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TRANSACTIONS

OF

THE CLINICAL SOCIETY.

VOL. VII.
TRANSACTIONS

OF

THE CLINICAL SOCIETY

OF

LONDON.

VOLUME THE SEVENTH.

LONDON:

LONGMANS, GREEN, AND CO.

1874.
NOTICE.

The present Volume comprises the Proceedings of the Society during its Seventh Session, October 1873 to May 1874.

The Council think it proper to state that the authors of the several communications are alone responsible for the statements, reasonings, and opinions contained in their respective papers.

August 1874.
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1867 Sir Thomas Watson, Bart., M.D., F.R.S., D.C.L.

1869 Sir James Paget, Bart., F.R.S., D.C.L.

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1873 Prescott Gardner Hewett.
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Frerichs, Fried. Theod., M.D., Professor of Clinical Medicine in the University of Berlin.


Langenbeck, Bernhard von, M.D., Professor of Surgery in the University of Berlin.

Ricord, Philippe, M.D., Ex-Surgeon in Chief of the Hôpital du Midi, and late President of the Academy of Medicine, Paris.

Ziemsen, H. von, M.D., Professor of Clinical Medicine at Erlangen.
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(V.P.) Vice-President.  (S.) Secretary.
(C.) Member of Council.

Non-Resident Members who have paid the Composition Fee for the Transactions are marked thus (†).

**Elected**

<table>
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<tr>
<th>Year</th>
<th>Name</th>
<th>Address</th>
</tr>
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<tbody>
<tr>
<td>1870</td>
<td>Allbutt, Thomas Clifford, M.D., F.L.S.,</td>
<td>Lecturer on the Practice of Physic at the Leeds School of Medicine, and Physician to the Leeds General Infirmary: 38 Park Square, Leeds.</td>
</tr>
<tr>
<td>1871</td>
<td>Althaus, Julius, M.D.</td>
<td>18 Bryanston Street, Portman Square, W.</td>
</tr>
<tr>
<td>1868</td>
<td>Anderson, John Ford, M.D.</td>
<td>28 Buckland Crescent, Belsize Park, N.W.</td>
</tr>
</tbody>
</table>

**Orig Memb**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Address</th>
</tr>
</thead>
<tbody>
<tr>
<td>1872-4</td>
<td>Andrew, James, M.D.</td>
<td>Physician to, and Lecturer on Medicine at, St. Bartholomew’s Hospital; 22 Harley Street, Cavendish Square, W.</td>
</tr>
<tr>
<td>1867-9</td>
<td>Anstie, Francis Edmund, M.D.</td>
<td>Physician to, and Lecturer on Materia Medica at, the Westminster Hospital: 16 Wimpole Street, W.</td>
</tr>
<tr>
<td>1872-4</td>
<td>Arnott, Henry, M.D.</td>
<td>Assistant Surgeon to St. Thomas’s Hospital: 6 Nottingham Place, W.</td>
</tr>
<tr>
<td>1873</td>
<td>Baümler, Christian G. H., M.D.</td>
<td>Professor of Materia Medica at the University of Erlangen.</td>
</tr>
<tr>
<td>1868</td>
<td>Baker, W. Morrant</td>
<td>Assistant Surgeon and Lecturer on Physiology and General Anatomy, and Warden of the College, St. Bartholomew’s Hospital, E.C.</td>
</tr>
</tbody>
</table>
List of Members.

**Elected 1868**
Bantock, George Granville, M.D.: 44 Cornwall Road, Westbourne Park, W.

**Orig Memb**
Barclay, Andrew Whyte, M.D., Physician to, and Lecturer on Medicine at, St. George's Hospital; Medical Officer of Health for Chelsea: 23a Bruton Street, Berkeley Square, W. (C. 1870-1.)

1869 Barker, Edgar: 21 Upper Hyde Park Street, W.

**Orig Memb**
Barwell, Richard (C.), Surgeon to, and Lecturer on Descriptive and Surgical Anatomy at, the Charing Cross Hospital: 32 George Street, Hanover Square, W. (C. 1872-4.)

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Basham, William Richard, M.D., Senior Physician to, and Lecturer on Medicine at, the Westminster Hospital: 17 Chester Street, Grosvenor Place, S.W.

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1868 Beigel, Hermann, M.D.: 2 Lichtenstein Strasse, Vienna.
1871 Bennett, James Risdon, M.D., Consulting Physician to St. Thomas's Hospital, and to the City of London Hospital for Diseases of the Chest: 15 Finsbury Square, E.C.
1874 Bennett, William Henry, St. George's Hospital, Hyde Park Corner, S.W.
1870 Bloxam, John Astley, Assistant Surgeon to Charing Cross Hospital: 8 George Street, Hanover Square, W.
1868 Brace, William H., M.D.: 7 Queen's Gate Terrace, Kensington, W.
1868 Bright, John Meaburn, M.D.: Forest Hill, S.E.
1868 Bright, George Charles, M.B.: 29 Lütthchen Strasse, Dresden.

**Orig Memb**
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Elected

**Orig Memb** Bryant, Thomas, Surgeon to Guy's Hospital: 53 Upper Brook Street, Grosvenor Square, W. (C. 1872.)

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1868 *†*Burton, John M.: Lee Park, Blackheath, S.E.

1871 Butt, William F.: 12 South Street, Park Lane, W.

**Orig Memb** Buzzard, Thomas, M.D. (C), Physician to the National Hospital for the Paralysed and Epileptic: 56 Grosvenor Street, W. (S. 1870–2, C. 1873–4.)

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1868 Carr, William, M.D.: Lee Grove, Blackheath, S.E.

1869 Carter, Robert Brudenell (C.), Ophthalmic Surgeon to, and Lecturer on Ophthalmology at, St. George's Hospital; Surgeon to the Royal South London Ophthalmic Hospital: 69 Wimpole Street, W. (C. 1873–4.)

1870 Casson, John Hornsey: Ashbourne, Derbyshire.

1868 Cavafy, John, M.D., Demonstrator of Histology and Lecturer on Comparative Anatomy at St. George's Hospital Medical School; Assistant Physician to the Victoria Hospital for Children: 13 Arlington Street, Piccadilly, W.

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1873 Chisholm, Edwin: Camden, near Sydney, New South Wales.

1868 Cholmeley, William, M.D., Physician to the Great Northern Hospital, and Margaret Street Infirmary for Consumption: 63 Grosvenor Street, W. (C. 1871–3.)

**Orig Memb** Church, William Selby, M.D. (C.), Assistant Physician to, and Lecturer on Comparative Anatomy at, St. Bartholomew's Hospital: 2 Upper George Street, Bryanston Square, W. (C. 1874.)

1873 Churton, Thomas: Erith, Kent, S.E.

**Orig Memb** Clapton, Edward, M.D. (C.), Physician to, and Lecturer on Materia Medica at, St. Thomas's Hospital: 10a St. Thomas's Street, Southwark, S.E (C. 1872–1.)
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**Orig Memb**

Clark, Andrew, M.D., Physician to, and Lecturer on Medicine at, the London Hospital: 16 Cavendish Square, W.

Clarke, Jacob Lockhart, M.D., F.R.S.: 64 Harley Street, Cavendish Square, W.

Clover, Joseph Thomas: 3 Cavendish Place, Cavendish Square, W. (C. 1873.)

Cooke, Thomas, Assistant Surgeon to the Westminster Hospital: 16 Woburn Place, Bedford Square, W.C.

Cooper, Frank W.: Leytonstone, Essex.

Couper, John (C.), Surgeon to the London Hospital and Assistant Surgeon to the Royal London Ophthalmic Hospital: 80 Grosvenor Street, W. (C. 1874.)

Critchett, Anderson: 21 Harley Street, W.

Croft, John, Surgeon to St. Thomas's Hospital: 61 Brook Street, Grosvenor Square, W. (C. 1870–2.)

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Day, William Henry, M.D., Physician to the Samaritan Free Hospital for Women and Children: 10 Manchester Square, W.

De Castro, James Cato, M.B.: Pau, France.

De Morgan, Campbell, F.R.S., Senior Surgeon to, and Lecturer on Surgery at, the Middlesex Hospital: 29 Seymour Street, Portman Square, W. (C. 1867–9, V.P. 1871–3.)

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1873  Donkin, Arthur Scott, M.D.: 49, Dover Street, Piccadilly, W.

*Orig Memb*  Donkin, John Langdon H., M.D., Physician to, and Lecturer on Materia Medica and Therapeutics at, the London Hospital: 39 Welbeck Street, W. (C. 1870–2.)

1874  Dowse, Thomas Stretch, M.D.: Highgate Infirmary, N.

1868  Drage, Charles, M.D.: Hatfield, Herts.

*Orig Memb*  Duckworth, Dyce, M.D., Assistant Physician to St. Bartholomew's Hospital: 11 Grafton Street, Bond St., W.

*Orig Memb*  Duffin, Alfred B., M.D. (C.), Physician to King's College Hospital: 18 Devonshire Street, Portland Place, W. (C. 1872–4.)

1869  Duke, Olliver Thomas, Assistant Surgeon, Bengal Army, India.

*Orig Memb*  Durham, Arthur Edward, Surgeon to, and Lecturer on Anatomy at, Guy's Hospital: 82 Brook Street, W. (C. 1867-9.)

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*Orig Memb*  Erichsen, John E., Holme Professor of Clinical Surgery in University College, and Senior Surgeon to University College Hospital: 6 Cavendish Place, Cavendish Square, W. (V.P. 1869-71.)

1868  Evans, Julian, M.B., Assistant Physician, Victoria Hospital for Sick Children: 123 Finborough Road, Redcliffe Square, S.W.

*Orig Memb*  Fagge, Charles Hilton, M.D., Assistant Physician to Guy's Hospital: 11 St. Thomas's Street, Southwark, S.E.

1868  Fairbank, Frederick Royston, M.D.: Lynton, Devon.

1868  Falconer, Randle Wilbraham, M.D., Physician to the Royal United and Mineral Water Hospitals, Bath.

1872  Farquharson, Robert, M.D.: 23 Brook Street, Grosvenor Square, W.

1872  Fenwick, J. C. J., M.B.: 30 Devonshire Street, Portland Place, W.

*Orig Memb*  Ferguson, Sir William, Bart., F.R.S., Sergeant-Surgeon to H.M. the Queen; Surgeon to King's College Hospital: 16 George Street, Hanover Square, W. (V.P. 1867-70.)

1868  Fish, John Crockett, M.B.: 92 Wimpole Street, Cavendish Square, W. (C. 1869-70.)

1872  Fisher, F. R.: St. George's Hospital, S.W.
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1872 Fox, Tilbury, M.D., Physician to the Skin Department of University College Hospital: 14 Harley Street, W.

1868 Gant, Frederick James, Surgeon to the Royal Free Hospital: 16 Connaught Square, W.

**Orig Memb** Gascoyen, George Green, Surgeon to the Lock Hospital; Assistant Surgeon to, and Lecturer on Surgery at, St. Mary’s Hospital: 48 Queen Anne Street, W. (C. 1869-71.)

1868 Glover, James Grey, M.D., Hon. Surgeon to the Holloway and North Islington Dispensary: 33 Compton Terrace, Islington, N.

1869 Goodridge, Henry Frederick Augustus, M.D., Physician to the Bath Royal United Hospital: Bath.

1871 Gover, Robert M., M.B., Medical Officer to the Millbank Prison: 3 Crescent Terrace, Millbank, S.W.

1868 Green, Thomas Henry, M.D., Assistant Physician to, and Lecturer on Pathology at, the Charing Cross Hospital: 74 Wimpole Street, W.

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**Orig Memb** Greenhow, Edward Headlam, M.D., F.R.S. (Treasurer); Physician to, and Lecturer on Medicine at, the Middlesex Hospital: 14a Manchester Square, W. (T. 1867-74.)

1874 Grigg, William Chapman, M.D., Assistant Obstetric Physician to the Westminster Hospital; Medical Officer of Queen Charlotte’s Lying-in Hospital: 6 Curzon Street, Mayfair, W.


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1870 Gwynn, Edmund, M.D.: 10, Hampstead Hill Gardens, N.W.
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Habershon, Samuel Osborne, M.D., Physician to, and Lecturer on the Practice of Medicine at, Guy’s Hospital: 70 Brook Street, W. (C. 1873.)

1872

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1873

Harley, George, M.D., F.R.S.: 25 Harley Street, Cavendish Square, W.

Orig Memb

Harley, John, M.D., F.L.S., Assistant Physician to St. Thomas’s Hospital: 75 Upper Berkeley Street, Portman Square, W.

1873

Harrer, Charles, M.D.: 34 City Road, Finsbury Square, E.C.

Orig Memb

Hart, Ernest: 59 Queen Anne Street, W. (C. 1867–8.)

1869

Haward, J. Warrington, Assistant Surgeon to the Hospital for Sick Children: 5 Montagu Street, Portman Square.

Orig Memb

Hawkins, Cesar Henry, F.R.S., Sergeant-Surgeon to H.M. the Queen, and Consulting Surgeon to St. George’s Hospital: 26 Grosvenor Street, W. (V.P. 1867–8.)

1868

Hay, Thomas B.: 43 Caledonian Road, N.

Orig Memb

Heath, Christopher, Surgeon to University College Hospital, and Lecturer on Operative Surgery in University College: 9 Cavendish Place, W. (C.1867–71.)

1868

Heslop, Thomas Prexious, M.D., Physician to the Children’s Hospital, Birmingham.

1868

Hewan, Archibald, M.D.: 9 Chester Square, S.W.

Orig Memb

Hewett, Prescott Gardner (President), Surgeon Extraordinary to H.M. the Queen; Surgeon to St. George’s Hospital: 1 Chesterfield Street, Mayfair, W. (V.P. 1869–71, P. 1873.)

Orig Memb

Hewitt, Graily, M.D., Professor of Midwifery in University College, and Obstetric Physician to University College Hospital: 36 Berkeley Square, W.

Orig Memb

Hicks, J. Braxton, M.D., F.R.S., F.L.S., Physician Accoucheur to, and Lecturer on Midwifery and the Diseases of Women and Children at, Guy’s Hospital: 24 George Street, Hanover Square, W.

1868

Hill, Berkeley, M.B., Surgeon to University College Hospital, Lecturer on Operative Surgery in University College, and Surgeon for out-Patients to the Lock Hospital: 55 Wimpole Street, W. (C. 1870–1.)

1868

Hill, Thomas Harvey: 4 Stanhope Terrace, Bayswater, W.
<table>
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<th>Year</th>
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<tbody>
<tr>
<td>1868</td>
<td>Holman, Constantine, M.D.</td>
<td>Reigate, Surrey</td>
</tr>
<tr>
<td>1868</td>
<td>Holman, William Henry, M.B.</td>
<td>68 Adelaide Road South, Hampstead, N.W.</td>
</tr>
<tr>
<td>1873</td>
<td>Hope, William, M.D.</td>
<td>5 Bolton Row, Mayfair, W.</td>
</tr>
<tr>
<td>1871</td>
<td>Houghton, Henry G., L.K.Q.C.P. Ireland</td>
<td>6 Mount Street, Grosvenor Square, W.</td>
</tr>
<tr>
<td>1873</td>
<td>Hunt, Ezra</td>
<td>18 Belmont, Bath</td>
</tr>
<tr>
<td>1871</td>
<td>Hutchinson, Jonathan, Surgeon to, and Lecturer on Surgery at, the London Hospital; Surgeon to the Hospital for Diseases of the Skin, and Surgeon to the Royal London Ophthalmic Hospital: 4 Finsbury Circus, E.C. (C. 1867-8.)</td>
<td></td>
</tr>
<tr>
<td>1873</td>
<td>Jenner, Sir William, Bart., M.D., D.C.L., F.R.S., Physician in Ordinary to H.M. the Queen and to H.R.H. the Prince of Wales; Physician to University College Hospital: 63 Brook Street, W. (V.P. 1867-70.)</td>
<td>19 Cavendish Place, W.</td>
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<tr>
<td>JOHNSON, George, M.D., F.R.S. (V.P.), Physician to King's College Hospital, and Professor of the Principles and Practice of Medicine in King's College: 11 Savile Row, W. (V.P. 1874.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>JONES, Sydney, M.B., Surgeon to, and Lecturer on Surgery at, St. Thomas's Hospital: 10b St. Thomas's Street, Southwark, S.E. (C. 1867–8.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>JONES, Thomas, M.D., Assistant Physician, Victoria Hospital for Sick Children: 19 Chapel Street, Belgrave Square, S.W.</td>
<td></td>
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</tr>
<tr>
<td>KELLY, Charles, M.D., Medical Officer of Health for the West Sussex District: Horsham, Sussex.</td>
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<tr>
<td>KESTEVEN, William B.: 401 Holloway Road, N. (C. 1870–2.)</td>
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<tr>
<td>LACY, C. de Lacy, House Physician, St. George's Hospital.</td>
<td></td>
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<tr>
<td>LANGTON, John, Assistant Surgeon to, and Demonstrator of Anatomy at, St. Bartholomew's Hospital: 18 Harley Street, W.</td>
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<tr>
<td>LAWRENCE, James E.: High Street, Wandsworth, S.W.</td>
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</tr>
<tr>
<td>LAWSON, George (C.), Surgeon to, and Lecturer on Practical Surgery at, the Middlesex Hospital, and Surgeon to the Royal London Ophthalmic Hospital: 12 Harley Street, W. (S. 1871–3, C. 1874.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LAWSON, Henry, M.D., Assistant Physician to, and Lecturer on Physiology at, St. Mary's Hospital: 10 George Street, Hanover Square, W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LEACH, Harry, Resident Medical Officer, Seamen's Hospital, Greenwich, S.E.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LEE, Henry, Surgeon to, and Lecturer on Pathology at, St. George's Hospital: 9 Savile Row, W. (V.P. 1870–2.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LEE, Robert James, M.D., Assistant Obstetric Physician to St. George's Hospital: 4, Savile Row, W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LITTLE, Louis Stromeyer: China.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LIVEING, Robert, M.D., Physician to the Middlesex Hospital: 11 Manchester Square, W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>MACKENZIE, Morell, M.D., Assistant Physician to, and Lecturer on Diseases of the Throat at, the London Hospital, and Physician to the Hospital for Diseases of the Throat: 13 Weymouth Street, Portland Place, W.</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
### List of Members

**Elected**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Occupation</th>
</tr>
</thead>
<tbody>
<tr>
<td>1871</td>
<td>MacCORMAC, WILLIAM</td>
<td>Senior Assistant Surgeon to St. Thomas's Hospital: 13 Harley Street, W.</td>
</tr>
<tr>
<td>1874</td>
<td>MAHOMED (FRED. AKBAR)</td>
<td>Resident Medical Officer, London Fever Hospital, Liverpool Road, N.</td>
</tr>
</tbody>
</table>

**Orig Memb**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Occupation</th>
</tr>
</thead>
<tbody>
<tr>
<td>1868</td>
<td>MARCET, WILLIAM</td>
<td>M.D., F.R.S.: 1 Place Massena, Nice. (C. 1867–9.)</td>
</tr>
<tr>
<td>1868</td>
<td>MARSH, F. HOWARD</td>
<td>Demonstrator of Anatomy at St. Bartholomew's Hospital: 36 Bruton Street, Berkeley Square, W.</td>
</tr>
</tbody>
</table>

**Orig Memb**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Occupation</th>
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</thead>
<tbody>
<tr>
<td>1868</td>
<td>MAUNDER, CHARLES F.</td>
<td>Surgeon to, and Lecturer on Clinical Surgery, and Demonstrator of Operative Surgery at, the London Hospital: 16 Queen Anne Street, W. (C. 1870–1.)</td>
</tr>
<tr>
<td>1868</td>
<td>MAY, EDWARD HOOPER</td>
<td>M.D.: High Cross, Tottenham, Middlesex, N.</td>
</tr>
<tr>
<td>1868</td>
<td>MEADOWS, ALFRED</td>
<td>M.D. (C.), Physician Accoucheur to, and Lecturer on Midwifery at, St. Mary's Hospital: 27 George Street, Hanover Square, W. (C. 1871–4.)</td>
</tr>
<tr>
<td>1873</td>
<td>MICKLE, WILLIAM JULIUS</td>
<td>M.D.: Grove Hall, Bow, E.</td>
</tr>
<tr>
<td>1874</td>
<td>MORGAN, JOHN HAMMOND</td>
<td>3 Sussex Place, Hyde Park, W.</td>
</tr>
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**Orig Memb**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Occupation</th>
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</thead>
<tbody>
<tr>
<td>1874</td>
<td>MOXON, WALTER</td>
<td>M.D., F.L.S. (C.), Physician to, and Lecturer on Pathology and Demonstrator of Morbid Anatomy at, Guy's Hospital: 6 Finsbury Circus, E.C. (C. 1874.)</td>
</tr>
<tr>
<td>1871</td>
<td>MURCHISON, CHARLES</td>
<td>M.D., F.R.S., LL.D., Physician to, and Lecturer on Medicine at, St. Thomas's Hospital, Consulting Physician to the London Fever Hospital: 79 Wimpole Street, W. (C. 1867–9.)</td>
</tr>
<tr>
<td>1871</td>
<td>MURRAY, JOHN</td>
<td>M.D., Inspector-General of Hospitals: 17 Westbourne Square, W.</td>
</tr>
<tr>
<td>1868</td>
<td>MYERS, ARTHUR BOWEN RICHARDS</td>
<td>Assistant Surgeon to the Coldstream Guards: Vincent Square, Westminster, S.W.</td>
</tr>
<tr>
<td>1873</td>
<td>MYRTLE, ANDREW S.</td>
<td>M.D.: Harrogate.</td>
</tr>
</tbody>
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**Orig Memb**

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Occupation</th>
</tr>
</thead>
<tbody>
<tr>
<td>1874</td>
<td>NORTON, ARTHUR TREHERNE</td>
<td>(C.), Assistant Surgeon to, and Lecturer on Anatomy at, St. Mary's Hospital: 6 Wimpole Street, W. (C. 1874.)</td>
</tr>
<tr>
<td>1874</td>
<td>NUNN, THOMAS WILLIAM</td>
<td>(C.), Surgeon to the Middlesex Hospital: 8 Stratford Place, Oxford Street, W. (C. 1873–4.)</td>
</tr>
<tr>
<td>1867–8</td>
<td>OGLE, JOHN WILLIAM</td>
<td>M.D., Physician to, and Lecturer on Pathology at, St. George's Hospital: 30 Cavendish Square, W. (C. 1867–8.)</td>
</tr>
</tbody>
</table>
List of Members.

Elected
1868 †Ogle, William, M.D., Physician to the Derbyshire General Infirmary: 98 Friar Gate, Derby.
1869 Oldfield, E., M.D.: Surinam.
1868 Oppert, Francis, M.D.: Germany.

Orig Memb
Paget, Sir James, Bart., D.C.L., F.R.S., Sergeant-Surgeon Extraordinary to H.M. the Queen; Surgeon in Ordinary to H.R.H. the Prince of Wales; Consulting Surgeon to St. Bartholomew's Hospital: 1 Harewood Place, Hanover Square, W. (V.P. 1867–8, P. 1869–70.)

1873 Parker, Robert William, House Surgeon to the Hospital for Sick Children: 49 Great Ormond Street, W.C.

Orig Memb
Pavy, Frederick William, M.D., F.R.S., Physician to, and Lecturer on Physiology at, Guy's Hospital: 35 Grosvenor Street, W. (C. 1869–71.)

Orig Memb
Peacock, Thomas Bevill, M.D., Physician to St. Thomas's Hospital, Consulting Physician to the City of London Hospital for Diseases of the Chest: 20 Finsbury Circus, E.C. (C. 1867–8, V.P. 1869–71.)


Orig Memb
Pick, Thomas Pickering (Hon. Secretary), Assistant Surgeon to St. George's Hospital; Surgeon to the Belgrave Hospital for Children: 7 South Eaton Place, Eaton Square, S.W. (S. 1874.)

1871 †Playne, Alfred, M.B.: Maidenhead.

Orig Memb
Pollock, A. Julius, M.D., Physician to the Charing Cross Hospital; Physician to the Foundling Hospital: 85 Harley Street, Cavendish Square, W.

1868 Pollock, James Edward, M.D., Physician to the Hospital for Consumption and Diseases of the Chest: 52 Upper Brook Street, Grosvenor Square, W.

1871 Poore, George Vivian, M.B., Assistant Physician to, and Lecturer on Forensic Medicine at, the Charing Cross Hospital: 30 Wimpole Street, W.

1873 Port, Heinrich, M.D., Assistant Physician to the German Hospital: 10 Finsbury Place North, E.C.

Orig Memb
Powell, R. Douglas, M.D. (C), Assistant Physician to Charing Cross Hospital; Assistant Physician to the Hospital for Consumption and Diseases of the Chest: 15 Henrietta Street, Cavendish Square, W. (C. 1874.)

1868 Prentis, Charles, Assistant Surgeon, Bengal Army.

Orig Memb
Quain, Richard, M.D., F.R.S., Physician to the Hospital for Consumption and Diseases of the Chest: 67 Harley Street, W. (C. 1867–9.)
List of Members.

Elected

Orig Memb  Ramskill, J. Spence, M.D., Physician to, and Lecturer on Medicine at, the London Hospital; Senior Physician to the National Hospital for the Paralysed and Epileptic: 5 St. Helen's Place, Bishopsgate Street, E.C.

1873  Ransford, Gifford: 27 Gloucester Place, Hyde Park.

1868  Rasch, Adolphus A., M.D., Honorary Physician to the German Hospital Eastern Dispensary: 7 South Street, Finsbury Square, E.C.

1874  Ree, Frederick, Obstetric Assistant, St. George's Hospital, Hyde Park Corner, S.W.

Orig Memb  Rees, George Owen, M.D., F.R.S., Consulting Physician to Guy's Hospital: 26 Albemarle Street, W. (V.P. 1871-3.)

1868  Reeves, Henry A., Assistant Surgeon to the London Hospital: 27a Finsbury Square, E.C.

1868  Rendle, James D., M.D., Medical Officer to the Government Convict Prison, Brixton: Park Hill, Clapham Park, S.W. (C. 1869-70.)

Orig Memb  Reynolds, John Russell, M.D., F.R.S., Examiner in Medicine at the University of London; Professor of the Principles and Practice of Medicine in University College; Physician to University College Hospital: 38 Grosvenor Street, W. (C. 1867-8.)

1868  Rice, Michael W., M.D.: 8 Sloane Terrace, Sloane Street, S.W.

Orig Memb  Ringer, Sydney, M.D., Professor of Materia Medica in University College, and Physician to University College Hospital: 15 Cavendish Place, W. (C. 1871-2.)

1873  †Roberts, David Lloyd, M.D., Surgeon to St. Mary's Hospital, Manchester: 23 John Street, Manchester.

Orig Memb  Rouse, James, Assistant Surgeon to St. George's Hospital, and to the Royal Ophthalmic Hospital, Charing Cross; Lecturer on Anatomy at St. George's Hospital: 2 Wilton Street, Grosvenor Place, S.W.


1868  Sanderson, Hugh James, M.D.: 26 Upper Berkeley Street, W.

Orig Memb  Sanderson, John Burdon, M.D., F.R.S., Professor of Practical Physiology in University College: 49 Queen Anne Street, W. (S. 1867-9, C. 1870, V.P. 1871-3.)

1873  Savage, George Henry, M.D.: Bethlehem Royal Hospital, S.E.

1869  Sedgwick, Leonard William, M.D.: 2 Gloucester Terrace, Hyde Park, W.
List of Members.


Sibson, Francis, M.D., F.R.S., Consulting Physician to St. Mary's Hospital: 59 Brook Street, W. (C. 1867-70.)

1868 Simms, Frederick, M.B.: 13 Albert Mansions, Victoria Street, S.W.

Simon, John, D.C.L., F.R.S., Surgeon to St. Thomas's Hospital; Medical Officer to H.M. most Honourable Privy Council: 40 Kensington Square, W. (V.P. 1867-70.)

1873 Simpson, George M., M.D., C.M.: Hampstead Lane, Highgate.

1872 Slight, George, M.D.: 25 Brewer Street, Regent Street, W.

1868 Smith, Heywood, M.D., Physician to the Hospital for Women: 2 Portugal Street, Grosvenor Square, W.

1868 Smith, Protheroe, M.D., Physician to the Hospital for Women: 42 Park Street, Grosvenor Square, W.

Smith, Thomas, Surgeon to, and Lecturer on Anatomy at, St. Bartholomew's Hospital, and Surgeon to the Hospital for Sick Children: 5 Stratford Place, Oxford Street, W. (C. 1869-71.)

1873. Smith, William Johnson, Surgeon to the Seamen's Hospital, Greenwich.

1873 Smith, William Wilberforce: 20 Bishop's Road, Paddington, W.


Southey, Reginald, M.D. (Hon. Secretary), Physician to, and Lecturer on Forensic Medicine and Hygiene at, St. Bartholomew's Hospital: 6 Harley Street, Cavendish Square, W. (C. 1867-70, S. 1873.)

1868 Squarey, Charles Edward, M.B., Assistant Physician to the Hospital for Women: 13 Upper Wimpole Street, W.

Stephen, Andrew, M.D.: 58 Queen's Gate, Kensington, S.W.

Stewart, Alexander Patrick, M.D. (V.P.), Consulting Physician to the Middlesex Hospital: 75 Grosvenor Street, W. (V.P. 1872-4.)

1871 Stewart, William E.: 12 Weymouth Street, Portland Place, W.

1874 Stirling, Edward C., M.B., House Surgeon to St. George's Hospital: 34 Queen's Gardens, Bayswater, W.
List of Members.

**Elected**

1872  SUTHERLAND, HENRY, M.D., Lecturer on Insanity, Westminster Hospital: 6 Richmond Terrace, Whitehall, S.W.

1868  SUTRO, SIGISMUND, M.D., Senior Physician to the German Hospital: 37a Finsbury Square, E.C.

**Orig Memb**

SUTTON, HENRY GAVEN, M.B., Assistant Physician to, and Lecturer on Pathology at, the London Hospital; and Assistant Physician to the City of London Hospital for Diseases of the Chest: 9 Finsbury Square, E.C.

1868  TATHAM, JOHN, M.D., Assistant Physician to the Hospital for Consumption and Diseases of the Chest: 1 Wilton Place, Knightsbridge, S.W.

**Orig Memb**

TEEVAN, WILLIAM F., Surgeon to the West London Hospital: 10 Portman Square, W.

**Orig Memb**

THOMPSON, EDMUND SYMES, M.D., Physician to the Hospital for Consumption and Diseases of the Chest; Gresham Professor of Medicine: 3 Upper George Street, Portman Square, W.

**Orig Memb**

THOMPSON, Sir HENRY, Knt., Surgeon Extraordinary to H.M. the King of the Belgians; Professor of Clinical Surgery in University College, and Surgeon to University College Hospital: 35 Wimpole Street, W. (C. 1867–8.)

**Orig Memb**

THOMPSON, HENRY, M.D., Fellow of St. John’s College, Cambridge; Senior Physician to the Middlesex Hospital: 53 Queen Anne Street, W.

1869  VERNON, BOWATER J., Ophthalmic Surgeon to St. Bartholomew’s Hospital: 44a Wimpole Street, W.

1868  WAGSTAFFE, WILLIAM WARWICK, Resident Assistant Surgeon to St. Thomas’s Hospital: Albert Embankment, Westminster Bridge, S.E.

1869  WALKER, JOSEPH, Dental Surgeon to the Westminster Hospital: 22 Grosvenor Street, W.

1870  WARWICK, RICHARD ARCHER, M.D., Surgeon to the Richmond Infirmary: 5 Hill Rise, Richmond, S.W.
List of Members.

Elected
1868 Watkins, Edwin T., M.D.: 61 Guilford Street, W.C.

Orig Memb
Watson, Sir Thomas, Bart., M.D., D.C.L., LL.D., F.R.S., Physician in Ordinary to H.M. the Queen; Consulting Physician to King's College Hospital: 16 Henrietta Street, Cavendish Square, W. (P. 1867–8.)

Orig Memb
Watson, William Spencer, M.B., Surgeon to the Royal South London and to the Central London Ophthalmic Hospitals: 7 Henrietta Street, Cavendish Square, W.

Orig Memb
Watson, Sir Thomas, Bart., M.D., D.C.L., LL.D., F.R.S., Physician in Ordinary to H.M. the Queen; Consulting Physician to King's College Hospital: 7 Henrietta Street, Cavendish Square, W. (P. 1867–8.)

Orig Memb
Weber, Hermann, M.D. (V.P.), Physician to the German Hospital: 10 Grosvenor Street, W. (C. 1867–71, V.P. 1873–4.)

1869 Wells, J. Soelberg, M.D., Professor of Ophthalmology at King's College; Ophthalmic Surgeon to King's College Hospital; and Assistant Surgeon to the Royal London Ophthalmic Hospital, Moorfields: 16 Savile Row, W.

1868 Wells, Thomas Spencer, Surgeon in Ordinary to H.M.'s Household; Surgeon to the Samaritan Free Hospital: 3 Upper Grosvenor Street, W. (C. 1873.)

1874 Wheelhouse, Claudius Galen, Senior Surgeon to the Leeds General Infirmary, and Lecturer on Surgery, Leeds Medical School: Hilary Place, Leeds.

1868 Whipham, Thomas Tillyer, M.B., Assistant Physician to, and Curator of Museum at, St. George's Hospital: 37 Green Street, Grosvenor Square, W.

1874 Whistler, W.M., M.D.: 80a Brook Street, W.
1871 Wight, George, M.B., C.M.: 428 Liverpool Road, N.

Orig Memb
Wilks, Samuel, M.D., F.R.S., Physician to, and Lecturer on Medicine at, Guy's Hospital: 77 Grosvenor Street, W. (C. 1871–2.)

Orig Memb
Willett, Alfred (C.), Assistant Surgeon to St. Bartholomew's Hospital: 36 Wimpole Street, W. (C. 1872–4.)

Orig Memb
Williams, Charles James Blasius, M.D., F.R.S., Consulting Physician to the Hospital for Consumption and Diseases of the Chest: 49 Upper Brook Street, W. (V.P. 1867–70.)

Orig Memb
Williams, Charles Theodore, M.D., Physician to the Hospital for Consumption and Diseases of the Chest: 78 Park Street, Grosvenor Square, W.

1870 Williams, William Rhys, M.D., Lecturer on Mental Diseases at St. Thomas's Hospital: Bethlem Royal Hospital, Lambeth Road, S.E.

Orig Memb
Willis, Francis, M.D.: Braceborough, Stamford.

Vol. VII.
Elected
1868 Wiltshire, Alfred, M.D.: Assistant Physician-Acoucheur, St. Mary's Hospital: 57 Wimpole Street, W.
1869 Wolff, Abraham, Surgeon to the Jews' Deaf and Dumb Home: 48 Gloucester Gardens, Hyde Park, W.
1872 Yeo, J. Burney, M.D., Assistant Physician to King's College Hospital, and to the Brompton Hospital for Consumption: 60 St. James's Street, Piccadilly, S.W.
## CLINICAL SOCIETY OF LONDON.—BALANCE SHEET, 1872–3.

