *viridis* (*M. citrata* Ehrh., *M. odorata* Sole, *M. adspersa* Moench).

Many forms of the group *sativa* (*aquatico-arvensis*) are only distinguished by the character of the calyx (tubular-campanulate with lanceolate acuminate teeth, as in *M. aquatica*) from *M. arvensis*, of which the calyx is campanulate-urceolate with short triangular teeth. This last characteristic is not, moreover, pronounced except in fertile individuals.

Leaves.—Only the characteristics of those on the principal stalk must be considered. Normally they are sub-sessile in the *Spicatæ*, and distinctly petiolate in *M. aquatica* and *M. arvensis*; disturbance of these characteristics furnishes a presumption of hybridisation. When we see on a *Mentha* of the *Verticillatæ* section, apart from all pathological or teratological conditions, shortening of the leaves in the middle of the stalk in comparison with those above as well as those below, resulting in a club-shaped appearance (*forma strangulans*), we may infer the intervention of a *Mentha* of the section *Spicatæ*, ordinarily *M. rotundifolia*.

We need not specify the signs of hybridisation of less value; those we have just defined will nearly always reveal the double origin of the varieties represented. Doubtless we must expect to meet with complex and embarrassing cases amongst the phenomena of hybridisation; these are, however, relatively few.

The characters of hybrids result generally from various combinations and mixtures of those of the parents, and it is rare for them to present a simple juxtaposition as in the type form of *Mentha Ayassei* Mlvd.,* which presents the globular inflorescence derived from *M. aquatica* with the unmodified leaves of *M. sylvestris*.

To sum up, from my personal observations, which are spread over more than forty years, the hybridisation of *M. aquatica* with *M. arvensis* (=*M. sativa*) is the rule everywhere, where these two species are spontaneously crossed; it is the same with the production of *rotundifolio-sylvestris*; on the contrary, the hybridisation of the other *Mentha* hybrids is exceptional—above all, the *arvensi-sylvestris*; the *aquatico-rotundifolia* and the *arvensi-rotundifolia* are less common, sporadic, and unstable. The *aquatico-sylvestris* and the *arvensi-viridis*, still more rare in France than the preceding, appear to be very widely distributed in certain countries of Central and Southern Europe. The *aquatico-viridis*, *rotundifolio-viridis*, and *sylvestri-viridis* are usually of horticultural origin.

Finally, I have hitherto sought in vain for an authentic case of the crossing of *M. Pulegium* with an *Eumentha*, and the lack of affinity, which this negative fact makes evident, authorises us in establishing the genus *Pulegium*,† and, *à fortiori*, in admitting the genus *Preslia* (*Mentha cervina*) and *Menthella* (*Mentha Requieni*).

* See *Menthæ exsicc. præs. gallicæ*, Nos. 39 and 40.

† We have seen, in 1879, in the herbarium of the Paris Museum, a *Mentha* furnished by the Grenier collection and named by Reuter *M. arvensi-Pulegium*, which showed all the characteristics of a *M. sativa*.

III.—Proofs of Hybridisation.

In a general way, the hybrid mints are to be met with in proximity to their presumed parents, offering characteristics intermediate between those of the parents, and are as a rule sterile. At this point I will mention that wherever *M. aquatica* and *M. arvensis* grow in the vicinity of each other they have, as it were, an irresistible tendency to become reciprocally fertilised, thus giving rise to innumerable varieties of *M. sativa* (together *aquatico-arvensis* and *arvensi-aquatica*). On the other hand, these are invariably lacking in countries where *M. arvensis* exists alone, unaccompanied by *M. aquatica*, and *vice versa*. This is therefore a powerful argument in favour of the double origin of *M. sativa*, and nullifies the opinion of those who look upon this polymorphic hybrid either as a *verticillata* variety of *M. aquatica*, or as a *sativa* variety of *M. arvensis*.*

The same observation applies to the hybrids resulting from the crossing of *M. rotundifolia* and *M. sylvestris*, products which I have designated under the comprehensive name of *sylvestres spuriæ*, by uniting those betraying a parentage with *M. viridis*.† These hybrids, often looked upon as legitimate species, are lacking in all the localities where only one of their parents exists—for example, in the environs of Paris, where *M. sylvestris* and *M. viridis* do not exist in a wild state.

It is, besides, easy to change to a certainty the assumption that we deduce from the examination of the facts in Nature. This can be done by experimental methods, either analytically, by referring the hybrids to their parent species, or synthetically, by reproducing them by a cross-fertilisation. I will not here insist on this question of technical arrangement.

IV.—Biological Remarks.

The floral dimorphism of mints has been known for a long time. In 1848 Bentham wrote in De Candolle's "Prodromus" (pars xii. p. 165): "Stamina exserta vel inclusa, flores majores vel minores, sæpius sexus diversitatem nec species diversas indicare jam omnes fere consentiunt." In all the species we observe a form *submas staminibus exsertis* with the stamens projecting beyond the corolla, and a *subfemina* form where the style only is exserted and projects beyond the stamens, which are enclosed in the tube of the corolla, and are often abortive; the flower is then smaller in all its parts. These peculiarities, biologically interesting, are of no value for classification. They are in uniformity with the law of balance, which regulates the relative development of the flower and the vegetative organs. The exuberance of these latter does damage to the former, and is a very frequent cause of sterility, independent of the hybridisations, in the genus *Mentha*.

V.—Duration of the Hybrid Mints.

Nearly every year, from 1871 to 1885, during the second fortnight of the month of August, I repaired to Provins (Seine-et-Marne), where I was

* See notably Bentham, in DC. *Prod.* xii.

† See Malinvaud, in *Bull. Soc. bot. Fr.* xxx. (1883), pp. 477, 478.

cordially welcomed by an old friend, Edward Bouteiller, whose name is frequently quoted in the classical "Flora" of Cosson and Germain. Together we visited numerous localities rich in varieties of the genus *Mentha*, the study of which was his favourite hobby. He possessed in his herbarium a considerable collection of the most instructive of the mints of those parts, including a series of specimens collected at various times, the oldest about 1830, in the same localities. A comparative examination of these witnesses of the past displayed an interesting fact. Dividing the time elapsed into periods of from ten to fifteen years, each one of these seemed to be marked by the appearance of certain hybrid forms not to be found in any of the others. Wishing to see whether this observation was also applicable to the more recent period, and accompanied by two colleagues, MM. G. Camus and Mellerio, in 1902, I decided once more to visit Provins, where I had not been since the death of my much-regretted friend, in order to revisit, after a lapse of eighteen years, some of the localities which I had formerly explored so assiduously, and to make a thorough investigation of their present state. Imagine my disappointment! The principal object of this present visit was the vicinity of the peat bog of Poigny, thus called because of the proximity of the village of that name, near which is found a great abundance of *M. aquatica*, *arvensis*, and *rotundifolia*, as well as of their hybrids. The *M. Schultzii* Bout. (*aquatico-rotundifolia*) noticed for the first time in 1874 * on the edge of the peat-bog, and seen there again, though in much less profusion, in 1882, had entirely disappeared. In the same way we unsuccessfully sought for the forms of *M. aquatico-arvensis* and *arvensi-rotundifolia*, formerly noticed in this prolific place,† with *M. rotundifolia. aquatica*, and *arvensis*. I only saw one variety of *M. sativa* differing from all those which had previously flourished there. A visit to other localities brought about similar results. Everywhere the legitimate species alone had persisted in the midst of perpetual changes in their train of hybrids.

Conclusion.

According to facts observed at various times in the same localities in Seine-et-Marne, and covering a period of seventy years, the duration of the mint hybrids, varying according to circumstances, is always limited, and the apparent stability of *M. sativa* in the neighbourhood of *M. arvensis* and *aquatica* is an illusion created by the continual production of fresh hybrid plants which succeed each other indefinitely.

According to an interpretation of the facts which up to now is only based on theory, the intermediate forms uniting two species, instead of being hybrids, correspond to the phases of incubation of new species in the course of formation, issuing from the old, but deviating from their successive differentiations, of which the last form, after the obliteration of the intermediates, at length reaches a definitely fixed type. This hypothesis receives no support from the observations I have made in the genus *Mentha*.

* See *Bull. Soc. bot. Fr.* xxii. (1875), p. 249.
† See Malinvaud, *Menthæ exs. præs. gall.* Nos. 29, 30, 44, 54.

INFERTILE HYBRIDS.

By JOHN H. WILSON, D.Sc., F.R.S.E., The University, St. Andrews.

A WIDESPREAD popular belief exists that infertility, or even complete sterility, is a very usual characteristic of hybrids. This belief may in large measure be traceable by association to the well-known instance of the mule amongst animals, and to some the term "hybrid" is synonymous with "mule."

It is well known that certain plants are much more easily intercrossed than others. It is an equally familiar fact that some hybrids are perfectly fertile, some more or less so, and some completely sterile. It is probable that sufficient material exists to permit of the elucidation of the problems involved in groups where many species and varieties have been crossed and observations carefully recorded. The questions, Why are certain plants so easy or so difficult to cross? and Why are the hybrids so fertile or so infertile? are often asked, and only incomplete answers have been given to them. Many problems which seem hopelessly obscure may yet be solved by Mendelian methods. In the present paper, however, all that is attempted is to describe a number of new hybrids with reference to peculiarities of their structure, and their behaviour under experiment, their infertility being the chief reason why they were chosen for study.

DIGITALIS HYBRIDS.

Digitalis lutea ♀ × *D. purpurea* ♂.

Records show that *Digitalis purpurea* and *D. lutea* have been repeatedly hybridised, both spontaneously and experimentally. A plant with this parentage was made the subject of careful examination and graphic description by Henslow seventy years ago.* It appeared spontaneously in a garden where the above species grew together. Henslow states that "the plant in question was undoubtedly a seedling from a specimen of *D. lutea*," and adds, "My plant exactly agrees in most particulars with a hybrid procured by Kölreuter in 1768 from seeds of *lutea* fertilised by the pollen of *purpurea*."

Focke mentions † that Kölreuter did not succeed in fertilising *D. purpurea* with pollen of *D. lutea*. Gaertner succeeded in producing the union, but failed in the great majority of attempts. Godron and Focke himself, however, found no difficulty in carrying out the cross. I did not succeed with it, but my experiments were few.

It may be taken as certain that it is much easier to effect the cross when *D. purpurea* is used as the pollen parent. My hybrids with this parentage bore a close resemblance to those described by other observers The leaves were deep green, and smooth on the upper surface. While

* *Camb. Phil. Trans.*, vol. iv. p. 257. † *Die Pflanzen-Mischlinge*, p. 315.

partaking decidedly of the form of *D. lutea*, the much greater size and the narrowing towards the base showed the influence of the other parent The flowers were rather more nearly intermediate than the leaves. They were pale rose-purple, over creamy-yellow, and, although a little smaller, conformed closely to those described by Henslow. They were quite destitute of good pollen. The fruit swelled considerably, but contained no good seed. The plants were perennial.

Digitalis lutea × *D. purpurea alba.*

Much interest attaches to hybrids between *D. lutea* and *D. purpurea alba*, the white foxglove. In the first series of hybrids secured,

Fig. 32.—Fasciated inflorescence of Digitalis lutea × D. purpurea alba.

D. purpurea alba was the pollen-parent. Compared with the series where the purple foxglove was used, it was very suggestive to note that a much more successful set of fruit was secured. The seed was of better quality, and the seedlings were stronger from the outset. The foliage was a few shades lighter than in the purple foxglove hybrids, and the inflorescences were very considerably taller.

The leaves were of the same shape as in the latter. The tallest plants reached the height of eight feet. In many, if not most of the plants, the inflorescences were more or less fasciated at the top, the amount of fasciation varying from a slight flattening of the apex to the production of a dense brush of flower-bearing shoots of very considerable length (fig. 32). When large, the weight of the fasciated mass bent the inflorescence over greatly. Several strong secondary flowering branches sprang from the lower part of the primary inflorescence.

The flowers were almost white or extremely pale shades of yellow, never the pure white of the white foxglove. The corolla was an inch long or little more, and much narrower than that of the purple foxglove series. Few or many purple spots occurred in the floor of the tube. The anthers, rudimentary at best, were often entirely absent. Usually one or more of the filaments had disappeared also. Nectar was copiously secreted, and bees, failing to reach it by the mouth of the corolla, very often pierced the base of the tube. The capsules swelled considerably, but no good seed was ever found.

The plants of the series so closely resembled one another as to be virtually all indistinguishable. Like the other hybrids, they were perennial. The vegetative and flowering crowns increased in number, but the inflorescences became less vigorous. In a few years the plants died out.

Contrasting the above two series, it is difficult to explain the obviously greater vigour of the hybrids derived from the cross with the white foxglove. It seems natural to assume that the white foxglove is a variety of the purple species, and that it is not at all likely to be constitutionally stronger. The pollen of the white foxglove is of the highest quality.

The very marked tendency to fasciation in the series is a further puzzle. Slight fasciation has been noticed on one occasion in *D. lutea*, but it does not seem to be at all a common occurrence. Peloria is a very familiar peculiarity in the foxglove. It remains to be shown whether peloria and fasciation are associated monstrosities. No peloria, however, has been seen in the hybrids.

Digitalis lutea and D. purpurea alba reciprocally crossed.

Some years after the above observations were carried out, an effort was made to secure reciprocal crosses between the same parents. This was not found to be difficult to accomplish. The pollen of *D. lutea* is indistinguishable from that of *D. purpurea alba*. There is, of course, a very considerable difference in the length of the style in the two parent plants.

The seed of the reciprocal crosses was sown and the plants cultivated under identical conditions. No difference was discernible until the flowers were produced, when it was seen that the two series differed very considerably in floral features. Those having the white foxglove as pollen-parent (fig. 33 *b*) bore a very great resemblance to the former series described above; in fact they were a repetition of that series, in all essential particulars. They had the same narrow, almost white corolla, and the same tendency to abortion of the stamens. In many cases the stamens had quite disappeared. In some a single stamen and in others two stamens were present. They differed from the previous set, in that they were not so very tall, and the fasciation was absent. Some of the flowers had a few carmine spots in the throat. Humble-bees were fond of the flowers. They pushed their heads well into the tubes to reach the honey. The flowers were not found to be pierced as in the former series.

Fig. 33.—Inflorescences of (*a*) Digitalis purpurea alba × D. lutea, (*b*) D. lutea × D. purpurea alba.

The plants of the series having *D. lutea* as the pollen-parent were equally vigorous with those of the reciprocal series. They were easily distinguished by the flowers (fig. 33 *a*) being considerably larger, and of a different form and colour. The mouth of the flower was much wider and the tube more capacious. The colour was almost always a very light rose-purple; in many cases, however, the tinge of red disappeared, leaving the colour almost the same as in the other series. Carmine spots occurred in the corolla. They varied greatly in number and position. A few occurred on the lip, but the great majority appeared in the tube near the mouth. The stamens were with few exceptions all present, and to outward appearance normal.

Microscopic inspection, however, showed that the pollen grains were shrivelled and useless. An occasional grain was found to be plump, but not quite normal. Nectar was secreted in considerable quantity. Bees visited the flowers busily, and they (including the humble-bees) had no difficulty in reaching the nectar by entering the mouth. The corolla was not pierced. If the pollen had been good, one would have expected seed to be set; but, although the ovaries swelled and gave promise of seed, none was got. Fasciation did not occur.

From the above descriptions it is obvious that in crossing with the plants in question the reciprocals are not alike. In form the flowers in each case take more after the seed-parent. It is a matter of very considerable interest to find the purple reappearing in those having *D. lutea* as pollen-parent, pointing to the fact of that colour being latent in the white foxglove. It is of further interest to notice that the flowers of this series are evidently the more nearly normal, although the pollen was taken from the smaller-flowered species. It does not appear that there is any reason to suppose that the pollen of *D. lutea* is more potent than that of *D. purpurea alba* because the former is a perennial plant and the latter a biennial one.

New series of the same reciprocal hybrids are at present in flower. Again the differences between the reciprocals are noticeable, the plants having *D. lutea* as pollen-parent bearing larger and more foxglove-like flowers. In this lot, however, there is more variation amongst the individual plants in respect of colour. Some are decidedly reminiscent of the purple foxglove, others are equally so of the yellow parent. As before, in all the plants the four stamens are all present and apparently well-developed.

In the reciprocal set there is also a much less rigid adherence to a type. The flowers are always narrower than the above, but many are wider than was ever found in the former corresponding series; in fact some are a very near approach to the smaller-flowered examples of those of the other series (*D. purpurea alba* × *D. lutea*). Not only so, a good many of them are well suffused with light rose-purple. Further, the stamens are better developed, although abortion is common enough.

The plants now in flower, while still demonstrating the fact that reciprocal crossing does not give similar results, show that, under certain conditions not discovered, the distinctions between the reciprocal crosses may sometimes be less marked. The white foxgloves used in the experiments were alike in being destitute of coloured spots. It should be noted that a few faint brownish-red markings often appear at the mouth of the tube of *D. lutea*, at the angles of the corolla lobes. Corresponding coloration has not been seen in the hybrids.

In summing up the observations on the three series of crosses having *D. purpurea alba* as pollen-parent, we observe as outstanding features that the first series was characterised by great vigour, very close approximation to a type, and an almost universal development of fasciation of a very pronounced kind. The second series was marked by also keeping close to type, but no fasciation appeared. The third series is decidedly more variable than either of the above in the form and colour of the flower, and there is no development of fasciation.

Whether similar distinctions would appear if reciprocal crosses were made between the purple foxglove and *D. lutea* I cannot say. Seedlings of *D. lutea* × *D. purpurea*, from a recent cross, have been grown under the same conditions as the latest lot of white foxglove crosses, and they are remarkably weak compared with these.

Passion-flower Hybrids.

Passiflora 'Constance Elliot' × *P. alba.*

In the report of the first Hybrid Conference * reference is made to a hybrid passion-flower having *P.* 'Constance Elliot' as seed-parent and *P. alba* as pollen-parent. It is noted that the single hybrid fruit contained 189 good seeds. Of 170 seeds sown, 144 germinated, and about 50 of the seedlings were kept long enough to show that they were all singularly alike in vegetative characters. Two of the plants were planted out in a spacious greenhouse. Both plants displayed a very vigorous habit, and one of them made quite phenomenal growth. The stem, 11 feet long and kept unbranched, measured $5\frac{1}{2}$ inches in circumference. It is hard to say what space this plant would have covered if it had been allowed to grow unpruned. It was necessary to limit its growth to a dense curtain of 256 square feet. A notable feature was its tendency to produce suckers from below, and these were observed to spring at a surprising distance from the base of the stem, in one case 15 feet, and in another 18 feet. It does not appear that either parent could approach this hybrid in vigour of growth.

The leaves of the established plants were larger than younger ones described at the Hybrid Conference, and the lobes relatively wider. The

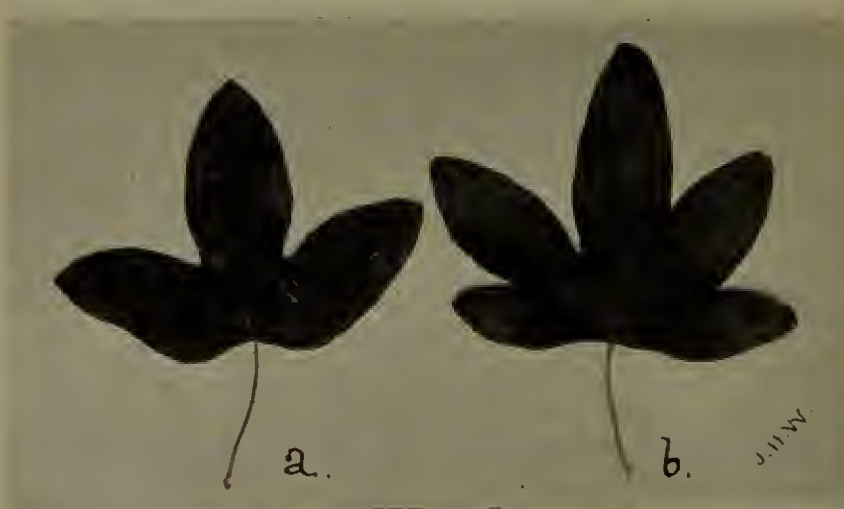

Fig. 34.—Leaves of (*a*) Passiflora alba × P. 'Constance Elliot,' (*b*) P. 'Constance Elliot' × P. alba.

great majority were 5-lobed (fig. 34 *b*). It was usual, however, to find a considerable number 3-lobed and also some lop-sided 4-lobed ones. Transition from the one form to the other was easy, there being five

* *Journal R.H.S.* vol. xxiv. p. 166.

radiating main veins in all the forms. The leaves of the seed-parent were 5-lobed, sometimes 7-lobed; those of the pollen-parent were 3-lobed. *P.* 'Constance Elliot' is a variety of *P. cærulea.* The starved specimens of the latter have been observed to bear small 3-lobed leaves.

The hybrid was extremely floriferous. The flower-stalks were $2\frac{1}{2}$ to $3\frac{1}{2}$ inches long, and curved upwards so as to place the flowers at an angle of about 45° with the horizon. The perianth (fig. 35) was $3\frac{1}{4}$ inches in diameter. The sepals and petals were white, the outer coronal rays white in their basal half and pale purplish-blue in the distal half, the central rays white, the anthers well developed, the styles purplish at the base and greenish-grey with deep red spots at the dilated extremities, the stigmas light greenish-drab. The styles of *P.* 'Constance Elliot' are

Fig. 35.—Flower of Passiflora 'Constance Elliot' × P. alba. Natural Size.

deep purple. Pollen was present, but in very small quantity, forming a very thin orange layer on green ground. The grains lay amongst a sticky secretion. Under the microscope they were seen to be deep yellow, and they varied very much in size and shape. A few grains, although probably deeper yellow than in normal pollen, were so plump and the sculpturing so clear as to justify belief in the possibility of their being functionally perfect.

Although the flowers were scentless, they were great favourites with both hive- and humble-bees. In the early condition the styles were depressed, and bees, especially humble-bees, passing round the flowers must often have effected pollination with pollen of the same or of other flowers. In many cases self-pollination would occur when the stigma, as it ascended to assume an elevated position, was rubbed at the edge against the anther nearest to it. By one means or other great numbers of fruit were set. As many as 150 were counted on the plant during the autumn of 1901. The fruits were well formed, oval, $1\frac{3}{4}$ inch long, and orange or deep yellow in colour. In every case they were perfectly destitute of contents.

Only one fruit on this original seedling plant was found to contain seeds (fig. 36). It appeared on the plant in 1900. No artificial aid was given to procure it. Everything points to its having been the result of fertilisation by the pollen of the same plant. The fruit contained 17 good

FIG. 36.—FRUIT OF PASSIFLORA 'CONSTANCE ELLIOT' × P. ALBA. NATURAL SIZE.

seeds. By oversight, they were not sown until the following summer, nineteen months after being collected. Seven germinated, and five grew well.

The other plant of the original series was also allowed scope to develop. While very vigorous, its spread was not half that of the plant described above, but its foliage and flowers were the same. This plant was not experimented with.

Progeny of Passiflora 'Constance Elliot' × *P. alba.*

The five plants secured from the self-set fruit of *P.* 'Constance Elliot' × *P. alba* have been cultivated in pots for a considerable time. They are distinguished by the numerals I.–V. The leaves of I. are quite similar in form to those of the parent, being mostly 5-lobed. Compared with the parent, the flowers are slightly smaller; the perianth is virtually identical, the petals being broad, white, and of fair substance; the bracts are smaller; the outer coronal rays are fewer, stouter, less regular, and milky-white in colour, forming in large part a single series composed in some places of the upper and in others of the under set; the inner rays and the rays leaning towards and encircling the gonophore are similar, in section the structure of the flower is almost identical. The pollen, even poorer than in the parent, is deep yellow and greatly misshapen.

In III. the leaves are of quite distinctive shape. They differ from those of the rest of the series and from the parent and grandparent forms in having a convex instead of a cordate base. They are almost always 3-lobed, 5-lobed leaves being quite exceptional; 4-lobed ones are of fairly common occurrence.

The flowers (fig. 37) resemble those of I. closely. The petals are narrower and more delicate. The sepals are also narrower. The outer

Fig. 37.—Flower of Seedling III. of Passiflora 'Constance Elliot' × P. alba. Natural Size.

coronal rays are more slender, and are almost as regularly disposed in two whorls as in the parent, and they are pure white. The inner rays are similar to those of the parent. The rays encircling the gonophore are out of sight, being only one-third the length of the corresponding series in the parent. In section the structure of the flower is seen to be the same, the only difference being the shortening of the innermost series of rays above referred to. The pollen is poor as in I.

This variety has been propagated by cuttings. Last year three fruits were borne on one plant, and two other plants bore respectively one fruit each. All the fruits were full of seeds. It cannot be doubted that these fruits were produced by the action of the pollen of the derivatives of *P. alba* × *P.* 'Constance Elliot,' resembling *P. alba*, grown alongside (see p. 193).

In plant V. the leaves are 3-lobed, but deeply cordate at the base; 5-lobed leaves are of very exceptional occurrence. The lobes are broad and blunt, not narrow and pointed as in III.

The flowers are white, and they cannot be distinguished in any way from those of I.

In plant II. the majority of the leaves are 5-lobed. Perfect 3-lobed leaves occur in considerable numbers.

The flowers (fig. 38) are as large as those of the others of the series above described, but they are "blue." The colour, as in the parent, is

FIG. 38.—FLOWER OF SEEDLING II. OF PASSIFLORA 'CONSTANCE ELLIOT' × P. ALBA. NATURAL SIZE.

confined to the coronal rays. Compared with corresponding parts of the parent, the petals and sepals are the same colour, but are somewhat smaller, and not of so good substance; the outer coronal rays are similar in colour and considerably longer, the longest being only a little shorter than the petals and sepals; the inner rays are very similar, but are a little longer and slightly deeper in colour; the stamens are similar, the pollen being equally inferior; the styles and stigmas are the same size and identical in colour. The section of the flower displays quite similar construction. Nectar is secreted copiously.

In plant IV. the leaves are usually 3-lobed. Perfect 5-lobed leaves occur. The flowers bear a very close resemblance to those of the parent. The perianth is a little smaller and is poorer in substance. The outer coronal rays are almost identical with those of the parent in size and colour. The inner rays are a little longer and correspond exactly with those of plant II. The rays leaning towards the gonophore are, as in plant III., abnormally short. The stamens are similar to, and the styles identical with, those of the parent. The stigmas, however, are dull purple. The section of the flower corresponds with that of the parent. Much nectar is secreted.

This plant has borne fully fertile fruit, clearly the outcome of fertilisation by pollen of one of the derivatives of *P. alba* × *P.* 'Constance Elliot' growing near by.

It is interesting to find that plants grown from cuttings of the original plant of *P.* 'Constance Elliot' × *P. alba*, under the same conditions as its seedlings just described, have borne fruit in very considerable numbers, the great majority containing seed, and many being quite full of seed. In one case a fruit was found to be quite empty save for the presence of one perfectly developed seed. There is every reason to believe that the setting of the fertile fruit was due to pollination by bees with pollen from the derivatives of *P. alba* × *P.* 'Constance Elliot' referred to below.

Passiflora alba × *P.* 'Constance Elliot.'

This reciprocal cross was effected in 1899. A fruit, in this case borne by *P. alba*, contained 200 seeds, but all were poorly developed except one. The single seed was sown and it soon germinated. A former cross of the same kind yielded a similar result, but the fate of the solitary seed then obtained has not been traced. The single seedling was strong from the first. It was planted out and grown under identical conditions with the plants of the reverse cross described above. It showed vigour quite equal to the less luxuriant of those two. It had to be confined to a curtain-like mass of 138 square feet. The stem reached the circumference of 3¾ inches. The leaves were a slightly paler green. The most distinctive feature, however, was found in the great majority of the leaves being 3-lobed (fig. 34 *a*) instead of 5-lobed.

The flowers were virtually identical with those of the reciprocal hybrid, the only evident distinction being in the colour of the styles, which were pale green throughout, with a few faint red spots. The styles of *P. alba* are pale green. The pollen was as inferior as that of the reciprocal hybrid. Bees were attracted in numbers to the flowers, but no fruit was found on this plant.

Plants in pot, grown from cuttings taken from the above original seedling, have borne many fruits. The first instance was noticed in 1902, when a plant bore two fruits. The first to ripen was torn off by some thoughtless person. In the remains, eleven fine seeds were found. The other fruit, gathered ripe, was 1¾ inch long and 1½ inch in mid-section, and rich orange in colour. Its form (fig. 39) was more rounded than that of the fertile fruit borne by the reciprocal hybrid described above. It contained twenty-three fine seeds. It seems quite certain that this fruit was the result of fertilisation by pollen of *P. alba* growing alongside. It is well to mention that the plant which bore

FIG. 39.—FRUIT OF PASSIFLORA ALBA × P. 'CONSTANCE ELLIOT.' NATURAL SIZE.

the fruit was pot-bound, a condition often conducive to success in the setting of fruit.

Derivatives of Passiflora alba × P. 'Constance Elliot.'

The seeds from the torn fruit were sown, but if seedlings came up all record of them has been lost. From the twenty-three seeds taken from the second fruit, three seedlings only appear to have been kept note of. It is probable that many more germinated. All three plants, marked respectively A, B, and C, appear to be identical with *P. alba*. The leaves are tri-lobed and light green ; the flowers (fig. 40) small, pure white, and in structural details correspond with that species. The pollen is very copious and evidently perfect. Fruit is borne freely by the plants and is all normal.

One fruit of B contained 74 seeds, and of these 50 were sown, the series being marked B I. Another fruit of B contained 164 seeds, of which 100 were sown, and marked B II. A fruit of C contained 73 seeds, of which 45 were sown.

A crowd of seedlings of all three series came up and were grown long enough to show that they were all alike in habit and foliage. Five of B I., four of B II., and four of C are now in the flowering stage. Those that have flowered have proved to be indistinguishable from *P. alba* in every particular.

To summarise briefly, we find that the pure white species, *P. alba*, crossed with pollen of *P.* 'Constance Elliot,' a pure white variety of

FIG. 40.—FLOWERING SHOOT OF SEEDLING C OF PASSIFLORA ALBA × P. 'CONSTANCE ELLIOT.' NATURAL SIZE.

P. cærulea, gave a hybrid in which the blue of the rays of *P. cærulea* reappeared and the size of the flower of the pollen-parent was retained. The hybrid crossed with its seed-parent resulted in complete restoration of that parent. Seedlings of the parental plant so restored have so far bred perfectly true.

P. 'St. Rule' *(P. alba × P. Buonapartea) pollinated by P. alata.*

It may be of interest to mention another example of infertility in passion-flowers. The variety 'St. Rule' described in the Hybrid Conference Report * was quite self-sterile, the stamens being highly abortive.

* *Journal R.H.S.* vol. xxiv. p. 160.

The application of the pollen of *P. alata* resulted in the development of a fruit of a size one would expect to be produced when the relative size of the fruits borne by the parent plants is considered. This fruit (fig. 41)

FIG. 41.—FRUIT OF PASSIFLORA 'ST. RULE' (P. ALBA × P. BUONAPARTEA) RESULTING FROM POLLINATION BY P. ALATA. NATURAL SIZE.

contained a very considerable number of seeds well advanced towards maturity, but none were perfect.

P. subcrosa.

In contradistinction to the usual condition of things in the passion-flowers, *P. subcrosa* affords an extremely interesting example of a self-fertile species. The flowers are minute and utterly lacking in the gay coloration of its better known relatives. It sets fruit with the utmost freedom, without the aid of insects. The fruit, in size and colour, resembles a black currant. Efforts to cross this species with others have failed up to the present.

BEGONIA HYBRIDS.

Begonia (*tuberous variety*) × *B. coccinea.*

Reference was made at the previous Conference * to crosses between tuberous begonias and *B. coccinea.* One of these plants has been kept

* *Journal R.H.S.* vol. xxiv. p. 176.

growing up to the present time. It was found that the plants made splendid growth the first season from the seed and gave great promise for the future. Trouble came when the problem of perennation was faced. The plants were never so vigorous again, and none of them bore any flowers. The specimen kept has made but little growth for some years. Its rootstock is a compromise between a tuberous and a fibrous condition. In winter a number of rather weak-looking stems persist. They are not erect, but more or less twisted, in one case drooping over the edge of the pot. In spring a few new stems arise from the rootstock in a manner resembling the young shoots from a tuber, but the stems of the hybrid are much more woody. The plant does not now grow more than 7 or 8 inches high. Although so old, clear indication of the influence of *B. coccinea* in its parentage can still be seen, the younger leaves being dotted with the silvery spots characteristic of the young leaves of that species. Young seedlings of *B. coccinea* are very conspicuously spotted.

B. hydrocotylifolia × *B. coccinea.*

Reference was also made at the first Conference to seedlings of *B. hydrocotylifolia* × *B. coccinea*. These showed the influence of *B. coccinea* in having the leaves thickly dotted with silvery spots.

Two series of crosses between *B. hydrocotylifolia* and *B. coccinea* have been grown, and are represented now by a few plants of each. In the one series the spots seem to disappear almost entirely in the adult leaves ; in the other they persist with considerable distinctness. The habit of the respective plants is not quite the same. In the one the main shoots are strong and upright, but all the branches assume a drooping position ; in the other the pendent condition is but slightly developed. The strongest plant is eight feet in height. In all the plants the influence of *B. coccinea* in habit, foliage, and flower is very marked. One sees the slight influence of *B. hydrocotylifolia* in the broader, blunter form of leaf. The leaves are often larger than those of *B. coccinea*. The flowers can scarcely be distinguished from those of that species. The staminate ones are not quite normal. These hybrids are sterile, so far as I have observed.

Begonia heracleifolia × *B. coccinea.*

The seedlings of this hybrid were spotted as in the above. A few plants have been preserved. They are much alike in general characters. *B. heracleifolia* possesses a very stout horizontal rhizome, partly sunk in the soil. Its internodes are very short. The scars of the fallen leaves are large and surrounded by coarse hairs. The leaves rise erect in a group from the apex of the rhizome and its branches. The leaf-stalks are very long, and very thickly beset with long and strong white hairs arising in irregular groups from purple spots. The base of the hairs is purple. Close under the leaf-blade, the stalk bears a thick fringe of similarly coloured hairs, of much greater length and strength than those lower down. The leaf-blade is very large, and is very deeply cut into eight or more lobes. The lobes are irregularly serrate, the teeth terminating each in a single white hair. The veins bear on the under side spots and hairs similar to those on the stalk.

In *B. coccinea* the stem is quite erect, woody, smooth, not thick, and bearing many branches. The internodes are long, and the scars inconspicuous. The leaf-stalks are short, smooth, and reddish. The leaves are elliptical, oblique, hairless ; the margins very obscurely crenate. Many silvery spots occur in the upper surface of the young leaves, but become faint or disappear in older and later leaves.

In the strongest hybrid plant the stem left unsupported has grown at an angle of about 30° with the ground. It is fairly stout, the internodes short, the scars fairly large. A few very short hairs occur on it, and numerous minute green leafy outgrowths. Branching is fairly free. The leaf-stalks are of considerable length, clothed sparsely with minute white hairs rising from faint purple spots. The hairs are longer and more numerous in a ring round the top of the stalk. A few minute hairs occur on the under side of the veins. The fully-grown leaves are large. One measured nine inches in length. They are markedly one-sided, with five or six moderately deep lobes with fairly distinct crenation.

Flowers are borne by the hybrid. They bear a great resemblance to those of *B. coccinea*, but are not quite normal.

Begonia Dregei × *B. heracleifolia.*

Begonia Dregei dies down to a massive fleshy rootstock, from which it sends up annually a considerable number of erect stems to the height of eighteen inches or more. These branch very freely. The stems are pinkish and are swollen at the nodes. The leaf-stalks and veins are pinkish. The leaf-blade at the insertion of the stalk is deep red, but elsewhere bright velvety-green, and without hairs. Bristle-like hairs, however, spring from indentations of the leaf-margin.

Of sixteen good plants of *B. Dregei* × *B. heracleifolia* only three have been grown for study. They are now five years old, and are in very good health. The largest plant has formed a dense mass of short shoots, lying at various angles with the ground. One is nearly horizontal, and a few are nearly erect. The longest stem is not more than five inches. The internodes are short. The stems and older branches are covered by grey scaly bark. Small leafy growths appear on the stems and branches, in some places in a crowd, but usually somewhat sparsely. They vary in size from quite minute bodies to outgrowths like young branches.

The leaf-stalks in the largest plant are pale pink, with a few short silvery hairs arising from points crossed by faint red streaks. The hairs close up to the blade are much longer and form a thin collar. The leaf-blade is red at the place of insertion of the stalk. The longest leaf-stalk measures 2¼ inches. The longest blade measured is 4 inches. A few fine short hairs are found on the veins of the leaf, and minute bristle-like hairs occur in the indentations of the margin.

The other two plants are indistinguishable from the above in all essentials. In the smaller one the leaves are darker green, and in the larger the leaves are rather more deeply cut. The former has the further distinction of bearing on the leaves the outgrowths referred to above.

The great majority of the leaf-stalks are quite destitute of them. The stalks bearing them are the hairiest. The outgrowths occur in the centre of two of the leaves, and a small one on one of the chief veins. None are found on the under side of the leaves.

The above plants have never been known to flower.

Reciprocal Hybrids of Begonia fuchsioides × *B. foliosa.*

Begonia fuchsioides and *B. foliosa* are much alike in habit of growth, foliage, and flower. Both are erect-growing, many-stemmed, fibrous-rooted species, with comparatively small leaves and flowers.

B. fuchsioides is distinguished from *B. foliosa* by being the stronger. Its leaves are stalked, elliptical, oblique, acute, with numerous minute serrations. The longest leaves are 1½ inch.

In *B. foliosa* the leaves are sessile, ovate, oblique, with a few deep indentations on the opposite margins near the apex, followed by minute serrations. The largest leaves are ¾ inch long. The flowers of *B. foliosa* resemble those of *B. fuchsioides*, but are smaller.

Reciprocal hybrids between those two species were easily secured. The one series cannot be distinguished from the other. All bear a very decided resemblance to *B. fuchsioides*. They are stronger growers than that species. The leaves are almost sessile, less acute than in *B. fuchsioides*, with indentations corresponding to those in *B. foliosa*, but less marked. The longest leaves are 1½ inch.

Seedlings of the hybrids have been raised in considerable numbers. In no case has Mendelian segregation been noticed in them. They have repeated the characters of the hybrid parents. It seems certain, however, that many of the seedlings of the second generation are not such free growers as those of the first. One has varied from the type, and produced leaves which curve backwards and give the impression that the plant is suffering from drought.

In connection with the matter of reproduction of hybrid characters, it may be of interest to mention that I find a repeat of that well-known hybrid, *B. weltoniensis*, coming true from seed year after year.

ZONAL PELARGONIUM HYBRIDS.

Pelargoniums afford a ready means of experimenting with plants having variegated leaves.* One series of experiments with these may be described. Two variegated zonal pelargoniums were chosen as parents. The variegation of the seed-parent was of a familiar kind, the peripheral zone of white enclosing a bluish-green centre and sending into it projections of more or less intense variegation. In the pollen-parent the variegation, also white, occupied the centre and margin of the leaves. The flower of the seed-parent was white, and that of the pollen-parent was probably scarlet, but the record has been lost.

The seedlings resulting from the cross were in the majority of cases non-variegated and coarse. A few were variegated from the first, but only one has been made special note of. Its cotyledons were blotched with white (fig. 42), thus demonstrating clearly that, whatever variegation

* *Cf.* Focke, *Die Pflanzen-Mischlinge*, p. 95.

may be due to, it can in certain cases be transmitted by way of the seed. In this connection it is interesting to observe that in some seedlings of very dark-leaved zonal pelargoniums a distinct crescentic mark, of a colour corresponding to that forming the zone in the leaves of the parent, appears in the cotyledons.

Very soon three distinct vegetative regions were differentiated in the seedling under discussion, one including leaves with normal chlorophyll development, the next with variegated leaves, and the third with leaves quite destitute of chlorophyll. If a leaf arose in a plane between any two regions, it embodied in itself features of both. It sometimes happened that a mere thread of white would pass up the edge of a leaf-stalk, otherwise green, and connect with the white segment of the blade.

FIG. 42.—VARIEGATED SEEDLING OF HYBRID ZONAL PELARGONIUM. NATURAL SIZE.

When this occurred the greater vigour of the chlorophyll-bearing part of the leaf was very obvious.

Ultimately a branch lying wholly in each region was produced. Variegation was only once seen in the green branch, a small patch of white occurring on one leaf. The variegation of the variegated branch was identical with that of the seed-parent. The albino portion showed marked persistence. It died back, but new branches soon appeared. No trace of green was seen in the branches. The leaves were many times smaller than the green ones. The stem was red. It was necessary to prune the shoots of the green part, otherwise they would have caused the variegated and white parts to dwindle.

Although the white part was to be regarded as a parasite, it flowered fairly freely. The scape was very light pink. The sepals were white, and, although destitute of chlorophyll, were as large as those of the normal flowers. The petals were normal in size, shape, and colour. The pollen appeared to be perfect. The pistil was of the ordinary size and shape, but the ovary was colourless, not light green. The stigma was bright red as usual.

A few experiments were tried to induce the flowers of the albino shoot to set fruit, but without success. When, however, the pollen of the albino flowers was used to fertilise the flowers of the green shoot, the results were very successful.

Four fruits were thus secured. They contained respectively 2, 4, 5, 5 good-looking seeds. Seeing that 5 is the full yield, it was evident that the pollen used must have been good. Record kept of seven seedlings derived from the above 16 seeds shows that two of them had plain green leaves, one faintly zoned, and three distinctly zoned. The seventh seedling appeared late; it was slightly variegated, and very dwarf and weak. The later history of the seedlings was not noted.

Centaurea ragusina (candidissima) × *C. Scabiosa.*

Passing reference was made at the Conference in 1899 to a hybrid *Centaurea*, the parentage being *C. ragusina* and *C. Scabiosa*. Since that

FIG. 13.—CENTAUREA RAGUSINA × C. SCABIOSA.

event the hybrid has developed its adult characteristics. It is a very strong-growing, hardy, herbaceous perennial, forming large tufts. It resembles in many ways its pollen-parent, *C. Scabiosa*, which with us occurs as a fine sturdy native, making its home amongst the sand-dunes of the seashore.

The original plant refused to flower in a pot. Plants propagated from it were planted out in the open, and after passing through the winter unprotected they flowered the following summer. The flower-shoots (fig. 43) are very numerous and attain a height of 2 feet 9 inches. The flowering period lasts a long time.

The leaves have assumed the parental characters in a remarkably intermediate degree; the lobing is more pronounced than in *C. ragusina*, and less than in *C. Scabiosa*. The leaves of the seed-parent are snow-white by reason of the felted coat of hairs; the adult leaves of the pollen-parent are green, with a few hairs of the same kind along with stouter ones. In the young leaves of *C. Scabiosa* the felt-forming hairs are fairly abundant. In the young leaves of the hybrid the hairs are so numerous as to render them white, in the adult ones they are sufficiently plentiful to give a distinctly tomentose appearance.

The capitula are of fair size, creamy-orange, shaded into salmon at the centre, some of the purple of the pollen-parent being mingled with the yellow of the seed-parent.

The peripheral neuter flowers are comparatively few in number and very inconspicuous. They form a single whorl and project ¼ to ⅜ inch beyond the involucre. They are often hidden by being overtopped or pressed down by the neighbouring florets. The anthers of the hermaphrodite disk florets may project a little beyond the corolla. The styles elongate and carry the stigmas ¼ inch above the corolla. The pollen is fairly abundant, but most inferior in quality; in fact, in the examples examined, not a single normal grain was to be found. This hybrid is quite sterile.

Brassica oleracea var. × *B. Sinapistrum*.

Unusual interest attaches to a hybrid having curled kale as seed-parent and charlock as pollen-parent. Six seeds, the contents of three capsules, were sown together, and five of them germinated. One of the seedlings proved to be more like a cabbage cross than anything else, and could not be accounted for on any ground except that it had been the result of self-pollination of one of the flowers experimented with. The other four seedlings were alike and soon betokened hybrid characters. One was potted, two planted out, and the fourth left in the seed-bed. They all, at an early stage, looked very similar to charlock. They shot into flower when quite young, and continued to flower throughout the whole season, until stopped by frost.

When in full vigour, the taller one of the two planted out reached the height of 5 feet, the mass of bloom being 2½ feet through. Nine flowers were occasionally found open at one time on a single inflorescence. This plant and its neighbour (fig. 44) were striking objects, being freely branched, well furnished with leaves, and topped by very numerous flowers, while crosses of the same age between curled kale and Brussels sprouts and the like were in the usual compact vegetative condition of the first season.

The leaves (fig. 45 *b*) partook strongly of the charlock characters (fig. 45 *c*), but were larger, firmer in texture, and more glaucous. The inflorescence was a long raceme. The flowers (fig. 46 *b*), bright yellow,

FIG. 44. BRASSICA OLERACEA VAR. × B. SINAPISTRUM.

bore a considerable resemblance to those of charlock, notably in the narrow, spreading sepals (fig. 46 *c*).

Although hundreds of flowers were produced on the plants in the plots, and were attractive to bees, no fruit was set. The pollen, examined microscopically, was found to be hopelessly poor.

FIG. 45.—LEAVES OF (*a*) CURLED KALE, (*b*) HYBRID, (*c*) CHARLOCK.

The seedling potted grew moderately well. Four of its flowers received pollen of a plant naturalised on the cliffs at Crail, Fifeshire, and evidently a degenerate curled kale; six of Swedish turnip: fourteen of a garden yellow turnip; eight of black mustard; eleven of white mustard; and three the pollen of its own flowers, such as it was. None of the flowers so pollinated produced a fruit with good seed. In one pollinated from the yellow turnip a fruit $\frac{3}{4}$ inch long was produced, and contained a very poorly developed seed.

Some of the inflorescences of the potted plant, somewhat "drawn," were 18 inches long, and bore during the season from 80 to 100 flowers.

FIG. 46.—FLOWERS OF (*a*) CURLED KALE, (*b*) HYBRID, (*c*) CHARLOCK. NATURAL SIZE.

The early flowering of the hybrids was indicative of the annual duration of the pollen-parent. The plants remained in good health until the month of January, when a severe frost so crippled them that they never recovered. In all probability they would not have persisted, although they had escaped frost.

Ribes Grossularia × *R. sanguineum.*

A hybrid between the gooseberry and the flowering currant was secured in 1898, the seed-parent being a popular kind of garden yellow, name unknown. Six seedlings appeared above the soil. Two of these continued to live for a time, and ultimately only one, the stronger, was left. This was planted out and is now a bush 4 feet high, and 4 feet through. Rather scanty notes have been made of it. The habit is more that of the flowering currant than the gooseberry.

The bark of the gooseberry shoots is light grey. The internodes of young, strong shoots (fig. 47 *b*) are thickly studded with slender prickles, the largest being $\frac{1}{4}$ to $\frac{3}{8}$ inch long and fairly persistent. Fruit-bearing twigs (fig. 47 *a*) are destitute of them.

The shoots of the flowering currant (fig. 47 *c*) are dark red and coated with a greyish bloom. The buds are large and protected by bright red scale-leaves.

Fig. 47. *a*, Gooseberry, Ordinary Twig; *b*, Ditto, Strong Young Shoot; *c*, Gooseberry × Flowering Currant, Ordinary Twig; *d*, Ditto, Strong Young Shoot; *e*, Flowering Currant, Ordinary Shoot. Natural Size.

The young shoots of the hybrid (fig. 47 *d*) are rich brown and well studded with prickles, the longest being $\frac{1}{8}$ inch, the majority shorter. They are rather less persistent than the gooseberry ones. The twigs borne on the older ordinary branches (fig. 47 *c*) are much finer, rather redder, and without prickles.

The strong "spines" at the nodes, so characteristic of the gooseberry, never occur in the hybrid.

The leaves, in respect of size, lie almost in the mean between the parent leaves, but in texture there seems to be a decided leaning towards the pollen-parent. Their minute structure has not been studied.

This hybrid has shown no inclination to flower, although now seven years old. It is in perfect health.

Rubus Hybrids.

The Logan-berry.

It has long been known that Rubi intercross in the field with great readiness. The difficulty in assigning wild forms to their systematic position bears this out.

Perhaps the hybrid of most interest to horticulturists is the Logan-berry, the reputed result of a natural cross between *Rubus ursinus* and *R. Idæus* var. The logan-berry scarcely needs description. It is to all appearance a bramble, the influence of the raspberry parent being evidently slight. One of the most noteworthy characters of this plant is its fertility. Its seedlings grow as readily as weeds. With me, they show no trace of the raspberry, but repeat the logan-berry. Amongst minor variations are the form and size of the flower and lessened fruit-production. Some of the seedlings, however, prove to be as good bearers as the parent.

The seedlings of the second generation do not in general vary any more than those of the first generation. One plant, however, in a set of over twenty seedlings from a free-fruiting seedling of logan-berry has been found to be indistinguishable in habit, stem, and foliage from the garden raspberry. It is very vigorous. The strongest stems are 7 feet in length, and stand erect without support. Unfortunately, no notes have been made of the fruit borne by this plant; but there seems no reason to suppose that it is a chance seedling raspberry.

Raspberry × Logan-berry.

Seeing that it is understood that the raspberry is one of the parents of the logan-berry, it is interesting to note the peculiarities of hybrids between the raspberry and logan-berry, the latter being the pollen-parent.

Nine plants with this parentage have been grown for a long enough time to bring out their characters. Most of them are much alike. They all show the blending of the raspberry and the bramble habit. One plant is distinct from its neighbours, its foliage being more pointed, and the venation more sharply defined. It is far more floriferous than the others, and has borne a very heavy crop of fruit. The fruit (fig. 48 *b*) was interesting in showing a compromise in the core, between the conditions seen in the bramble and the raspberry. When the fruit was pulled (fig. 48 *a*) one found that the cap of drupels did not separate from the core (torus), the terminal half of the core being attached in a certain degree to the drupels, although by no means so firmly as in the bramble.

The plants do not show any appearance of true raspberry characters, such as might have been expected if the parentage of the logan-berry is what it is stated to be.

Rubus occidentalis × R. rosæfolius (syn. R. sorbifolius).

Rubus occidentalis, the blackcap or thimble-berry, is a tall-growing shrubby species, reaching the height of 7 feet. The stems are coated for a foot or two from the ground with a purplish-glaucous bloom.

No crown is formed, but the main stem gives off branches low down. Suckers are not produced. The branches die back a little in winter, leaving dead points. Secondary branches are borne from successive buds. They about equal each other in size, and may be regarded as of the nature of spurs.

The leaves are pinnate, with usually three leaflets; occasionally there are five, and sometimes the lowest pair branch and produce each a pinnule pointing backwards. The leaflets are broadly ovate, with comparatively shallow serrations. A few minute prickles occur on the rachis of many of the leaves.

The flowers (fig. 49 A) are produced in terminal panicles. They are ¾ inch in diameter, the petals very small, irregular in size and number, and white.

FIG. 48.—FRUIT OF RASPBERRY × LOGAN-BERRY *a*, WITH DRUPELS REMOVED; *b*, ENTIRE FRUIT. NATURAL SIZE.

Rubus rosæfolius, better known in gardens as *R. sorbifolius* (the so-called strawberry-raspberry), differs in every way from the above. It spreads greatly by means of suckers, and crowns or tufts are formed here and there. The aërial shoots are annual, and attain at most the height of 18 inches.

The leaves are pinnate, five being the usual number of leaflets; often there are seven. . In form they are narrow, oblong-elliptical, with deep serrations, and a rugose surface. Many sharp, curved prickles are present on the rachis and the midribs of the leaflets.

The flowers (fig. 49 B) are borne singly or a few together in a very loose leafy panicle. They are very large, the largest 2 inches in diameter. The petals are broad, firm, white and symmetrical.

The hybrid between *R. occidentalis* and *R. rosæfolius* is a very marked one. It is now six years old. Its height is 4 ft. 6 in. It is fairly intermediate between its parents in respect of its method of perennation. The stems arise close to the surface of the ground or beneath it. The great majority of them, including the strongest as well as the weakest, die

FIG. 49.—FLOWERING SHOOTS OF (A) RUBUS OCCIDENTALIS, (B) R. ROSÆFOLIUS, (C) R. OCCIDENTALIS × R. ROSÆFOLIUS. NATURAL SIZE.

back to the base. The strongest stems are usually much branched, the branches being often long and spreading a good deal, and bearing branchlets. Many stems, however, including the tallest, are unbranched. Last spring, of 18 stems present, 4, all unbranched, did not die to the base. On one of these stems, alive at the height of 2 ft. 4 in., the bud at the height of 1 ft. 4 in. pushed into leaf, two buds higher up being left exhausted. Another stem pushed a strong bud at 1 ft. up, and the third a strong bud at 1 ft. 6 in. The fourth stem pushed a bud at 13 ins., two buds beyond that dying. No suckers appear. The leaves show the blending of the parental characters. There are usually five leaflets, sometimes seven, with occasional development of the supplementary pinnules as in *R. occidentalis*. The serration is moderately deep, and the surface rugose, but less so than in *R. rosæfolius*. Much red coloration is developed in the leaves. Prickles of an intermediate size occur on the rachis.

Flowers (fig. 49 c) are produced, but very sparingly. They are about the same size as those of the seed-parent, but are of quite a different shape from those of either parent. Their colour is white. They are absolutely sterile.

Rubus phœnicolasius × *R. Idæus var.*

The hybrid between the Japanese wineberry and the raspberry, and represented by only one plant, is a distinct one, showing many interesting intermediate characters. The stems of the wineberry are green and clad with a dense coat of rose-coloured hairs, with sparsely distributed green prickles $\frac{1}{4}$ in. long. In the raspberry the stems are coated with bloom, and are naked except for the presence of scattered minute dark prickles. In the hybrid the stems are pale green, clothed with minute rose hairs, with scattered deep purple prickles $\frac{1}{8}$ inch long.

The flowers of the hybrid are small, and open at a date later than the raspberry, but earlier than the wineberry.

This hybrid produced a few small fruits this season. It seems likely that when the plant is better established it will bear a fair crop. The date of ripening lies between the dates for the parents.

The President said they ought to have more facts as to the sterility of hybrids, because as soon as sterility manifested itself they were at a loss.

Professor Lotsy: We have been hybridising *Carex* at Leyden, and though we have experimented with thousands we have never got a single seed.

THE HEREDITARY TRANSMISSION OF COLOUR IN CROSS-BREEDING.

By C. L. W. Noorduijn (Groningen).

In the August number (1904) of "Album der Natuur," in which I gave a short explanation of Mendel's law of heredity, I ventured the opinion that experiments such as Mendel described, if prosecuted for a course of years, would lead to other results, because the dominance of the wild type over a variety is dependent upon the length of time during which that variety has been bred pure.

As is well known, the Abbot Gregor Mendel made the discovery that in crossing plants of the original wild type with any of its varieties, it mattered not which, all the resulting cross-breds had the external characteristics of the original type. The discovery and the behaviour of subsequent generations of these cross-breds induced him to relate the results of his experiments to the Society of Natural Science in Brünn, in February and March 1865, with the result that it was subsequently known as Mendel's law of heredity. Mendel selected from several seed shops some thirty-four different varieties of peas, and subjected them to a two years' test in order to make sure that they bred true.

It was some years before Mendel's law attracted general attention. In fact, it was not until the opening of the present century that anyone took up Mendel's ideas, and the discovery was made that an extensive field of investigation had been opened to the student of natural science. The result of my personal experiments is that I am not altogether able to agree with the conclusions which have been deduced.

We are aware that corresponding varieties of animals and plants may by careful selection—that is, by the rigid weeding out of those individuals showing a tendency to vary—after a few generations be induced to breed true, and to show no tendency to revert to the original type. In proportion to the period of time during which varieties have been bred true will their power to hereditarily transmit their characteristics be increased, and the force of reversion to the original wild type be diminished; whilst, on the other hand, a variety which is comparatively new will display this tendency to revert in a very much more accentuated degree. Hence it follows that if a comparatively modern variety is crossed with the original wild type, the latter shows itself as dominant or preponderant in its influence on the offspring; whilst if a variety which has been bred pure for a great number of years is similarly crossed, its characteristics have sufficient weight to dominate those of the wild type, and reversion is of rare occurrence. In view of what I have above stated, I have come to the conclusion that what is called Mendel's law is no law at all. The experiments I have made during the present year have satisfied me that hybrids sprung from a cross between the original wild type and a long-established variety of it show the characteristics of the variety, and so the fundamental principle of Mendel's law breaks down, and with it his whole theory. Mendel's experiments were made entirely with plants, but I am convinced that similar experiments in the breeding of animals will lead to the same results. These experiments have already been made upon rats, mice, and snails, and the investigators have come to the conclusion therefrom that Mendel's law is applicable also to the crossing and pairing of these animals, a view which, in my opinion, is not justified by the facts.

Albino rats and mice, when crossed with the original type, produce exclusively hybrids in entire correspondence with the wild type, and therefore are in agreement with Mendel's law. The reason of this is that the possession of pigment is dominant over the want of it. The first generation from the cross of the wild type with the albino being entirely like the wild type, one would assume that when members of that generation were paired together their progeny would follow exactly the characteristics of the parents. But this was not the case; they were albinos and common coloured individuals in the proportion of one to three—a result in agreement with Mendel's law. This, however, seems to me to be only a proof that the force of reversion can be stronger than that of heredity. To put Mendel's theory to the test, one should use varieties in which pigment is present, and which have been bred pure for a considerable time, and not albinos; and then one would find that different results would be obtained. Take, for instance, two contemporaneous varieties such as Nature sometimes presents us with, *e.g.* albino and cinnamon-coloured starlings. Here we have an equal power of hereditary transmission and an equal latent tendency in each to revert to the original type. By pairing such varieties together the two equal hereditary forces counteract each other, leaving the field open to the latent characteristic, with the consequence that the offspring take the appearance of common starlings. Cinnamon canaries have been bred pure for a shorter time than yellows. The pairing of cinnamons with yellows will not therefore result in perfect reversion, but the offspring will be variegated greens, *i.e.* green mixed with yellow. From this pairing we get cinnamon-marked young (always hens), but only when the father is a cinnamon, the reason of which is that the preponderance of the hereditary force is on the father's side. An albino starling paired with one which had some white feathers produced young entirely like common starlings. In verifying Mendel's law, it is necessary therefore in the first place to consider for how long the varieties to be experimented upon have been bred pure, in order to form an accurate idea of the respective forces of heredity on the one hand, and of atavism, or the tendency to revert to the wild type, on the other.

In modern varieties the force of heredity is weak in comparison with the tendency to reversion, and consequently in breeding we find that the latter force holds the chief sway.

Last year I received from Herr Böcker, the son of the well-known ornithologist at Wetzlar, two young wild cock canaries caught in Teneriffe. I crossed them this year (1906) with several different varieties of the domestic canary, viz. cinnamons, green variegated, and yellows. The young from the cross with the cinnamon were all in their nest feather entirely resembling the wild type, as I expected would be the case. Those from the green variegated cross were much the same as the last-mentioned, but several had white feathers in the tails and wings, and yellow towards the extremity of the belly. Those from the yellow cross were green variegated, and half, or rather less, had the yellow plumage of the variety.

The yellow colour in canaries has been known and bred for something like 350 years. In its hereditary characteristics it seems therefore to have acquired such force as that even when crossed with the wild type it is incapable of being suppressed, and can impress its likeness on a considerable portion of the offspring. This seems to lend proof to my view that Mendel's law is only applicable to cases of crossing between the original type and such varieties which either have been in existence for a short time only, or are not yet pure bred, or are albinos.

THE INFLUENCE OF THE PARENTS ON THE COLOUR OF THE HYBRID.

By F. J. Chittenden, F.R.H.S.

It seems to have been an object of most writers on the subject of hybridisation since the time of Herbert to formulate a law which should be a guide to workers in their efforts to improve plants, and especially have they sought to assign to the parents the influence which they each respectively exert upon the vigour, the form, and the colouring of the offspring. The idea that it is the pollen parent that governs the colour of the flowers of the offspring has been a very prevalent one, and one that is very frequently given expression to even yet.

Dean Herbert wrote, in 1835 : "For the purpose of obtaining a large or brilliant corolla it will be probably found best in the long run to use the pollen of the species which excels in those points, because the corolla in truth belongs to the male portion of the flower, the anthers being usually borne upon it or in some manner connected with it by a membrane; but upon the whole an intermediate appearance may generally be expected." * From the wording of this quotation it appears to be a rule arrived at by reasoning on a wrong assumption, though one perfectly legitimate so far as knowledge in those days went, rather than the result of his large experience in the raising of hybrid plants. Other writers have expressed similar views, for example: "Experiments . . . seem to justify this important inference, that as a general rule the properties of the male parent will be most conspicuous in the hybrid . . . Exceptions no doubt exist, and hybrids could be found which are either half-way between their father and mother or more like the mother than the father ; but as the means of judging at present exist, these would seem to be the exception and not the rule ; therefore the greater influence of the male may be taken as a tolerably safe guide in all experiments in this interesting art." †

Again, Burbidge writes : "The evidence of the prepotence of the male parent is well-nigh overwhelming ;" ‡ but a little before he says, "I have often found the mother more marked than the father in the hybrid offspring." §

At the Conference on Hybridisation held in London in 1899, Dr. Wittmack said that, in his opinion, "the father [has] the more [influence] upon the inflorescence, at least upon its colour ;" ‖ and Herr Max Leichtlin said, "The male parent gives more or less the colouring of the flowers, and if it is richer and freer flowering than the female, this property is transferred to the offspring." ¶

These quotations serve to emphasise the widespread belief in the influence the male parent has upon the colour of the hybrid.

* Herbert, *Amaryll.* p. 348. † *Gard. Chron.* 1844, p. 459.
‡ *Prop. and Improv. of Cult. Plants*, p. 170. § *L.c.* p. 1
‖ Wittmack, *Journ. R.H.S.* xxiv. (1900), p. 255. ¶ *L.c.*

Other writers, however, have denied that this belief has any foundation in fact, and for this reason I thought a comparison of the colours of hybrids with those of their parents, in order to test the question whether any rule of influence of pollen or seed parent on the colours of the hybrid could be stated, would be of interest to this Conference.

There are at least three reasons, in addition to the contradictions that have arisen, why this comparison seemed desirable.

(1) The importance of a knowledge of the laws governing inheritance from a practical as well as from a scientific standpoint.

(2) The case of reciprocal crosses; and

(3) The results of the recent investigations into the Mendelian laws of inheritance.

The first point needs not to be enlarged upon; its truth is self-evident. The second, concerning reciprocal crosses, presents itself as a contradiction to the statement that the pollen parent exerts the greater influence on the colouring of the hybrid, a contradiction which, one would think, should have shown what little reliance could be placed upon rules that seek to assign certain influences exclusively to the male or the female parent, for it was shown long ago that the results of reciprocal crosses vary very little from one another. M. Naudin, in describing some hybrids between *Datura ferox* and *D. lævis*, says the offspring of this reciprocal cross attained "the most complete development, and were so perfectly like each other that the two sets might easily have been regarded as one. . . . This is a new confirmation of what I have already announced ('Comptes Rendus de l'Acad. des Sci.,' 1862), that there is not a sensible difference between reciprocal hybrids of two species, and that in the first generation the hybrids of the same origin resemble each other as much as individuals of pure species from the same sowing." * Darwin, too, arrived at the same conclusion, for he writes: "Hybrids raised from reciprocal crosses . . . rarely differ in external characters." †

Col. Trevor Clarke found that the reciprocal crosses of *Begonia Dregei* and *B. heracleifolia* and of *B. cinnabarina* and *B. Pearcei* each produced plants which scarcely differed from one another and instances of this might be easily multiplied.

The third point is again one in which doubt is cast upon the belief that the male parent has a prepotent influence in determining the colour of the flowers, and the law of dominance, which is the first of the Mendelian laws, has received almost sufficient confirmation, I think, to show that we must look to special characters to impress themselves upon the hybrid, whether these characters belong to the male or to the female. It has long been known that certain characters are dominant over others, although attention has not been drawn to the fact so clearly as since the awakening of interest in the laws of inheritance, especially as propounded by Mendel, owing largely, in English-speaking countries at least, to the efforts of the President of this Conference.

Gaertner found that "some species of the same genus have a remarkable power of impressing their likeness upon their hybrid offspring," and again, "there are certain hybrids which, instead of having, as is usual, an

* Naudin, *Journ. R.H.S.* 1866, p. 2.
† Darwin, *Origin of Species*, 6th ed., p. 379.

intermediate character between the two parents, always closely resemble one of them." In one of his books Darwin writes, "Mr. Paul informs me that he fertilised the 'Black Prince' hollyhock with pollen of the 'White Globe,' the 'Lemonade,' and the 'Black Prince' hollyhocks reciprocally, but not one seedling of these three crosses inherited the black colour of the 'Black Prince.' . . . Mr. Laxton writes, whenever a cross has been effected between a white-blossomed and a purple-blossomed pea . . . the offspring seem to lose nearly all the characteristics of the white-flowered . . . varieties; and this result follows, whether these varieties have been used as the pollen-bearing or the seed-producing plants." *

This dominance of certain characters over others has been confirmed by recent work, and it seems likely that those whose work lies only with a small number of species in hybridising, as, for example, with the Narcissi, where *Narcissus poëticus* seems to show a greatly predominating influence in the colour of the cup over the *N. Pseudo-Narcissus* varieties, may find that the rule holds good, for they may use the dominating partner as the pollen parent as a rule, but the rule may not hold in other cases; it may not be of universal application.

Such a law, if it exists, can only be formulated after the examination of a large number of instances. The following table shows the result of the examination of 183 hybrids, mostly first crosses, though not exclusively so, belonging to 67 different genera. Only those cases where the parentage seemed to admit of no doubt have been taken, and the *Orchidaceæ* have been purposely omitted.

Out of these 183 hybrids we find 42 distinctly following the male parent in the colour of the flower, 46 show a dominating influence in this direction excited by the seed parent over the pollen parent, 92 show about an equal amount of colour derived from both pollen and seed parent, while in 3 instances the colouring follows neither.

Among the 92 hybrids showing equal amounts of influence from both parents, 19 were derived from parents whose colours were the same as one another, and may, perhaps, be omitted, leaving 73 in column 4 showing the influence of both parents in the colour of the offspring in about equal degrees.

We have, therefore, omitting these 19 examples, only 42 cases out of 164 in support of the belief that the pollen parent has the predominating influence in determining the colour of the hybrid, while there are 46 cases diametrically opposed to it.

These figures show that there can be no rule stated as to the dominating influence of the male or the female parent in all cases, or even in the majority of cases.

We must look rather to the discovery of the dominating characters, whether possessed by the male or female parent, upon which to formulate a law which shall be a guide on this subject in the future of hybridisation.

One point that has impressed itself upon me particularly in the examination of these hybrids is the frequency with which, in certain combinations of colours possessed by the parents, the yellow colour of one or the other has failed to be transmitted to the offspring: in other words, has proved

* Darwin, *Variation of Animals and Plants*, vol. ii.

to be a recessive character; thus, when orange and white flowers are crossed in several instances, pink or red flowers have resulted, the red of the orange being transmitted or dominant, the yellow recessive. When red or yellow flowers are crossed with white, it often happens that the hybrid shows these colours in a less pronounced degree: is, in fact, intermediate in colour.

Table showing the Influence of the Male and Female Parents on the Colour of the Flowers of the Hybrids.

Genus.	Number following pollen parent mostly.	Number following seed parent mostly.	Number nearly intermediate.	Number following neither.	Total.
Anemone	1	—	1	—	2
Anthurium	—	1	1	—	2
Aquilegia	—	1	1	—	2
Astilbe	—	—	1	—	1
Azalea	—	1	—	—	1
Begonia	5	2	8	—	15
Berberis	—	—	1	—	1
Billbergia	1	1	3	—	5
Bravoa	—	—	1	—	1
Calceolaria	—	1	1	—	2
Cereus	1	—	—	—	1
Chamæpeuce	—	—	1	—	1
Cistus	—	—	—	1	1
Clematis	—	—	1	—	1
Crassula	—	1	—	—	1
Crinum	1	2	—	—	3
Cyclamen	—	—	1	—	1
Cyrtanthus	1	1	—	—	2
Cytisus	—	—	4	—	4
Datura	—	—	—	1	1
Dentaria	—	—	1	—	1
Deutzia	—	3	3	—	6
Dianthus	4	6	5	—	15
Diplacus	—	—	1	—	1
Disemma	—	1	—	—	1
Escallonia	1	—	—	—	1
Forsythia	—	—	1	—	1
Fuchsia	—	1	—	—	1
Galtonia	—	—	1	—	1
Gentiana	—	1	—	—	1
Gesnera	—	—	1	—	1
Helleborus	—	—	1	—	1
Hemerocallis	—	1	—	—	1
Hibiscus	—	—	1	—	1
Hippeastrum	—	—	1	—	1
Iris	5	7	9	—	21
Kalanchoë	—	—	1	1	2
Lachenalia	—	1	3	—	4
Lilium	2	—	—	—	2
Lobelia	—	—	2	—	2
Lonicera	—	—	4	—	4
Mimulus	—	—	1	—	1
Montbretia	—	—	1	—	1
Narcissus	2	2	1	—	5
Nægelia	—	1	—	—	1
Nerine	—	—	5	—	5
Nymphæa	1	1	1	—	3
Papaver	—	—	1	—	1
Passiflora	—	1	1	—	2
Phaseolus	2	—	1	—	3
Primula	1	1	2	—	4
Rhododendron	3	3	6	—	12

Genus.	Number following pollen parent mostly.	Number following seed parent mostly.	Number nearly intermediate.	Number following neither.	Total.
Ribes	—	—	1	—	1
Richardia	—	—	1	—	1
Rosa	1	—	—	—	1
Rosanowia	1	—	—	—	1
Scheeria	1	—	—	—	1
Sempervivum	—	—	1	—	1
Spiræa	—	—	2	—	2
Senecio	2	2	2	—	6
Streptocarpus	2	1	3	—	6
Syringa	1	1	—	—	2
Tecoma	—	—	1	—	1
Tacsonia	1	—	—	—	1
Vallota	—	1	—	—	1
Vriesia	2	—	—	—	2
Zephyranthes	—	—	1	—	1
Total (67 genera)	42	46	92	3	183

HYBRIDISATION AND THE SYSTEMATIC ARRANGEMENT OF ORCHIDS.

By Professor Pfitzer, of the University of Heidelberg.

At the present time the natural arrangement of orchids is not yet quite settled. In England Bentham's system prevails, the chief sections of which are founded (like that of his great predecessor, Dr. Lindley) on the consistency of the pollinia and their various caudicles and glands. On the Continent of Europe, on the contrary, most botanists hold the opinion that these things are adapted specially to the fertilisation of the

Fig. 50.—Professor Pfitzer.

flowers by insects, and therefore that they are not very suitable organs for establishing the principal characters of the systematic groups. In 1887 I therefore proposed a systematic arrangement of orchids founded on the general morphological structure of the whole plant, and Dr. Engler accepts this system for his systematic works, so also does Monsieur Cogniaux for the *Flora Brasiliensis*.

I think that hybridisation may perhaps afford a means of ascertaining which is the right method of classification. Of course, if an orchid is fertilised with another without success, it proves nothing. We know that sometimes it is quite easy to obtain a hybrid by fertilising one species (A) with a nearly related species (B), but that it is impossible to get fertile seeds from the same plants by fertilising B with A. It follows, therefore, that failure in the fertilisation of two orchids must never be used as a proof that they are not nearly related.

But if, on the contrary, one can obtain a hybrid between two genera of orchids, I hold the opinion that they are certainly nearly related. On this point I differ from Mr. Rolfe, who says, in his paper on "Bigeneric Orchid Hybrids," that species, and genera too, will always have to be dealt with in the scheme of classification according to their structural peculiarities and differences, without reference to the possibility of hybridisation taking place between them.

Let us see how far hybridisation takes place between two genera which are not in the same subtribe. If we suppose that Bentham's system is a natural one, we only know one apparent example, viz.: the hybridisation between *Phaius* and *Calanthe*. Bentham placed *Phaius* in his *Bleticeæ*, *Calanthe* in his *Cœlogyneæ*. But in my opinion *Calanthe* should have its place near *Phaius* in the *Bleticeæ*.

There are some reputed hybrids which would certainly overthrow Bentham's system and my own, too, if they really existed. Mr. Rolfe has spoken of hybrids between *Zygopetalum* and *Lycaste*, *Odontoglossum* and *Zygopetalum*, *Chysis* and *Zygopetalum*. But the plants had not flowered in 1888, when Mr. Rolfe wrote his paper; and if actual living hybrids have resulted from these crossings we should certainly have heard of them ere now. The result must, I think, have been the same as in a case described by Mr. Veitch, at whose establishment seedlings were obtained by crossing *Zygopetalum Mackaii* with several species of *Odontoglossum*; but every one of these supposed hybrids proved afterwards to be *Zygopetalum Mackaii*; and it is quite improbable, nearly impossible to think, that in these experiments the pollen of *Zygopetalum* was not properly excluded. I believe, therefore, that the *Odontoglossum* pollen had sufficient influence to stimulate the *Zygopetalum* into producing an embryo, but without impressing any of its own characters. For there was not the smallest character of *Odontoglossum* visible in the seedlings raised—therefore they were not true hybrids. Again, some few years ago the "Gardeners' Chronicle" reported that a hybrid between *Phaius* and *Cymbidium* was exhibited at one of the Society's meetings, but this supposed hybrid I consider to be a similar case to that of *Zygopetalum Mackaii*. For it is said that the plant looked in every respect like a strong-growing *Phaius*. Perhaps some one present saw this plant and can say if any signs were visible of the crossing with *Cymbidium*.

It might be possible in this way to try some experiments in hybridisation which would throw more light upon questions of systematic botany. For instance, I would call your attention to the genera *Phaius* and *Thunia*. The flowers are so similar that Bentham considered *Thunia* to be only a section of *Phaius*. On the other hand, *Phaius* has a lateral, *Thunia* a terminal inflorescence, and the habit is very different. If it were possible to obtain a hybrid between these two genera their close affinity would be proved. This experiment would be most interesting, because we do not yet know of any hybrid between an orchid with a terminal and one with a lateral inflorescence.

It would also be of great interest to try the hybridisation of *Zygopetalum* on one side and *Promenæa*, *Huntleya*, *Bollea*, *Warczewiczella*, *Pescatoria*, on the other. Bentham includes all these plants under the generic name of *Zygopetalum*, but the variations of the leaves and the habit are very different. At present we know of no hybrid between an orchid with convolute leaves and one with conduplicate leaves, save only the reputed *Phaio-Cymbidium*, which would surely be an intermediate form, if it is really a hybrid.

Again, the genus *Trichosma* has much similarity to *Eria*, as regards the flowers; but in the morphological structure of the whole plant it is nearer *Thunia*. It has also been united with the genus *Cœlogyne*. A trial should be made to see whether *Trichosma* gives hybrids with *Eria*, *Cœlogyne*, or *Thunia*.

Bentham founded a special subtribe, *Notylicæ*, for the genera which have a gland at the base of the pollinia and a dorsal anther. This subtribe contains the genera *Cirrhæa*, *Macradenia*, *Notylia*, *Acriopsis*, *Telipogon*, *Trichoceros*, *Podochilus*, *Appendicula*. This group is an entirely artificial one: thus *Cirrhæa* is quite near to *Gongora*, *Notylia* to *Oncidium*, *Macradenia* to *Bulbophyllum*. If anyone succeeds in obtaining a hybrid between *Cirrhæa* and *Gongora*, or *Notylia* and *Oncidium*, it would be a strong argument for my opinion.

Octomeria is somewhat intermediate between the subtribes *Pleurothalleæ* and *Læliceæ*. Someone should try to cross it with *Pleurothallis* or *Stelis* on the one side, and with *Hartwegia* or *Arpophyllum* on the other.

The beautiful genus *Sobralia* is placed by Bentham in his subtribe *Vanilleæ*, because the pollinia are soft and granular. I consider that *Sobralia* is more nearly related to the *Læliceæ*, to *Cattleya*, and to *Lælia* itself. It would be very interesting if anyone would try to cross *Sobralia* with *Vanilla*, the next genus in Bentham's arrangement, and on the other side with *Cattleya*.

In conclusion, it may perhaps be useful to mention what differences have hitherto proved no obstacle in obtaining hybrids between different genera of orchids and what have so proved. The following differences present no obstacle to hybridisation:—

1. Absence or presence of a gland at the base of the pollinia (*Phaio-Calanthe*).
2. The number of the pollinia (*Lælio-Cattleya*, *Epi-Lælia*, *Lepto-Lælia*, *Zygo-Colax*).
3. A long or very short column (*Sophro-Lælia*).
4. A twisted or very twisted column (*Anœctonaria*).
5. A concave or flat stigma, or, on the other side, well-developed stigmatic processes (*Sophro-Lælia*).
6. An adnate or free lip (*Epi-Cattleya*, *Epi-Lælia*, *Odontioda*).

The following differences must, until further proof, be considered to be impediments to hybridisation:—

1. Monopodial or sympodial structure of the whole plant.
2. Lateral or terminal inflorescence.
3. Convolute or conduplicate variations of the leaves.

In the orchids we generally find that great differences in the structure of the flowers and the pollinia are easily overcome in hybridisation, but that morphological differences of the whole plant are a great obstacle. For example, the parents of *Epi-phronitis Veitchii* look very different, but they both have a terminal inflorescence and conduplicate leaves ; they are, therefore, not very different in their morphological structure.

If these facts speak somewhat for my own systematic arrangement, and against that of Mr. Bentham, I should like to say that the merits of Bentham's systematic arrangement generally, and especially that of orchids, are so great that they can only be slightly diminished by the information which has been obtained since his time.

I regret very much that I can give no results of orchid-hybridisations of my own. But the raising of hybrid orchids and bringing them up to the flowering stage requires such an amount of experience that it would be useless for me to name, in this Conference, where the most successful and eminent hybridisers of the world are meeting, the few interesting objects with which I have been personally concerned.

NATURAL HYBRIDS OF THE CATTLEYA GROUP.

By R. Allen Rolfe, A.L.S.

The object of the present paper is to collect together the natural hybrids of the *Cattleya* group, which have become rather numerous. The cause is not far to seek. Orchids are largely dependent upon insects for the fertilisation of their flowers, and as insects seldom confine their visits to one particular species, the pollen is very likely to be interchanged, and thus hybrids may occur wherever allied species grow together. In this group, as in some others, it is evident that hybridisation is largely a question of opportunity, for hybrids occur between some of the most structurally distinct species where they happen to grow intermixed, uniting the genera *Cattleya*, *Lælia*, and *Brassavola*.

From a botanical standpoint it is important that these curious intermediate forms should be taken at their true value, for they destroy the natural limits of species, sections, and genera. And in practice we find that, unless their real origin has been recognised from the outset, they have been classed as anomalous forms, or varieties, or as distinct species, according to the amount of difference they present from existing forms. In several cases polymorphisms of the same hybrid have been classed as distinct species, and in at least one case, distinct hybrids have been included under the same name.

The literature of the subject has become rather extensive, and is widely scattered in a multitude of books and publications; and the object of this paper is to bring it into a single focus, and to present the subjects in their true relations as far as possible. I would emphasise the last remark, because several forms are only known to me from description, and finality is at present impossible, besides which our knowledge of the geographical distribution of some of the species is still very imperfect.

Historical Summary.