<table>
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<tr>
<th>1872</th>
<th>£</th>
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<tr>
<td>Balance from last account</td>
<td>65</td>
<td>12</td>
<td>8</td>
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<tr>
<td>Arrears of subscriptions for 1870–1</td>
<td>1</td>
<td>1</td>
<td>0</td>
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<tr>
<td>Arrears of subscriptions for 1871–2</td>
<td>2</td>
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<td>0</td>
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<tr>
<td>Subscriptions for 1872–3</td>
<td>203</td>
<td>14</td>
<td>0</td>
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<tr>
<td>Admission fees from new members</td>
<td>25</td>
<td>4</td>
<td>0</td>
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<tr>
<td>Sale of 'Transactions'</td>
<td>19</td>
<td>16</td>
<td>8</td>
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<tr>
<td>Dividend on £216 10s. 2d. 3 per cent. consols</td>
<td>6</td>
<td>8</td>
<td>1</td>
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| Amount of stock standing in the names of the Trustees, Jan. 1, 1873, £216 10s. 2d. |
| Unpaid arrears of subscriptions, £2 2s. 9d. | £323 18 5 |

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<tr>
<th>1873</th>
<th>£</th>
<th>s.</th>
<th>d.</th>
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<tr>
<td>By cost of printing and binding Vol. VI. of 'Transactions'</td>
<td>89</td>
<td>18</td>
<td>0</td>
</tr>
<tr>
<td>Medico-Chirurgical Society for use of rooms</td>
<td>47</td>
<td>5</td>
<td>0</td>
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<tr>
<td>Refreshments and attendance</td>
<td>31</td>
<td>10</td>
<td>0</td>
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<tr>
<td>Printing, stationery, postage, &amp;c.</td>
<td>14</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>Collecting subscriptions</td>
<td>8</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Messrs. Longmans &amp; Co.'s commission and cost of advertising</td>
<td>6</td>
<td>15</td>
<td>9</td>
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| Balance due from the Treasurer | 126 | 3 | 2 |

| £323 18 5 |

E. HEADLAM GREENHOW, Treasurer. Audited and found to be correct, R. DOUGLAS POWELL, William Hope, R. REGINALD SOUTHEY, Hon. Sec.
CLINICAL SOCIETY OF LONDON.

RULES.

OBJECT AND CONSTITUTION OF THE SOCIETY.

I. The Clinical Society of London is instituted for the cultivation and promotion of the study of Practical Medicine and Surgery, by the collection of Reports of cases of interest, especially of such as bear upon undetermined questions in Pathology or Therapeutics.

II. The Society shall consist of Ordinary and Honorary Members.

III. All legally qualified Medical Practitioners shall be eligible for nomination as members.

IV. The officers of the Society shall be elected from among the ordinary members, and shall consist of a President, four or more Vice-Presidents, a Treasurer, and two Secretaries, who with twenty other members shall constitute the Council.

ELECTION AND ADMISSION OF MEMBERS.

V. Candidates shall be proposed in writing by three members, from their personal knowledge.

VI. Proposal papers, duly signed according to Rule V., shall be suspended for one meeting exclusive of that at which they are presented; the ballot taking place at the third meeting. No person shall be declared to be elected unless he shall have the votes of two-thirds of the members present in his favour; not less than fifteen being present and recording their votes.

VII. All new-elected ordinary members must be admitted by the President, or by some member acting for him, on or before the fourth meeting after their election, unless further time be granted by the Council.

FORM OF ADMISSION.

VIII. 'By the authority, and in the name, of the Clinical Society of London, I admit you a member thereof.'

ELECTION OF HONORARY MEMBERS.

IX. The Council shall have the power of proposing men of distinction in Medicine or Surgery for election as Honorary Members. The election of such members shall be conducted in the same manner as that of ordinary members, and they shall have the privilege of attending the ordinary meetings of the Society.
RETIREMENT OF EXPULSION OR MEMBERS.

X. Any member may retire from the Society after signifying his intention in writing to the President, and paying whatever contribution may be due from him to the funds of the Society.

XI. A member who has retired from the Society, and wishes to rejoin it, must be proposed and balloted for in accordance with Rules V., VI., and VII.

XII. The expulsion of a member can take place only at a general meeting of the Society, specially convened for the purpose; two-thirds at least of the members present voting for the expulsion, and not less than fifteen members recording their votes. Of such meeting the Council shall give at least fourteen days' notice in writing to all the members of the Society.

CONTRIBUTIONS OF MEMBERS.

XIII. Every ordinary member when elected shall pay in advance an admission fee of two guineas, and shall not be required to pay any further subscription for the session during which he has been elected.

XIV. Every resident ordinary member shall pay, in advance, in the month of October in each year, an annual subscription of one guinea; but members elected at any meeting subsequent to the first meeting in April of any year, shall be exempt from paying any subscription for the next following session.

XV. Any member whose subscription shall not be paid within twelve months after it has become due shall cease to be a member of the Society.

XVI. Ordinary members, residing more than six miles from Charing Cross at the time of their election, shall pay an entrance fee of two guineas, which shall exempt them from any further payment while they continue non-resident.

XVII. Ordinary resident members, on becoming non-resident, shall cease to be called on for the annual contribution while they continue non-resident.

XVIII. Ordinary members of the Society may compound for the entrance fee and annual subscription for life by a payment of fifteen guineas made in advance.

ELECTION OF OFFICERS AND COUNCIL.

XIX. All the ordinary members shall be summoned by letter to the Annual Meeting in January, notice being given a week beforehand, and the hour of meeting being stated.

XX. The President, Vice-Presidents, Treasurer, and Secretaries, shall be elected by ballot at each annual meeting.

XXI. One-third of the members of the Council shall be replaced at each annual meeting by an equal number of members chosen from the Society at large.

XXII. Balloting lists of the members recommended by the Council to fill the vacant offices shall be prepared by the Secretaries, and
Rules.

transmitted by post, together with the summons to the annual meeting, to every ordinary member of the Society.

XXIII. The Secretaries shall receive the lists during the first hour of the annual general meeting; at the end of the hour they shall be delivered by the President to two scrutineers, who shall report the result to the meeting.

XXIV. In the event of equality of suffrage the President shall determine by lot.

XXV. The Council shall have the power of filling up any vacancies which may occur in any of the offices of the Society between one annual meeting and another.

THE PRESIDENT AND VICE-PRESIDENTS.

XXVI. The President shall regulate all the proceedings of the Society and Council, state and put questions, interpret the application of the Bye Laws, and decide every doubtful point. He shall check irregularities and enforce the observance of the laws. He shall sign the Minutes of General and Council Meetings, and return the thanks of the Society to the authors of communications.

XXVII. In the absence of the President, one of the Vice-Presidents, the Treasurer, or some other member chosen by the Council, shall perform his duties.

THE SECRETARIES.

XXVIII. The Secretaries shall have the management of the correspondence of the Society and Council.

XXIX. The Secretaries shall attend all the meetings of the Society and Council. They shall take minutes at each meeting, which they shall read at the following meeting. They shall notify the election of candidates. They shall read cases which are forwarded to the Society by persons who are not members thereof.

XXX. The Secretaries shall keep a Register of all the cases read at the ordinary meetings of the Society, and shall receive and have charge of all papers intended for publication in the Society’s Transactions.

THE TREASURER.

XXXI. The Treasurer shall receive all money due to the Society, and shall make all payments which may be ordered by the Council; keeping a particular account of such receipts and payments.

XXXII. The Treasurer shall keep a printed receipt-book for annual subscriptions. Each receipt shall be filled up with the name of the payer, the date, and the session for which the subscription is paid. Each receipt shall be signed by the Treasurer and countersigned by the Collector, who shall, when he gives the receipt, enter and sign a copy of the particulars on the counterfoil of the receipt-book.

XXXIII. The President, one of the Secretaries, and two members of the Society, nominated by the President at some meeting of the Society previous to the annual meeting, shall form a Committee to audit the Treasurer’s accounts.
XXXIV. The Audit Committee shall present a written Report to the Society at the annual meeting, and shall be prepared to answer any questions regarding the state of the funds of the Society.

THE COUNCIL.

XXXV. The Council shall have the management of the affairs of the Society.

XXXVI. The Council shall meet on the second Friday in every month during the session, or oftener should they see occasion, at half-past 7 p.m. precisely. Three shall form a quorum. Notice of all extraordinary meetings shall be transmitted by the Secretaries to every member of the Council.

XXXVII. The President or any three members of Council may call an extraordinary meeting of the Council.

XXXVIII. The Council shall determine questions by vote, or by ballot if demanded; the President having, in both cases, a casting vote.

XXXIX. The Council shall deliberate and decide upon all questions relating to the publication of the Society's Transactions.

XL. The Council shall, from time to time, appoint such Committees, make such regulations, and issue such orders, as shall appear to them conducive to the good government of the Society and to the proper management of its concerns.

TRANSACTIONS.

XLI. The Transactions of the Society shall be published at such times and in such form as the Council may direct.

XLII. The Transactions shall be presented to all ordinary members of the Society who shall have paid their annual subscriptions.

XLIII. Every non-resident member of the Society elected prior to January 9th, 1874, shall be entitled either to purchase the Transactions of the Society at prime cost, or, on payment of a composition fee of two guineas, to have delivered to him at any place in the United Kingdom, without further expense, a copy of every volume of the Society's Transactions which may be published subsequently to such payment.

XLIV. Every non-resident member of the Society elected subsequently to January 9th, 1874, shall be entitled, on payment of the sum of three guineas in addition to the usual admission fee, to have delivered to him at any place in the United Kingdom, without further expense, one copy of every volume of the Society's Transactions which may be published subsequently to such payment.

XLV. Every resident member of the Society who may become non-resident shall be entitled, on payment of the sum of three guineas, to have delivered to him at any place in the United Kingdom, without further expense, one copy of every volume of the Society's Transactions which may be published subsequent to such payment.

ORDINARY MEETINGS OF THE SOCIETY.

XLVI. The Ordinary Meetings of the Society shall be held on the second and fourth Fridays in each month, from the second Friday in
October to the fourth Friday in May of each year, at half-past 8 p.m. precisely.

XLVII. Each member may introduce two visitors on writing their names in a book kept for the purpose. The same visitor shall not be introduced more than three times during one session.

XLVIII. The business of the ordinary meetings shall consist in the receiving of communications of two classes; those of the first class relating to cases of which the records are complete, those of the second class to cases still under observation.

XLIX. As regards communications of the first class, a written report of the case or cases must be forwarded to one of the Secretaries a week before the day of the meeting at which the communication is intended to be made. The report of each case must comprise a complete record of the state of the patient when first observed, investigated according to the most approved clinical methods; a statement of the family and personal medical history of the patient, including a narrative of the present illness previous to the patient's coming under observation; a record of the state of the patient when last seen, or in fatal cases a record of the post-mortem examination, together with an abstract of the progress and treatment of the case since first observed.

L. As regards communications of the second class, a brief statement of the principal facts of the case shall be furnished in writing to one of the Secretaries before the day of the meeting at which it is proposed to be communicated to the Society. If it appear expedient with reference to communications of this class, a Committee of one or more members may be nominated by the President, with the consent of the person making the communication, to co-operate with him in investigating the case.

LI. Every communication shall conclude with explanatory remarks by the author; and, whenever it be possible, the facts recorded should be demonstrated by the exhibition, either of the patient, or of photographs, drawings, or casts, and also by microscopical and chemical analyses. Cases already published by the author shall not as a rule be received by the Clinical Society.

LII. At the ordinary meetings of the Society nothing relating to its laws or management shall be considered. The discussions shall be confined to questions relating strictly to communications.

LIII. The election of new members may take place at any ordinary meeting, at which fifteen members shall be present. Ten shall form a quorum for ordinary business.

ANNUAL AND SPECIAL MEETINGS.

LIV. The Annual Meeting of the Society for the Election of the Officers and other members of the Council shall be held on the night of the first meeting of the Society in January.

LV. The President and Council may at any time, on giving notice a week beforehand, convene a special general meeting of the Society for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting.
LVI. New laws, or alterations in the existing laws, shall be proposed by the Council only at the annual meeting of the Society in January; notice of all such proposals being transmitted to every member together with the summons to the annual meeting. For the adoption of laws or changes so proposed, the votes of two-thirds of the members present must be in favour of the proposal.

LVII. Any member may suggest to the Council alterations in the laws, by letter addressed to the Secretaries.

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Regulations adopted by the Council in accordance with Rule XL.

1. Excepting when the Council shall for some special reason decide otherwise, communications shall be read in the order in which they are received. Cases not read at any meeting from want of time shall be taken in their order at the following meeting. Cases not read on account of the absence of the author shall be removed from the list; but if the author shall thereafter intimate to the Secretaries that it is still his attention to communicate any such case, it shall be replaced in the order of date of such intimation.

2. If any member, in furnishing to the Secretaries, in accordance with Rule L., a statement of the principal facts of the case under observation, shall express his desire that a Committee be appointed to cooperate with him in investigating it, and it shall appear to the President expedient to appoint such Committee, the case shall be narrated at the meeting of the Society next following the day on which the Report of the Committee is received by the Secretaries, immediately after which the Report shall also be read and discussed.

3. No discussion shall take place on any Report relating to a case already communicated and discussed at a previous meeting.

4. The President shall have authority to nominate, from time to time, as may seem expedient, Committees of the Society for the purpose of conjoint investigation of Clinical, and especially Therapeutical questions.

5. Each Committee appointed by the President to enquire into any Clinical or Therapeutical question shall consist of three or more members of the Society.

6. Each Committee shall report to the Society within a time determined by the President, not exceeding three months. If, however, it be not possible to complete the enquiry within the period appointed, it shall be competent for the President at any time to prolong it.

7. As a rule, the Report should relate only to original Clinical observations. Every fact recorded in it should be verified by at least two of the members of the Committee. With reference to any fact not so verified, it should be stated by whom it has been observed.

8. Every Report should consist of three parts, namely:

(1) Cases under observation;
(2) Summary of facts observed;
(3) Conclusions.
ADDRESS BY THE PRESIDENT,
PRESCOTT GARDNER HEWETT,
ON
PYÆMIA IN PRIVATE PRACTICE.

GENTLEMEN,—In taking the chair this evening, I cannot refrain from thanking you for the honour which you have been pleased again to confer on me. I confess that I had my misgivings as to my fitness when I took the chair last year; but, whatever may have been my shortcomings, thanks to the Council, and more especially to the unwearied zeal of our Secretaries and of our Treasurer, I am happy in being able to congratulate you upon the still increasing prosperity of the Clinical Society.

With these few but heartfelt words, I pass on to the more immediate business of this evening; and, in doing so, I shall follow out the course upon which I ventured to enter last year, and again give you some gleanings from private practice, the clinical results of which may be made valuable, and all the more so when contrasted with those of hospital practice; and the subject to which I shall direct your attention is that of Pyæmia, a subject than which none is more important, and the consideration of which, in some of its bearings, has of late years been largely occupying the attention of our profession. The few observations which I have now to offer you on this subject will be confined to cases occurring in private practice.

A young lady, æt. 15, stoutish, but of good general health, came under my care for a congenital cystic tumour at the root of the neck. When an infant, an attempt had been made to remove this tumour, but a bit of it was left, as it was closely adherent to the large vessels. For several
years after this operation nothing was noticed in relation to this tumour, but for two or three years previously to my seeing the patient it had, without any apparent cause, taken to growing; and when I was consulted it occupied the whole of the lower part of the left side of the neck, and projected beyond the clavicle. At a consultation with the late Mr. Keate, who had performed the operation, it was determined that single-thread setons should now be used, and two of these were introduced into the tumour. In a few days intense inflammation set in, and suppuration followed; a quantity of matter was let out, the swelling subsided, and for some days everything appeared to be going on satisfactorily; then came rigors and sweating, but without any increase in the local trouble. After a while it became evident that the patient was suffering from pyaemia, the mischief being in the left lung, and for several days the position was most perilous; but one afternoon, after a violent fit of coughing, a large quantity of offensive matter was suddenly brought up, after which the more dangerous symptoms gradually subsided, and the patient ultimately recovered, and remained perfectly well, without any further trouble as to the tumour. In this case the patient’s room—large, lofty, and well-ventilated—was in a well found house in one of the best streets in town.

A hale old gentleman, close upon 80, who had long enjoyed most excellent health, had a small warty growth in the skin, just over the insertion of the tendo Achillis. This growth I removed, as it constantly became chafed, and troubled him much in walking. I made him lay himself up for a few days, and then, as the trifling wound was all but healed, he was allowed to go about a little, but, being of very active habits, he one day took a longer walk than usual. This was followed by some slight inflammation of the wound, which, however, soon subsided, and he was on the eve of returning to his usual avocations when rigors and sweating made their appearance; then came an immense deep-seated abscess in the thigh, under the constant draining of which he ultimately sank.

I was called in consultation to a gentleman who was suffering from pyæmia, and who a short time before had had a small wart removed from the scrotum. He died. Details of this case are given further on, as I had had to attend this gentleman between three and four years previously for a former attack of pyæmia.
Shortly after I had begun practice, I removed a small sebaceous tumour from the scalp of a lady. Everything went on well for the first few days; then came erysipelas of the scalp, which was subsequently followed by pyæmia and death. As far as one could judge, the conditions under which this trivial operation was performed were all favourable ones. The patient, middle-aged, and not stout, had some months previously undergone, without a bad symptom, a similar operation for the removal of three other sebaceous tumours. The bedroom, in a house in one of our most open and healthiest squares, was large and well ventilated; the weather was temperate, and the patient had remained in the house after the operation.

I operated upon a middle-aged lady, of good general health, for scirrhus of the breast, and for the first eight days everything looked promising; then came a rigor, followed by erysipelas around the wound, which for a few days went on slowly spreading; subsequently pyæmia made its appearance, and under this she gradually sank. The house in which the operation was performed was in one of the best streets in Pimlico. The bedroom was of good size, and well ventilated.

About a month afterwards I operated upon another lady for scirrhus of the breast. Middle-aged and slim, she was of a sallow complexion, but in good general health. In this case, too, everything promised well for the first few days; then came a severe rigor and sickness; erysipelas soon made its appearance round the wound, whence it gradually spread. Matters went on thus for a few days; then there was pyæmia, and in a few days more the patient was dead. In this case the operation was performed a few miles out of town. The bedroom, very large and well ventilated, was in a large well-built house, with all modern appliances, and situated on a heath, on elevated ground, overlooking a wide expanse of country. As far as one could judge, no better or healthier situation could have been selected for an operation.

A lady, aged about 45, had a large sero-cystic tumour of the breast, some of the cysts of which occasionally suppured, and were, under such circumstances, sometimes opened and sometimes allowed to burst. Matters went on thus, as she would not hear of an operation, for between eight and nine years, the general health being as good as ever between the attacks. At this period a small cyst suppured, and was allowed to burst, shortly after which erysipelas made its appearance round the edges of the little sore;
then in a few days came rigors and sweating, with pain and
great swelling of one of the knee-joints, and in a few days
more this lady sank. At the time of the bursting of the
cyst the patient was in her usual good health; her bedroom,
fair-sized and well ventilated, was in a house in a part of
town generally considered to be one of the healthiest.

A young gentleman met with an accident to his shoulder,
which led to inflammation of the joint, and for this he
ultimately consulted me. After awhile the inflammation sub-
sided, leaving the joint somewhat stiff. Then, persuaded by
some friends, he went to a bone-setter, who pronounced that
the bone was out, and proceeded to put it back, as he said.
The manipulations gave him great pain, and were followed
by a recurrence of inflammation in the joint, for which he
once more fell under my care. Suppuration of the joint
ensued, and abscesses burst in various directions. Thus
matters went on for a time, then came rigors, profuse sweat-
ings, and a sodden appearance of the skin, with an anxious
countenance, a running pulse, and great loss of flesh—the
joint itself and the parts around it being meanwhile without
any increase of mischief. Although tall and slim, this
gentleman, up to the time of going to the bone-setter, had
been in good health, and his family was healthy. He re-
mained in this perilous condition for some time, looking as if
secondary abscesses might occur at any moment. All this
time he lived out of town, in a good country house, well
cared for, and with plenty of fresh air. With the winter
coming on I sent him to the South of France, where, after a
residence of some months, the more threatening symptoms
gradually subsided, and when he returned to this country
there was a decided improvement in his general health; but
he was still far from well, and he remained more or less ailing
for between two or three years, at the end of which period
he was in fair health, with a permanently stiff joint, the
abscesses about the shoulder having gradually dried up.

A middle-aged lady, in fair health, was supposed to be
suffering from a sharp attack of sciatica on the right side,
and for this she was ultimately sent to Wiesbaden; but after
a time, finding that there was no improvement as to the
pain, she returned home, and for some months went on
creeping about, under the supposition all the while that she
was suffering from sciatica. It was at this period that I
was consulted on account of a swelling which had been
gradually making its appearance in the corresponding groin.
The swelling proved to be an abscess which extended into the iliac fossa; and, on further investigation, the sacro-iliac joint was found to be the source of all the trouble. The abscess was allowed to burst, after which everything went on satisfactorily for some three weeks, when pyaemia made its appearance, and was followed by death in a week. This lady lived in a house on a hill, a few miles out of town. The house was in every respect well found, and her bedroom was airy and well ventilated.

I was telegraphed for a few miles out of town to a gentleman about thirty years of age, who was thought to be suffering from acute inflammation of both ankle-joints, and inflammation of the left lung. The case, on closer examination, proved to be one of pyaemia, in connection with suppuration about the tonsils. In a few days, the inflammation around the ankle-joints ended in the formation of matter, which was let out; after which this patient gradually recovered, and in a few weeks was restored to his usual good health. This case has already been alluded to as that of the gentleman who died of pyaemia after a trifling operation for the removal of a small wart from the scrotum. Between the two attacks of pyaemia in this case, there was an interval of between three and four years. The first attack occurred in the country, and the second in town.

Recently, too, I have seen another gentleman, who, in connection with suppuration about the tonsils, had symptoms of poisoned blood—rigors, profuse sweats, sodden skin—under which he sank.

A young gentleman, æt. 18, had a severe attack of typhoid fever, from which, however, he was making a good recovery, when, about a fortnight after all symptoms had disappeared, he again became feverish, and soon afterwards, without having met with any accident, he complained of pain along the shin-bone; swellings, which were very painful, formed along the surface of this bone; suppuration followed; the abscesses were opened, and went on discharging for some time, after which they gradually dried up without further mischief.

A lady’s-maid, æt. about 30, had a severe attack of typhoid fever in the country. She recovered, and, being considered convalescent, came up to town, shortly after which smart feverish symptoms made their appearance, and were soon followed by swelling, affecting principally the knee, and extending some distance down the front and inner
side of the leg. For awhile the symptoms were very severe, and ended in extensive suppuration about the upper part of the tibia; the matter was let out, and the patient gradually recovered; but the free use of the limb was not regained for a couple of years, and during this period several bits of bone came away.

A delicate middle-aged lady had typhoid fever, which in due course passed away, leaving her weak and ailing, with now and then a recurrence of slight feverishness. After a time, this feverishness became more marked, and then she began to complain of very severe, deep-seated pains in various parts; first, at the upper and inner side of the tibia; then, at subsequent periods, over the lower part of the shoulder-blade, along the middle part of the spine, over the ribs, as well as over the crest of the haunch-bone. The pain in each of these parts was followed by swellings, and ultimately by abscesses, some of which were large and deep-seated. The abscesses were allowed to burst, and then, after the subsidence of the swelling, a probe was, in each instance, easily passed down to the periosteum, and in some parts the bone was found bare. The drain was great; hectic set in; and, after intense suffering, with occasional but limited mischief about the lungs, this lady sank.

A delicate-looking girl, at 18, ran a needle into the fleshy part of the leg, where it broke off, but could be felt projecting slightly beyond the skin. It was pulled out, and she went about her work as usual; but in a few days the tiny wound festered, and in a few days more she was admitted into St. George's Hospital with symptoms of pyaemia, rigors, profuse sweatings, and swellings in various parts. Subsequently came evident signs of mischief about the lungs, and she sank within a month after the trifling injury. The needle, according to the patient's account, was quite clean; and she stated that she had never been laid up before, and had always had good health.

A young gentleman, at 18, and apparently in good health, whilst bathing, ran a small splinter of wood into the fleshy part of the great toe. The splinter was immediately plucked out, and he went about as usual for several days, as if nothing had happened. Then the spot became painful, and a tiny abscess formed; it was attended to, but in a few days it was followed by urgent symptoms—severe rigors and most profuse sweatings; abscesses formed in the leg—one, a very large one, was deep-seated and in the calf; as they appeared
they were dealt with, but for weeks the sweatings were so profuse that it was necessary to change the bed-linen several times in the twenty-four hours. Ultimately, however, the patient recovered, and he left his bed, a mere skeleton, between four and five months after the onset of the attack. His bedroom was fair-sized, well ventilated, with a large window looking over a wide expanse of country, the house, a recently built one, being on the outskirts of a town.

A gentleman, middle-aged, was tripped up in the street, and fell violently upon his elbow, the lower bones of which were thereby dislocated backwards, and partially thrust through the skin. The dislocation was easily reduced, and for a few days everything went on satisfactorily. Then came suppuration of the joint, followed in about a fortnight by severe rigors and profuse sweating, with swellings in various parts, and he died a month after the injury. His bedroom was a fair-sized one, well ventilated, and in a house in a good street on the north side of, and not far from, Hyde Park.

A little boy, æt. 6, met with a slight accident to his foot, which was followed by acute periostitis of the bones of the tarsus; this ended in suppuration; some of the joints were destroyed; and at different periods, several abscesses formed in various parts—over the great trochanter, over the crest of the ilium, and in the sacro-lumbar region, as well as over the bones of the skull in divers places. The suffering was intense, with low muttering delirium and rapid wasting, and to such an extent that the child was soon reduced to a mere skeleton. Thus matters went on for some weeks, after which the abscesses gradually dried up; ankylosis of the bones of the tarsus took place, and when last seen, about a couple of years after the attack, he was a strong, active lad. All this occurred some miles out of town, in a good house in a healthy part of the country.

I was summoned a long way into the country to a young lady labouring under symptoms of a typhoid character, the origin of which was obscure. It appeared, however, that she had recently had measles, which had been followed by a slight discharge from the left ear. The recovery from the measles had been good, and she was going about, when rigors and sweating occurred, and were followed by fever, a dry, brown tongue, and great prostration; and such were the more obvious symptoms when I first saw this young lady. On further inquiry, it was now found that the discharge
from the ear had stopped; the intellect was quite clear; there was no pain in the head, no swelling in the neighbourhood of the ear, but there was pain upon pressure immediately below the mastoid process, and this tenderness existed also some way down the side of the neck, in the course of the internal jugular vein. From this it was inferred that inflammation had spread from the ear to the lateral sinus and internal jugular vein, and that, in all probability, secondary abscesses would follow. In a few days a large abscess showed itself in the left sterno-clavicular articulation; then came pain and swelling about the left knee and ankle, and inflammation at the back and lower part of the left lung; and ultimately there was a large deep-seated abscess at the back of the left hip. The abscesses were opened in due course, but the mischief about the knee and ankle-joint, and in the lung, subsided; the patient gradually recovered, and in a few months was quite well again. The conditions under which this young lady was placed were unexceptionable—a large, airy, well ventilated room, looking on to an open country.

A young lady had, after her confinement, severe symptoms of low peritonitis, and was in great peril for some days; she recovered, however, and appeared to be going on well for a time, when the left shoulder became very painful, and for this I was asked to see her. Examination proved that the joint itself was not affected, but the parts around were much swollen and very painful, and especially so in front of the joint. Ultimately, a large abscess formed in this situation; it was opened, and the patient got well and went into the country, where, some weeks afterwards, deep-seated matter formed in the pelvic region; this burst into the vagina, and after a time she recovered completely, and has remained in good health ever since.

An officer in one of our light cavalry regiments, aged 18, came under my care for gonorrhœa. The symptoms were severe, and so he remained at home, and was treated with opiates and demulcents. About a fortnight after he had been under treatment, symptoms of what appeared to be gonorrhœal rheumatism made their appearance. First, the left shoulder-joint was affected, but after awhile the symptoms subsided; then, without any apparent cause, came rigors, profuse sweatings, and a dusky appearance of the skin, with great disturbance of the general health. These symptoms were soon followed by intense pain in the left
Address by the President.

sterno-clavicular articulation, which became much swollen, and in a few days presented evident signs of fluctuation. A large quantity of matter was let out. Inflammation and suppuration in and about the right hip-joint, accompanied by most intense pain, followed. In due course the matter was let out; and subsequently numerous small abscesses formed in the skin over various parts of the body. The patient was reduced to a mere skeleton, and, notwithstanding all possible care, the whole sacrum became exposed. The more intense symptoms about the hip having subsided, the patient was now turned over to the left side; but in a few days the skin over the great trochanter gave way, and the bone became exposed. He was then propped up so as to rest mainly on the ischiatic tuberosities; and, as the skin here after awhile gave way, he was once more turned on his back, the sacrum being by this time for the greater part covered over by healthy granulations; and whilst in this position the skin over the various spinous processes of the vertebrae which happened to touch the bed gave way. What with one thing and another, I never saw a patient suffer more intense agony, which was such, indeed, as to necessitate the full administration of chloroform fifty-five times consecutively for the dressing of the various sores; but, notwithstanding all this, he ultimately recovered with an ankylosed hip. The onset in this case was in the early part of the year; and the patient’s room—large, lofty, and well ventilated—was in a house in one of our great squares. When this gentleman first came under my care, he was one of the healthiest-looking young men I ever saw, strongly built and most active; and he subsequently was one of the most dashing light cavalry officers in our central Indian campaign.

Some time after the occurrence of this case, the late Dr. Bence Jones happened to mention to me that he had been summoned into the country to a young gentleman, who was suffering from well-marked pyæmia, without, as far as could be ascertained, any previously existing suppurating surface. I then told Dr. Bence Jones of the above case of pyæmia after gonorrhœa, and begged of him to ascertain, if possible, if his patient was suffering from gonorrhœa. The patient died before Dr. Bence Jones saw him again, but, at the post-mortem examination, the existence of gonorrhœa was clearly proved. A third case of a similar nature, and in a young gentleman, was also mentioned to me by Dr. Guéneau de Mussy.
Such, gentlemen, are the cases of pyæmia occurring in private practice, to which I wished to direct your attention. They are twenty-three in number, and twenty-one of them fell under my own notice; the remaining two, the last being well authenticated, have been alluded to simply on account of their extreme rarity.

And now, if we proceed to analyse the circumstances under which pyæmia occurred in these twenty-three cases, it will be found that an operation was performed in six instances only. In four of these, the first four, the operation was of the most trifling nature—a single-thread seton, a small wart on the heel, a small wart on the scrotum, a small sabaceous tumour of the scalp; and it was only in the remaining two that the operation was of a somewhat severe character—amputation of the breast. Moreover, the first four were all in different years, and not in the same locality; the last two were in the same year, and within a month of each other; but one was in town, and the other in the country. And here let me note that the third case is also mentioned among those of recovery, this patient having had two attacks of pyæmia at several years' interval, and in different localities.

Of the remaining seventeen cases, in which no operation had been performed, there was a broken surface in eleven, and in six there was not even an abrasion. Of the eleven cases in which there was a broken surface, it was but small in ten; ulceration of a small serocystic tumour of the breast in one, of abscesses in two, of tonsils in two, of bowel in typhoid fever in three; a needle broken in the leg, a small splinter of wood in the great toe. The eleventh case was the only severe one—compound dislocation of the elbow. The six cases in which there was no abrasion were—a slight injury to the foot, followed by suppuration, inflammation of the lateral sinus and internal jugular vein in connection with discharge from the ear after measles, abscess after parturition, and gonorrhœa in three. Of these seventeen cases, none occurred at the same period, or in the same locality.

As to locality, of the twenty-three cases, sixteen occurred in town, and seven in the country. Of the sixteen in town, all, with one exception—that of the young girl who, after running a needle into her leg, was admitted with pyæmia into the hospital—were in the best parts of town, scattered about in good houses, and in good-sized, well-ventilated bedrooms, and well cared for; in fact, to all appearances, under most favourable conditions. The country cases, seven, were
in different parts, and widely separated from each other; and their conditions, too, were in all respects apparently excellent.

As to age, the youngest patient was six, and the oldest close upon eighty. Of the remaining twenty-one, eleven were between fifteen and twenty-five, and ten between thirty and fifty.

As to local treatment, it was out of the question in several of these cases; and in several cases, too, all possible care and supervision on the part of the surgeon would have been of no avail. Of the twenty-three cases, eleven were under such circumstances.

In conclusion, pyaemia, it has been said, is caused for the most part, by hospital-air; by foul air consequent upon the aggregation of surgical cases in the wards of our large hospitals; but pyaemia occurs also in cases even when placed under the most favourable conditions—perfect isolation, large airy rooms in the country, with plenty of fresh air, and in every way well cared for. Pyaemia appears, too, at times to be connected with atmospheric conditions; several cases occurring without any apparent cause, at, or about the same period in different places. The two cases of amputation of the breast, which were within a month of each other, followed exactly the same course; one, however, being in town, and one a few miles out of town. And at the same time that these cases were under my care, other cases of pyaemia, which occurred in private practice at the same period, subsequently came to my knowledge.

Moreover, cases occur in which patients are apparently prone to pyaemia; as in the case of a gentleman who recovered from an attack of pyaemia, and a few years afterwards died of another attack.

The truth is, the causes of pyaemia are still to be worked out; and this, gentlemen, is a problem the working out of which I would strongly urge upon the Clinical Society.

At the conclusion of the President's address, upon which a discussion had been invited by Mr. Prescott Hewett, Mr. Jonathan Hutchinson arose and said:

I feel great diffidence in rising to begin this discussion; but I have a few facts to add concerning the investigation of pyaemia, as it occurs, not amongst human beings, but amongst the lower
animals. The facts I have to add are these: that pyaemia is extremely common amongst the lower animals, and it occurs amongst them when they are not crowded in the least, but when they are placed under the most perfect hygienic circumstances. I have been engaged the last year or two in collecting such facts as I could respecting the diseases of lower animals. Last spring, I had the misfortune to lose several ewes, after parturition, from pyaemia. I made post-mortem examinations of them all. Most of them were treated in the open field. In one most typical case the ewe had not been in contact with any other animal which had suffered from suppurative disease; she simply had a retained placenta; the placenta decomposed, and from the decomposition she had internal metritis and the most characteristic pyaemia. The lungs showed pyemic deposits. We found the same condition in several other animals under similar circumstances, proving most clearly that, at any rate, hospital conditions, or anything in the least allied to hospital conditions, are, as you have well stated in your remarks, not in the least necessary for the production of pyaemia. I unfortunately had another fact in the country bearing in the same direction. My managing man at the farm had his finger bitten by a calf. 't was not bitten deeply, and he thought nothing of it. He was teaching the calf how to drink, and it gave his finger a grip. The skin was not broken in the least. He was a man living at a farm-house in a very healthy part of the country, and he was not in contact with persons in the least diseased; the sequel was this, that his finger inflamed and he had a thecal abscess with intense inflammation of his hand. He had a very severe rigor, followed by jaundice; became better for a while, and was able to walk about, although still looking extremely ill and with his wrist and hand very much swollen; then a swelling suddenly formed in front of his hip-joint, and a very large abscess appeared; he had another abscess in his leg, and he ultimately died. Now if it should be suggested that I took him the pyaemia because I opened his thecal abscess after coming from a London Hospital, I would reply, that he had his first severe rigor the day before I cut it. It was not cut early enough, because I was only there occasionally, and I did not see him soon enough; I cut it on the day after the rigor which indicated the pyaemia. I do not believe there was any contagion. Only recently, I was asked in the same part of the country, down in Surrey, on one of the most healthy hills in the locality, to see the gamekeeper of one of my friends. This man had been bitten between his thumb and forefinger by a ferret. The other gamekeeper stated that he had been bitten twenty times before, and had never taken any hurt; and I daresay many gentlemen present can confirm me by their own experience, that there is nothing poisonous in the bite of a ferret. This poor fellow, a man of very temperate habits, died eight days after this very trifling injury, having had a very severe rigor or the third day after the bite, and acute inflammation of the whole of his arm. I should like very briefly to add, that my conviction, from all that I have seen of pyaemia, is this—that it will originate wherever veins or bones are implicated.
in the inflammation; and that the reason why injuries to bones are so
dangerous, and of course idiopathic periostitis amongst them, is simply
that whenever bone is inflamed you have great risk of inflammation of
the veins. Next we come to the final question, as to what extent
pyaemia is to be regarded as a disease due to hospital air, or to any in-
fluences which are incident to the confinement of patients in hospitals.
We shall be very premature indeed, if—reasoning from the facts you
have brought forward, and which I have supported by experience
amongst the lower animals, showing that pyaemia can undoubtedly arise
under circumstances extremely different from those of patients in
hospital wards—we conclude that therefore the patients in hospitals
have no special proclivity to it. My conviction is, looking at the results
of my private practice as compared with those of my hospital practice,
that the hospital patients are in far greater danger of pyaemia. I have
been trying, since I heard the title of your address announced, to
recollect whether I had lost any patients in private after operation from
well characterised pyaemia. I have seen a great many cases in private
death from pyaemia after periostitis, carbuncles, boils, &c., but I do
not recollect more than one or two somewhat uncertain cases of death
from this cause after major operations. But in hospital practice—not so
much in late, but in former years, and at the London Hospital—I have
lost a considerable number of cases after operations by pyaemia. At
the Metropolitan Free Hospital I never recollect to have lost one,
although it was a crowded, ill-ventilated hospital, placed in the worst cir-
cumstances, whilst the London Hospital is one of the best. I cannot, then,
help thinking that there is the possibility of the conveyance of some-
thing from patient to patient which may be the cause of pyaemia; and
my conviction is, that, under many circumstances, the inflammation of
bones and veins—of bone, because it leads to inflammation of a vein—is
very often a matter of contagion, and that patients confined in hospi-
tals are very much more likely to get the disease, as the result of con-
tagion, than those treated under ordinary circumstances. Hence, if this
line of reasoning be at all correct, we come to the principle upon which
we should attempt to diminish the risks of pyaemia in hospitals; not by
ventilating; there is no use whatever in introducing fresh air into the
hospital with a view to prevent pyaemia; at least, that is my conviction.
We have had the windows of a ward where pyaemia has been kept
constantly open, and have given the patients erysipelas by letting draughts
blow upon them when asleep, and yet have not prevented pyaemia. It
is a matter of contagion. What you want here is to take away the
source of contagion; so far as erysipelas and pyaemia are matters of
contagion, you must remove the source of contagion; and, if you can
prevent any wound from taking on erysipelatous action, you prevent it
from becoming a focus from which other cases may spread by its
proximity. It is to systematic isolation, and not ventilation, that we
must trust. As you would not ventilate a field to cure it of growing
thistles, but would root up each plant before it shed its seed, so too, in
my opinion—for the analogy, I think, holds good—ought we to proceed
in pyaemia, to attempt its eradication.
Mr. Brudenell Carter: I hope that, in the course of the discussion, some of the senior members, who can look back to the time when venesection was commonly practised, will tell us whether they ever saw a case of pyaemia following blood-letting. I think that, if Mr. Hutchinson's view be correct, pyaemia should have been more or less frequent after that little operation.

Mr. Charles Hawkins: I must apologise for presuming to offer an opinion upon subjects connected with large hospital practice; but it was my good fortune for many years to watch very attentively one of the largest operating practices in this country, certainly one of the largest for variety of operations. My experience spreading over nearer twenty years than fifteen, can bring to my mind many such cases as you have described, resulting from operations and accidents alike apparently simple. I am very glad that your paper has been read, because I think it will lead people to consider a little more than they hitherto have done the bad character given to London hospitals and hospital construction. There were two cases very lately that struck me as very good examples of the theory with which you have set out to-night. There was a gentleman, a lawyer, riding in the Park before going to Lincoln's Inn, who had the misfortune to be thrown from his horse, and to fracture his leg. When I say he was under the care of the President of this Society, it is sufficient to prove that he was well looked after. He was brought, as many of these cases are, into St. George's Hospital, and was placed in a ward in the new wing, which hitherto had not been occupied. He had for breathing space upwards of 12,000 cubic feet. He had a nurse told off to himself; and he had, as you know, everything in the requirement of diet that he would have had had he been your patient in Belgrave Square. The Governors of St. George's Hospital have never placed any limit upon the diet of their patients. That patient died, I think, within about three weeks, of pyaemia. In the next ward, where thirty or forty patients were lying, having only 1,200 cubic feet of air per head and attended to by one head nurse and her assistants—subject, therefore, to abundant opportunities of contamination by infectious matter—there lay a patient who had come to grief in bringing up a horse from Epsom. The result was his thigh was broken, his jaw was broken, and such an impression was made upon his skull, that, I think, before he left the hospital there were seven or nine pieces of bone taken from his cranium. That patient did well in the crowded ward. You may recollect some years ago an out-break of phagedena in a ward, so extensive that it was brought to the notice of the Board. That was in the ward at the top of the house, in the best ventilated and best placed ward in the hospital. In the wards below there was very little of this phagedena. There has been in the ward scarcely any phagedena since, or hardly any appreciable quantity. But we have had the means, through the munificence of one individual, of putting to the test the opinion which has been given with great earnestness for many years, that if operations were performed in the country, and we only
had convalescent hospitals where the patients could be sent out of
the contamination of London hospitals with closely packed beds, the
cases would do well. Through Mr. Morley's liberality, the governors
of St. George's Hospital were put in possession of a hospital at the top
of Wimbledon Hill, in, I suppose, perhaps the most healthy position
to which we could go within the distance. There are surgeons in this
room who have operated at both hospitals. We have seen the most
serious operations do well at St. George's, we have seen them do badly;
we have seen them do well in Wimbledon, and we have seen them do
badly. We have seen patients die at Wimbledon having a room all to
themselves, nurses for themselves, where there was no disease of any
contagious kind, for the patients were almost convalescent, with an air
blowing pretty nearly from the sea. That experiment has not as yet
solved the question of the wherefore this mortality amongst the patients
in hospitals: nor has it served to attach the blame to causes merely
local. For the patients at each hospital named died exactly in the
same way, whether after operations or after accidents, although circum-
stances so very differently. There was a hospital in this town which,
I suppose, was about the most perfect specimen of what it ought not
to have been that could possibly have been produced. It was a dis-
carded workhouse, built in a churchyard crammed full of bodies.
There were some great operations performed in this hospital, and there
was a very considerable amount of success. Then a new hospital was
set about; the best opinions were taken, and the hospital was built;
all the dead bodies were removed, and carried to a distance. I should
like those who have practical experience to tell me what alteration has
taken place in the results of the accidents and operations in those two
buildings; whether in the new building it has been necessary to shut
up any wards for a time, or whether the accidents and operations are
very much better than in the old building? I believe that, when you
find these fatal cases occurring in public practice, you will find them
in private practice. It was a result of Sir Benjamin Brodie's great
experience, that, under certain circumstances, he was exceedingly loth
to operate—in north-east winds, and in the great heat of summer; he
never much liked an operation when there was a sudden change of
weather.

Sir James Paget: I am sure that not the Society alone, but the
public, are greatly indebted to you for bringing this matter very pro-
minently before them; for, after all, the question which has been
raised by two of the previous speakers is mainly this, whether there
really be conditions in hospitals which render pyemia more frequent
in them than in private practice. The question is very difficult to
answer; because, even for those who have had large experience in
both hospital and private practice, the classes of cases with which they
have to deal are so different, and the classes of persons so different, that
it is very hard indeed to compare them. I suppose that very few
surgeons, even of the largest practice, have had many cases of ampu-
tation in private practice for what used to be at least a common cause
of operation in hospitals—diseases of the knee or elbow-joint. In private practice these hardly ever come to operation: they are by long care remedied; and we have not the number of cases of amputation in private practice which we can at all compare with those in hospitals. The classes of patients and of diseases are in the two cases so widely different, that it is hardly possible to get the exact numerical results by which the frequency of pyæmia in the two conditions could be determined. We can have only a very general impression; yet this may, after some years of experience, be considered almost as true as that which would result from a comparison of numbers. My own experience exactly accords with your own, that pyæmia in private practice is, making a certain deduction for the different class of persons with whom we have to deal, just as frequent, arises from just as trivial causes, occurs after the same class of injuries, and leads to the same fatal results, as it does in hospital practice. I tried once to compare my cases in private with those in hospital; and for six years, while assistant-surgeon to St. Bartholomew's, the number of cases was pretty nearly equal. The result, so far as it could be reckoned, with reference to what may be called 'plagues of surgery,' erysipelas, gangrene, &c., was that there was no marked difference between them, except that, as one had to do with the well-fed in the one place, and many of the starved, the intemperate, and the drunken in the other, there was an advantage in favour of private practice; but, barring this difference, there was none that could be fairly established. Among private patients, I have seen pyæmia arise from more trivial causes than I have ever seen in hospital practice. One case I may mention, where an ordinary chilblain in a young nobleman led to pyæmia and rapid death. He was at school in one of the healthiest districts in England, near Bagshot, under every condition of health. A young lady living at Kilburn, well placed for health, manner of life, house and all circumstances about her, had a slight chafing from the lining of her boot above her tendon Achilles; and from that, in a fortnight, she died, with abscesses in various parts of the body, and with all the characteristics of pyæmia. Again, the only two cases in which I have seen pyæmia follow operations for piles were in private patients. Happily, neither was fatal, but both were very well marked. I am sorry, too, that my experience after operations has not been so favourable as Mr. Jonathan Hutchinson's. Allowing for the differences of personal conditions of which I spake, I have seen quite as great a frequency of pyæmia after operations in private as in hospital practice. Of the very few amputations for disease of the knee-joint which I have done in private, one was for a wound of the knee. It was done at Croydon, on a fairly healthy lad, in a perfectly well-arranged house, well aired, with every condition of health. He died of well-marked pyæmia. After operations upon the breast, I have seen pyæmia as frequent in private practice as in hospitals; and I may here add, as it is, after all, the chief point to which the paper must tend, that not only with pyæmia, but with the other accidents, as they are called, of operations, I have seen
no reason to believe that hospitals are places of greater infection, as it is called, or of greater unhealthiness, than is met with in private practice. I have seen three cases of hospital gangrene in private, and only three under my own care at St. Bartholomew’s; and of those three in private practice, one occurred in one of the best houses in Harley Street, another in the best house in Regent Street, and another in St. John’s Wood, at distances of two or three years asunder, and with no cases of gangrene or any disease of the kind, as far as I know, within reach of the patients. I therefore come to the very clear conclusion that there is really nothing, I will not say in any hospital, but nothing in a well-managed hospital, which contributes to the production of pyæmia. My experience is limited entirely to my own private practice and to St. Bartholomew’s Hospital; and I would say for certain that, with the exception of a few times when I believe certain wards were under the management of careless sisters or house-surgeons, I have not seen anything in that hospital which would hinder the prevention of pyæmia more than it may be hindered amongst one’s patients in private practice. I am, therefore, very earnest in the hope that the term ‘hospitalism,’ which is being applied not only to this, but to many other diseases that occur in surgical practice, will be at once and for ever abolished. It seems to me a name altogether unfair. I believe there is not any fair evidence whatever that these maladies following surgical operations are more frequent in well-managed hospitals than anywhere else. But it is not on the ground of unfairness alone that I would speak; but because I believe a term such as ‘hospitalism,’ or such as ‘surgical fever,’ does lead astray altogether those who would inquire fairly into the truth. If pyæmia is to be studied only in hospitals, everyone will be studying it in a wrong direction. The thing must be studied, as you have yourself suggested, equally in private practice and in hospitals, and then we may come to a fair knowledge of the truth. Before sitting down, in answer to Mr. Carter’s question as to the frequency of pyæmia after venesection, I would say that I fear I am one of the few who have had any large experience in venesection. I am just old enough to have known it when it was rife in country practice; for, in the town in which I was apprenticed, it was customary to bleed on every market-day a considerable number of persons who came with no definite malady except that they believed it would be better to be bled. They came especially in the spring and fall of the year, both of which were elastic terms, extending over two or three months. I do not remember the total number bled, but it was as large a number as liked to come and pay a shilling apiece; so far as I remember, pyæmia never did follow. If it did, the patients must have retreated into the country, and there been lost sight of; but certainly there were a large number of townspeople bled of whom I should have known the fate. At the time I never saw a case of pyæmia, except after very severe operations. I therefore think venesection can hardly be charged with producing great liability to pyæmia. But then it should be said that the wounds
were always treated in the manner in which pyæmia, if arising from atmospheric causes, was likely to be averted; they were carefully bandaged, shut up, and very quickly healed.

Dr. Robert Barnes: I also remember the time, to which Sir James refers, of country bleeding, and have bled nine or ten people in the morning on market-days as quite a common practice. I have on one occasion seen pyæmia follow; it might have been from a foul lancet, and I cannot say what the cause was. I was also for some time physician to the Dreadnought. There was a hospital in very peculiar circumstances, isolated from everything on land, and surrounded by water. That hospital had been prone to pyæmia and to hospital gangrene to, I believe, a considerable extent. It came under the charge of Mr. Tudor, the resident surgeon, who was a man of great energy and painstaking, most particular in everything he did; nothing could be done upon which he did not have his eye. He went round dressing his patients in the morning with a pupil behind him; and, instead of using sponges, a piece of clean tow was used for every patient to wipe up the secretion, and was thrown overboard instantly. Nothing was ever used to two patients, I believe, for the three years or more during which the system was followed, and there was scarcely ever a case of pyæmia or any infectious disease in the ship. He attributed it, and I believe with very great reason, to the care he took in dressing the patients, putting aside everything that could possibly be carried from one patient to another. In hospitals on shore this is scarcely possible. You have a number of dressers, a large number of students who are using all sorts of things; and it is with horror I see sometimes a case of ovariotomy in a hospital where the fingers of half-a-dozen persons are poking into the peritoneum. So far from assenting to the proposition of Sir James Paget that cases of operation are not likely to be more dangerous in hospitals than in private practice, I entertain a profoundly different conviction as to this particular operation, on account of the enormously increased risk of infection, from the surroundings of the patient in a hospital, as compared with private practice. That cases do arise in private practice, apart from hospital influence, is a matter that cannot be disputed, especially after the evidence of yourself and other persons in this room, and what one sees in the practice in my department especially, analogous to that which Mr. Hutchinson has mentioned among the lower animals. There undoubtedly retained placenta, or some cause of the kind, may set up symptoms of pyæmia, and patients die from a form of fever which I call autogenetic, as arising from the patients' own conditions, apart from everything external. There is a point also bearing somewhat in favour of Mr. Hutchinson's idea that the veins are concerned, certainly in the uterus. If the placenta be not removed, it goes into decomposition, and there you have a series of veins of a kind peculiarly liable to be affected by the imbition of foul matter. There is no department of practice which does not yield some facts and some experience to illustrate this subject. It cannot be looked at entirely
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from one side, either inside a hospital or out of it, from one practice or another. It is one which should engage the earnest attention of every member of the profession.

Mr. Campbell De Morgan: I think that there are, perhaps, two questions mixed up together, with reference to the occurrence of pyæmia in hospitals. There is no doubt, I suppose, that if several patients upon whom operations have been performed be placed in the same ward and one get pyæmia, let the source of infection be what it may, the others are liable to the disease. In the event, then, of a case of pyæmia happening in the wards of a hospital, other persons brought directly or indirectly into contact with the afflicted person usually do fall victims to it, and, inasmuch as very great care is requisite to prevent such contact, there is risk of spreading pyæmia attached to hospital wards, and probability of greater average mortality from this disease within hospitals than without them. This, however, does not prove that pyæmia is specially generated in hospitals, nor justify us in calling it a special hospital disease. I suppose concurrent testimony would show that the observations which you have made are correct—that the disease does occur in private, dotted here and there, not communicated from one to the other as it might be in hospitals, and without any apparent cause such as is supposed to exist in hospitals. But may it not be from very much the same causes in both? I should doubt whether we may not have even in the best private houses much the same sources of contamination as we have in hospitals; nay, sometimes even more intense infecting causes than are to be found in any well-regulated hospital. The most fatal case of blood-poisoning I ever saw was in a lady living in what appeared to be a very well-ventilated house in the north of London. She died of most intense blood-poisoning, after the removal of a tumour in the neck, without any indications of phlebitis, and it turned out afterwards that there was a general feeling of malaise now and then on the part of some of those who inhabited the house, and they occasionally found a certain amount of unpleasant smell about it. There was no doubt that there was imperfect drainage. If you come to look to the connection of the various ventilating apparatus connected with the sewers and so on, I doubt whether there are many houses in London in which you would not find an imperfection which you would not allow to exist in a hospital if you knew of it. You may have this undoubtedly without any condition of which you or I or the patients themselves would be conscious. We had a very curious illustration of this, which has been quoted so often that I am almost ashamed to mention it again. In the Middlesex Hospital there were two beds, one on each side of a window, and patients in them almost invariably, if they had a cold or were operated on, had erysipelas or pyæmia. There was nothing in connection with the window in the part of the ward, so far as smell was concerned, to call attention to it; but it was found that at some distance below, in the area, in a line communicating with the window, there was a dust-bin. The dust-bin was ordered to be cleansed out, and the window was not allowed to be
opened again, and for two or three years we never had a case of erysipelas or pyæmia in those beds. Then the precaution was neglected, and the thing again occurred. I do not quite agree with Sir James Paget as to the notion of covering up the wounds in cases of bleeding having anything to do with the prevention of pyæmia. I think this was due to the immediate pressure on the vein, so that nothing could be carried through it, not from the exclusion of air. I am becoming sceptical as to the effect of air upon wounds in producing decomposition or giving rise to unhealthiness. I think that free exposure to the air, even of a hospital ward, seems to agree with wounds just as well as treating them in the most careful manner by dressings. I have adopted it in several cases of late, and certainly with very admirable results and with very little trouble indeed. I think that pent-up matter is very often the cause of all the mischief you get; but if it can drain away, and not be collected in the dressings, contamination would be thereby prevented. I agree with what Mr. Hutchinson has said, that pyæmia is connected a good deal with the conditions of the veins of the bone. There again it is from pent-up matter, as you get it in the cancelli of bone; the matter is collected and decomposing from lodgment, not from exposure to the air, but because the air does not pass freely enough to it. Anything that has a septic tendency, I take it, will have the effect of producing pyæmia or its allied condition, erysipelas, either in hospitals or in private. I quite think the notion of 'hospitalism,' or hospital disease, has been carried a great deal too far; that it is altogether a mistake to suppose that hospitals are so very much more prone to these diseases originally than private houses. That they will spread more in hospitals than elsewhere, except proper precautions are taken, is another question.

Mr. BRYANT: In listening to your paper the chief point that attracted my attention was the very interesting one in which you threw the weight of your authority to support the idea that pyæmia is not a purely hospital disease. This has long been very much my opinion, and I have formed it not simply upon the theory of the question, but upon the observations which I have been able to make both at Guy's Hospital and elsewhere. A few years ago, when I was working at the subject of pyæmia, it was an object of interest to me to find out whence these cases of pyæmia came; and I was very much struck with the important fact that some of the worst cases of pyæmia which we saw in Guy's Hospital were admitted with the infection, and that these cases all followed trivial accidents, such as small wounds, slight ulcers, boils, and carbuncular affections of the milder form. In analysing the appearances also met with after death, I found that after the trivial accidents the pathological results were far more severe than when they followed the graver accidents which we are in the habit of seeing in a hospital ward; that is to say, we were more apt to find multiple abscesses in patients dying from pyæmia following simple contusion, or abscess, or boil. Suppuration invaded a larger number of glands, as the liver, the kidneys, the spleen, and the heart. Of course, in the
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lungs we expect to find them. All this occurred in cases trivial in their origin, in cases of simple suppuration, or of suppuration in the cellular tissue following contusion. That fact was a most striking one, and, I confess, led me to think that pyaemia could not be regarded solely as a hospital disease, and to believe that we should find as much pyaemia probably outside the walls of a hospital as we find inside if we could get a fair statistical comparison of the two classes of cases. I was very much struck, likewise, on hearing your cases, with the fact that so many of them were attacked first by erysipelas; but I suppose we must class erysipelas amongst the so-called hospital diseases. This point is not, however, very clear, for during the last three years I have taken considerable pains to find out how far such an inference is correct; and I find, in looking at our erysipelas ward in Guy's Hospital, that two-thirds of the cases of erysipelas treated in the ward have been admitted from outside the hospital, and that only one-third of the cases have been drafted from the surgical or medical wards. Erysipelas, taking it merely from these statistics, cannot consequently be looked upon as simply due to so-called hospitalism, although, of course, erysipelas and pyaemia are connected with hospital work. There can be no questioning the facts that Dr. Barnes has given, and the facts which we are in the habit of noticing in hospitals, where, from the want of cleanliness, or inattention on the part of the dressers in the matter of sponges, &c., erysipelas and pyaemia are apt to spread; but to say that they are simply due to, or connected with, hospital practice is very unfair. Dr. Barnes gives us the most rigid test, puerperal patients, and we possess another in such a severe operation as ovariotomy; for it is well known that unless unusual care or extra precautions be observed in the performance of ovariotomy, or in the treatment of women after childbirth, no particular success will crown the efforts of the practitioner, and he is therefore most scrupulous in excluding all those extraneous influences which we know to have effect in producing erysipelas and pyaemia. Feeling, as I do very strongly, the great necessity of observing extreme care as to cleanliness, air, and so on, in hospital work, I must say I should not like to sit quiet without expressing my conviction that pyaemia and erysipelas are not really hospital diseases; that they are in their fullest extent, if we could get a fair amount of statistics, quite as common outside the hospitals as they are in them.

Mr. T. Holmes: The observations which Mr. Charles Hawkins made with respect to the hospitals at Wimbledon and St. George's brought to my mind a rather striking circumstance that occurred in my own practice at these two hospitals, and one to which I have no doubt he was alluding. Mr. Morley, as Mr. Hawkins justly observed, left us the opportunity of trying the experiment as to whether operations would succeed 'better in the country than in the town, and I thought at one time of testing this by performing a series of amputations at Wimbledon and comparing them with a series of similar amputations at St. George's. I commenced with two cases which seemed
to me to be very appropriate for the experiment. One was a middle-aged man suffering from chronic disease of the tarsal bones, a perfectly healthy individual who had never, as far as I could find, had any serious disease in his life, and certainly never suffered from erysipelas. The other was a man broken down by all kinds of dissipation, and no doubt, to a certain extent, a bad subject for an amputation, but otherwise there was nothing very remarkable about the case. I sent these two men down to Wimbledon, and performed amputation upon both on the same day, a few days after their admission into the hospital. They were treated in separate rooms, neither room having even been used before; one was in one of the wards of the hospital, the other in one of the private rooms. They were separate from each other, but attended by the same nurse; otherwise they were in exactly the same conditions as a man would be in private practice. The rooms were entirely free from all possibility of contamination. They were not attended by medical students at all, but by the resident medical officer of the hospital. There were no other cases in connection with them whatever, and all the other cases in the hospital were simply convalescent cases. I never saw two cases more likely to do well. One of them was certainly a case of amputation which anyone would have expected to recover merely Syme's amputation for chronic disease. Both of these people died; one of pyaemia, and the other of erysipelas. The erysipelas did not attack the stump at all, but simply the head. This appeared on the fourth or fifth day, and was followed shortly afterwards by sloughing of the skin of the back to an enormous extent, a piece as large as a soup-plate sloughing a few hours before his death. The patient died the fifth day after amputation. The other man died on the seventh day of pyaemia. I must say that, although the occurrence of two isolated cases of the kind proves nothing, still it discouraged me so far that I ceased to think it worth while to spend the great amount of time that would be necessary to carry the experiment further.

Mr. Croft: I had the pleasure of being Dr. Barnes's colleague at the Seaman's Hospital when he was physician, being then assistant-surgeon. I do not think that I gained, during my five years' experience of surgical work, the same impression which seems to have adhered to Dr. Barnes's mind since his leaving the hospital. I think that it would be scarcely fair to the reputation of those gentlemen whom Mr. Tudor succeeded to leave the Society under the impression that Mr. Tudor found a system of dressing at the hospital which could be condemned. When I say that it was under the care of Dr. Busk, I think that it is a sufficient guarantee that the utmost care was taken both surgically and hygienically. Mr. Busk had Dr. Rooke under him as resident surgeon, who worked there in, I think, no less energetic a manner than Mr. Tudor: and I think it is only fair to Mr. Busk and to Dr. Rooke, who is dead now, that I should say that I think they had done all that they could do. I know that, after some alterations had been made in the state of the hospital, an improvement took place in the number of cases of erysipelas, pyaemia, and phagedena;
but, after a time, while Mr. Tudor was there—I am sure if he were here he would bear out what I say—both pyaemia, erysipelas, and hospital gangrene were rife. I had many cases of sloughing bubo under my care to treat—hospital phagedæna, and not simply syphilitic phagedena. I do not wish to say anything about hospitalism beyond this, that I am extremely glad Sir James Paget has said openly at this Society he hopes the name will be dropped. The sooner the name is forgotten, I think, the better, for nothing is clearer in my mind than that it is a most unfair term. I think that the comparison between the experience at hospitals and the experience in private practice has perhaps taken up, I was going to say, too much of our attention; at any rate it seems now that we have arrived at a point when our attention should be, as you have proposed, turned in a new direction in investigating pyaemia. One is too apt to take a number of cases of pyaemia in the lump, and say that so many cases of pyaemia have occurred, without studying each particular case. It seems to me now that we have arrived at the stage when accurate reports should be presented. When I say 'accurate,' I mean reports which will extend not only to the general condition of the patient, the general condition of the house in which the patient was, or the hospital in which the patient was, but which will give details of the juices in the patient's body; I mean the condition of the nutritive fluid in the patient's body. When we have a series of reports of cases including such statements as I have referred to, we shall then have a good basis to go upon. At present, it is only here and there that we get an account of the condition of the patient's blood or lymph in pyaemia.

Dr. Barnes: The conclusions of Mr. Tudor were carefully published for two or three years, and I think that, if the Fellows refer to the reports, they will see that what I have said will be borne out.

Dr. Bastian: I shall confine my remarks as far as possible to the mode of origin of the disease. No cases of pyaemia have appeared to me more important than those which Mr. Prescott Hewett has communicated to the Society, in which there was no open wound or abrasion. There were six cases of this kind in a total of twenty-three. This is a very large proportion of such cases; still, I think it must be according to the experience of very many members of this Society that cases of this kind do occur from time to time—cases, for instance, in which pyaemia occurs, and acute necrosis of a long bone, without any external wound. I would also call attention to the fact that, in Dr. Murchison's work on Fever, he states that pyaemia has been known to occur, not at all infrequently, in cases of epidemics of typhus fever, and also in cases in which there have been no bed-sores and no external wounds of any kind. Here, therefore the pyaemia would appear to occur where there was no wound or abrasion. It seems to me that cases of this kind are capable of throwing a great deal of light upon the mode of origin of the disease, and may, perhaps, elucidate those other instances in which an external wound does exist; because, even if we take the cases in which pyaemia occurs with an open wound, whether it be in private practice or in the...
wards of a crowded hospital, the question presents itself; how the morbid condition is initiated. We hear a good deal about septic influences, and septic agencies, which are engendered under certain conditions; but we still have to learn whether these agencies, whatever they may be, act principally upon the wound itself, or primarily upon the general constitution of the patient, and thus secondarily lead to an impaired or vitiated condition of the secretions of the wound. Now, I believe that, generally, the first view would be the more popular one, that septic influences act directly upon the wound. Still, I think there are very many reasons which should lead us to suppose that the other mode of origin, through internal morbid conditions by dint of constitutional vice, that is, may be operative. Since the last meeting I have made a few observations on the condition of wounds and the condition of the fluids in wounds; that is to say, wounds which have been treated in the ordinary manner in hospitals, in which no special precautions have been taken to exclude contamination from any bodies which might exist in the air. I have examined these fluids mostly twenty-four hours after the dressings have been applied—that is, just before new dressings were applied. In some cases I have found that the pus was crowded with bacteria of different kinds—the ordinary rod-shaped form and also the dumb-bell form—and in other cases no traces of bacteria were to be found. I found, also, that these discrepancies seemed to be easily explicable; that wherever one had to do with a thoroughly healthy subject, and where the temperature was low, no bacteria, or other organisms, were to be found in the wound; but where the temperature of the patient was as high as 101 deg., or upwards, then in every case I have found bacteria existing, in more or less abundance, in the fluids bathing the wound itself. Now, of course, if bacteria are there—acknowledged by all to be the most potent of septic agents—if they are actually in the wound itself, we should have no reason to resort to the hypothesis of subsequent aerial contamination to explain the occurrence of putrefaction. I believe that, in certain states of the system, where a febrile temperature exists, and where the patient is in a somewhat unhealthy condition, the bacteria are actually bred in the fluids themselves. In proof of this I may mention a case which occurred, under my care, in University College Hospital a very short time ago. It was a case of acute pemphigus. The patient came into the hospital with very large blebs all over the body. The temperature of the patient was raised to a very high point; at one time to 104·5 deg. Many of these blebs remained perfectly intact for six or seven days. At different periods I examined the contents of the unbroken blebs, and I found that they were all crowded with organic forms. They contained bacteria, and they contained what is known as leptothrix in great abundance. The blood of the patient, however, showed no signs whatever of any organisms. I may say that this patient also recovered, notwithstanding the fact that very large surfaces of the body were completely bathed with septic organisms. Here organisms were apparently produced in the half-excreted fluids of a febrile patient, under
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conditions which excluded the possibility of ærial contamination. And so, I believe, in cases of open wounds, the bacteria which occur in them are very often produced independently of ærial contamination. Hence, if wounds can lapse, in certain states of the system, into a condition of this kind, in which their fluids rapidly and spontaneously putrefy, it seems of very little use to resort to the belief that the wounds themselves have been contaminated by any mere organic matter, or even by bacterial germs dropping into them; and the facts which I have related seem to me rather to support the notion that, in the cases where a wound exists, the first stage of the pyæmic process may be not at all dissimilar from that which happens where no external abrasion exists. That is to say, you may have a febrile condition, and some modification of the healthy febrile process, the wound itself, as a consequence of that, becoming unhealthy, symptoms of pyæmia at last developing themselves. It therefore seems to me that this is a point of view which is well worthy of being kept in mind, and that the real mode of origin of both classes of cases, those with and those without external wounds, may perhaps be identical.

The suggestion having been thrown out by Dr. Southey that the discussion should be strictly limited to the question of pyæmia, and to facts bearing upon or illustrating its communicability and sporadic origin,

Dr. Braxton Hicks said: It seems to me that, if the discussion be restricted to cases only where secondary deposits occurred, only a small portion of the case will receive attention, and our views as to the causation of the ill-doing of injuries will be very limited; because anyone who has watched the invasion of erysipelas and diphtheria in a previously healthy ward will see that others, besides those who present secondary deposits, are influenced in different ways, but all with conditions recognised as the result of blood-poisoning. In an invasion of diphtheria in a ward I have seen a woman, recently delivered, attacked by so-called puerperal fever; another, who had undergone a slight operation, had secondary deposits; another had diphtheria, and others various forms of blood-poisoning. During an epidemic of erysipelas, one, who was tapped for ovarian dropsy, had septic peritonitis; another, tapped at the same time, erythema and severe toxæmia; and all the plastic operations did badly for some time afterwards.