The earliest published allusion to the occurrence of a natural hybrid among Cattleyas that I have found occurs in 1856, by Lindley:—" When Mr. Skinner last returned from Guatemala he brought with him a small packet containing the flowers of three different Orchids, which he found growing from the same stock. One was *Cattleya Skinneri*, another was a dark crimson variety of *Epidendrum Skinneri*, the third, which was smaller than the first, but larger than the second, he suspected to be a hybrid between them " (" Bot. Mag." *sub* t. 4916). The plant figured as apparently identical with the third, flowered in the collection of J. D. Llewellyn, Esq., of Penllergare, and was named *Cattleya Skinneri* var. *parviflora*. It is pointed out that it is not the individual actually discovered by Skinner in 1854 or 1855; in fact it is thought to have been collected by Warscewicz; and although Lindley was " unable to recognise in it the presence of a natural hybrid," he added, "We think it a plant the history of which requires further confirmation." All the flowers mentioned above are carefully preserved on the same sheet in Lindley's Herbarium, and Lindley has added a pencil note to the three sent by Mr. Skinner: "*E. aurantiacum* is ♀ and not *B. Skinneri.*" When this note was added is uncertain, but in 1861 further evidence appeared. On March 26 of that year Messrs. Veitch exhibited a plant at a meeting of the Royal Horticultural Society under the name of *Cattleya* × *guatemalensis.* It is said to have been "an imported plant introduced by Mr. Skinner, who is of opinion that it is a natural cross between *C. Skinneri* and *Epidendrum aurantiacum*, specimens of both of which were sent along with it" ("Gard. Chron." 1861, p. 291). Shortly afterwards it was figured ("Fl. Mag." 1861, t. 61), and proved quite distinct from *C. Skinneri parviflora.* This view of the origin of *C.* × *guatemalensis* is now fully accepted, but it is, of course, conjectural which was the seed parent. A few other examples have since appeared among the parent species. *C. Skinneri* var. *parviflora*, however, is quite different.

Meantime other cases had appeared. In 1859 Reichenbach described *Lælia* × *irrorata*, from the collection of Consul Schiller, of Hamburg ("Hamb. Gartenz." xv. p. 57), and after pointing out its affinity with his earlier *Lælia Schilleriana*, and some resemblances to *Cattleya intermedia*, we find the additional remark, "Bastard ?" showing that the author suspected it to be of hybrid origin. A year later, he added *Lælia* × *euspatha* from materials sent by Dr. Casper, of Berlin, and M. Lüddemann, of Paris (*l.c.* xvi. p. 420), remarking, "I doubt not that this *Lælia* is a bastard." He then pointed out the resemblance to *Lælia elegans* in the pollen, and suggested that the novelty might have been derived from *Lælia Boothiana* or *L. purpurata* and *Cattleya intermedia* or some other species. In 1861 Reichenbach added his *Lælia* × *Stelzneriana*, previously described as a species, to the list, remarking: "This is perhaps a hybrid ?" ("Fl. des Serres," t. 1494). These remarks are significant, and it may be added that all the three just mentioned are now considered to be forms of Reichenbach's *Lælia Schilleriana*, described in 1855 as a species, but now considered to be a natural hybrid between *Lælia purpurata* and *Cattleya intermedia*, and consequently bearing the name of *Lælio-Cattleya* × *Schilleriana* (Rolfe in "Gard. Chron." 1889, ii. p. 155). The hypothesis has since been proved, Messrs. Charlesworth having raised it artificially from these two species ("Orch. Rev." 1898, p. 168), and the experiment has been repeated in two other collections.

After a rather long interval, in 1877, another curious intermediate form appeared, in the collection of J. W. Wilson, Esq., of Liverpool, and was described by Reichenbach under the name of *Cattleya* × *Wilsoniana*, "n. sp. (n. hyb. ?)." The author remarked that it was gathered in company with the old *C. bicolor*, and "there is not much doubt left that it ought to be regarded as a natural hybrid between this and perhaps *C. intermedia*, Grah." ("Gard. Chron." 1877, ii. p. 72). I have not seen it, but on account of its dark purple colour I have a suspicion that the second parent may have been *C. Harrisoniana.*

In 1877 also *Lælia* × *elegans* was added to the list. When describing the artificial hybrid *Lælia* × *Sedeni*, Reichenbach remarked: "The *Lælia elegans* (*Cattleya elegans* of Morren the elder) has always such pollinia with straps adherent on each side, and very often two pollinia on each strap. The *Cattleya* × *devoniensis* (one parent of *L.* × *Sedeni*) is a very curious product, since it is very much like *Lælia* × *elegans*; I even would regard it the same, if it was not said to descend from *Lælia crispa* and *Cattleya guttata*, when there is—at least of our actual knowledge—at the natal place of *Lælia* × *elegans* no *L. crispa* to be seen, and no *Cattleya guttata*, but the next cousins, *Lælia purpurata* and *Cattleya guttata Leopoldii*" ("Gard. Chron." 1877, ii. p. 424). It is now agreed that *L. purpurata* and *C. Leopoldii* are the parents, but we have not yet heard that seedlings from such a cross have flowered.

In 1882 there were three additions to the list, the first being *Lælia* × *Leeana*, which flowered in the collection of W. Lee, Esq., Downside, Leatherhead. It was described as a "n. hyb.?" (Rchb. f. *l.c.* 1882, i. p. 492), the author comparing it with *Lælia pumila*, *Cattleya Harrisoniana*, and *C. superba*. It is now believed to be a natural hybrid between *L. pumila* and *C. Loddigesii*—whence also the artificial hybrids *Cattleya* × *blesensis* and *C.* × *Vedasti* have been derived—and hence it is now known as *Lælio-Cattleya* × *Leeana* (Rolfe in "Orch. Rev." 1901, p. 311).

Cattleya × *Whitei* (Rchb. f. *l.c.* 1882, ii. p. 586) was said to have been found by Mr. White, a collector for Messrs. Hugh Low & Co., growing on a tree in company with *C. labiata* and *C. Schilleriana*, which were suspected to be the parents. Mr. Boxall afterwards told me that it was *C. Warneri* and *C. Schilleriana* with which it was found, and the hybrid has since been raised artificially from these two species (see "Orch. Rev." 1899, p. 292).

Lælia × *amanda* (Rchb. f. *l.c.* 1882, ii. p. 776) flowered in the establishment of Mr. W. Bull, of Chelsea, and was described as "no doubt a hybrid, to judge from the very unequal pollinia," and the author further remarked: "There is no difficulty in thinking of *Cattleya intermedia* as one parent, and the other might be *Lælia crispa*." Shortly afterwards Reichenbach suggested *L. Boothiana* as the second parent, as appears from an extract from a letter to Mr. Day, who made a painting of the original plant ("Orch. Draw." xxxviii. t. 11), after it was sold to W. E. Brymer, Esq., of Dorchester. It was also figured in the *Orchid Album* (iii. t. 135) from the same source. There are seedlings at Kew from this cross which should ultimately settle the point.

In 1883 two further additions were made, the first being *Lælia* × *Crawshayana* (Rchb. f. *l.c.* 1883, i. p. 142), which flowered in the collection of de Barri Crawshay, Esq., Rosefield, Sevenoaks. Reichenbach suggested that *Lælia albida* and *L. autumnalis* were the parents, but remarked that the possessor preferred to think of *L. albida* and *L. anceps*. The former parentage is now believed to be correct.

Cattleya × *Brymeriana* (Rchb. f. *l.c.* 1883 ii. p. 492) was also discovered by Mr. White, Messrs. Low's collector, who compared it to a short bulbed *Lælia elegans*. Reichenbach pointed out its resemblances to *C. superba* and *C. Eldorado*, and suggested that it might be a hybrid.

A year later he expressed great satisfaction in finding that the discoverer suspected that the plant was a natural hybrid between the species named (*l.c.* 1884, ii. p. 520), a view now universally accepted.

Cattleya × *intricata* appeared in 1884, in the establishment of Messrs. Hugh Low & Co. (Rchb. f. in "Gard. Chron." 1884, ii. p. 7), and was described as "one of those dreadful uniques, the pride of collectors, the dread of poor botanists who have to name them," and, after pointing out its characters, he added: "I cannot but endorse Mr. S. Low's views that it has the strains of *Cattleya intermedia* and *Lælia elegans*." Five years later it was sent to Kew from the collection of H. Little, Esq., of Twickenham, and an examination convinced me that the parents were *C. intermedia* and *C. Leopoldii*, from which it has since been raised by Messrs. Sander ("Orch. Rev." 1897, p. 169).

Cattleya × *Hardyana* was first recorded in 1884, though without a name ("Gard. Chron." 1884, ii. p. 211). It was described as "an extraordinary variety, evidently a natural hybrid between *C. aurea* and a

Fig. 51.—Cattleya × Hardyana. (*Orchid Review.*)

variety of *C. Gigas*," which was then in flower in the collection of George Hardy, Esq., of Timperley, Cheshire. A year later the name appeared, when it was said to have been purchased as *C. Sanderiana*, and that it flowered for the first time in 1883. Numerous other plants have since appeared, and it has also been raised artificially, first in the collection of N. C. Cookson, Esq., Oakwood, Wylam-on-Tyne, from *C. Dowiana aurea* and *C. Warscewiczii*—of which *C. Gigas* and *C. Sanderiana* are both forms ("Orch. Rev." 1896, p. 298). (Fig. 51.)

Cattleya × *resplendens* appeared in 1885 (Rchb. f. *l.c.* 1885, i. p. 692), in the establishment of Messrs. Hugh Low & Co. Reichenbach remarked: "Take a *Cattleya granulosa* and give it flowers of *C. Schilleriana*, the long sepals being placed as in the first, then you have this plant. . . . Mr. S. Low and his staff think it a mule between *C. granulosa* and *C. Schilleriana*, and they may most probably be right." What is believed to be identical has since been raised by Messrs. Peeters of Brussels from these two species ("Orch. Rev." 1900, p. 328).

Cattleya × *Lucieniana* (Rchb. f. *l.c.* 1885, ii. p. 456) flowered with Messrs. Linden, of Brussels, and was said to have the bulbs and leaves of *Cattleya Harrisoniana* and a flower much like that of *C. Isabella*, Rchb. f., but much darker and richer in colour. Reichenbach added: "There can be no doubt its parents are *Cattleya Forbesii* and *guttata* or *granulosa*." I have, unfortunately, not seen it, but what is suspected to be the same thing has since appeared, both with *C. Harrisoniana* ("Orch. Rev." 1903, p. 282) and *C. Schilleriana* (*l.c.* 1906, p. 208), and an artificial hybrid between these two species, recently exhibited by Messrs. Charlesworth, is supposed to represent the same hybrid (*l.c.* 1906, p. 251).

Cattleya × *Scita* (Rchb. f. *l.c.* 1885, ii. p. 489) was imported by Mr. B. S. Williams, "amidst a mass of *C. intermedia*," but the flowers were said to be "quite of the shape of the large variety of *C. guttata*." After describing the colour the author remarked: "There can scarcely be a doubt about its origin." Some time ago I found a fine painting among Mr. Day's "Orchid Drawings" (xlvii. t. 9), with the remark: "It seems to me that it may be a hybrid between *C. intermedia* and *C. amethystoglossa*," but the latter species has, unfortunately, been confused with *C. porphyroglossa*, Rchb. f., and the figure shows clearly that this was the second parent, as I have already pointed out ("Orch. Rev." 1903 p. 254).

Lælia × *porphyritis* (Rchb. f. *l.c.* 1886, i. p. 73) is a Brazilian plant which appeared in the collection of J. Day, Esq., of Tottenham. Reichenbach remarked that the pollinia led him to suspect it was "a hybrid between a *Lælia* and a *Cattleya*. The *Lælia*," he added, "may be *pumila*, but what is the *Cattleya*? Mr. Day thinks it may have been *C. Dormaniana*, which may be a mule itself." On referring to Mr. Day's painting ("Orch. Draw." xlv. t. 27) I find the record: "This plant was sent to me from Nova Friburgo, in Brazil, last year, amongst a batch of *Lælia Dayana* . . . I believe it to be a natural hybrid between *Lælia Dayana* and *Lælia Dormaniana*." The latter is a true *Cattleya* and I believe this parentage is correct. The plant is now known as *Lælio-Cattleya* × *porphyritis*, Rolfe.

Cattleya × *Measuresii* (Rchb. f. *l.c.* 1886, ii. p. 526) was imported by Messrs. Sander, and flowered in the collection of R. H. Measures, Esq., of Streatham. Reichenbach described it as a new natural hybrid, and compared it with *C. Aclandiæ*, which he thought was one parent, and added: "Mr. J. O'Brien thinks of *C. Walkeriana*." It has since been completely lost sight of.

Lælia × *lilacina* ("Gard. Chron." 1886, i. p. 342) was exhibited at a meeting of the Royal Horticultural Society, on March 9, 1886, by F. A. Philbrick, Esq., of Oldfield, as a supposed natural hybrid between *L. crispa* and *L. Perrinii*, collected in 1883, and was further supposed to be a form of *L.* × *Pilcheri*, raised from the same two species by Mr. Dominy, though differing somewhat in the lip.

Cattleya × *Sororia* (Rchb. f. in "Gard. Chron." 1887, i. p. 40) was introduced by Mr. B. S. Williams, who remarked that it had the growth of *C. bicolor*. Reichenbach compared the flower with *C. Harrisoniana* and *C. Walkeriana*, and remarked that he could not help thinking of the latter and then of *C. guttata* as the parents. Shortly afterwards it was figured in the "Orchid Album" (vii. t. 307), when it was said to have been imported with *C. bicolor*, *C. velutina*, and others of similar growth. It is now considered that *C. bicolor* and *C. Harrisoniana* are the parents, and the question is whether it is distinct from the earlier *C. Wilsoniana*, Rchb. f., which is known only from description.

Cattleya × *Dukeana*, Rchb. f., also appeared in 1887 (*l.c.* 1887, i. p. 576) in the collection of Dr. Duke, of Lewisham. It was bought among a bundle of *C. Leopoldii*, which was believed to be one of the parents. The old pseudobulbs were compared with *C. bicolor*, and as the side lobes of the lip were a third shorter than the column it is believed that *C. bicolor* was the second parent. I only know it from description.

The second part of Messrs. Veitch's "Manual of Orchidaceous Plants" appeared in 1887, and here (p. 74) I find a suggestion that *Lælia* × *Lindleyana* (*Cattleya Lindleyana*, Rchb. f.) is a natural hybrid: "The unequal pollinia suggest a hybrid origin, in which *Cattleya intermedia* may have participated." Two years later I inquired, "What would happen if Mr. Seden, or some of his enterprising co-workers, were to hybridise *Cattleya intermedia* with *Brassavola tuberculata*? . . . Would the hybridist be surprised if the so-called *Cattleya Lindleyana* appeared? . . . I think it most probable that such would be the end of the experiment. There has always been something mysterious about the plant, and more than once I have looked at dried specimens and drawings, and thought of *Brassavola*." I then showed that it was intermediate between the two species named, which grow together in the province of Santa Catherina, South Brazil, and named it *Brasso-Cattleya* × *Lindleyana* (Rolfe in "Gard. Chron." 1889, i. p. 437).

Lælia × *pachystele* (Rchb. f. *l.c.* 1888, ii. p. 596) was imported by Mr. Fred. Horsman, of Colchester, and flowered with R. H. Measures, Esq., of Streatham. Reichenbach described it as a "n. hyb. nat. ?" and remarked: "The flowers are equal to those of a rather good *Lælia elegans*.' It has since been considered as a form of the latter.

Four others were recorded during 1888, the first being *Lælia* × *Gouldiana* (Rchb. f. *l.c.* 1888, i. p. 41), which was introduced by Messrs. Sander. Reichenbach compared it with *Lælia* × *Crawshayana*, and remarked: "If it is a hybrid . . . you must think of *L. autumnalis* and perhaps of *L. anceps* as possible parents, by reason of the ground colours." Repeated comparison serves to confirm this view of its origin.

I am inclined to think that this hybrid originally appeared many years earlier, though I have not found a record. At all events there is a painting in Mr. Day's collection ("Orch. Draw." ix. t. 32), which bears the name of *Lælia furfuracea splendens*, and was drawn on February 10, 1868. Mr. Day remarked that the plant had longer bulbs than *L. furfuracea*, also longer leaves, of which there were always a pair to each

FIG. 52.—(1) LÆLIA × FINCKENIANA; (2) L. GOULDIANA; (3) L. ANCEPS SANDERIANA. (*Orchid Review.*)

bulb, and he added: "I am inclined to think this must be a hybrid between *L. furfuracea* and *L. autumnalis*, or perhaps *L. anceps*." There is only a single flower, but Mr. Day has added: "It is well represented by Mr. Durham in vol. vii. page 1" (this referring to the series now in the collection of Jeremiah Colman, Esq., of Gatton Park). It is remarkably like *L.* × *Gouldiana* (fig. 52, 2) in shape and markings, but is shown rather darker in colour. Its history is thus recorded: "This superb variety was obtained from Mr. Bassett, in 1863. Mr. Rucker has the variety, but I think it does not exist in any other collection. Since Mr. Bassett has given up his collection I have obtained his remaining plant. In January and February 1868 the three flowered superbly, lasting six or seven weeks in perfection. Upon one plant were two spikes of five flowers each. The colour is quite as dark as here represented." Is the variety still in existence?

Lælia × *Eyermaniana* (Rchb. f. *l.c.* 1888, ii. p. 91) appeared shortly afterwards, also with Messrs. Sander, who suggested *L. majalis* and *L. autumnalis* as the parents, remarking that the latter was seen in the same place. It is now believed that *L. majalis* and *L. albida* are the parents.

Cattleya × *Krameriana* (Rchb. f. *l.c.* 1888, ii. p. 323) is said to have been imported by M. Franz Kramer, gardener to Herr Rucker-Jenisch, of Kleinflotbeck, Hamburg, as a hybrid between *C. intermedia* and *C. Forbesii*, whose characters it clearly combines. It was afterwards raised by Messrs. Sander from these two species ("Orch. Rev." 1893, p. 2). It is now considered synonymous with the earlier *C.* × *Isabella*, Rchb. f., described as a species in 1859. *C.* × *Krameriana* seems to have been the last natural hybrid of this group described by Reichenbach.

Cattleya × *Patrocinii*, St. Leger, was the next addition to the list. It was described in a Brazilian newspaper, the "Citade de Rio," on May 28, 1890, as a natural hybrid between *C. Loddigesii* and *C. guttata leopardina*. Soon afterwards plants clearly having the same origin flowered with M. F. Kramer, at Hamburg, and with M. Peeters, at Brussels ("Orch. Rev." 1893, p. 343). In 1897 an artificial hybrid from the same two species, raised by M. Ch. Maron, flowered in the establishment of M. Fournier, of Marseilles, and received the name of *C.* × *Gaudii* ("Le Jardin," 1897, p. 310), but it was afterwards identified with the above (Rolfe in "Journ. Roy. Hort. Soc." xxiv. p. 192).

Lælia × *leucoptera* (Rolfe in "Gard. Chron." 1890, i. p. 42, in note) was originally described by Reichenbach as a variety of *L.* × *Crawshayana* ("Gard. Chron." 1884, i. p. 577), when it flowered in the establishment of M. A. A. Peeters, of Brussels, it being thought then that it was derived from *L. albida* and *L. autumnalis*. But on further comparison I came to the conclusion that *L. albida* and *L. furfuracea* were the parents ("Orch. Rev." 1895, p. 46).

In 1891 a handsome addition appeared, being exhibited at a meeting of the Royal Horticultural Society on June 23 by E. Gotto, Esq., The Logs, Hampstead Heath, under the name of *Lælia* × *Gottoiana* ("Gard. Chron." 1891, i. p. 793). It received a First-class Certificate, and was briefly recorded as "an apparent natural hybrid, with *Cattleya labiata* blood in it." I ascertained that it had been imported from Bahia as *Lælia tenebrosa*, but could not find evidence of a *Cattleya* of the *labiata* group being found there. Two years later, a similar plant flowered in the collection of R. H. Measures, Esq., of Streatham, and this was believed to have been imported with *Cattleya Warneri*, which supplied a clue to the missing parent, and the hybrid was described under the name of *Lælio-Cattleya* × *Gottoiana* ("Orch. Rev." 1893, p. 338). The occurrence of this second plant, together with the clearing up of the parentage of *Cattleya* × *Whitei*, supplied a clue to the habitat of *C. Warneri*, which had then "not been divulged," though it had been roughly indicated as some distance south-west of Rio de Janeiro, which is now known to be incorrect (see "Orch. Rev." 1893, p. 328). The origin of *Lælio-Cattleya* × *Gottoiana* was proved by Mr. James Douglas, in 1900, who raised it from the species named (*l.c.* 1900, p. 358).

Lælia × *Finckeniana* appeared in 1892, in the collection of C. W. Fincken, Esq., Hoyland Hall, Barnsley, receiving an Award of Merit from the Royal Horticultural Society on December 13 (fig. 52, 1). It was recorded as a supposed natural hybrid between *Lælia albida* × *anceps Sanderiana ?* ("Gard. Chron." 1892, ii. p. 744) (fig. 52, 3). Shortly afterwards a figure of it appeared ("Orch. Rev." 1894, p. 9, fig. 1), with the statement that it came out of an importation of *Lælia anceps* made by the Liverpool Horticultural Co. about four years previously, and was picked out by Mr. Fincken two years later as a supposed natural hybrid, on account of the distinct appearance of the pseudobulbs and leaves. A second plant afterwards appeared, in the collection of Baron Sir H. Schröder, The Dell, Egham, and was described as var. *Schrœderæ* (O'Brien in "Gard. Chron." 1895, ii. p. 762).

Lælio-Cattleya × *albanensis* (Rolfe in "Orch. Rev." 1893, p. 339) was introduced by Messrs. Sander, St. Albans, from Bahia, and flowered with them in 1893. It was described as evidently a natural hybrid between *Cattleya Warneri* and the true *Lælia grandis*. Shortly afterwards another plant was exhibited by Messrs. Linden, of Brussels, under the name of *L.-C.* × *Stchegoleffiana*, as a supposed natural hybrid between *Lælia tenebrosa* and *Cattleya labiata* ("Orch. Rev." 1894, p. 2). The origin of *L.-C.* × *albanensis* was afterwards proved (*l.c.* 1895, p. 164), a plant raised by Messrs. Linden, Brussels, from *Lælia grandis* crossed with *Cattleya Warneri*, and figured under the name of *L.-C.* × *Varjenevskyana* ("Lindenia," x. t. 466), proving identical.

Cattleya × 'Victoria-Regina' has a very curious history (fig. 53). It was described in 1892 as a new species, imported by Messrs. Sander, St. Albans (O'Brien in "Gard. Chron." 1892, i. p. 586). A large importation of it had previously been distributed, but when the plants flowered most of them proved to belong to *C. Leopoldii*. Two, however, proved different, in the collections of Hamar Bass, Esq., Burton-on-Trent, and W. Thompson, Esq., Stone, and after comparison of flowers and notes I came to the conclusion that these were natural hybrids between the Pernambuco form of *C. Leopoldii* and *C. labiata* ("Orch. Rev." 1894, pp. 7, 293). The cross was then made by Messrs. Sander in order to test the question (*l.c.* p. 327), but we have not heard the result. A figure of Mr. Thompson's plant was afterwards given ("Orch. Rev." 1895, p. 17, fig. 1).

Cattleya × *Claesiana* appeared in the establishment of Messrs. Linden, Brussels, whence it passed into the collection of the Right Hon. J. Chamberlain, M.P., who sent flowers to Kew in May, 1894. It was supposed to be a natural hybrid between *C. intermedia* and *C. Loddigesii*, though the evidence was not quite conclusive. On May 5, 1896, a plant from the collection of the Hon. Walter Rothschild was exhibited at a meeting of the Royal Horticultural Society, under the name of *C. intermedio-Loddigesii*. It was said to have been a natural hybrid received from Rio de Janeiro ("Gard. Chron." 1896, i. p. 593). In 1899 an artificial hybrid from the same two species flowered in the collection of T. L. Mead, Esq., Oviedo, Florida, and proved substantially identical ("Orch. Rev." 1899, p. 72).

Lælio-Cattleya × *Pittiana* (O'Brien in "Gard. Chron." 1894, i. pp. 264, 265, fig. 27) is said to have been imported from the neighbourhood of

FIG. 53.—CATTLEYA × 'VICTORIA-REGINA.' (*Orchid Review.*)

Pernambuco, Brazil, by Messrs. Sander, St. Albans, with whom it flowered in 1894. It was remarked that the parentage was conjectural, but the characters pointed to the old form of *Lælia grandis* and *Cattleya guttata Prinzii* (i.e. *C. amethystoglossa*) as the parents. This would indicate Bahia as a likely habitat. (Fig. 54.)

Cattleya × *venosa* (Rolfe in "Orch. Rev." 1894, p. 132) was introduced by Messrs. Linden, of Brussels, and was described as evidently a natural hybrid between *C. Harrisoniana* and *C. Forbesii*, having the shape and colour of the former, with a lip showing the characteristic markings and verrucose disc of the latter.

In 1895, *Cattleya* × *O'Brieniana* was added to the list of probable natural hybrids, the supposed parents being *C. dolosa* and *C. Loddigesii* (Rolfe in "Orch. Rev." 1895, p. 11). It was originally considered a distinct species ("Gard. Chron." 1889, ii. p. 700) and afterwards a peculiar form of *C. Loddigesii* (Rolfe in "Reichenbachia," ser. 2, i. p. 85, t. 40). In 1895 a plant flowered with M. A. A. Peeters, of Brussels, which had been imported with *C. dolosa* from the province of Minas

FIG. 54.—LÆLIO-CATTLEYA × PITTIANA. (*Gardeners' Chronicle.*)

Geraes, Brazil, and proved identical with Messrs. Sander's original plant. The characters are clearly intermediate between the two parents.

Lælia × *venusta* (Rolfe in "Orch. Rev." 1895, p. 47) originally appeared in the establishment of Messrs. James Backhouse & Sons, of York, in 1884, having flowered, it is said, "last autumn," and was described under the name of *L. autumnalis* var. *venusta* (Goldring in "Garden," 1884, i. p. 366, t. 438). Afterwards a plant appeared with Messrs. Sander, St. Albans, which Reichenbach called *L. autumnalis* var. *xanthotropis* (Rchb. f. in "Reichenbachia," ser. 1, i. p. 21, t. 10), and suggested that it might be a hybrid between *L. autumnalis* and *L. furfuracea.* After consideration of the known facts I came to the conclusion that *L. furfuracea* and *L. majalis* were the parents, and called it *Lælia* × *venusta.* There appears to be another *L. autumnalis* var. *venusta*, which I have not seen (S. Marshall in "Orch. Rev." 1895, p. 107).

In 1896 a natural hybrid between *Cattleya labiata* and *C. granulosa* was recorded, being exhibited by Messrs. Linden, Brussels, at a meeting of the Royal Horticultural Society held on October 27, under the name of *C.* × 'Le Czar' ("Gard. Chron." 1896, ii. pp. 534, 592, 593, fig. 104). It was afterwards figured in "Lindenia" (xii. t. 554), and ultimately described under the name of *C.* × *Imperator* (Rolfe in "Orch. Rev." 1897, p. 365). Other plants have since appeared.

Cattleya × *undulata* (Rolfe in "Orch. Rev." 1897, p. 254) flowered in the collection of Sir Trevor Lawrence, Bart., at Burford, in 1897, and proved so precisely intermediate between *C. Schilleriana* and *C. elongata*, both natives of Bahia, as to be regarded as a natural hybrid between them. The plant is at present unique.

In 1897 *Cattleya* × *hybrida* was added to the list of probable natural hybrids ("Orch. Rev." 1897, p. 333). A *Cattleya* flowered with Mr. William Brooks, Whitecross Nurseries, Weston-super-Mare, which had been purchased with others as *C. Leopoldii.* The batch proved to contain examples of *C. Leopoldii*, the old *C. guttata*, and the one now under consideration, which was believed to agree with the original *C.* × *hybrida* raised by Messrs. Veitch, it is believed, from *C. guttata* and *C. Loddigesii.* The latter is figured in the "Floral Magazine" (1881, t. 473). The parentage of Messrs. Veitch's hybrid has been much confused, and I only know the plant from description and figures. There is also a second *C.* × *hybrida*, derived from *C. Loddigesii* and *C. Aclandiæ*, which has been confused with the original, but is now known as *C.* × *Brabantiæ.*

Cattleya × *picturata* followed in 1898. A plant flowered in the collection of E. F. Clark, Esq., of Teignmouth, which was purchased from the Robinow collection as *C. intermedia.* On flowering it proved more like the old *C. guttata*, and was regarded as a natural hybrid between these two species (Rolfe in "Orch. Rev." 1898, p. 201). *C. picturata*, Rchb. f. ("Gard. Chron." 1877, ii. p. 584) is said to have been raised by Mr. Dominy, for Messrs. Veitch, from *C. guttata* and *C. intermedia*, and so far as can be judged from the description is quite intermediate between them.

Lælio-Cattleya × *Verelii* (Rolfe in "Orch. Rev." 1899, p. 340) flowered in the collection of F. W. Verel, Esq., The Grange, Newlands, near Glasgow, in 1899. It is said to have been purchased two years before as *Lælia* × *amanda.* It was remarked: "*Lælia Boothiana* is evidently one parent of the present hybrid, but the other is as clearly *Cattleya Forbesii*, for the shape of the lip, as well as the characteristic veining, are both stamped upon the hybrid." It now appears that this hybrid appeared as long ago as 1882, in the collection of W. E. Brymer, Esq., of Dorchester, whence it was painted by Mr. Day on December 5 of that year ("Orch. Draw." xxxii. t. 19), the painting showing the same characteristic lip. It had been obtained from Mr. Bull as *Cattleya Rothschildiana*, and differs from the original *Lælia* × *amanda.*

In 1900 *Lælia* × *Cowani*, which had been distributed two years previously, was suggested as possibly a natural hybrid between *L. flava* and *L. harpophylla* ("Orch. Rev." 1900, p. 122), but the evidence is not yet conclusive. It would be interesting to cross the two species together.

Lælio-Cattleya × *Binoti* (Cogn. in "Gard. Chron." 1900, ii. p. 370) flowered in the establishment of M. A. A. Peeters, St. Gilles, Brussels. It was found among a consignment of *Cattleya bicolor* sent from Brazil by M. Binot, and was described as a hybrid between this and some *Lælia*, probably some form of *L. pumila*. Shortly afterwards an artificially raised hybrid, possessing similar characters, flowered in the collection of the Right Hon. J. Chamberlain, M.P. It had been obtained from Messrs. W. L. Lewis & Co., though without this record of parentage ("Orch Rev." 1901, p. 304).

Lælio-Cattleya × *delicata* (Rolfe in "Orch. Rev." 1901, p. 61) flowered two years previously at Kew, having being sent from the collection of the Right Hon. J. Chamberlain, M.P., as *Lælio-Cattleya* × *amanda*, under which name it had been purchased as an imported plant. But it proved different, and was recorded as a natural hybrid between *Lælia crispa* and *Cattleya Forbesii*, whose characters it combines. It has since flowered annually in the collection.

Cattleya × *Dayana* (Rolfe in "Orch. Rev." 1902, p. 292) dates back to 1886, when it flowered in the collection of J. Day, Esq., of Tottenham. It was painted on August 7 ("Orch. Draw." li. t. 35), when Mr. Day remarked: "From a plant of my own, bought of Messrs. J. Veitch & Sons, in flower. I consider it a spotted form of *C. Forbesii*, but have sent a sketch of it to Prof. H. G. Reichenbach." In the index of the volume occurs the note "*Forbesii guttata ?*" indicating a suspicion that it was a hybrid between them; and it is so unmistakably intermediate between the two as to leave no doubt of its descent.

Cattleya × *Pittiæ* appeared in 1895, being exhibited by H. T. Pitt, Esq., Rosslyn, Stamford Hill, at a meeting of the Royal Horticultural Society on May 23, as a natural hybrid between *C. Harrisoniana* and *C. Schilleriana*, and received a First-class Certificate ("Gard. Chron." 1905, i. p. 333). It appears to be a form of the earlier *C. Lucieniana*, Rchb. f.

Cattleya × *Schrœderiana* is a very recent addition to the list (Rolfe in "Orch. Rev." 1905, p. 314). It was described as a species by Reichenbach as long ago as 1883 ("Gard. Chron." 1883, ii. p. 102), when introduced by Messrs. Sander, being afterwards reduced to an anomalous variety of *C. Walkeriana* (Veitch "Man. Orch." ii. p. 50). I have long suspected it to be a natural hybrid, on account of the small side lobes to the lip, and at length made out its parents to be *C. dolosa* and *C. bicolor*. It is said to be a native of the province of Minas Geraes, and I believe came home with *C. dolosa*. (Fig. 55.)

At the same time I indicated *C. eximia* (Rodr. "Gen. et Sp. Orch. Nov." i. p. 70), described as long previously as 1877, from a single individual, as possibly a natural hybrid between *C. bicolor* and *C. Walkeriana*. It was found on the borders of the Rio Parahybo, in the province of Rio de Janeiro.

Two or three suggested natural hybrids have been excluded from the preceding list, namely, *Cattleya velutina*, *C. Dormaniana*, and *Lælia* *harpophylla*, all of Rchb. f., for they appear to be good species. A few suggested secondary hybrids in the *Lælio-Cattleya* × *elegans* group have also been omitted, because the evidence is not conclusive, and authentic specimens are not available. Such hybrids may occur; in fact, Reichenbach, in 1888, suggested that there might be crosses "between mules and

FIG. 55.–CATTLEYA × SCHRŒDERIANA. (*Gardeners' Chronicle.*)

mules, or mules and species" ("Reichenbachia," ser. 1, i. p. 17); but this must be a question for the future. Some of the differences observed may be due to the well-known polymorphism of hybrids.

These forty-four natural hybrids consist of twenty-four *Cattleyas*, eight *Lælias*, eleven *Lælio-Cattleyas*, and one *Brasso-Cattleya*, and their geographical distribution may be expressed as follows (those preceded by an asterisk have also been raised artificially) :—

1. Mexico and Guatemala.

Cattleya × guatemalensis	= C. aurantiaca × C. Skinneri.
Lælia × Crawshayana	= L. albida × L. autumnalis.
Lælia × Eyermaniana	= L. albida × L. grandiflora.
Lælia × Finckeniana	= L. albida × L. anceps.
Lælia × Gouldiana	= L. anceps × L. autumnalis.
Lælia × leucoptera	= L. albida × L. furfuracea.
Lælia × venusta	= L. furfuracea × L. grandiflora.

2. Colombia.

*Cattleya × Hardyana	= C. Dowiana × C. Warscewiczii.

3. Upper Amazon District.

Cattleya × Brymeriana	= C. Eldorado × C. superba.

4. Bahia and Pernambuco.

*Cattleya × Imperator	= C. granulosa × C. labiata.
*Cattleya × Lucieniana	= C. Harrisoniana × C. Schilleriana.
?Cattleya × Measuresii	= C. Aclandiæ × C. Walkeriana.
*Cattleya × resplendens	= C. granulosa × C. Schilleriana.
Cattleya × undulata	= C. elongata × C. Schilleriana.
*Cattleya × Victoria-Regina	= C. labiata × C. Leopoldii.
*Cattleya × Whitei	= C. Schilleriana × C. Warneri.
*Lælio-Cattleya × albanensis	= Cattleya Warneri × Lælia grandis.
*Lælio-Cattleya × Gottoiana	= Cattleya Warneri × Lælia tenebrosa.
Lælio-Cattleya × Pittiana	= Cattleya amethystoglossa × Lælia grandis.

5. Minas Geraes District.

Cattleya × eximia	= C. bicolor × C. Walkeriana.
Cattleya × O'Brieniana	= C. dolosa × C. Loddigesii.
Cattleya × Schrœderiana	= C. bicolor × C. dolosa.
?Lælia × Cowani	= L. flava × L. harpophylla.

6. Rio de Janeiro and adjacent territory.

*Cattleya × Claesiana	= C. intermedia × C. Loddigesii.
Cattleya × Dayana	= C. Forbesii × C. guttata.
Cattleya × Dukeana	= C. bicolor × C. Leopoldii.
*Cattleya × hybrida	= C. guttata × C. Loddigesii.
*Cattleya × Isabella	= C. Forbesii × C. intermedia.
*Cattleya × Patrocinii	= C. Leopoldi × C. Loddigesii.
*Cattleya × picturata	= C. guttata × C. intermedia.
Cattleya × Scita	= C. intermedia × C. porphyroglossa.
Cattleya × venosa	= C. Forbesii × C. Harrisoniana.
Cattleya × Wilsoniana	= C. bicolor × C. Harrisoniana.
*Lælia × Pilcheri	= L. crispa × L. Perrinii.
Lælio-Cattleya × amanda	= Cattleya intermedia × Lælia Boothiana.
*Lælio-Cattleya × Binoti	= Cattleya bicolor × Lælia pumila.
Lælio-Cattleya × delicata	= Cattleya Forbesii × Lælia crispa.
*Lælio-Cattleya × Leeana	= Cattleya Loddigesii × Lælia pumila.
Lælio-Cattleya × porphyritis	= Cattleya Dormaniana × Lælia Dayana.
Lælio-Cattleya × Verelii	= Cattleya Forbesii × Lælia Boothiana.

7. Santa Catherina District.

Brasso-Cattleya × Lindleyana	= Brassavola tuberculata × Cattleya intermedia.
*Cattleya × intricata	= C. intermedia × C. Leopoldii.
Lælio-Cattleya × elegans	= Cattleya Leopoldii × Lælia purpurata.
*Lælio-Cattleya × Schilleriana	= Cattleya intermedia × Lælia purpurata.

Two of the above are preceded by a "?"—*Cattleya* × *Measuresii*, because the supposed parents are not yet known to grow together, while the habitat is unrecorded, and neither specimen nor figure is known; and *Lælia* × *Cowani*, because further evidence of its origin is required. In a few other cases some modification of details may be necessary as additional materials come to hand. In most cases, however, very little doubt remains as to the origin and parentage. Several have already been confirmed by experiment, though in a few cases I have not been able to compare the natural and artificial hybrids together.

The limits of the last four geographical divisions are not well defined, and they might have been treated as subdivisions of one, for a few of the parent species extend to two or more of them. The object, however, is to indicate the species which grow intermixed, and the hybrids which occur with them, respecting which the evidence is at present very imperfect. It is also possible that certain hybrids may occur in more than one division. For example, *Cattleya* × *intricata* is known from the Santa Catherina district, but the above list suggests that it may also occur near Rio de Janeiro, and the remark may possibly apply to others. Some interesting information might be given by collectors as to these points.

Taking now the forty species from which natural hybrids have been recorded, we find that *Cattleya intermedia* comes first, with no fewer than eight to its credit, while *C. bicolor*, *C. Forbesii*, *C. Leopoldii*, and *C. Loddigesii* follow with five each. From *C. Schilleriana* and *Lælia albida* four each are recorded, while *C. guttata*, *C. Harrisoniana*, and *C. Warneri* are each credited with three. Those with two each number thirteen, namely, *C. dolosa*, *C. granulosa*, *C. labiata*, *C. Walkeriana*, *Lælia anceps*, *L. autumnalis*, *L. Boothiana*, *L. crispa*, *L. furfuracea*, *L. grandiflora*, *L. grandis*, *L. pumila*, and *L. purpurata*. Lastly there are seventeen from which only a single natural hybrid has been recorded, namely, *Brassavola tuberculata*, *Cattleya Aclandiæ*, *C. amethystoglossa*, *C. aurantiaca*, *C. Dormaniana*, *C. Dowiana*, *C. Eldorado*, *C. elongata*, *C. porphyroglossa*, *C. Skinneri*, *C. superba*, *C. Warscewiczii*, *Lælia Dayana*, *L. flava*, *L. harpophylla*, *L. Perrinii*, and *L. tenebrosa*.

The species of *Cattleya* and *Lælia* not known to yield hybrids in a wild state are barely three-fourths as numerous, and a good many of these are believed to grow isolated. This is particularly the case with the monophyllous Cattleyas—the *labiata* group—five only out of seventeen being concerned in the production of natural hybrids—so far as our knowledge extends—though of artificial hybrids raised in gardens the name is legion, thus illustrating an opening remark of this paper, that in this group hybridisation is largely a question of opportunity.

We need not pursue the subject further, beyond remarking that the above may not include all the hybrids which occur in a wild state, for the facts already known suggest other possible combinations which have not yet been reported. Horticultural collectors might make some useful contributions to this interesting question, and we may add that hybridists might also make some useful contributions to botanical science by testing some of the combinations above suggested, especially where doubt exists, and thus immortalise their names in the literature of the subject. There are seedlings at Kew, about two years old, which should prove the origin of *Lælio-Cattleya* × *amanda*, their origin being *Lælia Boothiana* ♀ × *Cattleya intermedia* ♂. Several other crosses have been attempted, but without success.

As the literature of the subject is so widely scattered, it may be useful to append a list of references to the records, descriptions, and figures of the hybrids above enumerated, with their synonymy.

ALPHABETICAL ENUMERATION, WITH REFERENCES TO THE PRINCIPAL DESCRIPTIONS AND FIGURES.

1. Brasso-Cattleya × Lindleyana, Rolfe in *Gard. Chron.* 1889, i. p. 437; ii. p. 78.

Cattleya Lindleyana, Rchb. f. in *Berl. Gartenz.* 1857, p. 118; *Bot. Mag.* t. 5449; Day, *Orch. Draw.* xiii. t. 37; xxviii. t. 69.

Bletia Lindleyana, Rchb. f. *Xen. Orch.* ii. pp. 65, 112, t. 135.

Lælia Lindleyana, Nich. *Dict. Gard.* ii. p. 229; Veitch, *Man. Orch.* ii. p. 73; Cogn. in *Dict. Ic. Orch.* Læl. hyb. t. 10 (type); t. 10 A (var. purpurea).

1. Cattleya × Brymeriana, Rchb. f. in *Gard. Chron.* 1883, ii. p. 492; 1884, ii. p. 520; *Orch. Alb.* iv. t. 184; *Lindenia*, viii. t. 343; *Gartenfl.* 1902, p. 617, t. 1505 *Dict. Ic. Orch.* Cat. hyb. t. 1; Day, *Orch. Draw.* xlvi. t. 81.

2. C. × Claesiana, Rolfe in *Orch. Rev.* 1899, p. 72.

C. intermedio-Loddigesii, *Gard. Chron.* 1896, i. p. 593.

3. C. × Dayana, Rolfe in *Orch. Rev.* 1902, p. 292.

C. (unnamed), Day, *Orch. Draw.* li. t. 35.

4. C. × Dukeana, Rchb. f. in *Gard. Chron.* 1887, i. p. 576.

5. C. × eximia, Rodr. *Gen. et Sp. Orch.* nov. i. p. 70; Rolfe in *Orch. Rev.* 1905, p. 315.

6. C. × guatemalensis, *Gard. Chron.* 1861, p. 291; T. Moore in *Fl. Mag.* 1861, t. 61; Rchb. f. in *Gard. Chron.* 1888, ii. p. 378 (var. Wischhuseniana).

Epi-Cattleya guatemalensis, Rolfe in *Gard. Chron.* 1889, i. p. 491; *Orch. Rev.* 1893, p. 134.

7. C. × Hardyana, *Gard. Chron.* 1884, ii. p. 211; 1885, ii. p. 206; *Orch. Alb.* v. t. 231; *Lindenia*, vii. t. 305 (var. laversinensis); viii. t. 353 (var. Gardeniana), t. 373 (var. Statteriana); *Reichenbachia*, ser. 2, ii. p. 15, t. 55; *Orch. Rev.* 1896, p. 241, fig. 13.

C. Massaiana, Will. *Orch. Alb.* viii. t. 362.

C. Oweniana, *Gard. Chron.* 1892, ii. p. 312; *Journ. Hort.* 1892, ii. pp. 240, 241, fig. 33; *Gard. Mag.* 1892, p. 548, with fig.

C. Statteriana, *Journ. Hort.* 1892, ii. pp. 269, 277, fig. 38; *Gard. Chron.* 1892, ii. p. 378.

C. Dowiana aurea Statteriana, Gower in *Orch. Alb.* x. t. 468.

C. Warscewiczii var. 'Countess of Derby,' *Gard. Chron.* 1894, ii. p. 318.

C. Gigas var. 'Countess of Derby,' *Journ. Hort.* 1894, ii. pp. 274, 275, fig. 40.

C. Leopold II., L. Lind. in *Lindenia*, x. t. 479.

C. Warscewiczii var. Leopold II., *Orch. Rev.* 1894, p. 371.

8. C. × hybrida, *Proc. Roy. Hort. Soc.* ii. p. 619; iii. p. 369; *Orch. Rev.* 1897, p. 333; Day, *Orch. Draw.* xlvii. t. 15.

C. (unnamed), *Gard. Chron.* 1859, p. 672; *Proc. Roy. Hort. Soc.* i. p. 70.

C. picta, *Proc. Roy. Hort. Soc.* iii. p. 369.

C. hybrida picta, *Proc. Roy. Hort. Soc.* v. p. 173; *Fl. Mag.* 1881, t. 473.

9. C. × Imperator, Rolfe in *Orch. Rev.* 1897, p. 365; 1898, p. 328; 1903, p. 364; Cogn. in *Dict. Ic. Orch.* Cat. hyb. t. 26.

C. 'Le Czar,' L. Lind. in *Journ. des Orch.* vii. pp. 260, 274, 291; *Gard. Chron.* 1896, ii. pp. 534, 592, 593, fig. 104; *Lindenia*, xii. t. 554.

10. C. × INTRICATA, Rchb. f. in *Gard. Chron.* 1884, ii. p. 7; Rolfe, *l.c.* 1889, ii. p. 38; 1890, i. p. 763; *Orch. Rev.* 1897, p. 169.

C. Rossii, *Gard. Chron.* 1897, i. p. 354; *Orch. Rev.* 1897, p. 191.

11. C. × ISABELLA, Rchb. f. in *Wochenschr.* 1859, p. 336.

C. Krameriana, Rchb. f. in *Gard. Chron.* 1888, ii. p. 323; Rolfe in *Orch. Rev* 1893, p. 2.

C. fimbriata, Bohnh. *Dict. Orch. Hyb.* p. 6; *Orch. Rev.* 1893, p. 325.

C. Louryana, *Gard. Chron.* 1891, i. p. 683; *Orch. Rev.* 1893, p. 325.

12. C. × LUCIENIANA, Rchb. f. in *Gard. Chron.* 1885, ii. p. 456; *Orch. Rev.* 1903, p. 282; 1906, pp. 208, 251.

C. Pittiæ, *Gard. Chron.* 1905, i. p. 333; *Orch. Rev.* 1905, p. 171; *Gard. Mag.* 1905 pp. 515, 517, with fig.

13. C. × MEASURESII, Rchb. f. in *Gard. Chron.* 1886, ii. p. 526.

14. C. × O'BRIENIANA, *Gard. Chron.* 1889, ii. p. 700; 1890, ii. p. 702; *Orch. Rev.* 1895, p. 11; Cogn. in *Dict. Ic. Orch.* Cat. hyb. t. 8.

C. Loddigesii var. O'Brieniana, Rolfe in *Reichenbachia*, ser. 2, i. p. 85, t. 40.

15. C. × PATROCINII, St. Leger in *Citade de Rio*, March 28, 1890; Rolfe in *Orch. Rev.* 1893, p. 343; 1899, p. 254; Cogn. in *Dict. Ic. Orch.* Cat. hyb. t. 6.

C. Gaudii, Maron in *Le Jard.* 1897, p. 310; *Orch. Rev.* 1897, p. 360; *Journ. Roy. Hort. Soc.* xxiv. p. 192.

16. C. × PICTURATA, *Gard. Chron.* 1877, ii. p. 584; Rolfe in *Orch. Rev.* 1898, p. 204.

17. C. × RESPLENDENS, Rchb. f. in *Gard. Chron.* 1885, i. p. 692; Rolfe in *Orch. Rev.* 1900, pp. 296, 328.

C. Kerchoveana, Cogn. in *Chron. Orch.* i. p. 308.

18. C. × SCHRŒDERIANA, Rchb. f. in *Gard. Chron.* 1883, ii. p. 102; 1896, ii. p. 73, fig. 15; Rolfe in *Orch. Rev.* 1905, p. 314.

C. Walkeriana var. Schrœderiana, Veitch, *Man. Orch.* ii. p. 50.

19. C. × SCITA, Rchb. f. in *Gard. Chron.* 1885, ii. p. 489; Rolfe in *Orch. Rev.* 1903, p. 254; Day, *Orch. Draw.* xlvii. t. 9.

C. flaveola, Rchb. f. in *Gard. Chron.* 1888, ii. p. 473.

20. C. × UNDULATA, Rolfe in *Orch. Rev.* 1897, p. 254.

21. C. × VENOSA, Rolfe in *Orch. Rev.* 1894, p. 132.

22. C. × VICTORIA-REGINA, O'Brien in *Gard. Chron.* 1892, i. pp. 586, 808, 809 (*partim*), fig. 115 (excl. fig. 116); *Journ. Hort.* 1892, i. p. 349, fig. 60; *Orch. Rev.* 1893, pp. 9, 13; 1894, pp. 7, 293; 1895, p. 17, fig. 1; Rolfe in *Reichenb.* ser. 2, ii. p. 89, t. 85; Cogn. in *Dict. Ic. Orch.* Cat. hyb. t. 3.

C. × Crethus, *Journ. Hort. Soc. France*, 1901, p. 906.

C. Cogniauxii, Peeters in *Dict. Ic. Orch.* Cat. hyb. t. 22.

23. C. × WHITEI, Low ex Rchb. f. in *Gard. Chron.* 1882, ii. p. 586; *Orch. Alb.* iii. t. 115; *Gartenfl.* 1884, p. 197, t. 1159; Rolfe in *Orch. Rev.* 1899, p. 292; *Bot. Mag.* t. 7727; Cogn. in *Dict. Ic. Orch.* Cat. hyb. t. 20.

C. Russelliana, Mantin, *Rev. Hort.* 1897, p. 353; Rolfe in *Orch. Rev.* 1899, p. 293.

24. C. × WILSONIANA, Rchb. f. in *Gard. Chron.* 1877, ii. p. 72; Rolfe in *Orch. Rev.* 1901, p. 266.

C. Sororia, Rchb. f. in *Gard. Chron.* 1887, i. p. 40; *Orch. Alb.* vii. t. 307; Rolfe in *Orch. Rev.* 1901, p. 267.

C. Krameriana, *Dict. Ic. Orch.* Cat. hyb. t. 32 (non Rchb. f.); Rolfe in *Orch. Rev.* 1905, p. 312.

1. LÆLIA × COWANI, *Orch. Rev.* 1898, p. 376; 1900, pp. 78, 122.

L. flava var. aurantiaca, *Gard. Chron.* 1895, i. p. 468; *Orch. Rev.* 1895, p. 159; Cogn. in *Dict. Ic. Orch.* Læl. t. 19 A.

L. harpophylla var. dulcotensis, *Orch. Rev.* 1900, p. 123.

2. L. × CRAWSHAYANA, Rchb. f. in *Gard. Chron.* 1883, i. p. 142; Rolfe in *Orch. Rev.* 1895, p. 46; Cogn. in *Dict. Ic. Orch.* Læl. hyb. t. 4; Day, *Orch. Draw.* xxxi. t. 63.

3. L. × EYERMANIANA, Rchb. f. in *Gard. Chron.* 1888, ii. pp. 74, 91, 109, fig. 12; Rolfe in *Orch. Rev.* 1895, p. 47.

4. L. × FINCKENIANA, *Gard. Chron.* 1892, ii. p. 744; *Orch. Rev.* 1893, p. 30; 1894, p. 9, fig. 1; 1895, p. 45; O'Brien in *Gard. Chron.* 1895, ii. p. 762 (var. Schrœderæ).

5. L. × GOULDIANA, Rchb. f. in *Gard. Chron.* 1888, i. p. 41; *Orch. Alb.* viii. t. 371; *Reichenb.* ser. 1, ii. p. 23, t. 59; Rolfe in *Orch. Rev.* 1894, pp. 9, 10, fig. 2; 1895, p. 46; Cogn. in *Dict. Ic. Orch.* Læl. hyb. t. 1.

? L. furfuracea var. splendens, Day, *Orch. Draw.* ix. t. 32.

6. L. × LEUCOPTERA, Rolfe in *Gard. Chron.* 1890, i. pp. 42, 256 (in note); *Orch. Rev.* 1895, p. 46; Cogn. in *Dict. Ic. Orch.* Læl. hyb. t. 6.

L. Crawshayana var. leucoptera, Rchb. f. in *Gard. Chron.* 1884, i. p. 577.

7. L. × PILCHERI, D'Ombrain in *Fl. Mag.* 1867, t. 340; *Gard. Chron.* 1867, p. 239.

L. Pilcheriana, Rchb. f. in *Gard. Chron.* 1868, p. 815.

Cattleya Pilcheri, *Gard. Chron.* 1865, p. 222.

L. lilacina, *Gard. Chron.* 1886, i. p. 342.

8. L. × VENUSTA, Rolfe in *Orch. Rev.* 1895, p. 47.

L. autumnalis var. venusta, Goldring in *Garden*, 1884, i. p. 366, t. 438.

L. autumnalis var. xanthotropis, Rchb. f. in *Reichenb.* ser. 1, i. p. 21, t. 10.

1. LÆLIO-CATTLEYA × ALBANENSIS, Rolfe in *Orch. Rev.* 1893, p. 339; 1894, p. 2; 1895, p. 164; *Gard. Chron.* 1893, ii. p. 584; Cogn. in *Dict. Ic. Orch.* L.-C. t. 6.

L.-C. Stchegoleffiana, L. Lind. in *Journ. d. Orch.* iv. p. 297; *Gard. Chron.* 1893, ii. p. 756; *Orch. Rev.* 1894, pp. 2, 31.

L.-C. Varjenevskyana, L. Lind. in *Lind.* x. t. 466.

2. L.-C. × AMANDA, Rolfe in *Gard. Chron.* 1889, i. p. 802; ii. p. 78.

Lælia amanda, Rchb. f. in *Gard. Chron.* 1882, ii. p. 776; *Orch. Alb.* iii. t. 135; Day, *Orch. Draw.* xxxviii. t. 11; Cogn. in *Dict. Ic. Orch.* Læl. hyb. t. 2.

Cattleya Rothschildiana, Hort. ex *Orch. Alb.* iii. sub t. 135.

3. L.-C. × BINOTI, Cogn. in *Gard. Chron.* 1900, ii. p. 370; *Dict. Ic. Orch.* L.-C. t. 30; Rolfe in *Orch. Rev.* 1900, p. 357; 1901, p. 304.

4. L.-C. × DELICATA, Rolfe in *Orch. Rev.* 1901, p. 61.

5. L.-C. × ELEGANS, Rolfe in *Gard. Chron.* 1889, i. p. 619; ii. p. 79; *Reichenb.* ser. 2, p. 43, t. 20; *Lind.* viii. t. 347 (var. Broomeana); *Orch. Rev.* 1893, p. 235.

Cattleya elegans, C. Morr. in *Ann. d. Gand.* iv. p. 93, t. 185; *Bot. Mag.* t. 4700.

Lælia elegans, Rchb. f. in *Allg. Gartenz.* xxiii. p. 242; *Gard. Chron.* 1877, ii. p. 424.

Bletia elegans, Rchb. f. in *Walp. Ann.* vi. p. 427.

Lælia Brysiana, Lem. in *Ill. Hort.* iii. Misc. p. 48; iv. t. 134.

L. gigantea, Warn. ex *Proc. Roy. Hort. Soc.* ii. p. 247; Warn. *Sel. Orch.* i. t. 6.

L. Turneri, Warn. *Sel. Orch.* i. t. 12.

L. pachystele, Rchb. f. in *Gard. Chron.* 1888, ii. p. 596.

6. L.-C. × GOTTOIANA, Rolfe in *Orch. Rev.* 1893, p. 338; 1897, p. 362; 1900, p. 358; *Lind.* xiv. t. 658.

Lælia Gottoiana, *Gard. Chron.* 1891, i. p. 793.

7. L.-C. × LEEANA, Rolfe in *Orch. Rev.* 1901, p. 311.

Lælia Leeana, Rchb. f. in *Gard. Chron.* 1882, i. p. 492.

Cattleya blesensis, *Rev. Hort.* 1893, p. 424, with tab.

Cattleya Vedasti, Perrenoud in *Orchidoph.* 1891, p. 48, with tab.

L.-C. corbeillana, Bohnh. *Dict. Orch. Hyb.* p. 42.

L.-C. corbeillensis, Maron in *Journ. d. Orch.* vii. p. 290; Cogn. in *Dict. Ic. Orch.* L.-C. t. 5.

8. L.-C. × PITTIANA, O'Brien in *Gard. Chron.* 1894, i. pp. 264, 265, fig. 27.

9. L.-C. PORPHYRITIS, Rolfe in *Gard. Chron.* 1889, ii. p. 155.

Lælia porphyritis, Rchb. f. in *Gard. Chron.* 1886, i. p. 73; *Rev. Hort. Belge*, 1888, p. 37, with tab.; Kerch. *Livre d. Orch.* t. 21; Day, *Orch. Draw.* xlvii. t. 27.

10. L.-C. × SCHILLERIANA, Rolfe in *Gard. Chron.* 1889, ii. p. 155; *Orch. Rev.* 1893, p. 237; 1898, p. 168.

Lælia Schilleriana, Rchb. f. in. *Allg. Gartenz.* xxiii. (1855), p. 322.

Bletia Schilleriana, Rchb. f. in *Walp. Ann.* vi. p. 424.

Lælia Stelzneriana, Rchb. f. in *Hamb. Gartenz.* 1860, pp. 282, 420; *Fl. d. Ser.* t. 1494.

Lælia irrorata, Rchb. f. in *Hamb. Gartenz.* 1859, p. 57.

Lælia euspatha, Rchb. f. in *Hamb. Gartenz.* 1860, p. 420; *Reichenb.* ser. 1, i. p. 17, t. 8.

Lælia elegans Wolstenholmiæ, Rchb. f. in *Gard. Chron.* 1865, p. 698; Warn. *Sel. Orch.* ii. t. 29; *Orch. Alb.* vi. t. 285.

Lælia elegans alba, Burbidge in *Garden*, 1880, i. p. 132, t. 218; *Orch. Alb.* i. t. 30.

Lælia Warneri, Warn. *Sel. Orch.* iii. t. 1.

Lælia Measuresiana, Will. *Orch. Gr. Man.* ed. 6, p. 636; *Orch. Alb.* v. t. 207.

Bletia irrorata, Rchb. f. in *Walp. Ann.* vi. p. 426.

11. L.-C. × VERELII, Rolfe in *Orch. Rev.* 1899, p. 340.

Cattleya Rothschildiana, Day, *Orch. Draw.* xxxii. t. 19.

HYBRID ODONTOGLOSSA.

By DE BARRI CRAWSHAY, F.R.H.S.

WHEN I decided to read a paper at this Conference I thought an exhaustive one, to date, would be better than "a few remarks." Upon mature consideration, I think our knowledge has hardly arrived at a pitch high enough to make a paper really exhaustive; therefore in dealing with this most intricate and interesting subject, I propose to give what I term a synopsis and "a few remarks" upon some of the most easily understood results that have been achieved.

The evidence we have to deal with is in some cases questionable rather than positive, as in our earlier days we *Odontoglossum* raisers crossed all and sundry things, and in only some of the earlier hybrids did we produce anything equal to, or in advance of, Nature's creations; in the later stages, and quite recently, the ratio of success becomes increasingly greater. Perhaps it may be questioned whether our hybrids are in advance of Nature's, but I think after calm deliberation it will be agreed that we have already attained this result.

To prophesy, unless you know, is highly dangerous, but I do not fear to state my conviction that in the future we shall produce very many hybrids far in advance of even our present standard, by utilising what, for some unknown reason, Nature has deprived herself of the opportunity. (Perhaps she did so to avoid the confusion which we are now making—viz. the creation of hybrids to the exclusion of species, except in their finest forms.)

What I mean is, of course, patent to all; it is, selecting and crossing the finest forms of species whose habitats are such distances apart that they cannot cross each other, and then applying the same process to the resulting hybrids *inter se.*

Form, colour, and size will be the chief points in the hybrid of the future as regards its bloom, combined with a good constitution as regards its vegetative organs.

If anyone does commence breeding *Odontoglossa* now (and I sincerely hope this paper may be the means of inciting some to do so) I entreat them to use only good and fine parents, for we can get enough rubbish from the importations. Rather than produce poor results, produce none at all.

NATURAL HYBRIDS.

I shall but make a passing reference to these, as it is quite impossible to state that many of them are hybrids in the proper sense of the word; that is, the result of the union of two pure species. So many of them must be only crosses of a hybrid with a species, chance crossing having produced such numbers of bad varieties, even worse than the poor forms of the species. This remark applies to all the hybrids and crosses of

FIG. 56.—ODONTOGLOSSUM × AMABILE 'IXION.'

Flowers of a delicate blush-white, sepals bearing a fine marking of reddish-rose, and the middle of the petals occupied by clusters of blotches of the same colour, a band of irregular markings encircling those in the centre; lip white, with reddish markings around the crest.

O. crispum, *O. gloriosum*, *O. luteo-purpureum*, and *O. Lindleyanum*, which all grow together, and, aided by insects, make a grand confusion even under Nature's laws.

Mr. Rolfe would like all these natural hybrids proved. I admit it would be interesting, but we raisers all feel so sure of their parentage that we think life too short to do so, as we fear the bad forms we should produce would hardly repay the details of the proof, but could we produce fine forms of

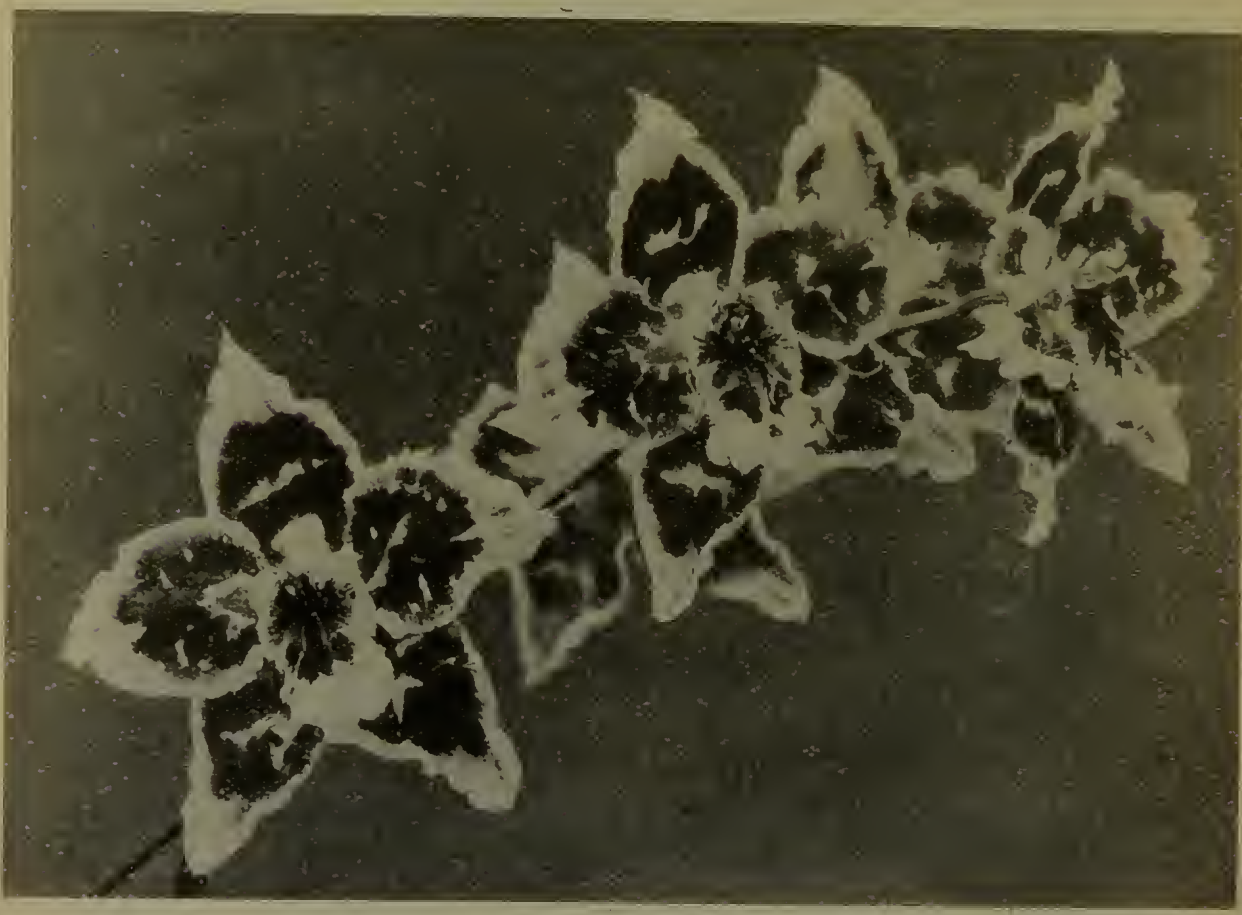

FIG. 57.—O. ARDENTISSIMUM. (*Orchid Review.*)
White; all blotching lilac purplish-brown.

them in quantity, perhaps our estimate of life would lengthen. Some of them have been proved, a list of which I append :

Wilckeanum	crispum × luteo-purpureum	Leroy at Baron E. de Rothschild's	May 1890
		Vuylsteke	May 1902
excellens	nobile × triumphans	Seden for Veitch	May 1891
loochristiense	crispum × triumphans	Vuylsteke	Jan. 1898
Wattianum	Lindleyanum × Harryanum	Crawshay	Sep. 1900
crispodinei (fig. 72)	crispum × Coradinei	Crawshay	Nov. 1904
bellemense	crispum × loochristiense	Fanyau	May 1905
Fascinator (fig. 66)	crispum × Adrianæ	Charlesworth	May 1905
Adriano-triumphans	Adrianæ × triumphans	Charlesworth	Feb. 1906
Stewartianum (fig. 87)	Andersonianum × crispum	Stewart	Mar. 1906

It may be objected to that I have placed the last five in "Natural hybrids," but they are manifestly equivalent to them, being possible in

FIG. 58.—ODONTOGLOSSUM × CRISPO-HARRYANUM 'ROSSLYN VARIETY.'

Sepals pale greenish-yellow; petals white, with brown-purplish blotches, the tips of both sepals and petals being pale violet-purple; lip white, with violet-purple blotches on the basal half; crest yellow.

their habitats, but that fact does not detract from their raiser's credit. 'Denisoniæ' I do not consider a true hybrid, but a "cross," as from the

Fig. 59.—O. formosum. (*Orchid Review.*)
White, faintly shaded rose-purple, spotting lilac-purple.

published illustration of it I feel sure it is the result of *crispum* × *Wilckeanum*, and therefore a reversion towards the stronger species from the weaker hybrid ("Floral Magazine," pl. 26).

Garden Hybrids.

List completed to November 27, 1906.

amabile (fig. 56)	crispum × crispo-Harryanum	Vuylsteke
amœnum	nobile × sceptrum	Vuylsteke
amandum	nobile × Wilckeanum	Vuylsteke
ardentissimum* (fig. 57)	nobile × crispum	Vuylsteke
ashlandense	Kegeljani × Adrianæ	R. Ashworth
bellatulum	crispum × tripudians	Vuylsteke
blando-nobile	blandum × nobile	de Lairesse
Bradshawiæ	Andersonianum × Harryanum	Bradshaw

* In 1899 M. Jacob for Baron E. de Rothschild at Armainvilliers, Paris, raised a hybrid between *crispum* and *nobile* (*O.R.* vol. vii. 1899, p. 65) which was apparently lost, for I have never met anyone who has seen it. It has not been shown in England, nor do I know anyone who has ever seen a bloom. Records in those early days were kept so carelessly that it almost now seems a myth, and this hybrid is so well known under the name of *ardentissimum* that it seems futile to adhere in this case to priority of name; for it almost appears to be a name only, owing to absence of records or exhibition. The reference given does not mention any spotting, and states that the flower was most like *nobile*.

Brandtiæ	luteo-purpureum × nobile	Charlesworth
Crawshayanum	Hallii × Harryanum	Crawshay
crispo-Harryanum (fig. 58)	crispum × Harryanum	Vuylstcke
Euphrosyne	sceptrum × crispo-Harryanum	Peeters
Eurydice	cirrhosum × hastilabium	Charlesworth
eximium	ardentissimum × crispum	Vuylsteke
formosum (fig. 59)	Rolfeæ × nobile	Vuylstcke
Fowlerianum (fig. 60)	Rossii × cirrhosum	Sander
fuscum	sceptrum × Wilckeanum	Vuylstekc
Fletcherianum	Edwardii × cirrhosum	Sander
Gladys	cirrhosum × crispo-Harryanum	Charlesworth
Hallio-Adrianæ	Hallii × Adrianæ	Charlesworth
Hallio-crispum	Hallii × crispum	Cookson
Hallio-xanthum	Hallii × Kegeljani	Crawshay
Juno	sceptrum × cirrhosum	Thompson
Lairessei (fig. 85)	Edwardii × Cervantesii	de Lairesse
Lambeauianum (fig. 61)	Rolfeæ × crispum	Peeters
lapidense	Hallii × Rolfeæ	Thompson
Lawrenceanum (fig. 62)	triumphans × Rolfeæ	Vuylsteke
mirificum	crispum × sceptrum	Vuylsteke
nitidum	crispo-Harryanum × Wilckeanum	Vuylsteke
Ossulstoni (fig. 63)	crispo-Harryanum × nobile	Charlesworth
Othello (fig. 65)	Harryanum × Adrianæ	Charlesworth
percultum (fig. 67)	Rolfeæ × ardentissimum	Vuylsteke
Pescatoreo cordatum	nobile × cordatum	Gardner
Phœbe (fig. 64)	cirrhosum × crispum	Charlesworth
Queen Alexandra (fig. 68)	Harryanum × triumphans	Charlesworth
Rolfeæ (fig. 69)	nobile × Harryanum	Vuylsteke
Smithii (fig. 70)	Rossii × crispo-Harryanum	Charlesworth
Souvenir de Victor Hye de Crom (fig. 71)	luteo-purpureum × Harryanum	Hye de Crom
Stella	sceptrum × triumphans	Thompson
Terpsichore	nobile × Kegeljani	Crawshay
Thompsonianum (fig. 73)	Edwardii × crispum	Thompson
Una	crispum × nevadense	Crawshay
Urania	crispum × cristatellum	Crawshay
venustulum	crispo-Harryanum × ardentissimum	Vuylstcke
Vuylstekeæ (fig. 74)	Vuylstekei × crispo-Harryanum	Vuylsteke
Vuylstekei (fig. 75)	loochristiense × Wilckeanum	Vuylsteke
waltoniense	crispum × Kegeljani	Thompson
warnhamense	nobile Hallii	Lucas
Wiganianum	Rolfeæ × loochristiense	Vuylsteke
unnamed	Vuylstekei × ardentissimum	Vuylstcke

This gives us fifty garden hybrids, four natural hybrids, and five secondary crosses, making a total of fifty-nine raised *Odontoglossa* known to me as having bloomed. Respecting *Thompsonianum* I think it would be unfair were I to pass over the name of the actual raiser of this remarkable hybrid, Mr. Rappart, of Liscard, Cheshire, from whom Mr. W. Thompson acquired the two plants.

The above lists give England the honour of introducing twenty-six of the fifty garden hybrids, M. Ch. Vuylsteke leading with nineteen, having bloomed *loochristiense*, his first published hybrid (O.R. vol. vi. 1898, p. 41), in January 1898. Mr. Charlesworth stands second with nine, his first entry into public notoriety as to hybrid *Odontoglossa* being made at R.H.S., February 11, 1902, with *Hallio-crispum heatonense*.

Now, as to those equal to, and in advance of, Nature. I consider the subjoined list will hardly be disputable :

amabile	Fletcherianum	percultum	*Thompsonianum*
ardentissimum	Fowlerianum	*Queen Alexandra*	*Vuylstekeæ*
Bradshawiæ	*Lambeauianum*	*Rolfeæ*	Vuylstekei
Crawshayanum			
crispo-Harryanum	*Lawrenceanum*	Souvenir de Victor Hye de Crom	waltoniense
eximium	Ossulstoni		
		Smithii	Wiganianum

giving twenty-one out of fifty, the nine in italics being all grand introductions. This result has been attained in the sixteen years since

FIG. 60.—ODONTOGLOSSUM FOWLERIANUM. (*Gardeners' Chronicle.*)
Ground colour rosy-violet, spotting of a blackish-purple shade ; lip violet-purple.

the original garden hybrid bloomed, but the whole of the above twenty-one have appeared in the last eight years, *crispo-Harryanum* being the first of them to bloom.

Analysing these twenty-one, I find the parents used are :—

			Kegeljani 1
crispum 7	nobile 3	Edwardii 2	loochristiense 2
Harryanum 6	triumphans 2	Rossii 2	luteo-purpureum 1
crispo-Harryanum 4	ardentissimum 2	Andersonianum 1	Vuylstekei 1
Rolfeæ 4	cirrhosum 2	Hallii 1	Wilckeanum 1

proving *crispum* and *Harryanum* to be the best species for founding the new races.

At present, the secondary hybrids, or rather crosses, are hardly of sufficient quantity to speak upon with any great amount of certainty; but an analysis of them gives a good result, thus:

ardentissimum ...	× crispo-Harryanum	= venustulum
	× crispum	= *eximium*
	× Rolfeæ	= *percultum*
crispo-Harryanum	× ardentissimum	= venustulum
	× cirrhosum	= *Fowlerianum*
	× crispum	= *amabile*
	× nobile	= *Ossulstoni*
	× Rossii	= *Smithii*
	× Vuylstekei	= *Vuylstekeæ*
	× Wilckeanum	= nitidum
Rolfeæ	× ardentissimum	= *percultum*
	× crispum	= *Lambeauianum*
	× Hallii	= lapidense
	× loochristiense	= *Wiganianum*
	× nobile	= formosum
	× triumphans	= *Lawrenceanum*
Vuylstekei	× crispo-Harryanum	= *Vuylstekeæ*

giving fifteen crosses, twelve of which (those in italics) are among the twenty-one finest.

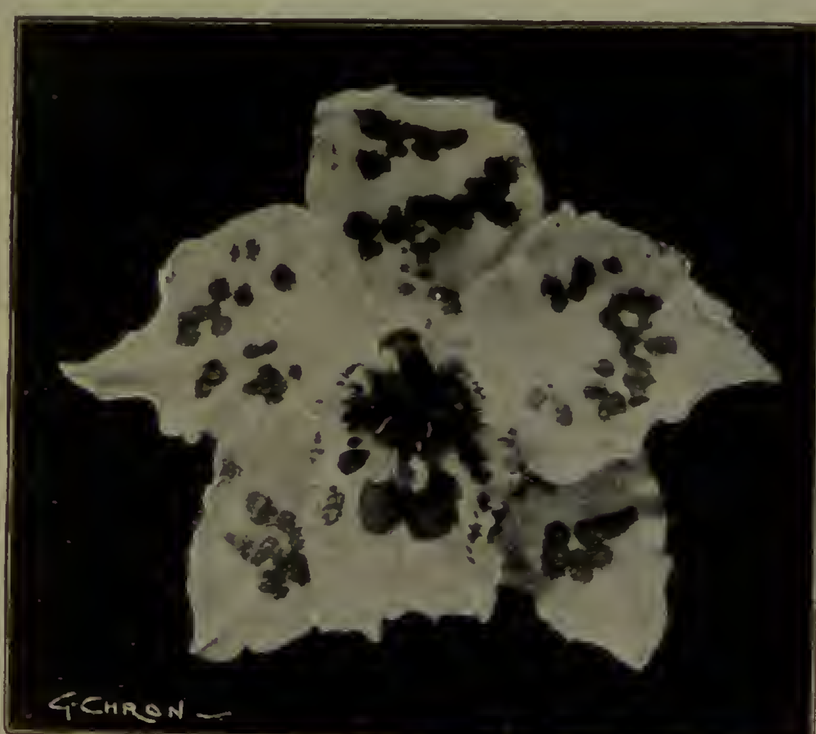

FIG. 61.—ODONTOGLOSSUM LAMBEAUIANUM. (*Gardeners' Chronicle.*)
White; spotting rich deep brown.

Should this remarkable rate of progression continue, the future has the promise of many wonderful things, but there is one great factor to obviate, viz. the deterioration consequent upon too much in-breeding. I advisedly say "too much," for though we talk of fixing characters, I fear the opposing influence of reversion in these "hybrids" will, unless we continually introduce new blood by the aid of fine varieties of so-called true species, cause some of us to be severely disappointed.

I think the above analyses fairly prove my earlier statement, that the raisers of *Odontoglossa* have produced many equal to and in advance of the natural hybrids.

Hybrids More Vigorous than Species.

Now observe some peculiar and good results of hybridity. Generally speaking, the hybrids are far stronger in constitution and easier to grow than the imported species, and in some cases their growth is equal to that

Fig. 62.—Odontoglossum Lawrenceanum. (*Gardeners' Chronicle.*)
Sepals and petals coppery-brown, with purplish overlaid markings; lip yellow, with violet-purple basal spotting.

of both parents: *crispo-Harryanum* as an example; a finer constitution can hardly be desired.

In colour, we have obtained a most beautiful violet in *O. Smithii*, by the action of the rose in *Rossii rubescens* upon the lilacine suffusion of *crispo-Harryanum* derived primarily from *Harryanum*. Consider the next step in advance with *Smithii* and *Cochlioda Noezliana*, which, though it will not be a true *Odontoglossum*, will be near enough for practical purposes, as in time this red blood will permeate through all the true *Odontoglossa*, and produce a race of plants culminating in the "Scarlet crispum." I feel sure this is but a question of time.

FIG. 63.—ODONTOGLOSSUM × OSSULSTONI 'W. H. HATCHER.'
White sepals and petals tinged with rose and heavily blotched and banded with red purple; front of lip white; crest yellow.

In *O. Wiganianum* (regarding whose parentage there have been different opinions, Mr. Rolfe thinking it *Rolfeæ* × *excellens* or *loochristiense*, as opposed to the R.H.S. Committee, who considered it was *Rolfeæ* × *Wilckeanum*, but upon further experience of several examples of this hybrid, and from the fact that later plants have the distinct, rather raucous sweet smell of *triumphans*, and no evidence of *luteo-purpureum*, I now fully agree with Mr. Rolfe that its parentage is *Rolfeæ* × *loochristiense*), see the abnormal increase of the *Harryanum* lip, larger sometimes upon little seedlings than upon fully grown Harryanums.

FIG. 64.—O. PHŒBE.
FIG. 65.—O. OTHELLO. FIG. 66.— O. FASCINATOR.
(*Orchid Review.*)

In *O. Ossulstoni* 'W. H. Hatcher,' a small seedling blooming for the first time, consider the remarkable depth of colour produced by the union of the lilac spots of the *nobile* (var. *Charlesworthii*) with the blotching of *Rolfeæ*.

Again, the gorgeous colour of *O. Vuylstekeæ* from the union of the crimson-brown of *Vuylstekei* and the purplish tint of *crispo-Harryanum*, and lastly the extraordinary depth of crimson-maroon in *O. Thompsoni-*

anum, created by the mingling of the violet-purple of *Edwardii*, with a lilac-rosy *crispum*.

We must carefully study these colour changes, try to advance upon them, and add form and size to those hybrids yet lacking in these elements of beauty, and here I may state that *form must be pre-eminent*.

Blotched Crispums are Hybrids.