Mr. Henry Lee: I think, with Dr. Hicks, that it would be exceedingly difficult to limit the discussion of this subject to pyæmia, strictly so called. I think, also, that it would be very difficult to limit the discussion to atmospheric influence independently of contagion—the contact of other than those things ordinarily contained in the atmosphere. It was my lot, many years ago, to be the house-surgeon of the Lying-in Hospital in York Road; and there, unfortunately, we had blood-poisoning to a very great extent. I shall never forget the impression that the cases I then saw made upon my mind. The skin became very hot, the pulse exceedingly weak, from 130 to 140 in a minute; and the poor patients almost invariably died, sometimes from erysipelas apparently, sometimes from mortification of the skin, sometimes
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from the effusion of serum into the peritoneal cavity, sometimes from the effusion of ill-organised lymph; and occasionally, when they lived long enough, and only when they lived long enough, they had purulent deposits. I cannot doubt that the whole of these cases originated in the same poison; therefore, it would be exceedingly difficult to limit this discussion to any disease which produced deposits of pus only. There was a time, which possibly some here may recollect, when this disease was supposed to be confined to one hospital in London. A little more observation taught us that it occurred in other hospitals perhaps quite as frequently. Now you, sir, have told us that it occurs in private practice as frequently as in hospitals. If we extend our observations a little further, the same disease which was once supposed to produce purulent deposits only will, I think, be found to produce a great many other secondary forms of inflammation quite unconnected with the production of pus. Again, with regard to the question of contagion, I think anybody who has fairly given the carbolic acid treatment a trial will see that the wounds under its influence behave very differently from those that are not so treated. It has been said that the carbolic acid prevents the entrance of germs into the wound, but I think we must consider that the same carbolic acid treatment will prevent the contact of many other things—will prevent the contact of dirty fingers, of dirty sponges, and many other accidents. In connection with this I would again refer to my experience in the York Road Lying-in Hospital. When we had the fever so exceedingly rife there, there was scarcely any puerperal fever out of the hospital, and the only two cases that occurred out of the hospital I had attended from the hospital, and I cannot doubt myself that I had been in some measure instrumental in conveying some poison or other to those patients. A case in point occurred last summer at Queen Charlotte’s Lying-in Hospital. I see one of the physicians of that hospital present, and he will set me right if I do not state the facts correctly. Several cases of puerperal fever occurred, and it was proposed to shut the hospital up. Being at the Board one day, I suggested that every nurse, every matron, and everybody about the hospital, should be told to wash their hands in carbolic acid before they touched a patient in any way. The result was that there was no more puerperal fever, and the hospital was shut up at its usual time, which is in September. A very important fact, which, perhaps, may not be known to many members of the Society, but which we have upon very good authority, indeed, is that, if we put a basin of carbolic acid and a basin of yeast together, the carbolic acid will prevent the yeast from rising. It prevents the development of what we call the ferment in the yeast. No doubt the influence is conveyed through the air, but the ferment itself is in the yeast, not in the air, and I have no doubt the ferment may go on quite independently of any atmospheric influence. A very remarkable case was mentioned to me by Dr. Druitt some time ago, where a body had to be exhumed after having been buried for several days. A post-mortem examination was made. He found there was no opening in the
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chest, but the pleura was, nevertheless, covered by a number of small fungi, or something like fungi, a kind of mildew. Here, of course, no atmospheric influence had entered, but there was the ferment in the body. The principal point to which I wish to refer is the distinction between atmospheric influence and contagion. I believe in atmospheric influence; I do not much believe in it as far as the production of pyæmia is concerned, but anything that will prevent the communication of diseased secretions or dirt from one patient to another has a material effect in preventing these morbid septic influences; I cannot myself distinguish septicæmia from pyæmia. I know, of course, as to results, that we can see that one effect is produced in one case, and in another, another; but in any given case, if it live long enough, you can never say that the secondary deposit will not take place. I think, sir, perhaps the Society may be a little misled by your paper, which I have read with great interest. I do not think anybody in ordinary practice will meet with exactly the same cases as you have related. Any physician in London, who commands a large practice, does not practise in his own sphere only. He has patients sent to him from different parts of the country, even from parts out of the country. The area of his practice is very much larger than the ordinary physician's would be. Therefore, for him to bring the cases that occur to him personally as a fair standard of those that would occur to other practitioners may possibly mislead. In my own case, I must say almost all the cases that have done badly have been those where some portion of the cancellous structure of the bone has been injured, or some vein has been wounded. It was asked at the last meeting whether cases of pyæmia did occur after bleeding. Now, we do not see many cases of bleeding; therefore, perhaps, many gentlemen would not be able to answer the question; but one of the first and best marked cases of pyæmia I ever saw was a case sent from Harrow, where a patient had been bled in both arms. He was in great pain; he constantly moved his arms about; and a few days after his arrival in hospital symptoms of pyæmia set in, and there were as well marked secondary deposits as ever I saw, preceded by inflammation of the vein. The vein did not close. Something entered it, as I suppose, and the inflammation ran up the vein, and was succeeded by secondary deposits. Another case, which made a great impression upon my mind, was one where the old operation of haemorrhoidal tumours was performed. Most gentlemen will recollect that a needle, armed with a double ligature, was passed through, and the ligature tied upon both sides. Now, if it should so happen that the needle goes through a vein, the vein is necessarily held open and is bathed in the secretions of the part. In the case to which I refer there were secondary deposits exceedingly well marked, not in the general system, but in the liver, showing that the poison, whatever it was, had gone through the superior haemorrhoidal vein. The few observations which I have made will, I hope, induce those gentlemen who are yet to speak to inform us whether they think that there are different forms of septic influence, which, either rightly or wrongly, we are in the habit of classifying under the collective term pyæmia.
Mr. Erichsen: I cordially agree with a great deal that has fallen from Mr. Lee; but, without going into the general subject that he has propounded, it appears to me the distinct issue to which the Society has been challenged by your address relates to the frequency of pyaemia in private practice, perhaps in relation also to its comparative frequency in hospital practice. Before saying anything upon the subject, I think it would clear the ground very materially if we were to have some distinct understanding as to what is meant by the term pyaemia in the present discussion, because it appears to me that the word is used in the most elastic manner, as referring to every possible kind of blood-poisoning, and not only so, but also including abscesses of various kinds, that appear to me to be entirely of a local character, and to have no reference whatever to blood-poisoning or to any other constitutional origin. I regret that I had not the advantage of being present when you delivered the very admirable address, but I have studied it as it appeared in the journals, and I presume I may take the reports there as being correct. In reading the address, which, I take it, was intended to prove the frequency of pyaemia in private practice, I was struck first of all by this circumstance, that a great number of the cases that you related as cases of pyaemia I should not have considered to have been cases of pyaemia. There is the essential primary difference of opinion between us as to the very nature of many of these cases. In speaking of pyaemia, and in any remarks that I shall make upon it, I shall consider it as a disease which is now recognised by pathologists as having its essential origin in a venous thrombosis, giving rise to embolism and to metastatic abscesses, in the lungs chiefly and elsewhere, as the results of these embolic deposits. If I look to the cases that you have related, and go through them, I see two or three that appear to me not to be pyaemic abscesses at all, but to be cases of simple abscesses of a lymphatic character, or arising from a direct continuance of irritation along the absorbents or the tissues of a limb. There is the case of an old gentleman, eighty years of age, who had a warty tumour removed from the heel, and who died from a large abscess that formed in the same thigh. Now, that appears to me to be an abscess such as one might have in the axilla after a puncture of the finger, or any other injury about the hand. There is a case also of a young gentleman who ran a splinter into the ball of his great toe, and had an abscess in his calf. It seems to me that that case also is not a case of pyaemia, but simply a case of localised abscess depending upon local irritation. There is also a case of a young lady who, after confinement, had a large intrapelvic abscess. Then there was the case of the young gentleman who had gonorrhœal rheumatism, and who got mischief in connection with it. Now, with regard to the subject of gonorrhœal rheumatism, no doubt the case is extremely interesting, because it seems to be one of those cases that bear very much upon a question that has not yet been very clearly elucidated, that is, the connection between urethral inflammations of any kind and blood-poisoning. There is some connection in some way that we can only recognise
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by the effects of the cause, of which we are entirely ignorant, that brings many of these urethral irritations very much into the category of true pyaemic conditions. We find, for instance, that there is no condition in surgery in which a more severe rigor takes place, occasionally followed by sweatings, than after the passage of an instrument down the urethra. We find, also, in these cases of urethritis, whether gonorrhœal or not, that blood-poisoning is very apt to ensue; not pyaemia, but blood-poisoning of some kind or another, somewhat allied, in fact too closely allied, to pyaemic conditions. It seems to me that that case is one of these remarkable instances of blood-poisoning. Then there are the cases of the two ladies who died after the removal of the breast. They had erysipelas, and it is said that pyaemia set in. I have fortunately never seen a patient die of pyaemia after the removal of the breast. I have seen women die of erysipelas occasionally. I have known ladies, in private practice, to have erysipelas after removal of the breast, but I have never known a case of pyaemia. If I appear to criticise your cases a little freely, I am sure you, sir, and the Society, will feel that I do so with the utmost respect for your position in the profession, and for the interest of your paper; and it is from the very interest of the paper, and the importance of your position in the profession as the author of the paper, that I am led to speak perhaps a little freely about it, because the whole matter is an extremely important one. Well, in one of the cases of those two ladies who died, it is said that pyaemic symptoms set in, but we are not told what those symptoms were. In the other case it is said that pyaemia set in. I confess I look upon erysipelas as a totally distinct disease from pyaemia. I think it is as distinct from pyaemia as scarlet fever is from smallpox, and that there is no connection whatever between the two, except a remote one in cause. Just in the same way as we find, from a stinking drain, one person will get scarlatina and another typhoid fever, so, from the same condition, one person may get erysipelas and another pyaemia. I do not think that the two conditions are interchangeable, or that they are at all allied, except remotely in their cause. No doubt in some cases of erysipelas, where there is a putrescent slough and filthy pus accumulated under the skin, you may get blood-poisoning; but in ordinary erysipelas, and where every attention is paid, as doubtless it was in those cases, I confess I should rather look upon the case as one of death from erysipelas than as a case of death from pyaemia, I cannot but think that there is one suspicious circumstance in connection with these two cases; that is, that they occurred within a month of one another, as if there were some medium of infectious intercommunication between the two patients, giving rise to what is undoubtedly an infectious disease, erysipelas, although it is very doubtful indeed, and there is no proof whatever that pyaemia is an infectious disease. These cases would also lead me to remark upon, if I may use the expression, the somewhat unsatisfactory nature of private cases, as cases on which to found anything like clinical or pathological observations, because we have no post-mortem examination, if I recollect right,
given in the address you have delivered to the Society; and I take it that it is extremely difficult in many cases to diagnose, during life, pyæmia from some of those other varied morbid poisons, and that it is only a post-mortem examination that will ultimately and definitely reveal the true nature of the disease. Then there is another point that struck me as very remarkable in the address. It is that you and I can look back to somewhat more than a quarter of a century's practice in London, approaching to a third, and yet, in a paper professing to show the frequency of pyæmia in private practice, there were only six cases during the very long period following operations, and out of those six, two were, in my opinion, certainly doubtful cases, appearing to me to be rather cases of erysipelas than of pyæmia. Now, if I wanted any fact to prove how comparatively rare true pyæmia is in private practice to what it is in hospital practice, I could scarcely get a better illustration than that afforded in this very paper that you have read before the Society. I have never seen a case of true pyæmia during the whole of my private practice in London. I know such cases occur; they do necessarily, for I have heard of them. I know that there is such a thing as self-infection in these cases; that you get putrescent sores, as Mr. Lee just now described so admirably. In a badly tied pile you may get an open vein into which pus may infiltrate itself, and in that way you may get pyæmia. Necessarily, you may get the pyæmia out of hospital as well as in it. I can only say, so far as I am concerned, in my own private practice I have never met with a case of true pyæmia after an operation. I mean pyæmia that is recognised by pathologists as such; pyæmia that is described by Virchow, depending on venous thrombosis giving rise to embolism, and leading to metastatic abscesses in the internal organs. That condition of things is, undoubtedly, extremely frequent in the London hospitals, and not only extremely frequent, but extremely fatal in them. Whether it is the result of air, or whether air is merely the vehicle of contamination, is the disputed point. I believe that air is the mere vehicle, or conduit, by which the infecting germ finds its way into the body. So far as my experience of pyæmia in hospitals is concerned, I may say that I have found it to be, as everyone else, I believe, has, most common after injuries and operations by which the medullary canal of a bone is opened, especially in the case of bones of the lower extremity. I am speaking on my own experience solely when I say that after operations on bones in the lower extremity, and after amputations in which the medullary canal is opened, it is by no means uncommon. I believe position has something to do with it. In the upper extremity the bone operated on is frequently somewhat dependent, and the pus gets more ready exit from the wound, and does not enter the medullary canal. In the lower extremity the stump is often raised, the thigh-stump often cocks up of itself, or the limb is raised, and there is a percolation of pus backwards into the medullary canal, into which venous sinuses open, and in that way infectious matter may get into the system. However it occurs, the fact is that operations
upon bones of the lower extremity are more liable to be followed by pyæmia than operations on the bones of the upper extremity. So in operations on the face, and in injuries to the head. I have no intention of entering into the general question of frequency of pyæmia in hospitals, or its cause there, because I have lately, quite unconnected with any discussion taking place here—as a mere coincidence of time—entered into that pretty fully elsewhere, and I should only be repeating what I have there said. My only object, in making these comments, was to endeavour to bring the discussion to the point that, it seems to me, is at issue, as to the frequency of pyæmia in private practice, and as to how far—I say it with all deference to you—the cases you have related are true cases of pyæmia, or whether many of them are not cases of abscess of other kinds than pyæmic abscess.

The President: Before proceeding further, I may as well take up the subject in reference to what Mr. Erichsen has just now very ably stated, that is, as to whether the cases I have related are what are generally considered to be pure cases of pyæmia. I must confess that Mr. Erichsen has thrown some doubt upon cases as to which I have no doubt whatever myself. The first case he mentioned was that of an elderly gentleman who had a small wart removed from the heel. Now, if Mr. Erichsen had read the case very attentively, he would have seen that I was very particular in stating most assuredly and most positively that there was no ground for the supposition that there was any inflammation whatsoever in the cellular tissue, between the heel and the thigh, or that inflammation had run up along the cellular tissue, as in the example which Mr. Erichsen gave of abscess in the axilla. In that case it is specially mentioned that the small wart removed was from the heel, that it was a superficial wart. It is also specially mentioned that the abscess in the thigh was a deep one, and in connection with the thigh-bone, and I do not know what continuation there could have been between the two. The abscess was followed by a rigor, and it came on three weeks or a month after the removal of the small wart. The gentleman had been on two different occasions walking about, and he was going to resume his avocations when the rigor supervened, and then came this large deep-seated abscess in the thigh in connection with the thigh-bone—an abscess extending from the knee-joint as high up as the hip. I cannot look upon this case as similar to the one Mr. Erichsen just now mentioned as a case of injury about the hand, and subsequently an abscess in the axilla. To my mind, the case I have related is certainly a case of pyæmia. Then there are those two cases of the removal of the breast, with regard to which Mr. Erichsen very properly, and with very great stress, says that the patients may have died of erysipelas—the facts are these. I may mention, before referring to the cases, that I found my paper was becoming over long. I did not wish my communication to exceed half an hour, and I therefore could not give all the details that might be desired, and I simply wrote down that symptoms of pyæmia did supervene. But in these cases, it is specially mentioned that the erysipelas was very slight in-
deed, and that the erysipelas never spread. It was never sufficient to kill the patient. The reason of my putting down that symptoms of pyaemia supervened was this. There was rigor, there was sweating, there was erysipelas. The erysipelas was very slight round about the wound, and it never spread more than, perhaps, to the size of the palm of one's hand. Some little time afterwards there came some other rigors, yet the erysipelas did not spread. To my mind the symptoms were simply those of pyaemia. It is quite true that there was no post-mortem examination. Unfortunately, one cannot get post-mortem examinations in private practice; and there, I must confess, lies the fault of the paper, but that is no fault of mine. I think, upon the whole, the cases are quite sufficient to warrant me in saying that they were cases of pyaemia. There was great difficulty of breathing. The patients died finally with difficulty of breathing after constant rigors and great sweatings; yet the erysipelas did not spread, and no infiltration took place about the wound. Then there is the case of the gentleman who had an abscess in the calf after a splinter in the foot. There was no continuation of anything like inflammation from the great toe to the sole of the foot, or from the sole of the foot to the calf of the leg. The abscess that occurred in that young gentleman's leg was a deep-seated abscess among the muscles of the leg, and there was no continuation of inflammation of any kind. They were not superficial abscesses along the cellular tissue. That young gentleman had rigors and sweatings; and when he left his bed, at the end of four or five months, he was reduced to a mere skeleton. I have never known that state of things produced by erysipelas, or by inflammation spreading from one part to another; therefore I put that down as a case of pyaemia. Another case to which Mr. Erichsen alluded, and upon which he laid great stress, was the case of the young lady who, after confinement, had an abscess underneath the shoulder. That, to me, was certainly a case of pyaemia. What else could it be? In three weeks or a month after the confinement this abscess occurred, and the patient had rigors. What else could it be but a case of pyæmic abscess? I think, with regard to those five cases that Mr. Erichsen has mentioned, I am warranted in looking upon them as cases of pyaemia. I think we are also warranted in looking upon the case of gonorrhœa as a case of pyaemia, for the case occurred thus. About a fortnight after the patient had gonorrhœa, there came on symptoms of gonorrhœal rheumatism. These symptoms subsided. Then, in about a week or ten days afterwards, there came severe rigors and severe sweatings, and there came that peculiar earthy appearance of the skin that one so constantly observes in pyaemia, and that is so characteristic of it. After that there came a very large abscess in the sterno-clavicular articulation; and then, after that, a large abscess in the hip-joint and round about the hip-joint. If this is not a case of pyemia I do not know what pyaemia is. There was pus in the urethra, and subsequently there was pus about the sterno-clavicular articulation, and pus about the hip-joint. In addition to that there were all the symptoms of pyæmia, and especially that earthy look that we see so
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commonly about the skin. Those are the cases to which Mr. Erichsen has alluded. No gentleman could have done it more courteously, and I am sure the Society and I are very much obliged to him for raising the point as to whether those were cases of pyaemia or not. We are much obliged to him for coming to criticise the cases, and for the manner in which he has done it. Still I must adhere to my opinion that these are cases of pyaemia.

Dr. Hicks: May I be permitted to say a few words in continuation of what I previously said? The only point that has been brought forward is this—Do cases of pyaemia occur in private practice as well as in hospitals? Now, unless we take in the whole circumstances attending the ill-doing of our patients, we can never clearly understand the causation of the results. The first point to bring forward is—Can irritating matter, or, as I would say, pyaemia, arise from injury without any wound? I think we all agree that there are cases in which that occurs. Our systems are differently constituted. Some have less tendency to recovery than others. The next point is whether open wounds themselves can produce irritating matter which shall be absorbed into the general system and cause those results with which we are acquainted. Of course, that is well understood. Then the question comes—What is that irritating matter? What is that poison? How does it act? Then we have to come to another question—What tends to produce those ill results? Your paper, sir, touches upon the same ground as one which I published some time ago in the Obstetrical Transactions. I took as many private cases as I could—not hospital cases, all simple cases of delivery—and I collected eighty-nine. They were cases of puerperal disease, commonly called puerperal fever. I analysed them as to their causation. I took the trouble to inquire, together with some gentlemen who assisted me, as to the surroundings of every one of those patients. I omitted every case as to which I had not made strict inquiry. I find, quoting from the paper, that, out of the eighty-nine cases, sixty-eight had been exposed to animal poisons. Upon further analysis I find that thirty-seven of those had been exposed to scarlet fever—manifestly, distinctly, without any doubt; six to erysipelas, seven to diphtheria, and two to typhus. Nine had decomposition of the uterine contents. There were a few other cases arising from various causes, but all of them connected with animal poisons. There were twenty-one out of the eighty-nine as to which we could find no history of that kind; some other influence, no doubt, deteriorated the general system, and so producing conditions of puerperal fever. If that be the case in puerperal fever, may it not be the case in private practice, and in operations in general wards? If you have attacks of diphtheria running through a ward, you cannot separate the one from the other as to causation, though you may have different symptoms.

Mr. Hulett: The course of the debate seems to me to have amply justified the suggestion I threw out, that we should have a clear and distinct understanding as to what we mean by the term 'pyaemia.' For it appears to me that almost all the speakers hitherto have used inter-
changeably the terms septicaemia and pyæmia; or, at all events, have made those two so thoroughly overlap and intertwine that it is hard to know what they mean. In particular I would instance Mr. Lee and Mr. Erichsen. Both in his recent public writings, and also this evening, Mr. Erichsen seems to me to use these terms in a convertible manner. I could have wished that some one much more skilled than myself in the matter should have attempted to lay down some sharp line of definition between these two conditions. It may be exceedingly difficult to do so in practice at the bedside, because these two conditions may co-exist in the same patient; but it seems to me, from experiments in the lower animals, we can imitate both pyæmia and septicaemia. Having their trains of symptoms so separate and distinct, there can be no doubt whatever of the perfect distinctness of the two affections. If, for example, you take perfectly fresh pus—not putrid in the least degree, pus that contains no flocculi, perfectly limpid and pure pus—and inject it into the venous system of a dog, you will get a great rise of temperature. We get shivering and malaise, and, after a certain time, the dog's recovery. You may inject a considerable quantity of pus, and you may repeat the experiment again and again, and the dog may recover. That I take to be simply pyæmia. If, on the other hand, you take pus which is no longer perfectly limpid, but a little flocculent—not putrescent—which contains particles of such a size that they may stick in the vessels; and, if you inject that pus, then you get the same primary symptoms, shivering, malaise, and rise of temperature, but you get abscesses and purulent deposits in different parts of the body, the internal visera, and so on. In fact, you may imitate simple pyæmia and the multiple pyæmia which we get in a human subject. On the other hand, if you take putrid matter—whether vegetable or animal—for instance, if you take cabbage leaves and let them rot thoroughly in a solution, and then get this foul stinking cabbage water, if you clear away all the solid particles as far as you can, you will, by these means, get a train of symptoms which may be free from metastatic abscesses. You get a high temperature; you get shivering—not always, but very frequently—you get vomiting, purging, collapse, and the rapid death of the dog. In the human subject there is something of the same kind, though most instances of septicaemia which come before us run a less rapid course, and the symptoms are less violently prominent. There is no doubt whatever, as Mr. Erichsen said, and as Mr. Lee, Mr. Hutchinson, and others have said, that in the case of multiple abscesses we find embolism. Conceding that—and I suppose everyone will agree to it—then comes the question which we must push a little farther. It is not every embolon that will give rise to one of these abscesses. Every physician must have seen many instances of embolism where some little piece of fibrine has been washed off some valve of the heart, for instance. We are all familiar with these instances, but we do not get pyæmic deposits in those spots. We have an admirable example in the embolism of the central artery of the retina, where you may watch the whole process during life; but you do not get pyæmic
abscess. It is not a mere fibrinous clot plus red and white corpuscles, but there is something in the clot which determines that there must be something infective. The question is,—What is that infective matter? We can produce in the lower animals, by the introduction of pus, all the symptoms of pyemia. It seems probable—that we cannot speak with absolute certainty—that this embolon must carry pus with it. You cannot get pyemia without pus. The question may be,—How does the pus get into the embolon? That is a question concerning which I feel considerable difficulty; nor do I at all clearly see, if the views of recent pathologists be correct, that pus is nothing more than strayed white blood, gone astray out of the vessels, why, when it gets back into the vessels, it should be so exceedingly naughty and set up all these disagreeable symptoms. That is a matter of great difficulty. Granted that metastatic abscesses depend upon embolism, are there any conditions which may predispose to the formation of blood-clots, of which these embola consist? Here we come to some remarks bearing on this point made in the earlier part of the debate. It seems to me there can be no doubt whatever that conditions which greatly depress the patient must conduce—indeed, we know that they do conduce, in point of fact—to the formation of clots of blood in the veins. Everyone will concede that everything which depresses the heart’s action leads to a great loss of blood, and we know that pyemia is more frequent, if you take a large series of cases, in the case of patients who have lost great quantities of blood, or are debilitated from other causes, as in the case of persons greatly morally depressed. We have an admirable example of this in any large wars. We saw it in the Crimea, and we saw it again in the French campaign. Although pyæmia was abundant on the German side, it was more abundant on the French side, where the French soldiers were thoroughly depressed. Any morally, as well as any physically, depressing cause is quite sufficient to co-operate in that way. There was a remark, which struck me very forcibly, made by Mr. Hutchinson, that in his judgment (if I understood him rightly) foul air was in no sense a condition of pyæmia. I think he alluded to his experience in the Metropolitan Free Hospital, and spoke of the hospital as being badly constructed, and the wards ill ventilated, and the atmosphere not what atmospheres of hospitals ought to be; yet, curiously enough, they were not much cursed with pyæmia, while in another hospital better circumstanced, the London Hospital, he had a large amount of it. He said, I think—unless I misjudge him—that foul air had nothing to do with it. Here, again, I would refer more particularly to military experience. We must have seen—those of us who at any time in our lives have had such experience—that where large numbers of men have been crowded together, and a thoroughly foul atmosphere had been produced, that has acted in a very depressing way. It may not be through the actual infection of putrid material directly into the wound, and so on, but by breathing foul air, and in those indirect ways you get the patient thoroughly depressed if you surround him with putrefactive material, though the putrefactive material may not be in itself the essential agent
of pyaemia. It seems to me that in that direction we may find an explanation why pyaemia is sometimes much more frequent in newly opened hospitals and in new wards. That is a thing which must have struck everyone. I think Mr. Holmes made the remark that in the Convalescent Hospital he had seen cases of pyaemia as frequently as at the old St. George's Hospital. I think, also, it was remarked by one of the other speakers that the same thing had happened in St. Thomas's Hospital. No one can have been in an hospital, or in a building where whitewashing or plastering has been going on, without being nauseated almost with the stench that comes from the putrefying size and other animal matter in the mortar, particularly the size in the whitewash, which is horribly offensive. Long after the smell has gone, so that you cannot detect it with the nose, still, if you are exposed a night or two to that kind of atmosphere, it may have a very depressing effect upon you. A comparison was drawn in the last debate by Mr. Hawkins between old and new hospitals in towns. Mr. Holmes shortly afterwards remarked on the necessity of drawing no comparisons except between hospitals which were tolerably similarly circumstanced. Mr. Charles Hawkins' remarks referred to the old and new King's College Hospitals; and it seems to me that you can make no comparisons between the two buildings whatsoever. Old King's College Hospital, in the memory of some of us, was a parish workhouse converted into a hospital under considerable difficulties; and, to a very great extent, all those measures which we should consider essential to the well-doing of a hospital were probably ignorantly disregarded. For instance, under the male accident-ward were large cellars piled from floor to roof with bones taken out of the neighbouring churchyard. The operating theatre was for many years the post-mortem theatre, and was separated only by a few yards from the male accident-ward, so that the conditions were essentially those which would induce infections of all kinds. In the new King's College Hospital there are no circumstances of the kind, so that it seems to me the conditions are so different that you cannot very well compare one with the other.

Mr. Savory: When, sir, the effects of this disease were first of all studied, attention was directed wholly to the investigation of internal causes, and the result was the theory embodied in the term pyaemia; and then, as if in recoil from this error, observation has turned entirely to the search after external causes, and the result of that has been the views embodied in the term hospitalism; and now it would appear the last error is worse than the first. Are we in a position, at the present time, to give any sort of answer to the question, 'What is the cause of pyaemia?' May we go so far as to say it is due to the action of poison in the blood; that the poison is of a septic nature, and produced by or associated with putrefaction, decomposition, or change? I dare not, and need not, now trespass upon the attention of this Society in proof, at length, of that doctrine; but it may suffice to say that, experimentally, we can take a decomposing fluid, inject it into the blood, and produce all those effects which are generally recognised as the effects of pyaemia.
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But now, confining attention entirely to the clinical aspect of the case, under what conditions is such a poison formed, when does it exist, and under what circumstances does it enter into the blood? It is formed during the decomposition of animal fluids, animal fluids in connection with the living human body. Now, animal fluids may decompose under varying circumstances; any dead or dying, or unhealthy animal fluid may decompose, and the difficulty is not to recognize the fact that it may decompose, but to prevent such decomposition from occurring. But that decomposition is hastened unquestionably by exposure. In what way that exposure may act is altogether another question within this one, but unquestionably free exposure to air favours the decomposition of animal fluids. But, most of all, the introduction of other matter in a state of active decomposition increases vastly the rate of decomposition in the original fluid. We know very well it is an established law that, when a substance in a state of change is brought into contact with another body of unstable equilibrium, the change of the first is communicated to the second. I am anxious now to avoid all guesses at how this may occur, and will speak only of the facts which lie visibly before us; and if we may recognise these facts as I have stated them, surely there is no difficulty in understanding, first of all, how pyaemia may occur anywhere, attack anyone, even under the most perfect hygienic conditions; that, nevertheless, it would be specially prone to occur where persons are concentrated together, and most of all where persons are concentrated together having open wounds, as in hospital wards; and, which is most important, if we are able thus to recognise the cause, we may have good hope that, by having our eyes thus opened, we may be hereafter able to prevent the operation of these causes, and to reduce even our hospital wards to places as healthy as any private apartment. But there is another question which seems to me, just at present, to be altogether overlooked. We constantly see decomposing fluids in contact with raw surfaces. We constantly see wounds bathed in putrid fluids, nay, we constantly see putrid fluids shut up in cavities, pent up under heavy pressure, as, for example, where there is a foul stinking abscess by the side of the rectum: how is it that these patients escape, as they constantly do? How is it that, in these cases, it is the rule, and not the exception, for people to escape blood-poisoning? Observe that this very fluid, which lies in contact with these raw surfaces, which is bathing wounds, which is thus pent up in cavities in the body, can, by a syringe, be introduced at once into the blood of an animal, and produce the most terrible forms of blood-poisoning. How is it? Surely there is another cause in operation, and, I venture to think, we shall find hereafter the solution of the cause in what may be termed the dialysing property of animal membranes. The best work done in late years in this direction are those experiments which Billroth and other persons have performed, showing that where granulations are healthy, when they exist in their integrity, they offer a decided obstacle to the passage of the material from without to within; but when these granulations become destroyed, either mechanically or by other means, whereby they are
brought into an unhealthy state, these fluids pass with fatal facility through them, and so gain entrance into the blood. Whether that view shall hereafter turn out to be correct or not, the fact remains strong enough that we may have fluids, which fluids can be shown by experiment to be potent enough to produce the worst forms of blood-poisoning, in contact for hours, days, or weeks with living animal surfaces, and yet the persons so carrying them about are wholly free from any symptoms of blood-poisoning. Can we go further than this? I daresay many present will say we cannot go as far; but can we go farther?—what has pus to do with pyæmia, for instance? As we have been already told, the not unhealthy pus, normal pus, may be injected into the circulation, and you do not get, as a necessary result, by any means, pyæmia. But pus is an animal fluid, which, of all fluids, is most likely to be found in contact with wounds, and, obeying the law of exposed animal fluids, is exceedingly likely to undergo decomposition; and then pus, undergoing decomposition, will produce pyæmia, as any other fluid will produce pyæmia which is in a state of decomposition. But I take it that there is no special relation between the production of pus and pyæmia, as cause and effect, beyond this, that pus is the most frequent of animal fluids formed; and being, like other animal fluids, prone to decomposition, is most likely, under these circumstances, to be the means or agency through which the poison is introduced into the blood. And what has phlebitis to do with pyæmia? Have not we a larger error here than the view which regards pus as the cause of pyæmia? How does phlebitis produce pyæmia? The view which gave rise to the notion that phlebitis was a necessary link in the chain of causation was that, under the influence of inflammation, the lining membrane of veins forms pus. But does the lining membrane of a vein form pus? Can we produce pus by inflaming a vein? Will not the lining membrane of a vein prove most obstinate under all circumstances of torture, and resist steadily the formation of pus? In point of fact, you do not get pus in the interior of a vein; you get a puriform, but not a purulent fluid; but this puriform fluid may be potent to produce pyæmia; if decomposed, it may produce pyæmia as any other decomposing substance may; and, if it be formed in the interior of a vein, and pass onward into the blood, then it may be one among the many causes of pyæmia, but not standing to pyæmia in the relation of a special cause. But then, supposing that phlebitis is not necessarily associated with pyæmia as cause to effect, how about thrombosis? We have been moved a step further back in this direction, I think, by Virchow's great theory, that the disintegrated fibrine which was heretofore mistaken for pus, the puriform fluid in the interior of the vein, passing into the circulation, produces pyæmia. As Mr. Hulke has stated to-night, we are perfectly sure of the fact that you may macerate fibrine, disintegrate it, inject it into the circulation over and over again, and get embolism and the effect of embolism, but not abscess as a consequence; most assuredly, you do not get pyæmia as a consequence. You may produce, up to a certain
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extent, the local effects of pyæmia by the injection of dis-integrated fibrine, but you cannot produce that disease, which is characterised less, perhaps, by local than by constitutional symptoms. How is it, then, if you may have—as undoubtedly you may, as everyone knows very well—pus daily, hourly, without pyæmia, phlebitis without pyæmia, thrombosis without pyæmia, pyæmia without pus, pyæmia without thrombosis, pyæmia without phlebitis? And, if you may have thrombosis and phlebitis without pyæmia on the one hand, and pyæmia without thrombosis or phlebitis on the other hand, are we justified any longer in connecting these together as holding a special relation to each other? How is it that, undoubtedly, in many cases of pyæmia after death, we do find that secondary abscesses are deposited in various parts of the body; we undoubtedly do find affections of the veins which are characterised by terms such as phlebitis and thrombosis? Because, no doubt, these are the local and pyæmia is the general effect of the introduction of the poison into the blood, and, as Mr. Callender has very well pointed out in his article on the subject, the veins act in this manner as channels to conduct the poison upwards, on the outside as well as on the inside. I really wish those who are continually asserting the constant relationship between phlebitis and pyæmia would tell us how it is, supposing even the veins remained open, as they are said to do, but which I do not think they do, in the case of bone which is operated upon—how, supposing those veins remain open, they minister to the introduction of a fluid, that they remain vacant spaces? Are they hollow? If not, by what are they filled? And, if they be filled with blood, does the blood remain fluid? Will not the blood coagulate in them? Has not that been a point upon which Dr. Wilks has insisted? and, if so, are they not, to all intents and purposes, occluded, and do not they, in this way, form as difficult an obstruction to the passage of any morbid substances into the blood as any other of the surrounding tissues would do? Therefore, I submit that, while we constantly get these conditions associated together, we yet lack the evidence to establish the relation between them as cause and effect. There is just one other point which I would throw out, by way of suggestion, in relation to this very important subject. If anyone will experimentally macerate fibrine, he will find it will take a very considerable time; it is a question of days to reduce it to such a state of disintegration as may be found in the interior of veins in the case of pyæmia. When we make post-mortem examinations in cases of pyæmia, we often find this disintegrated fibrine in the veins; but the pyæmia has arisen within a space of time after an operation, which, so far as we can judge by experiment—I would not rely too much upon this, but as far as we can judge by experiment, or use any knowledge within our reach—would be before the time that these clots could possibly disintegrate. They disintegrate in the progress of the disease; but I venture to think that there is great difficulty in this objection, that, in the large number of cases where disintegrated clot is found after death in cases of pyæmia, the rigor and rise of tem-
perature which mark the access of pyæmia has set in before we can conceive that the time has come for the disintegration of the clot. If, then, we can throw off these shackles of phlebitis and thrombosis as necessary links in the chain of causation of pyæmia, may we not rise to a larger and a fuller view of the subject, and ought we not to regard pyæmia simply as one form, or phase, or degree of blood-poisoning? Mr. Hulse has asked, with great point, for a definition of pyæmia; and we know very well (and, because I differ from him in this respect, I hold my opinion with great diffidence) that he regards septicæmia and pyæmia as essentially distinct; but I cannot find in what the distinction lies. He asks for a sharp line of distinction between the two. I wish very much he would give us one. Why I regard these two diseases or forms of disease as simply different degrees of one and the same disease is because, as Mr. Lee has remarked, you over and over again find instances in which the one passes into the other. Take pus—you do not want to go to decomposing vegetable or other animal fluids; you may do it with them—but take pus, and with it I could make a case of pyæmia or septicæmia according to order, by the length of time which I kept the pus before I injected it; and I know very well, in experimenting upon this subject, one may produce all degrees of the disease, and may say that the chances of getting secondary abscesses are in direct ratio to the length of time an animal lives after it has become inoculated with the poison. When the poison is thoroughly septic, when you have that terrible substance which Dr. Burdon Sanderson has shown us how to get in the peritoneal cavity of an animal, the blood becomes so poisoned and spoilt that it kills outright, and there is no time for the secondary effects to supervene; but it is in the more gradual cases, where there is time for the secondary effects to supervene, and where the patient passes through this most terrible crisis, that these abscesses form. Avoiding, if I can, being tedious on this matter, and therefore passing over other arguments which I think could be adduced to show that these two diseases are but different degrees of effect of one and the same cause, I would say lastly—and in this, sir, I should have you with me—that erysipelas is but a form of blood-poisoning. I think the evidence is conclusive against its being a local disease; at least, conclusive to my mind. I am afraid I must be defective there, because I am against Mr. Hutchinson. I think the evidence is conclusive, as Mr. Hutchinson would say with me, against its being a form of zymotic disease; and then, if it be separated from the exanthemata on the one hand, and from local affections on the other hand, where are we to place it? I think we must find a place for it amongst the effects of septic poisons. There are three grounds on which I would place erysipelas amongst the blood poisons. You constantly see erysipelas as the effect of the introduction of a poison, as of a poisoned finger. How often do you see a form of erysipelas spreading from that, a form which Mr. Baker has recently described under the name of erythema circens? Again, how often do we find erysipelas alternating with pyæmia? So con-
stantly do we see this, that when, after an operation, a patient has rigor or rise of temperature, I hail the advent of erysipelas as a favourable omen. I am glad to get out of it in that way, for if the effect of the poison be expended on the skin, the internal organs are more likely to escape. But most of all turn to our authors who describe post-mortem appearances of erysipelas, and who have no theory of this sort to support. Look at the description of the appearances found after death in erysipelas, in the article, either in the System of Medicine or the System of Surgery, and there you will find a description of appearances coinciding exactly with the post-mortem appearances of pyaemia. I say, therefore, for the same reason that I should class septicaemia and pyaemia together as but different degrees of effect of the same poison on the one hand, so I would class pyaemia and erysipelas together as different degrees of the poison on the other hand; and, through these, we should run down in a series of still milder cases, such as we see exhibited, for instance, in gonorrhoeal rheumatism. Therefore, I venture to say, in future we should direct our research especially to these matters; to examine again the grounds upon which phlebitis and thrombosis are associated with pyaemia as cause and effect; to see whether we can find any ground of distinction which will justify us in separating septicaemia, pyaemia, and erysipelas into diseases of different classes, and then, lastly, though not least, that in the future investigation we should direct our attention to the operation of internal as well as external causes. Your paper, sir—which we owe to a courage which nothing, I think, but very eminent success in the practice of your art would have given you—is sure to have great influence for good; and if it be the means, as I hope and trust it may, of directing attention with renewed energy to the study of this subject—the greatest, I venture to think, of all subjects of surgery—it will not be hereafter reckoned amongst the least of the many benefits you have conferred upon our profession.