I must now refer to this phase of my subject. With most people, they pass as "sports," or variations of the species, but how can *crispum*

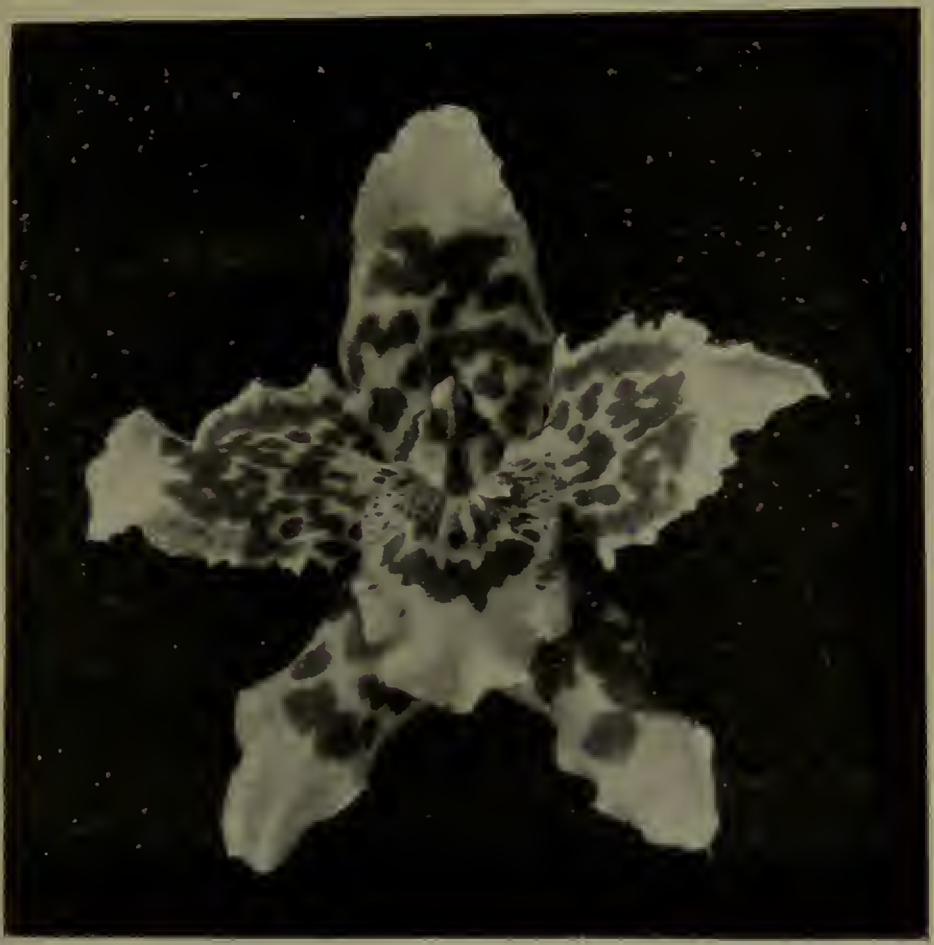

Fig. 67.—O. perculTUM 'Cybele.' (*Orchid Review*.)
White; markings of rich rosy purple-brown.

as a species be all and everything that people fancy? We know that it grows in company with many other species, therefore it is only reasonable to suppose that hybrids and crosses have been made, during times unknown, by thousands, owing to the erratic manner in which an insect searches for honey; Evolution and Reversion going on together, totally disregarded by "the bees."

No doubt it is not a primary hybrid, that having been proved by all those yet raised, but of very complex parentage; we all hope some day it may be proved, for to all *Odontoglossum* raisers it is the acme of their cult; and whoever first succeeds in raising blotched crispums in numbers and of good quality will have earned the gratitude and hatred of the owners of

Fig. 68.—Odontoglossum × Queen Alexandra 'Carmen.'
Sepals and petals yellow, heavily blotched with chestnut; lip white, marked with violet-chestnut; crest yellow.

Odontoglossa, according to the way each looks upon the matter, viz. scientifically or commercially.

At present, the proofs are extremely meagre ; for I consider that one plant out of a pod hardly constitutes much success in raising blotched crispums.

FIG. 69.—ODONTOGLOSSUM × ROLFEÆ MAJESTICUM.
Flowers white, profusely marked with purple ; crest yellow.

The results known to me consist of the following :—

Crawshay.	June 1900.	(*a*)
,,	March 1903.	(*b*)
Warburton.	November 1903.	(*c*)

Thwaites.	May 1904.	(*d*)
Thompson.	November 1904.	(*e*)
Charlesworth.	October 1905.	(*f*)
Vuylsteke.	June 1906.	(*g*)
R. Ashworth.	August 1906.	(*h*)

(*a*) This plant, the first known raised *crispum*, I did not exhibit till October 1903, hardly thinking the poor result possible, till it was

Fig. 70.—O. Smithii. (*Orchid Review.*)
Sepals and petals violet, with white ground under blackish-violet purple spotting; lip violet.

corroborated by my second success (!) in (*b*). The parents of (*a*) were a good *crispum* × *Wilckeanum*; result, absolute loss of all spotting and yellow ground, with the exception of a shade of that colour on the bases of the sepals.

(*b*) Parentage, good *crispum* × *crispum Crawshayanum*; result, the worst form possible of *crispum* and entire loss of the immense blotches of the male parent. (Figs. 76, 77.)

(*c*) Several seedlings were raised, but as far as I know only one, 'Vine House Seedling' (fig. 78), being blotched, all the others being unspotted or having small spots in sepals only; parentage being *crispum roseum* × *crispum Lindeni.*

FIG. 71.—ODONTOGLOSSUM × 'SOUVENIR DE VICTOR HYE DE CROM.'
Sepals and petals yellow, heavily barred with dark chestnut; lip white, with paler chestnut markings at the base; crest yellow.

(*d*) In this we have a most interesting result, one seedling between a fine *crispum Bonnyanum* × good blotched form on all segments; the seedling is almost identical with its mother both in spotting and form. (Figs. 79, 80, 81.)

(e) Here we have a like result to (c) between *crispum* 'Queen Empress' and *crispum* 'Victoria Regina' (the former a grand *roseum*, the latter a good spotted form); one well blotched seedling, 'William Stevens,' the others all reverted. (Fig. 82.)

(f) Again very similar results; *crispum roseum* × *c. heliotropium*, produces rosy ground and a few small spots, no heavily marked ones.

(g) *crispum* × *crispum*, both blotched forms: nothing has yet appeared herefrom worth calling attention to; a similar result to all the previous ones.

(h) *crispum* × *c.* 'Rossendale,' one unspotted seedling.

These eight results, all unsatisfactory from a commercial point of view, distinctly prove reversion, and blotched crispums have at present

Fi 72.—O. CRISPODINEI.
White; blotches brown.

FIG. 73.—O. THOMPSONIANUM.
Intense crimson-violet purple with rosy-lilac margins.

(*Orchid Review.*)

hardly been raised. Parallel evidence is abundant in *O. ardentissimum*, scores of which are unspotted and of bad form, infinitely worse than either parent. *O. eximium* also proves the same, even though it is the result of a fine *ardentissimum* crossed by a fine blotched *crispum.*

It will be extremely interesting to see the results from such crosses as *crispum Graireanum* × *Ashworthianum*, *Cooksoniæ* × *Luciani*, and other heavily blotched and coloured varieties; if these produce unspotted forms (as I am firmly convinced they will) then reversion needs no further proof, and blotched crispums will still remain a difficult commodity to produce in quantity.

Colour Arrangement.

A rather unexpected result of colour arrangement was proved by the appearance of *Odontioda Vuylstekeæ.* (Fig. 83.) Instead of the red of *Cochlioda Noezliana* mingling equally with the white of *Odontoglossum nobile* and producing an intermediate self-coloured flower (as many experts thought it would), it separated more or less and formed a basal area upon a larger white surface, on all sepals and petals, of deep vermilion, somewhat shaded by the rose in *O. nobile*, the remainder of the segments being quite a light rosy-purple. This semblance of a great blotch led many

Fig. 74.—Odontoglossum Vuylstekeæ. (*Orchid Review.*)
Ground colour white; overlaid with crimson-purple.

to consider *O. ardentissimum* the parent, and not *O. nobile*, but we have only to look at *Odontoglossum Lairessei* (*Edwardii* × *Cervantesii*) (figs. 84, 85) to see exactly similar conditions of dissociation and re-arrangement of the colours; the result being that the deep violet-purple of *Edwardii* has been relegated to a similar basal area of violet-purple, the remainder being a palish pink. It may here be contended that theory is at fault because in *O. Cervantesii* there is a basal area of spots wherein to gather the violet-purple, which is not the case in *O. nobile*; but this can be proved to be non-efficient, for, apparently, spotting of the other parent has no influence upon the violet-purple of *O. Edwardii*, as witness *O. Fletcherianum* (*Edwardii* × *cirrhosum*), in which the colour is solid and uninfluenced by the spotting of *cirrhosum.*

For the arrangement of colours in hybrids from parents of widely different characteristics, we must look to Nature's orderly mode of attracting the eyes of the fertilising insects, for though we attempt to raise what we expect or want, plant life and creation will always remain

FIG. 75.—ODONTOGLOSSUM × VUYLSTEKEI VIVICANS.

Pale yellow sepals and petals, heavily marked with chestnut-red; and white lip, with one large and some smaller chestnut blotches.

obedient to Nature's laws, even though we lead them afar from the path originally laid out for them. "So far and no further," we must occasionally bear in mind, or we may have some grave disappointments; but even with this in contemplation the *Odontoglossum* raiser will

FIG. 76.—O. SEEDLING (CRISPUM × CRISPUM CRAWSHAYANUM). (*Orchid Review*, 1905, 112.) White.

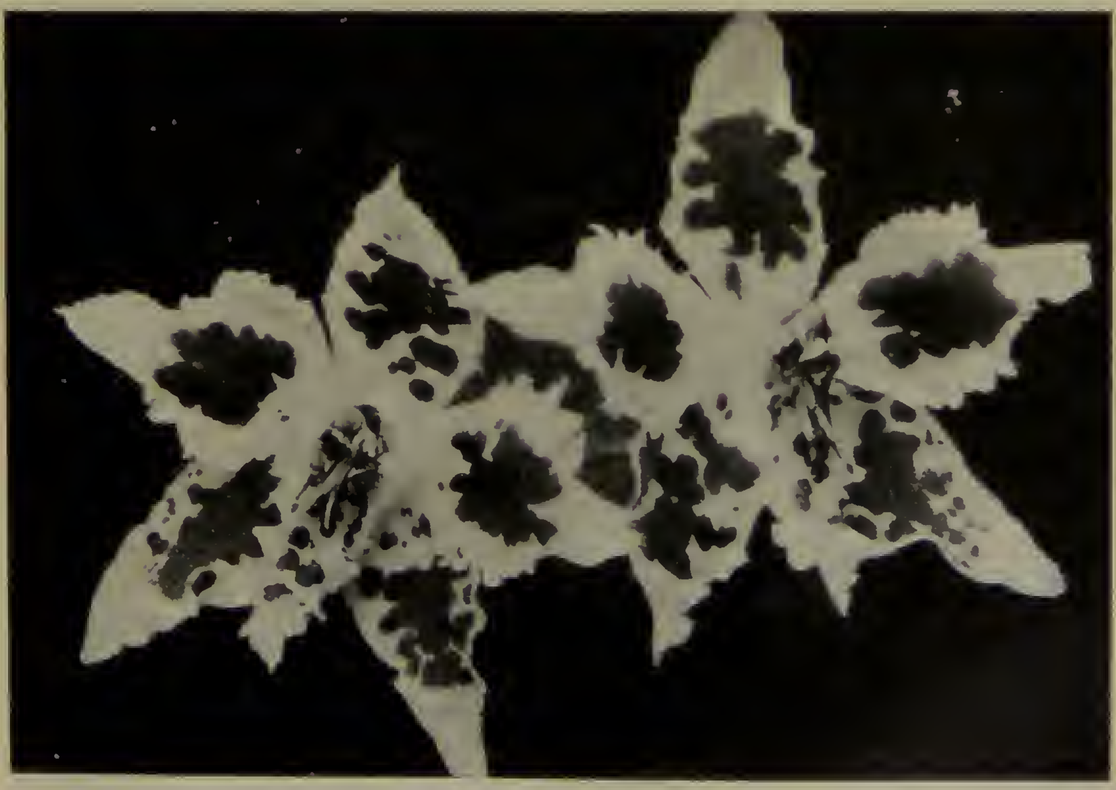

FIG. 77.—O. CRISPUM CRAWSHAYANUM. (*Orchid Review*, 1905, 113.) White; all markings rich deep crimson-brown.

Fig. 78.—O. crispum 'Vine House Seedling.' (*Orchid Review*, 1905, 145.)

Fig. 79.—O. c. Bonnyanum ♀. Fig. 80. O. crispum ♂.
Fig. 81.—O. crispum Seedling.
(*Orchid Review*, 1905, 33.)

produce more wonderful results, not possibly contemplated by Nature. Foremost among these will be a new race of 'Red Odontoglossa,' by crossing *Odontioda* with *Cochlioda*, selecting the largest and best forms for such a purpose. To this it may be objected that the recross will tend to reduce the size of the *Odontioda* × *Cochlioda*; but even if it does so in the majority, there will always be appearing among them giants, such as have already appeared in *Hallio-crispum* 'Theodora'

FIG. 82.—O. CRISPUM 'WILLIAM STEVENS.' (*Orchid Review*, 1905, 48.)

(fig. 86), and *amabile* 'Royal Sovereign' (fig. 88), these two being greatly in excess of the whole of the remainder of the plants of the same parentage.

REVERSION.

This is the most potent adverse factor we have to deal with and guard against; it will always trend towards the most powerful parent in the hybrid's genealogy. Each batch of seedlings raised from two species shows this, and each lot of plants, the result of more complex and hybrid parentage, proves it even more clearly, and from continued study of the matter, I think the aboriginal forms of all our so-called species were self-coloured flowers.

What process Nature employed to produce those species which are now parti- or multi-coloured I think only conjecturable; but we may assume with a fair amount of certainty that it was by hybridity, and by fixing, in very long process of time, certain characters suitable to certain conditions of the plants' habitats.

The "Latent Germ."

This will be no new subject to students of Darwin,* who will immediately see a clue herein that may be a closely related quality to reversion,

Fig. 83.—Odontioda Vuylstekeæ. (*Orchid Review.*)
Bloom 1904. Plant 1906.

but it may be a new idea to some hybridists who are not acquainted with its principles. To point my meaning, I cannot do better than relate the world-known results of the attempt to produce a hybrid between a nearly thoroughbred Arabian chestnut mare and a Quagga stallion. The hybrid was successfully obtained and the parents never re-mated, but two subsequent foals bred by that mare to black Arabians bore more or less the stripes and other marks of the Quagga, which were so evident that

* *Variation of Animals and Plants*, vol. i. ch. xi. p. 518. (Murray, 1905.)

the foals were not pure horses. Darwin elucidated this matter and laid down the principle that the entire system of the female is impregnated with the male element, and that the whole of the matter thus created is not consumed by the resulting offspring; hence there were marks of the Quagga upon the foals bred subsequently to a horse.

By analogy it is easy to see that a plant's system is influenced by a first crossing, and in a subsequent different cross may produce offspring bearing the impress of the earlier male parent.

Objection may be raised that this theory cannot apply to a plant, whose structure is totally different from that of an animal, as the former loses by dehiscence and separation the entire generative organs needed to produce seed, whereas the latter does not; but who can prove that the

FIG. 84. O. CERVANTESII. FIG. 85.—O. LAIRESSEI.

(*Orchid Review.*)

entire system of the plant is not affected or impregnated by the male element exactly in the same way as in the animal?

It is a fact that many natural hybrids do bear evidence of some points of more than two parent species in their habit, either floral or vegetative; therefore I consider we have here a possible clue which we may investigate, and thus discover something. With this object in view, I have made totally different crosses upon the same plant, a list of some of which I append.

Coradinei ×	triumphans	July 1901
	Harryanum	July 1902
	crispum (blotched var.)	November 1904
Pescatorei ×	Hallii	March 1901
	luteo-purpureum Vuylstekeanum	May 1903
sceptrum ×	Harryanum	July 1902
	nobile Veitchianum	January 1904

If any evidence of the earlier crosses appears in the later ones, then satisfactory proof will have been made regarding this theory, but the strongest proof would lie in such a case as the following:

nobile album × { Cochlioda Noezliana 1906
crispum xanthotes 1907

FIG. 86.—ODONTOGLOSSUM × HALLIO-CRISPUM 'THEODORA.'

Primrose-yellow, prettily spotted with red-brown. This flower's natural size was $4\frac{1}{4}$ inches across as figured; extreme length of lip, $1\frac{7}{8}$; breadth, $1\frac{1}{8}$; petals, $1\frac{1}{2}$.

If any red appeared in the second seedlings, then the most sceptical could not ask for further confirmation. I hope someone will be able and willing to investigate this, and in due time publish the records and communicate with me.

There is another manner, in which the theory that the male element pervades the female plant's system may be collaterally proved, and that is by a plant carrying two pods at once, crossed by distinct pollen, such as *crispum* × *Harryanum* and *luteo-purpureum*. If any evidence of *Harryanum* appeared in the "luteo" seedlings or *vice versâ*, it would be distinctly proved that the male element did pervade the whole plant. I have many crosses, made simultaneously, which will throw much light on this.

Fig. 87. O. Stewartianum. (*Orchid Review.*)

Between "Reversion" and "Latent Germ" I think we have enough to account for the variation of *Odontoglossum* hybrids, and need not be at all astonished at surprising results. (Perhaps this has already accounted for some seedlings which have not had the faintest resemblance to the "parentage guaranteed" that is so often given in the Orchid world.) Professor Dr. Tschermak stated in his paper at this Conference that hybridity was responsible for new and unexpected variations. I think it highly probable that in this action of the "Latent Germ" we have an explanation of his statement, with which I fully agree.

Conclusion.

At the present time we have only scratched the surface of this subject, both theoretically and practically; but looking at the number of investigators and raisers now at work, among whom the following are found—R. Ashworth, Bird, Bradshaw, Brooman-White, Bull, Cookson, Charlesworth, Lucas, MacBean, Pitt, Potter, Rochford, Sander, Stewart, Earl Tankerville, Thompson and Thwaites in Great Britain ; Hye de Crom, Lairesse, Pauwels, Peeters, Vuylsteke in Belgium, and Fanyau in France —I consider our knowledge will be increased a hundredfold in a few years more, compared with what we know to-day, and that the future in this case will not be "very much like the past," as was once laid down, as a proverb or "morale," by that great Frenchwoman, Mme. de Staël.

I here gratefully acknowledge the finishing touch given by the illustrations to this paper, and tender my thanks to the Council of the R.H.S. for the preparation of the blocks, made from the Society's paintings, and to the editors of "The Orchid Review" and "The Gardeners' Chronicle" for the use of their figures.

References.

In giving these by means of dates of original appearance, it will make them perhaps clearer than usual, as a complex lot of references overburdens a paper, so often heavy in itself. In some cases the plants have as yet made no public appearance. In the column "Figured," where possible, I give the reference to a photographic reproduction. In some instances, the figure is not of the original plant, but a later shown variety.

GARDEN HYBRIDS.

Name of Plant.	*Original Appearance.*	*Figured.*
amabile	R.H.S. March 3, 1904	G.C. June 3, 1905
amœnum	Temple, May 26, 1903	O.R. 1904, 201
amandum	Düsseldorf, May 1, 1904	
ardentissimum	Temple, May 28, 1902	O.R. 1902, 209
ashlandense	,, May 29, 1906	
bellatulum	O.R. 1902, 99	O.R. 1904, 80
blando-nobile	R.H.S. Feb. 14, 1905	
Bradshawiæ	,, Jan. 27, 1903	G.C. Feb. 2, 1903, 82
Brandtiæ	,, May 9, 1905	
Crawshayanum	,, July 16, 1901	G.C. July 27, 1901
crispo-Harryanum	Temple, May 25, 1898	G.M. June 4, 1898
Euphrosyne	Bloomed Dec. 1905 Published G.C. Oct. 6, 1906, 239	
Eurydice	Holland House, July 10, 1906	O.R. 1906, 265
eximium *	Temple, May 29, 1906	O.R. 1906, 240
formosum	Düsseldorf, May 1, 1904	O.R. 1905, 297
Fowlerianum	R.H.S. March 3, 1906	G.C. Mar. 17, 1906, 16

* Mr. Rolfe relegated this to a variety of *ardentissimum*; but M. Vuylsteke had previously sent me, in April 1906, blooms of other plants of this hybrid, which proved their parentage quite conclusively, and all were quite distinct from it. Some of the forms are certainly somewhat like it, and to anyone who did not know the parentage might pass for it.

Name of Plant.	*Original Appearance.*	*Figured.*
fuscum	Düsseldorf, May 1, 1904	
Fletcherianum	R.H.S. July 31, 1906	
Gladys	„ March 6, 1906	
Hallio-Adrianæ	„ Feb. 13, 1906	
Hallio-crispum	„ Nov. 24, 1896	
Hallio-xanthum	Bloomed Feb. 1906	
Juno	R.H.S. Sept. 15, 1903	
Lairessei	„ March 28, 1905	O.R. 1905, 81
Lambeauianum	Liège, May 8, 1905	G.C. Nov. 4, 1905, 324
lapidense	M.O.S. June 22, 1905	
Lawrenceanum	R.H.S. March 14, 1905	G.C. April 1, 1905, 197
mirificum	Ghent, April 18, 1903	O.R. 1904, 81
nitidum	Temple, May 31, 1904	O.R. 1904, 201
Ossulstoni	O.R. June 1905, 189	G.M. 1906, 418
Othello	Temple, May 30, 1905	O.R. 1905, 201
percultum	„ May 31, 1904	O.R. 1905, 273
Pescatoreo-cordatum	P. & M. Sale Room, Jan. 17, 1902	
Phœbe	Temple, May 30, 1905	O.R. 1905, 201
Queen Alexandra	„ May 28, 1902	G.C. July 7, 1906, 13
Rolfeæ	O.R. 1898, 270	J. of H. 1900, 475
Smithii	R.H.S. Dec. 5, 1905	O.R. 1906, 9
Souvenir de Victor Hye de Crom	Temple, May 23, 1900	G.M. 1900, 332
Stella	R.H.S. April 25, 1905	
Terpsichore	„ April 3, 1906	
Thompsonianum	„ April 25, 1905	O.R. 1905, 241
Una	„ Oct. 9, 1906	
Urania	„ Oct. 23, 1906	
venustulum	Temple, May 31, 1904	O.R. 1904, 201
Vuylstekeæ	R.H.S. Nov. 7, 1905	O.R. 1905, 361
Vuylstekei	O.R. 1902, 99	G.C. 1904, 3
waltoniense	R.H.S. Jan. 13, 1903	O.R. 1903, 49
warnhamense	„ April 25, 1905	
Wiganianum	„ April 11, 1905	G.C. May 6, 1905, 274
unnamed	Sent me Feb. 7, 1906. Not yet named by the raiser	

PROVED NATURAL HYBRIDS.

Adriano-triumphans	R.H.S. Feb. 13, 1906	
crispodinei (fig. 72)	„ June 20, 1905	O.R. 1905, 241
excellens	Temple, May 29, 1891	
Fascinator (fig. 66)	R.H.S. May 5, 1905	O.R. 1905, 201
hellemense	O.R. 1905, 176	
loochristiense	O.R. 1898, 41	
Stewartianum (fig. 87)	O.R. 1906, 121	O.R. 1906, 121
Wattianum	R.H.S. Nov. 11, 1900	G.M. Oct. 13, 1900
Wilckeanum	Temple, May 28, 1902	G.M. 1902, 347

Publications referred to herein: "Orchid Review," "Gardeners' Chronicle," "Gardeners' Magazine," "Journal of Horticulture."

ANALYTICAL CHART.

Secondary crosses containing only two species in their parentage, the result of a hybrid crossed with or upon one of its parent species :

crispum × Lindleyanum
Coradinei × crispum
crispodinei

crispum × gloriosum
Andersonianum × crispum
Stewartianum

crispum × triumphans
loochristiense × crispum
hellemense

crispum × luteo-purpureum
Wilckeanum × crispum
Denisoniæ

crispum × Hunnewellianum
Adrianæ × crispum
Fascinator

crispum × Harryanum
crispo-Harryanum × crispum
amabile

nobile × crispum
ardentissimum × crispum
eximium

nobile × Harryanum
Rolfeæ × nobile
formosum

Secondary crosses containing three species, the hybrid crossed with a species :

crispum × luteo-purpureum
Wilckeanum × sceptrum
fuscum

crispum × luteo-purpureum
Wilckeanum × nobile
amandum

crispum × Hunnewellianum
Adrianæ × Hallii
Hallio-Adrianæ

crispum × Hunnewellianum
Adrianæ × Harryanum
Othello

crispum × Hunnewellianum
Adrianæ × triumphans
Adrianæ-triumphans

crispum × Harryanum
crispo-Harryanum × cirrhosum
Gladys

crispum × Harryanum
crispo-Harryanum × nobile
Ossulstoni

crispum × Harryanum
crispo-Harryanum × sceptrum
Euphrosyne

crispum × Harryanum
crispo-Harryanum × Rossii
Smithii

crispum × gloriosum
Andersonianum × Harryanum
Bradshawiæ

nobile × Harryanum
Rolfeæ × crispum
Lambeauianum

nobile × Harryanum
Rolfeæ × Hallii
lapidense

nobile × Harryanum
Rolfeæ × triumphans
Lawrenceanum

Kegeljani × cristatum
cristatellum × crispum
Urania

Secondary crosses, the result of two hybrids, and containing three or four species in their parentage :

crispum × Harryanum nobile × crispum
crispo-Harryanum × ardentissimum
venustulum

crispum × luteo-purpureum crispum × Harryanum
Wilckeanum × crispo-Harryanum
nitidum

nobile × Harryanum nobile × crispum
Rolfeæ × ardentissimum
percultum

nobile × Harryanum crispum × triumphans
Rolfeæ × loochristiense
Wiganianum

Tertiary crosses, the result of two hybrids, and containing four species and three hybrids in their parentage:

crispum × luteo-purpureum   crispum × triumphans
Wilckeanum × loochristiense
Vuylstekei

crispum × luteo-purpureum
Wilckeanum × loochristiense   crispum × Harryanum
Vuylstekei × crispo-Harryanum
Vuylstekeæ

crispum × luteo-purpureum   triumphans × crispum
Wilckeanum × loochristiense   nobile × crispum
Vuylstekei × ardentissimum
unnamed

ADDENDUM.

Considering the future will so closely knit together *Odontoglossum*, *Cochlioda*, and *Oncidium*, I think it well to add the particulars of these bi-generic hybrids up to the present date (November 27, 1906).

Name of Plant.	*Original Appearance.*	*Figured.*
Odontioda Vuylstekeæ	Temple, May 31, 1904	O.R. July 1904, 208.
,, heatonensis	R.H.S. March 6, 1906	
,, Bohnhoffiæ	,, Sept. 25, 1906	
Odontonia Lairesseæ	,, June 20, 1905	O.R. July 1905, 217

Odontioda Vuylstekeæ	O. nobile × C. Noezliana	Vuylsteke
,, heatonensis	O. cirrhosum × C. sanguinea	Charlesworth
,, Bohnhoffiæ	O. cirrhosum × C. vulcanica	Charlesworth
Odontonia Lairesseæ	O. crispum × Miltonia Warscewiczii	de Lairesse

Fig. 88.—Odontoglossum amabile 'Royal Sovereign.' (*Gardeners' Chronicle.*)

FERN BREEDING.

By Chas. T. Druery, V.M.H., F.L.S.

It is a somewhat curious fact that, despite the recognised value of ferns as decorative foliage plants, the great bulk of forms accepted as improvements of exotic species have originated as chance seedlings, while few or no attempts are made to arrive at such improvements by systematic selective culture. This is doubtless mainly due to the great difference existing between spore-bearing and seed-bearing plants in their mode of reproduction. In the seed-bearer it is, as it were, an open-and-above-board operation, easily comprehensible, and, as a rule, easily controlled. There is the visible pollen and the equally visible stigma and attached seed-vessel, and with usually simple precautions against chance fertilisation, it is easy to bring together the potencies of two different strains, while Nature rewards the matrimonial agent very frequently by combining these potencies into fresh harmonies on novel and valuable lines. With the fern, however, it is otherwise, and the old botanists, baffled in their attempts to solve the mysteries of its reproduction, dubbed it a cryptogam or secret marrier. Later on, but still only about sixty years ago, the mystery was solved, and it was found that ferns produced what are to all intents and purposes flowers of two sexes and seeds which were fertilised by their conjunction and then behaved as seeds do generally, pushing forth roots and leaves in the ordinary way. These flowers, however, were so minute that only a keen eye could detect them, and they were, moreover, produced, not upon the fern fronds which represent the ferns as we know them, but upon the under side of a tiny green *Marchantia*-like scale, produced by the spore, and firmly attached to the soil by a multitude of rootlets.

Furthermore, it was found that, despite the immense diversity of ferns in form, habit, and size, the flowering stage was practically identical throughout, so that it was impossible to identify the fern until after the fertilisation of the seed and the appearance of the fern itself. This difficulty could of course be obviated to some extent by careful collection and isolation of the gathered spores, but with every care it was found that other spores shed from neighbouring ferns upon the fronds selected were almost unexcludable. In time it was demonstrated that practically the only way to hybridise or cross was to sow the two selected kinds somewhat thickly together and trust to chance for cross-fertilisation. Such cross-fertilisation was then accepted as effected, when some of the seedlings unmistakably displayed the joint characters of both forms sown. In this way a number of successful crosses have been effected, and experience has demonstrated that the chances of cross-unions are increased if, as soon as the prothalli or little scales have reached full size, they are flooded with tepid water for a few minutes. The fern-seeds are fertilised by means of antherozoids, tiny ciliated organisms endowed with locomotive power in water, and, strange to say, similar in every appreciable way to the spermatozoids of the animal kingdom. Normally the under side of the scale is bedewed with water, and through this they make their way direct to the embryo seeds situated a fraction of an inch distant, but, naturally, when the pan is flooded at the right time multitudes of these organisms are floated, as it were, broadcast, and thus have a chance of reaching and fertilising other embryo seeds at a distance. We thus see that although it is practically impossible, owing to their minuteness, to transfer the fertilising organisms systematically as we can the visible and tangible pollen grains, there is nevertheless a possibility of forming combinations on selective lines. Selective cultivation of exotic ferns, independently of crossing, has, we think, depended more upon the fact that the best varieties are naturally sown from, and hence improved ones naturally crop up from time to time, than on any systematic lines aiming at definite results, and if we study what may be termed trade ferns, such as are raised by the million for the market, we find the novelties to occur almost entirely among these. The novelties, on the other hand, which appear from time to time among the tenderer and rarer exotics are mostly imported "finds," the histories of which, however, it is usually difficult to ascertain. A curious fact in this connection is seen in the *Nephrolepis* tribe, which up to a very few years ago had not sported to any great extent, when suddenly new types, crested, congested, and plumose, succeeded one another so quickly that now there is a considerable range of forms, especially in *N. exaltata. N. e. Piersonii* and its improved form are evidently on the way to rival some of the best of our plumose forms of *Polystichum angulare* provided their cultivators do not confine themselves to the easy propagation by stolons of which this species permits. By spores alone can any great stride be obtained, and, judging by analogy with *P. angulare*, a single generation might suffice to yield abundance of far superior forms on finer and finer lines of subdivision. Curiously enough I have fronds of *N. exaltata*, given me at least ten years ago, in which most of the pinnæ are bipinnate on *N. Piersonii* lines; but unfortunately my advice then given as to sowing was not followed, and the plant was lost sight of. In this connection, too, I may mention that when *Gymnogramma Laucheana grandiceps* was introduced some thirty years ago, the raiser assured me that a large batch of it, all alike, originated from spores sown from a merely dilated pinnule on an otherwise normal plant, a point which is worth remembering, though the experience of selective raisers of British ferns tends to prove that local variations of this kind are rarely accompanied by a reproductive capacity on like varietal lines. It must, however, be confessed that we are quite in the dark as to the cause of these sports; but, judging by the general evidence connected with wild sports, they must arise from subtle local cell modifications on normal forms, which find expression probably in various ways, sometimes by bulbils or offsets, and sometimes through spores, or the modified sportive cell may even originate in the prothallus either in a fertilising antherozoid or an embryo seed. The theory which has been put forward that they are due to crossing will not hold water, at any rate in the majority of cases, since they so often occur in species which are quite isolated from others of the same genus, and any cross-fertilisation can therefore only occur between normals of the same species. Plant-breeding as applied to ferns has hitherto been almost entirely confined to British ferns, so far as systematic breeding for improved types is concerned. About fifty years ago the extraordinary number of "wild" varieties which were found scattered about in the natural habitats of the forty odd species indigenous to Great Britain led to the formation of a body of specialists who devoted their leisure to hunting for new varieties and also to raising these from their spores on both propagative and selective lines. Some of the results were extraordinary, tripinnate wild varieties of a normally bipinnate type yielding at once crops of even quinquepinnate forms, as in the case of Jones & Fox's plumose *Polystichum angulare*. The writer in his "superbum" strain of *Athyrium filix-fœmina* has been similarly fortunate in that species, acquiring some magnificent plumose varieties, plus a marvellously tasselled section, the second generation of the first sport breaking into two distinct branches.

My object, however, in addressing the International Conference is to indicate the peculiar value of the many beautiful varietal types of the British species as a means of introducing those types into closely allied exotic species. Many of the British types of variation have never so far been found to occur in exotics, and among these there are a number of forms which are not merely extremely curious, but also extremely beautiful. I may instance the Victoriæ type of 'Lady Fern' as one of the uniques, in which the long slender fronds consist of slender pinnæ set on in duplicate at right angles to each other, so that opposite pairs form a cross, and the whole frond a delicate lattice-work. Besides this remarkable feature, each division, and the frond itself, terminates in a long slender, many-stranded tassel. Unfortunately the species of *Athyrium* are few, and several of those which are classed with it are, I fancy, doubtful allies, the species being a botanic puzzle and classed with *Asplenia* and *Polypodia* in a perplexing and, to my mind, unjustifiable way. The possible field of hybridising here is therefore a small one, but it might well be tried by joint sowings on the lines indicated at the outset of my paper. The British shield ferns (*Polystichum*), *Lastreæ* (*Nephrodium*), *Polypodium vulgare*, *Blechnum Spicant*, the hartstongue (*Scolopendrium vulgare*), and the spleenworts, on the other hand, present a mass of varietal material which should be available for a considerable number of closely allied exotic species, and it is worthy of remark that exotic hybrids so obtained would probably, if tender, be considerably hardened by the alliance as well as beautified. A now well-known example of this kind is *Polypodium Schneiderii*, an undoubted cross between *P. vulgare elegantissimum* and *Phlebodium aureum*. This hybrid is nearly hardy and stands with impunity temperatures which kill the exotic parent entirely. There are many exotic species of *Polypodium* which would appear to be much nearer relatives than *P. aureum*, and of which no varieties are known. *P. vulgare* abounds in handsome sports, and although the finest of all, the cambricums or plumosums, are perfectly barren and are not therefore available, there are still enough fine-divided and cristate forms to work upon. *B. Spicant* is in the same position with a multitude of exotic Blechnums and probably the Lomarias also would submit to an alliance, the generic difference being a minor one. So with the *Nephrodium* family and the Asplenia, in which latter connection, though the genus generally appears to consist of very constant species, one of our species, *Asplenium Trichomanes*, has sported freely and prettily, and since difference in size of ferns presents no obstacle to intercrossing, here again is an extensive genus open as a working field. The hartstongue, too, is so closely allied to the spleenworts that already two well-established cases of hybridisation are recorded, and judging by the great similarity of type· the gigantic *Asp. nidus avis* might easily be induced to combine with some of the fertile frilled and tasselled hartstongue, to the hardening of its constitution and the diversification of its form. In this connection it must be borne in mind that combined sowings should only be made with spores of really fine varieties and to some definite end. The *élite* of our British ferns should be chosen as possible mates for fine exotic normals. It is just as easy to start on the "best" lines as on inferior ones. Thoroughly constant and symmetrical varieties should alone be used, and as the spores of different species vary considerably in their time of germination and maturation of the prothallus, some experience would have to be gained in order that the two sowings might be made at such periods that the maturation of both should be approximately simultaneous. In such cases, of course, the first sowing of the slower germinator would require to be a thin one. The spores should always be sown on soil carefully sterilised with boiling water, and after sowing the pot or pan should be continually covered with a piece of glass or a bell to exclude the subsequent deposit of stray spores or inimical germs ; particular care must also be taken to exclude worms, since a single worm, even a small one, can ruin a crop by disturbance. As an alternative to successive sowings, it has been suggested, and indeed we believe *Polypodium Schneiderii* was acquired in this way, that the sowings be made in separate pans, and mature prothalli picked out from each and inserted side by side, touching each other, in separate pairs. A point in favour of this mode is that spores, even of one and the same species or off the same frond, vary in speed of development, some maturing much more rapidly than others, so that there is often a succession of young plants for a considerable period; and another point is that, assuming careful sowing and fairly pure culture, the resulting couple of prothalli are bound to be of the two kinds, and in this way it might be even possible to determine the maternal or paternal relations of the offspring, which by joint sowing is impossible. The maternal parent is of course the fern-producing prothallus, while the paternal one would be the other, always assuming that the offspring proves to be a hybrid ; if not, it must be assumed that no cross has taken place, but that the two elements of the same prothallus combined. The risk of antecedent fertilisation cannot well be prevented, as the antheridia, or male cists containing the antherozoids, are usually scattered too much over the under side of the prothallus to be eliminated by any process of division such as has been suggested. It may be mentioned here that the prothallus is extremely tenacious of life and bears shifting, and even division, with impunity under congenial conditions of growth. So much for cross-fertilisation and the field open for its exercise in conjunction with the rich material the British species have supplied, and are supplying, since fresh "finds" are continually cropping up.

Now for a word of advocacy with our friends abroad as regards these British varieties as being themselves worthy of attention. Here and there I have heard of collections on the Continent, but they are very rare, and my point is this: all of the British species are natives of other countries, and some of them are almost ubiquitous, especially *Pteris aquilina*, which, by the way, I might have cited also as good material, for we have some grand varieties of it. Wherever then the same species are indigenous, these far more beautiful varieties are obviously open to cultivation. The United States, Japan, the European continent, would all be the richer by the acquisition of some of the best of our British forms, which would then not merely brighten their collections, but at the same time provide the needful spore material for the crossing and hybridising which my paper is intended to advocate.

In connection with our Colonies I may mention that in the spring of this year fronds and spores of a splendid, thoroughly bipinnate form of *Blechnum Spicant* were sent me by the finder, Mr. Geo. Fraser, of Ucluelet, British Columbia. This was found by him on Vancouver Island, and surpasses any variety of the type so far found here. I have sown this form and anticipate interesting results; for it is worthy of remark that whenever a fern sports in the direction of extra division it is extremely apt to develop this character to a greater degree in its progeny, so that with selective cultivation greater and greater dissection, and consequent beauty, may be arrived at. That the faculty of wide variation is not peculiar to Britain is also evidenced by the receipt from the United States of a fine polydactylous form of *Lastrea Thelypteris*, which in this country has afforded no "sports" at all so far. I therefore strongly advise similar careful search elsewhere for such varietal wild forms, since it is invariably from them that we obtain new types and fresh material for both selective cultivation and the hybridisation which I advocate.

THE IMPORTANCE OF HYBRIDISATION IN THE STUDY OF DESCENT.

By Dr. E. Tschermak, of the Hochschule für Bodencultur, Vienna.

Up to a few years ago it was the opinion of scientists that hybridisation was of no great consequence in the production of new forms or in the study of evolution. Practical breeders, on the other hand, had long learnt to regard artificial crossing as a means which in certain cases could produce apparently new forms. In any case, no sort of general regularity seemed to obtain. So as to secure the commercial benefits of lucky accidents, the cloak of secrecy was even thrown by many breeders over the origin of their new products. Consequently, even now many forms are wrongly described as hybrid; and on the other hand many novelties, the origin of which is not given, may be referred with certainty or probability to an intentional or an unintentional cross.

The knowledge that artificial hybrids between species possess very slight fertility, and that their produce may eventually often throw back to one or other of the parent forms, had formerly a noticeable effect in depreciating the value of hybridisation. It is true that Gärtner, Charles Darwin, and Focke recognised that apparently new characteristics may appear in hybrids, but they exclusively regarded such appearances as cases of throwing back to one or other of the parent forms—that is to say, as atavism.

Besides this historical or phylogenetic aspect, the claim of hybridisation to be regarded as having a direct rôle in the production of new forms and combinations has been established. For, according to the Mendelian law, which I may to-day regard as generally known—in the production of new hybrid forms all possible combinations of the parental characters occur.* In these results of segregation some may at first sight strike as novelties; their novelty, however, is due to the *combination* of characters, and not to the intrinsic characters themselves. This limitation appears—though only to the inexperienced eye—to be suspended in those interesting cases in which apparently simple characters of the parents are resolved into several component parts, and these components are recombined according to the Mendelian law. This phenomenon, which Bateson describes as "analytical variation of compound characters or allelomorphs," is specially noticeable in colour characteristics (de Vries, Tschermak, Bateson, Correns, and others). Also the converse case, *i.e.* the fusion or synthesis of hitherto separate components into an apparently new simple character, is possible—constituting a synthetical variation by combination of unit characters according to Bateson.

* That monstrous structures Mendelise both in the botanical and zoological kingdoms is amply proved. My own experiments, for instance, show that fasciation in *Pisum* behaves as a typical Mendelian recessive when crossed with the normal stem—as also do the compound ears of wheat and barley in crosses with normal ears.

The question may here be raised in passing, whether the Mendelian "valency" of a character—I mean its condition as dominant or recessive—justifies a conclusion as to its phylogenetic age. With regard to the common inclination to regard the dominant character as the older, I have frequently insisted on the fact * that, though such a deduction is generally permissible, it is by no means without exception. Not only can the valency change with the constitution of the families, but phylogenetically new characters can also be dominant; for example, the beardlessness † of barley obtained by Rimpau as a novelty on crossing may dominate over the possession of beards or hoods; or petalody of the sepals may dominate over the normal flower-structure in *Primula* and *Campanula*.

Already hybridisation derives a much greater importance as a means of forming new varieties from the possibility of a production of new Mendelian combinations of characters and component characters. First of all, from two parents which differ from one another in many ways a number of intermediate types may be sometimes produced: a fact which is most important for the practical breeder as well as for the student of evolution. Hybrid middle forms have apparently originated in great numbers from unintentional crossings, not to speak of those that have been artificially produced. But also of those species which in a wild state and under similar external conditions exhibit a number of constant varieties, very many may owe their variety of form, in part at least, to an original cross between distantly related varieties and to Mendelian segregation.

In strictly Mendelian cases a series of intermediate forms resulting from a cross can be shown to constitute a *discontinuous* series (as opposed to a series of individuals resulting from spontaneous and continuous variation) so soon as the attention is fixed upon single characters and not upon the collective impressions made by the whole. At all events in certain crosses which do not exactly follow the Mendelian scheme, but approach the *Zea* type of Correns, segregation results in the production of a whole series for continuous intermediate forms which carry the characters of both parents combined in various proportions.

I have, for example, obtained such transitional series by the crossing of bearded and hooded strains with unbearded barley, and by crossing barley with various numbers of rows, especially in regard to the degree of fertility of the lateral spikelets; also with rye and wheat in regard to the type of the spikelet and shape of the seed.

Anyhow the intermediate forms all appear to split further, but not all in the same way. It seems as if among the products of an impure or graduated segregation, fresh groups with various modes of transmission may arise in certain cases; and in every group it appears that greatly developed characters gain in their power of transmission. The hybrids with but a slight development of hoods or beards only produce few descendants with fully developed hoods or beards respectively.

I have hitherto only spoken of continuous transition series, as they

* *Zeitschrift für das landwirthschaftliche Versuchswesen in Oesterreich*, 1901, p. 1037; *Beihefte zum Bot. Centralblatt*, Band xvi., Heft 1, 1903, pp. 16, 17.

† United with this is the cryptomeric possession of beard (in barley).

result from impure segregation in the second generation onwards—departing from the true Mendelian system. I have, however, observed similar graduations among *Primula*, *Verbena*, and Beet hybrids of the first generation; yet these cases do not yet appear to me to be sufficiently clear to justify the publication of further details as yet. The production of many types (*Pleiotypie*) even in the first generation, with constancy of each single form, constitutes the characteristic of the *Hieracium* hybrids, according to Mendel, as well as of the so-called Macfarlane hybrids generally, and, moreover, among others, of the *Œnothera*-mutants of de Vries.* He regards, as is well known, such a condition as a proof that the differences among these forms or characters are *specific*, considering a Mendelian behaviour as an expression of merely racial difference.

At this point those cases of crossing should be mentioned (such as *Triticum vulgare* × *Tr. polonicum*) in which the splitting (*Spaltung*) at the second generation intensifies one of the parental characters such as the length of ear of one parent, and permits the appearance of a long series of gradations in that respect; but according to my investigations to date it produces no, or almost no, absolutely pure representative of the other parental character, *e.g.* the ear-length of *Triticum vulgare*.† The same is true of the close packing of the ears in crosses between square-headed forms of wheat and narrow-eared forms. Even in the following generations the one form of parent never again comes out pure, that is, free from a more or less distinct trace of the other.‡

Among the most significant results of hybridisation considered as a source of progress in evolution is the modern production of really new forms which cannot be regarded as mere combinations of characters already visible in the two parents. These cases should be spoken of as *hybrid-mutations*.

Some years ago I was able to state that in not a few cases such novelties produced by crossing have a regular behaviour and exhibit Mendelian ratios. For example, as my experiments have shown—with which those of Bateson and Saunders are in complete accord—there are certain races of peas (*Pisum arvense*), beans, stocks, and barley which when in-bred remain perfectly pure, but on crossing with another strain, chosen almost at random, give rise to new characters. For such forms I have proposed the term *cryptomeric*. In the cases I studied the two parents do not seem to take an equal share in the production of the new form; for the possession of the hidden character by one of the parents reveals itself in spontaneous variation. The other parent thus plays the part of "activator" or complement. The Mendelian ratios conform to the conclusion first promulgated by Correns, that in these cases two pairs of characters are concerned, namely, the possession or want of the character and of the activator. These regular hybrid-mutations are thus to be described as degressive or retrogressive in de Vries' sense; since there is a loss or a gain on one side only, and not a variation in several respects,

* Extreme cases (monotypic constancy, monolepsis) of this kind are presented by the false hybrids of Millardet.

† Similarly, according to Biffen's communication to this Conference, from the cross *Tr. vulgare* × *Tr. dicoccum* no pure *dicoccum* reappears.

‡ This case must be carefully distinguished from that of general dominance or partial dominance, and from the phenomena seen in the false hybrids of Millardet.

as in the spontaneous mutations of *Œnothera Lamarckiana.* When characters recognisable as belonging to the fundamental type of *Pisum arvense, e.g.* purple flowers, spots on the leaves, dark brown seed-skin, indent shape of seed, appear suddenly on a cross with an aberrant race showing defect of some character, *e.g.* pink flowers, absence of leaf-spots, seed-skin marked with a pattern or transparent, round shape of seeds, the case is evidently one of hybrid atavism. In other cases, nevertheless, we have certainly to do with actual novelties, in particular with defect-mutations, such as albinism, produced by hybridisation. It is therefore quite possible that, in the history of organic evolution, hybridisation has not rarely resulted in the production of new races, and perhaps in the appearance of progressive mutations. It may be remarked parenthetically that my observations (as well as those of Saunders and Bateson) on *Matthiola* hybrids and on barley hybrids incline me to doubt whether the production of absolutely pure gametes in Mendel's sense is of general occurrence. I suggest rather—at all events in certain cases which must be regarded as crypto-hybridisations—that there is a double potentiality with substantial prevalency of one or other of the characters, and particularly in the case of the recessives, which though of hybrid origin breed pure if in-bred ; that there is a latency and not a complete absence of the dominant character.*

One must, of course, on the one hand, be cautious in regard to a proposition of such far-reaching consequences ; on the other hand, it would certainly be dangerous to endorse the doctrine of gametic purity as one of which no doubt could be entertained.†

In so far as the new characters appearing as the result of a cross can be regarded as atavistic, they have a phylogenetic significance. Crossing in such cases is a means by which characters apparently lost in the phylogeny can be reactivated, and it thus throws light on the history of the form now existing. On the other hand the capability of a single species as regards production for varietal forms—its range of form, as it has been called (Goebel, Celakowsky, Heinricher, de Vries)—may be determined by study of its hybrid-mutations—just as it may by that of its spontaneous variations and mutations. Crossing is thus an experimental method by which the condition of the separate characters may be raised

* In these cases also a fresh cross may be enough to reactivate the original parental character which is latent, with irregular Mendelian valency (as dominant, co-dominant, recessive or co-recessive) or with polymorphism in the first generation.

In illustration: hoariness or pigmentation in the case of smooth or otherwise pigmented descendants from the cross smooth × hoary, or from white × coloured in *Matthiola*, Tschermak, Bateson. Cp. also observations of Cuénot, Haacke, Guaita, Castle (1903), Castle and Allen (1903), Bateson (1903), on mouse-crosses ; as also those of Darbishire and Hurst.

T. H. Morgan, *Science* xxii. 1905 and *Biol. Centralbl.* xxvi. 1906, p. 289, advocates generally the hypothesis of impurity of the gametes of hybrids (*i.e.* the double potentiality with prevalency or dominance of one character over the other, and a production of gametes of both kinds on an average in equal numbers). He would regard the extracted dominants which breed true as those in which the recessive character is latent, and those in which the recessive character is manifested (the extracted recessives) as latent-dominants, thus representing all these segregation-products as crypto-hybrids in the sense which I first proposed in October 1903.

† I have already advocated this point of view. *Beitr. z. Bot. Centralbl.* Bd. 16, H. 1. 1903, October, pp. 20 and 25. See also *Zts. f. das Landw. Versw. in Oesterr.* 1904, p. 23 ; *Arch. f. Rassen u. Ges. Biol.* 2. Jahrg., H. 5 and 6, 1905.

from a state of latency to one of full activity, or conversely may be lowered and suppressed.*

Cross-breeding also promises to give evidence as to the phylogenetic relations of two given forms based upon the behaviour of the differentiating characters in their transmission. The efforts of the earlier experimenters to determine questions of specific identity or distinctness in this way are well known. Similar distinctions have been made between *racial* or Mendelising characters (with the so-called "bisexual" inheritance) and *specific* characters (with so-called "unisexual" inheritance, according to Macfarlane) in the way which de Vries has elaborated in his treatise. I shall here only add the following conclusions from my own observations:

Not merely hybrids of various cultivated races, but also those made between cultivated and the wild or putative ancestral forms, such as I have bred on a large scale in the case of cereals; e.g. *Secale cereale* × *S. montanum* and reciprocal, *Hordeum spontaneum* × cultivated barleys, wheats × *Ægilops* and reciprocal, follow Mendelian laws—a fact which is probably an indication that these cultivated forms † arose by discontinuous and not by continuous variation from the wild form, followed by a process of selection.‡

The Mendelian system has, moreover, proved to hold good, not only for what are called organic characters, but also for so-called *adaptative* characters, *e.g.* for the length of the vegetative period. Thus, a cross between winter and summer rye gave a uniform intermediate first generation, with a tendency towards the summer type. When the first and second generations were grown in summer the following ratio appeared, the count being made when the stems began to shoot up: 2·5 summer type (shooting) : 1 winter type (resting), and a count made on harvesting gave 4·5 : 1. The average, therefore, was 3·1 : 1.

A winter cultivation of the first generation and a summer cultivation of the second gave an increased proportion of the winter type, and the ratio reached 1·98 : 1. It mattered far more whether the second generation was raised in summer or in winter. When the order was winter, winter, summer, the typical split-forms of the third generation gave the ratio 1 shooting : 10·5 resting. Summer, winter, summer gave similarly 1 : 9·49, and summer, summer, summer gave 3·4 : 1. The adaptative character "length of vegetative period" shows, therefore, clear indication of progressive susceptibility to the influence of external conditions, as shown by the changes in the Mendelian ratios.

* Cp. my paper in *Zts. f. das Landw. Versw. in Oesterr.* 1904.

† By way of comparison with the Mendelian behaviour of cultivated races of the same species, I have not only made crosses between cultivated races and wild species, but I have also begun experiments with two wild races of the same species, e.g.. *Anagallis arvensis* × *cærulea*.

‡ My crosses instituted among the *Hordeæ* (which are especially suitable for cross-breeding) contribute, among others, to a decision of these questions of systematic relationship. Crosses between continuous variants, *e.g.* in the several degrees of purple pigmentation in the pods, do not, according to my experience, show any Mendelian behaviour. I have repeatedly and expressly emphasised the distinction between the non-Mendelian continuously varying characters and the Mendelian discontinuously varying characters (*Zts. f. das Landw. Versuchsw. in Oesterr.* 1901, pp. 652-654; *ibid.* 1902, pp. 795 and 847; *ibid.* 1902, p. 23; *Beitr. z. Bot. Centralbl.* 1903, p. 17). The proviso must now be added that the bearing of Johannsen's principle of "pure lines" upon this thesis is not clear, pending a thorough investigation of the modes of variation of the lines.

I think that the little which I am able to contribute suffices to show that hybridisation is of no inconsiderable importance for the precise study of descent, since new forms may arise by crossing—just as they may by adaptation, by discontinuous variation or mutation, and—as was formerly held—by selection of continuous variations. Hybridisation thus presents us with a rich source of forms, and constitutes not rarely a method of experimentally testing questions of ancestry.*

Discussion.

The President: I am sure we heartily welcome Professor Tschermak amongst us as one who took a part in the original discovery of this principle. He had many difficulties to contend with, difficulties which would have quenched a less ardent zeal than his. I have had the pleasure of seeing the garden in Vienna where he works, and anyone who has seen the work carried on under such conditions must realise the immense advantage it would be to those engaged in such work to have a properly equipped and adequately appointed garden with all the necessary labour and technical skill at command. It is to be hoped that in this country there will before long arise such institutions, in which this work can be carried on, and I feel that such a Conference as this may do something to stimulate public interest in such matters and lead to the endowment of such institutions.

Turning to the paper itself it deals with a great variety of topics which it is impossible to speak on now. It raises the interesting question as to the production of new forms in crossing. Dr. Tschermak's work has shown that the appearance of such new forms is in reality due to recombinations of characters which are frequently introduced, though perhaps one does not always perceive them. He speaks of the difficulty, that has been noticed by others, of knowing whether the older or the more recent character will be dominant. His researches show that there is no general rule on that point. You cannot say, and nothing but actual experiment will show, whether the older or the more recent character will dominate. Then there are questions of difficulty from the fact that the forms which appear through crossing may be continuously connected, and that fact Dr. Tschermak attributes to the multitude of characters that have come in, and the vast complications that may be introduced through their many combinations and inter-connections. To the same fact he, I think, attributes the mixed F_1. That may be due to the fact that, though parental forms may appear to be alike and true to type, yet they may contain a number of cryptomeres, or hidden characters, whose influence does not appear as characters until they meet a complementary character. The appearance of these novelties in later generations he speaks of as hybrid mutations. He might have gone on to say that the appearance of such hybrid mutations raises the important question as to the extent to which the mutations of de Vries are due to similar occurrences.

* Cp. my paper "Die Lehre von den formbildenden Faktoren." *J.B. f. Pflanzen- und Thierzüchtung*, 1903; "Üb. Bildung neuer Formen durch Kreuzung," *Verh. d. internat. Bot. Kongress. in Wien*, 1905.

Another point Dr. Tschermak speaks of is the intensification of characters. Examples were seen in his crossings of wheats and barleys in the length of the ear. This he attributes to the recombination of a number of existing characters that we cannot perceive. Other cases there are where there is no return of a pure parental form, and that is for us a great difficulty. We do not understand what is the meaning of the crosses; we are not quite certain, at all events, of the cases where no return is made to the pure parental form. Mr. Hurst tells us for example that, from a crimson zonal pelargonium in the first generation, in the second generation he has got almost all the colours that occur in zonals with the exception of the crimson he put in to begin with. We have seen the same thing in fowls. I think Professor Tschermak suggests that that may be due to recombinations, or that dominant characters have been present, which hide the parental type and prevent it from reappearing.

These things show us, with regard to evolutionary conceptions, first, that we must be very careful in using results of crossing as an indication of history, because new forms that appear may either have occurred in the past history, or they may be novelties.

Professor Wittmack said that Dr. Tschermak crossed winter rye with summer rye, with the result of showing that the winter forms became much more winter forms, and the summer forms much more summer forms, thus showing that outer influences had a great effect on the length of the vegetation; that outer circumstances can have influence. Therefore the changing of Mendel's numbers altered the ratios.

The President: This is a most interesting line of inquiry as to whether the ratios can be altered by external progressive influence generation after generation. I think any evidence of a definite kind bearing on that particular part of the problem will be of great value to us.

Mr. R. A. Rolfe: As to the self-fertilisation of hybrids I think it is due to the fact that the pollen aggregates together in the mass, and insects cannot carry it away. I have got a good number of the second generation of hybrids self-fertilised. I am in hopes that in the second-generation-hybrid-flowers we shall get some rare and interesting evidence as to the way in which these characters work out. Variability seems to be increased by crossing; but however hard you may try to get *all* the qualities of both parents combined, as a general rule you cannot do it. You cannot always do as you like. What I have noticed is that when you take two forms that are very much alike—near relations—the variability is slight; but when you take two diverse forms of different descent, then the variability is very great.

The President: I must express the thanks of the Conference to Professor Tschermak, and I hope that in future he will be able to carry on his intensely interesting and valuable work under better conditions.

CASTRATION AND HYBRIDISATION IN THE GENUS *HIERACIUM*.

By C. H. Ostenfeld, of Copenhagen.

I have the pleasure of submitting to you a short report on my experiments in castration and hybridisation with some species of *Hieracium*. The experiments are not of any great importance from a practical point of view, but they are of no small scientific and theoretical interest, as the phenomena with regard to fertilisation and fructification in the *Hieracia* are very strange.

In the beginning I worked in conjunction with Dr. Raunkiaer, of Copenhagen, and we succeeded in proving that a great many species of *Hieracium* develop fruits without fertilisation, a phenomenon which is generally called parthenogenesis or apogamy. In 1902 Dr. Raunkiaer proved the same to be the case with all the forms of *Taraxacum* (dandelion) hitherto examined: and as it occurred also in the *Hieracium* species first examined, the fact suggested a doubt whether the numerous *Hieracium* forms mentioned and described as intermediate forms really were hybrids, which they most often are said to be. Nevertheless the doubt was not correct; some species of the genus readily produce hybrids. Mendel published in 1870 a short note on *Hieracium* hybrids produced by him; and lately Correns has published the letters from Mendel to Nägeli, in which we get a much fuller report of the great work of Mendel than in his own little paper. He succeeded in crossing a good many species of the sub-genus *Pilosella* and got a large number of hybrids; but there is a great difference with regard to the readiness of each species to produce hybrids. His experiments with species of the other sub-genus *Archieracium* had no result, with the exception of two hybrids both having *H. umbellatum* as father. Unfortunately, he does not tell us more about the last hybrids, which are of special interest, as they are the only ones of the *Archieracia* hitherto produced.

Besides Mendel, A. Peter, together with Nägeli, studied *Hieracium* hybrids extensively, but he has not done much by experimental methods. It is perhaps worth noticing that the first botanist who crossed *Hieracium* species with positive results is F. Schultz; he had already in 1856 published a little note about this. It is rather curious that we do not find other publications about the artificial hybridisation of *Hieracia*, and that the last fifteen to twenty years do not show any progress in that direction.

Before reporting my own experiments, I wish to say that at an early stage of my studies the apparent contrast between apogamy and hybridisation necessarily required cytological examinations of the egg-cell development; but, not being acquainted with the modern technique and methods, I myself was unable to do this, and I was very glad when Dr. Rosenberg, of Stockholm, accepted my proposal to do the cytological researches.

The method used by Dr. Raunkiaer for determining the fructification of *Taraxacum* without fertilisation is a very simple one. We cut off with a razor the upper half of unopened flower-heads. By this drastic operation the larger part of the corollas, of the styles, and the whole of the anthers and stigmas, are removed, while the basal parts of the corollas and the styles, together with the ovaries, remain. The "castrated" head continues to grow, and at last we get ripe achenes, easily recognisable in the short pappus-rays. The fruits are quite normal, and produce normal offspring.

Castration experiments with *Hieracium* species are similarly carried out, and the result has been the same—at least with regard to most of the species; but there are some exceptions. I have mentioned the two sub-genera *Pilosella* and *Archieracium*, both of which are very common in Europe and very polymorphic. The sub-genus *Pilosella* is characterised by leafless scapes and stolons, while the sub-genus *Archieracium* has a leafy stem and lacks stolons; the more technical systematic differences between the sub-genera are omitted here.

1. In America, where the *Hieracia* are of much slighter importance than in Europe, we find a little sub-genus *Stenotheca*, which reminds us in the vegetative parts of *Archieracium*, but it is more slender and has smaller flower-heads, rather like those of a *Crepis*. I have two species of this sub-genus under cultivation, and these two species do not set fruits at all after castration, while the intact heads give fruits of which at least a good many have the power of germination; consequently the species in question are quite typical with regard to the fertilisation phenomena. This result agrees well with the cytological examination made by Dr. Rosenberg.

2. The results are very different in the sub-genus *Archieracium*. My experiments include fifteen species belonging to very distant sections of the sub-genus, and among them only one species, *H. umbellatum* in the widest sense (*sensu latiore*), needs fertilisation, all the others giving plenty of fruits after castration. The only exception is the same species which Mendel used for his above-mentioned hybrids, and which lately have been examined from a cytological point of view by Prof. H. O. Juel. This author has found that the development of the egg-cell in *H. umbellatum* was normal. On the other hand, Prof. S. Murbeck has studied the cytology of the ovule of three other species of *Archieracium*, and he has stated that the egg-cell grows out to an embryo without fertilisation, *i.e.* apogamically. I think we may conclude that the same is the case in the fourteen species castrated by me.

3. In the sub-genus *Pilosella*, again, the condition of affairs is more complicated. I have used six species in my experiments, and five of them have given ripe fruits after castration, while one, *H Auricula*, needs fertilisation. Among the species with apogamy there are, for example, the well-known *H. Pilosella* and *H. aurantiacum*, and also a rather curious species which for years has been cultivated in the Botanical Garden of Copenhagen. It belongs to the species with many-flowered corymbs and is related to *H. magyaricum*, a species-group which is common in the eastern part of Central Europe; Prof. Blocki, of Lemberg (Galicia), has described it under the name *H. excellens*; it

is said to grow wild in Galicia. My reason for mentioning this species especially is that it is purely female, just as some *Taraxacum* forms. The anthers are present, but do not contain any pollen at all. This female form is able to set fruits when isolated under a cover-glass—as indeed also after castration—and is consequently apogamic. But if we examine the fruits of a head, we find that besides the full ones not a few are empty (20 to 30 per cent.). Also in other species of *Pilosella* such empty (barren) fruits occur among the fertile ones, while in the *Archieracia* nearly all the fruits are full. A probable supposition, which is supported by the cytological researches of Dr. Rosenberg, is the following: the fruits which under isolation remain empty need fertilisation for their development; the developed fruits have been produced apogamically. The species being female, the first category of fruits is never developed unless there are specimens with pollen (hermaphrodite or male) in the native country of the plant, or unless the pollen of other species is able to fertilise them. The last case has been tried and with positive results, and this hybridisation confirms our supposition.

I have produced several hybrids by crossing the *H. excellens* with *H. Pilosella* and *H. aurantiacum* as fathers; the parents are the same species and the same individuals which afterwards gave apogamic fruits when castrated. The offspring of such a cross consist of a few hybrids and of many plants like the mother; the cross-fertilisation has consequently succeeded only in some cases, the majority being apogamic. Another interesting point is that the sister-hybrids are not like each other. This was pointed out by Mendel, since it formed a contrast to his results with *Pisum* hybrids, where the primary hybrids are all alike.

The hybrids between *H. excellens* and *H. aurantiacum* have a deeper yellow flower-colour, and the under side of the outer corollas always bears a red stripe, which never occurs in *H. excellens*. The hybrid which is nearer to *H. aurantiacum* is hermaphrodite, the others are female like their mother; the latter are the stronger, especially with regard to their vegetative power. The characteristics of the hybrids between *H. excellens* and *H. Pilosella* are analogous; the most important ones are the largeness of the flower-heads and the ramification of the scapes.

The fructification-power of the hybrids is very limited; most of the fruits are empty, but the hybrids most nearly resembling *H. excellens* give fuller fruits than the others. It seems then as if the influence of the mother is both the stronger and the better one, and herewith the fact is consistent that most of the hybrids produced are nearer to *H. excellens*, a few are intermediate, and none quite like *H. aurantiacum*.

After isolation or castration, the hybrids have given some fruits which have germinated this year and will soon flower. Here we have the interesting fact that the power of the parents to develop fruits without fertilisation has been inherited by the hybrids.

At present I am not able to say anything about the second generation of the hybrids, as my sowings of last year, by some mistake, have failed. Mendel says that F_2 is quite like the first generation, and this seems very probable now, when we know that apogamy occurs in the genus.

In another hybrid I have found some phenomena which do not agree with that, but which perhaps are to be explained in another way.

By crossing *H. Pilosella* with *H. aurantiacum* an intermediate hybrid appeared. This hybrid has a very slight power to give fruits ; after castration all the fruits are empty, but in the heads which were neither castrated nor isolated some few full fruits occurred. These fruits have germinated, and the offspring had begun to flower when I left Copenhagen a few days ago. Most of these plants are different from their mother, the primary hybrid; a few are somewhat like it, others are nearer to *H. Pilosella*, others again are pure *H. Pilosella*, and a single plant differs from both its mother and its grandparents. As the heads have not been isolated, it is probable that this variation is caused by cross-fertilisation with *H. Pilosella* or with other species (the latter is probable as to the last-mentioned individual); but, on the other hand, the pure *H. Pilosella* argues Mendelian segregation. It is indeed necessary to repeat the experiments and isolate again, as at present it is quite impossible to give a sufficient explanation.

. . . .

It will be evident from the report here given that the investigations are only in their beginning. But the whole problem of the heterogeneity of the fructification phenomena in the genus is of such interest that I hope it justifies this short communication.*

The genera *Hieracium* and *Taraxacum* are usually considered as the most developed and youngest members of the *Cichoriæ*, which group again is placed as the highest and youngest of the *Compositæ*—the youngest and largest order of Flowering Plants. And, seen from that point, the abnormity or degeneration with regard to fertilisation in the two genera is not least interesting.

* A fuller report of the experiments, illustrated with coloured drawings of the hybrids, is published in English in *Botanisk Tidsskrift*, vol. xxvii. 1906, pp. 225-248.

FIG. 89.—1, 2, Hieracium excellens (a female species); 3, Hieracium Pilosella (hermaphrodite); 8, Hieracium Pilosella (a female form); 4, Hieracium aurantiacum (hermaphrodite); 5, Hieracium excellens × aurantiacum ⚥; 6, Hieracium excellens × Pilosella ♀; 7, Hieracium Pilosella × aurantiacum ⚥

CYTOLOGICAL INVESTIGATIONS IN PLANT HYBRIDS.

By Prof. O. Rosenberg, of the University, Stockholm.

It is especially through the researches of recent years that the cytologists and the students of heredity have come into close relation.

It has been proved that the principles of Heredity discovered by Mendel and others are in accordance with the phenomena of the development of the sexual cells. Especially have the Mendelian principles of segregation found their morphological parallel in the conception of the Reduction-division among the sexual cells of animals and plants, recently promulgated.

Some years ago, whilst examining a hybrid of *Drosera*, I found some interesting facts concerning the Reduction-division. This hybrid between *D. longifolia* and *D. rotundifolia* was particularly interesting because the cells of the parent plants had an unequal number of chromosomes in their nuclei. *D. longifolia* has forty and *D. rotundifolia* twenty chromosomes, and in the germ-cells twenty and ten respectively.

It is important to remember that the chromosomes are definite bodies in the nucleus, which in all probability are the bearers of hereditary characters. It was therefore interesting to follow the behaviour of the parental chromosomes in the new hybrid plant formed from the egg-cell after conjugation of the two nuclei.

As I have pointed out in previous papers, all the somatic nuclei of this hybrid showed thirty chromosomes, which is the exact average sum of those of the parents. At the development of the sexual cells, pollen grains and embryo-sacs, I also found some peculiar facts, which agreed very well with the new ideas of the Reduction-division and in some points provided morphological evidence of their correctness.

It is considered that the Reduction-process consists in a conjugation of parental chromosomes two and two, and that one chromosome (*a*) of the father fuses with one chromosome (*a*) of the mother. Several facts speak in favour of the correctness of this supposition, especially the interesting discovery by Sutton and Montgomery of unequal chromosomes. Sutton always found in somatic cells two chromosomes of a certain size, while in the germ-cells only one of these occurred.

It is, of course, very difficult to observe this conjugation of parental chromosomes, since the chromosomes of a nucleus are generally like one another.

In the case of *Drosera* there was a different number of chromosomes in each parent, *i.e.* ten from the father and twenty from the mother. I then found that in the conjugation-act of the Reduction-process there occurred ten double and ten free single chromosomes round the spindle-figure. From this it is clear that ten *D. rotundifolia*-chromosomes had conjugated with ten *D. longifolia*-chromosomes, and that the remaining *longifolia*-chromosomes, meeting no corresponding *rotundifolia*-chromosomes, were left free in the protoplasm. It often happened that these free chromosomes, instead of remaining in the protoplasm, were taken into the nuclei, but the process then was an irregular one. It must therefore happen sometimes that all the *D. longifolia*-chromosomes are concentrated within one nucleus and the other must consequently form pure *D. rotundifolia*-nuclei.

If it is true that the characters of a hybrid are determined by the combination of the chromosomes, it is very important to gain a knowledge of the pollen grains themselves and of the number and arrangement of their chromosomes. The pollen grains are generally of the same size and structure even among species of the same genus. In the case mentioned, however, the pollen grains of *D. rotundifolia* are sufficiently different from those of *D. longifolia*, and an examination of several pollen grains of the hybrid might afford some evidence as to whether the ideas of the cytologists and the hybridisers on the segregation of characters in the germ-cells are correct. Especially in the case of the hybrid *Drosera* where such different proportions of *longifolia*- and *rotundifolia*-chromosomes may occur in the pollen grains, the pollen grains should be a very favourable subject for a demonstration of the point at issue.

The four pollen grains of one tetrad, when ripe, lie free in the anthers. and it is therefore difficult to note which pollen-cells are sister-cells, and it is very necessary to know this, if one wishes to use the structure of the pollen grains as evidence of the correctness of our views as to Reduction.

In *Drosera*, however, the conditions are very favourable for demonstrating the point at issue. The four pollen grains are always joined in tetrads, even when they have reached the stigma. We therefore have here a rare case of germ-cells in a *hybrid, where the parent germ-cells are unequal and the four cells derived from each mother-cell always remaim connected with one another.*

As I said, it must happen that sometimes the *D. rotundifolia*-chromosomes in the hybrid must be purely segregated in one nucleus, and the *D. longifolia*-chromosomes in another. Of the four nuclei in a tetrad two and two have quite the same chromosomes, since the second division is an equation-division. Now in the *Drosera*-hybrid most of the pollen grains are like those of *D. longifolia*, but sometimes I found a very interesting structure of the tetrads: *two cells of them were quite D. rotundifolia-like, while the two others were D. longifolia.*

I therefore think that we have here a very good demonstration of the *results* of a Reduction-division, the important act in plants and animals, through which the characters of the sexual nuclei got fixed. The two pollen grains are quite *D. rotundifolia*, and the other two *D. longifolia*.

I also think that this is a good illustration of the hypothesis of gametic segregation put forward by Mendel.

Finally, I will in a few words touch upon another question concerning the cytological structure of hybrids. It is known that many hybrids show the peculiarity that their F_1-generation is not homogeneous, but exhibits a graduation in characters between those of father and mother.

I have had the opportunity to study such a case. I had the pleasure to examine from a cytological point of view the very interesting material of

Hieracium of Mr. Ostenfeld, especially one hybrid, which Mr. Ostenfeld had obtained by crossing *H. excellens* with *H. Pilosella.*

I examined some forms of this hybrid and found *that they had a different number of chromosomes.*

This depends upon the fact that the egg-cells in *H. excellens* differ greatly in their number of chromosomes. Some have fifteen, sixteen, seventeen, twenty, and so on. And this also depends upon the fact that the Reduction-division is irregular, resembling the facts I have found in *Drosera.* I cannot go further into detail, but I have followed step by step the development of the embryo-sacs, and have found that egg-cells really have different numbers of chromosomes.

If these egg-cells are conjugated with a male gamete from a species with a fixed number of chromosomes, as in that case, there must be different results, or, in other words, the plants produced from these egg-cells contain very different chromatin material, and thereby the differences in these forms may perhaps be explained. Of this, however, I will not say that this explanation has a general bearing. But it seems to me that this case indicates the necessity for a close co-operation between the cytologist and those who are experimenting on the question of heredity.

ON THE GERMINATION OF ORCHIDS.

By Noël Bernard, of the University, Caen, France.

On the roots of orchids, whether wild or cultivated, are found fungi which, living inside the cells of the cortex or of the tangled root-fibres, form lumps like balls of thread. This fact is well known, easy to observe, and furnishes one of the best examples of those associations, very common in nature, between plants and micro-organisms.

I have attempted to estimate the degree of the close connection of this association: the degree of the dependence that an orchid and its

Fig. 90.—Professor Noël Bernard.

fungus may have on one another. I will now explain the results derived from some of my experiments.

. . . .

The fungi of orchids can live apart from the roots in which they generally lodge. If one cuts off a small fragment from a contaminated root and sows the fragment in a sterilised tube on a suitable nourishing medium, the fungus develops freely. It can be transplanted from tube to tube without losing its power to grow. I have such pure cultures of fungi living after four years without their having been returned to the orchids.

In these cultures one easily recognises the orchid fungi by certain characteristics, especially because they continue to show here and there lumps similar to those which they form in the cells of the roots. Certainly these fungi in the natural course could live freely in the soil and propagate themselves from neighbour to neighbour; but they never produce spores, for they appear to be entirely unable to produce germs which can be disseminated in the air, a property so common in all other moulds.

I know three species of these orchid fungi; they belong to the *Rhizoctonia* genus; I have not yet given them any names; but a simple look at culture tubes in which they are confined, like those in fig. 91, enables one to distinguish them easily. One species seen indifferently in the roots of *Cypripedium*, *Cattleya*, *Cymbidium*, and *Aërides* appears to be the most widely distributed. A second species has not yet been met with outside the roots of *Vanda* and *Phalænopsis*. A third I have only found in the roots of an *Odontoglossum*. Other species of these orchid fungi doubtless exist, but in relatively limited numbers, assuredly much fewer than the number of orchid species.

. . . .

Although the fungi can live apart from their host-plants, the orchids themselves require the presence of their guests for their own development. I have sown the seeds of numerous orchids "aseptically" in sterilised tubes, on cotton or in jelly, saturated with a nutritive solution. I have for many months protected the seeds under covers from micro-organisms, in suitable temperature and light. In these conditions the seeds have not freely germinated; they swell, and later on they get green, but their growth remains insignificant.

On the other hand, if germs of the appropriate fungus are sown with the seeds, they commence to germinate almost immediately in a very regular manner. With the fungi which I possess, I have obtained not only the germination of *Cattleya* and of *Cypripedium*, but also of *Odontoglossum*, *Phalænopsis*, and *Vanda*, which are regarded by practical men as more difficult.

Fig. 91 shows culture tubes in which I have raised some of these plants, and also a tube in which the seeds have been placed without fungi, and are hardly developed at all.

In nature or in the greenhouse the same thing takes place as in my culture tubes. I have examined a large number of young orchids which had germinated in very varying conditions, and I always noticed that they were invaded by the fungus from the beginning of their life. The orchids are therefore practically dependent on their parasitic fungi, since they do not grow without them.

One of the greatest difficulties in conducting such experiments is to procure the fungus suitable for each kind of orchid seed. As a rule, I use fungi collected from the roots of an orchid of the same species, or of one nearly allied to that which bears the seeds. This is the best rule I can give, but it may not be an absolutely sure one. The properties of the fungi may indeed be different at the time of gathering from what they are at the time of using them. Also the fungi of *Cypripedium* and of *Cattleya*, kept cultivated in my tubes for three years, have lost the ability to cause the corresponding seeds to germinate, which property they formerly possessed. Again, the fungi obtained in one and the same

1 2 3 4

Fig. 91.

1, Seeds of *Odontoglossum* after four months sown on jelly, without fungus, and showing no noticeable development; 2, The same, after the same time, sown with the fungus from the roots of an *Odontoglossum*, and growing; 3, Seedling of *Phalænopsis*, nine months old, germinating through a fungus from the roots of a *Phalænopsis*; 4, Seedling of *Vanda tricolor*, seven months old, germinating through the fungus from the roots of an *Odontoglossum*, anomalously reduced to a relatively voluminous tuber and leafless.

year, at two different periods, from the roots of the same clump of *Odontoglossum*, have acted very differently on the germination of the same variety of seeds of this family. Everyone knows that the bacteria which produce a great many animal diseases are often more or less injurious, according to the conditions of their collection and of their culture. Similarly, the fungi of orchids are capable of varying degrees of "virulence"; they are more or less active, according to their origin and their age.

I have recently discovered that it is possible to restore a sufficient activity to the fungi when their powers have been impaired. It is enough to let them live for some time in the embryo of an orchid, the seed of which it is desired to germinate. The fungus can live for some time inside the embryo, although this does not develop, and when it is taken out after, say, two months, it can exhibit sufficient activity to make the seeds germinate which formerly appeared to resist its action. By using this process of passage through the embryo, I have been able to restore to my oldest cultures of fungi all their original activity which they had lost.

. . . .

Amateur orchid-growers have often told me that they attach great importance to the discovery of sure and practical methods of securing the germination of the seeds of orchids. Some of them, having knowledge of my researches, have begged some of the fungi to sow with their seeds; they have sometimes secured good results, but often they have met with no success. This is due to many causes, but chiefly to the fact that the fungi were to be used in the germination of species of orchids from which I had never made cultures. The fungi supplied had, moreover, not the requisite virulence. The cultures from which they were taken were more or less old, and had been prepared according to the requirements of my experiments, and not for the definite purpose for which my correspondents required them.

So as always to be able to produce a fungus suitable for the germination of orchids of a certain species, cultures of the seeds of that species should be made every year in the laboratory, in order to maintain and even to increase the strength of the fungus in question, thus keeping it alive without interruption in the embryo, whence it may be taken as required.

A laboratory attached to a greenhouse sufficiently well supplied, and specially reserved for this horticultural work, would without doubt be able at all times to furnish horticulturists with the fungi suitable for the germination of their seeds, but it must be borne in mind that this would necessitate continuous and minute work. The reward would be the possibility of obtaining more easily the germination of the seeds of hybrids.

. . . .

In taking account of the important part played by fungi in the germination of orchids, one would be able to hope for novel results.

In the course of experiments still incomplete, I have proved the possibility of raising the same orchid with the fungi of different species. For example, seeds of *Cattleya* have germinated with the fungus of a *Phalænopsis*, seeds of *Vanda* with the fungus of an *Odontoglossum*. The seedlings so obtained frequently differ a little from those cultivated with the fungus of the parent plant. With all due reserve necessary in so complex a subject, I believe it is possible that the mere change of the fungus may result in a variation of the species of orchid in question, a result which the experimenter should be able to control.

I might add that orchids are not by any means the only plants which live with fungi. The extension of the researches which I have instituted, whilst restricting myself to a single case, will possibly one day indicate new methods to patient experimenters in search of rational methods of culture applicable to a large number of plants.

The President: This is a most interesting question. The point is that, without these fungi, the orchids cannot germinate. It is not merely a mechanical case where it is necessary to break the covering, but the presence of the fungus seems to be essential to the process of germination. Another remarkable thing is that if the seeds are sown in the presence of the fungus when it has been long cultivated *apart* from the orchid, no effect is produced. The fact seems to be that the fungus has become weakened by long absence from the orchid; but if you can anyhow get it to recover its power it is able to act like a normal fungus.

From some of the papers we have heard to-day it appears as if we might hope to connect the visible facts of heredity with the microscopical facts, and when that is done we shall have reached a new era.

ON XENIA.

By Edward A. Bunyard, F.R.H.S.

The study of xenia, or the influence of foreign pollen upon the maternal structure, has aroused much interest and some experiment since the first days of artificial hybridisation, and on few subjects has there been expressed greater difference of opinion. A convenient summary of the earliest observations will be found in Darwin's "Animals and Plants under Domestication," and it was this varying evidence which led me some years ago to commence some experiments, the results of which, with evidence from other sources, I shall ask you to consider.

It is desirable at first to have some exact definition of what we mean by xenia, as the somewhat vague way in which this word has been used by many writers has contributed to the difficulties with which this subject is surrounded. Professor Tschermak suggests that xenia shall be applied to those cases where the pollen shall have caused, apart from the egg-cell and embryo-sac, variation corresponding to the pollen-parent upon the vegetative parts of the mother-plant. This definition seems to be exact, and I suggest that the word "xenia" should be confined to these cases. This will leave on one side all those cases where the embryo or embryo-sac is affected, such cases being due to Mendelian dominance or double fertilisation.

The discovery by Guignard of this latter process has thrown so much light on cases of supposed xenia that I may, perhaps, be permitted to quote from Professor Wilson's book on "The Cell in Development and Inheritance" (p. 221) a short description of this phenomenon:

"The pollen-cell possesses two generative nuclei, and one of these conjugates with the egg-nucleus, thus effecting fertilisation, the other conjugates with one of the polar nuclei thrown off in the course of mitosis. By a division of the fertilised egg-cell arises the embryo, while by division of the compound nucleus resulting from the fusion of the polar nucleus and the second sperm-nucleus are formed the endosperm-cells."

It will thus be seen that the embryo-sac is purely hybrid in its structure, and any effect of foreign pollen that may appear in it should, I suggest, be referred to as the effect of double fertilisation, as the meaning of xenia (guest-gift) prevents one from supposing that it was intended to apply to something which the guest himself brings and takes away with him.

That a large number of cases of supposed xenia must now be referred to double fertilisation can hardly be doubted, and some of these will be mentioned below, but we may select as a typical case the experiments made by Major Trevor Clarke and mentioned by Darwin in which a biennial stock with light brown seeds was pollinated with a variety having violet-black seeds. The resulting seeds showed in many cases the effect of so-called xenia. If, however, we examine the seeds of the ordinary biennial stock, we find that the black seeds have transparent seed-coats and the colour is entirely in the endosperm layer, the result seen, therefore, was undoubtedly due to double fertilisation, and not xenia.

The other effect which has been taken for xenia is that of Mendelian dominance. In *Pisum* this error was easy to make, and all early records of xenia in this genus are open to this objection.

In the experiments detailed below, which I have made myself, every precaution has been taken to ensure that sources of error shall have been eliminated. The flowers were emasculated before dehiscence of the anthers and carefully bagged, and pollen taken also from bagged flowers.

Maize.

Many experiments have been made with this subject, and cases of supposed xenia have been frequently recorded.

In 1902 I obtained a race with a red seed-coat, which was mixed in with the commercial yellow variety. This was cultivated, and without exception came true from seed.

This was crossed with the yellow variety, the pigment being held in this case also in the seed-coat, but no xenia was visible. Seeds of the cross were sown the following year, but owing to climatic conditions no seeds were produced.

Webber has made many interesting experiments with maize, and much care was taken to ensure that the varieties chosen reproduced their seed characters truly. In some few cases the colour of the endosperm layer was changed as a result of double fertilisation, but the changes of form figured seem likely to be due to Mendelian dominance.

De Vries, in an interesting paper in the "Revue Générale de Botanique," April 1900, quotes Vilmorin as having observed changes of colour only in his experiments and no changes of form of seed, and on his own part suggests, as did Koernicke, that those changes occur only when an older character meets a younger, an idea similar to that once entertained with regard to Mendelian dominance. I can find no trace of any influence of the pericarp having been noted in maize.

Beans—Runner and Dwarf.

I have tried very many experiments with these varieties of beans, but have had no success at all, the operation of emasculation being extremely difficult to perform, and the light colour of the pollen renders it almost impossible to ascertain if the stigma is pollinated or not. It is, however, notable that in these widely-grown plants, where in all cases the pigment is contained in the pericarp, I can find no recorded cases of xenia.

Peas (*Pisum*).

In the case of peas we come to the stronghold of xenia, as in this genus the evidence is most plentiful.

My own experiments were unfortunately mostly carried out with 'William the First' as a pollen-parent, and as I found out too late the variable character of the seed colour, my results are of no value. I have, however, repeated Darwin's experiment of crossing the blue-podded pea on to an ordinary green-podded variety, but in none of the eight pods which resulted could I see any influence of the pollen-parent.

In view of the great influence that premature ripening and other conditions of culture have upon seed colour and the lack of any chemical or microscopical tests, judgment must be suspended as to the occurrence of xenia in peas until a more exact knowledge of the pigments in question is available.

Peaches and Nectarines.

In the course of crossing these fruits I have made some between the white and yellow-fleshed varieties such as 'Pineapple' nectarine (yellow) × 'Elruge' Nectarine (white), 'Pitmaston Orange' (yellow) × 'Elruge' (white), 'Sea Eagle' peach (white) × 'Barrington' peach (yellow).

In none of the above was any influence visible to the unaided eye, but the seedlings which are growing now may give some signs of their hybrid origin.

Apples.

In these fruits xenia had been often recorded, and in view of the fact that there is no doubt that all effects seen are true influences of the pericarp, they offer a subject for experiment of peculiar interest. The case of the 'St. Valery' apple, mentioned by Darwin, which produced no pollen, but on being pollinated by other varieties gave fruits which resembled those of the male parent, will be well remembered. The case having thus been stated, it was but natural that corroborative evidence commenced to flow. My experiments, with one presumable exception, have so far failed to augment this stream.

The varieties chosen differed much in size and colour. For differences of size and form the following were used:—'Stone's' (large) × 'Old Nonpareil' (small russet), 'Rosemary Russet' (medium) × 'Stone's,' 'Rosemary Russet' × 'The Queen' (large, flat), 'Sturmer Pippin' (small) × 'The Queen.'

Differences of colour:—'Gloria Mundi' (green) × 'Rosemary Russet,' 'Gloria Mundi' × 'Hoary Morning' (distinct stripes), 'Stone's' (green) × 'Hoary Morning,' 'Stone's' × 'Old Nonpareil.'

In all the above cases no change was seen, though the crossing of a russet skin with a smooth green would seem to offer an easy and exact method of detecting xenia.

The only case in which any direct influence was seen was in the case of crossing a flower of 'Sandringham Apple' (large, faint stripes) with the pollen of 'Bismarck' (bright, non-striped red).

This fruit ripened and fell off three weeks before other uncrossed fruit on the tree, and was in shape and colour quite out of character, resembling a fine fruit of 'Cox's Orange.' This case was reported in the Journ. R.H.S. vol. xxiv. part 4, p. 1899.

This is the only case of presumable xenia I have ever seen. The resulting seedlings are now growing.

Summary of Results.

So far as my own experience goes, it seems that the occurrence of xenia as an influence of the pericarp is of rare occurrence, and, in my opinion, considerably more experiment is needed to establish the phenomena on a firm basis of positive fact. The methods of the recognition of colour are at present too inexact, and some chemical or microscopical test is needed. The experiments with apples and peaches and nectarines are open, of course, to the objection that the pollen of these fruits as hybrids would be mixed; but even then the resulting fruits might be expected to show some variation, though not corresponding to the pollen-parent. I cannot, perhaps, sum up the position better than in the words used by Bacon in his preface to the "Novum Organum" in reference to science in general, when he says "that it is not so much an opinion to be held as a work to be done."

ABSTRACT OF AN ADDRESS ON "PLANT BREEDING IN THE UNITED STATES DEPARTMENT OF AGRICULTURE."

By Dr. Erwin F. Smith, Pathologist in Charge of Laboratory of Plant Pathology, United States Department of Agriculture.

The speaker prefaced his remarks by saying that it was always disappointing to an audience to have to listen to a substitute; that he was no plant-breeder himself but only a pathologist; that he had, however, been closely associated with the plant breeders of the Department since the beginning of the work, and might therefore be able to express the views of an intelligent layman, and, moreover, was deeply interested in that phase of the subject which relates to the production of plants resistant to disease. No one regretted more than he that Dr. Webber could not be present and speak with authority concerning these interesting facts.

The subject is comparatively a new one in the United States Department of Agriculture, dating back not further than twelve or fourteen years. The Department has bred plants principally for four reasons, viz.

(1) For resistance to *disease*. Examples of plants bred for this purpose are cotton, melon, and the grape vine.

(2) For resistance to *cold*, *e.g.* citrous fruits.

(3) For resistance to *alkali*, *drought*, &c., *e.g.* alfalfa, wheat.

(4) For greater *productivity*, and for *quality*, in edible fruits, foliage, fibers, &c., *e.g.* pineapple, tobacco, cotton, maize.

Taking these subjects in order, I will first mention cotton. Some years ago the "Sea Island cotton" growers in the United States were greatly troubled by a mysterious disease which sometimes swept away entire fields of cotton, and often destroyed many plants in fields not so severely attacked. This disease persisted in ground once subject to it, and became more and more destructive as time went on, so that finally some of their best fields (tile-drained and heavily fertilised) had to be abandoned as waste land. I determined the cause of this troublesome disease to be a little fungus known as *Fusarium*, which lives over winter in the soil and which attacks the plant through the root system, filling the vascular or water-carrying bundles with its threads, and in this way crippling or destroying the plants. The work then assumed such proportions that it seemed wise to turn it over to an assistant, whose whole time should be given to the subject, in order, if possible, to find a satisfactory remedy for the widespread trouble. I picked out for this responsible post Mr. William A. Orton, then a recent graduate of the University of Vermont, who subsequently obtained most brilliant results in overcoming the ravages of this disease by means of selection. I might add in passing that Mr. Orton had never seen a cotton field until he went South on this perplexing and seemingly well-nigh hopeless mission. Very little was accomplished the first year. I well remember a notable conference with Mr. Orton at the close of the first season's work, when he was thoroughly discouraged, and expressed himself as feeling that the whole year had been wasted. I cheered and encouraged him as best I could and advised him to continue. The key to the situation was obtained the next year. Subsequently the work was carried out as follows, Mr. Orton receiving great assistance from some of the growers, particularly from Mr. Rivers, on whose plantation the very resistant "Rivers Cotton" originated.

In fields much subject to this disease it was observed that here and there a plant came to maturity and bore fruit. The seeds were selected from these unusually resistant plants, and the following spring they were planted on land subject to the disease. Many of the resultant plants contracted the disease, but a considerable proportion remained free from it or practically free. Selections this year were made from the most resistant plants, having an eye also to obtaining plants with other good qualities, such as productivity, shape of boll, length of fibre, &c. The same method was pursued the following year. In the course of four years plants were obtained with good productivity, good quality of fibre, and marked resistance to disease. Such plants stood up and bore a good crop on fields where the ordinary cotton made a total failure. The fungus was frequently found in the small roots of such plants, but it seemed to have lost its destructive power. For some years now the Department has sent out quantities of this cotton seed to the growers, and they have also quite generally begun to make selections for themselves from resistant plants. It is about seven years since this work was begun, and the growers now no longer fear this disease. Fields which were abandoned are again under cultivation, and the problem appears to be solved.

Melon.—Mr. Orton has also had charge of the work of obtaining resistant varieties of watermelons to replace varieties much subject to a soil disease which I was able to demonstrate to be similar to the cotton disease, *i.e.* due to a soil *Fusarium*. There are large areas in the United States (parts of Carolina, Georgia, Florida, and Texas) where this watermelon disease has prevailed to such an extent that the growing of melons on a commercial scale has been abandoned.* The melon is much more subject to this soil disease than the cotton to its disease (*i.e.* it is a less woody and less resistant plant), so that the disease often makes a clean sweep of the fields. The plants are attacked in all stages of growth, but will often appear to be all right until the melons are half-grown, and then suddenly the entire plant wilts and dies within a day or two, and the water-conducting system in the stem of the plant is then found to be plugged by the fungus, which enters through the root system. I have sometimes seen large fields in which there was scarcely a healthy plant. The extent of infection in the melon rendered it practically impossible to obtain any resistant plants by the process applied to cotton, namely, by simple selection. Mr. Orton found, however, that a plant known as the "citron" in the United States—that is, a vine † with deeply lobed leaves and

* In 1899, in the Southern States, a total of 117,551 acres were planted in watermelons (U.S. census of 1890). This statement sufficiently shows the importance of the crop.

† English readers must bear in mind that in America all trailing plants are called "vines." What is here meant is the race of hard-fleshed very firm melons which are used for the making of preserves. Since these are used for the same purposes as the true citron of commerce, they are colloquially known as "citrons."—Ed.

a hard, striped, roundish fruit, not unlike the watermelon, but inedible until cooked, when it is used for sweet pickles and preserves—was quite resistant to the disease. He used this plant for one of the parents and good varieties of watermelons much subject to the disease for the other, making a number of crosses. The seeds from these crosses when planted gave rise to about a thousand varieties of melons. There were all sorts of fruits—long and short, round and crooked, smooth and rough, plain, deep and pale green, and variously mottled and striped. Of the thousand or more varieties which resulted from these crosses, quite a good many proved resistant to the soil fungus, but only about six varieties had other qualities such as to make them worthy of further consideration. The seeds from these six plants were saved and planted the following year on land much subject to the disease in order to test on a large scale the qualities of the melons, and to learn more respecting their resistance to the disease. All continued to be resistant, but only one of the six proved to be a commercially satisfactory melon. The following year, therefore, only this one variety was propagated, but on a large scale and with excellent results. The plant is quite resistant to the disease, and produces a good melon—a melon which appears to be in every way equal to the best of the sensitive varieties which are firm enough to stand shipment. Acres of these melons have been grown on land so thoroughly infected that ordinary melons could not be fruited.

The Grape Vine.—Something like twenty years ago there suddenly appeared in California a very destructive disease of grape vines known as the Anaheim disease. Anaheim was at that time a prosperous village in the centre of a very productive grape region. All the land practically was cultivated in grapes. In the course of four or five years this disease prevailed so extensively that the vineyards were destroyed, the wine-presses were sold, and the land was thenceforth devoted to other crops. Many efforts were made by Mr. Newton B. Pierce to determine the cause of this disease, but without avail. He found, however, that certain unsatisfactory varieties of grapes were little subject to the disease, and, by making thousands of crosses between these and the best varieties, that is, those much subject to the Anaheim disease, he obtained a number of very resistant vines bearing superb bunches of fruit of excellent quality. Mr. Pierce's first crosses were made according to my recollection in 1892. Mr. Pierce also successfully crossed raisin grapes to resist *coulure*, a disease which renders the bunches ragged and worthless for market by causing the abortion of the whole or a large part of the berries when they are very small.

Resistance to Cold.—The Department was forced to consider the breeding of plants resistant to cold by an accident to citrus-growing in Florida. In the winter of 1894–95 there occurred a very severe freeze in Florida which defoliated most of the orange and other citrous trees. The trees immediately put out a crop of new leaves, which, while still young and tender, were destroyed by a second freeze occurring about six weeks after the first one. This second injury so weakened the trees that a very large number of them died (90 per cent. perhaps), and what had been a very prosperous citrus region, vying with California in the production of oranges, ceased to be one altogether, the land being subsequently used for truck crops and other purposes, the orchardists who were not entirely discouraged going farther south to begin over again. Mr. Walter T. Swingle and Dr. Herbert J. Webber then set to work to obtain resistant varieties by crossing choice oranges and other citrous fruits sensitive to frost with the extremely hardy *Citrus trifoliata*, which stands the winters well as far north as Washington,* and is occasionally cultivated as far north as Philadelphia, but which bears a small bitter, worthless fruit. They obtained many seedlings as a result of these crosses.† As soon as these had reached a size sufficient to furnish wood for budding, they were cut to pieces and budded upon the branches of older trees, in order to hasten their fruiting. In this way from many of these plants fruits were obtained at an early date, *i.e.* within three or four years. I saw and tasted many of these new fruits. Among the number, a dozen or more proved of much interest, the quality of the fruit in some cases being excellent. The variations among the seedlings of these trees, the second generation from the hybrid, are expected to be even more promising.

A large number of these plants were also found to be quite resistant to cold, so that when they could not be used for their fruits they were still available as hedge plants. Some of the citrous fruits thus obtained can undoubtedly be cultivated as far north as the Carolinas.

Resistance to Alkali and to Drought.—The Department's work on the production of "alkali"-resistant plants is still under progress. We have thousands of acres in our West which are capable of producing a great quantity of food for man and beast were it not for the fact that these lands are more or less permeated by harmful alkaline and neutral salts. Many of these districts are scantily supplied with rainfall, and are cultivated by means of irrigation, which sometimes washes out the alkali and at other times washes it in, as has been your own experience in Egypt. The problem was to find plants of agricultural importance which would grow on the best of these alkali lands. It was discovered that some plants, for instance, the date palm, will flourish in soils that contain so much alkali that ordinary plants cannot grow at all, and with this in mind Mr. Walter T. Swingle has made several trips to the Sahara, and has imported for the Department large numbers of such palms, which are now growing satisfactorily in several places in Arizona and California. Many of these palms have already fruited heavily, yielding dates of excellent quality, and there is not the slightest doubt but that we shall within a few years be growing our own dates—at least all of the finer table varieties.

The thought was that it might be possible also to find somewhere in the world alfalfa and other agricultural crops with a greater root resistance to alkaline water than that manifested by the ordinary varieties, the cultivation of which on these lands had failed. With this end in view, the Department sent out explorers to various parts of the Old World and also into our own alkaline tracts, the result being the discovery that there are certain types of leguminous and other plants

* During the last twenty years I recall only one winter in which it was at all injured.

† See Dr. Webber's paper in the *Report of the Hybrid Conference*, 1899, p. 128 *et seq.*—Ed.

which will tolerate much more "alkali" than ordinary plants, and, while this line of investigation is not yet completed, it appears to be very promising, and we have a good hope that we shall in the end be able to bring into cultivation considerable areas of these alkaline lands. These experiments are in the hands of Mr. T. H. Kearney, one of Dr. Webber's assistants.

In one instance we have taken advantage of a great natural selection occurring in another part of the world. In the middle of the United States, from the foothills of the Rocky Mountains to within a few hundred miles of the Mississippi River, and from Manitoba on the north to Mexico on the south, there is an area which used to be known as the great American desert, and some part of which was so mapped forty years ago. This area, extending westward from the 100th meridian through five degrees of longitude, and northward from the Rio Grande through twenty degrees of latitude, receives a scanty rainfall, varying from eight to fifteen inches. Considerable portions of this great region are well adapted to wheat, so far as the soil is concerned, but the climate is too arid. Our spring and winter wheats had been tried repeatedly in various parts of this region, but always unsuccessfully. There was not enough rainfall to bring them to maturity. The bulk of this land was therefore used as a thin pasture or left unoccupied. To Mark Alfred Carleton, one of my colleagues, now in charge of the cereal investigations of the Department of Agriculture, belongs the honour of having made it possible to cultivate wheat on these lands. As the result of observations in Russia, it seemed to him that the "durum" wheats (otherwise known as "hard" wheats or "macaroni" wheats), which are grown so successfully in the semi-arid districts of Russia, and which we had never cultivated at all, could be grown in our own West, where the soil and climate seemed to be much like those of the Russian hard wheat districts. Mr. Carleton was a Western man, and the idea so possessed him that he could scarcely think or talk of anything else. He travelled and observed extensively in our own West, collected rainfall statistics, and made a second trip to Russia. The more he examined into the question the more evident it became that here was a great opportunity. At his instigation the Department of Agriculture imported numerous varieties of durum wheat and tested them in many places in the West, at experiment stations and on the lands of private individuals. On the whole the trials were an immense success. Some varieties, indeed, proved unsatisfactory, but others did remarkably well, proving themselves admirably adapted to the conditions on our plains. The result has been the westward extension of our wheat belt several hundred miles over many degrees of latitude. The wheat growers were enthusiastic. Just here, however, unexpected difficulties arose, and Mr. Carleton had to fight his battle all over again with the millers. They did not like this new wheat; they would not buy it; they would not grind it. Various were the objections raised: It was too hard; it would not make good flour; to grind it required new mills and new machinery. Handling it commercially was therefore out of the question. For several years the battle raged. Mr. Carleton wrote endless letters, travelled, held conferences, persuaded, lectured, wrote for the trade journals, and finally won over the millers as he had previously done the farmers. They built new mills or added new machinery to old mills and accepted the situation. The end is nowhere in sight. From small beginnings six or seven years ago the durum wheat crop of the United States has increased steadily until last year (1905) it amounted to twenty million bushels, and this year to fifty million bushels, largely grown on semi-arid land where ordinary wheats will not grow. I regard it as one of the most brilliant of our economic achievements. In passing it is interesting to note that some of these wheats are also very resistant to rust ;* one variety is absolutely resistant.

Breeding for Greater Productivity, &c.—Dr. Webber has had great success in cross-breeding cottons for increased length of fibre and for greater productivity. I have seen upland cottons in his possession which had two or three times as great an amount of fruit on them as the ordinary varieties, and others in which the fibre was at least one-third longer than the ordinary fibre.

Prof. W. M. Hays, our Assistant-Secretary of Agriculture, is also greatly interested in plant breeding. While he was Director of the Minnesota Experiment Station he bred wheats very diligently, and among other striking results he succeeded in increasing the yield of the best strains of "blue-stem" spring wheat, on an average, two to five bushels an acre by simple selection.

Dr. B. T. Galloway, the chief of our Bureau of Plant Industry, has also been much interested in the improvement of plants by selection. He discovered, some years ago, that by always planting the heaviest radish seeds (crops grown under grass) he obtained plants of much greater uniformity, and which matured so much more quickly than the ordinary radishes that he was able to grow five crops in the time which had previously been devoted to four crops.

Dr. Galloway and Mr. P. H. Dorsett, now in charge of our introduction garden at Chico, California, were at one time greatly interested in violet culture on a commercial scale in houses near Washington. During a period of four years they selected violets for yield of flowers. When they began, the average yield on their plants, which were the average plants of the florists, was fifty flowers a plant ; when they ended they had a very uniform selected strain which yielded ninety flowers a plant.

For a number of years the Department has been greatly interested in increasing the productivity of the maize plant, and has had good success.

We have shown that by simple selection the yield of maize can be increased, over large districts, an average of 10 per cent. ; and, in isolated cases, as much as 20 per cent. These experiments have been in the hands of Mr. C. P. Hartley, one of Dr. Webber's assistants.

An effort is also being made to improve the quality of the maize kernel by increasing the nitrogen content. Several of our State experiment stations are also engaged on this problem, I believe. Just what will finally come of it I am unable to say ; but, if we could somewhat reduce the starch content in the maize kernel, and at the same time increase the nitrogen content, we should undoubtedly be able to make it a more palatable food product, and would probably be able to sell a good deal more of it to European countries than we can do at present.

* *Puccinia graminis, Puccinia Rubigo-vera.*

The Department has also undertaken to improve the pineapple by cross-breeding. This work was begun by Messrs. Webber and Swingle, and in recent years has been carried on by Dr. Webber. I have myself tasted many of these cross-bred pines, and some of them have proved to be remarkably good. The effort has been to procure, not only pines having an excellent flavour (we have many such already), but also those having other qualities specially desirable for our market, such as medium size, shallow eyes, juiciness, absence of hard core, attractive top, good shipping and keeping quality. Along with these qualities we have sought for increased vigour in the plant, increased resistance to disease, and absence of spininess in the foliage. Some of our cross-bred pines combine these qualities to a marked degree and indicate that one can obtain almost any sort of pineapple he desires by persistent cross-breeding.

I will mention only one other case. We have bred tobacco very diligently during the last few years in order to obtain a uniform quality of wrapper leaf of high character, and in this we have been remarkably successful. This work has been largely in the charge of Mr. A. D. Shamel, one of Dr. Webber's assistants. Starting out with an arbitrary standard of perfection, he has worked steadily towards producing plants having the desired qualities, and has now obtained many such plants, a portion of which I have seen. I have also seen photographs made by him showing whole fields of tobacco in which each plant looked exactly like every other plant; the leaves, when cured, having the right length, breadth, and thickness, the right texture, and the proper burning quality to make a first-rate wrapper leaf. All this has been accomplished within the last four or five years by diligent in-and-in breeding and careful selection. The tobacco has proved as plastic in our hands as the pineapple, and almost anything can be accomplished in the way of obtaining a desirable wrapper leaf by persistent endeavour.

Discussion.

Mr. H. J. Elwes, F.R.S., V.M.H.: As I listened to the speech of Dr. Erwin Smith I could not help seeing how progressive the United States Department is in all matters relating to the development of their ountry; and as the remarks I had proposed to make on another subject are particularly apposite to this question, I beg the permission of the President to allow me to say a few words now on the question of the hybridisation of trees.

When you consider the subject of the hybridisation of trees you will find it to be one of extraordinary difficulty, especially from the economic point of view, on account of the time necessary to see the results of experiments. But at the same time you will also perceive the enormous importance of the subject. Strange as it may seem, some of the most important scientists, including, I may say, Professor Mayle of Munich, absolutely deny the propriety of selection, and refuse to consider the possibility of applying to the breeding of trees, and the raising of them from seeds, the same laws which have been proved to be of enormous advantage to the world in respect of all other plants. Then it was that it seemed to me that it might be worth while for this Conference to consider the matter, and to ask—having regard to the fact that we do know enough to be able to speak confidently as to the possibility of raising hybrids—whether it would not be a proper subject to try and propose for the consideration of the various Governments that they should each help the interests of their country, and attempt on trees exactly what Dr. Erwin Smith has told us has already been done in the United States with regard to plants.

I do not suppose that in our lifetime anything definite can be proved, but I cannot admit on that account that it should be out of the question as a scientific matter, that mankind should apply to trees the same laws that he has already applied to plants and cereals. We all know that in some countries forestry is in importance second only to that of agriculture. In the United States it is the fourth commercial interest of the country. You may say that this has no connection with hybridisation, but I will appeal to M. de Vilmorin and others who have raised hybrid pines which have grown with extraordinary vigour, and whose timber already indicates a superiority over other forest species. My contention is, I think, a reasonable one, and I venture to express it in order to get information from those of you who know so much more than I do about the subject of hybridisation, and also for the purpose of bringing about, if possible, a combination of public establishments and combined experiments, such as no individual man could undertake properly even if he could expect to live a hundred years.

I should like to mention, in passing, that a vast number of larches in this country are attacked by the *Peziza Willkommii.* I have directed experiments towards discovering what may be done to enable the trees to resist this extremely destructive fungus. It is a matter which should be taken in hand generally, as it appears to me to be a subject of the highest possible importance.

Mr. W. Carruthers, F.R.S.: Like others at this Conference I wish to congratulate Dr. Erwin Smith, who is such a distinguished officer of the United States Government. I do not know that a more practical lesson could have been given us than the results that have been obtained at the instance of the United States Government and through the work that has been done by Dr. Smith and his colleagues. It seems to me that, notwithstanding our devotion to agriculture in this country, we are behind almost every other country in the world in our attainments on this subject, and I hope Dr. Smith has kindled a torch which will not soon go out, and that it may result in important work being done by our own Government for the benefit of all sorts of planters and cultivators. The President of the British Association hopes to get ten millions a year for scientific research. I am quite sure this Society could very profitably employ perhaps one-fifth or even one-tenth of that amount in carrying out investigations of this kind. When one sees what has already been done, and what are the practical results of these investigations, one cannot but wonder at our own Government's comparative inaction. When Dr. Smith was speaking about cotton, I remember when in this country we suffered serious injury from clover-sick land just as these fields in America were cotton-sick fields. That was caused by the fungus *Sclerotinia*, which does much mischief. Happily this *Sclerotinia* confines itself almost entirely to the clover, and does not touch grass or other components of our pastures; but the extent to which it affected the clover crops at the time I refer to it is impossible to realise. The late Sir John Bennet Lawes had a little plot in his grounds which was free from clover-sickness for thirty-five years, but at last it succumbed, and Sir Henry Gilbert, the distinguished chemist, believed that this was due to clover-sick soil. He investigated the soil and discovered thousands of *Sclerotinia*, by which future crops would have been utterly destroyed. I think we have got a lesson from Dr. Smith which we should try to put into practice in our own country.

The President: I am sure Mr. Carruthers's remarks find an echo in our hearts, and we thank the Government of the United States, who are the pioneers in this matter. We shall do well to follow them.

Mr. F. W. Moore, V.M.H., asked whether the tobacco alluded to by Dr. Smith was a local tobacco, or could it be used in all tobacco-growing soils?

Dr. Smith: I do not know that I can answer that question fully.

Mr. Geo. Gordon, V.M.H.: We have for many years past endeavoured to grow maize in this country as a vegetable, but with very little success indeed. But last year a friend of mine obtained some varieties of maize from America without name. Possibly they may be some of the hybrids to which Dr. Smith has referred. They are to-day growing amazingly, and are producing most satisfactory crops. I had hoped to be able to grow a selection so as to exhibit them at this Conference. I do feel that America has done excellent work.

Dr. Smith: The cotton fields are now covered with cotton. Fields which a few years ago were worthless are now covered with good cottons.

The President: When I was present at the Hybridisation Conference in New York I saw the striking photographs illustrating the work of Mr. Orton. I regret that Dr. Webber has not been able to attend this meeting, but I know that we will all agree that Dr. Smith has been an excellent substitute.

Dr. Hansen suggested that the results in the case of tobacco were obtained purely by inbreeding.

The President: That undoubtedly is the secret of the whole thing.

[See also pages 66 and 67.]

THE IMPROVEMENT OF THE SUGAR-CANE BY SELECTION AND HYBRIDISATION.

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The discovery that the sugar-cane produces fertile seed, from which can be raised seedlings possessing varied characteristics as well as increased richness of juice, has placed cane-growers in a position to endeavour to improve their varieties, so as to place the cane in an equally favourable position with the beet.

The attacks of various diseases and the general fall in the price of sugar made it necessary for all cane-growing countries to establish local departments to inquire into the best means of combating these disasters. Owing to the great influence of soil and climatic conditions on the yield of sugar, highly improved methods of cultivation and the use of modern appliances in manufacture received considerable attention, as being the most direct method of accomplishing a cheaper production. It was, however, found that a hardier race of plants, which would givo a greater tonnage of canes to the acre, was the first requisite, the quality of the juice being taken up for improvement later.

Although most of the older varieties of canes were found to suffer from the ravages of insects and disease, and in consequence a considerable loss of sugar was experienced, yet no serious steps wero taken to inquire into the methods of preventing this loss until in some instances total crops had been almost entirely destroyed. Then the minds of a few began to turn to methods of obtaining improved varieties of canes. It became absolutely necessary to produce canes which were more resistant to disease, and at the same time, if possible, varieties which would give a larger yield of sugar per acre. This increased yield of sugar could be obtained in three ways, the combination of all three being the goal aimed at. These were: (*a*) by an increased tonnage of canes per acre; (*b*) by an increased yield of saccharose in the juice with a higher ratio of purity; (*c*) by a freedom from rotten canes.

The differences apparent in existing varieties made it obvious that it was possible to produce new and improved types superior to those already under cultivation; but, liko all plants propagated mainly by cuttings, it was extremely difficult to notice slight variations amongst individual canes. Striking examples of seminal and bud variations had been noticed, and some of these had proved of value. The following four methods were those by which variations were utilised to improve the quality of the crops: (1) Selection amongst native varieties; (2) Introduction of foreign varieties; (3) Hybridisation between native varieties; (4) Hybridisation between native and introduced varieties.

The first two methods will be dealt with very briefly, as these methods were carefully described at the Hybridisation Conference held in New York in 1902.[1] (See References, p. 335.)

Selection.

The chief variations to be looked for amongst existing races of cane may conveniently be classed under three heads: (*a*) variations in habit and vigour of growth; (*b*) bud variations; (*c*) variations in sugar-contents of individual canes.

Variations in Habit and Vigour of Growth (a).—Amongst a large area of canes of any single variety, there were always to be seen some canes distinguishable by greater size and vigour. Planters were advised to select and cultivate such canes, as their greater vigour seemed to indicate a greater power of resisting attacks of disease. This method has been tested practically under scientific supervision in the West Indies, and it has been found that many of the canes thus selected were capable of producing larger yields of sugar. Investigation of the more vigorous canes showed that they frequently varied to a considerable extent from the main crop, and therefore it is quite probable that many of them, instead of being variations of the mother type, were really seedlings which had come up in the fields, and had become cultivated in the next crop. Some of these variations could not be accounted for in any other way, and it was this peculiar appearance of new varieties of canes that subsequently led to the discovery of canes growing from seed.

Bud Variations (b).—Bud varieties or *sports* are not uncommon in the sugar-cane. In fact, in 1886 a communication was addressed from the Royal Gardens, Kew, to all the sugar-producing colonies to stimulate inquiry into the advisability of searching for and cultivating these sports on a large scale, as it was probable that some of these varieties would prove hardier and give a greater amount of sugar than the original stock.

In the summary of the observations on bud varieties of the sugar-cane up to 1901, given in the "West Indian Bulletin," vol. ii. pp. 216–23, instances of such variations were recorded from widely separated countries, viz. Mauritius, Louisiana, West Indies and Queensland.

Since then, other bud varieties have been noticed in the West Indies, and quite recently two interesting accounts of such sports have come to hand from Madras and Queensland.

In Queensland, one of the seedling canes has produced a sport which gave an analysis of 19·72 per cent. saccharose as against 19·03 per cent. saccharose of the parent cane, and 18·97 per cent. of the next best seedling. It would appear that sports generally arise from striped or ribbon canes, and usually keep true to a whole colour, but instances have recently occurred in Barbados in which a green cane has given rise to a green and white-striped sport. Clark, Queensland, holds that "yellow sports have a tendency to grow sweeter than the coloured canes of the kindred variety." This is not borne out by the instance lately recorded from Madras, for a striped cane has been found to sport into red and white canes, and "whereas the white cane gave on analysis very similar results to the parent cane, the red sports excelled all other canes in the station in purity of juice."

In order to put the relative merits of the sport canes and the original stock to a strictly comparative test, they were planted at the Experiment Station, Dodds, Barbados, side by side, in the same field, with other experiment canes.

"The following figures, taken from the report of Messrs. d'Albuquerque and Bovell on the agricultural work carried on at Barbados 1900–2, under the direction of the Imperial Department of Agriculture for the West Indies, show the results obtained. For comparison, the return of White Transparent grown in the same field is also given:

TABLE I.

Cane	Canes. Tons per acre	Juice per acre in Imperial gallons	Pounds. Saccharose per gallon	Quotient of purity	Pounds. Saccharose per acre
Original Stock (ribbon)	21·80	2,696	2·310	93·03	6,228
Sport cane (white) .	27·27	3,555	2·270	91·64	8,070
White Transparent .	23·93	3,063	2·001	86·59	6,129

The yield from the sport cane in the experiments exceeds the yield from the original stock cane by nearly 2,000 lb. of saccharose per acre. This superiority was due to the higher tonnage of the white canes, their juice being slightly less rich in saccharose, and slightly less pure than the ribbon stock. The juice of both original stock and sport was rich in saccharose, and the results, so far, warrant their continued experimental cultivation." [2]

During the season 1903–5, this sport maintained its superiority over White Transparent, as may be seen from the following table, which gives the mean results for these two varieties obtained from experimental plots on black-soil estates, together with the results of a Ribbon Cane, and of a Red Sport cane. This latter, as can be seen from the table, has not proved satisfactory, but during the last growing season has been very vigorous and should give much better results:

TABLE II.

Cane	Canes. Tons per acre	Juice per acre in Imperial gallons	Pounds. Saccharose per gallon	Quotient of purity	Pounds. Saccharose per acre	Per cent. by No. of rotten canes
* White Sport . . .	26·06	3,407	1·954	84·45	6,645	1·55
* White Transparent .	22·83	2,760	2·123	90·45	5,864	2·35
† Ribbon Cane . . .	21·78	—	2·070	—	5,735	—
† Red Sport	19·23	—	2· 51	—	5,131	—

* These results have been taken from Pamphlet No. 40 of the Imperial Department of Agriculture for the West Indies.

† These results have been obtained through the courtesy of Mr. J. R. Bovell, Agricultural Superintendent, Barbados.

Without doubt, these bud varieties deserve much more attention than is given to them, both on account of their economic importance and also because the study of their variation may yield results of scientific and probably practical value. As to the cause of the real nature of these bud varieties very little is known at present. It has been suggested that the striped or ribbon canes are the results of cross-fertilisation, and that, therefore, the sports are to be considered as cases of dissociation and then segregation of hybrid characters, or of *atavism.*

It is supposed that unfavourable influences, either external or internal, temporarily encumber the growth of the young buds and predispose them to reversions. But if sports are of an atavistic nature and are favoured by influences that impede normal growth, how is it that they, almost without exception, give such excellent results when cultivated, being hardier and richer than the original stock from which they arose?

Variations in Sugar-contents of Individual Canes (c).—Bearing in mind the classical experimental work, with which the name of de Vilmorin is associated, in selection of the sugar-beet, by which, through the aid of science, the sugar-contents have been raised from 10 to 18 per cent., workers with sugar-cane were led to commence investigations with regard to the chances of obtaining canes of higher saccharine content. The occurrence of a wide range of variation in the percentage of saccharose in the juices of canes of the same age and variety was soon noticed, and the fact that the sugar-cane was propagated by cuttings naturally suggested that any improvement inherent in the plant could be developed more rapidly than if it had to be grown from seeds. Investigations in chemical selection have been carried out in the West Indies, but the results, so far obtained, are not at all conclusive.

d'Albuquerque, Barbados, at the last West Indian Agricultural Conference, 1905, stated that it would appear "that, with a given variety, the richness or poorness of the seed-cane (*i.e.* cane used for planting) does not affect the quality of the juice of the resulting crop." Harrison, British Guiana, concludes that the "relative richness of seedlings is qualitatively, if not quantitatively, constant." Watts, Antigua, states that "some difference is induced by the process of selection and, while this method of work is not likely to be followed by practical planters as a means of improving their canes, yet the fact is interesting from its scientific aspect as indicating that plants propagated by cuttings are subject to slight alterations." [3]

In Queensland it has been stated that improvement from single-stalk selection is not as great as would be expected, while in Java the evidence seems to point to the fact that selection among "cane-clumps" is likely to give better results than selection among individual canes.

The following is the summary of the results of chemical selection investigations by Kobus in Java up to 1902, and has been abstracted from the JOURNAL of the Royal Horticultural Society, 1903 (vol. xxviii. p. 298):

1. "The amount of sugar in the individual haulms of one sugar plant was apt to vary greatly."

2. "The variability of the amount of sugar in the different varieties was greatest in thick-stemmed varieties that had long been in cultivation, and least in young ones more recently selected from seed."

3. "The amount of sugar in the cane varied directly with the weight of the same."

4. "Heavy plants gave rise to heavy offspring."

5. "The descendants of plants rich in sugar were richer in sugar and heavier than unselected plants."

6. "Simple selection of cuttings of heavy plants did not lead to the production of forms markedly rich in sugar, though the resulting plants were in general richer in sugar. Indeed heavy plants poor in sugar seemed to have a worse effect on the amount of sugar in the progeny than did light plants poor in sugar."

7. "Extreme care had to be exercised in the selection of the ground on which the experiments were made; for even in apparently uniform soil great differences were apt to appear in the individual plants merely in consequence of local variations in the soil."

8. "Increased vigour as reflected in larger yield of sugar was accompanied by greater immunity from 'sereh' disease."

This report is valuable as it confirms some results obtained elsewhere, and at the same time presents many facts of great importance to those interested in raising seedling canes.

Introduction of Foreign Varieties for Field Crops.

The introduction and trial of standard varieties of sugar-cane from other countries is of considerable interest to planters, as probably this was the principal means by which the sugar-cane was distributed throughout the tropics.

Evidence, on the whole, seems to point to India and Polynesia as the original home of the sugar-cane, but it is now cultivated in various localities on both sides of the Equator ranging from the south of Spain, 37 deg. north, to New Zealand, 37 deg. south.

Of the older varieties of cane there appear to have been three or four which were extensively cultivated. In those countries where these are still free from disease, very few others have as yet taken their place; but where their cultivation has become impossible on account of the ravages of disease, others have been introduced to take their place.

In the West Indies, the Bourbon and Otaheite canes have almost entirely been replaced by other improved and hardier varieties.

In Java, the introduction of the East Indian cane "Chunnee" was rendered necessary owing to the home cane being very liable to the "sereh" disease.

Within the range of cultivation of the sugar-cane there are yet many countries where it might be largely grown if only the prevailing low prices should improve.

With the introduction of imported varieties it should be realised that there is always a danger of introducing new diseases and pests. It is important, therefore, that all imported plants be carefully fumigated and disinfected before being allowed to enter any country. Throughout the West Indies, laws of fumigation and disinfection of all imported cane cuttings are generally enforced, and, now that seedling canes are beginning to play such an important part in the improvement of the sugar-cane, and their introduction into new lands is becoming universal, it cannot be too strongly urged that all cane-growing countries should adopt means to prevent the introduction of new pests and diseases.

Hybridisation.

Having briefly reviewed the methods of selection, and the introduction of foreign varieties, it is now propcsed to deal with the question of the improvement of the sugar-cane by hybridisation. Although perhaps the contents of this paper may not appear to be in line with other papers read at this Conference, yet they may prove of value to our tropical possessions in showing what efforts are being made by cane-growers in the Colonies and elsewhere to compete with the beet-sugar production of European countries.

In Europe and America, much of the progress of agriculture during the last fifty years has been due to the continual improvement of the cultivated varieties of plants and to the production of new varieties.

In the tropics, such work, until lately, has been almost neglected, and therefore a record of practically the first work in this direction should be interesting. Although such work has been possible for eighteen years, it is only within the last decade that systematic attempts have been made to raise seedling sugar-canes on a large scale. The remainder of this paper can therefore be divided into two parts, the first dealing with the different methods of producing hybrid canes that have been adopted by those working for this improvement, together with some of the results obtained, and the second with the individual advances made by some of the more important cane-growing countries.

Historical.

The sugar-cane belongs to the *Andropogoneæ*, a tribe of the true grasses (*Gramineæ*). It has a solid stem, which often attains a height of nearly 20 feet, and contains the sweet juice from which the sugar is extracted. It is now generally conceded that all cultivated varieties of canes belong to one species (*Saccharum officinarum*, L.), but there are reasons for believing in the existence of at least three others. None of them, however, are regarded as of economic importance.

It would appear that sugar-canes probably produced from seed were observed at Barbados in 1848 and 1850, and the question respecting the possibility of growing seedling canes in the West Indies was raised at various times between 1859 and 1888. In the latter year Harrison and Bovell, from Barbados, communicated to Kew that they had sixty cane plants under cultivation and that they were almost satisfied that they were seedlings.[1] This eventually proved to be so, and from that time systematic attempts to raise new varieties of seedling canes at Barbados, British Guiana, and elsewhere in the West Indies have been undertaken with highly satisfactory results.

A similar announcement as to the possibility of raising seedling sugar-canes was made by Soltwedel in Java in 1887.

Previous to 1887 or 1888 it was generally believed that the sugar-cane, in common with the banana and other tropical plants, had lost its power of producing fertile seed, and that all recorded observations of new canes up to this time were bud varieties or sports. However, since the establishment of the fact that the cane does produce fertile seed, the improvement of the sugar-cane by hybridisation has made wonderful strides, and now experiments, conducted on scientific lines, are being carried out in Java, India, Hawaii, Queensland, Cuba, British West Indies and British Guiana, &c., with the hope of raising canes less susceptible to disease, and yielding a larger amount of sugar per acre.

Description of Flower.

Before dealing with the different methods of obtaining hybrids it may be advisable to give a description of the flower of the sugar-cane. The flower has often been described and figured, while good descriptions of the seed and its germination were given by Benecke in the "Bulletin of the Middle Java Experiment Station," 1889, and also by Morris in the "Journal of the Linnean Society," 1890.

The following description has been taken from notes made from the examination of many hundred flowers of different varieties during hybridisation experiments at Barbados in 1905–6:—

The inflorescence or "arrow" varies from 2 ft. to 3 ft. in length. It is repeatedly branched, each branch bearing laterally a number of spikelets. The numerous spikelets are one-flowered and hermaphrodite, and are generally arranged in pairs, one being sessile and the other stalked, at distances of a little more than $\frac{1}{8}$ in. on alternate sides of the slender, long branches. From the base of each spikelet, attached to the rachis, spring a large number of stiff, long, silky hairs, which give the inflorescence a glistening silky appearance in the sun.

The flower has the following formula:—*Glumes*, 2; *palea*, 1; *lodicules*, 2; *stamens*, 3; *ovary*, single; *style*, 1 (bifid).

The two glumes are nearly equal; oblong-lanceolate, acute; unawned; stiff; at first green, then purplish, the intensity of which varies in different varieties. The lower is two-nerved and measures 2·5 to 3·6 mm. long, by 0·7 to 0·9 mm. wide. The upper is distinctly one-ribbed, slightly keeled, and measures 2·8 to 3·8 mm. long, by 0·8 to 1·3 mm. wide. These measurements are the average of many investigations on different varieties, for whereas the size of the glumes is generally constant in any given variety, considerable variations have been observed between different varieties.

The palea is solitary, thin, white, membranous, and is enclosed in the upper glume, than which it is slightly shorter. It is ovate-lanceolate, slightly obtuse, generally smooth, and apparently unveined.

The two lodicules are free, minute, truncate or 2–3-lobed, and vary in colour from white to yellowish-green.

The stamens are three in number (during hybridisation experiments in Barbados in 1905–6 three instances of four were found and noted). The anthers are linear-oblong, versatile, and vary in colour from yellow when young to a deep yellowish-red when mature.

Ovary smooth, uni-carpellary; style one, bifid. The styles vary considerably even in the same variety, for in some instances a single style springs from the top of the ovary and soon becomes bifid, while in others there are two styles distinct throughout. The stigmatic plumes are always two in number, and are large, densely plumose, and dark reddish-purple when mature.

The fact that the anthers are versatile and the stigmatic plumes feathery would lead to the conclusion that the sugar-cane, like the majority of the grasses, is naturally wind-pollinated; for when the versatile anthers burst the pollen is much more easily scattered by the wind than would be the case if they were firmly fixed; it is also the more easily caught by the feathery stigmata. This is a point which is still under investigation, and is one of considerable importance in the work of hybridisation.

Methods of Obtaining Seedlings.

In some countries, the earliest method adopted for obtaining seedling canes was by a collection of fertile seeds or casually produced seedlings from the fields.

A later step was the identity of the seedlings from the seed-bearing parent. This method was the one early adopted by Harrison and Jenman in British Guiana. The cane from which the arrow was taken was carefully recorded, and thence commenced a stock of new varieties of canes with the parentage known on one side only.

A further stage was the raising of seedlings from two varieties of canes, by planting in adjoining rows varieties known to arrow at the same time. By this means there was a possibility that the pollen-bearing parent might be identified as well as the seed-bearing parent.

Thousands of seedlings have been raised in this way, but, although the seed-bearing parent was known and registered, the pollen-bearing parent was still uncertain. In consequence, a large majority of the seedlings were found to be less valuable than the seed-bearing parent originally selected. In many cases, however, it was evident that the resulting seedling canes were true hybrids. These hybrids, when they possessed a vigorous habit and a high saccharine content, were carefully propagated and subjected to a rigorous system of selection.

The fixing of good varieties was rendered more easy, as plants raised from cuttings come true to the parent forms and do not necessitate additional selection year after year. After these seedlings had been sufficiently investigated to warrant recommendation to the planters, they were gradually introduced into general cultivation, and have proved the means of overcoming to a considerable extent the ravages of disease, as many were hardier than their parent forms.

Through the uncertainty of the results of the above-mentioned methods of what may be called natural or chance hybridisation, it was considered advisable to conduct hybridisation under control, and by this means it was hoped to combine some of the desirable characters of both parents and therefore produce pedigree canes, which could be recommended for general cultivation.

In 1900 d'Albuquerque at the second West Indian Agricultural Conference, after discussing methods of securing natural hybridisation, which, however, did not ensure against the risk of pollen coming from an unknown source, recommended an artificial method of securing cross-pollination, *e.g.* "to bag each arrow under experiment some time before it is ripe, and when the arrows in the bags are ripe to shake the contents of the bags of one variety into the bags covering the arrows of another, the latter bags being temporarily opened at the top to receive the pollen, and then closed up; every possible precaution being taken to prevent during the transference the access of pollen from any other source."[5]

It was, however, pointed out that such a method did not entirely prevent self-pollination, and therefore it has been replaced by others in which this risk is not so great.

In 1894 it was found by Wakker, in Java, that the Cheribon cane did not bear fertile pollen while the pistil was normal, and therefore any seedling raised from this cane would be the result of cross-fertilisation. This was a great advance in the hybridisation problem of the sugar-cane. Kobus, by planting other good varieties, known to possess fertile pollen, by the side of this Cheribon cane, obtained thousands of seedlings as the result of intercrossing. Investigations in Java upon the raising of sugar-cane seedlings centred around this discovery, and therefore in 1902 a large number of the best seedling canes at Barbados were examined by Lewton-Brain[6] in the Laboratory of the Imperial Department of Agriculture for the West Indies to inquire into the proportions of fertile to infertile pollen in the anthers of different varieties. By this means it was possible to divide the West Indian varieties of canes into three classes: (1) in which the anthers show a large proportion of normal pollen; (2) in which the anthers show a very small proportion of normal pollen; (3) in which the anthers show a moderate proportion of normal pollen. If, therefore, an arrow of a cane producing much normal pollen is bagged with an arrow of a cane producing little fertile pollen, the risk of self-fertilisation is reduced to a minimum, and if fertile seeds are produced by these canes they will almost certainly be the result of hybridisation.

The possibilities of the hybridisation of the sugar-cane under control, by removing the stamens of one flower and the transference of pollen from another, were discussed by Boname, Mauritius, in 1899. It was thought, however, that this was almost impracticable on account of the large number of flowers on each panicle, and also through their microscopic size. It was also pointed out that it was not known with certainty whether the flowers of the sugar-cane were autogamous or not, and therefore emasculation would have to take place while the flowers were very young. The emasculation of immature spikelets of the sugar-cane without injuring the very delicate ovary and stigmatic plumes was thought to be an operation of considerable difficulty, and therefore the raising of seedlings by hybridisation under control was dismissed as being impossible. In 1900, d'Albuquerque stated that to ensure that the seedling-canes produced are the result of cross-fertilisation between the parents selected "would need the elimination of the anthers before they were mature, a very difficult task in a plant the parts of whose flowers are so small as in the sugar-cane"; but in 1904, Lewton-Brain, after consultation with d'Albuquerque and Bovell, performed experiments in artificial cross-pollination, in which the flowers of one variety were emasculated while still young, covered in a muslin bag, and then pollen from another variety was transferred to them by hand. This method of raising hybrids by artificial cross-pollination proved successful. Five stools of hybrid canes were raised in Barbados as the result of this work. It is reported that four pedigree hybrids have been raised in Queensland, and in Cuba about six hundred are said to be now under investigation.

The operation of emasculating the flowers has to be performed under a dissecting microscope upon a platform 8 or 9 feet above the ground. Such an operation, under field conditions, with a strong wind blowing is attended with considerable difficulty. Even when accomplished, an unfavourable season, with very hot, dry winds, or heavy rains, is likely to destroy the chance of good results. That so much depends upon the season may be seen by the results from Cuba. Four years' work yielded but two hybrid seedlings, while the work of a single favourable season produced six hundred.

Having established the fact that hybrids of sugar-cane can be obtained by cross-pollination under control, it remains to discuss briefly the best methods of attacking the problem of raising disease-resistant varieties with a large sugar-content.

Outlines for Future Work.

Formerly, with a nearly common standard of perfection, the attempts to procure an improved race of sugar-canes centred around breeding from the best varieties; but now, by carefully analysing the different characteristics of the different varieties under cultivation, it may be possible to breed methodically for definite objects.

The work on inheritance carried out by Mendel and communicated to the Brünn Society in 1865, and since so ably elucidated by Bateson, shows conclusively that the gametes are pure with respect to the characters they carry. Further, the work of Biffen with wheat-breeding should serve as a model on which breeding of sugar-cane should be carried on. By following such methods, instead of making a considerable number of crosses indiscriminately with the hope of obtaining some improvements, hybridisation on definite lines should now be carried out.

The first thing to consider, therefore, is what desirable characters are required to be chosen. As it is necessary that the hybrids should be an improvement commercially, only those characteristics of the cane which appeal to the planter should be considered. The chief amongst these are:

(1) Behaviour under extreme conditions of drought or excessive moisture.

(2) Maturity—whether early or late.

(3) Disease-resisting power.

(4) Milling qualities.

(5) Tonnage of canes per acre.

(6) Richness of juice in saccharose.

(7) Purity of juice.

It would be impossible at the outset to consider all these characters, and consequently it would be advisable to work with those which are of greatest value economically.

The essential characters to be considered are resistance to drought, resistance to disease, a larger tonnage of cane per acre, richness of juice in saccharose, and, in some of the northernmost countries, early maturity.

As a result of the previous work done in breeding sugar-canes, it is now obvious that a class of canes has been produced that possess, to a large extent, qualities that enable them to resist certain classes of disease. Most of the newer seedlings possess a thicker cuticle than the older varieties, and are, therefore, more or less immune from the attacks of insect pests; and possibly some physiological reaction within the plant enables it to withstand the attacks of certain fungoid diseases. More, however, requires to be done in this direction, for the root disease, for instance, is one which does a considerable amount of damage in the West Indies, Hawaii, and elsewhere. In Java it is held that a larger yield of sugar depends upon the cane possessing an increased vigour and also greatly upon immunity from disease, and therefore breeding for resistance to disease is one of the first points to be aimed at.

The tonnage of cane per acre is especially a point of great importance. In 1902, Harrison reports that "the results confirm those of previous experiments, that neither the addition of phosphoric acid, of potash, or of lime to the manures favourably affects the sugar-contents of the juice of the canes. The effects of nitrogenous manurings appear to be to somewhat retard the maturation of the canes, and thus the juice of canes manured with them is, as a rule, not so rich in saccharose as is that of canes grown without manure. But this effect is far more than offset by the larger yields of produce resulting from the application of nitrogenous manures and by the fact that the increases produced by the nitrogen are principally due to the development of the stalks in length and in bulk, and not to abnormal increases in the amounts of tops and leaves or the production of new shoots to the stool." Watts[7] and Cousins have shown that different manures influence greatly the yield of crop per acre without appreciably altering the saccharine richness of the juice. Moreover, Cousins,[8] Jamaica, holds that "beyond a certain point—24 per cent. saccharose in the juice—any increase in richness involves a reduction in agricultural yield." He also believes that "the line of development of the sugar-cane as a cultivated plant lies primarily in the direction of increased tonnage of cane, and secondarily in that of greater purity of juice."

As only a few of the varieties now under experiment possess over 20 per cent. saccharose in the juice, maximum productiveness has not been obtained; but nevertheless it would appear that disease-resistance and a larger tonnage of cane per acre, both of which depend largely upon increased vigour of the cane, should receive first attention.

With the view of obtaining some clue to the more prominent characters of the different varieties in Barbados, several arrows or inflorescences were bagged separately before they were ripe to ensure self-fertilisation, and many seedlings have been obtained. As the varieties chosen were hybrids, the records of this second generation should give, on analysis, results that will be of assistance in the subsequent hybridisation work, for the splitting of the different characteristics has been carefully noted.

Many of the previous records of work on the raising of seedling canes show that some varieties possess striking dominant characters, which are transmitted to their offspring. Kobus[9] states that "in some cases the fecundating power of the pollen of the Chunnee variety is so strong that more than 95 per cent. of the hybrids resemble the male parent." The hybrids in Barbados, as might be expected, also show that certain external characters resemble those of one of the parents.

In the experimental work carried on at Barbados on these lines, only those varieties that have stood the stringent tests on a large scale for a considerable time under varying conditions of soils and climate were chosen, as many of the newer seedlings show fluctuating variations when submitted to adverse conditions. Care in securing good parent varieties is of the greatest importance, because the number of varieties which may be kept under trial is limited. After having chosen the variety, it is essential that only the choicest individuals are taken for experimental purposes, for in Java it has been found that the amount of sugar in a cane varies directly with the weight of the cane, and also as a rule heavy plants give rise to heavy offspring.

Once having obtained the desired type of seedling, it will be easy to multiply it to any extent without the necessity of fixing the type by further breeding, as the sugar-cane, on a large scale, is propagated by cuttings, and not by seed.

Classification and an Appeal for Uniformity.

Having reviewed the method of obtaining seedlings and some of the problems for future work, it becomes necessary to discuss some of the results already obtained. Perhaps the best way is to describe the advances made by the various sugar-producing countries separately. Before doing this, however, it will be advisable to notice in passing the methods of naming and classifying the different varieties of canes.

Most of the older writers classified canes according to the countries of their origin; in many cases their true origin was unknown and therefore new names were provided. Subsequently local names were assigned to the same variety, and shortly a confusing number of synonyms was established. In 1890 Harrison and Jenman [10] recorded that, in their collection of the world's canes on the Experiment Stations in British Guiana, the *Bourbon* cane (one of the oldest varieties) was represented under six distinct names, and the *White Transparent* under four. (See fig. p. 328.)

They therefore suggested that a system of classification should be universally devised, and finally concluded that the best and easiest method was to arrange them in groups according to their outward characters.

Five classes were formed:—

(1) Yellowish-green or green, often blotched with red.
(2) White, vinous or brown tinged canes.
(3) Grey or pink tinged canes.
(4) Ribbon or striped canes.
(5) Claret or purple canes.

Stubbs [11] in Louisiana, however, only recognises three classes, as distinguished from the five of Harrison and Jenman, viz.:—

(1) White, yellow, or green canes.
(2) Striped canes.
(3) Solid colours other than in (1).

In comparing these two independent classifications and looking at the synonyms established, it is seen that differences occur, but they show fairly conclusively that the older cultivated varieties of sugar-cane were few in number, and presented only those differences which were due to changes of cultivation, climate, and environment.

Since the advent of seedling canes it has become customary to designate their origin by the letter of the country in which they are raised, with an affixed number, *e.g.* B. 147 (Barbados Number 147), D. 95 (Demerara Number 95), T. 24 (Trinidad Number 24), J. 30 (Jamaica Number 30), &c. Seeing, therefore, that the hybridisation of the sugar-cane is now becoming general all over the tropics, it is essential that some scheme for naming and classification be devised, or else a greater confusion than ever will be the result. All workers, therefore, in the production of seedling canes should see that a letter and a number be affixed to the new seedlings before distribution, and a system of classification, based on colour and other external appearances, be adopted.

If such or any other system were uniformly adopted, it would be easy to compare the results of a given variety when grown under different conditions and in different parts of the world.

Results already Obtained.

INDIA.

Efforts to improve the sugar-cane in India have only recently been made. With the establishment of the Samalkot Experiment Sugar Station in Madras, the cultivation of the sugar-cane under Indian conditions is being carefully studied. Several varieties of canes have been introduced from other countries, and the shipments from Mauritius and Barbados have given good results, the yield of these varieties comparing very favourably with the home canes.

One of the imported Mauritius canes was a ribbon cane called Striped Mauritius, and Barber, in his report on the station for 1904–5, states that this cane has given rise to bud varieties—red and white sports being produced. These sports have been carefully grown and analysed, with the result that the red sports have proved better than any other canes that are grown at the station in respect to richness of juice.

In 1903–4 a number of canes arrowed at the station, and an effort was made to obtain cane seedlings, but without success. In the following year a number of boxes were planted with arrows from different kinds of canes. Two seedlings were obtained from the Mauritius canes, but they lived only for a short time.

Although previous to this time repeated mention of cane seed has been made in different parts of India, no record of the seed being fertile seems to have been reported.

Barber states that these experiments with cane arrows were directed mainly towards the investigation of whether the sugar-cane produced fertile seed in India. This has therefore been shown to be the case, but it is thought that the burning dry air of the Indian climate is unsuitable to the successful raising of seedling canes, and that the cultivation of sports appears to be much more practicable than the raising of seedling canes. The raising of hybrid canes, however, would possibly be a means of combating many of the diseases that cause so much trouble to cultivators of sugar-cane in India.

QUEENSLAND.

The raising of cane seedlings has received some attention in Queensland, as reports to hand state that nine seedlings were obtained from arrows collected in 1889 and five from those collected in 1891. One of these earlier seedlings has given a white sport, referred to previously, which has proved to be the best of all the seedling varieties.

In 1900 a selection of West Indian seedling canes was imported, with the result that last year, at Wellington Point, some gave analyses which compared very favourably with the home seedlings, while the information gathered from the latest reports confirms the value of B. 208 (Barbados No. 208) as a cane for cultivation in Queensland. (See fig. p. 331.)

In 1901 there were obtained by the Queensland Acclimatisation Society 700 seedlings, of which 300 were approved plants, and, in 1903, 170 plants were selected out of 500.

In 1904 experiments in artificial cross-pollination were undertaken, and four hybrids were obtained. These were the results of a cross between B. 208 as seed-bearing parent and Striped Singapore as pollen-bearing parent. This shows that hybridisation is possible, and instructive results are expected to follow.

The following will show that seedlings are giving satisfactory results in Queensland:—In 1903 only one cane gave over 19 per cent. possible obtainable cane sugar, whereas in 1904 six exceeded this amount. How much this had to do with the season cannot be stated definitely, but it seems to point to the fact that in Queensland, as elsewhere, seedling canes may gradually supplant the older varieties.

Grimley states that B. 208 on one estate gave a "return of 69 tons 6 cwt. of cane per acre with 22·2 per cent. of sucrose, and Brix 23·09, or 21·45 per cent. of possible obtainable cane sugar, or over 14 tons to the acre. These results were obtained under irrigation, and the experiment plot was well manured. The average yield in Queensland per acre for the last seven years was 13·16 tons, so that B. 208 gave more sugar per acre than the average tons of cane per acre in Queensland." [12]

HAWAII.

With the establishment of the Hawaiian Sugar Planters' Association, the propagation of new varieties of canes, which are resistant to disease, and at the same time good sugar-producers, was considered to be of paramount importance.

In the season 1904–5 no young canes were obtained from the home-grown seed; but large numbers of seedlings were obtained from seed introduced from Barbados, Jamaica, and Trinidad. In all 279 seedling canes were obtained and planted out; 93 of these were cut up and replanted as cuttings, while the rest were allowed to remain to flower, when it was hoped that a considerable quantity of fertile seed would be obtained.

Artificial cross-pollination experiments were conducted last season, but so far the results are not known to the authors of this paper.

The introduction of foreign varieties is largely practised, seedlings from Demerara, Barbados, and Queensland have been introduced, and it is stated that "D. 117 holds the lead among the recently introduced varieties, and is a promising cane, worthy of trial under the diversified conditions of the island." Among other very promising seedling canes are B. 147, B. 156, B. 208, D. 145, and Q. 1.

LOUISIANA.

Owing to the shortness of the growing season, which is limited to about eight months on account of frosts, the home canes in Louisiana rarely arrow. Seedlings from these home canes have not been obtained, and therefore planters have to rely upon imported varieties. Seedling canes from Demerara, Barbados, Jamaica, and Queensland have been imported, and submitted to trial on the Experiment Station. A large number were found to be unworthy of recommendation to the planters, others are still under experiment, and two of the Demerara seedlings, viz. D. 74 and D. 95, have surpassed all the home canes.

D. 74 is a tall, green, erect cane with long internodes, long and deep roots, ratoons (*i.e.* sprouts for second crop) well, and has a large sugar-content. The individual canes are large and heavy. (See fig.)

D. 95 is a large, purple, erect cane with long internodes, long and deep roots, ratoons well, has a large sugar-content, and large individual stalks.

Blouin reports that both these canes are very hardy, mature early, and that their erect habit renders them better able to withstand storms and makes them more easy to harvest. (See fig.)

During 1905, D. 74 arrowed in Louisiana, this being the first seedling that has flowered in that State.* From this it may be inferred that this cane is one which quickly matures. If it matures while the older varieties remain immature, and gives a high sugar-content, it should prove to be a valuable cane to sugar planters in Louisiana. The planters fully appreciate the value of these varieties, as it is estimated that nearly four-fifths of them have introduced one or both of the Demerara seedlings into their cultivation, and if these canes continue to flourish nearly two-thirds of Louisiana's cane area will be planted with them in two or three years' time.

MAURITIUS.

A large number of varieties of canes are grown in Mauritius, amongst which are two sports of the Striped Tanna that have been submitted to extensive trial. The White Tanna is whitish and resembles the parent cane in many respects, and is now held in favour. The Black Tanna, also a bud variety of the striped cane, presents many characters of the parent cane, but is not extensively grown.

Seedlings were successfully raised shortly after the discovery of fertile seed in Java and Barbados, a large number of which were distributed to estates. These seedlings gave such good results that managers frequently started seedling nurseries of their own, and much confusion in nomenclature followed.

* Since this paper was written it has been announced that seedling canes have been successfully raised for the first time in Louisiana (*Agricultural News*, Barbados, vol. v. p. 307).

D. No 74.

D. No 95.

90 6 06

Weller & Graham Ltd Lith. London.

The first seedlings grown were chosen haphazard, but eventually various systems were evolved, such as planting in alternate rows and bagging the arrows on the chance of getting fertile seeds.

It is also interesting to note that as early as 1899 a method of what may be called natural hybridisation, by which several hybrids have been obtained in the West Indies, was fully discussed by Boname,[13] but was thought to be impracticable on a field scale. It was suggested that the inflorescence be enclosed in a muslin bag when quite young, and then the inflorescence of another be introduced when its flowers were ready for pollination. No record can be found of this method being practised in Mauritius.

The raising of seedlings in Mauritius appears to have centred around the collection of the arrows from their best varieties. The Big Tanna, which is one of their most vigorous canes, has received considerable attention, and a large number of seedlings have been obtained from it. Although many of these seedlings have proved to be worthless and others have shown great fluctuations, yet a considerable number has been produced, some of which not only show a greater saccharine content than the other varieties, but also a greater resistance to disease, and consequently give a larger yield of sugar per acre than most of the older varieties.

JAVA.

The raising and cultivation of seedling canes have been taken up to a considerable extent in Java, on account of their comparative freedom from disease. After the discovery of fertile seed of the sugar-cane in 1887, many of the larger planters cut the cane arrows, planted them, and raised large numbers of seedling plants. From these they selected such as had a high saccharine content and showed themselves able to resist disease for planting on a large scale, and then finally selected those which were best suited to their estates.

Owing, however, to the insufficiency of the trials before introduction into the general cultivation, much distress was incurred, and therefore planters began to look to the experiment stations for selected seedling canes.

In 1894, Wakker, the director of the East Java Experiment Station, discovered that the Cheribon cane bore infertile pollen while the ovary was normal. Bouricius crossed the Cheribon with the Fidji, and later Kobus crossed it with the Chunnee, one of the imported East Indian canes, for this showed a large proportion of fertile pollen. The two chosen varieties were planted alternately in rows in order to obtain natural cross-pollination. A very large number of seedlings was obtained by sowing seeds from the "self-sterile" arrows of the Cheribon, many of which combine the high sugar-content of the Cheribon with the disease-resisting power of other selected varieties.*

All the resulting seedlings are tested for four years in the station before being recommended for general cultivation. In this way a race of hardier

* In 1905, over 16,000 seedlings were raised at the East Java Experiment Station. Of these, the parentage of 7,170 was known on both sides, for they were produced by the above method, and that of 7,460 others was known on one side only.

canes has been established, and the sugar-content has not been noticeably decreased although one of the varieties used as a parent was rather low in percentage of saccharose.

The choice of the Chunnee variety as one of the standards to be used for crossing purposes has even been more valuable than the experimentalists dared at one time to hope; for, of all the seedlings at the experiment stations, those that are the descendants of the Chunnee are least subject to root-disease, as well as to other maladies. They are, however, somewhat hard, which is an inconvenience for crushing purposes, but it is not thought that this property is undesirable as it is counterbalanced by others that are useful.

Efforts are now being made to raise other races of plants: one—a more hardy race of seedlings—by crossing those seedlings already obtained with the immune variety, Chunnee; and the other—a richer race of seedlings—by crossing seedling canes with the Cheribon, and also with other seedlings.

Although the results are not coming out exactly as anticipated, an examination of the following table will show that considerable improvement has been made.

The contents of the following table have been extracted from that given by Kobus [14] in 1905, embodying the experimental tests with the different varieties of seedling canes at the East Java Experiment Station. The figures given by Kobus have all been converted into English units, so that they may be used for comparison with the results obtained in the West Indies. It illustrates clearly how the yield of many seedling canes is much better than that of the standard variety—Cheribon.

TABLE III.

No.	Soil	Tons of cane per acre	Per cent. pure sugar in cane	Lbs. sugar per acre
Cheribon	Light.	37·9	11·71	9,928
146	,,	62·8	13·55	19,085
213	,,	62·9	13·34	19,250
247 B.	,,	70·1	11·54	20,394

By this it can be seen that many of the seedling varieties give an estimated yield of sugar per acre of about double that given by the old standard variety.

So far, no records have come to hand from Java to show that hand cross-pollination has been successful, but now that it has been shown to be possible in several different countries there can be no reason why the raising of hybrid sugar-canes under control should not be as possible in Java as elsewhere.

CUBA.

Experiments have been conducted with the introduction of standard varieties and seedlings from Java, Queensland, and the British West Indies. After considerable testing, many of these are being introduced into the general cultivation. B. 208 has been giving excellent results both in percentage of saccharose and in purity of juice.

Four years of careful hybridisation resulted in but two seedlings, but during the last year (1905–6), owing to a favourable season, over 600 seedlings have been obtained by Atkins[15] at the Harvard Experiment Station, and nearly all these are the result of hand cross-pollination. Emasculation was effected during early morning when the anthers were full-grown but unexpanded, and pollination was continued for several days, the spikelets being kept under gauze cloth. It is moreover shown in his report that great care must be taken with the germination of the seeds, much depending upon the soil used, on the depth to which they are set, and on the watering.

This report is, without doubt, a valuable one, as it shows conclusively that, with a favourable season, seedlings of the sugar-cane can be obtained in large quantities as the result of cross-pollination.

BRITISH WEST INDIES AND BRITISH GUIANA.

Since the establishment of the fact in 1887 and 1888 by Soltwedel in Java and Harrison and Bovell in Barbados that the sugar-cane at times does bear fertile seeds, systematic attempts have been continued in the West Indies and British Guiana towards the raising of improved races of seedling canes. All the different methods of selection before referred to have been adopted, with the result that thousands of seedlings have been raised, from which a few good ones have been chosen and recommended to planters for trial. It was thought, however, that it was essential to select both parents, and the various methods to ensure the crossing of the chosen varieties were given an extended trial. The method of planting in alternate rows of varieties that had practically unisexual flowers, which has given such good results in Java, has been experimented with, but, owing to the success of Lewton-Brain in 1904 in Barbados in obtaining seedlings by hand cross-pollination, it is now held that artificial hybridisation of the sugar-cane is practicable and ensures the best results in the shortest possible time.

Having briefly referred to the methods adopted for the raising of seedling canes in the West Indies, some of the results already obtained may be reviewed in order to show what improvement has been made. The "Bourbon" cane was at one time the standard cane of the West Indies, but, owing to fungoid diseases, its cultivation had to be given up, and other varieties substituted in its place. In Barbados the cultivation of the Bourbon cane has been entirely abandoned, and another variety, the White Transparent, has taken its place as the standard cane.

Barbados.—Thousands of seedlings are raised yearly in Barbados from the planting of the arrows from the better varieties, and these are submitted to rigorous selection on the tonnage of canes per acre and the chemical analysis of the juice. During the last five years in Barbados over 20,000 seedling canes have been raised and planted out, but less than 1 per cent. of these have stood the stringent tests of field and chemical selection applied to them. In the season 1904–5 over 7,000 plants were raised from seed, and out of these only 95 were considered worthy of further propagation. It may be urged that a large number of seedlings are in this way wasted every year, but it is held by Bovell that, owing to the limited extent of the experimental grounds, it is necessary to limit the cultivation to seedlings that give an estimated yield of 30 tons of canes per acre and a saccharine content of over 18 per cent. This year, 1906, about 5,000 seedlings have been planted out, from which it is not expected to choose more than 100 for further propagation, and it is doubtful whether more than one of these will prove worthy of recommendation for planting on a large scale.

Work on these lines has been continuously pursued in Barbados since 1888, and the following tables of results, extracted from the reports recently issued by d'Albuquerque and Bovell on the experiment work with sugar-cane under the direction of the Imperial Department of Agriculture, show that many of these seedling canes give results vastly superior to the standard variety.

TABLE IV.

MEAN RESULTS—BLACK SOILS, FOR SEASONS 1900–5.

Cane	Canes. Tons per acre	Per cent. of rotten canes	Sacch. lb. per gall.	Q. of purity per cent.	Sacch. lb. per acre	Musc. yield. Tons
B. 1529 (1904–5)	28·92	1·54	2·406	92·18	8,477	3·03
B. 147 (1900–5)	28·35	3·77	1·912	86·88	7,006	2·50
B. 208 (1900–5)	24·72	4·93	2·250	90·70	6,863	2·45
White Transparent (1901–5)	25·22	5·99	2·038	89·70	6,453	2·30

TABLE V.

MEAN RESULTS—RED SOILS, FOR SEASONS 1900–5.

Cane	Canes. Tons per acre	Per cent. of rotten canes	Sacch. lb. per gall.	Q. of purity per cent.	Sacch. lb. per acre	Musc. yield. Tons
B. 1529 (1904–5)	27·12	1·67	2·270	93·79	7,428	2·65
B. 208 (1900–5)	26·78	5·52	2·146	91·23	6,695	2·39
White Transparent (1901–5)	22·24	4·93	1·979	90·09	5,404	1·93

It will be seen by the above tables that B. 1529 gave an average, in both red and black soils, of 2,024 lb. of sugar per acre more than White Transparent, while B. 208, a cane which has lately become extensively cultivated in different parts of the West Indies and elsewhere, gave a yield of 410 lb. in black soils and 1,291 lb. of sugar per acre in red soils more than the standard variety.

These tables have been prepared as they give the results of experiments over an extended number of years; but if the table, published in the report, which embodies the results of different plots of new varieties for 1903–5, be examined, it will be found that White Transparent comes out eightieth on the list of those cultivated in black soils, while the Bourbon is still lower. (See fig.)

It has often been urged that these results are based upon small plots, which do not furnish a sufficient quantity of cane for the tests to be of value to sugar planters; but tables are also given in the above-mentioned report which show that seedlings B. 147 and B. 208 are giving better

Bourbon. White Transparent.

300 6 06

[illegible] Lith. London

results than White Transparent when grown on an estate scale. These tables have been furnished through the courtesy of Mr. Cameron, and embody the results obtained on certain estates in Barbados under his direction, on which canes of different varieties have been grown, and show comparisons between 693½ acres of B. 147, 33 acres of B. 208, and 411 acres of White Transparent for the seasons 1903–5.

Jamaica.—Cousins, in his report on the work of the sugar experiment station in Jamaica for 1905, states that some very good seedling canes, resulting from naturally cross-fertilised seed, have been produced and are being submitted to a rigid selection.

About 3,000 seedlings are now being grown each year in Jamaica; therefore a series of Jamaica seedlings worthy of trial on an estate scale should soon be available.

In the trials of the imported varieties, B. 208 gave a tonnage of 65·5 tons of canes per acre, and is being recommended to planters "as the most promising seedling cane at present grown in Jamaica."

The author of the report also points out that about 100,000 plants of selected varieties were distributed during the past year, which clearly shows that the planters of Jamaica fully appreciate the introduction and trial of new varieties of canes.

Leeward Islands.—The results recently issued by the Imperial Department of Agriculture for the West Indies on the work carried on by Watts at Antigua show that B. 208 gave an average yield of 9,347 lb. saccharose per acre in plant canes and 5,001 lb. in ratoons, against 7,014 lb. in plant canes and 4,265 lb. saccharose per acre in ratoons of White Transparent. In St. Kitts, B. 208 gave an average yield per acre of 8,675 lb. saccharose in plant canes and 6,648 lb. in ratoons, against 7,014 lb. saccharose in plant canes and 5,861 lb. in ratoons of White Transparent, while B. 147 gave a yield of 7,133 lb. in plant canes and 6,174 lb. in ratoons.

As these figures are the mean results of a large number of plots carried on for four years in plant canes and for three years in ratoons in Antigua, and for five and four years respectively in St. Kitts, it shows that seedling canes are of considerable economic value to planters in the Leeward Islands.

Demerara.—In Demerara, up to the beginning of 1905, nearly one-third of a million of seedling canes had been raised by obtaining seed from good standard varieties, and 26,000 of these had been selected for field experiments. Harrison, at the last West Indian Agricultural Conference (1905), stated that 14,800 acres were under cultivation with varieties other than Bourbon, and of these about 13,000 acres were occupied by new seedling varieties, the favourite ones with the planters being D. 109, B. 147, D. 145, D. 625 and B. 208. It is estimated that D. 145 bears a ratio to the Bourbon in respect to saccharose yield per acre as 170·8 is to 100.

At the end of 1905, the area under cultivation in varieties of canes other than Bourbon extended to 18,000 acres, and as opportunity offers further extension is being undertaken. This is nearly one-fifth of the acreage under cane cultivation in British Guiana, and shows that planters have been ready to appreciate what has been done for them in the matter

B 147. B 208.

390. 6 06. Weller & Graham L^td, Litho, London.

Only those canes which had stood the strongest tests on a large scale for a number of years were used in the experiments. Over 600 spikelets were emasculated and artificially pollinated, of which over 400 were spikelets of B. 147 and B. 208. (See fig.)

The results of this work have not been satisfactory, as an unfavourable season with windy showery weather destroyed all chances of good success.

Some further particulars of the results obtained by Lewton-Brain in 1904 in Barbados may be interesting. He experimented with some of the best Barbados varieties as the parent plants, and as a result obtained five hybrids of known pedigree. These have been carefully grown, and although it is impossible at present to say what their commercial value will be, yet it may be interesting to record a few external features that have been noticed during the growing season. The pedigree seedlings consist of the following:

(1) Three holes of B.H. 1; cross between B. 1376 ♀ × B. 1529 ♂
(2) One hole of B.H. 15; „ „ B. 3289 ♀ × B. 1529 ♂
(3) One hole of B.H. 18; „ „ B. 3289 ♀ × B. 1355 ♂

Description of Parents.

In the following description of the varieties used in hybridising only the more important characteristics are noted, and are chiefly those which can be used in comparing with the descriptions of the hybrids:

B. 1376: Germinating power good; colour dull yellowish-green; habit of growth more or less recumbent; internodes cylindrical; eyes round; dried leaf-sheaths fall readily; disease-resistance fair.

B. 1529: Germinating power under average; colour red; habit of growth upright; internodes variable, but generally roundish; eyes round; dried leaf-sheaths somewhat adherent; disease-resistance good.

B. 3289: Germinating power fair; colour yellowish-green; habit of growth recumbent; internodes cylindrical; eyes round; dried leaf-sheaths fall readily; disease-resistance very good.

B. 1355: Germinating power fair; colour red; habit of growth generally upright; internodes variable, but generally roundish; eyes round; dried leaf-sheaths fall readily; disease-resistance fair.

Description of Hybrids.

Cross 1.—B. 1376 ♀ × B. 1529 ♂: Owing to some differences in the three holes of the cross B. 1376 ♀ × B. 1529 ♂, it has been proposed to cultivate them separately under different nomenclature. The following are the characters:

B.HH. 1 = B.HH. 3: Colour yellowish-green; habit of growth recumbent; internodes roundish; eyes round; dried leaf-sheaths somewhat adherent; disease-resistance fair.

B.HH. 2: Colour yellowish-green; habit of growth upright; internodes variable; eyes round; dried leaf-sheaths fall readily; disease-resistance fair.

All the canes from this cross were yellowish-green in colour, thus resembling the seed-bearing parent B. 1376, and not B. 1529, which is a red cane. The canes of two holes of this cross were recumbent in habit of growth, taking somewhat after B. 1376, while the canes of the other hole were upright—a characteristic of B. 1529. The canes were all above average size, therefore resembling B. 1376 rather than B. 1529, which is a thinnish cane, but they possessed internodes which resembled closely those of B. 1529. Two-thirds of the canes also resembled B. 1529 in that they had leaf-sheaths which were somewhat adherent to the stem.

Cross 2.—B. 3289 ♀ × B. 1529 ♂ =B.H. 15 : Colour yellowish-green; habit of growth upright; internodes roundish; eyes round; dried leaf-sheaths somewhat adherent.

The canes of this cross died early through the effects of the excessive drought that was experienced, and therefore the characteristics could not be closely followed.

Cross 3.—B. 3289 ♀ × B. 1355 ♂ = B.H. 18 : Colour yellowish-green; habit of growth slightly recumbent; internodes variable, but generally roundish; eyes round; dried leaf-sheaths adherent; disease-resistance fair.