Mr. Callender: I will, if you please, take up the consideration of this subject from the point at which it was left at the last meeting, in order that I may endorse all that fell from Mr. Savory with regard to our indebtedness to yourself for bringing before our notice facts which show us clearly that pyaemia is a disease which is not limited to hospital practice, but that it may occur under circumstances which at first sight might appear to be particularly favourable to its avoidance. And further, with reference to these cases, I should like to say that I think they show conclusively—at all events a considerable number of them do—that this disease must have arisen entirely in private practice, for I observe that they were cases in which the disease had been established before you, sir, had been called in consultation; therefore they could not in any way have been cases infected from hospital wards, the infection being carried from them to private rooms. I think, after the remarks I have heard made on this subject, that this is one of the important points to which our attention should be directed. Now, as you have, by referring to the occurrence of pyaemia in private practice, opened up the whole question of the consideration of this subject, I
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shall make bold to refer generally to pyaemia, and I shall make my remarks, so far as possible, practical and clinical. I shall here—as I think we ought always to refer in discussing such subjects at the meetings of this Society—especially to the question of treatment. Now, when some years ago I was called upon to take charge of a large number of hospital cases, I felt, as Mr. Hulke expressed himself, the necessity of having some distinct plan or scheme by which one might be guided with regard to the pathology of these various affections. I think we shall make but very little progress in the consideration of the subject of pyaemia unless we mark out for ourselves comparatively new routes along which we should travel. It may appear somewhat heterodox if I commence by saying that I think it is extremely desirable that we should ignore altogether the use of the term pyaemia. If by the use of the term we are to understand all those conditions in which, in the course of various diseases, the blood may become so affected that suddenly we may have the formation of abscesses in various tissues, or outpourings of pus from synovial or serous surfaces, then I imagine we must extend the term so as to make it include all those cases in which, in the course of the slow decay of chronic diseases or in tardy convalescence from various affections—such as typhoid fever or scarlet fever—we have the occurrence of these various suppurations. But it must be recollected that in nearly all these cases the formation of the abscesses is associated with a certain amount of inflammatory action; and this is a condition of things which I am not disposed to group among cases of pyaemia. Certainly it does not seem to be allied to those cases of pyaemia which we as surgeons consider to be typical, as interfering with the progress of surgical cases. Pyaemia is also a term misleading, if it is for a moment to be supposed that it is to be associated with the presence of pus in the blood; for nothing is more clear than that this notion, which originated in the early days of the consideration of the subject, is absolutely erroneous, for there is no evidence to show that pus, as such, is ever found to be present in the blood. Further, it is misleading in that it has induced observers to look for certain sources from which such pus might be derived. They have, for example, thought that the pus might come from inflammation of the veins; but I am not aware of any evidence that bears the test of strict scientific investigation which shows that the veins are inflamed, leading up to the production of pus; whereas, on the other hand, there is abundant evidence to show that veins may become filled with clots—such clots too may, as stated by previous speakers, undergo disintegration—may be filled with a puriform, but not with a purulent fluid. The term is further misleading in this respect—that it has induced surgeons to look more especially for an explanation of the occurrence of pyaemia in association with certain affections, particularly in cases in which it is supposed that the veins have lain ready (if I may use the expression) at the bases of wounds to take up any infecting product; as, for example, in connection with various injuries and diseases of bone, in connection also with another class of troubles which occur in the course
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of the puerperal condition following parturition. Indeed, if there is one thing to my mind clearer than another, it is this—in all these cases, as Mr. De Morgan has expressly pointed out, in such wounds, or injuries, or conditions of parts, we have the wounds so circumstanced that they are filled with cracks and crannies in which matter may lodge, and from which it is with difficulty dislodged—where it may putrefy and give rise to the serious affections with which we are, unfortunately, too familiar. Now, when I came, in the interests of the patients submitted to my care, to look at these considerations—when I made up my mind to ignore altogether the use of the term pyæmia in reference to these diseases—I had to consider how best I might group together, so that one might have certain distinct landmarks which might be followed in the treatment of the cases, the various pathological conditions which past experience seemed to me to associate with the various untoward results occasionally following the treatment of surgical cases; and I came to the conclusion that on the whole it was desirable to group the cases into three distinct sets. In the first group of cases I placed all those which might be spoken of as instances of primary septicaemia; cases, that is to say, in which the poisoning of the blood—you will pardon me if I use homely expressions in reference to all these matters; I think it is very desirable to speak in plain language regarding them—is due to the action of those acrid and irritating fluids which are poured out into the wound during the first twenty-four or thirty-six hours which follow its infliction, substances which are so acrid and irritating that if they are lodged in the tissues, lodged in the wound, if an escape or vent is not provided, they rapidly produce most serious constitutional symptoms. In the second group of cases I place all those that may be spoken of as instances of secondary septicemia; cases in which the poisoning of the system depends upon the decomposition of residua, whether those residua happen to be sloughs or matter which is passing into a putrid state. And it must be recollected, respecting either of these two conditions, that we may have either a small or a large quantity of poisonous material present in a wound, or we may have it passing into the system of one who is weak and feeble, and readily succumbs to its action, or through the system of another who is robust and strong, and resists its influence. Thus we have a great variety of modifications in the conditions, appearances, and progress of the cases which we have to treat. In addition to the two preceding classes, there is a third set of cases to which I should like especially to refer, which I would speak of as instances of thromballosis. We are all aware, as a previous speaker, I think Mr. Bryant, pointed out, that there is a condition extremely apt to occur in connection with con-
tusions—a condition in which the veins become filled with clots. These clots frequently undergo disintegration. This disintegration may occur in the course of chronic diseases. I have known it occur in cases of phthisis, but such occurrence is comparatively rare. On the other hand, when the system becomes poisoned through some septic influence, the change produced in the fibrine is such that, deposited in the veins,
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it rapidly does undergo this disintegrating process. When this disintegration has been established, the disintegrated fluid, after a certain time, passes into the general circulation, lodges at various points in the tissues or organs of the body, and there produces what I should prefer to call secondary deposits. These attract around them fresh coagula; these coagula again disintegrate and break down, and so we have formed, not abscesses, but softening, disintegrating masses of fibrine. And here I think is the material distinction between the cases I refer to and those I set out by speaking of as instances in which, in the progress of certain chronic diseases, abscesses are formed in various parts of the body. In the cases I now allude to we have secondary deposits formed without the inflammatory process, whereas in the other we have the abscesses invariably associated with local inflammation. Now, I should say, further, that this condition of thrombollosis must not be confused (and I am sure we require a distinct term to indicate it) with the condition which we have heard spoken of under the name of embolism. We are, of course, all familiar with the fact that the vessels may have clots formed within their canals. We know that these clots, having a bloodstream washing against them, may cast off certain portions. These portions may be floated through the circulation, and may lodge and establish a blockade at certain points. But there, so far as the clot itself is concerned, the change ends. The clot does not undergo disintegration, but remains with the blood as fibrine. Fresh clots form, which are also fibrinous; but I do not understand that in these cases of embolism those disintegrating processes ensue which, I think, are characteristic of the condition spoken of as thrombollosis, a condition which may occur independently, but is frequently mixed up with those two conditions that I have referred to as instances of primary and secondary septicemia. Further, I may say that, in just outlining the main points of an important pathological subject such as this, it is quite impossible to take into consideration various other matters which fringe round the subject. For example, we may have cases of erythema; we may have various forms of erysipelas. I have no doubt myself that these are, to a certain extent, connected with the greater changes to which I have been referring. What I wish to place distinctly before the Society is these three chief changes, because I think they lead us up to a very accurate view of the sort of treatment which, in the management of these affections, we ought to adopt. Passing on to the consideration of this question of treatment, I suppose most members of the Society will be at one with me when I say that very little can be hoped for from treatment in these cases when the disease is once fairly established. It is quite true that, in certain cases, a comparatively robust patient may drift through the affection, but I doubt very much whether any treatment which has hitherto been recommended has, except in the general sense of good nursing and good food, been of any avail. This leads me to speak strongly as to the importance of what I would term preventive treatment. I shall not dwell long upon the subject, because recently I have had occasion to refer to this treatment
of operation and other wounds; but I may point out what I consider to be the leading features which should guide us with reference to this treatment; and first, and I think I am justified in saying most important of all, I would put the question of drainage, so that all those acrid and irritating substances which are effused after the first few hours, and all those materials which may cause local and constitutional symptoms to arise, may be carried out from the wound. Next to this I place cleanliness, which goes, to a great extent, with the drainage, to which I have already referred. I may also speak of the use of those remedies which, of late, have been much spoken of under the term of antisepic agents. Beyond this I would refer to the ventilation of the wounds. It is, I conceive, absolutely necessary, in all these cases, that the air about them should be comparatively pure and clean. Then, again, I refer to the isolation of wounds. I do not myself believe in the contagiousness of these affections, except in the broad sense in which one may understand that the patient is in unfavourable circumstances when he is in a polluted atmosphere, especially polluted by excreta or effluvia from other pyæmia cases, or in the gross sense in which one may understand that a nurse may carry poison from one wound to another. In that sense I believe that these affections may be looked upon as contagious; but, with ordinary care and ordinary precaution, I do not believe they can be propagated by any influences short of those to which I have referred. Then, with regard to the question of treatment, I would say that I think we cannot too highly rate the importance of rest. And, if I may venture to throw out the suggestion, I would say that there has been a failure in this direction, which has given rise to the greater number of mishaps that have occurred, especially after amputations of the lower extremity. From the apparent necessities of these cases, from the dressings which they require, the parts have been disturbed, and not allowed that rest which, during the first four or five days after operation, should be invariably provided. With reference to this point, I do not know whether the members of the Society were attracted by a statement made by Mr. Savory at the last meeting (which I consider to be a very important one). He made it when speaking of the dialysing property of animal membranes. He thought it was not at all improbable (and I quite agree with him with reference to the probabilities of the case) that in all cases the granulations on the surface of wounds form a sort of shield or protection to the wound, and prevent the passage into it of septic agents; but that when, under various circumstances, these granulations are injured, when, more especially by the unrest of a wound, for example, the unrest to which amputations of the lower extremities are commonly subjected, these granulations are broken through, fissures are formed in the wound through which septic matters may filtrate, so as to pervade and poison the system. With these views with regard to the pathology of the affection, and holding strongly these opinions with regard to the preventive treatment to be adopted, all the patients under treatment in my wards have been rigidly disciplined during the three years I have filled
the office of surgeon to our hospital. I will not venture to intrude upon
the Society with the results of my own experience, but I may be per-
mitted to make this one statement with regard to those results, that,
during the three years to which I have referred, out of a great crowd
of patients under treatment in wards, there have been but two cases of
septicaemic poison, and but two cases of thromballosis; and I may say,
as it is possible that I might commit an error by omitting the statement,
that we had not one death from erysipelas arising within the wards of
the hospital. I cannot but feel strongly on this subject. I do believe,
and I can only trust that my belief will be confirmed by the experience
of others, that we have it in our power to control these affections. I
would not say that these diseases can be blotted out, because we know
that, with all the care, and with all the supervision lavished upon such
cases, errors of omission and commission will occasionally occur; but I
do believe that we have it in our power so far to suppress these affec-
tions that, practically, they may cease to embarrass us in the treatment
of surgical cases.

Mr. BARKWELL: I am sure no one in this Society can have listened
with more interest to the remarks of Mr. Callender than I myself have
done, and also to those which fell the other night from Mr. Savory.
But I cannot help being struck by the fact that most of those
whom I have heard speak at this Society have confined themselves
very much to the condition of affairs in the wound itself as a cause
of pyæmia; and although this is an important point—perhaps it may
be one of the most important—it is not, I venture to think, that which
your paper especially called forth. For we see, again and again,
patients with large foetid abscesses, whether in the ischio-rectal fossa, or
in the neighbourhood of bones, with the pus in the very worst condi-
tion, as far as we can judge, and in as bad an anatomical position as
may well be imagined, and yet these patients escape all pyæmic poi-
soning; while other patients, with comparatively no especial sign about
the wound, no foetid pus, no constriction, get pyæmic poisoning very
rapidly, either in the form of pyæmia proper (and I must use that word
until we have another), or its ally, erysipelas. And I cannot but think
that we must take the other side of the question. In all cases of poi-
soning, be they mere chemical poisoning or animal poisoning, we have
two things to consider—the poison brought to the system and the
system which takes it in—and I think your paper more especially
refers to that condition of the system which is liable to take in pyæmia,
or the matters which produce a pyæmic condition of blood. Whether
we consider that this condition of blood may be produced by what Sir
James Simpson called hospitalism, if there be such a thing, or whether it
may be produced accidentally, or be constitutional, we still shall find
that there is a certain condition of blood, or of system, rendering
the patient susceptible to this form of poisoning. I need not here
instance the peculiar condition of system which is combined with
albuminuria. You yourself have pointed out of what grave importance
this is in the prognosis of injuries of the head. We all know we must
not touch with the knife, if we can avoid it, patients who are affected
with albuminuria; if we do the probability is that the patient, if the
kidney disease be not bad enough to kill him, will die either of erys-
pelas or of pyæmia. Hence we find a state of blood which introduces
pyæmic poison even when the wound itself is in the most favourable
circumstances. This is a constitutional state. But let us come now
to the artificially produced state, that, namely, which has been called
hospitalism; and here we must distinguish (and I am sorry to say Mr.
Erichsen does not, to my mind, sufficiently clearly distinguish)
between badly ventilated and ill-drained and ill-kept hospitals, and
hospitals perfectly well kept, perfectly drained, &c. Every hospital,
or every private house that has bad drains and bad ventilation, will be
liable to spread a pyæmic condition as soon as an operation is per-
formed in that dwelling. It does not matter whether it be such a
hospital as there is at Lincoln, or in some other towns, or whether it be
a private dwelling in the healthiest part of the country; as soon as we find
bad ventilation, bad drainage, and uncleanliness, so soon do our patients
run a danger of pyæmia, and in this way we have the most strongly
marked artificial production of that disease. But now let us take the
case—and this is really where the comparison should come—between the
very best kept and constructed hospitals and private dwellings; and
the question is simply whether there is a larger percentage of pyæmia
in hospitals than there is in the dwellings. Well, sir, I am sorry to
say our statistics, in the one or the other, are not sufficiently complete
to answer this question; but we may gain some inkling of the subject
from considering the fact that none of us, I suppose, will dare to
operate upon a case of ovariotomy in the general surgical ward of a
hospital. Here, I think, it seems pretty clear that there must be some
amount of septic influence in the air. But this case is rather peculiar,
and there may be in other cases—in operations, for instance, upon limbs—
counterbalancing advantages in the hospital which may counteract the
slight pyæmic influence sufficient to affect a case of ovariotomy. These
counterbalancing influences I need not especially mention, but I would
point out this fact—that we have in our large and better ventilated
hospitals very few cases of pyæmia, considering the number of opera-
tions—I think, in some of them, only about 0·5 per cent. per annum
of cases of pyæmia after operations. Do we get a less amount of
pyæmia in private? Only the other day, Sir James Paget told me
that he had a case of hospital gangrene in private practice, and he said
that it was the fourth case he had seen during his career in private,
while in his public or hospital practice he had seen but three. This
is a most striking circumstance; and I cannot but think that, if all the
cases which occurred in private practice were put together, we should find
them very much larger and very much fuller than is usually imagined.
I was called in the year 1868 to see a patient, then under the care of Sir
Thomas Watson, on account of a knee affection, which had come on
during a certain disease of the lung; I forget just now what that
disease was; and, when I saw this patient and examined the knee, I
found it to be a case of pyaemia as clearly marked as any case that I have ever seen. The other joints became subsequently affected, and the patient died. The only source of suppuration in that case was an eruption in the skin, which the patient had scratched, and caused little sores and scabs to form, underneath one or two of which there was pus. At that time—it was in the year 1868, and I do not think I should be committing any breach of confidence in mentioning the name—Panizzi himself had pyaemia, and he was attended by Dr. Guèneau de Mussy, who has left this country. Dr. Guèneau de Mussy was attending the case, and Sir Thomas Watson was only called in afterwards; therefore, there was no conveyance of the pyaemic influence from a patient whom Sir Thomas Watson had previously seen with me to the other patient. These cases are very striking, and show that, at all events, in private practice we have a large amount of pyaemia. I am sorry to be able to bring forward another case which occurred to me but a very few days ago. A lady with a small exostosis on the metatarsal bone of the left foot wished me to remove it. She was 64 years of age, and perfectly healthy. I endeavoured to dissuade her from an operation, but she said the pain was such that she insisted on having it done. The exostosis was mushroom-shaped, with a very small stalk not bigger than a barley-straw. It was nipped off with the greatest case, and the wound was carefully dressed with carbolic acid. The house in which she lived was perfectly healthy and pure, and every precaution which could be taken was, I believe, taken in this case. But when I saw her six hours afterwards the pulse was failing. I learnt afterwards from her son that, previously to the operation, she had expressed her strong belief that it would kill her. In spite of this she met us with the greatest cheerfulness and courage. She had made every preparation for death, and, from the time of the operation, it was with the utmost difficulty we could get her to take food or any form of stimulant or medicine. This was on Thursday—yesterday week. On the Sunday night the temperature rose to 100.4. On the Monday morning the knee and shoulder-joint on the same side of the body, the left, became affected; and from that time, when pyaemia may really have been said to have set in, the patient rapidly sank, and died one day within a week of the operation. In that case I happen to know that every sponge, every piece of lint that I used was in perfect order. The instrument even had been washed in carbolic acid—not a very usual precaution with me, but I had taken it at that time—and there was no possible precaution omitted with regard to the dressing of the wound that I could have taken if I had now to do the case again. But then comes the question, whether we may not also have a moral influence so far depressing the system as to produce this pyaemia. I can only interpret it this way. In my own private practice I have had within four years two cases of pyaemia, one apparently idiopathic, the other following operation. Sir James Paget has had no fewer than four cases of hospital gangrene, and I am not aware how many cases of pyaemia he may have had. You yourself have brought forward a
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goodly number; and I cannot but feel that, if all the untoward cases occurring in this way were published, we should have a great many more. But it requires some courage to do this. The fact of such cases occurring in private practice is very depressing to a man's feelings with regard to his work, and to publish them afterwards is not the best way to procure more cases of operation. I fear we shall never get to the absolute bottom of these statistics; nevertheless, the undetermined number in private practice is, I suspect, very much larger than is generally known, and militates I think very much against the idea of hospitalism. Though, to conclude, I may say again, referring to cases of ovariotomy, my own belief is that there is a septic influence in the air of hospitals, which, however, in ordinary cases of operation is more than counterbalanced by the great attention and good diet which the patients receive there.

Mr. Brudenell Carter: On the first evening of the debate, when Mr. Hutchinson expressed an opinion that wounds of veins were especially liable to be followed by pyaemia, I appealed to some of the elder members of the Society as to their experience with regard to pyaemia following venesection. If I am not disqualified by that brief appeal, I should be glad to say a few words on the general question. I should like to carry this discussion—so admirably sustained by many members of the Society, and for the origination of which we are so deeply indebted to yourself—one step further in the direction which has been already indicated to us by Mr. Callender, viz., the clinical direction as opposed to the pathological. It strikes me that although we must unhesitatingly accept the account which Mr. Hulke has given us of the sharply defined line existing between pyaemia and septicaemia when these conditions are artificially produced in the laboratory, still we do not, in disease or in Nature, find the experiment tried for us with anything like the same precision; and I think we should be unable clinically to draw any definition, either of pyaemia or septicaemia, which shall exclude the other, unless it exclude also a great number of cases and conditions which must be recognised as intermediate between the two. But I think there is a certain clinical division into two classes, which may be drawn not only with truth, but also with some practical advantage in assisting us to gain notions of the nature and treatment even of these cases. I think we may recognise cases of septicaemia (I prefer to use the larger and more general expression) in which the patient poisons himself, and that we may distinctly separate such cases from those in which he is poisoned by another. The distinction which I endeavoured to draw between these two classes of cases, although it may be illustrated very frequently and in many ways, may, I think, be particularly well illustrated by the clinical history of gonorrhoea. I have long entertained the conviction that gonorrhoea is essentially to be looked upon as a constitutional disease, of which the urethritis is only one, and frequently by no means the only local symptom. A few weeks ago I was asked to see a young robust vigorous countryman, in his nineteenth year, well fed, well housed, well cured for in every way,
who had lately come to reside in London, and had contracted gonorrhoea. On the second day of his gonorrhoea his left eye became affected; the eyelids became exceedingly swollen, brawny, and hard, so that they could not be separated; the conjunctiva became ecchymosed; and the eye poured forth a quantity, not of pus, but of thin sanious discharge. On the fourth day the patient was attacked with acute pneumonia of the most intense severity, involving both lungs, which carried him to the brink of the grave, attended by such extreme depression that he slipped down in bed, became delirious, and had the general aspect of a patient in an advanced stage of typhoid fever. Now, although I cannot exclude the possibility of inoculation, as far as the eye is concerned, yet, when I consider the early stage of the affection, and how frequently inoculation commences in the right eye instead of the left, I cannot help thinking that the ophthalmia, pneumonia, and gonorrhoea were three local expressions of the same blood-poisoning which the patient received from the same source. I think we must draw a very definite line between a case such as this and a case such as you have related, in which, after three weeks or so of gonorrhoeal discharge, possibly from some absorption of gonorrhoeal discharge into the system, there was suppuration, or the symptoms of what is called gonorrhoeal rheumatism. The case related to us by Mr. Barwell seemed also to be one of those in which the patient distinctly poisoned herself. It seems as if there we could exclude, as Mr. Barwell had excluded, all external influences. I think we must admit that even cases in which a patient poisons himself are cases in which we may at once lay aside all considerations of hospital influence, and cases of that kind we must expect to find in private practice as well as in hospitals. On the other hand, I think we must recognise the possibility that a patient who prepares, so to speak, a fluid capable of acting in this manner upon himself, may also be the source of poison to others, if, by any chance, that fluid be conveyed from one patient to another by the hands of nurses, or, as Mr. Callender has suggested, if, by any less direct and gross method, it enter into the wound. Now, if we inquire into the causes which may make a patient poison himself, into the causes which may produce, instead of laudable pus, a fluid of acrid character capable of producing symptoms of septicæmia, must we not look very much to the general state of the patient's nutrition, to the condition of his nervous forces, to the state of his trophic nerves, if there be trophic nerves? I think the more we study the external aspect of wounds and many kinds of external inflammation—I speak especially from what I have seen of ulceration and inflammation of the cornea—we shall more certainly recognise that there are states of the general system, certain states of defective nervous force which impress themselves locally, and tend to produce disintegration of the tissue in lieu of natural and healthy repair. Must we not, then, look rather in such cases to the state of the patient's nervous system than to his surroundings, rather to the state of general nutrition than to the state merely of the atmosphere? On the other hand, may we not class all those cases of septicæmia in which poison is conveyed
to the patient from without in the category of those which it is in our
power; in a great measure, to prevent by those precautions which Mr.
Callender has so ably stated?

Mr. Cadge: If I avail myself of the privilege accorded to strangers
and interpose in this important discussion, I do this with less reluctance
because it may perhaps be not altogether uninteresting to compare
instances of provincial experience with those of metropolitan surgeons.
In doing so I will endeavour to keep clear of those abstruse questions as
to the origin, nature, and agency of the causes of pyaemia. I think
they are subjects far too abstruse to be fully elucidated by discussion,
no matter how distinguished the Society, or how eminent the members
of it may be. They seem to me rather to require patient working in the
laboratory, in the dead-house, and in the study, and I despair of any-
things like a full elucidation of the subject from this or any
discussion whatever. But with regard to the occurrence of pyaemia
and the facts surrounding it, there, I think, we are travelling quite
within the proper lines of discussion. You yourself, sir, have initiated
this discussion, and to this point I would simply, as far as regards my
own experience, confine myself. It is now about twenty years, or
rather more, since I became attached to the Norfolk and Norwich
Hospital. I am grieved to say that for a considerable part of that
period the institution has fallen upon evil times with regard to this
question of pyaemia. We have had within the last six or eight years a
decidedly increasing amount of pyaemia in hospital practice, and that not-
withstanding that the hospital is fairly well provided in regard to cubic
space and beds, and possesses a perfect system of drainage which has been
established within the last few years, and with other precautions which
have in times gone by seemed to secure us from these plagues of surgery.
Still the fact remains that during the last three years—I go no further
than that—we have had twenty-one or twenty-two deaths from pyaemia
in the hospital. Yet this is on the whole clearly and thoroughly to be
accounted for. The way of accounting for it seems to me to touch the
very question we have at issue between private and hospital practice,
and it arose chiefly, in great part certainly, from overcrowding the
wards of the hospital. During twenty, or twenty-five, or thirty years,
we have had no extension of the hospital. It accommodates about 140
patients, 70 or 75 on each side. We have had no extension, but during
that period there has been a large increase of population, and there has
been a still greater increase in the growth of mechanical contrivances
and machinery, particularly of agricultural machinery, which has
thrown into the hospital a very large increase of serious injuries, acci-
dents, and wounds of all kinds. I have no doubt in my own mind that
the increase and development of pyaemia in our institution is mainly
due to this increase of serious accidents and wounds which have not
been properly cared for by an increase of hospital accommodation.
Some proof of this is found, I think, in the following facts. Of course
by far the major part of these cases of injury and disease, and of all
other surgical operations and diseases, occurs on the men's side of the
hospital. Out of twenty-one cases of pyæmia during the last three years, only one has occurred on the women’s side, and this, I think, must be a proof that we have an overcrowding, not in the sense of a deficient cubic space, but in the sense of too numerous cases of a bad kind in that space which we could not well avoid. We have been obliged to take serious steps towards altering this condition of things, and those steps are now in progress. We have shut up one ward after another, we have abolished the use of sponges, we have weeded out beds from each ward, we have looked after the ventilation in a very careful way, and the result of these experiments we have not yet had time to prove. So far as we can tell, I am afraid that they have wrought no marked benefit, and I should not wonder that the end is that we either close the hospital or make a large extension or a full reconstruction of it. Well, if that be the case in hospital practice, I am happy to turn to the absence of pyæmia so far as private practice is concerned. I may be permitted to say that during the last fifteen or twenty years a considerable share of both surgical operations and all kinds of surgical cases has fallen in my way in the city and in the country for twenty or thirty miles round, and I think I can say that during twenty years and more, with one exception, I have not come across a case of pyæmia; and I am not sure that that one exception can be regarded as a typical unequivocal case. It was a case of lithotomy, in which a country gentleman of very florid colour and congested appearance had haemorrhage on the day succeeding the operation. On the third day there came on terrible rigors, which were repeated day after day, until death took place on the fourteenth day. At the post-mortem examination not a drop of pus was discovered in any part of the system; there was simply an enormous congestion of the visceral organs. The viscera, in the abdomen especially, were greatly enlarged, and there was a very turgid spleen. That, I think, is the only case. Neither have I seen any case following carbuncles or boils. In this way I have been led to think that one’s private practice was perfectly secure and very much opposed to hospital practice. I have almost unwillingly and tremblingly proceeded to operate in the hospital, but I have had a happy confidence and a perfect assurance that in all private cases I should avoid any of these disastrous consequences. It fell upon me, therefore, with some surprise when I read your admirable address, quoting such a large body of evidence with regard to the occurrence of pyæmia in private practice, and I felt still more surprised when I heard one speaker after another—notably one eminent speaker—declare that pyæmia was, in his experience, just as frequent, and arose from just as trivial causes, in private as in hospital practice. Then came the other speakers who had similar experience to my own to quote, having had no decided case of pyæmia following operations in private practice, and those gentlemen were, as far as I could ascertain, practising in the same sphere as yourself, Sir James Paget, and others. How are we to reconcile these discordant records of experience? There can be no doubt, I apprehend, that the nature of the practice must be the same, and that the variety of
cases must be very much the same, but that there must be some mode of coming to a conclusion, so as to make the one set of facts fit in with the other; and I can only explain it by supposing that there are some gentlemen—Mr. Erichsen, to wit—who would hesitate to designate as pyaemia certain cases which you yourself have given as instances of that disease. That is the feeling I have at this moment, and in looking over the carefully recorded cases you have given us, there are some of them that it would never enter into my mind to give as pure typical cases of pyaemia. Notably I refer to two or three cases of typhoid fever, which were succeeded by feverish symptoms during convalescence, and the development of abscesses in the limbs and in various parts—sometimes one, sometimes more—ending in recovery in all but one case. I admit I have seen cases of typhoid fever ending in development of abscesses during recovery; but certainly they differ as widely as cases can differ from the pyaemia with which I have been so familiar in hospital practice. Therefore, if you will allow me, I will compare, in a few words, the records of the same number of cases in the hospital that I have a distinct recollection of as to their main facts with those you have given us in your admirable address. Taking the last twenty-one cases of pyaemia in the hospital, I find that they all died. Of your twenty-one cases nearly half, if not quite a half, recovered. Now that is a striking contrast to begin with, and I think I shall have the assent of most surgeons when I say that pure typical pyaemia is marvellously fatal. Probably I am within the mark when I say that, in the experience of most persons, 90 per cent. of the cases of pyaemia would be fatal, probably more. In your cases ten out of twenty-one seem to have recovered. If it be said that the hospital cases represent a malignant form of pyaemia, it may be so; but in answer to that I can quote Sir James Paget’s remark that in his experience, which probably is London experience in the main, pyaemia was as fatal in private as in hospital practice. Then, again, in noting the pathological and anatomical facts of these twenty-one cases of hospital pyaemia, I have had post-mortem examinations in every instance, and I find that in twenty of them there were visceral abscesses, abscesses of the lung and the liver. Now in your twenty-one cases there could not have been abscesses of the lung and liver in quite half, because they recovered, and I should think in considerably more than half there probably were not any visceral abscesses whatever. There, again, is a considerable contrast in the anatomical facts. I notice also that all these twenty-one cases to which I have directed attention arose, with one doubtful exception, from operation or ulcer or some broken surface; but in six out of your twenty-one cases there was no wound, no operation, no sore, no broken surface whatever. Now I am quite ready to admit that pyaemia may occur without any broken surface. It is denied, I believe, by some authorities, but I think I have seen it, and I have no reason to doubt the fact; but, at the same time, most surgeons will agree that it is extremely rare. Six out of twenty-one cases seem to me to indicate a very marked excess of what I should have thought to be possible. I notice also that
in the twenty-one cases I have taken a note of, pyæmia followed in at least fourteen cases from surgical operations—some slight, such as fistula, some heavy, as in the case of stone and amputations; but of your twenty-one cases I think there are only six cases of operation, and those of a slight and trival character, so that I still feel a difficulty in bringing into harmony and unison such typical cases as I have reported and those twenty-one cases the notes of which you have been so good as to lay before the Society. If it be the case that private practice affords such a large body of evidence with regard to the occurrence of pyæmia, I scarcely know what inference to draw, except this—I may be pardoned for making it—that those patients who are afflicted with wounds or stand in need of operation will really do well, instead of flocking to London for its matchless surgery and superior advice, to remain and take advantage of the humbler attainments that they will meet with in the country, where they will exchange skill for greater safety. Still, I come to the conclusion in my own mind that pyæmia, if it does not find its birth-place, does find its natural home and resting-place in hospitals; and although a hospital may not be the mother of pyæmia, it is its nurse. Nevertheless, I cannot but express my gratitude to you, sir, for the bold and fearless way in which you have reported these most unwelcome cases, leaving us to think the matter over in every possible direction; and the direction of all others that it strikes me as most useful to think about it is not so much the mere treatment of it as that which Mr. Callender has pointed out, and that which he has done so much to illustrate—namely, the prevention of this disease. It strikes me that in that direction we shall conquer this terrible pest and plague; and I, for one, shall not rest satisfied in our own locality until we have subdued in our Norfolk and Norwich Hospital this dreadful pest down to the low level and satisfactory condition that Mr. Callender so well describes as existing at St. Bartholomew's.