The canes of this cross were drought-resistant, and resembled in colour and habit of growth B. 3289, in the shape of internodes B. 1355, but differed from both parents in possessing adherent leaf-sheaths.

Owing to the unfavourable season during this last year, it was thought advisable to cut up all the canes available from these crosses and not to submit any of them to chemical analysis, and therefore it is impossible at present to say what will be the commercial value of these canes. During this next year the characters of the hybrids will again be closely followed and recorded, in order to see if any of them are variable.

Self-fertilised Seedlings.

In 1904 several arrows of the better varieties were also bagged to obtain self-fertilised seedlings, in order to investigate, if possible, some of the dominant characteristics of our different varieties of sugar-cane.

B. 1529 gave 42 seedlings, which showed the following variations: *

Weight of canes per hole	. extremes .	5 lb. to	47 lb.
Saccharose per gallon .	. ,,	1·256 lb. ,,	2·398 lb.
Glucose per gallon .	. ,,	·028 lb. ,,	·139 lb.

It also showed that its red colour was a recessive character, a fact which is borne out by the seedlings obtained by the cross between it and B. 1376. It might also be thought that its upright habit is also recessive, for the self-fertilised seedlings presented habits recumbent to upright in the ratio of 3 to 1. One of its dominant features is the inherent richness of its juice, a fact already noticed when compared with the analysis of the juices of other seedlings grown under similar conditions.

B. 1376 gave 27 seedlings that also varied considerably, as may be seen by the following table: *

Weight of canes per hole	. extremes .	8 lb. to	50 lb.
Saccharose per gallon .	. ,, .	1·196 lb. ,,	2·015 lb.
Glucose per gallon .	. ,, .	·039 lb. ,,	·156 lb.

* The figures were obtained from Professor J. P. d'Albuquerque, Chemist in Charge of Sugar-cane Experiments, and Mr. J. R. Bovell, Agricultural Superintendent, Barbados.

It is impossible at present even to speculate upon its various characteristics, as the seedlings were so varied, but most of them were yellowish-green in colour and somewhat recumbent in habit of growth.

In all, 69 self-fertilised seedlings have been investigated, and therefore it may be held that the results above given have been deduced from a very small number, but it clearly shows that much can be learnt about the inheritance in the sugar-cane by inquiring into the dominant and recessive characteristics of the different varieties, and then it may be possible to build up an ideal cane.

Other Countries.

Although, in other countries, seedling canes have not been raised systematically, yet records show that introduced seedlings are giving satisfactory results in all places where they are cultivated.

In Pernambuco, Brazil, seedlings were first attempted to be raised in 1890 [17], and in 1899 it was reported that a seedling cane was giving excellent returns. It at first was immune from the "gumming" disease, but after cultivation for some time became more or less liable to attacks of this disease. Since then other seedlings have been produced, which possess a greater immunity from disease.

In Natal, West Indian seedlings, B. 109 and D. 95, sent from Antigua, have made satisfactory growth and are being cultivated on increasing areas throughout that colony.[18]

In Fiji, it is stated by Knowles, in his reports during 1905, that 35 acres of different varieties of canes are being grown for trial and for hybridisation experiments. This is possibly the first time that such experiments have been conducted in Fiji, and good results are being looked for.[19]

In Martinique, many of the West Indian seedlings, as well as many home seedlings, are giving larger yields of sugar per acre than the standard varieties.[20]

In Réunion there are large numbers of different varieties of canes under cultivation, but no mention of systematic attempts at raising seedlings can be found.[21]

General Conclusions.

In conclusion, it must be held, after careful examination of the various results, that the production of new varieties of canes by selection and hybridisation has proved a valuable means of improving the quality of the sugar-cane. The experiments carried on in the West Indies are most encouraging, for it has been shown that not only are the seedlings more resistant to certain classes of diseases through their increased vigour and growth, but that they also give a larger yield of sugar per acre; and the results from Java, Hawaii, Queensland, Louisiana, and elsewhere all confirm those obtained in these islands.

The success of the results already obtained should stimulate workers in this subject for greater efforts in the production of new races of canes, for it is not only necessary to improve the productiveness of the plant, but it is essential that races of greater disease-resistance be raised—for whereas many of the seedlings at present are immune to one disease, they are more or less susceptible to another—and also that a large number of varieties be at the disposal of cane-planters, owing to the great differences in climate and soils of cane-producing areas.

That climate and soil are the paramount influences exerted in the sugar-producing capacity of different varieties has clearly been shown by the difference in yields and other characteristics manifested by the same cane in different localities.

Therefore, following the example of European beet-growers, who think that the practice of persistently growing their crops under the same conditions of soil and climate is a mistake, the seedling canes are distributed in experimental plots on widely different areas and under different conditions. The seedlings are also grown in competition for a number of seasons before any definite conclusions are drawn as to their relative value, owing to the varying time of their maturity, the rapid deterioration of over-ripe canes, and the varying germinative power of the seed cuttings.

Whereas considerable improvement has been made by selection and natural hybridisation, it is expected that hybridisation under control should give desired results more rapidly, for by the careful choice of parents it is hoped to combine some of the good qualities of both parents in the offspring.

The chief difficulty against obtaining large numbers of hybrids has been due to the small size of the flowers and the general habit of growth, but by careful manipulation, as described in the "West Indian Bulletin," vol. v. pp. 362–3, and vol. vi. pp. 394–402, these difficulties can be surmounted, and good results should follow in seasons favourable to hybridisation experiments.

The increasing fertility of the newer seedlings—as shown by the fact that recently nearly 1,000 seeds from a single inflorescence have been known to germinate, whereas a few years ago thirty to fifty was the greatest number recorded—makes it probable that many of the difficulties that have previously kept this work in check will sooner or later be overcome.

Probably the greatest improvement in the future will result from first analysing the different characteristics of the varieties to be used as parent canes, by raising large numbers of self-fertilised seedlings and then building up an ideal cane, which will stand the rigorous tests of field selections and analyses in the laboratory. In the carrying out of this work great variations will be noticed, owing to the hybrid origin of the varieties to be used for crossing purposes; but then, by raising large numbers of self-fertilised seedlings, the heredity value of the parent varieties may be learnt from careful analysis of the offspring. In other words, an examination of varieties of canes for the so-called "centgener power" of Hays may be of practical importance.

In short, "the great expectations once held of seedling canes may not have been realised," yet "the greatest hope for the future lies in the expectation that it may become increasingly practicable to raise canes of definitely known parentage from carefully selected plants possessing to the greatest degree the characteristics of disease-resistance, high sucrose yield, heavy tonnage of cane, and the other properties which have been previously mentioned as marking a sugar-cane of high economic value."[22]

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The President: It is very satisfactory to know that our own Government is to some extent awakening in these matters; and we can only express the hope that when they have finished their operations in the West Indies they may find some little time and resource to devote to somewhat similar purposes in the old country.

THE BREEDING OF OATS, BARLEY, AND WHEAT.

By Professor C. A. Zavitz, Ontario Agricultural College, Guelph, Canada.

The fact that this international convention on hybridisation and plant-breeding, representing more than a dozen of the different countries of the world, is now being held in London gives some indication of the interest which is being taken in the study of heredity. There is probably no subject which is engaging the attention of the scientific world to a greater extent at the present time than that of breeding. It is a subject among subjects, and one which is deserving of deep thought and thorough, painstaking, and persistent investigation. Not only does it concern the affairs of the world from a financial standpoint, but it has an influence on life everywhere, whether of animals or of plants. May the discussions which are taking place at this international convention stimulate students in different parts of the land to assist more and more in the work of obtaining that information which will enable the people everywhere to understand better the principles of breeding, so that improvements can be made in those forms of life which come so much under the influence of man.

That phase of the general subject of plant-breeding allotted to me for discussion deals more particularly with the breeding of cereals. I shall discuss the subject under four principal headings, namely, the selection of varieties, the selection of seed, the selection of plants, and the production of hybrids.

Selection of Varieties.

I am convinced that the proper selection of varieties of cereals is of great importance, not only from the farmers' standpoint, but also as a part of the work in the improvement of cereals by plant-breeding. After growing, examining, and ascertaining the comparative productiveness of over two thousand varieties of farm crops in each of five years, and then observing the behaviour of some of those varieties in general cultivation, I wish to emphasise strongly the importance of variety in this work. Certainly great differences exist between different varieties of grain crops—in length of straw, strength of straw, date of maturity, susceptibility to rust, productiveness, quality of grain, and in many other respects. As some breeds of live stock have been bred for many years to fulfil certain purposes, so have many varieties of farm crops been raised for long periods of time with different objects in view. Some varieties are well adapted to rich loamy soils, others to heavy clay land, and still others to soil of a light character. For instance, in Ontario it would be unwise to grow the Joanette oats on a light weak soil or the Black Tartarian oats on a rich bottom land. It would be equally unwise to grow the

White Wonder peas on a poor soil or the Prussian Blue variety on land which naturally produces a large amount of straw. The best results could not be expected from growing the 'Turkey Red' wheat where the crop is apt to lodge or the 'Black Hulless' barley on rich alluvial soil. Decidedly better results can be obtained from growing the 'Longfellow' corn in Northern Ontario and the 'Leaming' variety in Southern Ontario than if this order were reversed. Much greater yields can nearly always be expected from the 'Siberian' than from the 'Black Tartarian' oats, from the 'Dawson's Golden Chaff' than from the 'Surprise' winter wheat, from the 'Mandscheuri' than from the Common Six-rowed barley, from the 'Early Britain' than from the 'Golden Vine' pea, from the 'White Wonder' than from the common small white field bean, &c. In regard to the yield per acre, there is a very great difference in different varieties. For instance, we have grown a number of varieties of oats and barley under uniform conditions in each of sixteen years, and, for the sake of illustration, I wish to say that the results of these experiments show an average yield of grain per acre of 88 bushels from the 'Siberian' and of 72 bushels from the 'Black Tartarian,' a difference of 16 bushels per acre per annum in favour of one variety over another. Again, in the case of barley, the 'Mandscheuri' gave an average of 70 bushels and the 'Mensury' an average of 59 bushels per acre during the same period of sixteen years. Here we have a difference of 11 bushels per acre per annum between two strains of the six-rowed barley. When we see such marked differences between varieties we are led to the conclusion that variety has an important place in the work of plant-breeding as well as in the practice of the farm. Professor Hugo de Vries, of Holland, after visiting the noted Luther Burbank in 1904, wrote an account of his trip, and among other things he stated that "as a general rule, it holds true that the results of crossing depend primarily on the selection of the varieties used for that purpose. These indicate the list of possibilities from which the choice and the combinations have later to be made. Outside of this list very little good is obtained, and then only by accident. This occurs very seldom in Burbank's cultures."

Selection of Seeds.

Within the past twelve years we have done a large amount of very careful work in order to determine the influence of different selections of seed upon the resulting crop. Fresh seed has been taken each year from the general crop of grain grown on the farm. The results, therefore, represent simply one year's influence from seed selection; but in order to obtain reliable data the experiments have been repeated from season to season in order to secure a good average of conditions of soil, temperature, and rainfall. For the large seed none but well-developed grains were selected; for the medium-sized sample the grains selected were of a uniform character, plump, and of medium size; and for the small none but sound, plump, and apparently good seeds of small size were used. In the selection of large plump grain ½ lb. of each class was carefully weighed and counted. A corresponding number was then taken of the medium-sized and of the small plump grains. The different selections were sown upon plots of similar size. The following table gives the average results obtained from the different selections :—

Crops	Number of Years of Tests	Yield of Grain per Acre from		
		Large Seed	Medium-sized Seed	Small Seed
		Bushels.	Bushels.	Bushels.
Oats	7	62·0	54·1	46·6
Barley	6	53·8	—	50·4
Spring Wheat	8	21·7	—	18·0
Winter Wheat	6	46·9	—	40·4

From the figures here presented in tabulated form it is most interesting to observe the marked influence of one year's selection of seed on each of the crops here enumerated. The large well-formed seeds produced stronger and more vigorous and more productive plants.

In other experiments along similar lines we have obtained better results from plump as compared with shrunken seed, from sound seed as compared with that which was injured in the process of threshing, from grain which was perfect in comparison with that which had sprouted in the field, and from seed which was thoroughly ripened in comparison with that which was harvested while it was still immature.

An interesting experiment has been conducted for thirteen years in succession in a systematic selection of seed oats. The selections were made with large, plump, black seeds and also with light-weighing and light-coloured seeds. The test was commenced in the spring of 1893 by selecting seed from the general crop of the Joanette black oats of the previous year. The selection made in each of the following years was from the product of the selected seed of the previous year. The selections each year were composed of an equal number of grains, and the seed was sown on plots of uniform size. As the selection for this experiment has been continuous, selecting the seed each year from the crop produced in the year previous, the *average* results are of but little value, but the *yearly* results are interesting, valuable, and quite suggestive. In the crop produced in 1905 it was found that the large plump seed produced 65·5 bushels and the light seed 44·7 bushels per acre. In each of the past few years the results have been much the same as those for 1905. In weight per measured bushel the crop produced from the large plump seed weighed 35·5 lb. and that from the light seed 24·3 lb. It is interesting to notice that the crop produced from the large plump seed required only 1,149 grains to weigh an ounce, while the crop produced from the light seed required 2,066 grains to make the same weight. It will be seen from the results here presented that the selection of the seeds themselves has an influence on the production of the crop, and should form a factor in the process of breeding.

Selection of Plants.

In the spring of 1903 some very choice grain of six varieties of oats, barley, and spring wheat was selected from the crops grown at the

Ontario Agricultural College in 1902. Of each of these six varieties one-sixteenth of an acre was sown with a grain drill by using every second tube of the drill; one-sixteenth of an acre was sown with a grain drill by using every tube of the drill; one-sixteenth of an acre was planted by hand, placing the seeds eight inches apart both ways; and one-sixteenth of an acre was planted by hand, placing the seeds one foot apart each way. It will therefore be seen that one and one-half acres were devoted to this work in 1903. No less than 9,972 seeds of each variety, or a total of 59,832 seeds of the six varieties, were planted by hand. The four methods of planting were used in order that a comparison might be made as to the best method to use in plant selection. It was found that the grain which was sown with a grain drill, either from every tube or from every second tube, gave a very poor opportunity for plant selection. From grain sown with a drill heads may be selected, but it is practically impossible to make a satisfactory selection of plants owing largely to the uneven distribution of the seed. When plants are grown at unequal distances apart they vary greatly owing to the relative amount of soil, moisture, and air furnished the individual plants owing to the uneven manner in which the seeds are distributed in the soil. On a careful examination of the plants obtained from the drilled seed it was found that some of them would be separated from all other plants by ten or twelve inches; while in other cases two or three plants would be growing so closely together that their roots and stems would become so much entangled that it was difficult to ascertain whether there was simply one plant or whether there were two or three or four plants, until a considerable amount of time and labour was expended in making the examination. It was therefore decided to make a few selections of heads, but not to make a selection of plants from the crop produced from the seed sown with a machine. The grains which were sown by hand, however, gave an excellent opportunity for the plants to grow under uniform conditions. As all the plants in each of the two methods of hand planting were at equal distances apart, it afforded an excellent opportunity for studying the stooling properties, the comparative strength of straw, and the size and uniformity of the heads &c. of the individual plants. When the crops of each variety on the hand-planted plots had reached the proper stage of maturity, careful examinations were made and the results recorded for reference. After this had been done a few of the very best plants were selected and harvested separately. All of the seed of the most promising plant of each variety was sown in the spring of 1904, and nearly all the grain produced in 1904 was sown in the spring of 1905. A number of the other choice plants of each variety was also selected and harvested separately, and afterwards the best seed was selected and sown in single rows in the spring of 1904. From those strains which gave the best satisfaction in 1904 a sufficient amount of seed was selected and sown on uniform plots in the spring of 1905, and the yield and the quality of the crops produced were carefully recorded. The results so far are encouraging. A statement of a few of the records is here given.

Increased Stooling Properties.—The crops grown from the seeds planted one foot apart each way showed the following average number of heads per plant from the selected seed in 1903, and from the seeds produced from the selected plants in 1904 to be as follows:—

Crops	Average Number of Heads per Plant	
	1903	1904
Six-rowed Barley (Mandscheuri) . . .	10·8	13·5
Two-rowed Barley (Chevalier)	26·3	31·7
White Oats (Siberian)	13·6	18·4
Black Oats (Joanette)	27·6	46·9

As the seeds were planted exactly the same distance apart in each of those two years, it is quite probable that the influence of the selection made in 1903 is largely the cause of the increase in the average number of heads per plant in the crop of 1904 as compared with that of the previous year.

Improved Strains of Leading Varieties of Spring Grain.—Upwards of one hundred selected strains of leading varieties of winter wheat and spring grains were grown in the experimental grounds on uniform plots in 1903. Fifty-six of the plots contained selected strains of spring crops described previously. Some of these strains are promising, as they indicate a greater yield of grain per acre than was obtained from seed produced from plants which had not been specially selected. The table which follows gives the highest yields per acre obtained in 1905 from seed resulting from the plants selected in 1903 as previously described. In comparison with these yields are those produced from selected seed from plants which were not specially selected.

Crops	Tons of Straw per Acre from Selected		Bushels of Grain per Acre from Selected	
	Seeds	Plants	Seeds	Plants
Six-rowed Barley (Mandscheuri) .	1·8	2·0	68·4	78·5
Two-rowed Barley (Chevalier) .	2·1	2·4	44·8	58·6
Hulless Barley (Guy Mayle) . .	1·6	2·0	47·3	48·6
White Oats (Siberian)	2·3	2·1	86·1	91·3
Black Oats (Joanette)	2·1	1·9	79·3	89·0
Spring Wheat (Wild Goose) . .	1·4	1·8	29·7	36·4

Although there is a slight irregularity in the yield of straw per acre, it will be seen that in every case the yield of grain from seed obtained from selected plants was higher than that produced from seed obtained from plants which were not selected.

The Production of one Seed Grain in a Period of two and a half Years.—As previously stated, the most promising plant of the thousands of plants of each of six varieties of spring grain grown in 1903 was saved and the seed produced was all sown by hand in 1904, from which crop the grain was carefully saved and was sown with an ordinary grain drill in the spring of 1905. The following table represents the yield of grain in 1903, and the yield of both straw and grain in 1904 and in 1905:—

Crops	Yield of Grain, 1903	Yield of Crop, 1904		Yield of Crop, 1905	
		Straw	Grain	Straw	Grain
	oz.	lb.	lb.	lb	lb.
Six-rowed Barley (Mandscheuri) .	2·3	148	68	2887	1629
Two-rowed Barley (Chevalier) .	2·4	112	56	3265	1119
Hulless Barley (Guy Mayle) . .	2·9	184	98	2178	2109
White Oats (Siberian) . . .	1·6	171	61	3553	2102
Black Oats (Joanette) . . .	1·2	196	74	8748	3439
Spring Wheat (Wild Goose) . .	1·0	46	15	542	241

From these results it will be seen that we obtained in 1905 fully 101 bushels of Joanette oats, 61 bushels of Siberian oats, 40 bushels of Mandscheuri barley, 35 bushels of Guy Mayle Hulless barley, 27 bushels of Chevalier two-rowed barley, and four bushels of Wild Goose spring wheat, as the direct result in every case from one seed planted in the spring of 1903. When we realise the fact than one single grain of the Joanette oats planted in the spring of 1903 produced over 100 bushels of grain in 1905, we learn something of the importance of securing even single grains of the highest possible value. In comparison with 100 bushels of the Joanette oats we obtained only about four bushels of the Wild Goose spring wheat under just as favourable conditions. The Wild Goose spring wheat has only a few heads per plant and a comparatively small number of grains per head. The crops which are here reported were greatly admired by about thirty thousand farmers who visited the College and examined the experimental plots in June 1905.

Oats and Barley grown on the same Farm for sixteen Years without Change of Seed.—The question of the advisability of making a frequent change of seed from one farm to another is one which has claimed the attention of farmers for long periods of time. It is a problem which it is exceedingly difficult to solve; in fact, it is practically impossible to find a solution which will comply with all cases. Any information, however, which can throw light upon this perplexed question should be welcome. If it is necessary to change seed grain every two or three years in order to keep up the vigour of the plants, the problem of seed selection is an exceedingly difficult one. We find at the present day a considerable number of the very best farmers who think that good results may be obtained by growing the same varieties on the same farm for several years in succession without the introduction of fresh seed from other farms, soils, or localities. At the Ontario Agricultural College eight varieties of oats and eight varieties of barley have been grown for sixteen years without change of seed. Care has been exercised each year to select the best grain for seed purposes. The crops have been grown on soil which might be termed an average clay loam, and in no one year out of the sixteen were the crops produced on either a light sandy or a heavy clay soil. The land received no commercial fertilisers whatever, but was manured with about twelve tons of farmyard manure per acre each four years. It has been cropped heavily with grain, roots, corn, potatoes, &c., and has probably changed but little in its productive capacity. As an accurate record has been kept regarding the yield per acre of each variety in each of these years, we are thus in a position to present results for comparison. The following table gives the average yields per acre per annum for each of the four periods of four years each; also the average yield per acre per annum for the whole period of sixteen years:—

Crops and Varieties	Average yield per Acre per Annum for each of four Periods of four years each				Average Annual Yield per Acre for 16 years
	1890–1893	1894–1897	1898–1901	1902–1905	1890–1905
Oats	Bushels	Bushels	Bushels	Bushels	Bushels
Joanette	84·8	88·7	84·9	102·4	90·2
Siberian	72·9	83·9	90·4	105·4	88·2
Waterloo	74·3	84·1	85·6	105·9	87·5
Oderbrucker	74·6	85·1	85·8	102·9	87·1
Probsteier	75·7	81·6	88·1	100·3	86·4
Bavarian	70·6	79·9	86·6	103·3	85·1
Egyptian	70·7	71·4	76·4	88·5	76·7
Black Tartarian	67·2	60·5	66·5	91·9	71·6
Barley					
Mandscheuri	60·3	72·2	70·3	76·4	69·8
Oderbrucker	53·1	61·6	68·4	68·9	63·0
Common six-rowed	50·5	56·7	68·4	68·9	61·1
French Chevalier	54·6	55·8	68·3	61·2	60·0
N.Z. Chevalier	49·6	56·7	68·2	64·7	59·8
Mensury	48·4	53·4	73·9	59·0	58·7
Black Hulless	38·8	39·1	47·5	50·1	43·9
Hungarian	42·7	34·8	42·2	50·8	42·6

The results are very interesting and quite suggestive. Without one exception, the average yield per acre for the last four years is greater than for the first four years for each variety grown during the sixteen-year period. The average results of all the varieties for each of the first, second, third, and fourth periods of four years each are given in the same order as just mentioned: oats, 74 bushels, 79 bushels, 83 bushels, and 100 bushels; barley, 50 bushels, 54 bushels, 63 bushels, and 63 bushels. It will therefore be seen that the average yield per annum for the last four years surpassed that of the first your years by 26 bushels per acre for the oats and 13 bushels per acre for the barley. The figures here presented show quite clearly that it is possible to grow the same varieties of grain on the same farm for a considerable number of years without change of seed, providing care is exercised each year in the selecting of the seed and in the handling of the crop.

The Production of Hybrids.

Even though we take great pains in selecting the best seed from the best plants of the best varieties of cereals, we find that the plants produced, although greatly improved in many respects, still have weaknesses. We observe that some varieties are specially strong in some features, and that other varieties are equally strong in other characteristics. With the hope of originating new varieties possessing the good qualities and eliminating the weak ones of some of the best varieties, efforts have been made to obtain the desired results through cross-fertilisation. With the aid of the investigations of Mendel, de Vries, Correns, Bateson, Tschermak, and others, we have obtained new light upon this problem which we are now studying in connection with our grain crops. In former years when crosses of cereals were made it was thought necessary to continue growing the crosses for six, eight, or ten years before the varieties became fixed. With the recent information obtained, certain definite results can be secured in considerably less time. At our Agricultural College we have crossed a few of our best varieties of spring wheat, winter wheat, oats, and barley. The work has been largely accomplished during the last four years. In 1905, we had about eight thousand hybrid plants, and the results were very encouraging. In all our crosses we are working along definite lines with the hope of securing what we are after. As, for instance, we have crosses between the Siberian and the Joanette varieties of oats. Of about two hundred and fifty varieties of oats which we have had under experiment the Joanette black has produced the greatest yield of grain, has been the greatest stooler, and has furnished grain which is the thinnest in the hull. This variety, however, is very short in the straw and unsuitable for general cultivation. The Siberian variety possesses straw of good quality and grain which is white in colour ; but the yield per acre is slightly less, the percentage of hull is rather more, and the stooling properties are not nearly as highly developed as are those of the Joanette. Already we have some exceedingly interesting crosses from these two varieties. Individual plants possess in themselves several of the desirable qualities of both parents. We believe that some of these qualities are already constant, but we are not so sure of others—such as productiveness. We also have most promising crosses from the Dawson's Golden Chaff and the Turkey Red varieties of winter wheat, the Herison Bearded and the Red Fife varieties of spring wheat, the Mandscheuri and the Two-rowed Chevalier varieties of barley, the Common Emmer and the Red Spelt, &c. Although there is a great difference of opinion at the present time as to the outcome of the recent investigations in plant-breeding, we believe, from what we have been able to observe from the work of others and from our own practical experience, that tremendous strides will be made along the line of plant improvement through cross-fertilisation within the next few years.

Conclusion.

It will be seen from what has been said that the work of the improvement of our grain crops in its best form means careful, systematic work conducted along definite lines and over a long period of time. This is brought about by first selecting good seed from the best plants of the best varieties and then by producing new varieties by means of cross-fertilisation, in order to obtain plants, eliminating as many of the poor qualities and incorporating as many of the best characteristics as can be brought together in any one variety, to fulfil a certain and a definite purpose.

HYBRIDS AND VARIATIONS IN WHEAT.

By Philippe de Vilmorin, of Paris.

The few observations which I propose to present to the Conference were made on the pedigree of wheats, which may be described as hybrids or crosses, according as to whether or not you rank them as cultivated wheats of different species.

When my father began, in 1878, cross-fertilisation between the different types of wheats, the object he had in view, and which he fully proved, was that all our wheats—with the exception of *Triticum monococcum*—all came from one and the same common origin. In consequence of his experiments, he was able to base his argument on two well-proved facts, which were—

(1) That the six* species of cultivated wheats can be crossed amongst themselves and give products indefinitely fertile.

(2) That on crossing any two of these six, the other four may appear amongst the results.

The observations of my father on this subject will be found recorded in the "Bulletin de la Société botanique de France" (Jan. and Dec. 1880; Jan. 1883; and Jan. 1888).

From then, the most interesting and marked types have been kept from year to year. Some of them have shown themselves to be wonderfully fixed, whilst others have constantly varied; and others, again, having appeared fixed for several years, have suddenly entered upon a stage of extreme variability. Certain strains had, from the very beginning, produced so many forms that, in 1890, it became necessary to start on a severe course of elimination. Besides this, in 1891, a strong frost destroyed many interesting types, particularly amongst the *durum* wheats.

Notwithstanding the dryness of the subject, I decided to enter into it, thinking that the experiments made, in some cases, on more than thirty generations upon the same family of wheats might be of some interest from the point of view of showing the transmission of the most marked characteristics amongst hybrids.

As for the families existing before my father's communications, I shall shortly summarise them up to that time. Those that have originated since that time I shall describe in all their variations from the beginning.

1. 'Chiddam d'automne à épi blanc' × 'Poulard roux velu de Beauce.'

(*Tr. sativum* × *Tr. turgidum.*)

This cross was effected by my father in 1878, who described the first phases of variation.†

* *Triticum sativum* Lam., *Tr. turgidum* L., *Tr. durum* Dsf., *Tr. polonicum* L., *Tr. Spelta* L., *Tr. amyleum* Seringe.

† *Bull. Soc. Bot. France*, Jan. 10, 1880.

It is difficult to find two more distinct types than those employed in this cross, as they belong to such distinct sections as to have been considered as different species, the wheat used as the seed-bearing parent being *white-eared*, *beardless*, *slender* and *glabrous*, while the pollen-bearing wheat was *red*, *bearded*, *compact* and *hairy*. Two plants arising from the first cross produced, in 1879, were very much alike. Their ears were semi-compact, red, beardless and were decidedly of the class of soft wheats. But, in 1880, the produce of these two plants was quite different, and, what was a particularly interesting fact, they produced types as different from themselves as they were from their common parents.

The offspring of the first plant was composed of *durum* red and beardless wheats. Up to the present time, it has given no variation worthy of remark, except that, in 1882, it produced a *durum* wheat with a *white* ear, but always beardless, which was destroyed by the frost in 1891.

The second plant, on the other hand, produced—

i. A soft wheat, white, beardless, but with the ear much more compact than the Chiddam d'automne à épi blanc.

ii. A *turgidum* wheat, white and glabrous.*

iii. A German wheat (*Tr. Spelta*) which, as years went on, gave slight variations of form and colour, but always remained a German wheat. In 1885 it produced two soft wheats, but has not done so since then.

To sum up, from this now ancient cross, there still exist two types, which appear to be quite fixed, of which one is a *durum* wheat, and the other a German wheat or spelt, when their parents are *Tr. sativum* and *Tr. turgidum*.

2. 'Blé de Pologne' × 'Pétanielle blanche.'

(*Tr. polonicum* × *Tr. turgidum.*)

The cross was made in 1881.

This experiment was described by my father ("Bull. Soc. Bot. France," Jan. 13, 1888). I shall therefore sum up very shortly the first years. One single plant from the first generation had a very long ear, loose, white and beardless (see figure in "Bull. Soc. Bot. Fr." xxxv., Plate I.). This was a *durum* wheat.

In 1883, the variation was disordered:

A represents the hybrid plant from the first cross, which has never varied since then. It still exists at the present time exactly as it came from the seed produced by the original cross. The only peculiarity being that this wheat, from 1883 to 1887, showed a tendency to produce two spikelets on some of the nodes of the rachis. I point out this fact, to which I shall have to refer again.

B was a beardless wheat, with a white and very slender ear, straw very hollow, of the Talavera type, with spikelets long and far apart, resembling a soft wheat. In 1884 and 1885, were picked out those plants most like a soft wheat, and in 1886, three forms were preserved:

* These two types, showing a return to the form of their parents, were destroyed in 1883.

Form **B**, very slightly modified in the direction of a soft wheat with white grain.

B[1] was a soft wheat, with shorter spikelets and red grain (both its parents being white-grained).

B[2] was a bearded soft wheat, resembling **B**, except in being bearded. **B** and **B**[1] reproduced true to themselves in 1887. **B**[2] produced seven bearded and three beardless out of ten plants.

C was a beardless soft wheat, with very shiny glumes. It produced in 1884, besides type **C**, a wheat **C**[1], bearded, with a compact ear, and with glumes of a dark grey, hairy and with the grain long, slender, red, and glazed: it was an intermediate form between soft and *durum* or *turgidum*, but nearer to *durum*. This form kept much the same until 1894.

In 1885, type **C** disappeared, in the sense that it produced two forms **C**[2] and **C**[3], both of them distinct from type **C**. These were wheats with widened ears, very pointed glumes, and similar enough to each other.

In 1886, type **C**[2] was abandoned as being less vigorous than **C**[3].

Type **C**[3] subdivided, in the same year, into two forms. The one which we kept as **C**[3] was a *durum* wheat, bright russet, bearded, but with very hollow straw, and shelling out easily.

The other form, **C**[4], had solid straw, falling beard, the glumes of the ear hairy and pointed, and of a grey colour. (Both parents had white ears.)

D was like a Pologne (*polonicum*), and was discarded in 1884.

Then, between 1883 and 1887, this cross between *Tr. polonicum* (Pologne) and *Tr. turgidum* ('Pétanielle blanche') had produced a great number of variations, of which seven were preserved and were still existing in the above years. These are **A**, **B**, **B**[1], **B**[2], **C**[1], **C**[3], **C**[4], which I am now going to describe.

Let us now examine what has happened with these different types.

A Has never varied from the beginning.

B.—This wheat, which we have noticed, was a soft one, beardless, with white grain; it has always produced soft wheats, reproducing its type more or less until 1890, when it was given up. But in 1889 produced two interesting variations:

- **B2** whose characteristics were a beardless spelt, with a very slender ear; but these peculiarities were not reproduced, and in 1890 this form was destroyed.
- **B3** was a wheat with a very compact ear, which has been preserved to the present time, and has never given any varieties, but has gradually modified its form, being more or less compact from year to year, but on the average being much less so than at the beginning.

B[1].—Had not varied until 1890, the time at which it was destroyed.

B^2 Family.

B^2.—In his communication of January 1888, my father pointed out that this variety produced, in 1887, out of ten plants, three beardless and seven bearded. The wheats of each class were unlike each other, and six forms were preserved; let us call them **B^{2}1, B^{2}2, B^{2}3, B^{2}4, B^{2}5, B^{2}6.**

Of all these forms, **B^{2}4** was most like **B^2**, always reproduced itself as a soft, bearded, white wheat until 1890, but, appearing less interesting than the other forms, was then destroyed.

B^{2}1 was a white, soft, beardless wheat, with very thin ears, unevenly placed spikelets, and red grain. In 1888, this wheat varied and produced four types, **B^{2}11, B^{2}12, B^{2}13, B^{2}14.***

B^{2}11 was a soft wheat, with a flat red ear, large, beardless, with red grain; in 1889 it produced three forms: **B^{2}111, B^{2}112, B^{2}113.**

> **B^{2}111** was like **B^{2}11**, and was destroyed in 1890.
>
> **B^{2}112** was a soft wheat, bearded, rosy, with a square ear and yellow grain. Destroyed in 1890.
>
> **B^{2}113** was a soft, bearded, white wheat, with round yellow grain; has never varied since then, and still exists in 1906.

B^{2}12 most nearly approached to **B^{2}1.** In 1889 it produced four varieties: **B^{2}121, B^{2}122, B^{2}123, B^{2}124.**

> **B^{2}121** was like **B^{2}12**, and was destroyed in 1890.
>
> **B^{2}122** was a beardless, white, soft wheat, with a large ear: it did not vary until 1898, and then produced two forms, **B^{2}1221** and **B^{2}1222.***
>
> > **B^{2}1221** was like **B^{2}122** in nine cases out of ten. It does not vary any more, and still exists in 1906.
> >
> > **B^{2}1222** was a single plant with a very long and spelt-formed ear, glumes very long, with long yellow grain, which never varied, and still exists in 1906.
>
> **B^{2}123** was a soft wheat, white, bearded, with a loose ear, grain yellow, long and very slender. Destroyed in 1890.
>
> **B^{2}124** was a soft wheat, white, bearded, compact, of the bristly type. Destroyed in 1890.

B^{2}13 was a soft white wheat, awned, with small round red grain. In 1889, it produced some bearded plants which were not followed up. **B^{2}13** was destroyed in 1890.

B^{2}14 was a soft white bearded wheat, with shiny-red grain; in 1889 it produced:

> **B^{2}141** (nine plants in ten) were like **B^{2}14.** Destroyed in 1890.
>
> **B^{2}142,** one plant with the ear whiter, straw shorter, same grain. Destroyed in 1890.

B^{2}2 was a soft, white, beardless wheat, flat, hairy, awned, with white grain. In 1888 it produced two forms:

> **B^{2}21** was like **B^{2}2**, did not vary in 1889 or 1890, and was killed by frost in 1891.

* The reader will not fail to notice that these must be read as B two, one one; B two, one two; B two, one three, and so on, and not as if they were B two, eleven; B two, twelve, and so on. B two, one two two one; B two, one two two two, and not B two, one thousand two hundred and twenty-one, or B two, one thousand two hundred and twenty-two. And so all through this paper.—Editor.

B^{2}22 was a soft, white, bearded wheat, hairy and spreading, with white grain; it did not vary and was killed by frost in 1891.

B^{2}3 was a soft, white, bearded wheat, glabrous, with a slender ear like a spelt, and white grain. It produced a marked variation in 1888:

B^{2}31 was like **B^{2}3**, did not vary again, and was destroyed in 1890.

B^{2}32 was a soft rosy-white, beardless wheat, of the Odessa type, with red grain. It varied in 1889, and produced:

B^{2}321, which was like **B^{2}32**, and destroyed in 1890.

B^{2}322 was a soft wheat, with long, beardless, white ear, which shed its seed badly. Destroyed in 1890.

B^{2}323 was a soft wheat, rosy, bearded, with a square ear and white grain. Destroyed in 1890.

B^{2}33 was a soft wheat, pale red, beardless, with pale red grain; it varied in 1889, and produced:

B^{2}331, which was the same as **B^{2}33** and destroyed in 1890.

B^{2}332 was a soft wheat, with a square pyramidal, red, beardless ear, red grain, of the Noé type. Destroyed in 1890.

B^{2}333 was a beardless, soft, white wheat, with slender, curved ear, and long white grain. Destroyed in 1890.

B^{2}334 was a bearded, rosy, soft wheat, with long yellow grain. Destroyed in 1890.

B^{2}5 was a bearded, soft, white wheat, very much the same as the **B^{2}4**, but later; grain pale red. This was a stunted plant, and was destroyed in 1888.

B^{2}6 was a rosy-coloured, soft wheat, bearded, ear square, compact at the tip, grain very wrinkled, poor, and was destroyed in 1888.

C^1 Family.

C^1.—This wheat, very curious in itself on account of its ambiguous characters, has produced, since 1887, several variations which we shall now examine; but the type has always persisted. One can say, however, that at the present time, after eighteen generations, it is nearer to *Tr. durum* than it was in 1887, when my father described it for the first time. It did not vary until 1894; but in that year, seven plants out of ten were beardless. Only one of the bearded plants was preserved, which reproduced itself well in 1895, but in 1896 gave two forms, **C^{1}1** and **C^{1}2**.

C^{1}1 was like C^1; in 1897 it varied again and gave **C^{1}11** and **C^{1}12**.

C^{1}11 was like C^1 and C^{1}1, and did not vary for four years; but, in 1901, it produced **C^{1}111**, **C^{1}112**, and **C^{1}113**.

C^{1}111, for the most part, was very like the type, and has never varied again to the present time.

C^{1}112 was very like **C^{1}12** (see below), but was larger: it was a grey hairy *turgidum*, with yellow grain. It produced, in 1902, some plants with rosy-coloured ears, which were not preserved, and, in 1903, produced two forms :

C[1]1121, to a great extent, kept true to type, has not sensibly varied since then, and still exists in 1906.

C[1]1122, one single plant, *durum*, rosy, with a small square and glabrous ear, very long beard, large yellow grain ; has not varied, and still exists in 1906.

C[1]113 was a wheat that looked like a spelt, hairy, with a greyish, long, very recurved ear, with large yellow grain ; returned, in 1902, to type **C[1]11**, and was destroyed.

C[1]12 was a *durum* wheat, with a hairy ear, the grain streaked with black ; very much like **C[1]1**, but with a longer ear, slender, the grain white and thin. In 1900, it produced some plants with white ears, which were not preserved. In 1901, it produced two forms : **C[1]121** and **C[1]122**.

C[1]121, for the most part, was like **C[1]12**. This wheat did not vary in 1902, but in 1903 it produced **C[1]1211** and **C[1]1212**.

C[1]1211, in nine cases out of ten, was the same as the type, and has not varied since.

C[1]1212 was a *durum* wheat having the same form as **C[1]1211**, but glabrous and white : it has remained fixed until now.

C[1]122 had a white ear, of the nature of **B[2]113**, with long white grain, looking like a soft wheat ; but, in 1902, it returned to type **C[1]12** and was destroyed.

C[1]2 was, in 1896, a *durum* wheat, pale rosy, glabrous, slender, with long yellow opaque grain. Has not varied, and still exists in 1906.

C[3] Family.

C[3].—This family was particularly interesting on account of the appearance of *durum* beardless wheats.

In 1886, **C[3]** was a *durum* bearded wheat, clear red, with very hollow straw, which easily dropped its seed (see p. 346). In 1887, it produced four forms, called **C[3]1**, **C[3]2**, **C[3]3**, **C[3]4**. As form **C[3]3** approached nearest to type **C[3]** we shall describe it first.

C[3]3 reproduced itself in 1888, but, in 1889, gave two varieties, **C[3]31** and **C[3]32**.

C[3]31, a *durum* rosy wheat, beardless, but awned.

From 1889 to 1906 it appeared year after year more or less strongly awned, and, in 1894, almost altogether bearded ; it still exists in 1906 in the form of a *durum* wheat.

C[3]32, which more nearly approaches to type **C[3]** and **C[3]3** than the preceding, is quite bearded and red ; it varied in 1894, and gave **C[3]321** and **C[3]322**.

$C^3$321. Nine plants out of ten followed the type. One single plant was chosen, which reproduced itself without variations until 1898, and then produced :

$C^3$3211 remained fixed and still exists in 1906. Nevertheless, we found, in 1903, a plant resembling **$C^3$31**, which was not preserved.

$C^3$3212 was a soft wheat, beardless, red, with white grain, which has not varied, and still exists in 1906.

$C^3$322. A wheat intermediate between *durum* and soft, with stronger ear and taller than **$C^3$321**, and beardless ; it produced two varieties in 1895 :

$C^3$3221. Nine plants in ten kept true to type **$C^3$322.** This type still exists in 1906, but gets nearer and nearer to a soft wheat.

$C^3$3222. One single plant, a bearded soft wheat, with a long and rosy ear, grain small and white, varied in 1896, and produced :

$C^3$32221, which followed the preceding type, has not varied since, and still exists in 1906.

$C^3$32222, a soft wheat, with a loose and slender ear, being very like a spelt in appearance, and large white grain, difficult to husk ; has not varied, and is still in existence in 1906.

$C^3$1 was, in 1887, a *durum* wheat, with clear rosy ear ; compact, resembling *Tr. monococcum*, slightly bearded, and with red grain (fig. 95). Did not vary until 1896, when it produced three forms : **$C^3$11, $C^3$12, $C^3$13.**

$C^3$11 followed in the same direction. It was a rosy-coloured *durum* wheat, almost beardless ; in 1899 it produced some plants with white and beardless ears, which were not preserved ; in 1904 it produced **$C^3$111, $C^3$112, $C^3$113.**

$C^3$111 followed the type, again varied the following year (1905), and produced :

$C^3$1111 conformed to type **$C^3$1** (fig. 95).

$C^3$1112 was a *durum* wheat, with ear round, red and half-bearded, grain yellow and not shiny (fig. 95). There were, besides, one plant bearded and one beardless *soft* wheat, of class **$C^3$113**, but these were not preserved.

$C^3$112 had a white ear, long and round, with hard glumes, of the spelt type, grain of *turgidum* ; reproduced itself in 1905, and still exists in 1906 (fig. 95).

$C^3$113 was a *soft* white, beardless, square ear, more compact at the top. It reproduced itself well in 1905, and still exists in 1906 (fig. 95).

$C^3$12 was a beardless white *durum* wheat ; has remained fixed since 1896, and is still in existence in 1906.

$C^3$13 was a rosy *durum* wheat, half-bearded, compact ; remained fixed, and exists still in 1906.

$C^{3}1$ $C^{3}1112$ $C^{3}112$ $C^{3}113$

FIG. 95.—POLOGNE × PÉTANIELLE BLANCHE.

$C^3 2$ was a *durum* wheat, with rosy ear, glaucous, slightly bearded, with red grain. In 1889 it produced $C^3 21$, $C^3 22$, $C^3 23$.

$C^3 21$, taken on the average, resembled the type, and was destroyed in 1890.

$C^3 22$, very like the one before, but with fragile axis. Destroyed in 1890.

$C^3 23$ was a fully bearded *durum* wheat. Destroyed in 1890.

$C^3 4$ was a *durum* wheat, with large square ear, slightly bearded, easily husked, with reddish grain; produced, in 1888, $C^3 41$ and $C^3 42$.

$C^3 41$ resembled the type, slightly more bearded; in 1889 it produced:

$C^3 411$, resembling $C^3 41$, and was destroyed in 1890.

$C^3 412$ very like the one before, but more compact. Destroyed in 1890.

$C^3 413$ was a *durum* wheat, with slender bearded ear, clear brown, with red grain. Destroyed in 1890.

$C^3 42$ was a beardless *durum* wheat, with a short and very compact ear. Destroyed in 1890.

C^4 Family.

C^4 was, in 1887, a hairy, grey, beardless *turgidum*. In 1888, it varied and produced $C^4 1$ and $C^4 2$.

$C^4 1$ was a bearded *turgidum*, which was killed by frost in 1891.

$C^4 2$ followed type C^4; it remained fixed until 1898, and then produced:

$C^4 21$ has remained true to type (nine plants in ten), and still exists in 1906.

$C^4 22$, represented by one plant, was a bearded *turgidum*, but otherwise very like $C^4 2$, becoming identical in 1899, and was destroyed on that account.

The most remarkable fact in this pedigree is that the wheat $C^3 1$, which appeared quite fixed, suddenly began to vary in 1896, but then only produced *durum* wheats of different kinds. In 1904, one of the forms split up into three, which were—

1. A wheat entirely a *durum*.
2. A wheat entirely a soft.
3. A wheat of no particular type, with a spelt-formed ear, and grain like a *turgidum*.

3. 'Blé Seigle' × 'Blé Buisson.'

(*Tr. sativum* × *Tr. turgidum.*)

In 1880, my father pointed out the curious fact that the produce of a soft wheat and a *turgidum* gave plants which all approached the spelts.

Amongst the numerous forms that appeared in 1880 (the second generation), only a single one was preserved: this was a spelt with a branched ear.

This rare anomaly well reproduced itself, and I am still cultivating it.

We shall notice, by the way, that a spelt with branched ear was also produced by the crossing of a *durum* and a soft wheat ("Bull. Soc.

Bot. France," tome xxx., Plate I.). It would be possible to multiply examples, but the same facts happen sufficiently often to make it unnecessary to quote them all.

The family of which I am now going to speak, however, is too interesting to be passed by.

4. 'Chiddam d'automne à épi blanc' × 'Blé Ismaël.'

(*Tr. sativum* × *Tr. durum.*)

This cross between a *durum* and a soft wheat was effected in 1878. The two grains fertilised produced, in 1879, two plants of a soft white wheat, with square ears.

The grains of these two plants (1 and 2) were sown separately, and gave very different results. In 1880, No. 1 gave 11 varieties, and No. 2 8 varieties.

The forms produced by No. 1, which we shall call 11,* 12, 13, 14, 15, 16, 17, 18, 19, 19^2, 19^3, and those by No. 2, which are 21, 22, 23, 24, 25, 26, 27, 28, were, since 1881, remarkable for their relative fixity; only the most interesting types were preserved and we had, in 1882:

15—A soft wheat, red, with compact ear, very regular. Destroyed in 1883.

16—This wheat, which was a *turgidum*, produced, in 1881, out of eight plants, six with glabrous ears (161) and two with hairy ears (162).

- 161 was a white *turgidum*, glabrous and bearded, with red grain. It has never varied, and still exists in 1906.
- 162 was a white *turgidum*, hairy, bearded, with short white grain (fig. 96). It produced, in 1895, two beardless soft wheats, which were not preserved, and, in 1882, two forms:
  - 1621 was like the type 162, and the origin of variations which we shall mention later on.
  - 1622, with longer straw, and longer in the ear, gradually returned to type 162, and was destroyed in 1901.

19^2 was a russet, beardless, soft wheat, very slender, and difficult to husk; it produced, in 1882 and 1883, some bearded plants, and was destroyed in 1883.

21 was, to begin with, a soft wheat, with large white beardless ears, with very short and stiff straw; in 1882, it varied a great deal, particularly as to the length of the straw; three forms were kept:

- 211. Very like the type, with short and stiff straw. Destroyed in 1883.
- 212 was a soft wheat, with large white ear, beardless, rather hairy and white grain. In 1885 it produced two forms:
  - 2121 was a soft wheat, with glabrous ear and long white grain; in the course of time it became hairy, and still exists in 1906.
  - 2122. This wheat, selected in 1885, was true to type, became glabrous in 1886 and was destroyed.

* See footnote on p. 347.

213 was a soft wheat with long straw, with a very flat red ear, easily husked and with long red grain. Reproduced itself in 1883, with the exception of one white plant, and was destroyed.

22. From wheat 22, of 1880, of which no description was kept, arose three forms in 1881 :

221. A *durum*, white, square, awned, rather hairy. It remained true to type for ten years, and was killed by frost in 1891.

222. A *durum*, red, glaucous, bearded, compact, easily husked, and in fact approaching to a *turgidum*, kept to type, and still exists in 1906.

223 was a soft wheat, white, beardless, pyramidal, and with white grain ; produced, in 1882, some feeble and uninteresting variations, and was destroyed in 1883.

23 was a *durum* wheat, glaucous, white and beardless. In 1882, it produced a bearded plant, which was not preserved ; after that it remained fixed until 1898, and then produced :

231 remained fixed, and later on, in 1904, was destroyed as too nearly approaching the following variety :

232. On the lower half of whose ear all the spikelets were abortive ; otherwise the ear has always been a glaucous, white, beardless *durum*. This curious characteristic has remained fixed, and the wheat still exists in 1906.

26 was a beardless, red, soft wheat, with a very compact ear ; it reproduced itself for two years, and was destroyed in 1883.

As it will appear, these types varied very little, and, in 1902, the only ones still in existence were 161, 1621, 2121, 222, 232 ; the others were either killed by frost or destroyed as not being of sufficient interest. But out of these five wheats still existing in 1902, four have not produced, to the present time, a single variation ; whereas No. 1621, which remained fixed for twenty-two years (with the exception of two beardless soft wheats, which appeared in 1895, and were not preserved), only produced one slight variation in 1898, that alone produced a large number of variations, which we shall now proceed to study.

1621, in 1902 (see above), was a hairy, white, and bearded *turgidum*. In 1903, it produced three forms : 16211, 16212, 16213.

16211 kept to type, and was only represented by two dwarf plants : it varied again in 1904, and produced two forms, 162111 and 162112.

162111 remained to type, and did not vary in 1905.

162112 was a *soft* beardless white (fig. 96). In 1905, it did not produce a single plant like the selection of 1904. Out of four plants, there were four forms, out of which one resembling 162111 was destroyed. The others were :

1621121. A bearded, white, soft wheat, with long and glabrous ear (fig. 96).

1621122. A soft wheat, white, bearded and hairy (fig. 96).

1621123. A soft wheat, white, beardless and hairy, but with a very compact ear (fig. 96).

All the last three had very large *turgidum* grain.

16212 was a red *durum* wheat, beardless, with round ear (fig. 97). It did not reproduce itself in 1904; out of four forms, not one resembled the type of 1903. We found one beardless *durum* like 222, destroyed; one beardless *durum*, with the lower part abortive and like 232, which was also destroyed; then two other forms which were kept, 162121 and 162122.

FIG. 96.—CHIDDAM D'AUTOMNE À ÉPI BLANC × ISMAËL.

162121 was a soft wheat, beardless, hairy, greyish-red, with white grain (fig. 97). In 1905 it produced nothing whatever resembling the selection of 1904. We found five forms:

1621211 was most like the type, a soft wheat, beardless, but with a long ear, glabrous, and less red than 162121 (fig. 97).

1621212 was a soft wheat, bearded, glabrous, reddish, abortive at the top, with large, red, short grain (fig. 97).

16212 162121 1621211 1621212 1621213 1621214 1621215

Fig. 97.—Chiddam d'automne à épi blanc × Ismaël.

1621213 had an ear resembling a *durum* wheat, bearded, glabrous, red and small, with fairly large white grain (fig. 97).

1621214 was a wheat intermediate between *durum* and soft, with ear bearded, glabrous and red, with white grain, akin to the one before (fig. 97).

1621215 had the appearance of a spelt, with long brown ear; was stiff, beardless, with large *turgidum* grain (fig. 97).

162122 was a soft wheat, beardless, with glabrous red ear, and long white grain (fig. 98). In 1905, it produced four forms out of seven plants:

162122 1621222 1621223 1621224

Fig. 98.—Chiddam d'automne à épi blanc × Ismaël.

1621221 resembled the type and was represented by four plants.

1621222 was a beardless soft wheat, glabrous, rosy, compact, and with long yellow grain (fig. 98).

1621223 was of the same class as the type, but with a white ear and large white grain (fig. 98).

16213 1621312 1621313 1621314

Fig. 99.—Chiddam d'automne à épi blanc × Ismaë .

162132 1621322 162133 162134

FIG. 100.—CHIDDAM D'AUTOMNE À ÉPI BLANC × ISMAËL.

1621224 was a red, bearded, soft wheat, with large *turgidum* grain; in short, the same as the type, only bearded (fig. 98).

16213, in 1903, was an awned, soft wheat, white and glabrous, with very long ear and white grain (fig. 99). In 1904, it produced four forms:

162131, which was like the type, but which continued to vary in 1905 and produced again four forms:

1621311, which remained true to type (five plants in ten).

1621312. A soft wheat, with very long beardless white ear, hairy, often twisting and yellow grain (two plants in ten) (fig. 99).

1621313, the same as the type, but with red ear and yellow grain (two plants in ten) (fig. 99).

1621314 was a soft, bearded wheat, with blackish-brown ear and yellow grain (one plant in ten) (fig. 99).

162132 was like 162131, but with hairy ear (fig. 100). It produced, in 1905, three forms:

1611321 was like the type, though with a tendency to have a narrow spelt-formed ear (seven plants out of nine).

1621322 had a small spelt-shaped ear, white, difficult to husk, with yellow grain (one plant in nine) (fig. 100). There was also a plant with a long glabrous white ear, like 1621311, which was not preserved.

162133 was a white, bearded, soft wheat, glabrous, with a very loose ear and white grain (fig. 100). It reproduced itself well in 1905, without any variation.

162134 was a white, bearded, soft wheat, glabrous, with a compact ear (fig. 100). It reproduced itself in 1905, all but one plant, which was hairy. This was not kept.

This group, as one can prove, is interesting from the number of variations which have arisen in these last few years.

Wheat No. 162, of 1881 (*turgidum*), gave no real variation before 1903, since No. 1622, of 1898, is still a *turgidum*.

In 1903, it produced a *durum* and a soft wheat; this last one not having so far produced anything but soft wheats, or about midway between a soft wheat and a spelt.

The *durum* wheat of 1903 (16212), which was not otherwise clearly defined by its characters (fig. 97), produced both soft and *durum* wheats, also a spelt (?); all wheats whose peculiarities were not clearly defined, and whose descent it will doubtless be interesting to follow.

The illustrations given above will help my descriptions.

5. 'Poulard d'Australie' × 'Blé barbu inversable.'

(*Tr. turgidum* × *Tr. sativum.*)

This cross, made in 1900, produced two grains.

1901.

Two strong plants with long brown ears, intermediate between the two parents, ear much bearded, black striped with white; grain white. The two plants were alike, so only one was kept.

1902.

Three forms appeared and were preserved.

One wheat resembling a spelt, with a hairy grey ear, loose, long and bearded; grain long and red, and stripping fairly easily (1).

A *turgidum* wheat, with a hairy, red ear, compact and square, bearded, and small red grain (2).

A soft, white, bearded wheat, with half compact and glabrous ear, being therefore less compact than the soft wheat employed in the cross (3).

1903.

1 reproduced itself fairly well, that is to say, it was grey (11*), except that two plants had, like the type, the appearance of a spelt, easily husking, but with a rosy-coloured and glabrous ear. One of these plants was preserved (12).

2 reproduced itself in the same form, and has not varied so far.

3, on the average, kept fixed (31), except one plant with a red, bearded ear (32), and several half-compact greyish, bearded, soft wheats (33).

1904.

11 did not vary.

12 reproduced itself in the same form, excepting one plant of soft wheat, with red and bearded ear, exactly the same as 32 in 1903, and which was not preserved on that account.

2 and 31 have not varied.

32 varied a great deal, the plants resembling the type being the minority (321).

There appeared some white bearded wheats, like 31, which were not preserved, and one plant with white beardless ears, with glumes slightly streaked with brown (322).

33 returned to the white bearded type (3) from which it originated and was destroyed.

1905.

11, 12, 2, 31 did not vary.

321 produced out of nine plants:

1 single plant with bearded red ear, like type (3211).

6 beardless red wheats, of which one was preserved (3212).

1 white beardless and 1 white bearded like 31, which were destroyed.

322 remained fixed, did not vary except as to height and size. The brown striping of the glumes more marked than in the preceding year.

To sum up, after four generations, this cross between a *turgidum* and *sativum* has produced:

11. A hairy grey spelt.

12. A rosy-coloured glabrous spelt.

2. A hairy red *turgidum*.

31. A soft bearded white.

3211. A soft bearded red.

3212. A soft beardless red.

322. A soft, beardless, striped wheat.

* See footnote on p. 347.

The cross made the following year (1901), between the same parents but in the contrary direction, produced, in the second year, some similar results.

Fig. 101.—Barbu inversable × Poulard d'Australie.
1, Barbu inversable ; 2, Poulard d'Australie ; 3, the hybrid, first generation.

The study of this family was not followed up, and there only remain the two photographs here reproduced.

Fig. 101 shows the two parents (1 and 2) and the hybrid from the first generation (3).

1 2 3 4 5

Fig. 102.—Barbu inversable × Poulard d'Australie. Second Generation.
Produce of 3, see fig. 101.

Fig. 102 shows the five forms, out of the six, that came out in the second generation from the above hybrid: *i.e.* (3), in fig. 101.

1 was a *durum* wheat, with a lax, bearded ear and short glumes.

2 was a soft wheat, with a compact, square, beardless ear.

3 was a *turgidum* wheat, with a brown half-compact, hairy bearded ear, and rather short glumes.
4 was a *durum* wheat, with a very short and very compact, flat, bearded ear.
5 was a *durum* wheat, with a compact, long-bearded ear.

6. 'Pétanielle blanche × Rieti' and 'Rieti × Pétanielle blanche.'

(*Tr. sativum* × *Tr. turgidum.*)

This double cross, between a *sativum* and a *turgidum*, was effected in 1903; so the results are yet very slight. However, the influence of the soft wheat appeared to predominate.

In the case where 'Rieti' served as the seed-bearing parent, the disordered variation, which should have appeared in 1905, was not noticed.

Out of nine plants, which were the direct result of the cross (first generation), eight were characteristic specimens of 'Rieti,' and I only kept the ninth plant, which was marked by the absence of beard. In 1905, the offspring of this plant were mostly wheats with bearded ears, more or less compacts, but not departing to any appreciable extent from the type.

In the contrary cross, the four plants produced in the first generation appeared to be 'Rieti,' but, in 1905, I found two forms which might be considered as *durum* wheats: one of them has a *turgidum* grain, whilst the other produced a well-marked *durum* grain.

7. 'Massy' × 'Chiddam d'automne à épi rouge.'

(*Tr. sativum* × *Tr. sativum.*)

This cross, the last I am going to talk about to you to-day, was made in 1901, between two soft wheats, and rather with the idea of trying to produce a useful wheat than to promote any new variation.

But, as early as in 1902, and therefore amongst the plants that resulted directly from the fertilised seed, I found one plant with red straw and a small greyish-brown ear.

In 1903, the offspring of this plant only produced white-eared plants, with the exception of two plants, which were very like the type of 1902. One of them was preserved.

In 1904, all the ears were freely streaked with brown, and the same peculiarity was well reproduced in 1905.

VARIATIONS.

Supernumerary spikelets.

As regarding *Tr. polonicum* × *Tr. turgidum* ('Blé de Pologne' × Pétanielle blanche'), I mentioned that a cross of the second generation showed a tendency, without reproducing it afterwards, to produce two spikelets on each joint of the rachis.

This curious anomaly was not noticed again until 1900; but in that year, and particularly in 1902 and 1903, it became very frequent in numerous varieties, and still continues to appear.

I here copy some entries from my experimental note-book. The qualifications apply to the excess of spikelets, say two, borne on the same notch of axis of the spike:

'Silver Chaff fall wheat,' 1904 (several).

'Jacinth wheat,' 1901–2–3–4–5 (several), 1906 (a few).

FIG. 103.—BLACK SOMALIS WHEAT.
Spikes with numerous supernumerary spikelets.

'Jacinth wheat' × 'Ladoga,' 1902–3 (several), 1904–5–6 (none).

'Blount's Lambrigg,' 1902 (a few), 1903–6 (none).

'Purple Straw' wheat, 1902 (a few), 1906 (one plant).

'Van Diemen,' 1901–6 (several).

'Cap à larges feuilles,' 1900–1 (a few), 1902 (many), 1903–6 (none).

'Gore's Indian wheat,' No. 6, 1900 (several), not cultivated since except in 1904 (none).

'Rattling Jack,' 1900–1 (many), not grown since.

'Red Tuscan,' 1904 (one plant).

FIG. 104.—VARIATIONS IN WHEATS.

An ear with lower and upper sterile spikelets; 3, an ear with one crooked supernumerary spikelet; 4, an ear with many supernumerary spikelets, side view; 5, an ear with many supernumerary spikelets, front view; 6, an ear twisted half round on its axis.

'Japhet,' 1904 (one plant).

'Chiddam d'automne à épi blanc' × 'Gros bleu,' 1901–6 (many).

'Prince Albert,' 1902–6 (several).

'Allora Spring wheat,' 1900 (several); 1901, not grown; 1902 (several); not grown since.

'Forty-fold,' 1905 (a few).

'Champlan,' 1902 (a few), never appeared since.

'Shirriff' × 'Noé' (dwarf), 1904–6 (a great number).

'White glabrous' in 'Blé de Crète,' 1906 (many).

'Briquet jaune,' 1906 (a few).

'Black Somalis' wheat, 1903 (year when received) to 1906 (a great many) (fig. 103).

I cannot help thinking that this phenomenon is one of the most curious, and I should be glad to know what is the explanation of it.

1 2

FIG. 105.
1, Red St. Laud wheat with bifurcated ear; 2, Shirriff × Noé wheat with numerous supernumerary spikelets.

What most puzzles one when trying to reason it out, is the fact that, notwithstanding the care taken in the examination of wheats at Verrières, the presence of supernumerary spikelets was never noticed (except in the case I mentioned in 1883) before 1900, and that since then they have appeared in a great many wheats of quite old standing, and amongst which 'Prince Albert,' for example, had been cultivated for very many years.

Another point to notice is that this curious variation has never appeared except amongst the soft wheats, and of those, only amongst the beardless varieties, with the exception of the 'Black Somalis' wheat.* Is it possible that it is the sign of a mutation ?—a mutation which will, after all, be not very permanent, as some of these wheats have shown no more signs of it for some years.

2. A soft wheat of which the ear is sterile at the base and tip.
3. A soft wheat with crooked supernumerary spikelet.
4. An ear with supernumerary spikelets, side view (fig. 104).
5. An ear with supernumerary spikelets, front view (fig. 104).
6. An ear turned half round on its axis (fig. 104).
1. 'Red St. Laud' wheat with bifurcated ear (fig. 105).
2. 'Shirriff' × 'Noé' with numerous supernumerary spikelets (fig. 105).

'Black Somalis' wheat (fig. 103).

Partial sterility.

In one of the affixed photographs (fig. 104, 2) is shown a wheat whose lower and upper spikelets are always sterile. This peculiarity arises from time to time and is transmissible. It shows a certain analogy to that of the double stocks which were talked about some time ago, and obliges us to admit the fact, paradoxical in appearance, that sterility may be hereditary. It is a phenomenon of lateral heredity; some fertilised ovules always reproduce a certain proportion of sterile ovules.

Variations in height.

In 1902, amongst some bastard but apparently well-fixed wheats, I noticed one plant ('Shirriff' × 'Noé') with very short and stiff straw, scarcely one metre (or 3 feet) high, when the others were 1 metre 40 centimetres, and whose parents were much the same height.

In 1903, from the seed of the same plant, I got 227 plants, of which 147 were dwarf and 80 were tall. This year, and ever since, the difference was always perfectly distinct between the tall plants and the dwarf ones, and there were no intermediate forms.

In 1904, I found 116 tall plants and 177 dwarfs. I also discovered two plants still higher than the 116 tall ones, one bearded and one with red ear (the two parents were white-eared), but both of them were of the hybrid type.

In 1905, the proportion of dwarf plants was less—85 to 196 tall ones. One of the dwarf plants had red ears.

In 1904 and 1905, many supernumerary spikelets were noticed amongst the dwarf plants as well as amongst the tall ones.

Variation in form.

Tr. durum ('Blé de Médéah'), a variety that had been fixed for a very long time, produced some soft white wheats, and one with a branched ear, as follows:

1. Slender and beardless.
2. Compact and beardless.

* The ten first named of these wheats were nearly related (section 4 of my classification), and for the most part of Australian origin.

3. Slender and bearded.
4. Compact and bearded.
5. Branched like a *Tr. turgidum* ('Blé de Miracle').

In 1902 and 1903 these types reproduced themselves in exactly the same form, except that, in 1903, the slender bearded form gave one beardless form.

This experiment was not carried any further.

Variations in the same plant.

The wheat described in the *Catalogue Synonymique* as Odessa No. 16, and also the wheat Ghirka of Bessarabia, often produced plants which agreed in all essential points with the type, but were white, and what is more extraordinary, some plants bore both coloured and white ears. The experiment of separately sowing the grain from the white and brown ears I made several times, and always found in the plants of the following generation both white and brown ears on the same plant. (A case coming close to that of partial sterility.)

Miscellaneous variations.

The few cases which I am going to shortly describe were not studied as to their descent. But still I can safely give them as variations, as they only differed from the type in one single characteristic.

The 'Rouge d'Altkirch' wheat (a very old variety) produced, in 1904, one hairy plant.

Sandomir, in 1904, one hairy plant.

Australie (Tailbouis) wheat, in 1904, several bearded plants.

Red St. Laud wheat, in 1904, one plant with bifurcated ear (fig. 105).

Beardless Odessa (1905) produced two hairy-eared plants.

Lamed *always* produces some bearded plants.

'Sandomirga d'Erivan' (1905), one beardless.

'Desert' (Bohar), one beardless plant.

'Trigo derespado de Murcia' (1905), several beardless.

Briasca, No. 2 (1905), one plant with long, large and flat ears.

THE INHERITANCE OF AWNS IN WHEAT.

By Charles E. Saunders, Ph.D., Cerealist, Central Experimental Farm, Ottawa, Canada.

The great value of a working hypothesis or of some newly discovered law of nature as a stimulus to scientific research is generally admitted, and no one is likely to question the worth of the discoveries of Mendel or of the most important conclusions drawn from them. Nevertheless it is unquestionably true that as soon as a new "law" is formulated some investigators are unduly influenced by it, and sometimes, perhaps unconsciously, try to make their observations accord with the law.

As a case in point, I may cite the statements made in regard to the inheritance of awns in wheat. It has been repeatedly stated by various observers, and is now generally accepted without question, that when a bearded and a beardless wheat are crossed the progeny in the first generation are beardless. And we are further told that in the second generation 25 per cent. of bearded and 75 per cent. of beardless plants are found. All this may be true in some instances, but to set it down as the rule is an unwarrantable abuse of the facts. The writer of this paper has raised nearly three hundred plants from seeds obtained by crossing bearded with beardless wheats. Many different varieties were used in making these crosses, but all were spring wheats. In the vast majority of cases the heads obtained from the plants of the first generation were by no means beardless; indeed plants which could fairly be called beardless were most exceptional. Other breeders of wheat in America with whom the writer has communicated have found similar results. For the purposes of this paper a rough classification of the kinds of heads obtained by the writer (in the first generation) has been made with the following results:—

More than one quarter bearded . . .	15 per cent.
One-quarter bearded	59 " "
Nearly beardless	20 " "
Beardless	6 " "

As will be seen the majority are classed as "one-quarter bearded," an expression which probably requires no explanation. Those described as "more than one-quarter bearded" were usually about one-third to one-half bearded. Plants with fully bearded heads were not observed, and the number of plants which could fairly be called beardless was very small. Almost all the plants were clearly intermediate in type between the two parents, showing, therefore, that awns are not to be regarded as a "Mendelian" character, that is to say, they are not necessarily either dominant or recessive in the first generation. The accompanying photograph shows some heads from plants of the first generation from seed produced by crossing bearded with beardless varieties. Some of these heads have the awns more fully developed than the average, but they are not at all unusual cases. (Fig. 106.)

Fig. 106.—Cross-bred Wheats produced by Crossing a Bearded with a Beardless Variety. These heads are from plants of the first generation.

In the second generation many kinds of heads are found, from fully bearded to beardless, the intermediate types representing so many different degrees of development of the awns as absolutely to defy classification.

Of course, if an observer is determined to confirm some "law," these intermediate forms may all be classed as bearded or beardless (according to the wishes of the individual); but surely in scientific researches the facts ought to take precedence over the theory.

While it is well understood that the word "beardless" is seldom or never used to signify the *entire* absence of awns, the writer altogether fails to see how the term can possibly be used to describe heads bearing a considerable number of well-developed awns; and he would suggest to fellow-workers in the breeding of new varieties of wheat that an accurate record of facts is highly desirable, whether they do or do not accord with any particular theory.

EXPERIMENTS ON THE BREEDING OF WHEATS FOR ENGLISH CONDITIONS.

By R. H. Biffen, University Department of Agriculture, Cambridge.

With the rediscovery of Mendel's Laws of Inheritance the subject of plant-breeding was at once placed on a fresh and a practical basis. Before this time the improvement of plants by cross-breeding was, as Lindley said some fifty years ago, a game of chance, with the odds in favour of the plant. Now the breeder has a fair conception of what is possible, and can with considerable definiteness plan his experiments so that they shall give the result required without leaving much to chance. In the future all such work will have to be conducted on the lines which Mendel once for all laid out for us, and plant-breeding will become a highly specialised subject.

In the following short account the attempt is made to show how Mendelian principles can be applied for the solution of the problems which confront those who would attempt to improve any particular crop. The case chosen as an example is that of wheat, the crop with which I have had more experience than others on which similar experiments are now in progress. It may be taken as typical of the researches which will have to be made before one can hope for much improvement in other crops.

At the outset it was necessary to obtain definite information as to the directions in which improvements were most needed. Here fortune was unusually favourable, for the millers of this country were calling into existence a small committee, the "Home-Grown Wheat Committee," which was charged with the task of improving, if possible, our English wheats. This committee consists of representative millers, farmers, and a chemist, whilst the breeding portion of the subject is in my hands.

A thorough discussion of the whole problem brought out the following facts: Firstly, that the varieties of wheat introduced within the last twenty years or so were distinctly inferior, in their capacity to yield good bread, to the wheats previously cultivated, and this, in spite of the fact that many of these varieties were said by their raisers to be eminently suitable for this purpose. Further, even the older varieties were not good enough to compete with the wheats imported from Canada, the States, parts of Russia, and so on.

As the natural outcome of this state of affairs, England has ceased to grow wheat to the extent which it did in former years, and if it continues, more and more wheat land will inevitably go out of cultivation. The miller and the baker of to-day have no use for the varieties grown at present, save to blend with better wheats.

Our modern conditions demand the quality found in certain foreign varieties and so lacking in our own. In passing, one may note that many other crops as we grow them are in a somewhat similar plight. The farmer, on the other hand, wants more grain and more straw than he has at present, a not unnatural demand, perhaps, but one difficult to supply when one comes to remember that, as it is, we grow larger crops per acre than any country in the world, with the possible exception of Denmark.

The starting point of the experiments was to import as many high-quality, or, as the miller terms them, "strong" * wheats, as possible, and grow them in this country to determine whether any were suitable for our conditions of farming. The attempt met with a certain amount of criticism, for it has always been supposed that strength was determined by climatic conditions, though it was known that wheat from California, Australia, and India, countries blessed with more sunshine than we enjoy, are far from strong. As the result of numerous trials, it was found that there was a certain amount of justification for this belief, for numerous wheats of great strength, when imported, deteriorated immediately under our climatic conditions. But—and this fact is of fundamental importance—varieties were found which have retained their strength perfectly for four seasons under the most varied conditions, and now give just as good results in the bakehouse as they do when grown in their native lands. We know of one case where one of these varieties has been grown for fourteen consecutive years in this country and is still as good as when it first reached these shores. It may be taken as proved, then, that some varieties of wheat are strong under our conditions of climate.

At the same time the majority of these are of no use to our farmers, for they lack the yielding power both of grain and straw which is essential here if profits are to be made.

Whilst these tests were in progress the inheritance of all the morphological characters of wheat was traced in detail and the foundations laid for a series of breeding experiments. These morphological characters need not concern us here, for they are of no importance when compared with problems of quality. They served simply to show the proper methods of carrying out the investigation we had in view, and to prove that the mere ringing of the changes on beards or no beards, rough or smooth chaff, was of little economic importance.

Those who continually handle wheat soon learn to appraise its value from the baker's standpoint, but it is not an easy matter to give a description of the difference between a strong and a weak grain which the uninitiated would appreciate readily. On the whole, strong wheats are characterised by a hard and more or less translucent endosperm, and frequently by a dull bloom difficult to describe. Weak wheats, on the contrary, are generally soft and opaque. Again, speaking in a broad sense, strong wheats, owing to their richness in proteids, as a rule have a higher nitrogen content than our English wheats, and in cases where judgment by eye does not appear satisfactory this affords a useful method of checking the result.

* In view of the fact that it is occasionally stated that strength is associated with the property of absorbing much water and giving a large number of loaves per sack of flour, it may be pointed out that strength is defined as "the capacity to yield large, well-piled loaves." By way of example, some Russian and most Indian wheats give a large number of loaves per sack. These loaves, however, are small and close in texture, and as bread has to be judged from the consumer's standpoint, such wheats cannot be considered as strong.

Strength and weakness, then, form what Mendel would have spoken of as a "pair of more or less" characters. Nevertheless, indefinite as they may seem, it is essential, if improvements in this crop are to be made, that their inheritance should be studied in some detail. During the last five years this part of the problem has been receiving considerable attention, and the broad outlines of the story have been traced. Numbers of typically strong wheats, known to retain this characteristic, have been crossed with our English varieties, and the story has been followed out from generation to generation. One such case may be taken as typical of many. The strong parent was Fife, a wheat largely grown in Canada; the other an old English variety with many good features, known as Rough Chaff. Besides differing in the quality of their endosperm, they differ in many other characters, but for the present endosperms only need be considered. The grains obtained as the result of the operation of crossing were rather shrivelled, and no statement can consequently be made as to their character. On sowing, they gave hybrid plants in which the grain was strong; judging by eye they appeared to be as strong as the parent Fife. There were no grains of the opposite character, showing that in this case the seed characters do not segregate in the expected generation. In the next generation, though individuals were produced with either strong or weak endosperms, a statistical examination showed that there were three of the former type to one of the latter, and a further separation into the colours corresponding to the red of Fife and the white of Rough Chaff gave the ratio of nine strong red, three strong white, three weak red to one weak white. Judging from the appearance of the strong and weak types, there seemed to be no question that the one series was as strong as Fife, the other on a par with Rough Chaff.

Confirmatory evidence was obtained by sending samples of the strong red and the weak white to the chairman of the Home-Grown Wheat Committee. The origin of the grain was not stated. He identified one sample as Fife, the other as Rough Chaff. In other words, the hybrids appeared to be identical in grain character with the parents. After this, there could be no doubt that strong and weak segregate from one another, and that, intangible as these characters may appear to many, and variable as they are under changing conditions of climate and cultivation, still they can be handled with the same definiteness as beards and no beards or rough and smooth chaff. In the following generation fixed forms of these types were isolated, and now field plots are being grown to test the matter further in the mill and bakehouse.

Since then many crosses between strong and weak wheats have been made, and though complications have been met with in certain cases, no facts have been found which tend to invalidate the conclusions arrived at from these first experiments.

At the same time these results have been checked in certain cases by estimating the percentage of nitrogen present in the grain, and the figures obtained point to the segregation of high and low nitrogen contents.

The drawbacks to many of these strong foreign wheats are their excessive liability to the attacks of yellow rust (*Puccinia glumarum*) and the weakness of their straw. In the majority of them the straw is thin and reed-like, and it does not give sufficient bulk per acre to suit the farmer. But rigid straws can be built up as readily as other undesirable characteristics can be eliminated, and neither of these features offers insuperable difficulties to the breeder of to-day.

Some five seasons ago certain facts suggested that the liability to the attacks of this fungus pest was possibly a Mendelian character. To test the matter, crosses were made between the most susceptible and the most immune varieties then available. They were not as suitable for this purpose as one could have wished, for the immune parent was far from being so in reality, though it was not nearly as susceptible as its fellow. The data obtained pointed in the right direction, and since then the experiment has been repeated and extended with far more suitable material. In these later experiments I have been able to make use of a wheat which, though it has been under observation for four seasons, has remained free from rust in spite of the fact that it has always been grown in company with the most disease-susceptible varieties obtainable. Crossed with Michigan Bronze, a variety more prone to rust than any other, it gave a batch of hybrids on which it was difficult to find an area an eighth of an inch square unattacked by the fungus. Even the awns and grain were infected. On comparing these plants with the susceptible parent, there appeared to be no difference between them in this respect; but whereas the parent hardly set a grain, the hybrids yielded a moderate crop, but one badly shrivelled by the attacks of the parasite.

Every available grain was sown in plots alongside the parent varieties, part on land which had been partially exhausted by a previous crop of wheat, and part on land which had carried a crop of clover the previous season and consequently was in high condition. The difference in cultivation, however, made no difference in the results of the experiment. The rust was late in appearing that season, but again every plant of the susceptible parent was stricken, and all of the immune type escaped entirely. The hybrid plots were badly attacked, and when the epidemic seemed to be well advanced the plants were sorted into two groups—namely, those which were attacked, and those showing no signs of disease even on the withering basal leaves. On some plots the diseased plants were cut out, leaving those free from infection for a subsequent examination. A few individuals which had escaped the attack at the early stage became infected later, and these were then added to the total of susceptible plants. The statistics showed that 1,609 diseased plants were present, and 523 immune, or a ratio of 3·07 : 1. There cannot, I believe, be any question that these latter were really immune, for they were surrounded by plants covered in rust, whose leaves were continually rubbing against them. Further, if a variety is susceptible, no plant of it under field conditions ever appears to escape.

The two parents differ from one another in other characters besides the immunity and liability to the attacks of rust, and it may be noted that, as one would expect, individuals similar to the immune parent morphologically but susceptible to rust were found, and also the rust-proof form of the susceptible parent.

Other experiments have been carried out on these lines, and though the statistics have still to be analysed, there is no doubt that they have given the same results. There are indications that other diseases besides yellow rust can be eliminated by searching for types, perhaps valueless in other characters, but possessing this immunity. Of the problems this opens up to the pathologist nothing need be said here: for the present we are concerned with plant improvement, and it need only be noted that, according to the report of the International Phytological Bureau in 1890, the attacks of rust cost Germany some £20,000,000 sterling. Such figures give one some idea of the stakes the plant-breeder can now play for, and, thanks to the work of Mendel, with the reasonable certainty of winning.

ON RAISING STRAINS OF PLANTS RESISTANT TO FUNGUS DISEASE.

By E. S. Salmon, F.L.S., Hon. F.R.H.S., South-Eastern Agricultural College, Wye, Kent.

The time has now come—I think it will be admitted on all sides—when it is imperative that attention should be paid in this country to the work of raising strains of cultivated plants which shall be resistant to certain fungus diseases. Such work, to be carried to a successful and practical issue, demands careful experiments carried on continuously by specialists over many years. It is to any large extent beyond the scope of private enterprise, and farmers and fruit-growers must agitate until we have some such institution connected with our Board of Agriculture as the Bureau of Plant Breeding of the U.S.A. Department of Agriculture.

The necessity, from the practical standpoint, of the raising of plants resistant to fungus disease has been brought home to me very forcibly during the present season. I have lately visited a large number of orchards and plantations in Kent and Surrey where such fungus diseases as the following are rampant: Apple Scab or Black Spot (*Fusicladium dendriticum* Fckl.), Apple Powdery Mildew (*Podosphæra leucotricha* Ell. and Everh., Salm.), Canker (*Nectria ditissima* Tul.), Brown Rot (*Monilia fructigena* Pers.), Cherry Leaf Scorch (*Gnomonia erythrostoma* Auersw.). Here the loss of many thousands of pounds has resulted from the planting of certain varieties of trees very susceptible to these diseases. Take the case of one fruit-tree, the apple; because of the injuries inflicted by such fungus diseases as Apple Scab or Black Spot, Apple Mildew, and Canker, the growing of apples is becoming in many districts a risky venture, owing to the lack of knowledge as to what strains of apples are resistant to these diseases, and to no attention having been given to the breeding of new strains more resistant.

Now, in all the mixed orchards and plantations which I have visited, a fact most promising for the success of the breeding of disease-resistant strains has always been in evidence. No matter what the fungus disease was, certain varieties or strains of plants have stood out as more or less disease-resistant. Thus, in a cherry orchard where the 'Waterloos' were so badly attacked by the Cherry Leaf Scorch for a number of years consecutively that the trees were rendered quite useless, and had all to be regrafted, the 'Turks' of the same age, and planted in alternate rows in the same orchard, stood season after season practically immune to the disease. Again, with Apple Scab, in plantations where certain varieties, such as 'Bismarck,' have been attacked, on the wood, leaves, and fruit, so virulently that the affected trees have had to be grubbed, many other varieties, such as 'Bramley's Seedling' and 'Beauty of Bath,' have remained immune or nearly so. Similarly with Apple Mildew.

In the different "constitutions" thus shown by the various varieties or strains we have, clearly indicated, a basis for the plant-breeder to work on in breeding disease-resistant plants. The existence of different "constitutional" characters in the strains of plants they have bred has of course long been known to practical horticulturists. But I should like here to point out the fact that varieties and "races" of plants have each a definite "constitution" with respect to fungus disease, and that this fact is now capable of scientific demonstration.

TABLE.

	Sown on								
Oidium on	*B. mollis*	*B. interruptus*	*B. commutatus*	*B. secalinus*	*B. velutinus*	*B. racemosus*	*B. arvensis*	*B. tectorum*	*B. sterilis*
B. interruptus	+ (18)	+ (10)	– (12)	– (25)	+?(10) – (5)	– (11)	– (29)	+ (16)	– (20)
B. hordeaceus	+ (29)	+ (31)	+ (10) – (9)	+? (9) – (13)	+?(10) – (6)	– (15)	– (28)	+ (21)	– (13)
B. commutatus	– (13)	– (9)	+ (117) – (1)	+ (22) – (3)	+ (6) – (1)	– (13)	– (10)	+?(16) – (13)	– (7)
B. racemosus			– (12)			+ (27)		– (3)	
B. velutinus	– (9)		– (6)		+ (16)	(9)	– (3)	– (12)	
B. arvensis	– (9)						+ (7)		
B. tectorum								– (8)	+ (7) – (1)
B. arduennensis	– (6)		– (3)					+?(5) – (2)	– (3)

\+ = full infection +? = 'subinfection' – = no infection

The number of leaves of each species inoculated is shown within parenthesis.

As illustrating the manner in which a plant's "constitution" shows itself with respect to a fungus disease, I may refer here to some experiments which I carried out recently at Cambridge and at Kew. The fungus used was the Corn and Grass Mildew (*Erysiphe Graminis* DC.). As a result of over 2,000 inoculation experiments which were made, I was able to show that by using the index of the reaction to the attacks of "biologic forms" of the fungus,* the presence of definite physiological (or constitutional) characters in a plant can be demonstrated. We find, first, that distinctive "constitutional" characters in this sense exist

* A detailed account of these experiments, and the results obtained, will be found in the following papers: *Beihefte z. Botan. Centralbl.* xiv. 261 (1903); *Annales Mycolog.* ii. 255, 307 (1904); *l.c.* iii. 172 (1905); *The New Phytologist*, iii. 55 (1904); *l.c.* iii. 109 (1904).

concomitantly with the specific morphological characters. Thus the species *Bromus mollis*, *B. interruptus*, and *B. tectorum* have each a "constitution" rendering them liable to full infection by the fungus on *B. interruptus*, while *B. commutatus*, *B. secalinus*, *B. racemosus*, *B. arvensis*, and *B. sterilis* are all completely immune, and *B. velutinus* partly immune, to the fungus in question. And similarly with respect to other "biologic forms" of the fungus (see Table). Such physiological or constitutional characters are constant and render the plant possessing them susceptible or immune in a definite manner, so that the various species of *Bromus*, according to their specific constitution—if one may use the term—behave differently to the attacks of the "biologic forms" of the fungus.

We find further that two different plants which are so closely allied morphologically that they belong to one species differ constitutionally. Such a case, for instance, is supplied by the species *B. commutatus* and its variety *racemosus*. No trace of infection results when *B. racemosus* is infected with spores from *B. commutatus*, or *vice versâ*. A further proof of the difference in constitution of the two plants is shown by the resistance of *B. racemosus* to spores from *B. secalinus* and *B. hordeaceus*, while *B. commutatus* is susceptible in both cases.

The fact of the immunity of *B. racemosus* against so many "biologic forms" of the fungus (see diagram 1) is the more remarkable on account

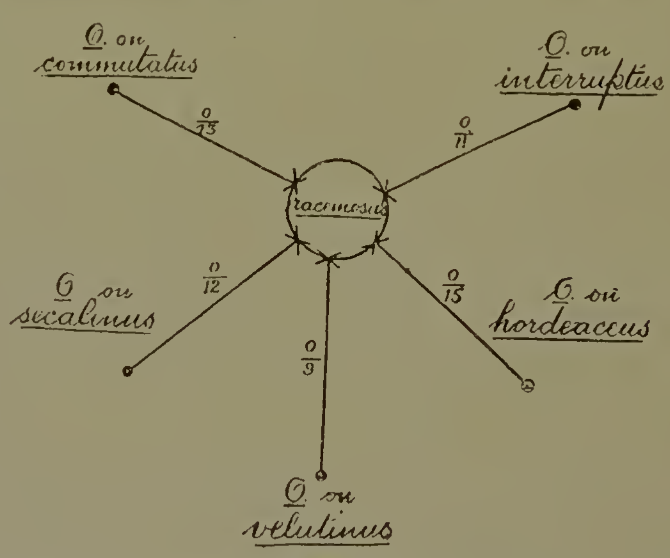

DIAGRAM I.

In this diagram, and in the following one, the number of inoculations made and the results obtained are expressed in the form of a fraction, in which the numerator indicates the number of times in which infection resulted, and the denominator the number of leaves inoculated.

of the existence on this species of a special "biologic form" which is able to infect it virulently. We see from this case how necessary it is for the scientific investigation of any supposed case of disease-resistance in a plant that a thorough study is made of the various forms of the fungus, so that the exact degree of "immunity" can be ascertained. In the present case it would seem that plants of *B. racemosus* might stand among virulently diseased plants of *B. commutatus*, *B. interruptus*, *B. hordeaceus*, *B. velutinus*, and *B. secalinus*, and remain perfectly free from the mildew in question; but if plants of *B. racemosus* already infected are placed in their neighbourhood, and spores from these reach them, the "immunity" would completely disappear, and the disease be virulent. It is very probable that such cases of partial immunity, *i.e.* immunity against all but one of the numerous specialised forms of the fungus, which appear inexplicable until the specialisation of parasitism shown by the fungus is known, are common among cultivated plants and their fungus diseases.

Now, as a rule—in the case of the fungus we are considering—each species of *Bromus* shows "constitutional" characters which hold good for all examples of the species obtained from different localities. But there are exceptions to this rule, and we find that the problem becomes complicated by the existence of "biologic forms" of the host-plant: that is, forms or races which are morphologically identical, but which differ "constitutionally" or physiologically as shown by their different behaviour to the same fungus. Such a case is illustrated by Diagram II.

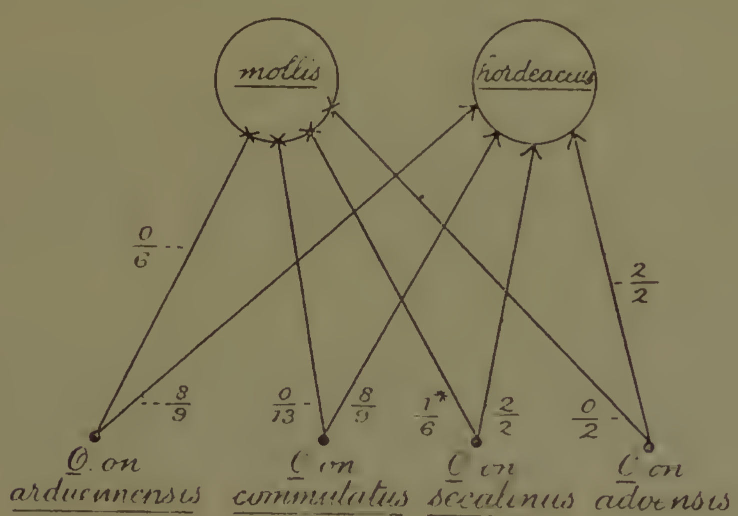

DIAGRAM II.

Diagram illustrating the different constitutional characters of the two plants *B. mollis* and *B.* "*hordeaceus*," which are morphologically identical. *B.* "*hordeaceus*" is infected by the four forms of *E. Graminis* on *B. arduennensis*, &c., while *B. mollis* is immune against their attacks.

The plant here called *hordeaceus* has been grown at the Cambridge Botanic Garden from seed originally received (under this name) from the St. Petersburg Botanic Garden. On the plants arriving at maturity, they were identified by both Professor Marshall Ward and Dr. Stapf as being identical morphologically with *B. mollis*. Nevertheless this race of *B. mollis* which has been called *hordeaceus* possesses different "constitutional" characters from those shown generally by the species. In the comparative inoculation experiments which were made, *B. mollis* proved completely immune against the attacks of the fungus on *B. arduennensis*, *B. commutatus*, and *B. adoënsis*, and practically so against the fungus on *B. secalinus*, while *B. hordeaceus* in every case proved susceptible. We must conclude, therefore, that the morphological species *B. mollis* includes two "races," or sets of individuals, possessing distinctive physiological (or constitutional) characters—that is to say, with regard to the forms of the fungus in question, an immune and a susceptible race.

When we seek to discover the nature of immunity—that is, to ascertain exactly in what manner the constitution of a plant enables it to resist disease—we find, by using certain cultural methods which I have lately described,* that it is possible to demonstrate that immunity in no way depends on any structural or anatomical peculiarities, such as hairs, ribs, thickness of the cell-wall or cuticle, or the chemical nature of the cell-wall. The immunity shown is not to be ascribed to the failure of the germ-tube of the spores of the fungus to penetrate the leaf-cells, but to the inability of the fungus to develop further the incipient haustorium which is formed, or if, as occasionally happens, the haustorium is completely developed, to its incapacity to adapt itself to the intracellular conditions obtaining in the plant. That is to say, immunity in these cases depends on the power possessed by the plant of preventing, by means of certain physiological processes, the attainment of that balance whereby working-relations between the haustorium and the host-cell are brought about and maintained.†

In those cases, as, for example, in the mildews, the rusts, and probably many other diseases, where the place of the decisive conflict between parasite and host is intracellular, and the issue (which gives immunity or susceptibility to the plant) determined by the "constitution" of the plant, the most important question in connection with the breeding of disease-resistant plants becomes the one as to whether such "constitutional" characters are Mendelian as regards the laws of their transmission. Mr. Biffen's work ‡ has now answered this question; we know, for instance, that with regard to rust, susceptibility and immunity are definite Mendelian characters, the former being the dominant one. Remembering the all-important position which "constitutional" characters occupy in connection with the breeding of disease-resistant plants, it seems to me impossible to overestimate the importance of Mr. Biffen's discovery.

What I have wished to do in these few remarks was to point out that our present varieties of cultivated plants show very different "constitutions" with respect to fungus diseases; that such constitutional characters, where they have been tested, have been found to be fixed for the species, variety, or race, and confer immunity or susceptibility on the plant in question; and that such characters appear unchanged in "hybrid" offspring in the definite manner following Mendel's law. If the scientific experimenter can make use of the practical knowledge of the horticulturist with respect to the "constitutions" of the plants he has bred, there is every reason to hope that considerable success will soon attend the efforts of the plant-breeder to breed, by crossing and selecting, strains of plants more and more resistant to fungus diseases.

* "Cultural Experiments with 'Biologic Forms' of the *Erysiphaceæ*." [*Phil. Trans. Roy. Soc.* vol. cvii. 107 (1904).]

† A detailed account of these phenomena will be found in my paper "On the Stages of Development reached by certain 'Biologic Forms' of *Erysiphe* in cases of Non-infection." [*The New Phytologist*, iv. 217, plate 5 (1905).]

‡ See *Journ. Agric. Science*, i. 40 (1905).

Discussion.

The President: There can, I think, be no doubt that this group of papers on cereals has raised the most interesting and important points, from a practical and economic point of view, that have come before this Conference.

Mr. Elwes: We have had some admirable papers, and the subject is of such great importance that some criticisms should be made. I should like to say that I think Mr. Biffen's paper a most admirable one, but I would say (judging from my experience of growing wheat on my own land in three wheat-growing counties for thirty years) that the complexity of the subject is so extraordinary, that unless an experimenter is going to produce what the practical farmer really requires, he may waste much time in working for a result which will not have so much a practical as a scientific value. Modern milling has made the production of two qualities in wheat—strength and colour — a necessity, strength meaning the ability to absorb water. As long as water is cheaper than wheat the interests of the miller cannot be the interests of the consumer. I deny that hard wheat will make better bread from a medical or from a chemical point of view, nor from the point of view of any consumer who is not saturated with prejudice like the majority of the agricultural and other labourers of this country. Old English white wheat will make the best bread to be found in the whole world.

Again, the question of the value of the straw must not be allowed to fall out of sight, for the straw is often worth almost as much as the wheat. Indeed, I remember the case of a man selling his barley straw at £4 a ton, and buying Russian barley at £3 16s. to feed his cattle. And so I feel very strongly that experimenters must not ignore the question of straw-production. The up-standing power of the straw is also very important, and I ask the gentlemen who are working for us in different countries always to bear in mind that the farmer would look, not so much to some one or other of the points aimed at by these experiments, but to the sum total of what may be produced, which to him is of infinitely more importance than an improvement in one of its details, accompanied, as it well may be, by a loss in another. The scientific man and the farmer are living and working and trading under totally different conditions, and the farmer is not able to reproduce the results or to work on the lines which obtain on experimental farms.

Professor Hansen said that at the present time there were tens of thousands of acres in the States under straw. It was at present of no value whatever, and was burnt.

M. de Vilmorin: In France we try fine white wheat, and it stands well; but generally speaking it has a short straw. To expect a fine wheat to give a long straw is like trying to make an early variety bear a heavy crop. It is not to be done yet. I think the definition of "strength" is very vague and difficult, and it has nothing whatever to do with the actual chemical composition of the gluten. We should like to have a chemical method of finding out the baking qualities.

Professor Wittmack: I have very much enjoyed Mr. Biffen's paper. I should like to ask if you are in correspondence with Mr. Humphreys.

Mr. Elwes: Yes.

Professor Wittmack: Then the matter is in your hands, and you will succeed. But in a climate like that of England there cannot be grown strong wheats. Mr. Humphreys got his 'Fife' wheat from Manitoba, as his whites never did well in England, Germany, or France. This variety of 'Red Fife' has proved good in England and very strong also, and it did not lose its strength. Is all our theory wrong? Wheat cannot become very rich in gluten when the season is long, because the leaves have a long growing period during which they assimilate and form more starch; and so the English grains are very rich in starch, because they have long vegetation. If there is a short-vegetation period there cannot be so much assimilation of starch, and the production of gluten will be greater. How does it come that this variety gives a strong wheat?

M. de Vilmorin: This 'Red Fife' is a quick-growing variety. It is not a Canadian but a German variety. The story of this wheat is worth being known. Fifty years ago Mr. Fife, a farmer in Ontario, asked one of his friends to send him some wheat from Dantzic. He received his wheat and sowed it in the spring; but it proved to be a winter wheat, so that none came to maturity except a few plants here and there, which were supposed to be a variation. This was grown by him and was distributed to all the farmers round, and is now the most important wheat grown in Canada and the Northern States. But this wheat was known all the time in Galicia. It never originated in America. It was imported from Germany, and it is curious to see that after being cultivated for fifty years—one strain in Manitoba and one in Central Europe—they are still now identical.

The President: This is among the most important questions to the human race. Professor Lankester has asked the Government for £10,000,000 to discover the origin of disease. I think we might ask for something to carry on our investigations into the laws of its transmission. I can only think with admiration of Mr. Biffen's paper, and the experiments he has made with regard to the transmission of rust.

SOLANUM COMMERSONII (THE SWAMP POTATO).

By Professor WITTMACK, of Berlin.

SOLANUM COMMERSONII is a potato of the eastern coast of South America, not of the western, whence we suppose *S. tuberosum* to have come. It was first found by Commerson in 1767 in the neighbourhood of Montevideo, on the banks of the river Mercedes. Later on, *S. Ohrondii*, which was found in the sand of an island in the mouth of the Plata River near Montevideo, was established as a distinct species, but it now appears to be identical with *S. Commersonii.*

In the year 1896 Prof. Heckel, of the Botanical Garden at Marseilles, received some tubers of a potato from Uruguay and determined them as *S. Commersonii*; and he was absolutely right. In 1903 he had the kindness to send me some tubers from it, and I cultivated them in the economic garden of our Agricultural High School at Berlin. It is a low-growing plant with remarkably long stolons, a metre or more in length. The tubers are small, of a pale yellow colour, and have many lenticels. Their taste was at first bitter, as Dr. Heckel reported. The leaves are interrupted pinnatifid, the single pairs very distant from each other and somewhat blunt, never so sharply pointed as in the common potato. The single pairs do not cover the next pair. The flowers are white and of a very agreeable odour, somewhat like sweet peas or a little more like honey. They are deeply divided, much deeper than with *S. tuberosum*, and forming a star as in the tomato, for which Commerson mistook it. In opening in the morning the corolla reflexes, and afterwards, at noon, it closes; the next morning it opens again.

By the kindness of Messrs. Sutton of Reading, I am able to show flowers of *S. Commersonii* and of its violet variety, as well as of the 'Blue Giant,' a variety of the common potato, and also of *S. Maglia*, *S. etuberosum*, and other wild species.

The chief difference between *S. Commersonii* and *S. tuberosum* lies in the calyx. The calyx teeth are short and triangular, whilst in *S, tuberosum* they are long and awl-shaped (subulate). This is a character which never changes in our common potato, and one must, as Darwin and De Candolle have said, pay special attention to the characters of such organs as man does not use.

Monsieur Labergerie, a gentleman living at Verrières, Dép. Vienne, France, also had tubers from Dr. Heckel, and cultivated them with great care, on highly fertilised soil.

In the very first year a strange thing happened: A blue or violet-coloured potato was found under one plant, and this M. Labergerie propagated. He found that it produced large tubers and in great abundance, and in 1906 he offered it for the first time in commerce under the name of "*S. Commersonii* type or variété violette."

M. Labergerie has meanwhile found other variations from the original type of *S. Commersonii.*

There were some people in France, notably Monsieur Philippe de Vilmorin, who doubted whether the blue variety was actually produced by *S. Commersonii.* In all its characters it closely resembled a variety of the common potato called 'Blaue Riesen' (Blue Giant) raised by a German, Herr Paulsen, at Nassengrund, Lippe Detmold.

M. Labergerie was good enough to send me tubers of his violet variety and also of all his others. Of these I gave some to my son-in-law, Mr. A. Dreyer, at Plenkitten, Kreis Mohrungen, East Prussia, where he has very heavy soil, which our garden in Berlin has not; and a fortnight ago I received flowers from him, and I find the calyx teeth of Labergerie's violet variety are exactly as thin and long and hairy as in *S. tuberosum*, the form also of the corolla is quite the same, not deeply incised, not star-shaped, as in *S. Commersonii.* The stigma, too, is capitate or bilobed-capitate, not compressed from both sides and bilobed-roof-shaped as in *S. Commersonii.*

The day before I left Berlin, I noticed in the "Gardeners' Chronicle" the same opinion as my own expressed by Mr. Sutton, and by those Fellows of the Royal Horticultural Society who had seen the plants growing at Messrs. Sutton's. On Tuesday, the 31st July, at the fortnightly meeting of the Society, Mr. Sutton also exhibited the white type of *S. Commersonii* side by side with the violet variety; and for comparison, the 'Blue Giant' from Mr. Paulsen, the breeder, at Nassengrund, Lippe Detmold. He also added other wild species, a wild type of *S. tuberosum*, *S. Maglia*, and *S. polyadenium.*

We are all quite sure that M. Labergerie is a man who works with great care, and his honesty is undoubted; but as to err is human, so we were convinced that this was a case in which an error had crept in unawares, notwithstanding all precautions, and that a tuber of 'Blue Giant' must have been in the soil at the place where he planted *S. Commersonii.*

Such was my standpoint up to yesterday morning. Then I received a letter from M. Labergerie, in which he replies to the doubts which I had expressed to him before leaving Berlin. He says he has never had the 'Blue Giant' in his garden, and that the plot of ground had, since 1882, only been planted with cabbage, salads, and sorrel (*Rumex Patientia*). Moreover, his father had grown the 'Blue Giant' in 1894 or 1895 (eleven years ago) without success on a plot of ground 10 kilometres distant. And when, in 1905, the present M. Labergerie heard people say that it and his potato were identical, he bought some tubers of 'Blue Giant.' He now tells me that the former has very large tubers, 400–1100 grammes, such as are seldom found in *S. tuberosum*, but are common in *S. Commersonii* violette. Another thing which he considers is a proof that it is not the same is that the stolons have become considerably shortened.

In 1904 and 1905 he obtained from his *S. Commersonii* two yellow varieties, spotted with violet, or yellow with violet eyes; and also from a yellow form of 1903 or 1904 he obtained the same violet type as in 1901. He has, moreover, obtained still many other forms, some of these resembling other common varieties of the potato.

Now if this all is so, as it is stated by M. Labergerie—and I for one have no reason to doubt of his correctness—we have in *Solanum Commersonii* a plant which, in the words of De Vries, "has just arrived at a state of mutation," and this has very probably been brought about by the better cultivation which it has received at the hands of M. Labergerie.

Postscript.—Berlin, November 13, 1906.—After the close of the Congress and the visit to Cambridge, I saw the potato trials of Messrs. Sutton at Reading, and I could find no difference between *Solanum Commersonii* violette and *S. tuberosum* 'Blue Giant.' Later in the autumn I found in my own trials that the tubers of the violet *S. Commersonii* had, in the raw state, a slight taste of hazelnut, as M. Labergerie had already stated ; the flesh also was purer white than in 'Blue Giant,' where it was more of a cream colour. But now, two months later, these slight differences are not so perceptible. They are, on the whole, trifling.

The whole matter, therefore, does not yet seem to be settled. The wild type is still a little bitter. The starch granules of all three present no exact differences.

HYBRIDISING AT THE ANTIPODES.

By H. H. B. Bradley, Sydney, Australia.

Narcissus.

Near Sydney the blooming season is generally about April to June for Polyanthus varieties, June to October for trumpets, July to October for Poeticus ; generally speaking, the weather from April to July is fairly showery, August less so ; September and October variable, often with very hot drying winds ; and this seems to influence the setting of seed. Many of the Polyanthus set seed freely without artificial pollination : the early trumpets the same ; the incomparables and late trumpets are bad seed-bearers, while the poeticus varieties are uncertain, sometimes setting freely, at other times badly. On the other hand, on the mountains where the season is later and the climate moister, such varieties as 'Emperor' and 'Sir Watkin' set seed on nearly every bloom that is not cut ; with me 'Sir Watkin' has never seeded, and it has always been hard to get seed from 'Emperor' ; but one wet September I got a lot of seed from 'Emperor' by different crosses ; in this case, of course, the flowers from which pollen was taken were cut early and kept indoors so as to get dry pollen.

Working trumpets with pollen of polyanthus, I got any quantity of good 'Tridymus,' which here are hardy ; I used such varieties as 'Duchess of Albany,' 'Grand Monarque,' and 'Soleil d'Or' on 'Countess Annesley,' 'Princeps,' 'M. J. Berkley,' &c. : all of these crosses gave 'Tridymus' and nothing else ; but using 'Paper White' many of the seedlings were polyanthus, the others being 'Tridymus' with white perianths and white or pale lemon corona.

Crossing 'Paper White' with pollen of trumpets, the result was only polyanthus, many being merely repetitions of the 'Paper White' seed-parent, while others were slightly improved forms of the seed-parent.

Working such polyanthus as 'Apollo' and 'Gloriosa' with pollen of Incomparabilis 'John Bull,' the seedlings so far are only polyanthus, some very much improved in size of perianth, and slightly (very little) lengthened corona, but lessened number of pips * to a head, while others were very poor degraded polyanthus with small pips and small heads ; but in some of these degraded forms the colour influence of the pollen-parent was evident ; in these cases the colour of the perianth, which in the seed-parent was white, in the seedling was yellow. In most of these crosses, the 'grass' is more like that of the Incomparable, and the flower spathe appears almost simultaneously with the grass, opening when the

* In a letter received from Mr. Bradley in October 1906 he says :—"Please add a note. I spoke too hastily on the experience of one year's flowering only, when the pips were certainly, as stated, much fewer in number than in the seed parent. This year (1906) this has been entirely changed, and some of the heads have as many as twenty-three pips each. Generally the stamens are biscriate like the seed-parent, but in one of them the stamens are equal in length to Incomparable."—Ed.

grass is about 6 inches long. Curiously enough, these seedlings were all so much earlier in blooming than either parent that comparison was impossible; these seedlings flower in May, while the parents do not bloom till July; on the other hand, 'Tridymus' seedlings were all much

FIG. 107. —'JOHN BAIN' CROSSED WITH POLLEN OF ODORUS RUGULOSUS.

later than the parents. Trumpet seedlings are generally earlier than their parents in blooming.

Hitherto all 'Tridymus' seedlings have proved sterile, no matter what the cross may have been.

'Odorus,' so far, has given no seed, and its pollen is hard to get; but I have two seedlings from this cross, one from a bicolor which gives a charming 'Tridymus' midway between the two parents, the other a self-yellow 'Tridymus' equally intermediate.

FIG. 108.—SEEDLING FROM 'M. J. BERKELEY.'

$3\frac{1}{2}$ inches across; pollen parent unrecorded; perianth white; trumpet pale sulphur changing to pure white.

In crossing narcissus, where the bloom of the seed-parent is not required for other purposes, I cut the bloom with a short stalk, cut away perianth and corona and remove the pistil: this leaves a sort of paint-brush, so to speak, with which it is easy to dust the pistil of the seed-flower with pollen ; but if the pollen bloom is also to be used for seed, I cut from it the filaments of the anthers as long as I can, and then take one of these with a pair of pliers, and so dust the pollen on the pistil of the seed-bloom. In working blooms of polyanthus I split the lower side of the tube of the intended seed-bloom before the pollen is free, and then remove the anthers. Splitting the lower side leaves the perianth and tube as a protection from the sun for the pistil.

Up to the present the blooms are all worked where the bulbs are growing out of doors, and none have been covered or otherwise protected.

Fig. 109.—Another Seedling from 'M. J. Berkeley.'
$3\frac{7}{16}$ inches across; out of the same seed-pod as fig. 108, but quite distinct; white.

Some of the Leedsii and Barrii varieties are bad at seeding, while others set freely—'Barrii Siddington' sets seed freely, while from 'Barrii Conspicuus' I have had only two pods of seed in twenty years: these were both twin flowers crossed with *poeticus ornatus*, also twin flowers, and both were lost through bursting unexpectedly. 'Maria Mag. de Graaff' seeds freely, while from 'Minnie Hume' it is hard to get any seed.

Hippeastrums.

One of my first attempts was with pollen from a beautifully coloured white heavily striped bloom of the old style, rather narrow in gape and divisions of the perianth. This pollen I worked on a red flower, and also on a 'Vittatum' variety; and amongst about 350 seedlings there were only three red individuals, the seedlings taking strongly after the pollen-

FIG. 110.—'PRINCEPS' CROSSED WITH POLLEN OF JOHNSTONI 'QUEEN OF SPAIN.'

parent. Pollen from these I worked on various hybrids which I had procured from Messrs. Veitch, but I only obtained repetitions of the pollen-parents. Then, again, using pollen from these seedlings on to the Veitch hybrids, the next generation was practically the same, showing a marvellous prepotency in the original pollen-parent.

By choosing suitable parents I have had no difficulty in breeding brilliant reds with a clear white edge to every division of the perianth ; but to do this I have had to use some of the old varieties ; and though I have worked the broad-petalled varieties with this pollen, and have retained the marking, the blooms, on the other hand, retain the form of the pollen-parent to a large degree.

Seeing that the top division of the perianth is always the largest and best-coloured, I generally use the anther, the filament of which is adnate to this division ; whether this be the reason or not I do not know, but the progeny generally have more equal divisions to the perianth, and the bottom division is greatly improved.

On the other hand, with a view to getting as white a bloom as possible, I use the bottom division (generally all white) from the white red-striped varieties ; and in the seedlings the flowers have much less colour ; but the shape of the bloom is spoilt, the divisions being narrow.

With us most hippeastrums are garden plants doing well out of doors ; but *H. pardinum* is a greenhouse plant, in fact is often grown in a hothouse ; it is, however, very potent as a pollen plant, and using its pollen on red-coloured Veitch hybrids I have raised what is practically a hardy *H. pardinum*, which has stood out of doors and flowered fairly well for two or three years.

My hippeastrums (with the exception of *H. pardinum*) are all garden varieties, and so contain the blood of many ancestors ; hence it is impossible to expect any precise results ; still, in working, I hope for a result in some of the progeny partaking of both immediate parents, and it is remarkable how often the result is as sought, notwithstanding my rough methods.

Here my method is similar. I cut off an anther with its filament, holding the filament ; there is no difficulty in dusting the pollen on the pistil of the seed bloom, and here, again, the blooms were never covered or protected in any way.

Gloriosa.

G. Plantii grows like a weed here out of doors, increasing very fast both from the tubers and from seed ; but under the same conditions *G. superba* is difficult, and in ten years I have only had two blooms, so I conceived the idea of getting a *G. superba* with the constitution of *G. Plantii* by crossing *G. Plantii* with pollen of *G. superba* and succeeded in the first attempt. Most of the seedlings took after *G. superba* in habit, colour of stem, and foliage, and the blooms also, though perhaps not quite so large ; a few were repetitions of *G. Plantii* ; these latter were fertile, while those taking after *G. superba* have so far been sterile, though they have been blooming for five years and have been tried with pollen of *G. superba*, *G. Plantii*, and *G. Rothschildiana*. All of these seedlings are quite hardy and do well out of doors, flower freely, and increase rapidly from the tubers.

Here my method of working is to cut off an anther and holding this in a pair of pliers to touch with its pollen the pistil of the bloom to be impregnated. In this case, again, the blooms were not protected in any way.

Liliums.

L. speciosum roseum × *L. auratum* gave progeny but little differing from the seed parent; this progeny crossed again with *L. auratum* gave a great variety running from pure white through *L. s. roseum* up to *L. s. rubrum*; many of the white seedlings were highly papillose upon the inner surfaces of the divisions of the perianth and then to the extremities of those divisions; and while there was not one *L. auratum* among all the seedlings, all of them showed the parentage strongly in the foliage. Unfortunately I imported about this time some lily bulbs which must have had disease, and so saturated my garden with lily disease that I practically lost every lily I had. Method of crossing is the same as with *Hippeastrum.*

Sporting.

This is a subject allied to hybridisation, probably the cropping out of a latent effect of the hybridisation of an ancestor, and it seems that with either old age or ill-treatment there is a probability of such a sport. Thus from an old plant of 'La France' rose I have had sports, both 'Mlle. A. Guinoiseau' and 'Duchess of Albany'; from 'Princess Alice' carnation under similar circumstances have sported both white and pink selfs, and these sports have been readily fixed by taking off cuttings high up on growths showing the sport; so also with chrysanthemums.

Graft Hybridisation.

I have only commenced this, so cannot say I have achieved any results; but I will mention two things that happened in my garden. A twenty-five-year-old rose 'Ethel Britten' budded on American noisette developed from the neighbourhood of insertion of the scion a shoot that had a bloom which puzzled some of our best rosarians, who, not knowing what it was, pronounced it midway between the two (stock and scion); and an old neglected plant of rose 'Souvenir de M. Métral' budded on American noisette developed three shoots from about the insertion of the scion. These grew as American noisette stems, thorns, and foliage for a foot or eighteen inches, and then in response to a little kindly treatment all the shoots continued as 'Souvenir de M. Métral,' and eventually bloomed true to that variety.

Bigeners.

I cannot say I have had much success with hybridising between two genera—certainly I raised two bulbs by crossing a *Crinum* with pollen of a daffodil. At first the foliage gave great promise, being bifarious, but gradually in one it changed and became turbinate as in the seed-parent —the other is about midway in habit of foliage; but the bloom of both is merely *Crinum*, and both are thus far sterile though they have bloomed for several years, and have been worked with pollen of both parents. The *Crinum* parent is a most prolific seed-bearer. Another attempt was

Hymenocallis calathina × *Crinum* (? variety); the result is a much dwarfened *H. calathina* which blooms seldom, and the staminal cup is often split. During many years this bulb has produced but one seed; this was last season. Another attempt was *Pancratium* (sp.) × a daffodil; the result was a quantity of seed which came up freely, but gradually died out, so that the third year saw the last of them, and this without any of them blooming.

Attempts to cross *Hippeastrum* with pollen of *Sprekelia* failed, though I have had pods of seed from this cross filled with apparently good seed; but none of it would grow. On the other hand, I have among seedlings of *Hippeastrum* which had fertilised themselves, some which seem to approach *Sprekelia* in form of bloom and in the grouping upwards of the five upper divisions of the perianth, while the bottom division is long and straight.

Amaryllis Belladonna × *Lycoris aurea* gives a plant generally like *A. Belladonna*: the flowers are smaller and have the wavy divisions of perianth of a *Lycoris*, but not to the same extent.

Calostemma luteum × *Pancratium maritimum* gave a quantity of fertile seed, and the seedlings are growing strongly, but none have flowered yet, so it is uncertain if the cross has taken. Curiously, several of the seeds developed two plants, and one seed gave three plants from the one seed.

These notes would be incomplete were I not to mention the names of other local workers in this field. Amongst others G. H. Kerslake, of Potts Hill, who has done much work amongst chrysanthemums, cactus-dahlias, bouvardias, &c.; H. Selkirk, of Killara, amongst daffodils &c.; A. Clark, of Essendon, Victoria, with daffodils; T. Godwin, of North Sydney, with orchids; L. Buckland, of Camperdown, Victoria, with daffodils.

SOME PRACTICAL EXPERIMENTS IN CROSS-FERTILISATION IN NEW SOUTH WALES.

By GEORGE KERSLAKE, of Sydney, Australia.

PROBABLY there are few countries which offer better natural climatic advantages for cross-breeding plants than New South Wales. The range of subjects that can be grown without the aid of expensive glass structures is very large, though at some seasons difficulty is experienced in procuring a supply of pollen, on account of the dryness of the air and hot winds, which frequently render it impotent in a few hours. However, the difficulty is easily overcome by artificial development in a more suitable atmosphere.

In relating a few incidents in my experience here, I do not know how far my methods will agree with those practised in other countries, as in a great measure I have been playing a lone hand in this far-off country, having few of the advantages of intercourse with those of similar tastes and inclinations, which the more populous centres naturally afford. It is my intention to only casually refer to a few peculiarities noted in connection with some of the popular races of flowers, such as the rose, chrysanthemum, dahlia, &c., which have engaged my attention for many years, as in my opinion they offer very few difficulties to any intelligent operator. It is when we get off the beaten track of those subjects which have been crossed and recrossed for a considerable time, that problems so frequently occur which theory may explain.

But their practical solution is quite another matter, for the simple reason that without practical experiment we are ignorant as to whether we are working in a grove which nature will respond to, or one which will defy our most persistent efforts under all circumstances. A case in point may be of interest. A number of blooms of Opuntias were emasculated, and as the period is short during which the unopened flower can be safely operated upon (about ten hours) before the stigma arrives at the receptive stage, a fine jet of water under moderate pressure is directed into the flowers, to remove the stamens after being severed, which is rather difficult in the immature flower. This method is perfectly safe and effective in this dry climate, and is adopted with most Cacti, excepting Epiphyllums and a few others that flower in winter.

In the case of *Opuntia Piccolomini* × *Cereus Spachianus* and of *O. Piccolomini* × *Phyllocactus Schlimmii*, the union was complete in every instance; while *O. elatior* and *O. senilis*, operated upon with the same male parents and under the same conditions, resulted in an absolute refusal. A certain amount of irritation or swelling of the fruit was noticeable for a short time, but neither reached fertility. This behaviour is difficult to explain, as the plants were of equal age and had been fruiting regularly for some years. These experiments are most interesting as showing how some members of the same family will at once respond whilst others positively refuse to be influenced by sexual intercourse.

Lilium tigrinum × *L. elegans Wallacei* resulted in every flower operated upon producing huge pods of seed. But when *Lilium speciosum album* × *L. tigrinum*, and *L. speciosum rubrum* × *L. tigrinum*, and *L. tigrinum* × *L. speciosum album* were tried, the results were very different, as the female organ in a very short space of time showed unmistakable signs of decay, and in a couple of days had quite withered. This was so not in one instance only, but the whole forty blooms used in the experiment showed the same symptoms. As evidence that this was no fault of the prospective seed parent, some later flowers were tried, *L. speciosum album* × *L. speciosum rubrum*, when they at once returned to fertility. The same thing occurred, only under less favourable conditions, with *L. speciosum album* × *L. auratum*, and the same × *L. sulphureum*. However, under the circumstances I am reluctant to conclude, without farther extended trial, that a union of *speciosum* and *auratum* cannot be effected, as so much depends upon what may be termed the seed-bearing mood of the plant, which is often absent when the reproductive organs show the most perfect development.

The family of the amaryllids undoubtedly offers a wide field for investigation. In 1903 a rather extended trial was made to induce a union between *Crinum Moorei* and *Crinum yemense* × *Vallota purpurea*, and *Brunsvigia* × *Vallota*. At the very commencement the indications were not encouraging. However, to make the tests conclusive, they were continued through the whole flowering season, more especially to see whether any atmospheric conditions prevailing during the time would be more favourable for inducing fertility than others: bright and hot days; cool, dewy and cloudy; the cool atmosphere of early morning; the dry heat of midday; and the cool dry evening air, were all alike in failing to produce what appears to me to be a forbidden union.

Crinum yemense × *Hymenocallis macrostephana* crossed without any difficulty, as did also *Brunsvigia Baptisii* × *Lycoris aurea*. But the most interesting was that effected between an unnamed variety of *Hippeastrum* and *Agapanthus umbellatus*. The *Hippeastrum* flowers here in October and November, but in favourable seasons a stray scape is again thrown up in January and February. It was one of these that made this cross possible; otherwise the pollen of *Agapanthus* would have to be preserved for a period of ten or eleven months, which is far too long. There were four flowers on the scape when they were pollinated with *Agapanthus umbellatus*: of these four, one was injured by being too early emasculated, a week before they reached the receptive stage (probably through being produced in the off season). Two flowers perished, but the remaining one produced a full pod of seeds. At this stage, though the seeds were of normal appearance I doubted their fertility. However, they were sown, and nine plants resulted. Three died in the seed pan; but six still remain, which have not yet reached the flowering stage, but are strong and healthy and appear to be evergreen. This being a case of overlapping botanical divisions I never intended divulging it until the plants had flowered. But a recent report that a gentleman near

Sydney had effected the same cross has led me to believe that there is nothing remarkable about it.

I will now briefly refer to the unreliability of sports, as male parents, to impart colour in the chrysanthemum. One or two instances must suffice to illustrate what has proved invariably the rule in practice. In a general way colour can be well controlled in the chrysanthemum. Two varieties raised by me and well known in England may serve to explain my meaning. 'Lady Trevor Lawrence' × 'H. Cannell' resulted in 'Oceana,' and 'Edwin Molyneaux' × 'Stanstead Surprise' produced 'Australie.' This is a fair example of control of colour. But when 'Mme. Carnot' × 'Charles Davis' were used, there was not the slightest indication that 'Charles Davis' was the male parent, as all the resultant plants were either shades of deep mauve, or amaranth, which pointed conclusively to 'Vivian Morell' from which 'Charles Davis' had sported. 'Mrs. Barclay' × 'G. I. Warren' behaved much in the same way as if 'Mme. Carnot' had been the pollen parent. Many other instances could be mentioned which show more conclusively than in any other way that these sportive characters, at least in the chrysanthemum, are only superficial changes which are incapable of being imparted to another. The same may apply to the numerous sports among roses; but as far as my experience goes it cannot be so easily proved, as with very few exceptions the rose appears to be the most uncontrollable, as regards colour, among any of the popular flowers that I have manipulated.

I now pass on to the *Bouvardia*. It is now more than ten years since I commenced to manipulate the *Bouvardia*, which, by the way, does remarkably well in this climate, and treated as "cut-backs" with good culture frequently attains the height of six to eight feet in one season, and flowers profusely for at least six months in the year. I thoroughly believe in an hybridist or plant-breeder having preconceived and definite aims, well thought out, before any action is taken. This involves the choice of parents, which is perhaps the most important factor in assuring success in any future operation, whether it concern fruits, flowers, or vegetables. To a very large extent, on a female parent that can be relied upon to produce good stock, hinges the success or otherwise of future crosses. And when one is found (and there are not too many), work it in all directions, until another is proven to be better. This does not always involve the highest type in these respective kinds, as we shall presently see in reference to the *Bouvardia*. In going through the best cultivated varieties of *Bouvardia* I was struck with the very large sweet-scented flowers of *Humboldtii corymbiflora* in comparison with such varieties as 'President Cleveland,' *Hogarthii*, *Vreelandii*, 'Priory Beauty,' and others of similar size and form. My aim was thus fixed to get the large flowers of *H. corymbiflora*, combined with the larger truss and more free-flowering qualities of the smaller flowered section. With this object in view I chose *H. corymbiflora* as the seed parent on account of its naturally seeding freely, which very few of the other varieties do at all. I do not say they are sterile, as the composition of the soil and environment are such important factors in bringing about fertility, that it is a difficult matter to decide when anything is definitely sterile. For five consecutive years I crossed *H. corymbiflora* with all the best commercial varieties known to me. Large numbers were raised and tested every year, with the result that I was surrounded with a host of mere weeds, none being equal to any of the parents used. At this stage I began to seriously consider whether to abandon the enterprise or adopt some other course to gain my object. The plants previously raised were carefully scrutinised, and one white variety was found, which was raised three years previously, which seemed promising; and as it was already bearing seed this decided it to be the variety to operate upon, as I consider it is waste of time to work upon anything that shows no natural disposition to fruit. This variety, though of no commercial value, was named 'Progenitor' merely for the sake of identification. It proved a difficult variety to emasculate, as the stamens were set deeper in the tube than any variety I have met with; it necessitated laying open the tube nearly down to the junction; a delicate operation as the pistil will not bear exposure without injury, even when fully developed. 'Progenitor' was crossed the first season with 'Priory Beauty,' 'President Cleveland,' *Hogarthii*, and 'Laura.' About one hundred plants were raised from the seeds obtained from these crosses, and eighty reached the flowering stage the following autumn. The control of colour was more complete in this instance than in any other of the numerous crosses my notes record. As I have before mentioned, the seed parent was white, but not one white variety appeared in the whole batch of plants. Those pollinated with 'President Cleveland' and *Hogarthii* were various shades of red, and those with 'Priory Beauty' shades of pink, while the 'Laura' influence resulted in varied shades of salmon. The blooms of most were very large, and in some instances the trusses were immense, while a good many showed a leaning towards the sparsely flowered heads of *H. corymbiflora*. As most of the best still remain here, I can only point you to one which has recently been distributed in England, viz. 'King of Scarlets,' which was among the first batch of meritorious varieties raised. Nearly all produced the sweet jasmine perfume which characterises *H. corymbiflora*, though in 'King of Scarlets' there appears to be a departure, as in it there is a decided leaning towards vanilla.

By the above remarks on the *Bouvardia* I have no doubt the hybridist will perceive a deep object lesson, insomuch that a revolution may be effected when all outward appearances are most discouraging. In the above instance there was three years' loss of energy by persistently following one course, instead of following up any little break of character at first obtained, as this is clearly a case that needed the second generation to accomplish what was only partially done by the first. And the first step in this instance had more the appearance of a retrograde character than an advanced stage. More recently these crosses have been repeated, and have brought about a notable increase in the segments from the normal four to five and six, which gives the flower a rounder and much fuller appearance. I may mention one exceptionally fine form named 'Magnificent' which has outdistanced all others. This usually has six segments, and as the pips are from an inch and a quarter to an inch and a half across, and as about fifty of these go to make an ordinary truss, its appearance at a casual glance is most unlike any *Bouvardia* known in cultivation. However successful one may be in one direction, failure is sure to follow the plant-breeder in another, or at least such is my experience. For many years past I have been repeatedly trying to obtain an improved yellow *Bouvardia*, but I have failed completely to bring about the fertility of *B. flava* or to impart its colour to any other.

In concluding this paper I can only express an oft-occurring thought, how little we know by actual experiment in this vast field of research, as to where Nature is willing to open her rich storehouse to the hand of man, and where, on the other hand, she effectually defies any intrusions, simply because we are ignorant of her natural ways, and have in a certain sense to grope about until we find them.

THE BREEDING OF COLD-RESISTANT FRUITS.

By Prof. N. E. Hansen (State Agricultural College and Experiment Station, Brookings, South Dakota, U.S.A.).

In the great grain- and stock-growing region known as the prairie North-West, comprising the northern part of the Mississippi and Missouri river-valleys of the United States of America, considerable trouble has been experienced in the cultivation of the fruits brought originally from the milder and moister regions of Western Europe. The history of our western prairie horticulture records a multitude of failures from this cause. For example, it has cost, at a low estimate, over £20,000,000 to learn that the apples of Western Europe are not adapted to the climatic extremes of this vast continental region. The periodical crucial-test winters, such as 1855-6, 1872-3, 1884-5, 1898-9, show that they lack resistance against severe winter freezing. Many thousands of seedlings have been raised from this West European race in the endeavour to advance the limits of successful apple cultivation northwestward, but without permanent success, the test winters usually making an end to the experiment. The introduction of the Russian race of apples has advanced this limit far north and northwestward of the former limits, and rapid progress is now being made with their American seedlings and with crosses of the Russian with the West European type, the latter being usually known as the American apples.

Plums from many countries have been tested upon this fertile inland plain, but after "the smoke of battle" has cleared away, the native plums of the prairie North-West (*Prunus americana*) remain in undisputed possession of the field. Great advances have been made in breeding large plums of good table quality from this indigenous race; hybrids with plums from other parts of the world are also coming on. So rapid has been the progress with pure native seedlings that an ingenious western experimenter has ventured the opinion that our native western plums are really descendants from plums brought over in the Mongolian migration from Eastern Asia, especially Japan, by the prehistoric ancestors of the present North American Indians.

The history of raspberry culture is in a measure a repetition of the failure of the apple—an utter lack of success with the West European raspberries. The native prairie race is now being ameliorated.

While working in fruit-breeding since 1895 in South Dakota, and for some years previous to that time in Iowa, my endeavour has been to discover some underlying law in the ever-shifting panorama of phenomena. Two years ago the number of fruit seedlings was fully a quarter of a million; the number has since been augmented by the raising of many thousands of new seedlings, and decreased by the destruction of many thousands of inferior seedlings every year.

The explanation that is to my mind the most satisfactory is the law of De Candolle, given in his "History of Cultivated Plants." De Candolle states that no species of plant has extended 100 miles north of its original limits within historic times, although seeds have been carried far north of the original limits by birds and other agencies; and that changements of form or duration are required, or periods of four or five thousand years are needed, for it to endure a greater degree of cold. In testing a collection of over 500 varieties and species of ornamental trees and shrubs from various parts of the world, their different degrees of hardiness under severe climatic conditions, which meant at one time 40° Fahr. below zero with the ground bare of snow, led me to investigate the geographical range of each species. De Candolle's law of hardiness, just quoted, appeared to me a satisfactory explanation of the behaviour of these many species of plants.

In connection with the law just quoted it will be of interest to give some of the costly experience, in the prairie North-West, of our nurserymen and tree-planters. I will give only a few out of many instances.

The box elder (*Acer Negundo* or *Negundo aceroides*) from Virginia, winter kills in the North-West, while the local form of the box elder indistinguishable from it by botanical characters, is perfectly hardy. The tree-planters of Manitoba have found that the box elder from several hundred miles south will not endure Manitoba winters, while the same species from their own locality, known as the Manitoba maple, is perfectly hardy. In Russia the box elder was once considered tender in the north; it was found that their seed was gathered near St. Louis; since then seed from the far north has been tested and found hardy.

The red cedar was formerly brought to the north in large quantities from Tennessee, which is well to the south. Northern nurserymen have learned that they must cultivate only the northern form of the red cedar to avoid total failure.

Robert Douglas, of Illinois, found a great difference in hardiness between the northern and southern forms of the black walnut. The former lived, the southern died, in Northern Illinois.

They have found also that they must be careful as to the source of western conifers. For cultivation in the prairie North-West, the form from the Pacific slope of the Rocky Mountains is tender, while if gathered on the eastern slope of the Rockies it is hardy.

Much more evidence might be given along the same line, all going to show that Nature has done this great work of acclimatising species, but that thousands of years are needed for the work. The converse of the law appears to be true, that a species cannot be extended southward to any great extent beyond its natural limits. Munson, of Texas, has found that the northern *americana* plums are winter-killed in Texas, because the buds start prematurely at the first warm spell in mid-winter. In Russia a similar tendency has been noticed with the Siberian larch when brought into Southern Russia.

It will be noted that this law applies only to wooded plants that must endure the winter, not to annual plants such as Indian corn or maize. This species has been advanced many hundreds of miles northward by shortening the season. In what is considered its native home, in the tropical or semi-tropical regions of South and Central America, it takes seven months for maturity, and attains a height of twenty feet, while at its northern limits it is five feet or less in height, and takes less than three months for maturity. This work was done by Indians long before the advent of the white man. It will be noticed that the plant has not lost its need for a high degree of heat during its ripening period, and has not increased its power of resistance against frost. In the winter-time the dry seed, if kept dry, will resist any degree of cold. The plant, in other words, has been shortened in season only, and not changed in its capacity to endure cold.

It is now quite evident, from a survey of the whole field, that hardiness cannot be obtained by selection alone. This is the work for Nature, not man, to undertake. It is unprofitable for him to begin a labour that takes many thousands of years for completion. But hardiness can be obtained by crossing with a hardy species.

As to hardiness being a Mendelian character, I know not. In our work of selection hitherto we have insisted on large size and good quality of the fruit, as well as hardiness, which has compelled the destruction of thousands of inferior-fruited seedlings which were hardy; and in plants propagated by budding and grafting it has not been necessary to fix the type. That hardiness can be transferred, by crossing a tender species, now admits of no question. For example, my hybrids of the wild prairie strawberry with the French ever-bearing type survive, while the French parental type the winter kills. The hybrid of Western sand cherry with a Chinese apricot is hardy, while the Chinese species (*Prunus Simoni*) is winter-killed.

The question arises—"What is hardiness?" Some fifteen years ago the Iowa State Horticultural Society had an investigation conducted to determine the nature of hardiness in the apple. Chemical examinations were made of the wood of hardy and tender varieties; the cell structure was examined under high powers of the microscope, and the number of palisade cells in the leaf was investigated. All led to negative results. It would be a great advantage to be able to determine by chemical or histological examination as to whether a new variety of apple would prove hardy in our test winters, but we must regard the problem as unsolved. Hardiness can be transmitted; it is something intangible to superficial examination, but inherent in the plant itself.

The United States Department of Agriculture has extended the citrus fruit belt northward, by hybridising the cold-resistant *Citrus trifoliata* of Japan with choice sweet oranges. This work is of the greatest possible value, and incidentally illustrates this same possibility of hardiness being imparted by crossing with a hardy species.

There is a limit to the northward extension of the cultivated apple (*Pyrus Malus*) even by the hardiest representatives of the Russian race. This is very likely determined by the cold-resistant capacity of the indigenous race of *Pyrus Malus* in Russia itself. To extend the apple limit north-westward it will be necessary to hybridise with the pure Siberian crab (*Pyrus baccata*); this is now being done in many places. The work of Thomas Andrew Knight one hundred years ago in England, would help in this work, were it possible to find the hybrids of this ancestry which he originated at that time. However, from the thousands

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of seedlings of this parentage so far produced, by design or as chance seedlings, in the United States, we have not secured the winter-keeping capacity which is so greatly desired. To illustrate this need, I may add that the Minnesota State Horticultural Society has offered one thousand dollars reward to anyone who will originate an apple equal in hardiness to the 'Duchess of Oldenburg,' in size and quality to 'Wealthy,' and in winter-keeping capacity to 'Malinda.' So far the prize has not been awarded.

One of my favourite plants is the Western sand cherry (*Prunus Besseyi*); this bush form of the cherry is a native of the dry plains of the North-West, and is a favourite fruit of the Sioux Indians. In going over a patch of 25,000 plants of the third generation under cultivation last summer, some plants were found bearing fruit fully one inch in diameter, and of good quality; this shows the cumulative effect of selection. Apart from its being a promising species from the fruit standpoint, it is of interest as a possible dwarf stock for peaches. I find that peaches upon this stock bear as potted trees when less than three feet in height.

The converse of the law also appears true, that the southern limits of a species may be extended by crossing with a heat-resistant species. The best example I know is the Kieffer pear. This pear originated as a chance seedling near Philadelphia, and is a hybrid of the Chinese sand pear with the Bartlett or some other choice representative of the West European type. This variety has made possible the extension of the commercial pear belt several hundred miles southward in the United States. We hope now to obtain better quality by infusing a larger percentage of the European species in the Kieffer and its seedlings.

In illustrating the work of plant-breeders in general, I have sometimes said that we are looking for the Shakespeare of the species, and that no minor authors will suffice. The light thrown upon plant heredity by Mendel and De Vries gives us great hope for the future. The De Vries mutation theory itself has been aptly termed a mutation of Darwinism.

The modern plant-breeder rides an automobile upon the highway of evolution, and the theories of De Vries and of Mendel may be two of the wheels. It is a great and inspiriting thought that evolution is, so to speak, a kangaroo and not a snail, and that a new and valuable plant may appear suddenly, as Minerva sprang full-fledged from the head of Jupiter.

We are thankful for all this new and recent light upon heredity, and are endeavouring to apply the principle in as many ways as possible. But we must deal in large numbers. From the ashes of millions of seedlings must arise, phœnix-like, the new creations which will dominate our prairie pomology.

HYBRIDS AMONG THE *AMARYLLIÆ* AND *CACTACEÆ*, WITH SOME NOTES ON VARIATION IN THE *GESNERACEÆ* AND THE GENUS *SENECIO*.

By A. Worsley, F.R.H.S.

Whether the Mendelian hypothesis accounts for all the variations noted in hybrid and cross-bred plants and their descendants, or only for some such variations, is, I think, an open question. Certainly in mongrels and florists' plants generally it has been ascertained that colour variations follow Mendelian law to some extent.

But colour is only one character among many. It is a character which is by no means constant even in species, and is one in which we are sure of noting variable results among garden plants even without the advent of foreign pollen. How variable such results may be is well known to horticulturists, and I have tabulated some of these results for purposes of comparison. (See Cineraria, p. 413).

As to form, it does not appear that the Mendelian hypothesis has been demonstrated. In some cases the results have been in accord therewith, but in others diverse and even contradictory results have been noted by various observers.

Now, it is perfectly clear that if, subsequent to cross-fertilisation, mutations occur according to any definite rule and uninfluenced by any external conditions whatever, such mutations have their cause and their continuation solely in the act of cell-union; then (in such a case) we may trace back mutation in its entirety to this one act, and to the subsequent growth of protoplasm. Moreover, we must greatly modify the accepted theory of plasmic homogeneity. For if from one union of diverse cells, wholly uninfluenced as to essential character by any subsequent external conditions, *must* spring diverse individuals differing from each other in essential characters of a predetermined kind; then, clearly, we must contemplate heterogeneity of protoplasm; and must realise that, notwithstanding aspectual similarity, there must exist in such cases an essential heterogeneity or diversity of protoplasm such as exists between different species. Not only so, but we must also admit that the incidence of such heterogeneity is governed by Mendelian law.

If we maintain that every permanent character * is acquired by cell-union and in no other way, then it is clear that an individual in which every cell was normal could only produce, if self-fertilised, absolutely

* That is, a character capable of being reproduced in offspring by any sexual process.

typical descendants. Such a plant with its descendants would constitute a "good" species, such as earlier botanists believed in, but which we now know has no existence outside the imagination of some. No! Quite clearly is it borne in upon us that abnormal or variant cells exist in every organism, and it is to the union of such cells *inter se*, or with those which are normal, that we must trace every variation from the parent occurring in self-fertilised plants. In such cases the union of normal cells produces typical offspring; the union of abnormal cells with normal cells produces non-typical or variant offspring (instances of mutability); and the union of two abnormal cells (where such union can bear fruit) may reasonably be supposed to produce deformed, erratic, and often sterile progeny.

Perhaps the comparative fixity of some species may be due to the fact that the abnormal cells are, in such cases, not only few but also unable to mate with those that are normal, or that such unions are sterile. Again, the extreme mutability of florists' plants may be due to the great relative number of abnormal cells rendering almost every class of variant possible among the offspring.

It is the task of the exponents of Mendelism to discern whether cell-union mutability in its widest sense is really governed by any law which can be made clear to us by mathematical expression.

Now, it seems to me that the exponents of the Mendelian thesis have established a *prima facie* case, but I do not think that up to the present they have done more than this. The majority of their experiments have unfortunately been carried out among extremely mutable plants of garden origin, rather than among comparatively stable species.

In my own experiments I have dealt with this latter class of plants, and I cannot say that my results have done much to establish their contention. In general I have found that inter-specific hybrids are fairly equipoised between their parents, without showing a complete dominance of one over the other in any one character. In cross-bred plants I have noted the tendency of a certain percentage to revert to the ancestral (specific) types. This is notably the case in the Garden Hippeastrums, in which reversion to *H. vittatum*, or at least to the colour-markings of *H. vittatum*, is common. If any Garden Hippeastrums in which white is the predominating colour be self-fertilised, several individuals will probably revert to *H. vittatum* as far as colour and markings are concerned, although retaining the size, and often the width and regularity, noticeable in the perianth segments of the parent. Such reversion is not to any *H. vittatum* which I have ever seen wild, but rather to a glorified form of the species, such as might have been produced in gardens by many generations of careful selection. In fact, what I have noticed is a colour reversion rather than a general reversion, and does not bear out the Mendelian law in its entirety.

In general I have noticed that cross-bred (mongrel) plants which possess great colour-variety continually produce from seed a fair percentage of individuals bearing the ancestral colour or colours. These plants are always the best seed-bearers, and those most widely removed therefrom in colour are generally infertile or nearly so. This is specially noticeable in *Cinerari*.

A LIST OF SOME ASCERTAINED INTER-SPECIFIC HYBRIDS † AMONG THE *CACTACEÆ*.

	Parentage.		*Name of Hybrid.*	*Authority.*
1.	Phyllocactus crenatus	× Cereus grandiflorus	= P. Cooperi	Nicholson *
2.	,,	× P. phyllanthoides	= P. Vogeli	De Laet
3.	,,	× Epiphyllum sp.	= alleged hybrid, *circa* 1870	Hovey *
4.	,,	× (unknown)	= ,, ,, ,,	"Bot. Reg." 1844, 31 *
5.	P. Cooperi	× P. Ackermanni	= P. Coopermanni	Worsley
6.	P. Ackermanni	× P. Cooperi	= P. Helenæ	,,
7.	,,	× C. speciosus (Schumann)	= C. Feastii, &c.	De Laet
8.	,,	× C. MacDonaldiæ	= (unflowered)	Worsley
9.	C. MacDonaldiæ	× Phyllocactus sp.	= ,,	Kew
10.	C. speciosissimus	× P. Ackermanni	= C. unnamed	Laboret
11.	,,	× C. flagelliformis	= C. Mallisoni ("Bot. Mag." 3822)	,,
12.	,,	× ,,	= 5 more alleged hybrids	,,
13.	,,	× (unknown)	= C. hybridus	Hort. Berol.
14.	,,	× ,,	= Epiphyllum (?) hybridum	Hort.
15.	C. grandiflorus	× C. speciosissimus	= C. Haagei	Laboret
16.	,,	× ,,	= C. Maynardii	Lem.
17.	,,	× ,,	= 7 more alleged hybrids	Laboret
18.	,,	× C. affinis (?)	= C. albisetosus	Monville *
19.	,,	× P. crenatus	= P. Wrayi	Gürke *

NOTE.—All those starred have some doubt attached to them, which is dealt with below.

Some, at least, of the genera in the order *Cactaceæ* are badly founded, and it is quite clear that *Cereus* and *Phyllocactus* are one genus, and not two. Presumably the latter habit of growth is antecedent to the former (geologically), following the known sequence of change (differentiation) from the simpler to the more complex. Hence crosses between the two, however many-angled they may be in their seedling stages, approximate to *Phyllocactus* (in the stem) when they reach the flowering stage. ‡

Hybrid No. 1.—I do not think that the original *P. Cooperi* was a hybrid. Mr. Cooper, after whom it was named, tells me that at the time no hybrid origin was claimed for it. Beyond this it exhibits no trace of hybrid origin, and is in close alliance with *P. grandis* (Lem.), although not in identity therewith. I have, therefore, treated *P. Cooperi* as a species. The splendid (white to lemon-yellow) flowers of *P. Cooperi* are borne generally on the lower half of the stems, and often on the ground-level, but are occasionally also terminal. It is a night-opening cactus, and each flower lasts for two nights and two to three days; for a few hours shortly after expansion, they exhale an astonishing lemon-like fragrance. It fruits freely, and may be grown in a cool house. Even admitting the contention of Messrs. Nicholson, de Laet, and others, that hybrids have been raised identical with *P. Cooperi*, this would not prove that the original *P. Cooperi* was a hybrid.

† Throughout this paper the female parent (of every cross-bred plant) is given first.

‡ Some exceptions exist.

Hybrid No. 3.—Raised at Boston.

Hybrid No. 5.—This is fairly equipoised between either parent. Of the eight divergent characters, the hybrid was nearest the female in four, nearest the male in one, and intermediate in three. There was no absolute "dominance" in any character excepting the colour of the stigma. Sixty-five individuals were thus generally equipoised, and two were indistinguishable from the female parent (see Table I.). Some of these (individuals) were indistinguishable from the 'Isabel Watson' raised at Kew.

Hybrid No. 6 is also equipoised, and without any instance of absolute dominance. In the eight divergent characters, the hybrid was nearest the male in three, nearest the female in one, and intermediate in four (see Table II.). The flowers of this hybrid are larger than either parent, and are hence easily distinguishable from those of hybrid No. 5.

Hybrid No. 7 is said by Monsieur de Laet to have produced the most beautiful colours, to some of which such names as 'Conway Giant' have been given.

Hybrids Nos. 10–14 were all raised out of *C. speciosissimus* [*C. speciosus*, Schumann], which is three-, four-, or (generally) five-angled, and carries smallish but widely expanded red flowers, flushed with magenta. From the results of crosses with this species have originated most (if not all) of those garden *Cacti* which are flushed with magenta; 'Isabel Watson' is the most beautiful example of this class. *P. Ackermanni* (as male) produced plants with three or four angles, but which became ultimately (in the flowering growths?) two-sided and unarmed, and Laboret said that for this reason he classed *Cereus* × *Phyllocactus* hybrids under *Phyllocactus*. *C. flagelliformis* (as male) is said by Laboret not only to have produced the *C. Mallisoni* ("Bot. Mag." 3822), but five other (presumably distinct) hybrid *Cerci* which were all more or less like the parents, and those which he had seen (in flower?) had the metallic flush of the female.

Hybrids Nos. 15–18 were all raised out of *C. grandiflorus*—one of the so-called "rat-tailed" section, which generally (as in the case of this species) carry very large and beautiful, but ephemeral and night-opening, flowers. Those of *grandiflorus* are white and fragrant. *C. speciosissimus* (as male) produced the five-angled hybrid *Haagei,* and also *C. Maynardii* (Lem.), whose bright red flowers remain open several days. But besides these, Laboret lists seven (other) hybrids as resulting from this same cross! *Hybrid No.* 18 was brought from the Antilles, and is said to have been raised by *C. affinis* (Salm.). It is seven-angled, and Laboret suggested that it may be a distinct species.

Hybrid No. 19.—I have only seen the flowers in a partly faded state, but the growths are very far removed from those of *C. grandiflorus.*

Alleged Hybrids and other Crosses. Good Species, &c.

Phyllocactus crenatus is said to have been crossed with an *Epiphyllum,* by Mrs. Hovey, of Boston, about 1870.

Mr. Peacock raised a number of cross-bred plants, and is said to have raised "hybrids" on *P. crenatus.*

Monsieur de Laet has used *P. Ackermanni, crenatus,* and *phyllanthoides* in hybridising, and has ascertained by analysis of seedlings that

P. anguliger, Ackermanni, grandis, latifrons, phyllanthoides, strictus, crenatus, and *Hookeri* are good species.

Laboret listed thirteen hybrids as "known by the Catalogues," * but which were, apparently, only names to him. Of the twenty-three alleged hybrids between *Cereus* (of the section to which *C. grandiflorus* belongs) and various *Phyllocacti,* one at least (*C. latifrons*) has been since admitted as a species. Yet this does not disprove the allegation that it was also raised by hybridisation, for the crossing of extreme forms is sure to produce a mean, just as the cross between *Crinum pedunculatum* and *C. zeylanicum* (Bury, Hex. 30) produced a plant indistinguishable from *C. amabile* [*C. augustum*].

Explanation of Tables. Page 410.

Under "Reversion" are tabulated all instances in which some (often supposititious) ancestral character reappears, or in which the characters of other species appear. The appearance of characters unknown in either parent and not traceable in any (presumed) ancestral or collateral species has been tabulated under "Reversions not traceable to either," although some might argue that such were something new.

Fractions have been made use of in cases in which it was impossible to decide the category; for instance, hybrid *Cacti* which bore two-sided and three-sided stems on the same plant would appear in each category as ½.

A refers to *all divergent characters,* viz. those which are dissimilar in the parents *inter se.* These are usually equipoised in hybrid offspring, but occasionally an odd plant may show dominance of one parent.

B refers to the *colour* of the perianth (and of stigma in *Cacti*).

C refers to the *form* (shape) of the perianth.

D to *colour-markings* on the perianth.

E to *foliage* (stems in *Cacti*).

F to *rootstock.*

G to *habit* of entire plant.

H_1 to *time* of expansion of flowers (whether day- or night-flowering).

H_2 to the carrying of *terminal flower-buds* in addition to those that are axillary.

J to the *fragrance* of the flowers.

K to the carrying of aërial in addition to terrestrial *roots.*

Hybrids among the Amaryllíæ

(Excluding *Narcissus, Galanthus,* and other hardy genera).

I dealt very fully with these in the "Gardeners' Chronicle" of January and February 1901, when I gave a list of 27 hybrids. Since then some more have been notified.

As *Elisena* and *Ismene, Cyrtanthus* and *Vallota, Eucharis* and *Urceolina,* and (probably) *Brunsvigia* and *Amaryllis* have been reconciled, it will be difficult in future to support the contention of so many genera as have been admitted by some authors.

Among hybrids raised by myself are the following:—

1. *Crinum scabrum* × *C. Moorei* = *C. Worsleyi* (W. Watson in "Gard. Chron." Feb. 2, 1901). A beautiful plant, equipoised between its parents. Sterile.

2. *Crinum giganteum* × *C. amabile* = *C. amanteum.* Fairly equipoised, but inferior in beauty to either parent. Probably sterile.

* Besides those noted in the above list of hybrids as known to him.

—	Equipoised.	Nearer Female.	Identical with Female.	Nearer Male.	Identical with Male.	Reversions to Ancestral or Collateral Characters.	Reversions not Traceable to Either.	Colour of Perianth. Albinos.	Colour of Perianth. Selfs.	Colour of Perianth. Bicoloured.	Colour of Perianth. Tricoloured.	Colour of Stigma like Female.	Colour of Stigma like Male.	Colour of Stigma Equipoised.
TABLE I.														
A . .	65	—	2	—	0	—	—	—	—	—	—	—	—	—
B . .	14	—	$32\frac{1}{2}$	—	0	—	$20\frac{1}{2}$	—	—	—	—	0	67	0
C . .	—	$44\frac{1}{2}$	—	$22\frac{1}{2}$	—	—	—	—	—	—	—	—	—	—
D . .	—	—	—	—	—	—	—	0	42	24	0	—	—	—
E . .	$36\frac{1}{4}$	—	—	—	—	$30\frac{3}{4}$	—	—	—	—	—	—	—	—
H_1 .	28	—	12	—	27	—	—	—	—	—	—	—	—	—
H_2 .	—	51	—	16	—	—	—	—	—	—	—	—	—	—
J . .	28	—	16	—	23	—	—	—	—	—	—	—	—	—
K . .	—	62	—	5	—	—	—	—	—	—	—	—	—	—
TABLE II.														
A . .	9	—	0	—	0	—	—	—	—	—	—	—	—	—
B . .	7	—	0	—	2	—	0	—	—	—	—	1	8	0
C . .	—	4	—	5	—	—	—	—	—	—	—	—	—	—
D . .	—	—	—	—	—	—	—	0	9	0	0	—	—	—
E . .	6	—	—	—	—	3	—	—	—	—	—	—	—	—
H_1 .	2	—	5	—	2	—	—	—	—	—	—	—	—	—
H_2 .	—	1	—	8	—	—	—	—	—	—	—	—	—	—
J . .	4	—	3	—	2	—	—	—	—	—	—	—	—	—
K . .	—	2	—	7	—	—	—	—	—	—	—	—	—	—

Table I.—Analysis of *Phyllocactus Ackermanni* × *P. Cooperi*, all from one fruit.
The female is day-flowering, blood-red self with purple stigma, and scentless; the male is night-flowering, white self with white stigma, and very fragrant.
NOTE.—Three orange-flowered individuals appeared out of sixty-seven.

Table II.—Analysis of the reverse cross (*Cooperi* × *Ackermanni*), all from one fruit.
NOTE.—No orange out of nine individuals flowered.

NOTE.—Treated as garden plants, these two hybrids are distinct from each other, and, especially under the characters H_2 and K, show botanical dissimilarity.

3. *Ismene calathina* × *Elisena longipetala* = *Ismene festalis.* Fairly equipoised, but of a stature and size of flower far exceeding either parent. Herbert mentioned the alleged existence in the Andine regions of giant Ismenes attaining about the same stature as this hybrid. Sterile.

4. *Hippeastrum vittatum* × *H. equestre* = *H. mandevilli.* Inclining somewhat towards the female, especially in colour and markings. Fertile.

5. *Hippeastrum vittatum* × *H. rutilum* = *H. rutilo-vittatum.* Inclining towards the female, some individuals being indistinguishable therefrom. Fertile.

6A. *Hippeastrum aulicum* × *H. vittatum* = *H. Ruriki.* Strictly equipoised. The flowers are smaller than either parent, but very brilliant in colour. Sterile, as are all hybrids raised on *H. aulicum.*

6B. The same hybrid, but raised on a tawny-red form of *H. aulicum.* This produced two types of flowers, one of a pale amethystine-pink, utterly dissimilar from those of 6A, and unlike either parent. [A collateral relative of the female has mauve-coloured flowers (*procerum*).] The other type of individual resulting from this cross was indistinguishable from the type produced by 6A. No intermediate forms have yet appeared. Probably sterile.

7. *Hippeastrum aulicum* × *H. equestre* = *H. aulictre.* Nearly equipoised, but inferior in beauty to either parent.

8. *Hymenocallis filamentosa* * × *H. Moritziana* = *H. Erustii.* A robust hybrid, fairly equipoised, but notably like the male in the foliage. Carries seeds, but all have failed to germinate.

Other recent hybrid *Amaryllicæ* that I have seen are :

1. *Nerine flexuosa* × *N. undulata* = *N. erubescens* (ex Kew), probably sterile.

2. *Nerine roseo-crispa* (ex hort. Elwes). Nearer the male. Sterile (?).

3. *Nerine pudica* × *N. corusca* = *N. Zoroasteri* (ex hort. Elwes). Fairly equipoised, but like the female in the foliage ; a beautiful plant. Fertile. [I have raised this hybrid since.]

4. *Nerine flexuosa* × *N. pudica* (ex hort. Elwes). Fairly equipoised, beautiful. Sterile.

5. *Hæmanthus Katherinæ* × *H. magnificus* = *H. Andromeda.* Raised by Mr. Hoog of Haarlem. This robust and fairly hardy hybrid is nearer the female in the flowers. Fertile.

6. *Callipsyche mirabilis* × *C. aurantiaca* = *C. kewensis.* Strictly equipoised. The same hybrid has been raised by Sir Charles Strickland, Bart.

The following hybrids and plants are on record, but I have had no opportunity of investigating their parentage :

1. *Crinum Kirkii* × *C. longifolium* = *C. Kircape.* On record.

2. *Crinum Todaræ* is said to be a hybrid raised in Italian gardens, and presumably named after Prof. Todaro. I have this alive, but have never seen the flowers.

3. *Cyrtanthus lutescens* × *C. Tuckii*, raised by Mr. O'Brien, is said to be not unlike *C. O'Brieni.*

4. *Hæmanthus Katherinæ* × *H. puniceus.* On record. Fertile.

* Not far from *H. amœna.*

5. *Hæmanthus hybridus* (?). Various fine forms, possibly of hybrid origin, and evidently related to *H. Lindeni*, have recently been sent out by Messrs. Linden, but without any genealogy. Some are very beautiful.

Various Hybrids and Attempted Hybrids that have come under my notice.

Tropæolum Lobbianum × *T. majus* produced fertile hybrids nearest the male, but have the winter-blooming character of the female. I think there is only one species including both these names.

Tropæolum Lobbianum × *T. peregrinum* = *T.* 'Richmond Comet.' This is equipoised and carries seed freely, but none of it will germinate. Mr. Gumbleton sent me a similar plant of unknown parentage from gardens at Darmstadt, under the name of *T.* 'Isola Bella,' which fails to seed with me.

Raspberry × Blackberry (*R. incisifolia ?*) = Logan-berry. Fruit like a large raspberry, but foliage distinct from either. Blackberry blood clearly evidenced in thorns and especially in stipules. An American hybrid raised by Judge Logan.

I experimented with several *Gesneraceæ*, especially with *Gloxinia* (*Sinningia*), *Tydæa*, *Isoloma*, *Nægelia*, and *Lietzia*. Notwithstanding the known affinities of some *Gesneraceæ*, I was unable to reconcile *Gloxinia* (as the female) with any of the others, although by pollinating *Isoloma* with *Gloxinia* I produced some seedlings with almost regular and round-spotted flowers which were absolutely sterile and pollenless. Moreover, these seedlings carried a great number of multi-petaled flowers having the same number of segments (7) as their putative male parent (the *Gloxinia*) had. Still, I never regarded these as hybrids, because these characters only appeared in a very small percentage of the seedlings, the great majority being apparently pure Isolomas.

I made many attempts to reconcile the single-flowered *Habranthi* and *Zephyranthes* (between which I fail to discern any generic difference) with *Hippeastrum* (true). *Hippeastrum*, as female, never formed fruit with this pollen ; but as male, rarely failed to impregnate these *Habranthi* and *Zephyranthes*. Yet the seedlings were all typical *Habranthus* or *Zephyranthes*. I made fully thirty such crosses, and have raised as many as seven generations of self-fertilised plants from them, but without the slightest trace of *Hippeastrum* showing in any of them. Some would call these "False Hybrids," but I think that, in such cases, we should be satisfied to call them "Attempted Hybrids."

The two following hybrids were raised at Kew:

(1) *Cheiranthus kewensis* [*C. mutabilis* × *C. Cheiri* (hort.) = *C. hybridus* : *C. mutabilis* × *C. hybridus* = *C. kewensis*], see "Gard. Chron." 20/2/04 with fig. The results of these crosses resemble the female in habit, in size of flowers, and continuity of blooming, rather than the male, but have the fragrance of the male. *C. kewensis* is fertile, and nearly, if not quite, hardy in the London climate. Out of perhaps thirty or forty seedlings of *C. kewensis* raised at Isleworth one only has produced flowers the size of those of the original male parent [*C. Cheiri*

(hort.)]. Perhaps this instance may be claimed as partially confirmatory of Mendelism.

(2) *Primula kewensis* [*P. floribunda* × *P. verticillata*], see "Gard. Chron." 3/3/00; and 31/3/00 with fig. "Garden," 2/06, p. 67; and 8/06, p. 62. A chance hybrid of which only one plant was noted, but since verified by experiment at Kew ["Garden," 3/2/06]. This is apparently an instance of a new species originating through a single hybridisation, for this hybrid reproduces itself from seed with the fixity of character usually associated with old-established species. It is nearly equipoised between either parent, but perhaps leans towards *floribunda*, which it resembles in its extended period of blooming, but shows some trace of the mealiness of *verticillata* in its foliage. In this hybrid we have an interesting instance of mutation in primary sexual characters. For some years the hybrid was regarded as sterile, but presently one plant was noticed to bear both the typical pin-eyed flowers and also some which were thought to be thrum-eyed. On pollinating the latter with the former, fertile seeds were, for the first time, produced. Since then the seedling plants usually carry both (alleged) classes of flowers and seed freely enough. Mr. Bateson maintains that none of the flowers are strictly thrum-eyed, but that the difference is merely one of length in the stamens. At least this is undoubted, that this mutation in a primary sexual character was associated with fertility in a plant previously sterile. This hybrid is not hardy, and does not thrive very well with me.

Colour Changes.

I have had no experience of new colours arising except through hybridisation; other instances of colour-mutation which have come under my observation may be classed as reversion, recurring erraticism, or albinism.

A Cineraria, *crimson with white eye*, isolated and self-fertilised, produced:

19 crimson with white eye
8 white self
9 purple self
1 pink self
7 bicoloured (all blue and white)
1 tricoloured (white, red, and purple)
—
45 plants.

The only apparently new colour is blue, appearing in 37 per cent as blue or purple. This is the ancestral purple of *C. cruenta*, appearing as red or blue, or (together) as purple. We find these three colours in some shades and combinations in 82 per cent., and the remaining 18 per cent. are albinos. No new colour appeared. 42 per cent. were the colour of parent, in 78 per cent. such colours were partially present, 40 per cent. showed some loss of colour, and 20 per cent. complete loss.

A self-fertilised *Gloxinia* showed 65 per cent. the colour of the parent, 100 per cent. showed some of the colour of the parent, 35 per cent. some loss of colour. No new colour appeared.

In the two species, *Agapanthus Mooreanus* and *Lobelia tenuior*, both self-fertilised, from 83 to 90 per cent. came true to colour, from 10 to 17 per cent. were albinos. No new shades.

Phyllocactus Cooperi (white) × *P. Ackermanni* (red) gave nothing but reds and pinks. Reverse cross gave 33 reds, 14 pinks, 3 orange, and 17 magenta. The appearance of orange is easily accounted for. The blue shade is perhaps due to a colour-erraticism latent generally in *P. Ackermanni*, but almost typical of several *Cacti*. Even in *P. Ackermanni* the stigma is always bright purple.

Pink appears as a result of crossing red or orange with white, as in

Hippeastrum (p. 411, hybrid 6B).

Kalanchoë kewensis.

Begonia weltoniensis [both in cross and reverse cross].

But red crossed with white also produces orange in at least a certain percentage of seedlings, as in the results I have obtained in crossing

Hippeastrum (crimson self × *H. vittatum*).

Phyllocactus Ackermanni × *P. Cooperi* (3 orange out of 67).

Pink is also a colour-erraticism dominant in some plants (or is the result of crossing blue with white). In my garden I had only blue and white forms of *Scilla festalis* for some years, after which a number of pink seedlings appeared. This pink form is well known, but was absent from my garden. Perhaps there is some red in the purplish-blue forms of the blue *S. festalis*, which may result in pink, if the result of a cross with an albino is to destroy the blue colour cells (?). Otherwise the pink is a latent erraticism. Or possibly the albino is a hybrid and casts coloured (fixed) forms in a certain ratio according to Mendelian law.