Mr. Adams: Amongst the interesting observations arising out of the valuable paper which you have communicated to this Society, it appears to me that the two of greater prominence are, first, the establishment of the fact of pyæmia occurring in private practice in a very much larger proportion of cases than hitherto has been supposed; and, secondly, the facts which have been arrayed, especially by Mr. Erichsen and Mr. Callender, with regard to the prevention of pyæmia when it arises in hospital practice, for we now know that it is liable to occur both in private and in hospital practice—I will not say with equal, but with a more or less equal degree of certainty. With regard to the observations made as to the prevention of the spread of pyæmia, I would second the remarks made by Mr. Callender with some experience in a smaller hospital, the Great Northern. In large hospitals like St. Bartholomew's and the London—and my recollections go back to the old days of St. Thomas's—we know that operations were frequently almost suspended for a time during the prevalence of pyæmia in the wards. Surgeons almost refused to operate, or they operated only in cases of extreme emergency. Now it happened in the Great Northern
Hospital, about two years and a half ago, that our surgical deaths reached 50 per cent. from pyaemia. This of course called for an inquiry. The staff and the committee were anxiously engaged in investigating all the circumstances connected with the nursing department, the ventilation, the cubic space, and so forth. After that time a change occurred, and a house-surgeon (Mr. Young) was elected from one of Mr. Callender's and Sir James Paget's house-surgeons, and he was therefore well instructed in the general principles that Mr. Callender has enunciated this evening. Our surgical deaths then rapidly diminished, and within the last year and a half, out of three hundred surgical cases, we have had only eighteen deaths, and not more than one—and that a doubtful case—of pyaemia. This result was considered to be due not only to the diminution of the number of beds in some of the wards and the increase of the cubic space for breathing, but also to the preventing of cases of severe accidents and large suppurating wounds accumulating in one ward, and especially to the care paid in dressing wounds within the period which Mr. Callender has specified after the accident. All sponges were abolished in the wards. They are used in the operating theatres, and they are only kept for operation cases. All materials are quickly removed from the wards. With these general directions our deaths have now declined; though, after what Mr. Cadge has said, we can hardly depend upon such sanitary arrangements being always attended with equally good results.

Mr. Durham: The subject of pyaemia is one of very great interest and importance; and if we wanted any evidence of the interest felt in this subject by all our London surgeons, I suppose we could scarcely find any better than that afforded by the number of large and well-attended meetings that have occurred for the discussion of the subject in consequence of your opening remarks. It is an interesting subject, from whatever point of view considered; and that it may be considered from a great number of points of view is well illustrated by the remarks of the different speakers in connection with the subject.

I venture rather to come back to the point from which you started—the question of pyaemia as occurring in private practice—for it does appear to me that this is a most important view of the case to take, for at the present day we hospital-surgeons are so accustomed to have thrown in our teeth the great horrors of hospitalism or the fatality that attends it, that it is a great matter of consolation to find men—our seniors—men of large experience, like yourself and Sir James Paget, coming forward and telling us that you meet with such a number of cases of pyaemia, which some would make out to be especially a hospital disease, in private practice. I have met with at least five undoubted cases of pyaemia in private practice, and in each case I venture to say it arose altogether without any evidence of its having been conveyed by contagion, the cause to which some persons seem to attach certain of those cases of pyaemia that have occurred in the private practice of distinguished hospital surgeons. It has been said that, if these surgeons get pyaemia in their private practice, it is because they, their assistants,
or their nurses, have conveyed the poison to their patients. I stand here to protest against the truthfulness of any such view. There is an inference drawn, too, against which I feel bound to enter a protest, that under particular circumstances hospital surgeons ought to undertake no private operations. It has been distinctly said to me: 'You hospital surgeons convey these diseases about; you hospital surgeons have no business to operate in such and such cases.' Your cases, sir, many of them, are certainly free altogether from the possibility of being thus interpreted, and it so happens that the cases which have occurred under my own eye were also free from such an imputation; for I had nothing to do with them till the pyaemia had certainly been established. Of those cases, five in all, I should like to mention two: the first because of the manner in which the malady arose; and the second because of the extremely successful and satisfactory results which attended the treatment. The first case to which I refer was one that made a very great impression, indeed, upon me at the time, though it is many years since. A young gentleman, in the middle of his examination at the University of London, having got through the first week, was rather depressed on the Friday, and on the Saturday he was extremely ill. That night he had a severe rigor. On Sunday he was still more ill, and I saw him. He presented all the symptoms of pyaemia; the dusky-ness of complexion; the dry, glazed, reddish tongue. He had effusion in one shoulder and effusion in the knee on the opposite side, with the general mental trepidation usually observed. I recognised the serious condition of the young man, and called in my colleague, Dr. Wilks, the next day, who saw him with me. The effusion, which took place first in one knee and in the opposite shoulder, subsequently took place in the other knee, and then in or about the testicle, then in the calf and the other leg; and on the Wednesday following this young man died. It was not until after his death I learned, and then I had it on the strongest possible evidence, that he had, unfortunately, been suffering from an attack of gonorrhoea at the time. This I take to be a case of unmistakable, indisputable pyaemia, owing its origin to gonorrhoea. I refer to the case because you mentioned a similar one; at the same time observing, no doubt perfectly correctly, that such cases were very rare. The next case to which I would venture also to call attention, as occurring in private practice, was a case to which I was called by my friend, Dr. Hunter, of Lavender Hill, Clapham. It was to see a lady twenty-four years of age, who had always had very fair general health. She had aborted on the 19th of October, and had hemorrhage afterwards. The vagina was plugged, and secale cornutum administered, and the hæmorrhage arrested. I need not trouble you with all the details of the case, but would merely say that the mother was extremely depressed by the fact of her only other child having a severe attack of scarlet fever; this child had been attacked while sleeping in the same room with its mother. The moment the scarlet fever was recognised the child was sent away. On October 28, that is, nine days after the abortion, the patient had rigors, and all the symptoms of
pyæmia set in, accompanied with sleeplessness, and so on. She continued in this state and had renewed rigors until November 9, and at that time fluctuation was discovered in the anterior part of the right thigh, about the junction of the middle and lower third; also evidence of fluid in the left knee-joint. Under such circumstances a consultation was proposed, and I was called in. About the nature of the case there was no room for doubt; as to its origin, that seemed to be pretty clear also. I made a very free incision right through the muscles into the large abscess in the front of the thigh, and evacuated a considerable quantity of ill-formed pus. The femur was laid completely bare, and denuded of periosteum for three or four inches. I passed my fingers up and down it. I then proceeded to tap the opposite knee-joint. I passed a trocar and canula into the knee-joint, and evacuated from that from four to six ounces of serum and some ill-formed flocky kind of pus—the kind of stuff we are in the habit of recognising in the joints in these conditions. The patient was very much relieved, and she went on pretty well for some days. About a fortnight subsequently I was again called in, a fresh collection of fluid having taken place in the opposite leg, deep down amongst the muscles. I again made a free incision, and found that the tibia was bare for the space, again, of two or four inches. That was the last time I saw the patient, until she came to call upon me perfectly well, having entirely recovered. The places healed up; there was no exfoliation of bone. She moved her knee-joint perfectly freely, and in every respect was in good health. That is a very satisfactory and successful case, and I bring it forward as an illustration of the method of treatment. I believe the success of our proceedings was owing to the fact that we administered to her very large doses of quinine. It has happened to me to have seen a great many cases of pyæmia in hospital practice; and of late years, whenever I have seen the slightest indication of the symptoms setting in, I have immediately given large doses of quinine, and in cases in which pyæmic symptoms have been well established, and which, I have no doubt, would have gone on to a fatal termination, I have also given large doses of quinine, and in some cases the most satisfactory results have been obtained. I have seen, over and over again, a patient with a dry, red, glazed, brown, furred tongue, with a rapid pulse, high temperature, having had rigor a short time previously, with evidences of suppuration occurring in one part or another. I have, in such cases, given half a drachm of quinine, and repeated it in three or four hours, and on the next day I have seen the patient in a totally different condition: the temperature down, the pulse lower, the tongue clean and moist, and the patient expressing himself well. I beg respectfully to recommend this kind of treatment to the members of the Society—large doses of quinine, three or four times a day, regularly, and whenever there is a prospect of a rigor coming on. If the patient cannot take it, it may be given subcutaneously. The most striking case I have seen of the beneficial effect of this treatment was a case in which the quinine was administered subcutaneously.
Dr. A. P. Stewart: I must beg the pardon of the Society, if, at the fag-end of what appears to be an essentially surgical debate, I should rise to protest that pyæmia, so far as my experience goes, is by no means essentially a surgical disease, and that it is by no means necessarily connected with open wounds, as has been stated in the course of this discussion. I can look back nearly forty years, when I was resident in the Fever Hospital at Glasgow, in a time of extreme prevalence of epidemic typhus, with very limited accommodation for the multitude of patients that presented themselves, during a time when the wards were exceedingly overcrowded; and at that time, and for the period of perhaps four or five months, pyæmia, in its essential characteristics, was very prevalent, without a single open wound existing among those affected by it; that is to say, as a sequela of typhus. About the crisis, or rather after the crisis, intense rigors came on, with dusky, and even jaundiced skin, extreme rapidity of pulse, glazed tongue, and painful joints; finally, most of the joints of the body were proved in almost every case, upon post-mortem examination, to be filled with pus, not only the larger, but the smaller joints; and there were visceral abscesses, that is to say, purulent deposits in the lungs. This occurred, not in one or two cases, but in a multitude of cases, without a single wound having been present in any one of these patients; added to which, in a considerable number of them there was inflammation of the veins to a very great extent, with large swelling of the limbs. Now this, I think no one would deny was essentially pyæmia. The two conditions were an intensely contagious, malignant disease, and great overcrowding such as has been stated by Mr. Cadge to have been present in the Norfolk and Norwich Hospital; and, I believe that, wherever you have these same conditions, you will have the same results. I recall particular attention to the remarks made by my late colleague, Mr. D. Morgan, on a former evening of this discussion, which, as it appeared to me, recalled a case that had been already repeated usque ad nauseam as occurring in the Middlesex Hospital, in which, from bad air in a particular portion of the ward, there were continual cases of pyæmia occurring; on the removal of that condition the pyæmia ceased, and did not return for a series of years, till the same condition occurred again from neglect, and pyæmia recurred. Again, upon removing this unsanitary condition, the pyæmia ceased once more. This fact bears greatly on the question we seek to solve, whether pyæmia does not arise mainly from bad air in some form or another, overcrowding, the presence of malignant disease, or of blood-poisoning in some shape or other, not necessarily from surgical operations, or from the presence originally of pus in the system, but blood-poisoning such as happened in those cases of typhus which I mentioned, which proved fatal to such a large a number of cases of typhus in the Glasgow Fever Hospital. I believe you will find similar blood disease wherever there are the same or analogous anti-hygienic conditions.

Mr. Spencer Wells: The difficulty that I have in entering upon this discussion arises from the extreme amount of material that is
thrown before us in your paper, and in the various debates which have been held upon it. I have been looking them through in the very admirable reports that have been given in the medical journals, and it seems to me there are so many points for discussion, that the difficulty is in selecting any one of them which one could bring before the Society in any reasonable time. If, however, I confine myself to the concluding sentence of your paper, in which you ask the Society to investigate the causes of pyëmia (and I suppose, by that, you would mean causes of pyëmia in private practice particularly), the subject will be narrowed, and one may, in the few minutes at one's disposal, endeavour to throw a little light upon it. If we look upon pyëmia in the same light, the causes of the disease must be pretty much the same either in hospital or in private practice; and the only question would be, in which is the disease most frequent? There is just one question which I hope you will answer when you come to reply; namely, the sort of proportion that the twenty-one cases in your paper bear to the number of cases which have occurred in your hospital practice during that time. It seems to me, indeed, that the number of cases is very considerable, even during the twenty or thirty years of your practice over which they have run; and the information I should like to obtain is whether, during that time, in St. George's Hospital, there has not been a very much larger proportion of cases of pyëmia. So far as my own experience goes, looking back to the very commencement of my professional life, I should say that it is only in large or crowded hospitals that I have ever seen the disease. In St. Thomas's Hospital, during my student days, we had hardly begun to recognise it. Erysipelas was very rife, but I really saw nothing of pyëmia until years afterwards, at Palermo, when the hospitals there were crowded after the bombardment; and again in Paris, in 1848, when I was sent by the Medical Department of the Navy to report upon gunshot wounds. There one saw the disease every day in the crowded hospitals, where the wards were filled with insurgents and soldiers who had been wounded at the barricades during the fight under Cavaignac. Previously to that—from 1841 to 1848—I had been in a very large naval hospital in Malta. The number of patients was considerable, but we had enormous wards. They were like churches; and, although there were some forty or fifty patients in each ward, the breathing space was so ample that I do not believe there was a single case of pyëmia during the whole of the six years that I served there. I cannot recall a single case. I have seen cases of erysipelas come in from ships; but, with care in isolating those patients, the erysipelas did not spread. When I went to Paris in 1848, and visited the crowded hospitals, I saw the disease every day. I come to London, and see no pyëmia at all. Then I go to the Crimea, and the very first thing I meet with in the hospitals at Smyrna is pyëmia. The wards were crowded. Patients were there with frost-bites, and a few with gunshot wounds; and, on the least disturbance of any of the sloughing tissues about their feet or hands, pyëmia came on, and the patients died, just as they die here in London in some of the hospitals
that are crowded, and are placed under conditions that might be improved. Then, sir, returning from these two spheres where pyæmia was observed, I practised in London, a good deal in private, I may say, and partly in a small hospital; and in both divisions of practice I have performed a very large number of operations, in which the patient is supposed to be extremely susceptible of any poisonous influence. Various speakers in this discussion have pointed to ovariotomy as an operation which cannot be performed in hospital wards with an ordinary chance of success, or as an operation which is much more likely to be followed by some poisoning influence upon the patient than almost any other operation in surgery. I have now performed more than 600 of these operations, and a great many other cases in which the abdominal cavity has been opened, and a certain number of other operations during the last fifteen years; but neither in private practice, nor in the hospital, have I seen one fatal case of pyæmia. Saying that, I must just explain for a moment what I mean by pyæmia. I mean the disease which we know by recurrent chills, by high temperature by profuse sweatings, followed by deposits of pus in some joint, or in cellular tissue, or in some internal organ. Taking that as what I mean by pyæmia, and what, I fancy, is pretty generally meant by it, I have never seen one fatal case, either in the hospital or in my private practice; while the only cases that I could call pyæmia, that I have seen, have been slight cases of feverishness following operations, with swelling of some joint, called rheumatism, with some little hurry of breathing, probably accounted for by some pneumonia, or some pleurisy, or with some little jaundice, accounted for possibly by some congestion of liver, but the patients getting well. With the exception of these slight cases, I may say that pyæmia has been to me quite unknown, both in the hospital and in private. What I have known other people call pyæmia is a condition which I should call septicaemia—a condition in which, a cyst being emptied of its contents and air admitted, the contents left in the cyst, or contents which have formed afterwards, have putrefied, and then the patient has been poisoned by this decomposing fluid. That condition I have frequently seen, ending sometimes in death and sometimes in recovery; generally ending in recovery, if the cyst containing the putrefying fluid were removed early enough; frequently ending in death, if it were only drained, or if it were imperfectly drained, or simply washed out by a repeated injection of some antiseptic—iodine or carbolic acid. Under any of these modes of treatment, death has generally followed, but there has been no secondary deposit of pus, no pus in joints, no pus in internal organs; therefore I do not believe that the disease could correctly be called pyæmia at all. But I think we might safely say, any mode of preventative treatment, or any kind of condition which could prevent a patient from suffering from septicaemia, might also fairly be looked upon as likely to protect him from pyæmia; and all the difference that I can see between the probable causes of septicaemia or pyæmia, in hospital practice or in private practice, is the greater liability of the patient in hospital practice to
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contagion. It seems to me you have very much the same conditions to guard against, and the patient is very much under the same conditions, whether he be in a small room with one or two people—a patient, for instance, in a small room in a lodging-house in London, with a nurse and some one of his relatives—three people together, in a small room, with imperfect ventilation—or whether he be in a ward of a hospital where there are twenty or thirty patients all crowded together, with a certain number of nurses; with this exception, that in the one case you may have contagious influences bearing upon your patient, and in the other you have not. It seems to me that your private patient is guarded more or less from contagion, to which the hospital patient is subject. Therefore the question of prevention bears mainly upon the question of contagion or infection, or its absence; and you must, when you are searching for the causes of pyæmia, and endeavouring to avoid them, in the first place, put aside everything that is likely to expose the patient to the inoculation of a poison, or its reception through the breathing organs, and then you have to look to all the conditions about the patient which would render him liable to receive a poison. If he be breathing pure air, I suppose his blood is likely to be in a much better state, and he is much more likely to throw off any injurious influence to which he is exposed, than a patient who has been depressed by bad air, or by imperfect clothing, exposure to cold, insufficient or bad food or drink, or any other cause which may put a patient into a state of health in which he is less likely to throw off any poison, or the effects of any injurious influence to which he may be subjected. The point which Mr. Callender raised, and which, I think, is of very great interest in this matter, particularly with regard to the causes and the prevention of pyæmia, is that, by very great care in the treatment of wounds, by keeping the wound in a state of perfect rest, by maintaining the most scrupulous cleanliness, by avoiding anything that can possibly injure the patient, locally or generally, you may obtain in a hospital just as good results as can be obtained in private, and may have pyæmia under control. In that I most fully agree with him. I do believe that, although you may occasionally find pyæmia arising in the most healthy country-house, and in the best situation, as a rule the disease is one which is under our control; which ought to be, and which will be, prevented, when we have obtained in our hospitals as good general conditions as we have in private houses. When you can place a patient in a hospital in the same conditions as one who is in a private house, with all the comforts of life about him, and can protect him from contagion and infection, then I believe you will get just as good results in the hospitals as you do in private practice, and pyæmia will be no more rife in the one than in the other. If this discussion tend to spread that belief amongst us, I believe it will lead to a much less mortality following operations than exists now, and that there will be no such thing as what may be called cases of excessive mortality after operations. I hope that will be the result, and that we may avoid the error, which we are apt to fall into, of saying, 'Oh, it does not matter; you do an operation; you cannot tell
whether the patient is going to get well or not; you may do a slight operation, and the patient will die; you may do a serious operation, and he will recover, and you do not know why it is so.' I trust that we shall be led by this discussion, and by your paper, to examine more carefully into the causes of excessive mortality after operation, and determine to avoid them, exercising the most rigid self-examination, whenever a patient dies that ought not to die. If you do a serious operation, attended necessarily with great risk to life, and the patient die, nobody is to blame; but when, after some trivial thing, some little operation of no moment, the patient is cut off, and cut off by some cause which might possibly be prevented, then, I say, the most rigid self-examination should be made by the surgeon, and he ought to go into every circumstance surrounding the patient, everything about the operation, everything about himself and his assistants; and I believe, if that be done, and every case be carefully examined into, some cause or another will be found. If that be the result of this paper, I think you will have to be congratulated most sincerely upon having brought the subject before the Society. Your example will be handed down to others, and the good you have done yourself will be multiplied exceedingly by those who follow after you.

Dr. Gordon: I was, through the chances of war, besieged in Paris, and I had many opportunities, through the great courtesy of the French medical men in military service, of seeing the wounded on and from the field of battle upon a scale such as I hope I may never be called to witness again; I also had many opportunities of seeing the terrible ravages committed amongst those wounded men by pyæmia and allied diseases during the time they were under treatment in the ambulances and in the hospitals. Those hospital diseases by which mortality was chiefly occasioned included hospital gangrene and erysipelas, although both were in a much smaller degree than seems to have prevailed according to the accounts of the old medical officers who served in the Peninsular army. Pyæmia prevailed to quite as great an extent, I should think, as it did during the Peninsular or any other war. I may mention, with reference to the observations made by a previous speaker with regard to the clear distinction between pyæmia and septiæmia, that we saw a number of cases in the hospitals in Paris, some of which were described as pyæmia and others as septiæmia; and I must say that, although others, no doubt better qualified than myself, were able to draw distinctions between those cases at the bedside, I very often failed to do so. It seemed to me very much that the one was simply a stage or form of the other. I may observe that since this discussion commenced I have referred to the works of the French authors on the subject, and I find that whereas one set describe septiæmia as being an acute and very fatal form of pyæmia, the patient seldom or never recovering, another set (and among them, if I mistake not, M. Legouest) describe it as a chronic form of pyæmia, from which recoveries are very frequent. I mention this point as indicating that, according to my view, the distinction is not yet completely drawn between the two. There have been many discussions in Paris as to the
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particular poisons by which pyæmia and septicæmia are respectively occasioned; and I have no doubt that you, sir, and others present, have read the discussions which took place upon this very subject some time ago, shortly after the war, before the Academy of Medicine. If I mistake not, it was M. Verneuil who described the active principle of septicæmia as being capable not only of being collected, but of being tested. He described it as an alkaloid. He further described it as capable of being converted into a sulphate, and the solution of the sulphate when inoculated as being capable of producing the disease. I mention these things in order to support the view that I have just expressed, that in many cases I was unable to draw a distinction between the two diseases, further than that I believe the one to be a modification and a stage of the other. I may observe also, with reference to the statements that have been made before the Society regarding the prevalence of pyæmia in civil life, that I have been more or less intimately associated with military hospitals and with soldiers for a period of thirty-three years, during which period, except in a time of war, I have never had occasion to see a single case of pyæmia in a military hospital. I believe that nothing can more testify to the success and efficiency with which sanitation is carried out in military hospitals and barracks than that fact, when compared and taken in connection with what we have heard with regard to the occurrence of the affection in ordinary civil life. The period at which pyæmia usually happened, and the class of cases in which it usually happened, during the late war, were these. As a rule, it followed more particularly severe injuries, such as shell-wounds affecting the large bones and joints when implicating the heads of the bones. In these cases the symptoms usually showed themselves from the fourteenth to the sixteenth day after the receipt of the wound. It was very seldom indeed, whatever method of treatment was followed, that the symptoms appeared before that date. After the manner in which the last speaker has described the symptoms I need not again refer to them. I will only say that the symptoms, as observed in Paris, were precisely those that have been already mentioned, but in many instances in an extremely aggravated degree; that is to say, that the first chill, instead of being an ordinary shiver, was sometimes extremely intense. The succeeding fever was equally intense, the temperature frequently rising to 104 degrees. The state of perspiration was so intense that very frequently the whole bed became so completely wet as to appear soaked. There is one symptom, however, that I may mention that has been alluded to in this discussion; that is, the peculiar odour arising from the perspiration in these cases. I have seen and heard it described as an odour similar to that of malt. To me it is anything but like the odour of malt, which is more or less sweet and agreeable; the odour arising from pyæmia—at all events, such pyæmia as I have seen—is extremely sickening and heavy, and of a kind that when once perceived can never be mistaken. Another symptom that I may mention is the peculiar aspect of the countenance. Immediately on the occurrence of the attack the countenance becomes anxious, sharp, and desponding; the whole expression is that of despair;
the mind very soon becomes disturbed; but I have seen many instances in which, after considerable mental disturbance and delirium, the patient seemed to recover his ordinary senses for some time before death. But a remarkable indication was the extreme despondency with which all patients attacked with this terrible disease were seized immediately on the symptoms displaying themselves. Another indication was the attack of local pain. Sooner or later, usually after the first attack, local pains manifested themselves, sometimes in the form of arthritis, always ending with purulent deposit, sometimes in the form of pneumonia and bronchitis; and I have reason to believe that many deaths in Paris that absolutely depended upon pyaemia were returned officially as arising from those diseases. Therefore I think that in such instances either the precise nature of the cases could not have been apprehended, or probably there might have been reasons for returning them otherwise than they really were. I may also observe that the period during which life was prolonged after the first attack of pyaemia was, as a rule, from four to ten days. It was very seldom indeed that pure pyaemia permitted the patient to survive beyond the tenth day. Instances were seen and recorded in which life was prolonged to the twentieth day, but these were extremely rare. With regard to the treatment and the question of recovery, I find, according to the reports of the meetings of this Society, that the recoveries are recorded as being not by any means unfrequent. As I have remarked in the work that I published on the war, so far as I remember I have never seen a case of pyaemia as it occurred in Paris recover. And on reading the work on Military Surgery, by Legouest, I find that he only records in the whole of his experience—and he is a very old French army medical officer—that the chances of recovery from pyaemia when it has fairly commenced in a state of war (I say nothing with regard to ordinary civil practice in times of peace) are very small indeed. There were several kinds of treatment employed. One was—and I believe it is that which was adopted most frequently—the administration of very large doses of quinine. Various disinfecting agents were applied to the wounds. Bisulphites, such as the bisulphite of soda, were also employed, but, I believe, not followed by any success. M. Demarquay, whom I followed a good deal during the siege, in order to prevent the occurrence of pyaemia, was in the habit of spreading a chlorinated tincture of iron over the raw surfaces; but I am afraid that the circumstances conducive to pyaemia were so very powerful that the application of his remedy had very little effect in preventing it. There was one important measure that I think ought always to be adopted in such cases, but, unfortunately, in hospitals so crowded as we saw in Paris, it was impossible to carry it into effect; that is, immediately on the occurrence of a case of pyaemia, to isolate the patient. In some instances this was done; but after the great battle the wounded came in and had to be admitted on such an enormous scale that it was perfectly impracticable to isolate even a tithe of them. As to the predisposing causes of the affection as witnessed in Paris, of course the usual conditions of war were the most formidable.
These conditions included a great and constant fatigue, want of sleep at night, continual exposure to the weather, irregular, badly-cooked, and insufficient food. Then, above this, was the loss of morale by want of success; and I believe it is the experience of all military surgeons that those diseases are always found to be more prevalent in an unsuccessful than in a successful army. This question of want of morale, and the despondency arising from it, became a matter of particular attention by one of the operating surgeons, M. Gosselin. In all cases admitted into La Charité, he took the greatest possible trouble to raise the confidence of the men. He appealed to them and to their friends to encourage them in every possible way, and I believe in many instances with very good result. Another point has arisen, as to whether this is a hospital disease or not. Now, in Paris we found that, according to the number of severe cases in a hospital, the disease seemed to increase. It oscillated, in fact, according to the number of severe cases in the hospital. That is to say, soon after each of the great battles, in a period of fourteen or fifteen days, pyaemia became extremely prevalent. And after the number had been thinned—as unfortunately was frequently the case—the relative prevalence of pyemia became less. In some ambulances, again, in which only trivial cases were admitted, the relative prevalence of pyemia was extremely small. Hence I take it to be that in hospitals the chances of patients becoming attacked with pyaemia are in relative proportion to the number of severe cases, either in the ward or in the building. In that way it seems to me clearly to be dependent upon hospital conditions. I may also say that the fatality of the wounded, not only from pyaemia, but from the other diseases that I have alluded to, increased progressively during the siege. After the first battle that took place round Paris on the 21st of October, the success with which the wounded were treated was something so remarkable that we were led to believe that almost any case would recover. However, as the fatigues connected with the campaign, the exposure, the bad food, and other causes contributed their quota to lower the general system, the mortality became greater. In November it became very considerable. After the battle of the 21st of December it was very great indeed. After the last battle, on the 19th of January 1871, the mortality was so great that it seemed that almost every person who had a severe injury died. It was also remarkable in the aspect of the wound that reaction seemed never fairly to set in. There was sanious discharge from the time of the admission of the patients. Their vital powers gave way, and they seemed to sink. Among the active causes of the disease noted at the time, one was the great loss of blood to which the soldiers already debilitated by the causes I have mentioned were exposed. Many of them were left for hours on the field of battle before they could be attended to. Many others, in consequence of the immense crowd of the conveyances by which they were brought into Paris blocking up the roads, were detained for hours before they could be brought into the hospital; and when they were brought in the surgeons were comparatively few, the attendants still fewer, and sometimes not
well trained. The result was that from these and other causes very
great delay sometimes took place in performing the necessary dressings.
Then, again, the patients as they were brought in had to remain in the
clothes in which they had been wounded, many of them completely
soiled with the discharges of blood and other matters, all of which began
to decompose by the time they were brought into the hospital. In
many instances the clothing, after being taken off, instead of being
immediately removed from the ward, was allowed to remain hanging
about the walls. Cleanliness was neglected in the wards, and in that
way I believe a great deal of pyaemia was caused. These circumstances
arose from no fault of the medical officers, or of the private medical
men. The hospitals were not under their superintendence. They had
only to treat particular patients in particular beds, and the general
arrangements were beyond their control altogether. Another cause, I
think, was a very important one. In dressing the wounds after the
patients were admitted into the hospital, sufficient attention was not
paid to the necessity of removing them from one bed to another. The
bedding became more or less saturated, if not with actual discharges,
at any rate with emanations, and I believe that many cases of pyaemia
were thus introduced. Although the bedding was apparently clean
the whole thing had not been changed, and thus pyaemia was occasioned.
Professor Mosetig of Vienna, who had charge of the ambulance of the
Corps Legislatif, adopted the plan at a very early period, and carried it
on throughout the siege, of having two beds for every wounded man.
Whatever the nature of the wound, it was dressed at least once a day
and sometimes twice. Every man once in twenty-four hours was tho-
roughly cleaned, bathed, and sponged, the whole of his dressings were
changed, and he was moved on to a completely fresh clean bed, and I
believe that had a very considerable effect in diminishing the mortality.
Another plan of treatment, which I believe had also a considerable effect
in modifying or preventing the occurrence of pyaemia, was that adopted
in the American ambulance, that is, the employment of oakum in the
treatment of wounds, especially of the lower extremity—wounds of such
a nature as prevented the patient being in a recumbent position.
Oakum was employed so as to completely surround in a thick layer the
whole of the wounded limb. All discharges soaked into it, and in that
way it seemed to me to prevent the decomposition of the discharges,
and at the same time kept the wounds clean. In contradistinction to
that, I may mention the French system of treating wounds. I believe
many were in the habit of putting on too many folds of cloth. First,
there was the charpie; over that, cotton; then compresses, then
bandages. Immediately these were taken off the emanations from
them showed what was going on.

Dr. Burdon Sanderson: I may say, sir, with perfect truth, that
when I came first to this discussion I made up my mind in the most
determined manner that I would not take any part in it, because my
whole object was to listen to the results of the experience of surgeons
as to the important question which you, sir, have brought before the
Clinical Society, viz., the origin of hospitalism or pyaemia from external
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causes. But inasmuch as it is extremely difficult, when the causation of any disease is discussed, not to get on the other subject, the question of its nature, it is natural enough that that point should have been brought before the Society by various speakers; and this constitutes my excuse, not for speaking at any length upon the nature of pyaemia, but for taking it as a starting-point. If I were asked the question, straightforwardly and categorically, what do I think is the nature of pyaemia, I should certainly refuse to answer; or, at all events, I should content myself with placing before my interrogator the opposing opinions which are entertained upon the subject, and leave it to him to choose which of them he preferred. But if the question were asked by some one who really desired information—as, for example, by an intelligent student—I certainly should not give him the husks of opposing opinions. I should endeavour to explain to him that, although we know very little indeed about pyaemia, and should like to know a great deal more, yet we do know something. There are, I think, some certainties at which we may arrive with reference to pyaemia. In the first place, I think: it is a certainty, and has been shown to be such during the progress of the present discussion, that there is such a thing as hospitalism. To deny it would be to deny the whole experience of surgical hospitals, and more particularly of hospitals devoted to puerperal cases. It is altogether impossible to disregard this evidence; therefore we may put this down as the first certainty with reference to the disease—a certainty which I am sure there is no one in this room disposed to question. Then the next certainty is that pyaemia is not always dependent upon external causes. The evidence of this is to be found in the cases which you, sir, have brought before the Society, and there are a number of other cases, more familiar perhaps to physicians than to surgeons, which very much strengthen this position. In the first place, there is the category of cases, to which Dr. Stewart referred in the course of one of the discussions, resulting from typhus fever—cases which occur in the period of what would otherwise be convalescence. I do not, of course, refer to cases in which there is diffuse cellulitis, as, for example, in the neighbourhood of the parotid gland, often occurring after typhus fever, but rather to cases of actual pyaemia, cases characterised by all the symptoms of pyaemia, and usually associated with pyemic arthritis. These constitute one series, and they lead on to another set of cases, which are likewise tolerably well known to physicians—cases of pyaemia of the most exquisite character, having both the local and constitutional conditions of pyaemia, and resulting from acute necrosis of various kinds. Of course, the most common of these cases are those which arise from necrosis of the temporal bone—cases which run a chronic course for a certain time, and afterwards symptoms of pyaemia break out; cases which are characterised, again, by pyemic arthritis, and are accompanied by the characteristic off-and-on fever of pyaemia, alternate shiverings and high temperature, in fact, presenting the characteristics of the disease in its most exquisite form. Of course, the cases associated with necrosis of the temporal bone might possibly
be supposed to be of external origin. The suspicion might arise, in
the case of the temporal bone, that the infection had been introduced
into the primarily affected parts from without, but this certainly could
not apply to the many other cases in which the disease commences from
necrosis of other bones. Many such cases as these are on record, in
which there is not even the possibility that the disease can have arisen
from without. Well, by the combination of these two certainties, one
comes to this conclusion, that one must not regard the question of the
external origin of pyæmia, the influence of external circumstances in
producing pyæmia, as an alternative question. It cannot be regarded
as a question on which one must entertain one opinion or the con-
trary. We must admit both; we must admit the influence of hospita-
listism and of external conditions generally; and, on the other hand, we
must admit that the whole process of pyæmia can originate in the
organism independently of external influences. That is completely
supported by the results of experimental pathology, it being well known
that the whole series of pyæmic phenomena may be induced in animals
under conditions which absolutely exclude the influence either of atmo-
spheric or other external causes. Then I think another certainty which
we may arrive at, with reference to pyæmia, is this, that it is a process
which has a beginning and a termination of a definite kind; that is to
say, that in every pyæmic process you may distinguish a focus, a centre
of origin, lines of diffusion or of distribution, and secondary results
from the distribution. In every case you have an initial process in
which infection commences, from which the infection spreads, and
secondary processes which arise out of this primary one. Well, this at
once suggests important questions, and perhaps the most important and
fundamental questions in the pathology of pyæmia. In the first place
we have to consider what is the primary process, what goes on at
the focus or the foci (because, of course, you may have any number of
foci); and, secondly, how the diffusion or distribution of the infective
material takes place. As regards what goes on at the focus, I think
there is one point we may assume to be tolerably certain, and which
everyone would admit, that you must have there the process of inflam-
mation. And if that is the case, the next question that arises is, what
is there special in this? Because we know that processes of inflamma-
tion, which are perfectly similar to each other in their general cha-
acters, may in one case prove to be infective, that is, may give rise
to general symptoms and secondary inflammations elsewhere, and
in another case may fail to do so; consequently, there must be some
special character in a local inflammation which gives to it the power of
infecting the system. One knows what answer would be given to this
question by many pathologists, and by several who have spoken during
this discussion. They would say at once, it is the septic character of
the inflammatory process, it is the destructive character, the necrotic
character of the process. Well, fact and observation do not support
this conclusion; neither the facts of clinical experience, I think, nor
certainly the facts of experimental pathology. In the first place, we
know that purely septic products never produce the intense irritation which is produced by pyaemic products or by the products of pyaemic inflammation; and, on the other hand, we know, what I think is a striking enough fact, that all pyaemic products lose their irritative property—their property of exciting the tissues to inflammation—by being kept. That is to say, a liquid which, in the first instance, when it is perfectly fresh, is extremely irritant, and cannot be brought into contact with living tissues without exciting a most intense reaction, after a few hours, that is, as soon as the septic process has commenced, gradually loses that intensity of action, and passes into a much feebler kind of action, which one may truly recognise as septic. Consequently, we may entirely separate from each other those two modes of action—the septic action, and the action which is dependent upon the peculiar inflammations which are infective. But, at the same time, we must admit that there is no pyaemic action which has not, at the same time, septic qualities. It is now well established, I think, that in every form of pyaemic inflammation—whether it be a primary or a secondary one—in every focus of pyaemic action you have always the presence of septic products. Then the other question is how these products are diffused; and here, again, we come across the opinions which have been expressed by one or two speakers. The ordinary surgical answer to the question would certainly be, 'I believe, at present, that it takes place through the venous system; that thrombosis of the veins, with phlebitis and the formation of emboloi in the circulatory system, are the necessary conditions to the completion of the pyaemic process.' Well, there are a great many reasons why pathologists have now extended the meaning of pyaemia far beyond the limits of those cases which are characterised by the fact of embolism on the one hand and the fact of phlebitis on the other. In the first place, we know that emboloi may be introduced into the circulation without producing phlebitis, may lodge in the tissues and remain there, and not produce any results, even though embolism consist of fibrine. Secondly, we know that substances which are not large enough to be embolic at all, which are not of a sufficient size to plug any vessel, yet produce pyaemic results. And, further, we have examples which show that, in many cases of pyaemia in which all the characteristic symptoms of the process are present, there is no affection of the veins at all; that, in fact, the channel of communication is the cellular tissue. So all pathologists, at the present time, I think, are agreed in believing that the channel of conveyance from the first focus to the secondary foci, and from each secondary focus to the other foci (which to them are secondary), may be either the lymphatic system or the veins. We must not assume the one or the other to be the exclusive carriers of pyaemia. The practical result of all this is that we really do know very little about the process, but that one or two things we know with tolerable certainty. Then as regards its bearing upon the practical question, not only of the causation, but of the management of pyaemia, it strongly supports the desirableness of fixing our attention especially upon the origin of the
disease; that is, upon the original primary focus from which the process starts, and, consequently, the desirableness of following out those indications of treatment which have been particularly referred to this evening by Mr. Spencer Wells—the immense importance of watching against those circumstances which surround the infecting surfaces at the time which must necessarily correspond to the beginning of the process. Then, further, as regards the prosecution of pathological research, the direction in which we ought to work for the purpose of advancing our knowledge of pyæmia appears to me distinct enough. What we have to do is not to study the ultimate results of the disease in post-mortem examinations, but to study the starting-point in each case and investigate it, in the confidence that by this method only we can arrive at results corresponding to those which have been already arrived at in experimental pathology—to follow out, in fact, the same course that has already been followed in experiment, and to look for the primary indications of the channels of absorption. The examination of the lymphatic system in connection with the foci of infection is a question of great importance in human pathology, as it is in the pathology of the experimental studies which have been already pursued. It is not the case that no studies of this kind have been undertaken, but they are very few. I may refer to one interesting set of cases lately published by Dr. Heiberg, of Christiania, with reference to puerperal pyæmia, in which this sort of careful anatomical investigation of the tissues immediately connected with the source of infection has naturally led to very interesting results. In surgical pathology there is no doubt that it might also be pursued, as it often happens that amputated limbs give us very excellent opportunities of obtaining the tissues about parts which are the seats of origin or centres of location of pyæmic infection, and of studying such tissues in a perfectly fresh state.