Certain red Cannas and yellow Cannas were both tested and came true from seed when self-fertilised. When crossed, orange-reds and brick-reds predominated. When the yellow was the female, all the crosses were equipoised as to colour. When the red was female, 8 per cent. were pure reds, 92 per cent. equipoised.

A crimson-self *Hippeastrum* crossed with *H. vittatum* gave 4 crimson selfs, 4 bore the colour and markings of *H. vittatum*, and 31 were of intermediate colours (this is a colour analysis only).

HYBRIDS OF *HEMEROCALLIS*.

By G. Yeld, M.A., F.R.H.S.

I have been engaged for many years in hybridising the *Hemerocallis*, so well known to the gardener through its most popular species *H. flava*.

H. flava was a prominent plant in the garden of the house I now occupy when I came to it thirty years ago. It pleased me well. Then I saw *H. Middendorffii* in the York nurseries of Messrs. Backhouse & Son. The sight of it suggested to me that a cross between it and *flava* would be likely to produce a good flower. I made the cross and was delighted with the flower which rewarded me. This seedling was named 'Apricot' to indicate the colour. It is, roughly speaking, about half-way between the parents—varies considerably in height—and has a pleasant fragrance. It received an Award of Merit from the Royal Horticultural Society at the Temple Show, 1893.

In 1895 I received an Award of Merit from the Royal Horticultural Society for another hybrid, named 'Frances,' very dwarf, but in other respects like 'Apricot.' It has unfortunately died out. I think it must have been from *minor* (syn. *graminea*) crossed with *Middendorffii*. The mother plant dies out in my garden, so that, assuming the parentage to be correct, it is not surprising that the hybrid perished.

From *H. flava* crossed with *H. Dumortierii* (syn. *H. Sieboldii* and *H. rutilans*) I have obtained a number of plants which vary very considerably in colour and height.

The best is perhaps 'Flame,' orange with maroon on the back of the flower, and having deep maroon buds. It is not so tall as *flava*. A large clump makes a brave show.

'Estmere' is similar but is more like *flava* in shape, it is sometimes as tall as *flava*, and has a more open flower with less maroon on the back.

'William Dean,' as tall as *flava*, is of deeper orange with but little maroon on the back of the flower.

'Beauty' has a shapely, round, well-opened flower with dark maroon back. It was exceptionally good this summer.

There are other forms too. The flowers vary in height as well as in colour, some being as tall as *flava*, others, especially 'Flame,' being considerably shorter. These hybrids have in a more or less degree the unpleasant smell of *H. Sieboldii*.

The forms 'Gold Dust,' 'Sovereign,' and 'Orangeman,' now in commerce, are very similar to—if not identical with—my *flava* × *Sieboldii* hybrids, but I believe my plants were first in the field.

I have a very deep-coloured, low-growing form named 'Tangerine,' the origin of which is lost. It is very distinct and comes late enough to escape the frosts which often severely injure *Middendorffii*. Its height varies from just over to just under a foot.

From *flava* × *aurantiaca major* I have obtained a very fine flower named 'Corona,' which gained an Award of Merit from the Royal Horticultural Society in June 1905. The blossoms are large, well shaped, well opened, fragrant, and of a rich orange-yellow. The plants vary somewhat in height; the tallest, which have, I think, the best flowers, reach 4 feet. (The average height of *flava* in my garden is about 3 feet.) The leaves show clearly the influence of the pollen-parent.

One of the seedlings has rather narrower petals than his fellows, and, at any rate this year (1906), is later in blooming. Roughly speaking, the 'Corona' is very much the flower which I expected to obtain from this cross.

H. flava × *H. aurantiaca* has produced a very tall flower—about 5 feet in height—something like *flava* in shape, but larger and of a deeper yellow. The plant has not yet had time to develop its characteristics. The foliage is taller than that of 'Corona,' just as the leaves of *aurantiaca* are longer than those of *aurantiaca major*.

Taking *H. Thunbergii* as seed-parent, I have made several crosses.

H. Thunbergii × *H. Middendorffii* is a very floriferous plant, showing plainly the presence of *Middendorffii* blood in its shape, the flower being much rounder than *H. Thunbergii*. It is pretty much half-way between its parents, and blossoms much earlier than *Thunbergii*.

H. Thunbergii × *H. aurantiaca* has given me many seedlings. One ('Chrysolite') has a large, rather pale yellow flower; another ('Halo') has, as its name indicates, a halo round the centre of the inside of the flower. It produces its blooms, which are round and broad-petalled, very freely. Another seedling, while pale in colour, keeps the same interior markings as 'Halo.' Another has a bloom of a rich almost fiery-orange without any markings, but it is not so round in shape as 'Halo.' I may say that I made this particular cross with the object of obtaining a flower with a halo—as in *aurantiaca* there is a suspicion of such a marking as would on a lighter-coloured flower probably be conspicuous. Of course a hybridiser needs imagination.

I have one large plant which gives rich orange flowers borne on very stiff stems. The label has unfortunately been lost, but I believe the plant was produced by crossing a *Thunbergii* × *aurantiaca* seedling with *Middendorffii*. The plant blooms early (before *flava* this summer at any rate) and has very vigorous foliage of a very peculiar green.

As to *H. fulva*, I have many times used pollen of it, but never with success, and it, moreover, refuses to seed. I have looked for seed of it in many places—in Auvergne, in Switzerland, in Northern Italy, near Naples, in Sicily, in the Lipari Islands, in Northern Asia Minor, in Trans-Caucasia; but I have never yet found a single pod, and fairly persistent inquiries have failed to bring to light an instance of its seeding. *H. Thunbergii* for some time refused to seed in my garden, but eventually it seeded freely, so that I have not absolutely given up *fulva*, though my hopes are "attenuated to exiguous proportions."

In a letter, for which I have to thank Herr William Müller (I have made use of Herr Müller's letter in the table of hybrids at the end of this paper), who writes from Vomero, Naples, I learn that Herr C. Sprenger, of Vomero, Naples, has raised some new hybrids from *H. fulva maculata* crossed with *H. citrina*. *H. fulva maculata*, which has a large (much larger than ordinary *fulva*) and effective blossom, refuses so far to produce seed in my garden. The name is, as Herr W. Müller says in the "Gardeners' Chronicle" for September 1, 1906, p. 158, "unfortunate," as there is nothing maculate about the flower. It closely resembles what I should look for in a single form of the plant, which generally appears in catalogues as *disticha plena*.

A List of Hemerocallis Hybrids.

H. flava	× aurantiaca	Yeld
	× aurantiaca major	Yeld
	× Middendorffii	Yeld
	× Sieboldii	Yeld
H. Thunbergii	× aurantiaca	Yeld
	× aurantiaca major	Wallace
	× citrina	Sprenger
	× Middendorffii	Yeld
	× minor	Sprenger
H. minor	× citrina	Sprenger
	× Middendorffii (?)	Yeld
H. minor crocea	× Thunbergii	Sprenger
H. citrina	× aurantiaca major	Sprenger
H fulva maculata	× aurantiaca major	Sprenger

AMERICAN FLORISTS' IDEALS.

By J. H. Troy, of New York, U.S.A.

Apart from the academic interest in plant-breeding, the mere raising of new varieties and types for their own sake or for the sake of scientific study and determination of relationships between different groups of plants, there is an intensely practical side of the question. Upon our side of the Atlantic this aspect of plant-breeding receives a far greater consideration than does the other. We may be even too practical in America. At all events our plant-breeders set out with extremely high ideals. It is not an excuse for the introduction of a new form that it is merely different from other things; from our ultra-utilitarian standpoint we insist that it shall be better. It is for this reason that economic crops have received and are receiving such close attention from our Government Department of Agriculture. The entire force of that organisation which embraces men of high scientific attainments is devoted to the production of plants which will meet and overcome conditions of practical horticulture and agriculture which may indeed be regarded as national problems. The whole energy of this expert staff is bent towards combining the better qualities of the different plants into one new type that shall be vastly superior to anything that has been had before. For instance, we seek for disease-resistant varieties which will put into the hands of the cultivator the means of livelihood that is at present barred.

This Conference is being made familiar with the details of the department's work through another member who represents our national Government; but the problem before so vast a territory as the United States embraces many plants and crops which are outside the scope of staple foodstuffs and agricultural field crops. There is the æsthetic phase of plant-growing in which the work is being carried on slowly, silently, by isolated individuals, as purely business propositions, and without any subsidy from scientific institutions or national funds. The florists of America have not been behind their brethren in the Old World. We have already made great strides in the production of new and distinct ornamental plants. Some of the fruits of these efforts are not unknown to English horticulture. The American carnation, developed by pure process of breeding from the European type of flower, has already recrossed the ocean and is receiving favourable attention at your hands. Its distinctive characteristics are familiar to you in such varieties as 'Enchantress,' which you receive as a type of the American carnation. In roses our florists and gardeners have made distinct advances along lines quite different from those followed out by the Old World raisers, and in your trade catalogues at this time a few of these are found, but their source of origin is unannounced. When the Crimson Rambler and Wichuraiana rose reached our shores some dozen years ago, they were seized upon by many cultivators, were blended, and an entirely new race of what may be called rambler hybrids was originated.

In these days of much closer relations and interchange of commerce between the two nations, it is not without interest to measure up the standards by which the one judges the production of the other. The American horticulturist is distinctly exacting: he looks for a combination of superlative qualities. The mere fact that a plant is new, or a variety distinct from others that are already known, does not give it one extra bit of value. The standards are ultra-utilitarian. We want all our flowers to measure up highly in all their attributes, and a test of commercial value is applied very severely in all cases. As a matter of fact it may be acknowledged that the American horticulturist, the American florist, is engaged in catering to an uncritical public. Now, don't misunderstand me. I don't wish you to infer that I am stating that the great American public does not exercise judicial qualities in its appreciation of plants and flowers. What I do mean to say is, that a flower is measured for itself, for the purpose to which it is to be put, and not merely against its associates for distinctiveness. In other words, we are not breeding for connoisseurs. The keynote of appreciation is selection, not collection. Where the European florist will grow twenty, fifty, or a hundred varieties, his American prototype will find that he can supply all the needs of the public by growing not more than half a dozen. Why? Because there is room for only one red that is best from all points, one pink, and so on. Whereas in agricultural plant-breeding the ideal in view is the raising of varieties that shall not succumb to disease, that shall be hardy above the usual limit of that kind of plant, or that shall be particularly resistant to drought; so in the florists' fold we look for a flower or a plant that shall be extraordinarily productive of flowers, the flowers themselves of perfect form, of pure colour, that will mix with the majority of others without producing colour discords; and in decorative plants we look for pure effects, clear-cut colour schemes, and nothing of the intermediate lower grades. In ornamental plants, for instance, I may take as example the highly decorative *Pandanus Veitchi*, which with its beautiful bands of colour may be regarded as an ideal in its type. The colour scheme of this plant is in harmony with the general contour of the whole and the arching of each individual leaf. On the other hand, the spotty effect of the variegation in a plant like *Dracæna Godseffiana* is not pitched in the same artistic plane, and does not appeal with equal force.

Pure colours are much sought for. Variegated flowers are regarded with less and less favour every year, and with the exception of carnations may be practically ignored as commercial possibilities, and even in that flower their appreciation is on the wane. The European horticulturist who hopes to meet the wants of the American market should place this fact before him above all others, purity and brilliancy of colour; then having attained that he must put it on to a plant that is of itself beautiful. In all the popular flowers the foliage is regarded as of nearly equal value with the flower itself.

To give you a concrete example, I will quote from a communication of one of our most prominent introducers of foreign chrysanthemums. "A novelty should first of all have a good habit, the foliage must be luxuriant and carried right up to the flower, and the stem should be stout enough to carry the flower erect on a stalk three feet in length, the flower itself large, of incurved form and of sufficient solidity to withstand shipping and handling. As individuals the flowers grown for European markets and shipped in bunches (having had but little disbudding) find no place with us. The ideal flower is one that answers the requirements when grown one flower to a stem."

In roses there are two distinct ideals: the one by far the larger interest, that of growing under glass, the flowers being forced for winter, is characterised by a long bud, bright colour, freedom of growth, so that a flower can be cut with a good length of stem, not less than two to three feet, and preferably one that lights up well under artificial light. The five best roses in the New York cut-flower market are the following in the order named: 'American Beauty,' 'The Richmond,' 'Bridesmaid,' 'Madame Abel Chatenay,' and 'The Bride.' The other type of rose which has been developed very greatly in our own country is for outdoor planting. We want a type of roses that will stand our hot sun and cold winters, and flower continuously from June to November in quantity, self-brilliant colours preferred; the best type of a recently introduced rose which I can give you is 'Killarney.' We want rose plants that will be decorative and ornamental, even when out of flower, as trellis plants. The newer ramblers are much valued, and where they are suitable for forcing in pots for flowering at Easter time for indoor decoration they are still more desirable.

Very few American amateurs are connoisseurs of the rose in the way that the European horticulturists are. We cannot conduct a Rose Society along the lines that are so successful in European countries. The commercial standard is introduced, and no matter how beautiful a flower may be of itself, if it does not hold its colour properly, and if it does not fulfil its decorative requirements of rigid stem and healthy, abundant foliage, it cannot find favour with the American.

With these specific instances of ideals in the most popular flowers of the day, I may leave the subject, reiterating that the American market stands wide open to any flower or plant which will meet these high ideals, but that it is no place for the curio-raiser to send his productions to, as there is no demand for a thing purely on the ground of intrinsic novelty.

PRACTICAL PLANT-BREEDING, MORE ESPECIALLY IN RELATION TO THE GLADIOLUS.

By H. H. Groff, of Ontario, Canada.

Through the influence of my late father, I have always been interested in the advanced products of horticulture in all its departments, and for many years my interest has also extended to several types of pure-bred animals and birds, during which time I gained valuable experience in breeding poultry, pigeons, rabbits, dogs, cattle and horses. The knowledge gained during my experiments with animal life has been of incalculable value in enabling me to determine the best system and practice likely to assure the most satisfactory results, during my past fifteen years of practical experience in plant-breeding.

There are two classes of plant-breeders, both of which are doing good work of more or less value from the scientific and economic view-point, in the interest of advanced knowledge and our advancing civilisation.

The first of these is the breeder who works for the purpose of proving his theories, and who, by a limited number of recorded crosses, is able to place the simple analysis of his investigations in presentable form for educative purposes.

The second, or the breeder for practical results, cannot do this without placing limitations upon his activity, which means his experience and success, as it is only the man who makes many crosses who can hope to approach even the border of a field of limitless possibilities in results.

Such a worker will secure innumerable examples and illustrations of the points valued by the theoretical breeder, but such, being relatively barren of practical results, will soon be forgotten ; and it matters little that this is so, for the only value that such records would have is that, on a repetition of the cross, a duplicate result could be secured, and a type thereby multiplied. This, however, is practically impossible in cross-breeding.

As has been stated by me before the American Breeders' Association, the only admissible system to be practised for the purpose of producing the highest average in types of economic value, is that of breeding from domestic specific types as sires on selected females, according to the practice of animal-breeders. The use of wild species with the hope of attaining a similar ratio of such results is relatively absurd, as the only value that any wild species can have to a breeder for practical results is as foundation or laboratory stock, to be discarded yearly with their early hybrids as he advances step by step towards his ideal.

Now here it may be well to state that, if the breeder uses his full opportunity, this ideal will be a progressive quality, and his standard will advance yearly as he sees the results attained by unlocking the treasuries of ages of the past in scientific, though unrecorded, practical plant-breeding ; and, by the same means, he hastens evolution and draws the natural harvest of the eons of the future to meet the scientific harvest from the eons of the past, within the area of his trial grounds as well as within the grasp of his mental and physical activity.

I have spoken of limitations, and the man who will most feel the restricting force of these limitations is the one who specialises his specialty, and by the production of innumerable examples of the possible practical results he desires to attain, opens up new and improved avenues for further advancement, until he becomes mentally stalled between these results and the horizon of the visible field, a horizon which will broaden as he advances, to an eternity of possibility beyond the conception of the human mind.

I have frequently stated that the plant-breeder, with the full complement of the chemical constituents of his laboratory stock in a condition of control, can do anything he may wish to do in producing types of his specialty at will. Not only is this so, but he will be surprised to find in the yearly course of his practice that he will develop more advanced types, and thus will have forced upon his recognition the interesting fact that he can do all he had hoped to accomplish and far more; and further, that the advancing years of his activity extend the visible field of possible satisfaction; and still further, that these possibilities broaden infinitely with each succeeding year of production and added knowledge.

The foregoing evidence and argument could be multiplied and prolonged indefinitely were it needful, but I will pass on to the subject of this address—the results to accrue from the practical aspect of plant-breeding—for this is the great and valuable end of all our effort, mental or physical, theoretical or practical. Barren of results, the arm-chair scientist may cease to theorise, and the field worker abandon his labours and investigations while grilling under the summer sun, for results are the standard by which man's work is judged.

By practical plant-breeding I mean the application of that knowledge of the science which will enable the worker to secure the highest ratio of economic value in advanced results of an æsthetic or utilitarian character.

This is one of the most important features of plant-breeding, as the value of the knowledge of what may be done by crossing is small when compared with that of the practice which will give manifold results of value to mankind, and the highest average of quality for the time and area occupied by the operator.

In my work on the *Canna*, which embraced all available species and early European hybrids, as well as the latest and best productions obtainable, I proved yearly the correctness and value of my contention, for seven years of select breeding gradually eliminated types of no commercial value, until in the last season not only were discards practically *nil*, but the value and quality of the selected seedlings were equal to those of the best novelties of European introduction.

The great value of the system advocated by me is the fact that the success of breeding depends much upon the removal of every influence adverse to increased multiplication of advanced types. This will be appreciated by those workers on bulky plants and trees of slow maturing habit, requiring a large acreage for development, and the fact that I am speaking from an experience with nearly a million new hybrid gladioli, a plant that requires comparatively little space, although needing from three to five years to mature from seed. Fifteen years of unbroken work on this, now my sole specialty, has also proved the value of my views in practice. In the progression of my system the first five years only is known to commerce, having been discarded by me ten years ago; the second series of five years is a little known commercially, and received the Pan-American Exposition Gold Medal and St. Louis World's Fair Grand Prize; while the third series of five years is all in my personal possession, and unknown outside my trial grounds.

I mention the above to make my statement more clear, for the reason that while my Canadian and United States representatives use over one hundred acres in multiplying and maturing my introduced productions, the five acres of my own breeding and trial ground are ample for my personal supervision, in view of the yearly increased average of high quality developed under the system of breeding practised by me. This means that, in the daily work of selecting from thousands of seedlings blooming in series of yearly production, the object lesson is most apparent in passing from section to section, with their gradual but markedly increased ratio of high quality, and newer and more valuable types.

It is therefore imperative that the breeder should specialise, that he should use every obtainable wild species of his specialty, and in using each for the purpose dictated by his judgment and experience thus control and render amenable to his direction the vital forces and chemical constituents of this foundation stock. By using all obtainable species he multiplies the possibilities for practical results and increased diversity in the material to be evolved from the product of future years, and yearly discarding species and early hybrids as they are superseded in the course of his operations.

Wild species are only of value so far as they may supply some desirable quality for incorporation into a domestic type containing other good qualities, such as size, vigour, vitality, and adaptability. Illustrating from my specialty, the blotch of the small *Gladiolus purpureo-auratus* can be imparted to a six-foot domestic type, free from the objectionable cowled habit of this species; and the throat mottling of the weak-growing *G. Saundersii* can be transmitted to a race of strength and vigour, with the added influence of its wide, open flowers; and so on indefinitely.

That the foregoing can be done is good reason for not developing race hybrids, with the consequent loss of the most important quality of general adaptability to changed conditions. The natural development of wild species is usually accomplished by restricted conditions of habitat, an influence of ages impossible of neutralisation by a few seasons' crossing. So highly do I appreciate this feature of adaptability that in bringing my productions to maturity I grow them on four kinds of soil—sandy, sandy loam, clay loam, and humus or vegetable deposit—and before use in breeding they are proved in this quality in order that it may be also transmitted in crossing. Breeding from wild species is therefore of little practical value, as the farther our removal from their many objectionable features the better, when by proper selection their best qualities can be controlled and applied according to our knowledge and discretion.

As I have spoken lightly of the value of pedigree types from wild species, it is only fair that I should give good reasons for my objection.

Plant-breeding is in its infancy, and too little has been done of a practical character in quantity to secure the general results to be attained by specialisation and selection from hundreds of thousands of composite hybrids.

How many animal-breeders would be satisfied with sires whose progeny were largely weeds? How were these high-class animal sires produced? How are new domestic races and strains of cattle, sheep, dogs, poultry, pigeons, and other animals and birds obtained? Certainly not by the general practice of plant-breeders.

Of what practical value is the knowledge of the component ratios of life forces in simple hybrids, in comparison with that knowledge giving results in the highest ratios of useful and valuable qualities?—thereby saving labour, time, space, and expense, and giving, in the place of curios, the highest possible percentage of quality in economic types.

My advice to plant-breeders is to multiply types by many thousands, using specially proved selections as sires, on the lines practised by successful animal-breeders. Select and develop domestic races and sections of such high quality, vitality, and general adaptability, that their progeny will not only be of higher quality than the parents, but that this quality will be produced in quantity in the highest possible ratio. This is practical plant-breeding.

It is not necessary at a conference like this to detail many of the more simple effects of the influence of the vital forces directed by the operator in hybridisation, such as control of colour, form, and special markings in the flower, size, habit, vitality, and reproductive powers in the plant, or the increase or diminution of the component chemical constituents affecting the commercial value of our productions.

The operation of crossing, to be practical, must be understood from the important aspect of its blending of diverse chemical constituents, and the critical breeder will be interested in observing the daily decomposition set up by his experimental blending, and the chemical action referred to, as this frequently causes partial or complete disintegration of the forms resulting from such crosses.

I have said "daily" for the reason that this influence is apparent from the germination of the seed, and its daily development during every season preceding the maturity and fixity of a type, until its dominancy and stability are assured. This lesson reads from one day to five years in my specialty. I do not mean that each single variety needs this daily scrutiny, but that in the daily development of many thousands of seedlings some live one day, others two, and so on daily, until the close of the season, the seed-bed is an object lesson on the lines to which I have referred, and that stability is not assured even at full maturity.

For practical and valuable economic results it is therefore not sufficient that the breeder should be able to produce types of symmetry and beauty, but he must add the qualities of stability and adaptability to changed conditions to ensure due satisfaction for the ultimate grower.

In closing I will record one of many unique results in my experience in exhaustive work on this one plant, for the purpose of illustrating the subtle yet distinct character of the vital forces directed by the plant-breeder. From among some types showing a tendency to produce double and semi-double flowers I had selected one of proved value as a useful breeder, after some years of experimental trial. The influence of this type not only carried the tendency of petal multiplication, but the seeds produced twin corms from time to time, an effect not manifested in the offspring of normal types. Experiences like this prove the contention advanced in the early part of this paper, that the hybridist does not need to spend time in special analysis for the purpose of securing examples of an interesting character, but that the great secret of success and satisfaction is large production from high-class composite parents bred for the purpose, and that by these means many new, valuable, and interesting types can yearly be produced, in addition to the highest ratio of useful and beautiful varieties, developed by practical plant-breeding.

CARNATION-BREEDING IN AMERICA.

By C. Willis Ward, Queens, N.Y., U.S.A.

Though I have devoted a considerable time to the breeding of carnations and to the study of them during the past fourteen years, in view of the complexity of the subject and my lack of scientific knowledge, I feel diffident in placing my views before a conference composed of the most advanced scientific talent of the world. I shall not attempt to enter into a scientific discussion of the subject, but will endeavour to state in plain language what seems to me to have been thus far accomplished.

As most people know, the original carnation, which has been known in history for several centuries before the Christian era, was a five-petaled single bloom about one inch in diameter and of a pinkish-mauve colour. It was distributed in its wild state over the whole southern half of the temperate zone in Europe, but was known more particularly to historians as inhabiting France and Northern Italy. It was found in abundance in Normandy, from whence it is generally believed to have been introduced into Great Britain about the time of the Norman Conquest. Even so recently as 1874 it was found in a wild state covering the Castle of Fallaise, in which William the Conqueror was born. It was described by Theophrastus as early as 300 B.C., and has been frequently mentioned in history since that date.

The carnation of to-day, the subject upon which I am working, is the product of several centuries of hybridisation and culture. It is an open pollinated species, and mother plants can be chosen from amongst hybrids, as well as by inbreeding upon the same plant or upon plants of the same variety. A variety once produced from seed is easily perpetuated for a certain period by propagation from cuttings which are easily rooted and usually secured in abundance. Improvements in varieties may be made by bud-selection, and new varieties are sometimes secured by bud-variation (sports), as well as from seed-variation and by hybridisation.

In raising varieties from hybridised seed very few improvements are produced, the proportion being one good variety for every thousand hybrids grown, and probably as little as one decided advance in each five or ten thousand hybrids grown; and unless some law is discovered whereby we can forecast more surely than we can now what certain specified hybrid seed will produce, it would seem as if even this low proportion of valuable new varieties would decrease rather than increase in view of the higher standard which is being demanded from year to year.

Up to the present time I have been pursuing my studies in breeding upon the single subject of colour alone, basing my work upon the theory that any laws developed in colour would hold good when applied to the development of other desired qualities; and this hypothesis I still believe to be correct.

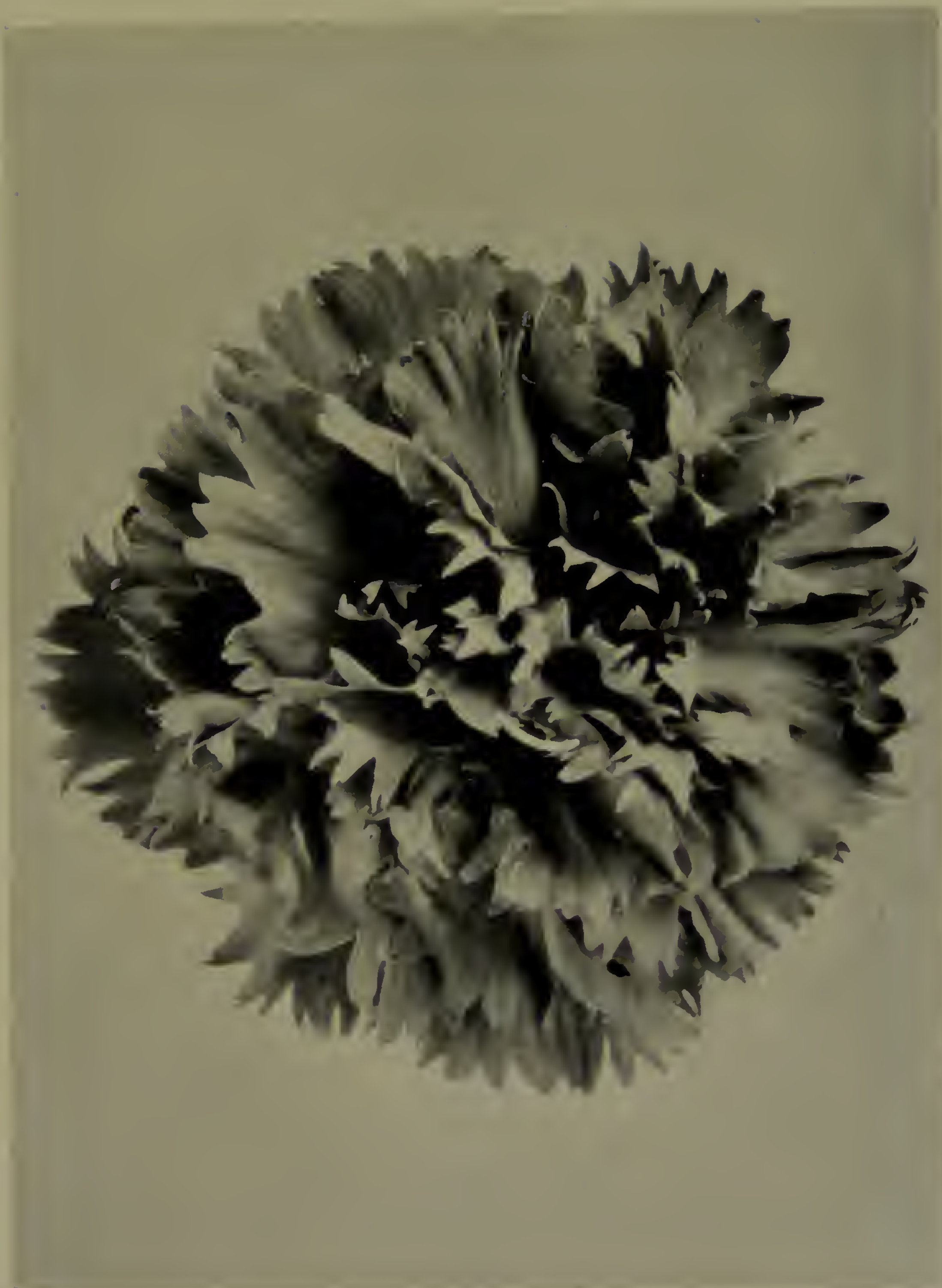

FIG. 111.—'BON TOM' (*Blake*).
Fringed scarlet seedling; petals deeply serrated.

FIG. 112.—'MRS. THEODORE ROOSEVELT' (*C. W. Ward*).

Carmine or cerise-pink; pedigree seedling, sixth generation from 'William Scott' × 'Daybreak'; illustrating both fine calyx and fine form, with petals just sufficiently serrated.

Fig. 113.—'President Roosevelt' (*C. W. Ward*).
Brilliant scarlet, shaded with maroon (called crimson by American growers); illustrating an ideal calyx which never bursts under proper cultivation.

Fig. 114.—Malmaison Carnation 'Jumbo.'
Such forms now and then appear among American seedlings, always proving of little, if of any, commercial value.

Fig. 115.—A Two-storied Seedling.

Many odd forms appear among American hybrids, this being one of the most striking, consisting of two complete flowers, the upper bloom growing out of the centre of the lower one. Such forms almost invariably bear neither seed nor pollen.

Fig. 116.—'Alpine Glow' (*Ward*).
Colour flesh pink; seedling illustrating long strong-stemmed blooms.

Fig. 117.—'Mrs. James Dean' (*Ward*).
Delicate rose-pink; illustrating large blooms, fine calyx, but stems not strong enough.

The tools used are very simple, consisting only of a pair of delicate tweezers and a microscopic magnifying glass. I used to employ a fine camel's-hair brush for transferring the pollen, but eventually abandoned it, as I found the use of the tweezers less difficult, and speedier work could be done than with the brushes.

In hybridising the carnation the first step is to remove all the anthers from the mother flower before they develop, and to watch the pistil until it is in a proper receptive condition. The anthers of the selected male parent are watched until just bursting, and the pollen is in the condition of a dry powder. The anther is then removed by means of the tweezers, and the pistil of the mother flower is fertilised by lightly touching it along its entire length with the pollen-bearing anther. Conception generally takes place within twenty-four hours after this operation provided the conditions are favourable.

In order to pursue my colour studies with reasonable method I have divided the work into the following sections :

1. Fancy section, comprising all varieties peculiarly marked, of various colours.

2. Purple and blue section, comprising all purple flowers and any which may show a tendency towards blue in colour.

3. Crimson and maroon section, comprising all shades of crimson, maroon or scarlet-maroon.

4. Scarlet section, comprising all shades of red and scarlet.

5. Light pink section, including all shades of salmon, flesh or daybreak tints.

6. Dark pink section, including all rose, cherry and deep pink tones.

7. White section.

8. Yellow-variegated section, including all varieties having a yellow ground with scarlet, crimson or pink markings. I have not as yet been able to establish a pure yellow section, for even though the seedling plant may give pure yellow flowers for a time, as soon as propagation is carried on to any extent, more or less of variegation appears.

9. White-variegated section, comprising all varieties with white grounds marked with various colours.

10. Pink-variegated section, comprising all varieties having a pink ground of any shade with markings in deeper colours.

In the purple and blue section I have not as yet succeeded in producing anything that might be termed a blue ; for, although we have had several varieties showing a distinct blue shade, there has invariably been enough pink in it to convert the tone into a purple or a mauve. The crimson, dark and light pink, scarlet, and white sections have been fairly well fixed ; but the yellow, white and pink variegated, and blue sections will be very difficult to fix, as they are essentially mosaics, and there is a constant tendency towards variation in all their progeny.

In order to render the study of pedigrees easier, I have devised a system of "bench-cards" * which are used as labels tied to each variety under experiment, and which at the end of the season are filed away in a card index, thus preserving all of the original records made in my work. I find this system exceptionally useful, as I am able to determine at a

* For specimens of these see p. 430.

glance the pedigree of any variety when working upon it. My object has been to study-out some system whereby the colour tones might be purified, and rendered more brilliant or delicate and more pleasing to the eye; and while I have paid some considerable attention to size of flower, freedom of bloom, integrity of calyx, length and strength of stem, and fragrance, the purity of the colour has been my first consideration. In order to develop more brilliant shades, I have, as a rule, confined my crosses to plants both bearing flowers of the colour which I desire to improve—that is to say, crimsons were crossed with crimsons, light pinks with light pinks, dark pinks with dark pinks, whites with whites, and so on; and the result seems to indicate the following hypothesis, viz. that the percentage of colour contained in the parentage for several generations back controls to a very large extent the colour of the progeny. I do not, of course, mean that by using parents on both sides that have practically a scarlet pedigree for several generations back *all* scarlets will be invariably produced, but that where such parents are used, the chances of securing the improved scarlet colour desired are greatly increased. True, the progeny of such parentage frequently show many shades varying from the red or scarlet, such as pinks, maroons, purples, and even white and yellow grounds with more or less variegation of all of the colours mentioned. Again, a cross between a white variety, having practically a white pedigree for several generations back, with a scarlet variety, having a mixture of scarlet and white pedigree for several generations, frequently produces scarlet flowers; but, as a rule, such scarlets are not as brilliant or persistent as those produced from plants having mainly scarlet pedigrees. One of the best ways of proving this hypothesis is to take two scarlet parents the pedigrees of which are mainly white. By crossing these, very few scarlets will be produced—sometimes none at all—but a large percentage of the progeny will frequently be pure white, or white grounds marked with scarlet or various shades of pink. I have sometimes thought that by taking a scarlet variety of comparatively pure scarlet pedigree and crossing it with a white variety, the pedigree of which is largely scarlet, more clear and persistent scarlet tones are produced than where the pure scarlet pedigrees are used. For I have often found that with pure scarlet pedigrees there is a tendency for the colour, even though it be very brilliant, to blacken, or turn a slaty shade when exposed to bright sunshine. While I have not been able to prove to my satisfaction that this hypothesis is right, there still seems to be evidence enough to warrant the assertion that if you desire free-blooming early habits, use parents having early free-blooming pedigrees. The same should hold good if you desire to produce varieties with extra large blooms, or perfect calyx, or any other peculiarity in habit or form of flower; it also seems to hold good in regard to fragrance. Again, the early-blooming varieties usually have small to medium-sized blooms, and the very large flowering varieties are generally late and inclined to be shy bloomers. This can to some extent be overcome by crossing large-flowering late-blooming varieties with the early free-blooming ones which have the largest blossoms. This, if followed up, will in the end produce large-flowered early-blooming varieties. Working on these lines, I have already succeeded in producing 'Alma Ward,' which under our intensive culture gives flowers ranging from $2\frac{1}{2}$ to a little over 4 inches in diameter on stems from 2 to 3 feet in length, according to the season; and these flowers are produced in sufficient abundance to make the variety very profitable commercially. It is also a fairly early bloomer, coming into flower by October 1. This variety was obtained by crossing 'Prosperity,' a large-flowered, not very free variety, with a white seedling having a very large flower and long stems, and quite a free bloomer.

Mr. Peter Fisher, the raiser of 'The Lawson' and 'Enchantress,' two of the most satisfactory of our American commercial varieties, secured his strain from crosses between 'Daybreak' and a sport of 'Tidal Wave,' two quite large-flowered very free-blooming as well as early varieties. In 'The Lawson' a curious blending of the qualities of both parents may be observed. 'Daybreak' is a long-stemmed variety, 'Tidal Wave' usually has quite short stems, or at least distinctly below the average in length. 'The Lawson' has quite short stems in the early part of the season, but as the season advances they become as long as those of the parent 'Daybreak.' Double flowers may be produced by crossing single blooms with pollen from very double ones, and the more double the pollen parent is, the greater seems to be the proportion of double-flowering seedlings produced.

Among seedlings from hybrids we sometimes meet with what might be called "seed-sports," such as a light pink or flesh colour, usually marked with crimson edges or stripes like those which are now and then produced from crossing varieties with comparatively pure crimson pedigrees for several generations.

Commercial Importance of the American Carnation.

The commercial value of the carnations grown in America has risen to such a volume that carnation-growing may be considered one of the substantial professions, the annual value of the American product being upwards of one million pounds sterling, including the sales of both flowers and plants; and the capital invested may possibly amount to upwards of five millions of pounds sterling if the value of real estate and buildings devoted to the business be considered.

The recent great improvement in the American winter-blooming carnation has resulted in placing upon regular sale blooms of such remarkable size, and of length and strength of stem, as to create an immense demand and a marked advance in the price of the blooms, which seems in a measure to keep pace with the increasing demand. Twelve or fifteen years ago fifty cents to one and two dollars a dozen was considered an average or even high retail price, but nowadays one to three dollars a dozen can easily be obtained, and frequently five to six dollars, sometimes eight, and in rare instances even ten dollars a dozen is secured, for some extra fine or rare variety.

Apart from its commercial importance, there is a sentimental side to carnation-growing. The production of new hybrids which are distinct improvements upon existing varieties is always a pleasure, and serves to keep one thoroughly interested in the divine flower, and amply repays the close application necessary to produce practical improvements.

Number $\frac{303}{01}$ Named	Pedigree Lawson × 6/00
Robt. Craig	Colour Scarlet
If this Pedigree had been $\frac{3}{5}$ scarlet, $\frac{1}{5}$ yellow, $\frac{1}{10}$ crimson, $\frac{1}{10}$ pink, a much more brilliant and durable scarlet would likely have been the result.	Pedigree that produces a very brilliant scarlet.
	Colour Pedigree
	$\frac{1}{5}$ yellow $\frac{2}{5}$ pink $\frac{1}{5}$ scarlet $\frac{1}{5}$ crimson

Number $\frac{208}{05}$	Pedigree $\frac{228}{02}$ crimson × $\frac{222}{02}$ crimson
× No. $\frac{207}{05}$	Colour Crimson
Pedigree mainly crimson. The white blood tends to lighten up the crimson. A strain of yellow blood would make the crimson pigments more brilliant.	$\frac{209}{00}$ × Prosperity $\frac{209}{00}$ × Prosperity $\frac{92}{98}$ × 8/97 cr.
	Colour Pedigree
	$\frac{5}{8}$ crimson $\frac{3}{8}$ white

Number $\frac{211}{05}$	Pedigree $\frac{310}{99}$ crimson × $\frac{222}{02}$ crimson
× No. $\frac{205}{05}$	Colour ——
	$\frac{209}{00}$ × Prosperity $\frac{92}{98}$ × 8/97
Pedigree largely crimson and white. Produces some very pure brilliant crimson seedlings.	Colour Pedigree
	$\frac{2}{3}$ crimson $\frac{1}{6}$ white $\frac{1}{6}$ " var.

$\frac{06}{05}$	Pedigree $\frac{303}{01}$ × $\frac{506}{03}$
× No. $\frac{308}{05}$	Light Scarlet
Crosses to be made 1906	Lawson × 6/00 $\frac{508}{00}$ × $\frac{569}{02}$ 3/98 × $\frac{103}{98}$ Crimson × $\frac{35}{97}$ P. 7/00 × Prosperity 3/98 × $\frac{31}{98}$
$\frac{303}{01}$ 5 pods ✓✓✓✓✓ $\frac{303}{04}$ 5 " ✓✓✓✓✓ Beacon 5 " ✓✓✓✓✓ $\frac{505}{04}$ 5 " ✓✓✓✓✓ $\frac{503}{03}$ 3 " ✓✓✓ $\frac{504}{03}$ 3 " ✓✓✓ $\frac{2621}{02}$ 5 " ✓✓✓✓✓	Colour Pedigree
Copy of card used during winter of 1905	5 d. pink 2 yellow 2 white var. 2 crimson 1 purple 1 prosperity 1 scarlet
Check marks [✓] show number of pods fertilised. Pedigree that often produces a light scarlet that fades in the sun.	

Number	Pedigree
$\frac{517}{05}$	Nelson Fisher × 550/02
× No. $\frac{510}{05}$	Colour Dark Pink
Pedigree mainly dark pink. The purple tends to produce magenta shades. A strain of yellow would tend to produce salmon tints and pure pinks.	$\frac{100}{00}$ × Prosperity $\frac{810}{99}$ × Ethel Crocker
	Colour Pedigree $\frac{2}{3}$ dk. pink $\frac{1}{6}$ purple $\frac{1}{6}$ var. white

Number	Pedigree
$\frac{609}{05}$	$\frac{2653}{02}$ × 622/00
× No. $\frac{604}{05}$	Colour White
This same pod of seed produced:— $\frac{608}{05}$ white $\frac{610}{05}$ white $\frac{606}{05}$ white	$\frac{729}{00}$ × Prosperity 3/98 × $\frac{88}{97}$ D. Mary Wood 72/98 Blush W. Cloud
Pedigree mostly white and yellow; produces mostly white and cream-white seedlings.	Colour Pedigree $\frac{6}{10}$ white $\frac{3}{10}$ yellow var. $\frac{1}{10}$ white var.

Number	Pedigree
613/05	$\frac{2621}{02}$ × 615/13
× No. 602/05	White
This same seed pod produced:— $\frac{615}{05}$ white $\frac{618}{05}$ white	$\frac{613}{00}$ × Prosperity 3/98 × $\frac{39}{96}$ $\frac{72}{98}$ D. × $\frac{616}{00}$ $\frac{98}{98}$ $\frac{62}{98}$ $\frac{8}{97}$
Pedigree largely white; produces many white seedlings.	Colour Pedigree $\frac{6}{10}$ white $\frac{2}{10}$ crimson $\frac{1}{10}$ var. white $\frac{1}{10}$ var. yellow

THE CARNATION.

By James Douglas, V.M.H.

Like the auricula this garden favourite is exceedingly variable in its character, and not only so, but is constantly reverting to its original source, or rather to the single form which may still be found on old castles in Normandy (and probably yet lingers in England), where it has been found growing with its roots searching for a scanty subsistence amongst decaying mortar and humus in small quantities. The gardener takes the progeny of this wilding, which has been improved by ages of cross-breeding and selection, plants it out in the garden or in flower-pots in a mixture of the best loam enriched with manure of various kinds, some of it composed of blood and bones; and even this is not enough, the plants are stimulated with manure in the water applied to them, with the result that very large flowers are obtained. But the result of over-feeding in carnations is the same as over-feeding in men and animals, disease attacks the plant sooner or later, and in some cases it is difficult to eradicate it.

How the wild carnation (*Dianthus Caryophyllus*) has been trained to its high state of perfection it is not easy to say. It is not necessary that hybridisation should have taken place, indeed it is unlikely. The plant may have been introduced to gardens from its native habitat. Cultivation would soon produce seedlings of varied forms and colours, some of them would have an extra petal or two, and saving seed from these, by a process of careful selection, semi-double and double flowers would be produced of various colours. This process, as is well known, takes much longer in the case of a plant like the carnation than in that of such a fugacious flower as the poppy. The Rev. W. Wilks found a variation of the common poppy in his garden and with the instinct of a true gardener he saved seed from it, and next season the variation was greater, and he set himself the task of changing this noxious weed into a beautiful garden flower, and the Fellows of this Society know how great has been his success. The Shirley Poppies still produce flowers of many beautiful colours, but single; this must be because double flowers are not wanted, as the Shirley Poppies insist on taking to the double form freely enough when double flowers are wanted. This is an excellent illustration of what can be accomplished in a few years by one earnest and careful cultivator. We might desire to know the name of the gardener who first obtained a break in the wild form of the carnation, but that name is lost in remote ages. The Romans may have brought the carnation into England, as it has been suggested that the carnation is not a native of Britain, and that it has either been sown or planted where it has been found. How botanists arrived at this conclusion I am unable to say; but even if the seed of a double variety was sown, the offspring would soon become single-flowered only, as we find a percentage of 10 per cent. of single-flowered varieties are produced from seeds saved from the very best named double varieties. If left to nature, the single-flowered varieties would produce seed most freely, and the double forms would drop out of existence. We do not read much about the carnation until the reign of Queen Elizabeth. William Turner published a Herbal about 1550, which was dedicated to Queen Elizabeth. In Turner's time the flowers were much varied in colour, and had taken on the double form. The author adds that they were "made pleasant and sweet with the labours and wit of men and not by nature." This is conclusive evidence that gardeners were cultivating and improving the carnation about the middle of the sixteenth century. How early we can only conjecture. Shakespere alludes to the carnation in *The Winter's Tale*, and it was evidently surmised that the flowers had been produced by hybridisation, and on this account Perdita refused to grow them. The dialogue in *The Winter's Tale* between Polixenes and Perdita, when she was distributing the flowers at the sheep-shearing, "is a most beautiful and striking touch of individual character," and shows the great knowledge Shakespere had of gardening. Perdita says :

Sir, the year growing ancient,
Not yet on summer's death, nor on the birth
Of trembling winter, the fairest flowers o' the season
Are our carnations, and streaked gillyflowers,
Which some call nature's bastards : of that kind
Our rustic garden's barren ; and I care not
To get slips of them.

POLIXENES : Wherefore, gentle maiden,
Do you neglect them ?

PERDITA : For I have heard it said,
There is an art, which, in their piedness, shares
With great creating nature.

POLIXENES : Say, there be ;
Yet nature is made better by no mean,
But nature makes that mean : so, o'er that art,
Which, you say, adds to nature, is an art
That nature makes. You see, sweet maid, we marry
A gentler scion to the wildest stock ;
And make conceive a bark of baser kind
By bud of nobler race : This is an art
Which does mend nature.—change it rather ; but
The art itself is nature.

PERDITA : So it is.

POLIXENES : Then make your garden rich in gillyflowers,
And do not call them bastards.

PERDITA : I'll not put
The dibble in earth to set one slip of them :
No more than, were I painted, I would wish
This youth should say, 'twere well.

From this, at all events, we know that the carnation was carefully cultivated in this country in the seventeenth, eighteenth, and nineteenth centuries, but certainly not with such care and scientific accuracy as it is being cultivated at the beginning of the twentieth. For garden purposes the carnation is divided into two classes, the carnation and picotee. The picotee is merely a colour variety of the carnation, but is distinguished from it in having a ground colour of white or yellow with a narrow margin of colour round each petal. The prevailing colours are rose, red, or purple, of various shades, and a perfect picotee should have no spots or colour on the petals, except the fine line, or broader margin of colour. The white ground varieties have been brought to a higher state of perfection than the yellow grounds.

The border carnation is again subdivided into self colours and what are termed fancies ; the last named have yellow, white, or buff grounds, marked and striped, of various colours—purple, rose, lilac, crimson, scarlet, &c. There are also the flakes and bizarres, the well-known types grown principally for exhibition purposes, and exhibited on white cards to show off better the flakes and stripes. The florists take advantage of this to dress out the petals with tweezers and pull out those that do not show the correct colours.

The tree or perpetual flowering varieties are so named because they produce growths from the main stem, which in their turn produce flowers in summer and winter.

The Malmaisons are a distinct class, and like the perpetual flowering varieties do best in a glass structure: they are known by their immensely large flowers, and many remarkably beautiful varieties have been produced by cross-fertilisation during the past fifteen years. We are indebted to Mr. Martin Smith, V.M.H., for producing many beautiful varieties, taking the pollen obtained from the blush and pink Malmaisons, and using it on the flowers of the best border varieties.

Cross-fertilisation is necessary to obtain good results. You obtain a carnation of a vigorous habit, of medium height; the flowers holding their heads erect and a flower with a non-bursting calyx, the stigmatic part of the flower rises from the centre in the form of two horn-like processes. The pollen is found in the form of a yellow powder attached to the anthers; it must be sought for amongst the petals, and be conveyed with a fine camel's-hair brush to the stigma of the seed-bearing parent.

At present not much has been done in the way of hybridising the carnation, but it may be hybridised with any species of *Dianthus*. Mr. Percy Williams produced a useful garden plant by applying the pollen of the common Sweet William (*Dianthus barbatus*) to the stigma of a crimson coloured carnation, and produced a very double flower like a carnation with the foliage of a Sweet William. It is grown in gardens as *Dianthus* 'Lady Dixon.'

THE AURICULA.

By James Douglas, V.M.H.

The auricula is an old-fashioned favourite and highly interesting as a garden plant; and if we can trace its origin by any process of selection or hybridisation an interesting problem would be solved. If this Conference had been held in April I could have exhibited a series of auricula plants in bloom which would have demonstrated to some extent the variability of the cultivated auricula, and its not infrequent reversion to what may be supposed was its original source. Most cultivators of the auricula are not at all disturbed about the source from which their favourite was derived in the first place. They have a standard of excellence in their mind, and those who attempt to produce new varieties by cross-breeding, destroy or neglect every variety not up to this standard. In the auricula proper of gardens there are four classes, and every new production is made to fit into one or other of these classes. There is first the green-edged, with the centre tube of the corolla of a yellow tint; next the paste, caused by a thick coating of white farina: this centre is enclosed in a ring of very dark, almost black, maroon, and the outer margin of the corolla is green; the grey edge is like the green in every respect except that the part which is green is lightly dotted with white spots of farina. The white-edged section differs from the grey in having a thicker coating of meal over the green ground. The fourth class is entirely different. The yellow tube and centre disc of dense white farina is enclosed in a margin of one unshaded colour: it may be dark maroon, red, violet, or yellow; the yellow colour was not favoured by the florists in the early years of the fancy, but very good, well-formed yellow varieties have been produced in recent years. From a scientific point of view it has been unfortunate that every plant produced outside the above well-defined limits has been neglected; but in recent years many curious forms have been selected from amongst the thousands of seedlings produced annually, some of them greatly resembling the descriptions given of the varieties in cultivation at the time Parkinson wrote the "Paradisus, or Garden of Pleasant Flowers." The next type of auricula is termed, for gardening purposes, the Alpine auricula. The name is well understood, although it is well known that all the auriculas are Alpine. The florists have set up a standard of excellence for the Alpine auricula as they have done for the edged type.

The Alpine auricula must not have any meal (farina) on flower or leaf, and the corolla, instead of having a white centre powdered, has a smooth centre of yellow or cream colour entirely free from meal, with a margin of maroon or reddish-maroon, apricot, or purplish-maroon, darker near the central disc, and shading to a paler colour at the margin. In all the sections a strong point is the position of the stigma. If it protrudes from the mouth the flower is termed "pin-eyed"; and if the anthers are placed down in the tube, with the stigma protruding, no florist worthy of the name would allow such a variety any space in his garden. The anthers ought to fill the tube, with the stigma well down and invisible. The origin of the garden auriculas was fully discussed twenty years ago at a conference on the genus *Primula* held under the auspices of the Royal Horticultural Society, and as showing how difficult the subject is, many theories as to the original source of the auriculas were advanced, but no facts. The National Auricula Society's exhibition was held at the same time, April 21, 1886, but no hybrids between the Alpine and garden or edged auricula were exhibited. Since that time I have raised many crosses between the two: they form very good garden plants for border culture; the centre is a pale yellow lightly mealed, and the meal extends less or more to the margin.

The auricula has been cultivated in gardens for upwards of 300 years, and we can trace its gradual improvement to the edged form through many years of patient cultivation. Leading botanists took part in the 1886 conference, and it was generally agreed that the origin of the edged auricula is *Primula Auricula*, and the origin of the Alpine auricula, *P. pubescens*; but it has been pretty well proved by evidence obtained from specimens found in the Tyrol that *P. pubescens* is a hybrid between *P. Auricula* and *P. hirsuta*. The Alpine auricula has not been so long, nor can it be said that it has been so carefully, cultivated as the edged type. The late Mr. Charles Turner of Slough began the improvement of the Alpine auricula about fifty years ago. He had very poor material to work upon, but by careful crossing he succeeded in obtaining the type of auricula now in cultivation, which, of course, has been greatly improved in recent years. The National Auricula and Primula Society's exhibitions have done much to stimulate cultivators to improve and grow to a high state of perfection these beautiful garden plants. It is pretty well known that the Alpine primulas are difficult subjects to cultivate and establish in our gardens, and the same difficulty was experienced 300 years ago. An old author, Clusius, studied the Alpine flora of the Austrian and Styrian Alps, and attempted to cultivate the wild primulas of those districts; but two only responded to his efforts to establish them in his garden. These two were the *P. Auricula* and *P. pubescens*. I have grown the wild forms of *P. Auricula* and varieties of it, such as *P. Balbisii*. These cross freely with the garden auricula; but no amateur would care to hark back to the original species, either as a pollen- or seed-bearer. It would be a most interesting subject for an amateur with plenty of spare time to investigate this subject. Some of the botanists who took part in the conference suggested that some species with purple flowers such as *P. venusta* might have been the parent of the purple, maroon and red-coloured auriculas, some are dark almost black selfs, but the edged flowers all have this rich dark ground colour, and it was not thought likely that these could be produced from yellow flowers; but we all know how the common primrose has broken into quite as many colours as the self auricula, and the cowslips sport into colour quite as freely; but we have evidence that the dark-coloured auriculas have been obtained from the yellow *P. Auricula*, for I have frequently raised seedlings from dark red self-colours of various shades of yellow, some a paler yellow, and others of a deeper tint; and, what is even more unusual, a variety has been produced with dark red flowers and one yellow blossom on the same truss. I have also produced many seedlings from the edged flowers without the dark ground colour. They have a yellow ground in place of the very dark red or maroon, with a green, grey, or white margin, and these greatly resemble the auriculas described in the herbals of the seventeenth century. There were many careful cultivators of garden flowers in the seventeenth and eighteenth centuries, and hybridising was understood and practised. Much good work might have been done and no record made of it. In these enlightened days the horticultural weekly papers are glad to receive and record everything worthy of note, but there were none in those days, and no Royal Horticultural Society to give certificates: the work was handed down from one generation to another; how it was done we are left to conjecture. Before the conference of 1886 was held I looked up every species and variety of *Primula* that might have been the parents of the garden auricula. The "Botanical Magazine" contains a good coloured plate of *P. Palinuri*, 3414. It has golden-yellow flowers. Mr. G. C. Churchill alluded to this plant in a paper written subsequently and published in the "Gardeners' Chronicle," May 1, 1886, p. 562. *P. Auricula* was figured in the "Botanical Magazine," tab. 6837.

HYBRIDS AND HYBRIDISATION AMONG BULBOUS PLANTS.

By C. G. van Tubergen, Junr., of Haarlem, Holland.

Of the exceedingly numerous varieties of bulbous plants now grown in Holland and elsewhere in such amazing numbers, all, with very few exceptions, owe their original existence to having been raised from seed. It appears however that, with the exception of the modern raisers of the Daffodil, very few attempts indeed have been made by the general cultivators to do any artificial crossing, or, if any such has been done, to keep a record of it. Whether rightly or not, to me it has always appeared that however beautiful an artificially raised hybrid plant may be, it loses a part of its interest if the parents are unknown. The following is an enumeration of some of the hybrids among bulbous and tuberous rooted plants which have been raised in our nurseries, with brief notes of anything that struck me as being worth noticing :

Lilium.—Very numerous crosses among various species have been effected, and many seedlings are still under observation ; a good and note-

Fig. 118.—Lilium × Mar-Han (L. Hansoni ♂ × L. Martagon album ♀).

worthy race has sprung from the crossing of *Lilium Martagon album* with *L. Hansoni*. It is of particular interest to note that whereas *L. Martagon album*, if raised from seed, almost always comes perfectly true, scarcely ten among a thousand plants reverting to the typical purple Martagon Lily; out of the mingling of *L. Martagon album* with *L. Hansoni* not a single *white* martagon occurred. *All* plants (several hundreds) that showed no influence of the pollen-parent (*L. Hansoni*) reverted to the typical purple Martagon Lily. Those that showed the influence of *L. Hansoni* developed into stately, tall-growing lilies with broad, dark green foliage in whorls and pyramidal spikes, composed of very numerous flowers. The ground colour of the flowers of these hybrids is a more or less pronounced pale buff-brown, either flushed with crimson or with deep orange, and with purple spots. The individual size of the flowers much exceeds that of either parent. I named this strain *L. Mar-Han* (fig. 118), and I have already distributed two or three distinct varieties of it, while others are still in course of propagation. As far as I know, the cross effected by Mr. Powell of Southborough between *L. Martagon dalmaticum* and *L. Hansoni* either produced true hybrids or gave *dalmaticum* pure. Other crosses which gave good results were effected between *L. pardalinum* and *L. Parryi* and also between *L. pardalinum* and *Humboldtii.* These, however, have lately been raised also in America.

Brunsvigia Josephinæ.—This remarkable plant freely flowers with me, and I several times fertilised it with pollen of *Amaryllis Belladonna.* I have now large bulbs, four to six inches across, of these hybrids, which so far have not yet flowered, although some are over ten years old. It is very remarkable that the plants, though raised from seed of the *Brunsvigia,* show no influence of the mother parent, the bulb and foliage being that of an *Amaryllis Belladonna.*

Colchicum.—Some very interesting plants came out of a cross between *C. Sibthorpii* and the double white-flowered form of *C. autumnale.* The seedlings either produced a large, broad-petaled form of *C. Sibthorpii* or gave perfectly double-flowered *C. Sibthorpii,* the flowers being composed of hundreds of narrow petals of a lilac-red, faintly chequered with white. These double flowers are perfectly sterile, whereas in the double white-flowered *C. autumnale* one occasionally finds a good pistil with potent pollen.

Eremurus.—Some very strong-growing hardy hybrids, capable of resisting severe late spring frosts, which will kill or hopelessly damage flower-spikes and foliage of *E. robustus* and *E. himalaicus,* have been raised in my nursery by crossing *E. himalaicus* with early flowered forms of *E. robustus,* the result giving a fair percentage of immensely strong-growing plants, throwing spikes seven to eight feet in height, with flowers of a pale rose colour. These hybrids flower a little later than *E. himalaicus* and before *E. robustus* is out. Though not so showy as a finely developed specimen of *E. robustus,* the hybrid, which I named *E. him-rob,* has the particular advantage of being capable of safely escaping the often deadly injurious effects of late spring frosts. A very interesting and delicately beautiful plant is *E. Tubergeni,* which was produced by crossing *E. himalaicus* with pollen from an early flowered form of *E. Bungei.* In this plant the foliage has the deep green colour of that of *E. Bungei,* but is almost as broad as that of *E. himalaicus,* while the spikes and individual flowers most resemble those of *E. himalaicus*, the colour being a delicate pale primrose-yellow. Seedlings of this hybrid either produce true *E. Tubergeni* or *E. himalaicus*, but I have not observed any *E. Bungei* to reappear among them. Hybrids between *E. Bungei* and *E. robustus* or *E. Olgæ* (the latter were also raised in Sir Michael Foster's garden at Shelford) give plants in which a coppery salmon-yellow of the flowers predominates. In habit of growth and colour of the flowers some of the seedlings cannot be distinguished from *E. Warei*, which I have always regarded as a natural hybrid between *E. Bungei* and some rosy-coloured variety, but not a true species.

Freesia.—Up to very lately these charming plants only occurred in white and creamy-yellow shades, and though yearly raised by the million from seed in France and Italy, seem to have sported very little. Crosses between the small-flowered orange-yellow *F. aurea* and *F. refracta* gave interesting hybrids, but not an improvement on either parent. The advent of the rosy-crimson-flowered *F. Armstrongi* from South Africa has been a most welcome addition, as this at once opened a wide field for producing some more variation of colour among these so deservedly popular flowers. *F. Armstrongi* itself being a rather delicate grower, I made no attempt to fertilise it with pollen of *F. refracta alba* or *F. Leichtlini*, but placed pollen of *F. Armstrongi* on as many different shades and forms of *F. refracta*, *F. refracta alba*, and *F. Leichtlini* as I could procure. The results so far have been most encouraging, and I have now a strain of tall-growing Freesias with as many as nine individual flowers on every spikelet, of which every bulb produces several, in colours varying from the palest rose to carmine and purple-red. A small percentage also came in shades of orange, buff, and coppery-rose. A selection of these seedlings, with flowers of a violet-rose shade, I exhibited in the spring of this year (1906) at one of the fortnightly meetings in the Hall, when an Award of Merit was given to it by the Floral Committee. I also attempted to cross *F. Armstrongi* with *F. aurea* and *vice versâ*, but both ways with very poor results.

Gladiolus.—There are so many magnificent strains of hybrid Gladioli now being grown that I made no attempts to further improve the various races known as *gandavensis*, *Lemoinei*, *nanceianus*, *Childsi*, and others, but turned my attention to the original species. If the charming and so deservedly popular *G. Colvillei* and its chaste variety 'The Bride' should have been raised from *G. cardinalis* and *G. tristis concolor* (personally I very much doubt the correctness of this statement), then why should not the intercrossing of other South African species also be likely to give good results? Thus far I must own that, although a good many very pretty hybrids have been raised in my nursery, I have up to the present time only one or two strains of these Gladioli that may prove commercially useful. A selection of crosses between *G. alatus* and *G. cuspidatus* are dwarf-growing, very free-flowering Gladioli which flower in the open ground quite three weeks before the earliest of the *nanus* or *ramosus* sections, which, as is well known, precede the *gandavensis* and other strains in time of flowering from three to four weeks. These *alatus* × *cuspidatus* Gladioli, apart from their usefulness in flowering so early in the open ground (end of May), are very welcome additions to the *Gladiolus* family, as each bulb produces from two to five spikes of about a foot in height, with flowers of fair size and of a charming colour of rosy-salmon with golden-brown markings. They are admirable for filling small glasses for table decoration, and other choice floral work. This strain I named 'Express,' and from various sides I have already received letters expressing gratifying satisfaction with the habit, time of flowering, and general usefulness of these Gladioli. Other strains, results of crosses between the best and showiest of other South African Gladioli, are in course of development, and one or two, at least, seem likely to yield satisfactory results.

Hymenocallis.—With a view of ascertaining the correctness of the supposed parentage of *H. macrostephana*, after some years' trial I managed to have the two supposed parents *Hymenocallis speciosa* and *Hymenocallis* (*Ismene*) *calathina* in flower at the same time. The results showed absolutely different plants from *H. macrostephana*, being much broader and thinner in the leaf; the formation and size of the inflorescence and of the individual flowers also being quite different. When in good condition, this hybrid *Hymenocallis*, the first authentically on record between the evergreen section *Hymenocallis* and the deciduous Ismenes, is a magnificent plant, with an umbel of over a foot and a half across, with large, snowy-white individual flowers, exceeding in size even the large-flowered *H. macrostephana*. This hybrid has been distributed under the name of *H. Daphne*. Crosses between the white *H. calathina* and the yellow, green-banded *H. Amancaes* gave charming mules of a delicate sulphur-yellow. These, however, have also at various times been raised in England.

Iris.—The deep sandy soil and the climate of Haarlem seem to suit a very large portion of the Iris tribe, and from time immemorial Irises have been grown and improved by Dutch cultivators. *I. Xiphium* (Spanish) and *I. xiphioides* (*anglica*) strains, if raised from seed, will still yield agreeable surprises, but it is doubtful whether these really differ from those that were in cultivation a hundred and more years ago. So many species of the subgenus *Xiphion* being now in cultivation that were unknown to our ancestors, some eight to ten years ago I commenced intercrossing the Spanish, Portuguese, and Moroccan species of *Xiphion*, not using, however, the ancestors of the strain that are now known as the Spanish Irises. From these crosses various modifications at last resulted in a highly important race of very large-flowered Xiphions, of the form and shape of the Spanish Irises, but flowering quite a fortnight earlier. The flowers of this strain (which is not yet in commerce) show the same range of colours as is met with in the ordinary Spanish Irises, but the flowers are of unusual size and great substance, the falls being from $1\frac{1}{2}$ to 2 inches across, and the entire flower measuring over $4\frac{1}{2}$ inches from tip to tip. It is interesting to note that, whereas in the ordinary Spanish Irises the yellow colour is so abundantly represented, it was only in the later and latest generations of seedlings of my new strain that good and pure yellows have been developing. It is also interesting that, by continually selecting only the earliest-flowered varieties, the strain now obtained flowers nearly three weeks before the ordinary Spanish Irises, which, considering the fact that so many tens of thousands of these Irises are annually used for forcing, is another salient factor in the eventual commercial importance of this new strain.

Among species of the *reticulata* group the mingling of the richly coloured *I. Bakeriana* with selected forms of *I. histrioides* and *I. reticulata* gave charming combinations of colour among these very early-flowering gems. At present the influence of *I. Danfordiæ* is not apparent.

The *Juno* group of Bulbous Irises, which in the last twelve years has received important additions by the introduction of so many Asia Minor and Central Asiatic species, presented another field of work. These Irises, usually flowering at the time when sharp, late, night frosts occur, are not easy to cross, or rather to obtain good seed from, and I find that it is only once in every four to six years that my patient labours among these give

FIG. 119.—TYPE OF FLOWER OF NEW HYBRID XIPHION IRIS.

any satisfactory results, in so far as the obtaining of any seed is concerned. My earliest successes in this group came from crossing *I. persica purpurea* with the old *I. persica*, and from hybrids between *I. sindjarensis* and *I. persica*. The former I introduced under the name of *I. purpureo-persica* and the latter as *I. sind-pers*, both obtaining awards from the Royal Horticultural and other Societies. Later crosses produced the lovely *I. sind-pur* (*I. sindjarensis* × *I. persica purpurea*), *I. pur-sind* (the reverse cross), and others. The principal charms of these early-flowering bulbous Irises are their extreme hardiness, their free-flowering character, and their rich colouring.

RHIZOMATOUS IRISES.—No section offers greater interest to the plant-lover than the extremely interesting and beautiful group of *Oncocyclus* and their near allies the *Regelia*. Considering the great care that in our climate the successful cultivation of the *Oncocyclus* group demands, it has been my aim, by intercrossing the latter section with the easily grown *Regelia*, to raise a strain that would combine the beautiful and large flowers of the true *Oncocyclus* with the hardy character and free-flowering qualities of the *Regelia*. I tried crosses both ways, but soon found that hybrids raised from the *Oncocyclus* with the *Regelia* did not possess any more vigour than their seed-parents, whereas the *Regelia* section (*I. Korolkowi*, *I. Leichtlini*, *I. vaga*, &c.) crossed with pollen of the *Oncocyclus* gave birth to a hardy, free-growing and free-flowering race (fig. 120). Some of the varieties from these crosses have now been in cultivation in my nursery for over eight years, and the accompanying illustration, which shows a portion of my stock of *Iris Hecate* (one of the varieties raised from *I. Korolkowi violacea* × *I. iberica insignis*),

FIG. 120.—TYPE OF FLOWER OF REGELIO-CYCLUS IRIS.

speaks for itself, that in point of vigour and free-blooming qualities this new race, which has been distributed under the name of *Iris Regelio-cyclus*, leaves nothing to be desired. Another point in favour of this race is that in the open ground it flowers with the very earliest members of the Rhizomatous Irises, such as *I. præcox*, and some *pumila* varieties, preceding the host of ordinary bearded Irises (*germanica*) by from three to four weeks. From the *Regelia* parents they also inherited the desirable gift of producing two flowers in each scape, a second flower taking the place of the first on withering. Especially beautiful in this strain are the hybrids of selected varieties of *I. Korolkowi* type × with *I. susiana* and *I. iberica*, and also crosses between *I. Korolkowi violacea* with the purple-red *I. Mariæ*. Strange to say that a cross between *I. Korolkowi concolor* (which I find is not quite so vigorous as are the other *Korolkowi* varieties) and *Iris Mariæ*—reputed to be one of the most difficult of the *Oncocyclus* tribe—produced a group of exceedingly vigorous varieties, of which the now well-known 'Artemis,' a very vigorous variety with rich purple and violet-black coloured flowers, may stand as the type. Without going into cultural details, the fact that the stock of some of the varieties (all of course propagated by division from single, selected specimens of special merit) in many cases now consists of several hundred plants, conclusively shows that this race has come to stay in our gardens, and will not ultimately dwindle away as the pure *Oncocyclus* always do.

Naturally my attention also turned to the hybridising of the best varieties of the ordinary bearded Irises (*germanica*) with the *Oncocyclus*. A beautiful and very large flower came out of a cross between *I. iberica*

FIG. 121.—IRIS REGELIO-CYCLUS.

and *I. germanica macrantha*, the flower measuring not less than six inches across and of a beautiful blue, with broad, spreading falls, heavily bearded, and with a dark, black-blue central spot. Unfortunately in our climate, with its damp summers, we cannot give to these plants the dry, baking heat of southern countries, and the plants consequently are very shy-flowering, as are also similar crosses of *Oncocyclus* with *I. pallida* and *I. germanica*. Hybrids raised by Sir M. Foster, however, among these groups, notably *I. paradoxa* and *I. iberica* crossed with *I. variegata* and *I. sambucina*, are almost as free-flowering as is the ordinary *I. germanica*.

NERINE.—Notable hybrids in this beautiful group of autumn-flowering bulbous plants arose from the intercrossing of *Nerine pulchella*, of which I grow an almost evergreen variety, with the best of other species and hybrids. I cannot understand why all the plants which were raised from this cross, both ways, developed a much later blooming character than either parent, so much so, that they usually are about at their best the second and third week in December. These hybrids are very vigorous, with foliage $1\frac{1}{2}$ to 2 feet in length, and with flower-spikes of corresponding height. The range of colours in these hybrids is from pale rose to bright carmine, and the umbels, composed of very numerous flowers, measure from six to seven inches in diameter. Unfortunately these hybrids seem

Fig. 122.—Regelio-cyclus Iris var. Hecate.
Showing hardy character and free-flowering qualities.

to be absolutely sterile, so that I fear it will be very difficult, if not impossible, to further develop this strain, which, even as it is now, proves very valuable for producing bright-coloured effects in the cool conservatory at the dullest time of the year.

Narcissus.—Crosses among these have for the last eight to ten years been largely made in my nursery, and some good varieties have been raised. My observations on this class of bulbous plants, however, do not materially differ from those of other modern raisers of the Daffodil, and do not therefore call for any particular comment.

ON THE DERIVATION OF SOME RECENT VARIETIES OF ROSES.

By Arthur William Paul, Waltham Cross, Herts.

On the occasion of the last Conference on Hybridisation held by this Society in 1899, it was remarked at the opening of one of the sessions that the great value of these meetings is that they connect together the scientific aspect with the practical. It is my desire in the present paper to deal more particularly with the latter, or practical aspect, as regards the derivation of some of the most striking and remarkable roses for garden ornamentation that have appeared in recent years, and in so doing I propose to select for consideration those varieties that exhibit especially distinct traits of colouring, form of flower, habit of growth, and floriferousness, in preference to those remarkable only for the size and regularity of their blooms.

I think it will be conceded that in no other flower have we so many garden varieties as in the rose, and no other flower has enjoyed for so long a time an equal degree of popularity and extended cultivation both in this country and other lands. The rich materials which Nature has provided in a large number of wild species, and their widely differing characteristics and forms of beauty, have placed within the reach of hybridisers and crossbreeders opportunities for the variation and improvement of old forms and the evolution of new ones which I believe exist in no other single genus of ornamental plants, whilst the appreciation on the part of lovers of gardens of the results of successful labours in this field have proved a worthy recompense for the expenditure of time and skill involved therein, and a cheering incentive to fresh efforts.

From a very early period in the history of raising roses from seed, the wide variation in the character of the seedlings has been remarked. A French writer, seventy years ago (Boitard "Manuel de Roses," 1836, quoted in "The Rose Garden," 10th edition, p. 115), calls attention to this fact, and cites in illustration the experience of more than one well-known raiser of that time who constantly obtained plants of *R. spinosissima* among seedlings raised from carefully selected seed of *R. indica*, and also the presence of plants of *R. ferox* among seedlings of *R. rubiginosa*. My father, the late Mr. William Paul, always preferred to employ, when possible, plants on their own roots for the purposes of crossing and seed-bearing, so as to avoid the possible influence of a foreign stock on the progeny.

Looking back upon the rose as a garden flower one hundred years ago, we gather from the literature of that period that the number of garden varieties, apart from the botanical species, was comparatively limited, and their origin for the most part was a matter of speculation; but from that period onwards they have increased with marvellous rapidity, and the various sources from which they were derived are better authenticated. The first in the field as raisers of new varieties were the French rosarians, and for many years they held almost the monopoly in this branch of horticulture, such raisers as Hardy, Desprez, Prevost, Vibert, Souchet, Laffay, and Portemer earning well-deserved distinction in their day. With the march of events, however, nearly all their introductions have now disappeared. The large collections of *R. centifolia* (Moss and Provence Roses), *R. gallica* (French or Provins Roses), and other summer-flowering hybrid varieties gave way to the Damask Perpetuals and Hybrid Perpetuals, many of which in their turn are now yielding place to the Hybrid Teas, the 'Rambler' roses and other classes, and at the present time, although we still receive many good roses from France, some of the most valuable introductions are raised in the United Kingdom or reach us from Germany or the United States. So large has been the number of fresh introductions that have appeared during the past fifty years that collections have become unwieldy in extent, and I fear that not a few good roses have been consigned to oblivion for want of space. We have, however, without doubt preserved the best of them and abundantly sufficient for our purposes. There is, nevertheless, one class of older roses whose gradual disappearance, I think, is to be deplored from the present standpoint of rose-growing, and that is the stronger-growing hardy Noisette roses which are so valuable as autumn-blooming climbing roses. Some of these still exist in old gardens, but they have lost their names, and it does not appear possible to identify them with any degree of certainty.

As regards the particular rules that might be expected to govern the successful hybridisation and cross-breeding of the various species and families of roses, it may be said that the conditions vary. In some, such as the Sweet Briers, *Rosa multiflora*, and *Rosa Wichuraiana*, where the species or types are clearly defined and the ground comparatively untrodden, the obtaining of fresh varieties of merit has hitherto been a direct and comparatively simple matter. In the case of other classes, however, such as the Hybrid Teas, where the materials are more complex and the ground already occupied with established favourites, some of the leading raisers find it necessary for the attainment of their desired ideals to make successive crosses through two or more generations. Thus the distinct and beautiful decorative rose 'Gruss an Teplitz' was obtained by crossing in the first instance 'Sir J. Paxton' (Bourbon) with 'Fellenberg,' (Noisette), the seedling so obtained was again crossed with 'Papa Gontier' (Tea), and the progeny of this latter union was finally crossed with 'Gloire des Rosomanes' (Bourbon); and at the recent rose show of the German Rose Society, the seedling provisionally selected for the special prize of 3,000 marks (£150) and to bear the name 'Otto von Bismarck' was stated to have been obtained by a cross between 'Caroline Testout' and 'Grossherzogin Maria Dorothea,' the progeny being crossed again with 'La France.' In the grounds of a leading French raiser I was recently shown a considerable collection of intermediate forms, possessing various desirable qualities of colour, form, or size of petal, which the proprietor had selected from his various seedlings for the purpose of cross-breeding and seed-bearing, and which are in no one else's hands but his own.

It must, however, be admitted that many beautiful roses have been obtained by sowing naturally fertilised seed gathered at hazard, and some excellent new varieties of roses have also been obtained from branch sports of existing varieties. In the latter connection may be mentioned the Tea roses 'The Queen,' 'Rainbow,' and 'Madame Chédane Guinoisseau,' the Hybrid Teas 'White Lady' and 'Augustine Guinoisseau,' the Hybrid Perpetuals 'Duke of Fife' and 'Mrs. Sanford,' and many climbing forms, in some of which, such as Climbing 'Belle Siebrecht' and Climbing 'Captain Christy,' the additional vigour of growth adds greatly to the value of the variety for garden decoration. These dimorphisms or dichroisms seem, as perhaps might be expected, more prone to occur among varieties of those classes which by successive hybridising and cross-breeding have become furthest removed from their original source.

As a result of the intercrossing of so many varieties of different species and sections, the proper classification of many of the new introductions is becoming a matter of increasing difficulty, as the offspring is often found to possess the characteristics of more than one parent so evenly balanced that it might with equal propriety be referred to more than one class. This is especially the case with the Hybrid Tea roses, at present kept in a class by themselves, some of which might for horticultural purposes be appropriately classed with the Hybrid Perpetuals, whilst others might without violence to existing ideas be classed with the Tea roses. Again, in the case of some of the newer varieties of 'Rambler' roses it is difficult to determine whether they should be referred to the *multiflora* or *Wichuraiana* groups, which, although botanically closely allied, are horticulturally distinct.

Of recent years the varieties of *Rosa lutea* have been used as pollen-bearing parents with good effect. To the crossing of the Hybrid Perpetual variety 'Antoine Ducher' with the 'Persian Yellow' we owe the hybrid brier 'Soleil d'Or,' a rose which, although somewhat uncertain, is capable of giving flowers of great splendour and richness of colouring both in summer and autumn, and which cannot be otherwise regarded than as an acquisition of considerable interest and horticultural merit. As the result of further crosses of somewhat complex nature on the part of another raiser we have 'Gottfried Keller,' a very distinct hybrid of delicate and rich colouring, which also blooms in autumn, and which will be highly appreciated as a garden rose when it becomes more widely known. The first step in obtaining this variety was the crossing of the Hybrid Perpetual 'Pierre Notting' with the Climbing Tea 'Madame Bérard,' the offspring being crossed with 'Persian Yellow' and the progeny therefrom again fertilised with pollen from a direct cross of 'Madame Bérard' with 'Persian Yellow.' Flowers of 'Gottfried Keller' fertilised with pollen from the Hybrid Perpetual 'Charles Lefebvre' in the Waltham Cross Nursery matured their seed and have produced seedlings which resemble 'Gottfried Keller' in their brier-like foliage and habit of growth. On the occasion of a recent visit to the grounds of M. Pernet-Ducher of Lyons I saw a large breadth of the new hybrid rose named "the Lyons Rose," which he has already publicly exhibited at Lyons. This rose, which has flowers of a rich shade of salmon-pink, shaded with yellow at the base of the petals, is a seedling from a Hybrid Tea fertilised with pollen of 'Soleil d'Or.' Following on the introduction of so many excellent varieties of an earlier date, M. Pernet-Ducher has earned by his later crosses a distinction which entitles him to the congratulation of all lovers of roses.