Dr. Fayrer: I came here this evening with no intention of speaking; but as I have had, unfortunately, a very large experience in the disease which has been discussed here, I cannot sit still after the invitation that has been given me to say something. For the last thirteen years of my life I have had to do with one of the largest hospitals in India, where, unfortunately, pyæmia was only too rife in all its worst and most concentrated forms. I feel much diffidence and great reluctance to say anything in the presence of men whose names I have hitherto known, with whom now I have had the opportunity of making personal acquaintance, and from very many of whom I have learned what little I may know, except in so far as it may have been supplemented by practical experience and observation. I have been somewhat surprised to hear pyæmia spoken of as a surgical disease. Perhaps it may be that, being a military surgeon, and therefore also a physician, I am not accustomed to regard disease from the two points of surgery and medicine apart. Be that as it may, I regard pyæmia as a disease which is not more surgical than it is medical or obstetrical. I believe you see the worst forms of it in all; I am perfectly certain that I have. I have not had the advantage of hearing the former discussions; I
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wish I had done so; and I confess I have not read, as I ought to have done, what has been written on the subject; therefore, I am hardly in a position to speak as to what may have been said here. I believe one point in the discussion has been whether pyaemia is simply a question of hospitalism or whether it occurs in private practice. In my experience it occurs largely in both. I have always looked upon it as essentially a hospital disease, a result of overcrowding, of too little superficial and too little cubic space, from the massing of sick men together. How it is produced in that way I have never been quite able to know; but when I was at the height of my conviction upon that point, when for two, or three, or four years I had had the worst forms of pyaemia under my care that could possibly present themselves, something that made all surgical operations so hopeless that one dreaded to perform them, especially if they involved a bone; I then also, under the most favourable circumstances, in isolated rooms at the top of well-ventilated houses, with every precaution that cleanliness and sanitation could devise, had some of the most aggravated expressions of the disease pyaemia. For example, a man operated upon for stone died after a few days (although for the first three or four days he had been doing perfectly well) with his knee-joint full of pus and his pericardium full of pus. Another man died in the same way, after operation for stone, in an extreme state of jaundice. You may say that is not pyaemia; at all events it is blood-poisoning. I confess the term pyaemia is not one that, to my mind, commends itself. I am not aware that pus is taken into the blood, and, if it be not, why should it be called pyaemia? Septicemia is a better term, or even hyperinaemia. Toxikæmia, to my mind, is better than any, because it is an undefined term which expresses the presence of a poison. Where it comes from precisely I know not; but I believe it takes place in the presence of suppuration, not of healthy suppuration, not of the ordinary suppuration that takes place in a common wound or abscess, but under certain conditions, most frequently when men are crowded together in hospitals, but occasionally under other circumstances. It is quite unnecessary to detail the symptoms of the disease; Dr. Gordon has given them to you, and I have no doubt you know them all much better than I can describe them; but, perhaps, I might have said something about them as I have seen them. The cases I observed were of the most aggravated form, with rigors, profuse sweatings, and very rapid death. I should like to say something, because it is a subject that has always interested me, about the pathology, the structural changes that take place. I have heard constantly about diffused abscesses, and I have seen them frequently. I have seen a man take a long walk, become much fatigued, and very nearly die afterwards from diffused abscesses in the areolar tissue, underneath the muscles. That, I have no doubt, is a sort of pyaemia. I have seen that take place in people who lived in crowded neighbourhoods, ill-fed, and living under unfavourable conditions. There is, I think, an idiopathic pyaemia that does not follow wounds, but does especially follow that affection of bones in which the medulla becomes involved in the
condition first described by the French surgeon, M. Roux, as osteo-
myelitis, or osteo-myelitis. That, I think, has produced the worst form.
Examination after death did not reveal collections of matter, but it
revealed local gangrenes and local deaths in large numbers throughout
the lungs, frequently in the liver, and not unfrequently in the spleen.
That they were abscesses I do not deny; but that they were abscesses
in the first instance I do deny. They were deaths. Just as a bit of areolar
tissue dies under the skin, and forms a core of a boil, around which
suppuration takes place afterwards to get rid of it, so a portion of a
lung died, or a portion of the liver, or of the spleen, or of the kidneys,
or of any of the viscera; and if the person lived long enough, if he did
not succumb to the poison, suppuration took place round about these
portions of dead tissue, and you found a collection either of puriform
or purulent matter as the result; but very frequently death took place
before any suppuration could possibly have occurred. That is one
frequent result of pyaemia, as I have observed it in Calcutta and in some
other parts of India. But there is another and a peculiar condition of
the blood, that poisoning of the blood, whatever it may be caused by,
whether due to that indefinite thing malaria, or to some defective power
on the part of the liver or spleen, insanguinification, which leaves the
blood in a peculiar hyperinotic condition, which makes it coagulate and
form fibrinous clots where it ought not to do so. That, in my expe-
rience, has been one of the worst and most fatal results of the disease,
which, for want of a better term, I have been accustomed to term pyaemia
(but I have never been satisfied with the term), clots forming in the
right side of the heart, or in the pulmonary artery, and so causing
death. These are conditions which we have been accustomed to look
upon as results of what is called pyaemia—abscesses frequently under
the muscles, in the liver, and elsewhere. Then, as to the curability of it,
I think it would be hard to say it is always fatal, but it very frequently
is; I believe, if it go on to structural changes, such as I have described, it
always is. I am speaking of the disease as I observed it. I am inclined
to think it is totally different in this country, because it is impossible
that such things as I have seen could escape the observation of those
who have studied the disease here. I only wish that there was some
one here who had seen as much of it as I have in India, and who could
corroborate my views. I think it may not be incurable at times. I
remember one notable case, in which a young man, who had all the
worst symptoms of pyemic disease, with osteo-myelitis of the femur,
recovered after amputation at the hip-joint, removing the whole of the
affected bone, this being done just in time before the structural changes
took place, which must inevitably have resulted in his death. As to the
milder cases, I should think they frequently recover. It is much too late
to speak about prophylactic measures, but I believe they are all comprised
in these: cleanliness, sufficient space, proper nutrition, and good air.

Dr. Moxon: I scarcely like, at this late hour, to occupy the time of
the meeting, but what I have to say, fortunately, will compress itself
into a very few words. This discussion has a similarity with one that
is being carried on in a sister Society (the Pathological), and the similarity is, to my mind, instructive. In each case the inquiry is as regards certain local changes—are they constitutional or not? And it is curious to observe that the modes in which the local is thought to become constitutional are very similar in the two cases. Indeed, the question how a sore shall develop pyaemia is so like the question how a tumour shall develop a general carcinosis, that, I take it, it would be extremely difficult for anyone to define that difference. So that much of what is said in the sister Society will, no doubt, apply to the question before us, especially when viewed in the light to which I wish shortly to allude; that is, in reference to the probable existence of a general constitutional condition, which, I think, has not received quite so much attention from recent pathological writers as might have been devoted to it. I have myself seen a good deal of pyaemia, having inspected nearly all the cases that have occurred at Guy's Hospital during eight years. I heard something of the circumstances in each of these cases. Of course, many of them turned out to be of little interest, being common and ordinary; but a few had special features, and it was to bring forward the special features of two of the cases that I rose. The cases were not unprecedented. You, sir, have mentioned two that were similar, and which occurred in your private practice; they were cases of pyaemia arising from gonorrhœa. I saw two of these cases myself; and I recollect, further, that I was made acquainted with another by Dr. Habershon, on the occasion of one of the inspections, when he mentioned to me that he had several years before seen a case of pyaemia arising from gonorrhœa. I have also twice examined cases of idiopathic pyaemia; I do not mean of the kind alluded to by Dr. Fayrer, that is, pyaemia arising from acute periostitis, but I mean cases where the person had burst quickly into a pyaemic fever, so that, throughout the viscera and in the joints, pus rapidly developed in immense quantities, no local sores being discovered to explain the pyaemia. I searched for these carefully. Thus in one case, thinking that a carious tooth might have done it, I pulled out all the man's teeth to examine the maxillary antra, and I had previously looked through every other corner of the body. These were certainly cases of idiopathic pyaemia—what else could they be? Now, one cannot suppose but that some very grave constitutional cause must have brought about such a result as this, both in the gonorrhœal and the idiopathic cases. What should make one person with gonorrhœa get pyaemia when so many thousands escape? There must be something very special in the constitution of the person, and that something, as a constitutional factor, must surely be very important when the local factor in the idiopathic cases is absent, and in the cases of gonorrhœa is so striking. I examined the urethra in both cases of gonorrhœa, and there was no breach of the mucous membrane. Such cases appear to me to be parallel with scarlatinal rheumatism. Certainly one observes in young children, who have not any ulcers in the throat, but only scarlatinal inflammation there, a certain amount of rheumatic symptoms, not always running on to pus-formation,
but often doing so. All these forms of the disease must be thought of before we can regard pyaemia from so narrow a point of view as would see in it only a local condition spreading along very defined lines, and extending until it secondarily affects the constitution, as some would teach. I think, when we look upon these cases—I speak especially of scarlattinous rheumatism—we must say that the constitutional side of pyaemia is of very great importance. And, while I am speaking of scarlattinous rheumatism, I should like to remind the Society of certain very important statistics which Dr. Braxton Hicks has published. I believe he states that, after investigating carefully the causes, in a great number of cases, of puerperal fever, he found that a large proportion—about 65 per cent.—were clearly traceable to a direct exposure to measles, diphtheria, or scarlatina. I think that statement is of the utmost importance, and, coming from Dr. Braxton Hicks, it cannot be doubted. I believe I am right in saying that this kind of investigation is rare. If such investigations were more general—if, as Mr. Spencer Wells has suggested, all the circumstances that could throw light upon the general conditions of the system in these terrible cases were inquired into—if every contagion could be hunted up, we should get lights on the subject that would be more fruitful in our clinical success than are those mere pathological investigations which are now so fashionable. I confess I was sick of cutting up the veins and slitting up the lymphatics; and, when I came upon the accounts published by Dr. Hicks, it seemed to me that a new light was opened upon pyaemia; and I am still in hopes that the work initiated in this Society will be fruitful in teaching us something that will help us forward in this truly very promising direction.

Dr. Glover: Would Dr. Moxon tell us what he saw in the veins and lymphatics?

Dr. Moxon: It is a long story.

Dr. Glover: If he has seen every case in Guy's Hospital for the last eight years, and made a careful examination of the lymphatics and veins, I am sure, in a few words, he could tell us whether he saw anything to explain the facts that have been referred to.

Dr. Moxon: I may mention that in one case, which I should certainly call a case of pyaemia from the general conditions, there was a great wound about the hip which had led to swelling of the leg. On examination I found that the swelling arose from the inflammation about the wound having entered into the crural vein. The vein was full of clot, and the clot, when examined by the microscope, was pus. That is a point that, I think, has not been sufficiently asserted. It has been asserted, following Professor Goodsir's old observations, that clots in the veins are not pus. That is quite contrary to my experience. I quite admit that Goodsir was right in describing a process of disintegration of clot, but I am sure it is an entire mistake to say that clots that are found in the neighbourhood of suppurations—clots within the veins—are not pus. I say that, in nearly all cases of thrombosis of the cerebral sinuses, the clot is pus. It certainly was so in the case to
which I referred. The clot ran right up the vein, and pointed its nose into the right auricle. In the pulmonary artery of the upper lobe of the left lung there was a large piece of clot, which under the microscope was identical with pus. The portion of lung which corresponded with that artery was broken down into a mass which would be called abscess. There were no other abscesses in the lung. I give that as a typical example of secondary suppuration—the real conveyance of the disease from point to point.

The President: The time has now come when this discussion must close, and I will occupy the very few minutes that remain by speaking only on one or two points. First, it is quite clear that it has brought out before us the great divergence of opinion existing among surgeons as to what is and what is not pyæmia. I find myself directly opposed to one of our first authorities and teachers in London, Mr. Erichsen; and also to another great provincial surgeon, Mr. Cadge. Those gentlemen would limit pyæmia, at any rate true acute pyæmia, to abscesses which may be called visceral and multiple, so that any case that had no multiple abscesses, and no visceral abscesses, was not, for them, pyæmia. Now, that is a point I cannot admit. I admit that pyæmia has external abscesses, and that they are not altogether confined to the internal organs. I admit, whenever I find pus in the joints, in the serous membranes, in the peritoneum, or the pleura, or the pericardium, that they are all cases of pyæmia. I do not want, for a case of pyæmia, multiple abscesses at all. If I have one abscess, I consider that a pyæmic abscess, under certain circumstances. Mr. Erichsen pointedly asked me whether I consider the case of a slight wound or chafing on the heel, combined, as it had been, with a large deep-seated abscess in the thigh, to be a true case of pyæmia. I have no hesitation in saying that I do consider that a true case of pyæmia, and I cannot conceive why it should not be admitted to be such. As a matter of course, there are certain restrictions. Supposing it can be clearly proved by the surgeon (and one must take the surgeon for what he is worth) that there is no inflammation whatever of the absorbents spreading over that wound, that there is no inflammation spreading along the cellular tissue of the leg; if it had spread along the cellular tissue of the leg, it must have spread under the subcutaneous cellular tissue of thigh, and of this there was no sign. There was merely a small wound at the heel, and this large deep-seated abscess in the thigh. Then the discussion has brought out another point very markedly—that is, that pyæmia exists in private practice. And here I will refer to Mr. Spencer Wells' question. I had no intention, in bringing these cases before you, of instituting any comparison whatever between hospital and private practice. The title of my paper was, 'Cases of Pyæmia in Private Practice.' I did not mention anything about hospitals, neither had I any intention of doing so. I merely wanted to show that I had seen a certain number of cases in the years in which I had been in private practice. I therefore cannot answer Mr. Spencer Wells' question, and give him the relative number of cases that I have seen in hospitals and in private practice.
Having settled that there are a number of cases occurring in private practice, it is clearly proved, as a matter of course, that all these cases are not due to what is generally called 'hospitalism.' And here I would say that 'hospitalism,' in this discussion, has been used under two very different significations. It was first used, by Sir James Simpson, as meaning the hospital itself, and nothing else; and the deduction that he drew was that we were to pull down our hospitals and build new ones; that we were to divide the wards into smaller wards; that the larger the ward, the more infectious it was; and that the older the hospital, the more infectious it was. With such deductions as those, supposing them to be true, the word 'hospitalism' is a proper word; but now I heard 'hospitalism' described as being, simply, the overcrowding and aggregation of a certain number of bad cases in a ward. This has nothing to do with the hospital; it has to do with the surgeon—with the person who has charge of the hospital. It is simply a case of maladministration. I confess I am one of those who, like Sir James Paget, regret very much the use of the word 'hospitalism' in this new sense. I do not know that there is any word that can do more harm. When Sir James Simpson first brought out his work, wherever I went, in my own neighbourhood, I found people declining to become governors of hospitals. 'What is the use of it?' they said. 'See what Sir James Simpson says. You are to pull down your hospitals and build new ones. You are not even to have those; you must have tents and huts.' That was constantly said in the West End of the town. It was a subject of conversation wherever I went. I have no doubt that the result was that our hospitals lost a great many subscribers. I therefore look upon the present use of the word with some little degree of fear, for we must bear in mind that these discussions are reported in the medical journals, which go into the clubs and are read there. Patients will now say (and very naturally), 'What is the use of my going into an hospital? I had better die of my own disease than of one that I shall be sure to catch if I go there.' Then there is another point that has been brought out very prominently in this discussion, in reference to what has fallen from some gentlemen with regard to contagion. It has been said that hospital surgeons can carry this disease about with them, and convey it to their patients. Now, I think, if the cases I have brought before you had been very carefully attended to, this question could not possibly have arisen. There were twenty-one cases under my own care, and of those there were twelve that I literally had nothing whatever to do with, being simply called to visit them in consultation. Some of them were 200 or 250 miles away from London. All that I had to do was to say, 'This is not a case of typhoid fever, but a case of pyæmia;' and so it turned out. There were two cases of pyæmia with ulcer of the tonsils, and in both these cases pyæmic symptoms had set in before I saw the patients. There were three cases of pyæmia with fever. It is said that they were not cases of pyæmia. All I can say is, that I looked upon them as such, simply because there was ulceration of the bowel. If it be known
that ulceration of the skin can lead to pyæmic symptoms, why may not ulceration of the bowel do the same thing? It is true that there is a form of fever called by Sir William Jenner pyogenetic, but in it abscesses appear at a much later period than in the cases that I have brought under your notice. With regard to the case of injury to the foot, I was called to see it fourteen or fifteen miles out of town, and the symptoms of pyæmia were there, the abscesses were there, when I arrived. In one case I went 250 miles to see a young lady with inflammation of the lateral sinus, spreading down the internal jugular vein. I told the surgeon that it was not a case of fever, but of pyæmia, and that, in all probability, in a week or ten days, we should have abscesses. In a week or ten days the abscesses came, and I had to open them. Then there were three cases of gonorrhœa, and another case of ulceration and abscesses. All those cases I was called to see after the symptoms of pyæmia had set in; so that I had not on my conscience the crime of having taken pyæmia to my patients. Mr. Erichsen states that he has never seen pyæmia in private practice—

Mr. Erichsen: No, no; after operations in private practice.

The President: Mr. Erichsen has had a great number of cases of pyæmia in hospital practice. As a matter of course, they were contagious, and if the disease could be carried from one to another, why should he not have carried it about as well as myself? Mr. Cadge said that he was afraid of operating in his hospital because pyæmia was so rife, though he was confident enough with regard to operations in private practice. Why should not Mr. Cadge carry about pyæmia as well as myself? I think one of the very best proofs we have had was that given by the first speaker—Mr. Jonathan Hutchinson—who told us that he had a number of ewes that had died from pyæmia. Now, surely nobody could accuse Mr. Hutchinson of having taken these ewes pyæmia, yet they all died of it. They were examined; they had multiple abscesses; and there was no doubt whatever of their having died from pyæmia.

The discussion terminated with a vote of thanks to the President, on the motion of Mr. Callender, seconded by Mr. Erichsen.
COMMUNICATIONS.

I.—Case of Parasitic Sycosis. By Tilbury Fox, M.D.

PARASITIC SYCOSIS is so very uncommon in London, at least in my experience, that it is clinically important to note individual cases as they occur; and the case itself, which I brought under the inspection of the members, will I doubt not be examined with interest.

A gentleman, Mr. J. C., was sent to me by his medical man on Oct. 9, for my opinion. He stated 'that his face had been bad for six months.' The disease began as a small red scurvy spot at the upper part of the left lip, and from thence it gradually spread in the form of a ring, with an inflamed and very distinct edge. After a while, and when it had invaded the whole of the left cheek, the disease appeared to die away at the upper and outer part. But the disease had spread meanwhile to the chin below, though not much to the right side of the middle line. The general area of the circular patch was red, dry, and scaly; the edge, as stated before, being exceedingly well defined, and raised. Latterly small streaks or pustules have appeared here and there, especially about the left upper lip and chin. When I saw the patient, the disease in the cheek had disappeared; but the left upper lip, the side of the mouth, and the whole of the left half of the chin, was diseased, being red and scurfy, with a well-marked elevated scaly edge, the surface here and there being studded with little tubercles the size of a split pea and less, some six or seven in all. These lumps appeared during the last month; 'they seem not to come to a head, but die away, as it were, to re-appear in some contiguous spot.' Under the chin is a second patch, having all the characters of tinea circinata, and of the size of a shilling.

The patent complains of itching in the part. His disease had been treated with parasiticides, but apparently with only slight benefit.

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On pulling at the hairs seated in the little tubercular swellings, they come away readily, and are seen to be swollen and opaque; on placing them under the microscope they are observed to be loaded with mycelial threads and conidia, whilst these same elements surround the hair-shaft in abundance.

Remarks.—The case is interesting on account of its rarity, and as illustrating the relation between ordinary ringworm of the surface and parasitic sycosis; for the tinea sycosis in this case commenced as tinea circinata, a distinct patch of the latter existing also under the chin when I first saw the patient. It will be observed that the disease, in this case, was unlike the common non-parasitic or simple sycosis (eczema of the hair-follicles of the beard), in the fact of its being unsymmetrical, in beginning in an indolent manner, in the development of tubercles that progressed very slowly, and with very little, if any, suppuration; and further in the fact that the hairs were loosened in the tubercular parts, came away very readily and without any pain on being pulled at, and without their attached root-sheaths; whilst they were, moreover, loaded or surrounded by fungus elements.

The treatment suggested was depilation, and the use of a weak nitric oxide and ammonia-chloride of mercury ointment, freely, in connection therewith. It is scarcely necessary, perhaps, to remind the Society that the occurrence of parasitic sycosis is denied by Hebra.

II.—Cases of Spasmodic Asthma treated with Hydrate of Chloral. By C. Theodore Williams, M.D. Read October 10, 1873.

It will be generally admitted, I think, by members of the Clinical Society, that few diseases present more difficulties of treatment than spasmodic asthma; for while some cases seem to yield easily to the action of drugs, others are very intractable, and will stand the fire of the whole Pharmacopoeia battery without flinching, and then, as if to mock us, subside under the influence of mental emotion or excitement, or some other agent whose unruly workings are incapable, at present of being reduced to the exact laws of science.
Any addition to our stock of anti-spasmodic remedies is acceptable; and considering how wonderfully chloroform acts on some asthmatic patients, it seemed only natural to give a trial to a drug which closely resembles it in chemical constitution and can be more safely administered, viz. chloral.

Professor Biermer, of Zurich, has treated asthma with chloral for several years; and although I have seen no notice of the subject, I believe that in England others beside myself have been employing it in the treatment of this complaint. It is with the view of eliciting the experience of practitioners on this point that I venture to bring three cases of spasmodic asthma treated with chloral before the Clinical Society.

**Case I.**

Mrs. W., æt. 23, a native of the Isle of Man, consulted me Aug. 23, 1873. There was no asthma in the family, and her health had been good up to November 1872, when after severe cold she was attacked with symptoms of spasmodic asthma, combined with catarrh, and had been liable to the former complaint ever since. Dr. Wood, of Douglas, who attended her during the greater part of her illness, wrote to me as follows:—"The attacks usually lasted about three weeks, then ceased for a week, and then recurred. Though the dyspnœa was troublesome, she could generally lie down. The cough was incessant, and only relieved after large quantities of thick expectoration had come up. The appetite was bad, especially as regards solid food. The pulse from 100 to 120, and small; the temperature never exceeded 99°F. and often fell to 97°F. During the attacks there was hyper-resonance of the whole chest, and loud wheezing sounds. Some improvement was observed during a visit to Manchester, and on the whole the symptoms have been rather less in summer than during last winter, when for four months she did not quit her bedroom. The ordinary remedies have been tried and found of no avail, though inhalations, particularly of creosote, did most good."

The patient travelled in one day from Douglas to London, and on the following day, Aug. 23, came to my house. She had slept well in an hotel in the Strand, but was still greatly fatigued from the journey. Aspect worn and weary; pulse 80, weak; respiration and temperature normal; slight cough, with frothy expectoration; bowels regular; cata-
menia every fourteen days. On examining the chest I found some hyper-resonance, and the breath-sounds weak. The thorax was well formed; the heart-sounds were healthy, and there was no displacement of organs.

She stated that she suffered most in a damp atmosphere; and therefore, hoping that the comparatively dry air of London might prove a beneficial change, I prescribed nothing but an effervescing potash draught, wishing to test the effects of change of climate first.

Aug. 25.—She slept well the second night, but on the following day, during a heavy thunderstorm, the asthma returned with marked violence. The fit began at 3 or 4 in the afternoon of the 24th, and lasted all night, and she was conveyed by her husband to me in a cab at 1 o'clock to-day.

Aspect dusky; dyspnœa great; respirations, 40; pulse, 100; cough incessant; rhonchus and wheeze audible throughout the whole chest. The spasm is also accompanied by vomiting.

I sent her home, and ordered her to take 20 grains of chloral every three hours.

On visiting her later in the day, I found that after the first dose the dyspnœa began to subside, and she slept an hour.

26.—Has taken three doses of chloral and slept soundly last night, but has taken no food for twenty-four hours. There has been no return of the dyspnœa, and to-day she breathes easily, but some rhonchus is still audible over the posterior regions of the chest. After this she took one more dose of chloral, and the following day no wheeze was audible anywhere. The chloral was then discontinued; and though she remained a week longer in London, no fresh attack occurred. She left for Paris, and since then I have not heard of her.

Remarks.—This case was peculiar in the absence of marked orthopnœa, and was less severe on this account, but the long duration of the asthmatic attacks did not augur well for treatment. The sudden relief after the first dose of chloral, and the cessation of the spasm after a few more doses, are phenomena which can hardly be attributed to anything else than the drug's action, though the non-recurrence of the fits may be possibly assigned to the patient remaining in the smoky London atmosphere.
Case II.

Wm. McC., a red-haired youth, æt. 16, was admitted into the Brompton Hospital, under Dr. Quain, Jan. 9, 1873. Six years ago, after a severe wetting, he had bronchitis, and since then has been subject to attacks of spasmodic breathing, generally occurring once a week and lasting three days.

The attack begins with a feeling of tightness in the chest, so that the patient cannot take a deep breath; and then follows shortness of breath, wheezing and orthopnoea; this continuing through the first night, and the next day he is slightly better, but at night the severe symptoms return. On the third day the breathing gradually becomes easy.

The lad is high-shouldered; the thorax slightly barrel-shaped. Chest not unduly resonant, except in the cardiac region. Breath somewhat feeble, but expiration prolonged, being of the same length as inspiration. No displacement of organs.

He had no attack during the first ten days of his residence in the hospital, but on Jan. 19 a severe one came on, and he had orthopnoea all night. On the following day, the 20th, I found him sitting up in bed suffering from marked dyspnoea, the expiration being difficult. Sonorous rhonchus was audible over the whole chest, and wheezing sounds over the posterior regions.

He was ordered 15 grains of chloral and 20 minims of tinct. scillæ in an ounce of peppermint-water, every four hours.

Jan. 21.—Was much relieved after the first dose, and slept soundly last night. To-day his breathing is easy, but he says his head is queer. To continue the chloral three times a day.

28.—Has had no more attacks of dyspnoea, but to-day his respiration is slightly wheezy. To repeat the chloral every four hours.

Feb. 10.—The medicine has been given three times a day; but as no attacks had occurred Dr. Quain has stopped it, and he is now taking 15 grains of bromide of potassium twice a day.

March 25.—Has been free from attacks for six weeks, except some slight dyspnoea, which occurred during one night three weeks ago, and did not interfere with sleep.

Has gained 13lbs. in 2½ months.

Breathing easy and fairly deep; pulse, 96; expiration slightly prolonged.
Left the hospital.

Remarks.—The regularity of the attack and its well-ascertained duration enabled us to judge fairly of the effects of the chloral, and certainly these seemed to have been very beneficial, first on the spasmodic attack itself and afterwards on the wheezy indications of a second nerve-storm which was brewing.

Case III.

Catherine C., æt. 27, was admitted under my care into the C ward, Oct. 25, 1872, with the following history:—No hereditary predisposition existed, and she was well and free from chest affections till she went, as kitchen-maid in a family, to Uxbridge, two years ago. There she slept on a second floor, but lived on a very damp basement. The soil was clay, and the ground rising rapidly behind the house. Cough came on with transparent expectoration, and two months later she was attacked with fits of difficult breathing, which commenced generally at 4 or 5 A.M., when she had orthopnoea and loud wheezing; the attack lasted from two-and-a-half to three hours, usually ending in exhaustion and slumber. During the rest of the day her breathing was free, and she was able to perform her duties. She returned to London and became out-patient at St. Bartholomew's Hospital for about a year, and in January 1872 she became an in-patient under Dr. Southey, and remained so till April 10. Dr. S., in answer to my inquiries, informed me that she had somewhat benefited by inhalation of nitrite of amyl. During this time she had attacks every morning, and sometimes additional attacks in the afternoon, but during her residence in the hospital she was well enough to assist in the kitchen during the day, and even carried coal-scuttles about the ward.

On admission into Brompton her cough was troublesome; expectoration frothy; tongue clean; appetite good; bowels and catamenia regular. Pulse, 80; respiration, 24; temp. 98°. During the two months previous to admission the morning attacks had lasted two hours regularly. On physical examination the chest was found very resonant. Some wheezing sounds and musical rhonchus were audible; the expiration was considerably prolonged; there was no marked displacement of the heart or liver.

In the first month of her residence in the hospital the attacks were moderate, and seemed to be somewhat benefited by small doses of stramonium and iodide of potassium.
Dec. 2.—The attack came on this morning, and has lasted ever since (till 4 p.m.).

Stramonium cigarettes, lobelia tincture, and ammonia and ether, have all been used, with no effect. I ordered her to inhale chloroform to the amount of 3 ss on a handkerchief. Mr. Bartlett and my clinical assistant, Mr. Williams, administered it. The breathing was somewhat relieved, but she became livid, and the pulse so irregular that Mr. Bartlett desisted at once, and gave stimulants. Under these the pulse became regular, but the breathing became as difficult as ever. The fits after this were more frequent and more prolonged. She got no sleep, and could not lie down at all.

On the 9th I prescribed 10 minims of tincture of belladonna, in camphor water, every four hours. Six doses of this were taken without relief.

Dec. 10.—Aspect livid; respiration, 32; pulse, 72; inspiration inaudible; expiration prolonged; heart and liver considerably displaced downwards. Ordered extract. belladonae, ½ gr.; pil. assafcetid. comp. gr. iii.; 4tis. horis.

Dec. 11.—No relief followed. Breathing was as difficult as ever. ½ gr. of morphia was then injected hypodermically. The breathing became easier, and she slept, and this morning the usual attack has been slighter.

Another attack came in the afternoon, and was again, to some slight degree, relieved by the hypodermic injection of ½ grain of morphia, but to a less extent than before.

Before the next attack came on she was removed from the ward, which she shared with three other patients, and placed in a separate one, which was kept at a rather lower temperature; in fact, at about 58° Fahr. This did not prevent the recurrence of the attacks, and as, during the next one, which occurred on the evening of the 12th, she became very livid, it was hardly thought safe to repeat the morphia. I determined to try chloral, and gave it in the following form every four hours:—Chloral hyd. gr. xv., syrpsi tolu. 5ss., tinct. scill. 5j., aquæ menth. pip. 5j.

After the first dose she fell fast asleep, and on waking up the breathing was perfectly easy. The dose of chloral was then reduced to 10 grains every four hours.

On the following day I found her asleep, the skin perspiring, and respiration 24, slightly noisy; pulse, 100. During the next four days she took the chloral, but less frequently, and no attack occurred.

On the 18th the wheezing returned, the chloral was in-
creased to 15 grains every four hours, with a similarly good effect. The dose was again reduced, but she continued to take 10 grains of chloral three times a day till Jan. 8, when I thought it desirable to omit it, in order to try how far the absence of the attack was due to chloral. The next day the breathing was difficult; she had not slept well; the attack lasted from 7 A.M. to 11 A.M., and in the afternoon she was so distended that she could not put her stays on. She returned to the chloral, and remained in the hospital taking it for three weeks. She had several attacks, but they were of a slight description, and contrasted strongly with the severe ones which occurred shortly after admission.

Jan. 25.—I examined her chest, and found the hyper-resonance diminished, some wheezing sounds were audible, but fair breathing could be detected throughout a great portion of the lungs; the heart and liver were still slightly displaced downwards. After leaving Brompton she had a recurrence of the attacks for upwards of two months, and she attended as out-patient at St. Bartholomew’s. Since that time she has been pretty well, and manages to walk long distances, but still has a slight attack most mornings.