The Hybrid Tea roses, which at the present time stand second to none in general estimation as garden roses, are as a class of comparatively recent introduction, the series having commenced with 'La France' and 'Captain Christy,' introduced by Guillot and Lacharme in 1867 and 1873 respectively. In a general way it may be said that they are the results of crosses between varieties of the Hybrid Perpetual and Tea-scented classes; but although we are given to understand that the earlier introductions were the results of direct crosses made between these two classes, more extended experience tends to show that in order to obtain the greatest percentage of successful results successive or indirect crossing must be employed. The origin and successive development of this most beautiful class of roses are exhaustively treated of in a paper read last year by M. Viviand-Morel of Lyons at the Rose Congress at Paris, and subsequently published in the Journal of the Lyons Horticultural Association. In this field British raisers have been eminently successful, some of the earliest varieties having been raised by the late Mr. Henry Bennett, who showed the potentialities of the cross by the introduction of a numerous series, including 'Lady Mary Fitzwilliam,' 'Grace Darling,' 'Viscountess Folkestone,' and others which, although introduced some twenty years ago, still remain in general cultivation. 'Lady Mary Fitzwilliam'—itself the result of crossing the Tea rose 'Devoniensis' and the Hybrid Perpetual 'Victor Verdier'—has been largely used as a factor in obtaining new varieties, and we have it on record that two well-known roses, 'Caroline Testout' and 'Antoine Rivoire,' were the results of crosses in which the pollen of 'Lady Mary Fitzwilliam' was employed, the seed-bearing parent in the case of 'Caroline Testout' being the Tea rose 'Madame de Tartas,' and in the case of 'Antoine Rivoire,' the Tea rose 'Dr. Grill.' I have also seen it stated that 'Madame Abel Chatenay' was a cross between 'Dr. Grill' and 'Lady Mary Fitzwilliam'; but the raiser's description at the time of its introduction gave 'Victor Verdier' as the pollen-bearing parent. Of recent years a large number of most valuable additions to this class have been obtained by various raisers on similar lines, but a distinct break was obtained a few years ago in 'Gruss an Teplitz,' alluded to above. As a decorative rose this variety is in the first rank, and it is to be hoped that it may prove a starting point for further crosses of a similar nature with a view to obtaining a series possessing similar characteristics of freedom of habit and blooming, with flowers of different shades of colour. Another very fine rose of recent introduction which, although generally classed with Hybrid Perpetuals, possesses some of the characteristics of the Hybrid Teas, is 'Frau Karl Druschki.' This rose, which is probably the finest white rose for general purposes at present in cultivation, is stated to be a cross between the Hybrid Perpetual 'Merveille de Lyon' and the Hybrid Tea 'Caroline Testout.' Some other noteworthy Hybrid Teas of recent introduction with whose parentage we are acquainted are: 'Earl of Warwick' (Tea 'The Queen' × H.T. 'Belle Siebrecht'); 'Pharisäer,' a seedling from H.T. 'Belle Siebrecht' (the pollen-bearing parent not stated); 'Richmond' (H.T. 'Lady Battersea' × 'General Jacqueminot'); 'Königin Carola' (H.T. 'Caroline Testout' × H.T. 'Viscountess Folkestone'); in the flowers of this variety the colour and characteristics of each parent are clearly discernible; 'Madame Jules Gravereaux' (Noisette 'Rêve d'Or' × H.T. 'Viscountess Folkestone'); 'Étoile de France' (H.T. 'Madame Abel Chatenay' × H.P. 'Fisher Holmes'); 'Madame Léon Pain' (H.T. 'Caroline Testout' × Tea 'Souvenir de Catherine Guillot'); 'Billiard et Barré' (H.T. 'Alice Furon' × Climbing Tea 'Duchesse d'Auerstaedt'). In the case of M. Pernet-Ducher's later series of Hybrid Tea roses which include 'Madame Ravary,' 'Le Progrès,' 'Prince de Bulgarie,' 'Joseph Hill,' 'Mélanie Soupert,' 'Marquise de Sinety,' 'Instituteur Sirdey,' 'Paul Ledé,' 'Madame Philippe Rivoire,' and others, I have seen no authoritative statement of the crosses employed, but I think we shall not be far wrong in assuming that the rich yellow and orange tones of colour in the flowers may be due to the direct or indirect employment of pollen from varieties of *R. lutea*. With the ever-increasing store of material available, there are surely possibilities of further distinct and valuable advances in the Hybrid Tea and Hybrid Perpetual classes.

Although botanically closely allied, from a horticultural standpoint the Chinese or Bengal and the Tea-scented roses have been regarded as distinct classes; but of late years, by the crossing of varieties, we have obtained a class of Chinese roses which are approximating in size and form of flower as well as in shades of colouring to the Tea roses, whilst in the Tea section we have a new series of decorative varieties which combine the stronger habit of growth in the Tea roses with the excessive freedom of flowering of the Chinese. To the former category, which may be termed 'Téa-Chinas,' may be referred such varieties as 'Madame Laurette Messimy,' 'Madame Eugène Résal,' 'Irene Watts,' 'Queen Mab,' 'Aurore,' 'Comtesse de Cayla,' and others, whilst to the latter, which may be termed 'China-Teas,' belong 'Corallina,' 'Souvenir de Catherine Guillot,' 'Souvenir de J. B. Guillot,' 'Enchantress,' and others possessing similar characteristics. All these are most valuable for garden ornamentation, especially in the late summer and early autumn months, and further distinct introductions will be most welcome. A new hybrid Chinese rose, possessing traits peculiar to itself, is 'Petrus Donzel'; in the colour of the flowers and the habit in which they are produced there appears to be some affinity with 'Gruss an Teplitz,' but the growth of the plant is not so vigorous.

A remarkable example of the variations to be obtained by hybridisation in roses is afforded by the dwarf-growing Polyantha or Multiflora roses now so largely used for massing and edging in gardens as well as for pot culture. This series commenced some years ago with 'Pâquerette' (Guillot), 'Perle d'Or' (Dubreuil), 'Anne Marie de Montravel' (Rambaux), 'Gloire des Polyantha' and others, which were obtained by crossing *R. multiflora* (*polyantha*), a strong-growing summer-flowering species, with the dwarfer-growing *R. indica* or other autumnals, the result being quite a new class, possessing the floriferous habit and large corymbs of flowers of *R. multiflora* combined with the autumnal flowering qualities of the other parents and a dwarf and regular habit of growth. Later introductions in this class have been the result of successive and more extended crossings, and in some instances have led to the production of larger individual flowers, although in smaller bunches. A recent variety, however, which possesses to a remarkable degree the original characteristic of producing its flowers in handsome corymbs, is 'Madame Norbert Levasseur'; its rich masses of crimson blossoms recall those of the 'Crimson Rambler,' and its period of flowering would appear to extend from earliest summer until late in autumn. I have been unable to obtain any authoritative information as to the parentage of this variety. Some distinct dwarf autumnal-flowering Polyantha roses of recent introduction are:

Aschenbrödel—Dwarf Polyantha 'Petite Léonie' × *R. lutea bicolor* (Austrian Copper).
Rosalinde—Dwarf Polyantha 'Georges Pernet' × a seedling from 'Crimson Rambler.'
Katherine Zeimet—Dwarf Polyantha 'Étoile de Mai' × Dwarf Polyantha 'Marie Pavić.'
'Petit Constant'—Dwarf Polyantha 'Mignonette' × Tea 'Luciole.'
Primula—Dwarf Polyantha 'Mignonette' × unnamed seedling.
Eugénie Lamesch—Multiflora 'Aglaia' × Noisette 'William A. Richardson.'
Léonie Lamesch—Multiflora 'Aglaia' × seedling Polyantha.

It is remarkable that in the case of 'Eugénie Lamesch' the union of two strong-growing climbing varieties should have resulted in a progeny of dwarf habit of growth. There is, I think, in *R. multiflora* always a tendency to give dwarf forms from seed; and in the case of 'Waltham Rambler,' raised in the Waltham Cross Nurseries, the original seedling plant was quite dwarf in habit and the variety did not develop its climbing form until budded on various stocks.

For town gardens under unfavourable climatic conditions, and for hedge planting and other bolder purposes in the rose garden, the Rugosa roses have proved most valuable acquisitions, and some very distinct and handsome hybrids have been introduced of late years: these, while retaining the vigour and hardiness of the type, have lost some of the roughness of contour which is sometimes objected to in the latter. Commencing with 'Madame Georges Bruant' (*Rosa rugosa* × Tea 'Sombreuil'), which was introduced from Poitiers in 1887, we now have a series of large double-flowered varieties of various shades of colour produced on strong-growing shrubs of absolute hardiness. A distinct variety with fringed petals, called 'Fimbriata,' was the result of a cross between *R. rugosa* and the climbing Hybrid Tea 'Madame Alfred Carrière.' One of the most beautiful of recent introductions is 'Conrad Ferdinand Meyer,' raised in Germany as the result of a cross between 'Gloire de Dijon' and 'Duc de Rohan' (H.P.), the offspring being again crossed with the Rugosa variety 'Germanica.' Other new varieties that have reached us from the neighbourhood of Paris are 'Souvenir de Philémon Cochet,' a naturally fertilised seedling from 'Blanc double de Coubert' and a distinct advance on the latter variety and 'Rose à parfum de l'Haÿ' (*R. damascena* × 'General Jacqueminot' × *R. rugosa germanica*). The last, as its name implies, is remarkable for its fine perfume. I think that as the newer varieties of this group become more widely known, their fine decorative qualities will receive wider recognition, and the thanks of rosarians are due to Monsieur Gravereaux of Paris for directing attention to this section.

Probably no class of rose is more in evidence in gardens at the present time than the strong-growing varieties known as 'Rambler' roses. These owe their popularity as well as their current appellation to the 'Crimson Rambler,' a variety of *Rosa multiflora*, which reached our shores from Japan, unheralded and unknown, about twenty years ago, and had already been in cultivation here for several years and under more than one name before its merits were finally demonstrated in the Slough nursery, where the magnificent effect of the established plants in full bloom procured for them the honour of a visit from her late Majesty Queen Victoria. I have never seen the parentage of this variety stated with authority, and it is probably unknown, but it was easy to foresee from the first that hybridisers would soon attempt to obtain counterparts of it in other shades of colour. The first to reach us were the yellow, pink, and white 'Ramblers' from Germany known as 'Aglaia,' 'Euphrosyne,' and 'Thalia,' raised from the type *R. multiflora* crossed with 'Rêve d'Or,' 'Mignonette,' and 'Pâquerette' respectively, and which are still valuable climbing roses. Many other seedlings of varying degrees of excellence, and with both single and double flowers, have been raised and introduced since; some the result of natural fertilisation, others by hybridising or cross-breeding, *R. Wichuraiana* either as pollen or seed-bearer having been employed with good effect in some instances. Some of the best and most distinct of these may be particularised as follows:

'Blush Rambler'—semi-double pink flowers. Parentage not stated.

'Débutante'—pale pink, distinct both in foliage and form of truss. Parentage not stated.

'Gruss an Zabern'—'Euphrosyne' × Tea 'Mme. Ocker Ferencz'; double white, habit not quite so vigorous as others of the section.

'Hélène'—(H.T. × 'Aglaia') × 'Crimson Rambler.'

'Hiawatha'—a seedling from 'Crimson Rambler,' other parent not stated; single flowers, crimson with white eye.

'Kathleen'—'Crimson Rambler' × 'Félicité Perpétue;' single flowers, rosy-red with white eye.

'Leuchtstern'—a seedling from 'Crimson Rambler;' single flowers, pink with white eye.

'Non Plus Ultra'—a dark crimson form of 'Crimson Rambler' obtained by crossing this variety with the dwarf Polyantha 'Blanche Rebatel.'

'Philadelphia Rambler'—'Crimson Rambler' × H.P. 'Victor Hugo'; a brilliantly coloured form, but the flowers are produced singly or in small clusters.

'Psyche'—'Crimson Rambler' × Dwarf Polyantha 'Golden Fairy.'

'Rubin'—fine brilliant red. Parentage not stated.

'Trier'—'Aglaia' × H.P. 'Mrs. Sharman Crawford'; double white flowers; blooms in autumn.

'Wallflower'—'Crimson Rambler' × Tea 'Beauté Inconstante.'

'Waltham Rambler'—from naturally fertilised seed; single flowers, pink with white eye.

'Wedding Bells'—a seedling from 'Crimson Rambler'; double flowers, pink with white eye.

Special mention must be made of the three American varieties 'Dorothy Perkins' (*Rosa Wichuraiana* × H.P. 'Madame Gabriel Luizet'), 'Lady Gay' (a seedling from 'Crimson Rambler,' other parent not stated), and 'The Farquhar' (*R. Wichuraiana* × 'Crimson Rambler'). These gorgeous decorative roses possess a certain family likeness, and under varying conditions of cultivation, soil, and climate are apt at times to resemble each other, although in the trials at Waltham Cross the points of difference have been apparent, especially when the plants have been grown under glass. Probably 'Lady Gay' will be found to be the finest of the three for general purposes of garden ornamentation.

Two most valuable properties of *R. multiflora* are the prodigious quantities of flowers the plants produce, and the long period during which the flowers remain in beauty on the plants. Even the single-flowered varieties maintain their beauty in the garden for five or six weeks if the weather is fine at the time of flowering. In the progeny of crosses with *R. Wichuraiana* we have obtained in addition the glossy and almost persistent foliage of that species, as well as the tendency to produce strong prostrate or lateral shoots, which render such varieties valuable for carpeting rough ground and banks as well as for climbing.

I have previously alluded to the autumnal-flowering dwarf forms of *R. multiflora* known as Polyantha roses, which have been obtained by hybridising this species with others of a low-growing nature and perpetual-blooming qualities, and I think that the facility with which *R. multiflora* produces its seeds, and the strong tendency to variation shown by the progeny resulting from artificial fertilisation, render this species one of the most promising for obtaining further distinct variations for our gardens. By judicious and persevering hybridising with autumnal-flowering species of a strong habit of growth, there seems no reason why we should not obtain climbing roses that will be as effective in the autumn as in the summer, in the same manner as are the dwarf Polyantha roses; and, by further crosses with varieties of persistent foliage, a new class of evergreen climbing roses might be evolved which would surpass the older varieties of Sempervirens and Noisette roses.

By the hybridising of *R. Wichuraiana* with varieties of the Tea-scented and Noisette classes we have also obtained some distinct and valuable additions to our climbing and running roses, some with single flowers, whilst others have double blossoms. They commenced a few years ago with a small series from America, and subsequently some valuable additions have reached us from France. Some of the most distinct are the following:

'Albéric Barbier,' *R. Wichuraiana* × Tea 'Shirley Hibberd.'

'Edmond Proust,' ditto × Tea 'Souvenir de Catherine Guillot.'

'Eliza Robichon,' ditto × Noisette 'L'Idéal.'

'Ferdinand Roussel,' ditto × Tea 'Luciole.'

'Gardenia,' ditto × Tea 'Perle des Jardins.'
'Jersey Beauty,' ditto × Tea 'Perle des Jardins.'
'Pink Pearl,' ditto × H.T. 'Meteor.'
'Réné André,' ditto × Noisette 'L'Idéal.'
'Wichuraiana rubra,' ditto × 'Crimson Rambler.'
'Joseph Billiard,' ditto × 'Madame Eugène Résal.'

With the exception of *Wichuraiana rubra*, these will all be found to vary from the type (*R. Wichuraiana*) in flowering earlier in the summer, whilst, notwithstanding the crosses with autumnal-flowering varieties, none of them can be described as autumnal bloomers.

My notes would not be complete without a reference to the hybridisation of the sweet brier, which in the hands of the late Lord Penzance yielded some beautiful decorative forms, the early summer being their especial season for blooming. Mention should also be made of *R. lævigata*, the 'Camellia rose' of the south of Europe, already reputed to be one of the parents of the hybrid Banksian *Fortuneana*, and which has given us of late years the beautiful single pink hybrid known as the 'Anemone rose' (not to be confused with *R. anemoneflora*), which appears to be hardier than *R. lævigata.* Attempts at fertilising flowers of the Anemone rose at Waltham Cross have hitherto failed to result in the production of seed. Some handsome single-flowered seedlings from *R. macrantha*—itself of hybrid origin—have also been recently added to the effective varieties of garden roses blooming in early summer. Of the many interesting crosses of other species and varieties of roses now being carried out and tested in various botanic and private gardens and nurseries it is too early to speak from a horticultural point of view, as the results are not in general cultivation; but it is reasonable to suppose that, when the time arrives for the next Conference of this Society on hybridisation and cross-breeding, substantial progress will have been recorded in the evolution of valuable new forms for the ornamentation of our gardens.

List of some hybrid roses exhibited by Wm. Paul & Son, Waltham Cross, at the Royal Horticultural Hall, July 31, 1906:

Hybrid Tea 'Earl of Warwick' (Tea 'The Queen' × H.T. 'Belle Siebrecht').
Hybrid Tea 'Countess Cairns' (T. 'President' × H.T. 'Caroline Testout').
Hybrid Tea 'Madame Léon Pain' (H.T. 'Caroline Testout' × T. 'Souvenir de Catherine Guillot').
Hybrid Tea 'Irene' (T. 'Madame Jules Finger' × H.T. 'Caroline Testout').
Hybrid Tea 'Mrs. Isabelle Milner' (T. 'Princess of Wales' × H.P. 'Robert Duncan').
Hybrid Tea 'Gruss an Teplitz' (Bourbon 'Sir Joseph Paxton' × Noisette 'Fellenberg' × T. 'Papa Gontier' × 'Gloire des Rosomanes').
Hybrid Tea 'Warrior' (T. 'Marie Van Houtte' × H.T. 'Princess Bonnie').

Fig. 123.—Hybrid Perpetual Rose 'Frau Karl Druschki.'
H.P. 'Merveille de Lyon' × H.T. 'Caroline Testout.'

Fig. 124.—Hybrid Brier Rose 'Gottfried Keller.

(H.P. 'Pierre Notting' × Tea 'Madame Bérard') × 'Persian Yellow' × ('Madame Bérard' × 'Persian Yellow.')

Fig. 125.—Hybrid Rugosa Rose 'Conrad Ferdinand Meyer.'
(Tea 'Gloire de Dijon' × H.P. 'Duc de Rohan') × *rugosa germanica.*

Fig. 126.—Hybrid Multiflora Rose 'Lady Gay.'
A seedling from 'Crimson Rambler.'

Fig. 127.—Hybrid Wichuraiana Rose 'Alberic Barbier.'
R. Wichuraiana × Tea 'Shirley Hibberd.'

Fig. 128.—Hybrid Wichuraiana Rose 'Jersey Beauty.'
R. Wichuraiana × Tea 'Perle des Jardins.'

Hybrid Tea 'Celia' (T. 'Marquise de Vivens' × H.T. 'Marquise de Salisbury').
Hybrid Tea 'Königin Carola' (H.T. 'Caroline Testout' × H.T. 'Viscountess Folkestone').
Hybrid Tea 'Étoile de France' (H.T. 'Madame Abel Chatenay' × H.P. 'Fisher Holmes').
Hybrid Tea 'Dora' (H.T. 'Antoine Rivoire' × H.P. 'General Jacqueminot').
Climbing Tea 'Madame Jules Gravereaux' (Noisette 'Rêve d'Or' × H.T. 'Viscountess Folkestone').
Dwarf Polyantha 'Eugénie Lamesch' (Multiflora 'Aglaia' × Noisette 'Wm. Allen Richardson').
Dwarf Polyantha 'Aschenbrödel' (Polyantha 'Petite Léonie' × 'Australian Copper').
Rugosa Rose à parfum de l'Haÿ [(*R. damascena* × H.P. 'General Jacqueminot') × *rugosa germanica*].
Rugosa 'Conrad F. Meyer' [(T. 'Gloire de Dijon' × H.P. 'Duc de Rohan') × *rugosa germanica*].
Wichuraiana 'Dorothy Perkins' (*R. Wichuraiana* × H.P. 'Madame Gabriel Luizet').
Wichuraiana 'Jersey Beauty' (*R. Wichuraiana* × T. 'Perle des Jardins').
Hybrid Brier 'Soleil d'Or' (H.P. 'Antoine Ducher' × 'Persian Yellow').
Hybrid Brier 'Gottfried Keller' [(H.P. 'Pierre Notting' × T. 'Madame Bérard') × 'Persian Yellow' × (T. 'Madame Bérard' × 'Persian Yellow')].
Hybrid Perpetual 'Frau Karl Druschki' (H.P. 'Merveille de Lyon' × H.T. 'Caroline Testout').

Tea-China Roses :

'Madame Laurette Messimy'
'Arethusa'
'Cora'
'Petrus Donzel'
'Madame Eugène Résal'
'Aurore'
'Cardinal'
'Comtesse de Cayla'
'Queen Mab'
'Irene Watts'
'Alice Hamilton'

China-Tea Roses :

'Souvenir de Catherine Guillot'
'Sulphurea'
'Madame Renée de St. Marceau'
'Madame L. Poncet'
'Enchantress'
'Fairy Queen'
'Margherita di Simone'
'Princesse de Sagan'
'Corallina'
'Dainty'
'Souvenir de J. B. Guillot'

LECTURE ON HYBRID *PELARGONIUM GRANDIFLORUM NANUM*

By Max Bürger, of Halberstadt, Germany.

My predilection for *Pelargonium grandiflorum*, generally called 'English' or 'Odier,' commenced in my childhood. At that time these plants were more favoured by the amateur than by the professional horticulturist, and were only occasionally to be found in nursery gardens. Precisely for that reason, the impression made on me—a gardener's son—was at that time so great that even now I am charmed by the vivid recollection of those sitting-room windows, which were yearly filled with a wealth of bloom by these pelargoniums.

The striking luxuriance of pelargoniums, even without the care of a gardener, and in small overcrowded dwellings, particularly in rural cottages in the vicinity of dung-heaps &c., can be explained by the fact that pelargoniums are to a great extent capable of absorbing large quantities of nitrogen from the atmosphere * by means of their fine, small glandular hairs. A better explanation of this lies, however, in the fact that the pelargonium, being a native of the Cape, thrives better in winter in the dry atmosphere of a room than in the humid one of a greenhouse.

As a young gardener, I found later, that is about thirty years ago, in some nursery gardens, a rich assortment of these pelargoniums. Their names showed that they were of a French and English strain.

I was greatly impressed by their splendid range of colour, but not at all by the beauty of the plants, which were mostly long and straggling, each individual branch having to be supported by a stick. After the short blooming season, it was a leafless, weak, undesirable-looking skeleton.

Twenty years ago, I greatly admired the large variety of colour, which had been derived from the original type-colour (white with pinkish-lilac pencillings), but at the same time I regretted that it had then been impossible to give to the plants a finer, more vigorous growth, and a more robust constitution. By degrees many gardeners took up the culture of pelargoniums, and many novelties were obtained in England, France, and Austria, but only a few were perceptibly better in the above respect.

Some of the best varieties were 'Mabel,' 'Mme. Thibaut,' and 'Viennese Pearl,' which soon became widely known and distributed everywhere. They were propagated in many nurseries, particularly in Vienna and Zittau, in large quantities, and in the springtime became more and more popular market plants.

The rapidly increasing popularity of these plants with the public

* Herr Bürger's opinion that his pets are capable of absorbing free nitrogen from the atmosphere must be accepted with caution as at least "not proven," indeed the present state of our knowledge on the subject would seem to make it improbable.—Editor.

induced most gardeners to cultivate them; but many soon gave up the cultivation again, as they required too much attention.

Now I also followed my hobby, and entirely gave myself up to the cultivation of these plants.

I had a strong determination and also a conviction that I should succeed in improving the plants and rearing one easy of culture, and at the same time a fine specimen of good marketable value. I justified myself in this hope by my former success in obtaining new strains of vegetables and annuals by hybridisation. My stock-gillyflower strains, 'Giant Excelsior' and 'Large-flowered Victoria Bouquet,' and my

FIG. 129.—SEEDLING HYBRID PELARGONIUMS.

keeping-beans, more especially 'Bürger's Fadenlose' (Bürger's Stringless), are still in the front rank.

At that time I was practically the only one to occupy myself with such hybridisations, in order to procure a specific improvement, or a particular colour which was lacking in the strain, instead of leaving it to chance, as was usually done.

Through my many hybridisations, I had learnt much by careful observation, and arrived at many new and interesting facts which I hoped to turn to good account in my study of pelargoniums.

I did not conceal from myself that these experiments with pelargoniums would involve me in greater difficulties, and much greater expense and expenditure of time, than the experiments with annuals. Without either great trouble or expense, I could soon bring the annuals into bloom in the open; while to do so with pelargoniums required at least a whole year's cultivation in a frame. Therefore I entered on this task after much deliberation and careful consideration, in order not to waste too much time and money. However, it has required a far greater sacrifice than I at first anticipated.

During the first years the results were altogether insignificant, and it was only after fifteen years of the most arduous exertions that it was possible to exhibit the first collection which showed an entirely new strain, of which the distinguishing features at once arrested the attention of the beholders.

These improvements have been retained in every way, so that my strain is now known all over the world, and wherever it is introduced all the old types are supplanted.

In spite of this I work, year in, year out, unceasingly, for the perfecting of my pelargoniums, as I have not yet entirely gained the high standard of excellence which I have set before myself to aim at, and to which new ideals may constantly be added.

I set myself the task of eliminating from these plants their worst faults:

1. Their long straggling habit of growth.
2. Their poor foliage.
3. Their liability to aphis attack.

But I am still striving to fix in them further improvements, *e.g.* perpetual bloom during the whole summer, their utility as bedding-out plants, and tenacity of blooms.

I perceived that my first and foremost task was to raise a compactly-growing plant, which my experience with other plants had shown was possible. Almost all plants grown from seed, sooner or later, acquire a low habit of growth; thus we have dwarf forms of almost all annuals, *e.g.* dwarf stocks, dwarf asters, dwarf phloxes, dwarf balsams, &c., also dwarf peas, dwarf beans, &c. And we frequently meet with these dwarf forms amongst wild plants, particularly under trees.

The dwarf forms are produced for the most part in elevated situations, but they also exist in the plains; and, as I have already said, they particularly frequently occur in the course of cultivation.

I have often pondered over the problems of these developments, and the horticultural science of that time gave no evidence whatever as to how these dwarf forms could be arrived at with certainty.

My observations indicated that the dwarf forms must have conditions of life which hindered them from arriving at a full development of their normal growth.

Therefore the problem was to manufacture such conditions! At this time there came to my assistance an old gardening experience which is expressed in the well-known proverb: "New seed, much growth; old seed, much fruit," and likewise in the world-wide remark, "This tree has flowered itself to death."

The underlying meaning of both these sayings is identical, or, at all events, both rest on the same law of Nature, which I wish to express in the following terms:

All vegetable life in the grip of death, seeks, with the last strength it has, to reproduce and disseminate itself, *e.g.* the tree which has flowered itself to death has, for certain, in the previous year felt the death germ, and for that reason used up all possible nourishment, in the forming of flower-buds only, which otherwise would have gone to the enlarging and strengthening of the whole growth. We can, moreover, still further apply the above-mentioned saying, in that we may say, the younger and more vigorous a tree is, the poorer the show of blossom; the older and more miserable, the richer. Also the other proverb: "New seed, much growth; old seed, much fruit," is explained by the same fundamental principle, in that with age the seed loses, in a recognisable manner, its germinating capacity; it therefore is also an organism which is in the grip of death, and will consequently be at more pains to use up less strength towards the growth of the plant than for the speedy formation of numerous reproducing organisms.

For this reason I used in my propagating experiments only those organisms which were commencing to show signs of decay.

At the same time I also tried crossing this 'Odier-Pelargonium' with all the other species of pelargonium that I could obtain, in order—if possible—to procure an upright form instead of the old straggling bush form.

During the first years of my experiments, I had scarcely any results worth mentioning. The seedlings always became taller, some of them reached one metre in height before they bloomed, and the results of my labours had for the most part to go to the rubbish heap. My colleagues, when they visited me, laughed at my extraordinary efforts in culture; yet I did not allow myself to be discouraged, but was content with the smallest signs of improvement, in the hope that in succeeding generations better results would be visible.

Though the results of my efforts were apparently so poor, they were, nevertheless, extremely interesting, and they also kept observation and expectancy at the utmost stretch, although the sacrifices involved were most discouraging.

I have quietly continued following the prescribed method, and found even in the next generation of seeds a marked advance, which yearly became greater and more astonishing, until ultimately I arrived at the upright form and my seedlings in the autumn are more like young primulas than pelargoniums. The stem has quite disappeared, and only a full luxuriant rosette of leaves clothes the pot, in the middle of which in the spring, often as early as February, the flower-buds appear. This has become the typical form of my strain, of which the principal feature consists in the height of their growth being limited and always restricted to one central truss, which then forms side-shoots out of all the axils of the leaves, which in their turn end in trusses of blooms, so that the plant presents a compact low mass of foliage overshadowed by a splendid bouquet of bloom.

In the meanwhile the foliage has also become much more luxuriant; not only thicker, owing to its low growth, but also the individual leaves are larger, darker, more succulent, and more vigorous. I consider that for these results I am indebted to crossing with zonal pelargoniums. In this hybridisation I next proposed to import into the large-flowering varieties the fire or scarlet-red of the zonals, and also to make the leaves capable of resisting the attack of aphides.

I was not for a time successful in either of these aims, but could constantly detect more favourable indications in the foliage.

At this point I should say that the hybridisation between these two plants presented unlooked-for difficulties, which were, in fact, only overcome after I had made improvements in both, following out the theory of Herr Lindemuth, Inspector of Gardens. But even after this I was only successful in the crossing between these two plants when I used *Pelargonium grandiflorum* exclusively as seed-bearer (female plant). With the zonal pelargonium as seed parent unfortunately I never succeeded.

I should have expected better results if I had been able even once to make the zonal pelargonium the female plant. I explain to myself the failure of the zonal pelargonium to become the seed-bearing parent in the following way, but whether my theory is correct or not I do not know.

Since *Pelargonium grandiflorum* is larger in all parts of its blossoms than the zonal pelargonium, its pollen grains may be too large to find an entrance into the pollen-tube of the zonal pelargonium, and therefore fertilisation becomes impossible.*

It is a most disappointing thing that most crossings, and those precisely the most difficult and the ones from which the best results are to be looked for, produce infertile seedlings. It is just these that are so important in the continuation of further fertilisation, that the experiment must be repeated, with, according to circumstances, either the female or male plant, until the desired result is obtained. I have, in fact, found this to be the case in obtaining the scarlet colour as the result of hybridisation between these two parents. I had unfortunately to reject, amongst the most remarkable hybrid seedlings, those which most distinctly exhibited the zonal strain, because they were absolutely infertile, and I had to snatch at those in which I could detect the smallest zonal trace. These were always crossed again with the scarlet zonal, till finally I arrived at my originally longed-for fiery-red strain,† 'Friedrich Engel.'

Now it became easier to raise a scarlet. There followed from this 'Andenken an Wildpark,' and ultimately 'C. Holzmann.' This last shows a pure scarlet with a dark blotch, but unfortunately is completely infertile, and therefore cannot be used for further experiments in hybridisation. This is the more unfortunate since a similar pure scarlet has never, up to the present time, appeared again in my seedlings. This one always reminded me of quite an old variety—'Hofgärtner Huber,' which had quite a small bloom, but of a clear scarlet colour, and was also completely infertile; in fact, the generating organs were mostly quite

* In the process of translation we are afraid this sentence has become confused. The pollen-tube proceeds from and grows out of the pollen grain, so that Herr Bürger probably means that the tube proceeding from the grain of *P. grandiflorum* pollen may be too large to find its way through the style of the zonal. But this appears to us somewhat improbable.—EDITOR.

† 'Perle von Halberstadt u. Fr. Engel.'

absent. I believe this variety was also a hybrid between *Pelargonium grandiflorum* and *Pelargonium roseum* (*rosodorum*).

I have come to the conclusion that one may make the following rule with regard to pelargoniums—namely, that every hybrid which has inherited an equal number of features from both parents is always infertile, and those only are fertile which incline more to one side, and that the female side.

I had much trouble in producing deep, dark blotches in the scarlet.

At first, all the fiery-red blooms were without markings—'Fireball,' 'Perle von Halberstadt,' &c. Later on I succeeded in getting a small dark marking in 'Fr. Engel,' but even this was not velvety enough. Only after I had crossed this fiery-red strain with a violet did it present larger, darker, velvety blotches. But through crossing with the violet I had spoilt my fiery-red, which had been obtained with so much trouble, as then there appeared more of a carmine colour.

I crossed and recrossed them a multitude of times before I arrived at a fiery-red with beautiful dark blotches, as in 'Andenken an Wildpark' and 'C. Holzmann.'

I am still striving to get more of the influence of the zonals into my strain, because by so doing I hope to ultimately make it free from aphides.

In my latest novelties there appears also to be more *peltatum* influence than formerly, and this shows its effect in a remarkable manner in the progeny.

I had not set any great store by the crossings with *peltatum*, because I had not found anything remarkable as the direct results therefrom. I only arrived at a violet variety, 'Grossmütterchen,' in which the parentage was easily detected by the form of its blooms and the scent of its foliage. However, this plant disappeared ten years ago, because I did not think it sufficiently worthy to take the place of a female plant in my hybridisations. Since that time I have never crossed with *peltatum* again. So much the greater, therefore, was my astonishment when a few years ago a seedling came to me which certainly owed its parentage to *peltatum*:

(1) By the smell of its leaves, and

(2) By the peculiar sort of hoariness of the upper parts.

This is the variety "Ballkönigin," one of my best, for it succeeds splendidly in window boxes, and on balconies and in sheltered positions in the open, and blooms the whole summer through.

It goes without saying that I tried to reproduce these excellent qualities in all my new varieties. I therefore owe my best results at the present time to "Ballkönigin" and to the *peltatum* influence, in spite of the fact that I had trusted it least of all.

Moreover, it seems to me that the blooms of these later varieties stand better—that is, they do not drop off so easily, which would certainly be a decided improvement, as then the blooms would be more valuable as cut flowers. On the other hand, the dropping-off of the petals is a promising sign because as soon as the seed-vessel has been fertilised, the blossom sheds its petals.

The artificial fertilisation of pelargoniums is in the highest degree easy and the result may be depended upon, since the pistil is so prominent that it is almost impossible for one single flower to be self-fertilised. On the other hand, for example, how difficult this is in the case of the stock-gillyflower, in which one is almost too late when one even opens the bud for the purpose.

In a reliable artificial fertilisation of a pelargonium, it is almost impossible for any foreign influence to take effect; hence the observations made in this case are of quite peculiar value.

It is of course necessary to have very exact data, from which must be constructed our fundamental rules, in order therefrom to continue constructing further and further until the ideal is attained.

To go into this with further particulars would take too long, as it involved fifteen years of work and cannot be disposed of in a few moments.

I only want to make one more observation, for which—up to the present—I have no better explanation than that even in the fertilisation of plants "inclination" also plays a part.

It is a very remarkable fact that a natural hybrid multiplies more rapidly; firstly, it is seldom quite infertile, and secondly, its descendants are nearly always constant—that is, they retain the new qualities. On the other hand, artificial hybridisation produces many infertile hybrids, of which the descendants always revert to the type. As an example of this:—The Stock which was a natural hybrid between the Dresden and summer Stock formed a white 'Excelsior' stock and remained constant; that is to say, my next year's seedlings produced only symmetrical plants with one peduncle, and none at all which reminded me of the primitive 'Dresden' form. But when I crossed this new variety with coloured 'Dresden' and 'Giant' varieties (*Riesenbomben*), in order to arrive at an 'Excelsior' with a variety of colours, I had in the following year scarcely one quite true-coloured plant amongst them; even the white ones had reverted. From this it took six years before I arrived at an assortment of ten constant colours.

In conclusion, I wish to say that if I have accomplished much with my pelargoniums, as appears to be the case from the numerous recognitions which I have received from people of all countries, I am still far from having attained the ideal which I have set before myself, since with each new result obtained one's aims become wider, and it would be a source of great pleasure to me to see at least some of these realised.

Above all, I should like to advance so far that pelargoniums would be raised entirely from seeds, as is the case with cinerarias and primulas. Visitors to my nurseries, where thousands of seedlings are raised annually, are always charmed with them, and believe this time to be not far off.

In a conscientious raising of seeds, my strain remains entirely constant and I obtain a great variety of the most charming colours, of which scarcely two are quite the same. It needs no gardener to say that a strain grown from seeds is much more satisfactory than one grown from cuttings, for there is much more vigorous growth in a seedling.

Finally, people will be won over to a strain from seed, because the greater the wealth of bloom and the longer the blooming season lasts, so much the less will be the formation of a growth suitable for cuttings.

Thus my aim must be to get a more plentiful production of seed which will germinate with more uniformity, as at present some germinate at the end of fourteen days and others not for six months.

THE CROSS-BREEDING OF PEACHES AND NECTARINES.

By H. Somers Rivers, F.R.H.S.

Having so little definite result to point out in the breeding of fruits, I feel rather an impostor in undertaking to make some remarks on the subject.

I do not mean definite result as regards new varieties of fruits which have proved of value, but as to relationship, inheritance of characters, and so forth.

The time which elapses before a seedling tree can be judged by its fruits, and the space it occupies whilst arriving at maturity, militate against experiments being carried out on a sufficiently large scale. There is no direct commercial gain to justify any great outlay in obtaining new fruits. When a new fruit is once known and of proved value, it is quickly propagated ; the raiser has no monopoly.

I propose to deal with peaches and nectarines, since it is with regard to the cross-fertilisation of these that I have the fullest data. With the older varieties raised at Sawbridgeworth, record was kept only of the seed-parent. Indeed, at first the pollen-parent was entirely disregarded, but as many varieties of peaches and nectarines were grown together in the different houses, the bees must have cross-fertilised them.

In a paper my grandfather read in 1866 at the International Horticultural Exhibition and Botanical Congress, "On Raising Peaches, Nectarines, and other Fruits from Seed," he stated that his idea forty years before was that the old varieties of fruits would reproduce themselves in an improved form if successive generations were raised from seed. Later, though the crossing was carefully effected between the two selected parents, no note was made of the pollen-parent, the offspring being recorded with the name of its mother only.

From the peach blossom which has been cross-fertilised this spring, we may confidently expect to be able to judge the result of the combination ten years hence ; if nothing untoward has happened to the seedling in the meantime, possibly a year or two sooner. The seeds do not always germinate the first year. To ensure this early fruition of one's hopes, the tree must be carefully tended and grown under glass ; by the time it is four or five years old, it takes up as much room in the orchard-house as would a pot tree which is giving a good crop of fruit and requires as much attention. Probably, after all, the new variety will have no special merit to warrant its retention, and, of no use for firewood, the tree will be burnt on the rubbish-heap.

It is a labour of Sisyphus, but the stone occasionally lodges on the top of the hill.

Darwin ("Animals and Plants under Domestication," 1868, vol. i., p. 336 *et seq.*) gives arguments for the supposition that the peach is derived from the almond. They are not, however, very conclusive. If this were so, one might reasonably suppose that amongst very numerous seedlings cases of reversion would occur. I have never noticed any but true peaches and nectarines. He goes on to say: "Whether or not the peach has proceeded from the almond, it has certainly given rise to nectarines. Most varieties, both of peaches and nectarines, reproduce themselves truly from seed." The following are the results of thirty-five crosses:

	♀		♂			
1. Six crosses of	Peach	×	Peach	resulted in	5 peaches	and 1 nectarine.
2. Eight ,,	,,	×	Nectarine	,,	5 ,,	and 3 nectarines.
3. Two ,,	Nectarine	×	Peach	,,	8 ,,	and 2 ,,
4. Eleven ,,	,,	×	Nectarine	,,		11 ,,

From this it would seem that the peach is dominant.

No. 1.—The nectarine produced by the peach-by-peach cross is interesting, since, although the parents were both peaches, the seed-parent's descent on the female side is through three nectarines, its great-great-grandmother being a peach. The pollen-parent is the result of a nectarine ♀ crossed by a peach ♂, its grandmother being a nectarine descended from two generations of peaches.

No. 2.—In six of the peach ♀ by nectarine ♂ crosses (resulting in three peaches and three nectarines), the seed-parent descended from three generations of nectarines, the first of which was a seedling from a peach. The seed-parent of the remaining two peaches had peaches for its progenitors on the distaff side for two generations.

No. 3.—The two nectarines from the nectarine ♀ by peach ♂ were derived from two separate crosses, each of which gave one peach and one nectarine. In the one case, the pollen-parent was a seedling from a peach, which came from a nectarine; in the other, the pollen-parent came from a peach, but the seed-parent's female ancestors were a nectarine, a peach, and a peach.

No. 4.—All eleven nectarine-by-nectarine crosses yielded nectarines, notwithstanding the fact that in ten of them either the seed- or pollen-parent, or both, had peaches amongst their immediate or remoter seed-parents.

Reverting to Darwin, further on (*loc. cit.* pp. 340 and 341) he gives a list of peach trees which have borne nectarines or fruits part peach and part nectarine. This is not so very uncommon: I have seen such fruits myself; but the Carclew nectarine is still, as far as I know, unique. This was "an ungrafted seedling nectarine, which, when twenty years old, bore a fruit half peach and half nectarine, and subsequently a perfect peach."

The two suggested explanations, which Darwin gives but dismisses, have neither of them been borne out here. The first is that the trees on which this bud-variation has occurred have been in every case hybrids between the peach and nectarine, and have reverted by bud-variation or by seed to one of their pure parent forms. The second is that the fruit of the peach has been directly affected by the pollen of the nectarine. On none of the numerous seedlings from peach and nectarine has one of these mixed fruits ever been noticed at Sawbridgeworth, nor has any case of xenia occurred.

The flowers of peaches and nectarines may be broadly classed as large and small. Both vary considerably in size, shape, and colour in the individual varieties.

In large-flowered peaches and nectarines some of the filaments are frequently partly petalloid. This generally occurs down one side, the anther-lobe on that side being aborted. With small flowers I have never noticed any but perfect filaments; the flowers are usually more numerous, and pollen is always very abundantly produced by small flowers, whereas in large flowers it is often scanty. When trees are forced, the anthers of large flowers are sometimes obsolete, and the filaments very stunted. As to flowers, the results of thirty-nine crosses are:

	♀		♂			
1.	Large	×	large	=	20 large,	2 small.
2.	Large	×	small	=	8 ,,	2 ,,
3.	Small	×	large	=	1 ,,	6 ,,

No. 1.—The two small-flowered trees in the large × large flowers are of different parentage. Both crosses have given two seedlings, one with large and one with small flowers; but in the one all the seed-parents in the ancestry of both sides are large-flowered, whilst in the other the mother of the pollen-parent was large-flowered, and the seed-parent came through a large-flowered mother from a small-flowered grandmother.

No. 2.—With the two small from large × small, in one case the seed-parent has a small-flowered ancestor one generation away; in the other, all the seed-parent's female ancestry are large-flowered, and the two resulting seedlings from this cross have small and large flowers respectively.

No. 3.—Lastly, the large-flowered seedling from small × large flowers is one of two seedlings from one stone; one with large, the other with small flowers. Here the seed-parent comes from a line of small-flowered female ancestors; the pollen-parent has a large-flowered mother and a small-flowered grandmother.

Lindley gives a curious case of bud-variation as regards flowers ("Guide to the Orchard and Kitchen Garden," 1831, p. 282). Hunt's Large Tawny nectarine originated with him at Catton in this way from Hunt's Small Tawny. In 1826 he noticed "a few of the maiden plants in the nursery with much larger blossoms than those on the other plants, but promiscuously intermixed among them." He thought the budders had made a mistake, but found that these flowers did not correspond with any others in his collection of peaches and nectarines. He potted two or three plants, and in 1828 forced them; "their blossoms still maintained their enlarged character, and were succeeded by fruit which differed in no other respect from the original sort than in being larger, yet ripening about the same time."

The extra-floral nectaries or glands on the petiole and at the base of the leaves of peaches and nectarines are of two shapes, either kidney or round, when the leaf is crenate, or they are entirely wanting when the leaf is rugose and deeply, often doubly, serrated.

Leaves with round or kidney glands are more susceptible to the attacks of the blister fungus, those with no glands to the mildew. Eighty crosses give the following results :

1. Kidney	× Kidney	=	9 kidney,	0 round,	1 none.
2. Kidney	× Round	=	9 ,,	4 ,,	0 ,,
3. Round	× Round	=	4 ,,	9 ,,	4 ,,
4. Round	× Kidney	=	11 ,,	12 ,,	6 ,,
5. Round	× None	=	0 ,,	2 ,,	4 ,,
6. None	× Kidney	=	0 ,,	2 ,,	1 ,,
7. None	× Round	=	0 ,,	0 ,,	2 ,,

No. 1.—The seedling without glands is from a cross in which the pollen-parent's female ancestors both have kidney glands; the seed-parent's mother has no glands. A second seedling of this cross has kidney glands.

Nos. 3 and 4.—Out of forty-six seedlings ten have no glands. In one case only, where the pollen-parent's mother had serrate leaves, do non-glandular leaves occur in the female ancestry of either parent. In this cross, R × K, the six resulting seedlings are: 3K, 2R, 1N; the three female progenitors of the seed-parent have round glands.

No. 5.—All six seedlings are the result of the same cross; the pollen-parent's mother has round glands, as have the seed-parent's two female progenitors.

No. 6.—The three seedlings from the same cross; the seed-parent's mother has round glands.

No. 7.—Both seedlings from the same cross; the seed-parent's mother has round glands; the pollen-parent's parents both kidney glands.

Seedlings seem to ripen approximately at the same time as one or other of their parents. They frequently appear to derive some characters from the one and some from the other parent indifferently; sometimes they follow one or the other alone.

A good illustration of this is the offspring of a very distinct nectarine, itself a chance seedling. This nectarine tree is peculiar in being devoid of colour. The flowers are small and white, the shoots pale green, glands kidney-shaped, and fruit with pale flesh and a white skin, ripening midseason.

This nectarine crossed with 'Noblesse' peach ♂ gave a midseason nectarine with a crimson cheek and flesh red at the stone, small pink flowers, and kidney glands. 'Noblesse' has large flowers and no glands, the fruit is without colour, flesh white to the stone. Here the colour in the seedling must have come from some ancestor.

Crossed with 'Early Rivers' nectarine ♂, which has large flowers, we have two seedlings, both nectarines; one with small pink flowers and a coloured fruit ripening early, following the pollen-parent; the other, midseason, with pure white clingstone fruit, having small white flowers. Two parallel seedlings resulted from another cross with a different nectarine as pollen-parent.

A cross of two nectarines, ‘Dryden’ ♀ (midseason, small flowers) by ‘Early Rivers’ ♂ (early, large flowers), gave two seedlings from one stone, one with large flowers ripening at the same time as the seed-parent, the other with small flowers ripening with the pollen-parent. The fruits of both seedlings resemble the seed-parent in appearance.

There appears to be no correlation between the size and colour of the fruits and the size of the flowers or the shape or absence of glands. I cannot venture to dogmatise on the above results; no doubt a larger series of recorded crosses would allow definite deductions to be made.

THE CROSS-BREEDING AND HYBRIDISATION OF PEAS AND OF HARDY FRUITS.

By W. Laxton, F.R.H.S.

The object of cross-breeding, from a practical and commercial point of view, may be described as an effort to eliminate the bad, and intensify the good characters of a plant. With this object in view we naturally select as parents two plants bearing, in a greater degree than their neighbours, the features we wish to intensify. Having made the crosses and got our seedlings up, we expect to find some bearing the desired characters in an improved form, but in actual practice we are often greatly disappointed, and feel inclined to throw them all away. We have, however, been taught by Mendel to be patient, and not to expect too much in the first generation; if Mendel has only done this he has done more good than one at first realises, for he has saved from destruction many latent improvements of future generations, and encouraged the cross-breeder to proceed with his work instead of abandoning it in disappointment and disgust. But we must not expect too much from Mendelism. The progress of improvement in annuals such as the pea will show us that real improvements are slow, and the combining of particular characters in one plant takes many years of careful work, even with a knowledge of Mendel's theories. The late Thomas Laxton, who may be said to be the follower of Knight and McLean in the cross-breeding of *Pisum*, devoted many years to working on the pea, and his principal breaks which *stand* after all these years are not many in number. His work was not conducted in the dark, as he knew long before others that the pea was a self-fertilising flower, requiring to be emasculated in the early bud state, and that breaks were not to be expected in the first generation, although he had not worked out any law as to the ratio in the second and third generations, as Mendel has done. Great as are the benefits arising from the knowledge of Mendel's law, we find that we cannot arrive at any desired result without much labour and patience, and still something must be left to chance in the *combining* of many desirable characters in *one* plant, and great quantities of crosses will have to be made before we attain the end. Mr. Bateson, Mr. Punnett, Mr. Hurst, and others have worked on most careful lines, and can tell us how many generations will have to be raised if we desire to combine, say, five or six characters which are known to be either dominant or recessive, in any particular plant.

In hardy fruits, such as the apple, pear, plum, or strawberry, we have been working with impure strains, and have had to trust to chance combinations of desirable characters, hoping by crossing from our best cross-bred seedlings to get the greatest number of breaks. Unfortunately, many of these are combinations of three or four characters with an important one missing. These constitute *failures*, as in a commercially valuable fruit we have to combine many good points. Take, for instance, an apple—(1) flavour; (2) size; (3) colour; (4) earliness or lateness; (5) cropping qualities; (6) constitution. Chance may favour the hybridist and he may hit the mark, in combining four or five out of the six, but the one "missing" character condemns the product as useless. Therefore, as we have not any guide as to these characters being either dominant or recessive in their generation, we have to make very many crosses before we succeed in combining them all in one plant. In the whole of my experience in cross-breeding, I have never been able to exactly reproduce any one of our seedlings, in all its characters, by re-effecting the *same* cross. For instance, 'Royal Sovereign' strawberry was raised by crossing 'Noble' × 'King of the Earlies,' and although this cross has been effected many times since, we have never reproduced just 'Royal Sovereign.'

The Cross-fertilisation of the Pea.

The artificial cross-fertilisation of the pea, like that of most other plants, is very easily effected. It is not the actual crossing that is expensive and laborious, but it is in the after part that the labour and expense accumulate in the sowing and selecting, and re-sowing and re-selecting, and afterwards thoroughly fixing the variety.

In practice, the bloom is opened in the very young bud stage, emasculated, and pollinated in the usual way. We do not find it necessary to protect each individual bloom from insects if the pollen is applied in the early stages, as the pistil is already well protected from outside interference by the pollen grains applied. The pea, being what is termed a self-fertiliser, is almost, if not perfectly, immune from insect interference. Hence the practicability of growing side by side various types of peas, and securing a true stock on re-sowing. The late Mr. Laxton was one of the first experimentalists to prove this, and to point the fact out to Darwin. After having effected the various crosses desired, and harvested the pods, the seed is sown in the following spring. In the first generation no deviation from the dominant seed-parent is noticed or expected. The second season the produce is again sown, and here we look for the breaks desired. If, of the parents of the cross, one is tall and one dwarf, we find nearly all the produce will be tall, but a few dwarfs and semi-dwarfs are also noticed, and are selected from the others. The appearance of a very large proportion of tall peas and a few dwarfs is satisfactorily explained by Mendel's theory, and how far this law applies to semi-dwarfs which are also found (that is, intermediate in height to the two parents) I am at present unable to follow or explain. The produce of this cross is again sown the following season. All the dwarfs, semi-dwarfs, and talls being sown separately, we find in this generation a further splitting-up of the types, dwarfs still appearing amongst the tall selections in the proportions of the second generation, but the dwarfs and semi-dwarfs do not further break away in height, but come true. Therefore, we have to grow and select each cross through at least three generations before any fixed and definite type can be secured. Afterwards, the selected seedlings, if any—in many cases they are all useless—are further sown and tested, and we find "rogues," or false peas, still appearing for many generations, mostly of a very wild or common type. It is curious to note that in the varieties of cultivated peas this wild type appears to be the same in all, and occurs continually as a "rogue." It is easily distinguished by its "vetch-like" growth and short curved pods. In further reference to Mendel's theory, we find from examination that "blunt" or "square-ended" pods are dominant; that is, if a pointed pod of the 'Duke of Albany' type is crossed with a square-ended pod of the 'Ne Plus Ultra' type, the result will give a dominant square-ended pod in Mendelian proportions. Early-ripening and late-ripening varieties crossed together do not give some earlies and some lates, and some intermediate in season, but nearly all the produce will be *late* in ripening: here again Mendel's law applies. Also if two early varieties are used in the cross, one would naturally expect the product to consist in the main of early-ripening seedlings, but in actual practice we find that this is not so; and for this reason it is necessary, if any useful results are to be obtained, to effect many crosses.

To the fact that we use impure, not fixed strains, in our crosses—that is, seedlings of the second and third generations that are not fixed types—I attribute our deviations from Mendel's law, and believe that it is from this fact of mixing impure bloods together that the greatest breaks are to be looked for. In peas, again, we do not find that the same cross effected several times gives each time the same result. For example, take 'Gradus,' which was a seedling from 'Earliest of All' ♀ × 'Duke of Albany' ♂. This cross has since been made many times, but we have not yet found a pea exactly like 'Gradus' in any of the crosses.

The attempted cross-fertilisation of *Lathyrus odoratus* with various perennial species such as *L. latifolius*, *L. grandiflorus*, *L. pubescens*, has so far proved a failure with us, the pollen only being sufficiently potent to irritate the ovary without the formation of fertile seed.

Strawberries.

Our work amongst these has extended over a great number of years, the late Mr. Laxton having received his first certificate for a new strawberry from the Royal Horticultural Society as far back as 1866. Since then we have duly effected and actually sown some 1,500 crosses. Taking an average of some twenty seedling plants from each cross, at least 30,000 seedlings have passed through our hands during the last fifteen years. The actual cross-fertilisation of the strawberry is very simple. The bloom selected is opened in the bud stage and the anthers cut out, the pollen from the selected male parent is at once applied, and again twice after the bloom has expanded. This work is conducted under glass, and all insects as far as practicable excluded by means of tiffany fastened in front of all doors, ventilators, &c. The objects to be sought in crossing and raising seedling strawberries from a commercial point of view are many amongst the chief being;

1. Constitution and vigour.
2. Flavour and quality.
3. Solidity and external firmness to adapt the fruit for transit.
4. Colour.
5. Size and appearance.
6. Fertility.

The latter points many market growers will hold as constituting the blue blood of the strawberry, while on the other hand private gardeners will put quality in the foreground, as strawberries are grown to be eaten as well as to be looked at and be sold. This adds to the necessity for procuring a sufficient number of differing varieties so as to provide for each particular requirement. We ourselves have perhaps been foolish —looking for the philosophers' stone—in seeking to blend all the desired qualities in one. I need hardly say that this happy goal has not yet been reached, and the pleasure may yet be looked forward to by other workers in the field of strawberry raising. But, to be practical, what is really wanted are early, main crop, and later varieties, having good-sized, high-flavoured fruits, with a firm exterior, the colour of a bright glossy-scarlet. The conical or heart-shaped form may perhaps find most favour, but the shape should be regular, the plant hardy, moderately vigorous and sturdy, and fairly productive of runners, a stout footstalk carrying about ten or twelve even and regular-sized fruits, held above but not far from the ground; the fruits of good and distinct flavour, it not being necessary that all should assimilate in this respect, variety being desirable to suit varying tastes; and lastly, if these qualities can be imparted to fruits suitable for forcing, a material gain would be secured.

These are the points needful in a commercially useful strawberry; but just how they are to be obtained is quite a different matter; for if only one of these essential points be wanting, the plant may have to be discarded as worthless. For instance, size and colour, without firmness and flavour, would brand the seedling as useless. The greatest breaks we have yet secured have been through using as parents selected seedlings, having most of the necessary points combined, but lacking perhaps one or two of the essential particulars enumerated above.

The 'Bedford Champion' was raised from a compound cross as follows :—

- 'Bedford Champion'
  - unnamed seedling
    - 'Noble'
      - 'Sharpless'
      - Forman's 'Excelsior'
    - 'Sir J. Paxton'
      - unknown
      - unknown
  - unnamed seedling
    - 'J. Ruskin'
      - 'Dr. Hogg'
      - 'Black Prince'
    - 'Scarlet Queen'
      - 'Noble'
        - 'Sharpless'
        - Forman's 'Excelsior'
      - 'King of the Earlies'
        - 'Black Prince'
        - 'Viscomtesse'

From this compound crossing of blood we believe the greatest breaks in this and many other fruits may be expected. It is curious to note the great variations in the seedlings from the selfsame cross, repeated several times. Out of several hundreds resulting from the cross, no two will be exactly alike.

Hybrids.

In our experiments in hybridising other hardy fruits, we have been successful in raising some rather interesting plants.

(1) We have a hybrid raised from a Japanese Plum × Peach. This gives what appears to be a combination of characters in foliage, at any rate, intermediate between the two parents, and we rather anxiously await the fruit (fig. 131).

(2) Japanese Plum × Moorpark Apricot gives us also what appears to be a combination of characters of the two species (fig. 130).

(3) Greengage Plum × Moorpark Apricot also with the combination of characters of both parents in the foliage.

The *modus operandi* in crossing the above is similar to the strawberry: that is, the anthers of the bloom to be crossed are cut out whilst yet the bloom is in the bud stage, the pollen of the selected male parent being immediately applied, and once or twice afterwards; all insects being as far as possible excluded.

In the case of stone fruits such as peaches, plums, &c., the pulp should be removed from the stone immediately the fruit is ripe, otherwise if it is allowed to remain we find the kernel inside the stone will not keep, but soon becomes mouldy and rotten. The kernel is sown at once, and germinates the following spring. It is then budded on a congenial stock, and in the following season is potted up. From the time the cross is effected to the fruiting stage takes about five or six years.

Logan-berry Crosses.

The Logan-berry, an American fruit, said to be a hybrid between the raspberry and the blackberry (which parentage we feel is doubtful), has been recrossed again with various English Raspberries in order to secure, if possible, the elimination of the hard core, so objectionable in the Logan-berry, and at the same time to improve the flavour of the fruit. In this we have entirely succeeded, having selected a seedling bearing the characters of the Logan-berry, except that the fruit is an enlarged form of 'Superlative' Raspberry.

Pyrus japonica Crosses.

We have attempted many crosses between *Pyrus japonica* and various cultivated forms of *Pyrus communis*, the object being to secure, if possible, a red-flowered pear. At present our seedlings are not old enough to bloom, but appearances suggest that the cross has been effective.

Apples and Plums.

We have made many crosses between all the best varieties, but at present have not fruited many, and as good varieties are so numerous we are resolved to destroy all that are not improvements. We find that the seedlings (the result of crossing some of our best varieties of apples) show a great tendency to revert to a "wild" and "spiny" growth resembling the crab in habit; probably this arises from the "spiny"

The Hybrid — Japanese Plum ♀ — Moorpark Apricot ♂

Fig. 130.

habit of growth being a "dominant" character, and we may find that the smaller number of better growth are true recessives in this character in the second generation. The great difficulty in following out the Mendelian characters in these hardy fruits arises from the fact that at least five years must elapse from sowing to fruiting each generation; and from the fact that fruit trees occupy much space, the expense of raising twenty or thirty seedlings from *each* seedling in the second generation to test Mendelian characters may be imagined.

The same remarks apply generally to Plums. We have, however, fruited the following crosses, and give a brief description of the results:—

Victoria × Sultan = Yellow plum, something like 'Jefferson,' of good flavour.

Sultan × Early Prolific = Shape and colour of 'Prince of Wales,' but larger, and of good flavour.

Monarch × Pershore = Small yellow plum; this looked like a good cross "on paper"; but the seedling is quite valueless.

Greengage × Sultan = Small, black plum; valueless.

Grand Duke × Czar = Large, very early, black, and of good flavour.

Finally, I must say that considering the time, labour, and expense involved, the raising of new and improved varieties is, for a nurseryman, a slow and disappointing process, and commercially unprofitable, and I shall hail with delight the time when the workers in Mendel's footsteps can direct us how to attain the desired result by a less laborious, and quicker route.

"COPYRIGHT" FOR RAISERS OF NOVELTIES.

At the fifth session of the Conference the question of "copyright" for raisers of novelties was referred to, and, at the conclusion of the session,

Mr. George Paul, V.M.H., rose and said: We ought not, I think, to separate without referring to a subject which is of vital importance to all raisers of plants, and that is the subject of protection being sought from the State for those who spend their time and money in raising novelties. Why are so many well-known growers absent from us to-day? I notice that while Mr. Arthur Paul and Mr. Rivers, who are practical raisers, have been present, we cannot help missing such men as M. Lemoine of Nancy, M. Pernet-Ducher of Lyons, Mr. Dickson of Belfast, and others. The fact is, these gentlemen do not like to tell us, or to show, what they have done in their experiments, because when once their knowledge becomes public, they have not the slightest chance of receiving any pecuniary reward for their labours. If they were properly protected from being deprived of the due reward of their labours, they would no doubt be much more willing to come forward and help us, and place their invaluable experience at our disposal. I have in my time raised a good many things, and some of you would be astonished to know the value of a fine new rose: £50 or £100 is the outside figure; and out of that has to be paid the cost of advertising and cataloguing, &c. That appears to me to be a ridiculously small return for all the risk and labour, added to the observation and experience which have taken the best years of one's life to amass. I think we should pass some resolution.

Professor Hansen: I believe, in law, a seedling is regarded as the gift of God, and it would be hard to patent that; but could we not hope to have some law fashioned which would give a bonus to the man who does such skilled and valuable work as that which has come before us over and over again during the sessions of this Conference?

The Chairman: I think it would be unwise to pass a resolution as suggested, unless we could give some indication of the way in which legislation could be brought about, and, if enacted, could be enforced. I imagine that everyone sympathises with such a change in the law, but surely our discussions to-day show what a very great difficulty there would be in enforcing such law, because we have gentlemen from all parts of the world maintaining that a thing is new, and others, equally capable, maintaining that it is old.

Mr. Geo. Paul: Take the case of roses. Some one sells me a rose for 7*s.* 6*d.* From three plants purchased at Christmas time I would guarantee to raise by next June from 200 to 500 of that variety, by certain methods of propagation. In order to get anything, the raiser *must* sell to his fellow-nurserymen. They promptly propagate, and, by the end of six months, they will sell their stock at 1*s.* each—the same price as the raiser is selling his. He has not even a limited protection. I can see no reason whatever why the raiser should not have the power to retain the sale in his own hands, and be able to bring an action at law for infringement, in the same way that one publisher can against another who pirates his productions. A certificate from the Royal Horticultural Society would meet the case, if it were made illegal for anyone, except the man who holds the certificate, to sell.

Professor Wittmack : I would like to support the suggestion of Mr. Paul. We in Germany have got such protection. But we do not go to the Government. We are protected by the laws of our Agricultural Society, which is a very great Society. A man who has bought a specimen of a novelty from the raiser, dare not himself sell it, within three years, so that the raiser may have that time in which to reap the due reward of his knowledge, skill, experience, and labour. This is done by a law of our Society. Surely a Society like the Royal Horticultural Society could do the same, and they should expel a man who did not do as they wished.

Mr. James Douglas, V.M.H. : It appears to me that protection would cut both ways. If Mr. Paul sends out a new rose, and I get it for 7*s.* 6*d.*, I, having bought it, can do what I like with it. It is my property. I, too, might send out a new rose, and Mr. Paul might buy it, and do what he liked with it. Mr. Paul propagates other people's new roses, and other people propagate his. It is perfectly fair. But protection would restrict the sale of the plant, as the people who now pay 6*d.* or 1*s.* would have to pay much more for it. Now, we do not want to restrict the sale of plants, but to increase it. I raise carnations and send out a new carnation at 3*s.* 6*d.* I see they are sold much cheaper by others next year. Well, if a man can sell cheaper, let him. I do not think you would be glad for long, even if you got legislation.

Mr. Druery : There is an enormous practical difficulty in applying any principle of copyright to plants. The case is different with regard to books. You can establish and secure a copyright by depositing a volume, and if anybody infringes that copyright you have your remedy, and you can refer to the deposited volume for proof ; but it is not so in the case of a plant. How could you possibly do it ? Many raisers may be occupied in cultivating the same class of plants, and two or three of them might get something very similar, at the same moment—so similar, in fact, as to be practically indistinguishable.

The Chairman : The point raised by Mr. Paul is a most interesting one, but there are evidently two sides to it, as to most other things, and, unless there were a very decided majority in favour of it, I do not think it would be wise for us to move in the matter.

ON THE PHYLOGENY OF ORCHIDS.

By PROFESSOR PFITZER of Stuttgart.

[EVERY member of the 1906 Conference will have grieved to hear of the death of Professor Pfitzer, which occurred soon after his return to Stuttgart. His amiability and the depth and extent of his learning were noticeable to all; and the botanical world—and the horticultural world on its more scientific side—has sustained an irreparable loss.

The following paper which he had intended sending for the Conference Report was found upon his writing-table, and was very kindly forwarded by his executors, but unfortunately not in time to be placed in its proper position in the volume. If any errors are found in it they will doubtless be due to the intense difficulty of deciphering the writing, which is all in English, the Professor having been an expert linguist, added to his other remarkable attainments.—EDITOR.]

It is always very dangerous to say anything about the phylogeny of a group of plants if there are no palæontological evidences about their ancestors. On the other hand, it can hardly be expected that these evidences will ever be found about orchids and their allied plants. Therefore we must either entirely forgo any approach to this question or we must try to solve it with the clear consciousness that our suggestions have but a very limited value. But if a man has been working for many years on one particular family of plants, it is natural that he should form some opinion on this question, so that perhaps it will be forgiven in Darwin's country if I venture to communicate to this Conference some of my ideas on the phylogeny of orchids.

Generally speaking, the order *Scitamineæ* is considered the nearest relation to the *Orchidaceæ*, and they certainly stand very near to each other. It is, however, almost impossible to suppose that the orchids have been derived from *Scitamineæ*.* The outer whorl of the perianth, so remarkably developed in orchids, is large only in *Musaceæ*, which are very dissimilar to orchids in all other respects; in the *Zingiberaceæ*, which would come nearer to this family, it is generally reduced to a short tube split on one side; and if we would derive orchids from *Zingiberaceæ* it would be necessary to suppose that the single leaves of the calyx of *Zingiber* had become free and large. Also the apparent similarity that both groups—setting aside the *Cypripedieæ*—have only one perfect stamen, is no real link between them, because the only stamen in orchids is the "unpair" of the outer whorl, in *Zingiberaceæ* the "unpair" of the inner whorl, while the one developed in orchids is entirely wanting in *Zingiberaceæ*. So the similarity in the diagram of the flower is only apparent; both families may have been derived from one common ancestor, which varied in two different ways, and gave on the one side a progeny that

* The *Scitamineæ* include such genera as *Canna, Hedychium, Maranta, Musa*, *Zingiber*.—EDITOR.

became the now existing family of *Zingiberaceæ* and on the other a progeny that developed into the now existing orchids.

I think the *Commelinaceæ* are out of the question. Not because they have a superior ovary (a character which is often over-valued in systematic arrangement), but because they are a very highly differentiated type, and differentiated in quite another way—the calyx and the corolla are quite different from each other, and not, as in most orchids, very similar. In the second place the inflorescence of *Commelinaceæ* is sympodial, not racemose as in orchids; and, if the flowers are not radiate, the plane of symmetry is an oblique one, as in *Gladiolus*. If some stamens are sterile or wanting, as in *Commelina* and *Cochliostema*, they are not at all identical with those which are suppressed in orchids, but their place is also given by the oblique symmetry of the flower. I should think it impossible to derive a plant with simply racemose flowers and a radiate symmetry, from another with the much more complicated sympodial inflorescence and oblique symmetry of the flower; on the contrary I suppose that the *Commelinaceæ* are one of the most recent and most highly differentiated types of monocotyledonous plants.

Thus it becomes necessary to regard plants similar to the *Liliaceæ* and *Amaryllidaceæ* of present times, as the common ancestors of orchids, *Scitamineæ* and *Commelinaceæ*. If we take the normal diagrams of *Amaryllidaceæ*, we have six perianth leaves and six stamens; an inferior ovary similar to orchids, but the seeds and embryos are quite apparent. The *Iridaceæ* may have been derived from the *Amaryllidaceæ* by the abortion of the inner whorl of stamens, and the *Hæmodoraceæ* by the suppression of the outer whorl.

We must now examine how far the diagram of *Amaryllidaceæ* (in some genera) comes near to that of orchids.

A symmetrical structure of the perianth, in the way that the median plane separates two equal portions, is not uncommon in the *Amaryllidaceæ*; we may take *Hippeastrum*, *Sprekelia*, *Alstrœmeria* as examples. But in the *Conanthereæ* the stamens have a tendency to become transformed into staminodes on the same side of the flower as that in which they are suppressed in orchids, in the superior half of the not resupinated flower, as is pointed out by A. Colla,* J. Miers,† and J. G. Baker.‡ In *Zephyra* only the two lateral stamens of the outer whorl are staminodial; in *Tecophilæa* the median one of the inner whorl also; in this genus only the three stamens are left which occur fertile in orchids. In the same genus the two lateral segments of the inner perianth whorl are resupinated by twisting, in so much that the perianth also is mediano-symmetrical as in orchids.

In the genus *Cyanella*, as far as the stamens are concerned, we approach nearer to orchids. In this genus, which seems to have convolute leaves, there is one species, *C. orchidiformis* Jacq., which has three posterior stamens with long filaments and short anthers, and three anterior stamens

* "Plant. rarior. in region. Chilens." *Mem. Accad. Torin.* xxxix. (1835), 19, 20, t. 55.

† "On the *Conanthereæ*," *Trans. Linn. Soc.* xxiv. (1864), 501, t. 53.

‡ "On *Colchicaceæ* and the Aberrant Tribes of *Liliaceæ*," *Journ. Linn. Soc. Bot.* xvii. (1878), 493.

with very short filaments and large anthers; and three other species with five stamens of the first kind, and one very different, much larger, declinate stamen of the latter kind, which corresponds exactly with the only fertile stamen in monandrous orchids. Miers plainly says "one fertile stamen placed anteriorly and five substerile anthers." In the description of *C. capensis* ("Bot. Mag." t. 568) the interesting remark is made that upon the fertile stamen the style is incumbent.

By combining the mediano-symmetrical flower of *Tecophilæa* with its staminodial posterior stamens and the large stamen of *Cyanella*, we have nearly the diagram of orchids; but it is also necessary to compare the other parts of the flower, and the whole plant, in the tribe of *Conanthereæ*, with the most simple orchids.

The ovary is superior, semi-superior, or nearly inferior in *Conanthereæ*. If we acknowledge that the column of orchids is only a prolongation of the axis, in which the carpels of the inferior ovary are included, there is no difficulty in seeing that the ovary of orchids may have been derived from that of *Conanthereæ*. There are numerous small anatropous ovules in this tribe, but the ovary is trilocular, as in *Apostasieæ*, and in *Phragmopedilum* only among orchids, and the seeds have a horny endosperm, which has quite disappeared in orchids.

The *Conanthereæ* have a subterranean tuber, a distinct cylindrical stem and vaginate, distichous, conduplicate, narrow or broader convolute leaves (*Cyanella*). The inflorescence is racemose, as in most plants with mediano-symmetrical flowers.

I must also mention here the little group of the *Philydraceæ*. They have a superior ovary, but the diagram of the racemose flowers is very similar to that of monandrous orchids. In *Philydrum* the placentæ do not meet in the middle of the nearly unilocular ovary, and the seeds are small and very numerous. These plants are found in the tropical parts of Asia and Australia, while the *Conanthereæ* grow now in South America and South Africa. All these parts of the globe have had no general overflowing of the sea since the Early Tertiary period, and may therefore still retain ancient types of plants.

If we are of the opinion that these plants, and the few now existing *Conanthereæ* and *Philydraceæ*, are the remainder of the connecting link between *Amaryllidaceæ* and *Orchidaceæ*, there arises the question which of the latter are the most ancient and original types.

I think that the articulation of the leaf in monocotyledonous plants is a character which points to a recent origin. We find this articulation only in the tribes of *Bambuseæ* among grasses, the highest group of this family, and in most tribes of orchids (the *Apostasieæ*, *Cypripedieæ*, *Ophrydeæ*, and *Neottieæ* excepted), and I should think they are the oldest orchids. Also most species of these tribes have granular pollen, and are terrestrial. We may suppose that the possibility of dejecting the lamina of the leaf was developed as the orchids began to grow as epiphytes, and were sometimes constrained to diminish their water-evaporating surface if they would keep themselves alive in times of drought.

Of the orchid tribes named above; the *Apostasieæ* come very near the diagram of *Tecophilæa*, the perianth is only a little symmetrical; even the prolongations on the back of the perianth-leaves, which are present in most *Conanthereæ*, are very distinct in the *Apostasieæ*, but the ovary is still trilocular and the style slender, with three fertile stigmatic lobes. We have only to cancel the three staminodes of *Tecophilæa* in order to obtain the diagram of *Neuwiedia*; if we reduce the anterior stamen to a staminode, we have the diagram of *Apostasia*; and if we suppress it entirely, that of *Adactylus*.

From a diagram like that of *Tecophilæa* we can also construct that of *Cypripedieæ*, by strengthening the symmetrical structure of the perianth, and reducing the anterior stamen to a staminode, while the three staminodes of *Tecophilæa* are entirely wanting. The American genus *Phragmipedium* still retains the trilocular ovary, while in the gerontogæous *Paphiopedium* the ovary is trilocular only at the apex, and in the northern circumpolar genus *Cypripedium* the septa have entirely disappeared.

Of the monandrous orchids the *Neottieæ* come nearest to *Conanthereæ* and *Philydraceæ*. In *Thelymitreæ* the perianth is nearly radiate; in addition to the fertile stamen there are two very large staminodes, which are also present in *Diurideæ*, but in combination with a distinctly symmetric perianth. The stamen is quite normal in these plants, the anther upright, on a short filament, and only the transformation of the "unpair" lobe of the stigma into a rostellum gives quite a new character, developed as an adaptation to the fertilisation of the flowers by insects. When a column is produced, as in *Cephalanthera*, it has been observed that it arises quite late; in a bud of 4 mm. length of *C. rubra* there is no trace of a column to be found, from which it may be concluded that the length of the column is not an old or important character.

I think the contact of the apex of the anther with the top of the rostellum, as it occurs in *Neottieæ* and in *Cyanella*, is an older type than the complicated manner in which (in *Ophrydeæ*) the pollinia develop their caudicles towards the base of the anther. On the other hand, the long stigmatic processes of *Habenaria* and allied genera come nearer to the structure of *Amaryllidaceæ* than the sessile shining stigmatic surfaces of most *Neottieæ*. Perhaps we may infer that the different development of the rostellum was the cause why the pollinia applied themselves either to its apex or to the base of the anther.

Besides, in *Apostasieæ*, *Cypripedieæ*, *Neottieæ*, and *Ophrydeæ* we do not find articulated leaves; yet in some *Lipariđeæ*, as *Malaxis*, *Microstylis*, of which the former genus with its upright anther also appears to be an old type, the pollinia are waxy without any appendix, while they are granular in *Apostasieæ*, *Cypripedieæ*, and most *Neottieæ*. The sectile pollen of some genera of the latter is very similar to the parcel-divided pollinia of *Ophrydeæ*, because we find many pollen masses only in plants highly adapted to fertilisation by insects. The granular or sectile pollinia would also be a reason for considering the above-named groups as less changed by adaptation than the rest of the orchids; we may infer that the next step was the formation of caudicles; and the last, the existence of a distinct stipes or gland which separates from the rostellum.

If we compare the morphological structure of the whole plant, we find that all orchids with granular or sectile pollen have terminal inflorescences, while in the *Epidendreæ* and *Vandeæ* of Bentham some groups have terminal, others lateral inflorescences. The whole group of *Monopodiales*, whose stems grow many years at the apex, and have only lateral inflorescences, have also a distinct gland at the base of the pollinia. I think that this is the tribe which has reached the highest degree of evolution, and is the other end of the chain which begins with *Apostasieæ*.

This monopodial type is not monophylitic. There are very few monopodial orchids in America; in the structure of their flowers the *Huntleyinæ* are very near the *Zygopetalinæ*, and may have been derived from them. The *Dichæinæ* approach the *Oncidiinæ*; the genera *Ornithocephalus* and *Lockhartia* show a distinct tendency to monopodial growth. I think the former are an undeveloped stage of the latter; also the strictly American *Pachyphylleæ* may have the same origin.

The bulk of the *Monopodiales*, the *Sarcanthinæ*, are strictly gerontogæous; they may be a higher developed branch of the *Cymbidiinæ* which are also for the most part of Malayan origin. The genera *Cyperorchis*, *Cymbidium*, and *Grammatophyllum* show a distinct tendency to pass from the sympodial mode of growth to the monopodial one.

A question which remains to be considered is, how the very small and imperfect embryos of orchids are developed from the normal embryo of monocotyledonous plants, and how the endosperm of the latter was lost.

Generally speaking, we find very imperfect embryos in a seed without any nourishing tissue (1) in parasitical plants (*Orobanche*, *Balanophoraceæ*); (2), in saprophytic plants (*Monotropa*, *Pyrola*).

The seeds of *Orobanche* do not germinate if neither the seeds nor the roots of the plant on which the *Orobanche* feeds are present. So it becomes necessary for the latter to produce an enormous quantity of seed; and the seed itself is exceedingly small, in order that very many seeds may be produced with a restricted quantity of material. Large quantities of seed perish, without the possibility of ever reaching the host-plant.

On the other hand, we know, from the researches of M. Noel Bernard, that the seeds of orchids may show the first signs of germination without symbiosis with a fungus, but that their further development depends upon the presence of this fungus (see p. 292 *et seq.*). Nearly the same difficulty arises as in *Orobanche*, and it becomes necessary that large quantities of very small seeds should be produced.

In the case of the epiphytic orchids it is also probable that the dispersal of the seeds is effected in such a way that they reach the higher branches of trees where the orchid grows, and have sufficient light for prospering. We find here either adaptations for the dispersal by birds, as in epiphytic *Aroideæ* and *Bromeliaceæ*, or these very small seeds, which are carried away by each current of air, as in orchids. Especially in these epiphytic orchids it is quite uncertain whether the seed will meet, on the branch of a tree, with the fungus which is necessary for the further development of the seedling, and the very numerous small light seeds are more profitable than the few heavy ones of *Aroideæ*, &c.

One might even put the question, whether the reduction of the seeds to such an exceedingly small size in orchids did not happen because they originally grew as epiphytes, and whether the now existing terrestrial orchids have not been derived from epiphytic forms. But I think this idea will not bear further consideration. It would be almost impossible to derive the peculiar type of *Ophrydeæ* from any epiphytic group of orchids, and it is improbable that the unarticulated leaves of the terrestrial forms arose from the articulated leaves of the epiphytes by reduction. On the contrary, it is much more likely that terrestrial orchids first arose from amaryllidaceous plants, like the now existing *Conanthereæ* and *Philydraceæ*, and that these, again, gave origin to the much more complicated epiphytic forms.

We are far from any certain knowledge of the way in which the orchids have been developed from their ancestors. But perhaps the considerations I have had the honour to communicate here may lead others to further analyse this question, and so this paper may not be quite useless.

INDEX

Nota Bene:—*In the following Index the words "Hybrid" or "Cross-bred," "Inheritance" and "Heredity" are omitted as far as possible, as one or the other of them is concerned with almost every entry.*—W. W.

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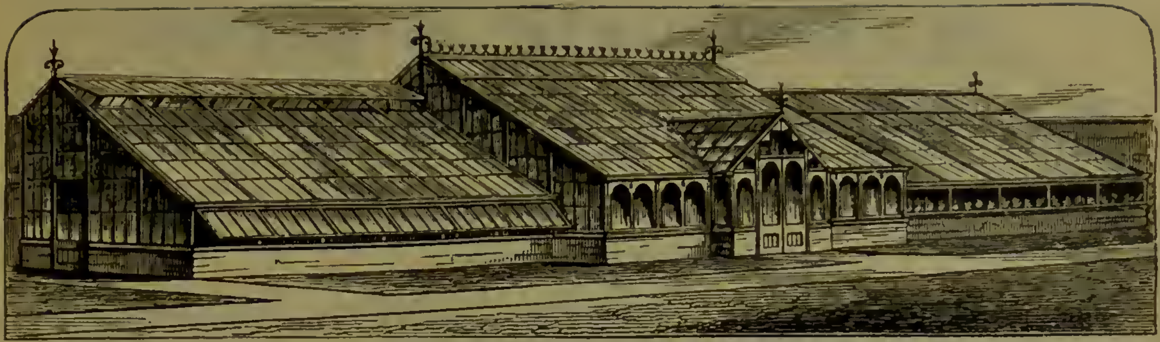

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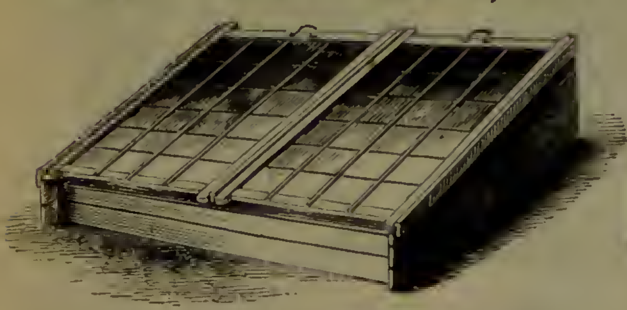

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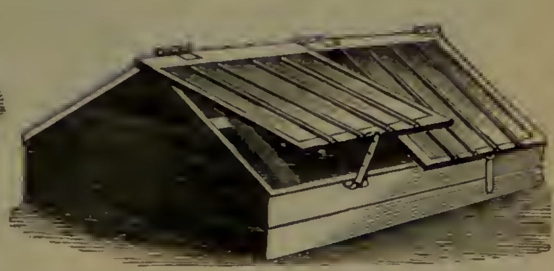

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