Remarks.—There are several remarkable features in this case.

The asthma was clearly traceable to damp, as the patient enjoyed good health and was free from the slightest approach to asthma until she unfortunately took the situation at Uxbridge which obliged her, not, it is true, to sleep on a damp ground floor, but to spend the greater part of the twenty-four hours in that region. The onset of the disease marked its origin, as catarrhal symptoms preceded the spasmodic by months, but lately the catarrhal symptoms have been quite thrown into the background.

The case was a very severe one, and I did not anticipate much relief from the use of drugs; and I was accordingly pleased with the results of the chloral, which certainly did more good than all the other medicines put together. The favourable results at first were so striking that I felt strongly inclined to doubt if we were not attributing too much to the chloral, and that the change of ward and the lower temperature had not something to do with the improvement.

The immediate return of the attack on my omitting the chloral left me no alternative but to give the drug full credit for the improvement.

Another point worth notice is the failure of the chloroform.
This seems rather remarkable, as the chemists tell us that chloral is simply chloroform, plus carbonic acid and water, and that the whole action of chloral depends on the chloroform being set free in the blood by the action of the alkaline salts. The danger from chloroform in those cases is chiefly owing to the amount of emphysema present, and to the lowering action on the generally weak and dilated heart. The chloral effects gradually, it may be observed, diminished, not being nearly so potent as at first, but still controlling, though not curing the fits, and placing the patient in a much more comfortable state of health than she had experienced for years before. The partial removal of the attacks resulted in a diminution of the temporary emphysema.

In conclusion I may say that I have notes of upwards of twenty cases of asthma treated with chloral, and in all but two it has given considerable relief. One of these failures was a patient of Dr. Pollock's, in whom the asthma was curiously connected with the occurrence of menstruation. She was subjected to a variety of treatment, including chloral, without much benefit, but she was relieved at last by going to Southampton. The other failure was a patient of my own, in whom the chloral gave some ease at first, but had to be discontinued on account of vomiting and a purpuric eruption following its use. When these passed off it was repeated in smaller doses, but these had little or no effect.

Though I do not think the drug infallible, yet it seems to me to be another weapon on which considerable reliance can be placed in contending with so distressing and unmanageable a malady as spasmodic asthma.

III.—Bright's Disease in a Syphilitic Subject. Death (in the 2nd Stage) when the Kidney was still large and pale.

By REGINALD SOUTHBY, M.D. Read October 10, 1873.

JOHN BAYLEY, æt. 21, fishmonger. A well-nourished, but pale young man, with dark hair and grey eyes, quick and intelligent expression; height 5 ft. 8 in.

Family History, good.—Personal, he had scarlet fever when a child. Fourteen years ago was in this hospital under Dr. Baly for inflammation of the bowels, and was ill for some weeks with dysenteric diarrhoea; about a year and a half ago had an attack of haematemesis. He was exerting himself, felt suddenly sick, experienced some pain in his stomach, and
vomited about three pints of clotted blood. He was treated as an out-patient under Dr. Hensley, and quickly got well without recurrence of any similar symptoms.

Has had syphilis and two attacks of gonorrhoea. There are scars of suppurated buboes in both groins, and marks of circular ulcers upon the ankle. Latterly has been subject to repeated mucous bronchial catarrh.

Present illness.—On Sept. 17, 1872, he had a fall from a horse, and injured his left ankle, which was bruised, red and inflamed, in consequence. He admitted that both his legs had been a little swelled before the accident. He was admitted into Darker Ward, under Mr. Callender, who discovered that his urine contained $\frac{3}{4}$ rds albumen, and transferred him to my care.

Oct. 4, 1872.—Right foot and ankle are still somewhat cedematous, otherwise no noticeable anasarca; appetite good; some thirst; tongue clean and moist; bowels regular; urine said to average from $1\frac{1}{2}$ to 2 pints daily, is pale and of 1020 sp. gr., and contains from $\frac{3}{4}$ rd to $\frac{3}{4}$ rds albumen; feels habitually cold, and very often has slight shivering attacks, occasional headache, fugitive pains, and sudden and copious perspirations; his temperature ranges from 99-4° to 100°. Pulse, from 84 to 120, is sharp and jerky, suggesting an empty arterial system and impoverished blood. Slight systolic murmur at heart's apex, precordial dulness not increased, impulse natural. Apex beat, between fifth and sixth ribs $1\frac{1}{2}$ inch inside nipple line.

Thorax, Resonance and Respiratory sounds everywhere normal. Abdomen full, rather sensitive to pressure.

There is a squamous eruption, with copper-coloured patches on both legs.

He was kept in bed, and placed on full meat diet with imperial drink * for his thirst, and as medicine had acetici. dil. m.xx sp, æther. nitros. 3 j, aqua. 3 j, ter. The record of the progress of the case is well told by the urine-table at end of this Paper.

I found shooting pains in the arms and legs noticed occasionally as symptoms. On Oct. 21 slight cough; the urine increased in quantity up to $4\frac{1}{2}$ pints on Oct. 19, but still contained $\frac{3}{4}$ ths of albumen.

On the 24th, with two pints of urine passed, the sample boiled coagulated entirely. On that day he had aching pains in left lumbar region and vomited, and his tongue was furred. Pulse, 84; temp. 98·4°. Although his bowels were

* Cream of tartar drink, flavoured by orange-peel.
open, I ordered pil. col. co. gr.x, to be followed by hst.

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seized with acute pains in arms and calves of legs, and in head, especially behind ears; lost all appetite; felt sick and became hot and thirsty; had a severe rigor, which lasted for half-an-hour; sense of fulness and soreness in the abdomen, became distressing to him, and he vomited at length with some relief to himself.

Note at 11 A.M.—Has just broken out into a profuse sweat. Temp. 100·2°; pulse, 120, free and strong; tongue more furred, brownish fur, tip and edges red and dry; slight puffiness of face. While getting out of bed cried out sharply on account of pain which suddenly seized his left hand, but quickly subsided.

Urine, 2½ pints; sp. gr. 1018; albumen, 2ths.

20.—Had prolonged shivering last night with severe pain, and some tenderness in right and left hypochondriac regions and behind both ears; had some sleep, but awoke about 4 A.M. with another rigor.

At 9.30 A.M. his axillary temperature was 105°.

At 11 A.M. temp. 104·6°; pulse, 120; respiration, 32.

No abnormal chest sounds. Abdomen full, tender, and tympanitic, especially over transverse colon; bowels open six or seven times, relaxed; tongue as before. Throat red and dry, and tender over tonsils. I examined his body for any rash; there might be a slight blush over skin of neck and chest, but it was not well marked.

Ordered hst. quinæ, gr. v, 6tis horis.

21.—The temperature fell last evening to 103·5°, and this morning marks only 101·8°. There is no increase in pain, but he has passed a sleepless night, and the tongue is drier and more furred. At 3 p.m. the temperature rose to 105·4°; pulse, 120; respiration, 24. He could take nothing but liquids, some essence of beef-tea, iced milk, and sucked the juice of some oranges with comfort to himself.

Ordered acid. phosphor. dil. m.xv, syrup auranti. 3i, aquæ 5j, 4tis. horis.

The abdomen was become so tender about umbilicus, that I ventured to diagnose peritonitis.

22.—Passed a large ascaris lumbricoides; bowels have been much purged; feels rather better. Temp. 103·4°.

23.—Temp. 104·4°. Has had some sleep; complains of his throat; the fauces and mouth look red and inflamed; cheeks are flushed; slight blush of redness in patches over body, especially arms. Is this an attack of scarlet fever which he has contracted? Ordered a tepid bath and some wine.
The febrile symptoms suffered no abatement; the extreme abdominal tenderness betoken localised peritonitis; the temperature continued rising to 106°, which it reached on morning of the 26th; he was sponged with tepid water with vinegar, but passed into an insensible state on this morning (26th), with dilated pupils, staring protruded eyeballs, and occasional convulsive twitchings. The breathing became sobbing and stertorous before death.

Autopsy.—Body well nourished; no anasarca. Turbid greenish fluid escaped in moderate quantity upon opening abdomen. Omentum and intestines very adherent together and to abdominal wall, by thick tough fibrous bands of old adhesions. On the surface of the intestines, especially the small, were numerous soft flakes of recent exudation.

There were firm old adhesions between capsule of liver and peritoneum of the abdominal wall. Mesenteric glands considerably enlarged. Mucous membrane of small intestines swollen and hyperæmic.

In large intestines, and especially descending colon, there were scars of old ulcerations, with bridges of mucous membrane across some of them, and small outgrowths of mucous membrane around others.

Lungs, pleura, and heart normal; stomach natural; liver so adherent to diaphragm that its capsule stripped off in removal. It was large, soft, and congested. An ordinary high temperature liver.

Spleen considerably enlarged and very soft. No amyloid reaction in viscera.

The kidneys together weighed 17 oz. They were large and flabby. The capsule stripped off readily, leaving the surface pale.

Cortices mottled, of a yellowish white colour dotted with a few red points, and locally congested vessels.

Pyramids distinctly striped by congestion of the vascular areas.

Remarks.—As a case of Bright's disease this may be reckoned a highly anomalous one, or I should not have ventured to intrude it upon the attention of this Society.

It happened in a syphilitic subject, and in a man who had intestines damaged by old dysentery.

These cases of large pale kidney rarely furnish much urinary water. Yet, with occasional slight deficiencies, this patient's urine-chart exhibits an almost invariable abundance of urinary water. Then the intercurrent attacks of sub-acute
peritonitis this man had are quite anomalous complications of the ordinary disease.

The pleura and pericardium we know are frequently the seat of inflammatory attacks in the course of Bright's disease, but the peritoneal membrane is rarely invaded.

Lastly, the mode of death is peculiar. What did he die of? Why this high temperature? There was no contagious fever in my ward at the time, whose poison he could have imbibed. Yet might this not have been scarlet fever poison, introduced somehow?

Neither in acute peritonitis nor in chronic renal disease is it usual for the temperature to rise high. The fact is one which must have repeatedly forced itself upon all observers, that when the kidneys are diseased so far as to fail to eliminate the normal amounts of urinary solids, that the bodily temperature becomes depressed.

The exception in this instance is either a most rare anomaly, or we must accept the rise of temperature as evidence of some fresh blood-poisoning, producing its reaction in the nervous system. I am inclined to surmise scarlet fever or acute rheumatism as the disturbing element in this case myself. The patient had sharp pain once in the course of his febrile exacerbation. This, however, is a mere speculation upon my part, and I have brought the account before the Society because my experience of renal disease has taught me how anomalous this mode of fatal termination is in it, and especially to invite the publication of any parallel instance.
Table showing State on Admission, Oct. 11, and Progress.

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<tr>
<th>AGE</th>
<th>Weight</th>
<th>Skin</th>
<th>Temp.</th>
<th>Pulse</th>
<th>Resp.</th>
<th>Heart lesion or not</th>
<th>Dropy</th>
<th>Lung symp.</th>
<th>Digestive symp.</th>
<th>Nervous symp.</th>
<th>Other symptoms</th>
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<th>DATE</th>
<th>Urine, 24 hrs. Quantity</th>
<th>Colour</th>
<th>Coloured by Heat</th>
<th>Reaction</th>
<th>S. G.</th>
<th>Amount of Sediment</th>
<th>Albumen</th>
<th>Extractives</th>
<th>Microscopic Characters. Casts</th>
<th>Bowels A</th>
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<td>1034</td>
<td>Scanty</td>
<td>1</td>
<td>—</td>
<td>—</td>
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<td></td>
</tr>
<tr>
<td>10</td>
<td>Oij 3/4</td>
<td>&quot;</td>
<td>—</td>
<td>1035</td>
<td>&quot;</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
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<td></td>
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<tr>
<td>11</td>
<td>Oij 3/4</td>
<td>Clearer</td>
<td>—</td>
<td>1028</td>
<td>&quot;</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>12</td>
<td>Oij 3/4</td>
<td>Bright yellow</td>
<td>—</td>
<td>1015</td>
<td>Slightly turbid; more red</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>13</td>
<td>Oij 3/4</td>
<td>&quot;</td>
<td>—</td>
<td>1014</td>
<td>&quot;</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>14</td>
<td>Oij 3/4</td>
<td>&quot;</td>
<td>—</td>
<td>1012</td>
<td>Less</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>15</td>
<td>Oij 3/4</td>
<td>&quot;</td>
<td>—</td>
<td>1020</td>
<td>Turbid</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>16</td>
<td>Oij 3/4</td>
<td>Orange yellow</td>
<td>—</td>
<td>1028</td>
<td>Very thick</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>18</td>
<td>Oij 3/4</td>
<td>Muddy</td>
<td>—</td>
<td>1025</td>
<td>Slightly turbid</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>19</td>
<td>Oij 3/4</td>
<td>Orange colour</td>
<td>—</td>
<td>1018</td>
<td>&quot;</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>20</td>
<td>Oij 3/4</td>
<td>Yellow</td>
<td>—</td>
<td>1020</td>
<td>Slightly turbid</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Oij 3/4</td>
<td>&quot;</td>
<td>—</td>
<td>1025</td>
<td>&quot;</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>Turpentine treatment</td>
</tr>
<tr>
<td>Date</td>
<td>Oj</td>
<td>Colour</td>
<td>pH</td>
<td>Turbidity</td>
<td>Sediment</td>
<td>Other Observations</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Nov. 22</td>
<td>Oj½</td>
<td>Deep yellow</td>
<td>1025</td>
<td>More turbid</td>
<td></td>
<td>A few fatty casts, small diameter, long, fatty epithelium</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Oj</td>
<td>Light brown</td>
<td>1040</td>
<td>Very turbid</td>
<td>7/5</td>
<td>Passed a stringy piece of mucus, entangling red and white blood-cells and fatty glandular epithelium</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Oj</td>
<td>Orange yellow</td>
<td>1030</td>
<td></td>
<td></td>
<td>Fatty casts of medium and small diameter Fatty débris epithelium</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Oj³</td>
<td>Faintly acid</td>
<td>1025</td>
<td></td>
<td></td>
<td>Casts of same character, more abundant</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dec. 2</td>
<td>Oj½</td>
<td>Neutral</td>
<td>1020</td>
<td>Turbid throughout</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Oj</td>
<td>Faintly acid</td>
<td>1035</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>Oj³</td>
<td></td>
<td>1030</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>Oj½</td>
<td>Neutral</td>
<td>1028</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6</td>
<td>Oj</td>
<td>Darker</td>
<td>1030</td>
<td>More so</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>Oj½</td>
<td>Faintly acid</td>
<td>1028</td>
<td>Clearer</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>11</td>
<td>Oj</td>
<td></td>
<td>1030</td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>12</td>
<td>Oj</td>
<td></td>
<td>1030</td>
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<td>13</td>
<td>Oj</td>
<td></td>
<td>1030</td>
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<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Date</td>
<td>Urine, 21 hrs. Quantity</td>
<td>Colour</td>
<td>Coloured By Blood</td>
<td>Reaction</td>
<td>S. G.</td>
<td>Amount of Sediment</td>
<td>Albumen</td>
<td>Extractives</td>
<td>Microscopic Characters, Casts</td>
<td>Bowels A</td>
<td>Observations</td>
</tr>
<tr>
<td>------</td>
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<td>--------------</td>
</tr>
<tr>
<td>14</td>
<td>Oj</td>
<td>Darker</td>
<td>—</td>
<td>Neutral</td>
<td></td>
<td>More turbid; completely solid, with heat and HNO₃</td>
<td>—</td>
<td>—</td>
<td>Urine full of fat casts</td>
<td></td>
<td></td>
</tr>
<tr>
<td>17</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1035</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>18</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1030</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>19</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1018</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1020</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>23</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1015</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>24</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1015</td>
<td>Less turbid</td>
<td></td>
<td>—</td>
<td>Medium &amp; small sized fatty casts abundant</td>
<td></td>
<td></td>
</tr>
<tr>
<td>26</td>
<td>Oj½</td>
<td></td>
<td>—</td>
<td>Acid</td>
<td>1012</td>
<td>½ of oj</td>
<td></td>
<td>—</td>
<td>No blood corpuscles seen Large-sized granular and epithelial casts; long, thin fatty casts, renal and vesical epithelium, undergoing fatty degeneration. Fus-cells? crystalline aggregations of vibriones, small uric acid crystals</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

It has long been believed, and many physicians still believe, that the epiglottis protects the cavity of the larynx so effectually that nothing can penetrate as far as the glottis, and yet in the case of simple catarrh accompanied by hoarseness—that is to say, expressly implicating the vocal apparatus—they do not hesitate to prescribe local emollients, such as tisanes and demulcent gargles; and success encourages this kind of treatment, which is of everyday frequency. The fact ascertained, and the cure obtained, they do not hesitate to seek the explanation; or they confine themselves to putting the question, if there be not a therapeutical action on the intra-laryngeal mucous membrane exerted from a distance, and by continuity of tissue, by means of the mucous membrane of the deep parts of the throat? The laryngoscope, by allowing a view of the local organ, gives the key to this mystery; and the essence of this demonstration is to show that a fluid can pass through the epiglottis, and that it then bathes the glottis itself and all the portion of the vocal chords accessible to view in the laryngoscopic mirror during the act of closure of the glottic aperture.

The experiment is made with a small quantity of fluid, so calculated as to fill pretty exactly only the sub-epiglottic cavity. I take a small quantity of water into the mouth, and throwing the head slightly backward, I let it drip by its own weight into the laryngeal or sub-epiglottic cavity. I introduce the laryngoscope, and the liquid is very easily seen, subjacent to the epiglottis, which may be dry; the fluid may be seen to bubble in the supra-glottic cavity under the influence of little bulks of air which expire through the glottis. If the fluid be transparent (such as water), the white colour of the contracted vocal chords may be seen through it.

This very easy experiment causes me no unpleasant sensation, and it may be prolonged throughout the whole period of a long expiration, or indeed as long as I can hold my breath. It proves that it is possible to apply medicated fluids in the form of a gargle to the mucous membrane of
the larynx. But it does not not follow because a thing is possible that it is easy to use it in common, and it might be possible that practice and skill peculiar to myself have rendered me specially adapted for the purpose.

I have ascertained, however, that others can gargle the larynx like me; and if I have found that by some the performance of this act is at first not easily attained, I have found a much greater number, who, when properly instructed, have been able to gargle in the most natural way in the world as far as their glottis. At Cauterets, where a number of bathers gargle every year with the sulphurous waters of these important springs, I have been able to repeat them on a large scale, and to establish a method for facilitating the use of this useful ablution even for the least skilful. There are indeed many ways of gargling, and the least efficacious consists in elevating the base of the tongue and breathing by the nose, relaxing the muscles of the velum palati so as to bring the uvula into contact with the tongue. The pharynx and the tongue are thus placed altogether beyond the action of the gargoyle, which remains exclusively in the mouth. Respiration is possible by the nose; and as the buccal cavity so limited is not very deep, the individual, in order to prevent the fluid from flowing out of the mouth, is obliged, if he is standing, to bend himself painfully backwards and to throw back the head in the most fatiguing manner possible. Some prefer to sit down, or better still to lie down, in order to give themselves this sort of buccal bath.

Another method consists in opening widely the back of the mouth and throwing the head back. The liquid falls then easily into the pharynx, but the tension of the neck on the throwing the head back occasions little movements of deglutition which are not easily stopped, and which introduce in spite of oneself a part of the gargoyle into the stomach. Finally, there is a third method which includes the two foregoing, by means of which it is very easy to introduce at once into the mouth, the pharynx, and the supra-glottic part of the larynx, a medicated fluid without swallowing a drop of it. It is to this third method that my experiments have led me, and I desire to submit some practical rules for it.

To gargle effectually the pharynx and the larynx it is only necessary—

1. Slightly to raise the head.
2. To open the mouth moderately.
3. To protrude the chin and lower jaw.

In an address on medicine, delivered at Oxford in the autumn of 1868,* I referred to a peculiar form of disease occurring mostly in young women, and characterised by extreme emaciation, and often referred to latent tubercle, and mesenteric disease. I remarked that at present our diagnosis of this affection is negative, so far as determining any positive cause from which it springs; that it is mostly one of inference from our clinical knowledge of the liability of the pulmonary or abdominal organs to particular lesions, and by proving the absence of these lesions in the cases in question. The subjects of this affection are mostly of the female sex, and chiefly between the ages of 16 and 23. I have occasionally seen it in males at the same age.

To illustrate the disease I may give the details of two cases, as fair examples of the whole.

Miss A., aged 17, under the care of Mr. Kelson Wright, of the Clapham Road, was brought to me on Jan. 17, 1866. Her emaciation was very great. (Vide Woodcuts Nos. 1 and 2.) It was stated that she had lost 33 lbs. in weight. She was then 5 st. 12 lbs. Height, 5 ft. 5 in. Amenorrhoea for nearly a year. No cough. Respiration throughout chest everywhere normal. Heart-sounds normal. Resps. 12; pulse, 56. No vomiting nor diarrhoea. Slight constipation. Complete anorexia for animal food, and almost complete anorexia for everything else. Abdomen shrunk and flat, collapsed. No abnormal pulsations of aorta. Tongue clean. Urine normal. Slight deposit of phosphates on boiling. The condition was one of simple starvation. There was but slight variation in her condition, though observed at intervals of three or four months. The pulse was noted on these several occasions as 56 and 60. Resps. 12 to 15. The urine was always normal, but varied in sp. gr., and was sometimes as low as 1005. The case was regarded as one of part in tubercular case impressed me at once with find no visceral disease. Paper are fac-similes of the original photo-examination of the chest and pper was read.
4. To emit, or form the intention of emitting, the sound of the double vowel oe.

The simultaneous performance of these four movements open largely the back of the mouth, lift the velum palati and uvula, separate the base of the tongue from the posterior wall, and allow the liquid to gravitate into the cavity of the larynx.

Thus gargling throughout the whole period of a long expiration and inspiration is impossible. Those who are most skilful succeed in learning how to make the fluid come back through the nasal fossae (as is done with tobacco-smoke), thus bathing the mucous surfaces in the most complete manner. The experimental proof of the penetration of the gargle into the larynx is the impossibility of respiring. Whoever respires while gargling, gargles badly. Very little practice is necessary to learn how to gargle in this way without swallowing a drop of the fluid; the less the head is thrown back, the less the need for swallowing is felt, and it may in this way be altogether put an end to; and, on the contrary, the more the head is thrown back the greater the stimulus to deglutition and the more of the gargle is swallowed. The following shows in summary the differences between the old method of gargling and that which I propose to substitute for it:

**Old Method.**

1. Respiration continues during gargling; it is accomplished through the nose.

2. Throwing the head back is inevitable in order to keep the fluid in the mouth.

3. Deglutition is inevitable and involuntary.

4. The gargle does not pass beyond the cavity of the mouth.

**The Author's Method.**

1. Respiration is impossible during gargling. One expiration only can be performed.

2. The head is very little or not at all thrown back.

3. Deglutition is absolutely under control.

4. The gargle bathes the whole cavity of the mouth, pharynx, and the supra-glottic portion of the larynx.

and the larynx it is only

head.

moderately.

and lower jaw.
Various remedies were prescribed—the preparations of cinchona, the bichloride of mercury, syrup of the iodide of iron, syrup of the phosphate of iron, citrate of quinine and iron, &c.—but no perceptible effect followed their administration. The diet also was varied, but without any effect upon the appetite. Occasionally for a day or two the appetite was voracious, but this was very rare and exceptional. The patient complained of no pain, but was restless and active. This was in fact a striking expression of the nervous state, for it seemed hardly possible that a body so wasted could undergo the exercise which seemed agreeable. There was some peevishness of temper, and a feeling of jealousy. No account could be given of the exciting cause.

Miss A. remained under my observation from Jan. 1866 to March 1868, when she had much improved, and gained in weight from 82 to 128 lbs. The improvement from this time continued, and I saw no more of her medically. The Woodcut, Miss A., No. 2, from photograph taken in 1870, shows her condition at that time. It will be noticeable that as she recovered she had a much younger look, corresponding indeed to her age, 21; whilst the photographs, taken when she was 17, give her the appearance of being near 30. Her health has continued good, and I add a fourth photograph taken in 1872.

It will be observed that all the conditions in this case were negative, and may be explained by the anorexia which led to starvation, and a depression of all the vital functions; viz., amenorrhoea, slow pulse, slow breathing. In the stage of greatest emaciation one might have been pardoned for assuming that there was some organic lesion, but from the point of view indicated such an assumption would have been unnecessary.

This view is supported by the satisfactory course of the case to entire recovery, and by the continuance of good health.

Miss B., æt. 18, was brought to me Oct. 8, 1868, as a case of latent tubercle. Her friends had been advised accordingly to take her for the coming winter to the South of Europe.

The extremely emaciated look (vide Woodcut, Miss B No. 1), much greater indeed than occurs for the most part in tubercular cases where patients are still going about impressed me at once with the probability that I should find no visceral disease. Pulse 50, Resp. 16. Physic examination of the chest and abdomen discovered nothing
abnormal. All the viscera were apparently healthy. Notwithstanding the great emaciation and apparent weakness, there was a peculiar restlessness, difficult, I was informed, to control. The mother added, 'She is never tired.' Amenorrhoea since Christmas 1866. The clinical details of this case were in fact almost identical with the preceding one, even to the number of the pulse and respirations.

I find the following memoranda frequently entered in my note-book:—'pulse 56, resp. 12; January 1868, pulse 54, resp. 12; March 1869, pulse 54, resp. 12; March 1870, pulse 50, resp. 12.' But little change occurred in the case until 1872, when the respirations became 18 to 20, pulse 60.

After that date the recovery was progressive, and at length complete. (Vide Woodcut, Miss B., No. 2.)

The medical treatment probably need not be considered as contributing much to the recovery. It consisted, as in the former case, of various so-called tonics, and a nourishing diet.

Although the two cases I have given have ended in recovery, my experience supplies one instance at least of a fatal termination to this malady. When the emaciation is at the extremest, oedema may supervene in the lower extremities—the patient may become sleepless—the pulse become quick, and death be approached by symptoms of feeble febrile reaction. In one such case the post-mortem revealed no more than thrombosis of the femoral veins, which appeared to be coincident with the oedema of the lower limbs. Death apparently followed from the starvation alone. This is the clinical point to be borne in mind, and is, I believe, the proper guide to treatment. I have observed that in the extreme emaciation, when the pulse and respiration are slow, the temperature is slightly below the normal standard. This fact, together with the observations made by Chossat on the effect of starvation on animals, and their inability to digest food in the state of inanition, without the aid of external heat, has direct clinical bearings; it being often necessary to supply external heat as well as food to patients. The best means of applying heat is to place an india-rubber tube, having a diameter of 2 inches and a length of 3 or 4 feet, filled with hot water along the spine of the patient, as suggested by Dr. Newington, of Ticehurst.

Food should be administered at intervals varying inversely with the exhaustion and emaciation. The inclination of the patient must be in no way consulted. In the earlier and
To face page 24.

Miss B. No. 1.

Miss B. No. 2.
Sir W. Gull on Anorexia Nervosa.

less severe stages, it is not unusual for the medical attendant to say, in reply to the anxious solicitude of the parents, "Let her do as she likes. Don't force food." Formerly, I sought such advice admissible and proper, but larger experience has shown plainly the danger of allowing the starvation-process to go on.

As regards prognosis, none of these cases, however exhausted, are really hopeless whilst life exists; and, for the most part, the prognosis may be considered favourable. The restless activity referred to is also to be controlled, but this is often difficult.

It is sometimes quite shocking to see the extreme exhaustion and emaciation of these patients brought for advice; yet, by warmth and steady supplies of food and stimulants, the strength may be gradually resuscitated, and recovery completed.

After these remarks were penned, Dr. Francis Web directed my attention to the Paper of Dr. Laségue (Professor of Clinical Medicine in the Faculty of Medicine of Paris, and Physician to La Pitié Hospital), which was published in the Archives Générales de Médecine, April 1873, and translated into the pages of the Med. Times, Sept. 6 and 27, 1873.

It is plain that Dr. Laségue and I have the same malady in mind, though the forms of our illustrations are different. Dr. Laségue does not refer to my address at Oxford, and it is most likely he knew nothing of it. There is, therefore, the more value in his Paper, as our observations have been made independently. We have both selected the same expression to characterise the malady.

In the address at Oxford I used the term Apepsia hysterica, but before seeing Dr. Laségue's Paper, it had equally occurred to me that Anorexia would be more correct.

The want of appetite is, I believe, due to a morbid mental state. I have not observed in these cases any gastric disorder to which the want of appetite could be referred. I believe, therefore, that its origin is central and not peripheral. That mental states may destroy appetite is notorious, and it will be admitted that young women at the ages named are specially obnoxious to mental perversity. We might call the state hysterical without committing ourselves to the etymological value of the word, or maintaining that the subjects of it have the common symptoms of hysteria. I prefer, however, the more general term 'nervosa,' since the disease
occurs in males as well as females, and is probably rather central than peripheral. The importance of discriminating in such cases in practice is obvious; otherwise prognosis would be erroneous, and treatment misdirected.

In one of the cases I have named the patient had been sent abroad for one or two winters, under the idea that there was a tubercular tendency. I have remarked above that these wilful patients are often allowed to drift their own way into a state of extreme exhaustion, when it might have been prevented by placing them under different moral conditions.

The treatment required is obviously that which is fitted for persons of unsound mind. The patients should be fed at regular intervals, and surrounded by persons who would have moral control over them; relations and friends being generally the worst attendants.

**APPENDIX.**

As a further illustration, I may add the following correspondence on one of these cases with Dr. Anderson, of Richmond.

Miss C., at 15 years 8 months, was sent to me in April 1873. The clinical history was that she had been ailing for a year, and had become extremely emaciated. (Woodcut, Miss C., No. 1.) The catamenia had never appeared. Pulse 64, resp. 16. Very sleepless for six months past. All the viscera healthy. Urine normal. Lower extremities oedema-tous. Mind weakened. Temper obstinate. Great restlessness. No family history of disease beyond the fact that the maternal grandmother had had peculiar nervous symptoms. I wrote the following letter to Dr. Anderson:

‘Dear Dr. Anderson,—I saw Miss C. to-day. The case appears to be an extreme instance of what I have proposed to call “Apepsia hysterica,” or “Anorexia nervosa.” (See “Address on Medicine at Oxford,” 1863.) I believe it to be essentially a failure of the powers of the gastric branches of the pneumogastric nerve. It differs from tuberculosis, though that state may subsequently arise, by the pulse, which I found to be 64, by the breathing, 16, the cleanness of the tongue, &c. In fact, the disease will be most correctly interpreted if it is remembered that no symptom more positive than emaciation is presented in and throughout its course.

‘I would advise warm clothing, and some form of nourishing food every two hours, as milk, cream, soup, eggs, fish,
Sir W. Gull on Anorexia Nervosa.

chicken. I must only urge the necessity of nourishment in some form, otherwise the venous obstruction, which has already begun to show itself by oedema of the legs, will go on to plugging of the vessels. With the nourishment I would conjoin a dessert-spoonful of brandy every two or three hours. Whilst the present state of weakness continues, fatigue must be limited, and if the exhaustion increases beyond its present degree the patient should for a time be kept in a warm bed. I do not at present prescribe medicines, because the nursing and the food are more important than anything else. Such cases not unfrequently come before me; but as the morbid state is not yet generally recognised, I should be glad if you would second my wish of having a photograph taken of Miss C. in her present state, that we may compare it with some later one, if, as I hope, our plan of treatment is successful, as in my experience it generally is. I would, as I say, enclose a prescription, but I feel it most necessary to insist on food and stimulants, at least for a time.

‘Yours truly,

April 30, 1873.’

On May 24 I received the following note from Dr. Anderson:

‘Dear Sir William,—I enclose photograph of Miss C.

There is rather an improvement in one respect, viz. there is less aversion to food. Want of sleep and swelling of the feet are the two great troubles. You have given us all new hope, however, and I trust I may one day send you a plump photograph, like what she was two years ago. With renewed thanks, I am, dear Sir William, yours very truly,’

On Oct. 23, 1873, I received a further report.

‘Dear Sir William,—Miss C. is now at Shanklin, but returns very soon. I hear she is much better. She had a bad slough on the leg near the ankle, from persisting in wearing a tight boot.

The great difficulty was to keep her quiet, and to make her eat and drink. Every step had to be fought. She was most loquacious and obstinate, anxious to overdo herself bodily and mentally. I will give you particulars when they return, but I am told she is much improved. Rest, and food, and stimulants as prescribed, undoubtedly did her a great deal of good. She used to be a nice, plump, good-natured little girl. Believe me, &c.’
The last report I received was on April 15, 1874.

'Dear Sir W.,—I am sure you will be delighted to hear that Miss C., in whose case you were so kindly interested, . . . has now made a complete recovery, and is getting plump and rosy as of yore. . . .' (Vide Woodcut, Miss C., No. 2.)

VI.—Cases of Poisoning by Homoeopathic Concentrated Solution of Camphor. By George Johnson, M.D. Read November 14, 1873.

Miss T., aged 20, consulted me at the beginning of September, on account of certain nervous symptoms which she and her friends believed to have been caused by an overdose of camphor, taken some months before. The following history was obtained from the patient, her mother, and Mr. Drake of Brixton, who was called in on the occasion.

Miss T. had always enjoyed good health, and had never shown any symptoms of nervous disorder. On Feb. 19, 1873, she had a slight cold and sore throat, for which she took at bedtime a dose of Epps's homoeopathic 'Concentrated Solution of Camphor.'

The patient says that, in the presence of a servant who watched her, she dropped 25 drops into a wine-glass of water and drank it off. The medicine at once caused a burning sensation in the mouth and throat, which made her call for more water, which she drank. She then took some gruel and lay down in bed. She believes that she immediately went to sleep. In a short time her sister, who occupied another bed in the same room, hearing her make a strange noise, spoke to her, but receiving no answer she got up and found the patient insensible, foaming at the mouth, black in the face, and violently convulsed. The sister had often seen epileptic fits in a lady of her acquaintance, and she ran to her mother and said that her sister was in a fit. In a few minutes the convulsion ceased, and was followed by vomiting of a pink, probably blood-tinged fluid, which smelt strongly of camphor. Meanwhile Mr. Drake had been sent for, and when he arrived about an hour had elapsed. She was then in a deep sleep, from which she could be aroused with difficulty. The throat looked inflamed, the tongue was
covered by a thick creamy fur, and the pupils were dilated. A mustard emetic was given, and more camphorous liquid was ejected from the stomach. Some strong coffee was then taken. For several hours the breath continued to smell of camphor, and she was very drowsy during the night and the following day. For several days she complained of pain and tenderness over the stomach, increased by taking food; numbness of the tongue continued for a fortnight, and the left arm and leg were partially paralysed. She dragged the left foot along the ground in walking. Gradually these symptoms passed away; but when she consulted me, on September 2nd, she complained that since taking the poisonous dose she had never passed a night without being disturbed by distressing dreams; she had continued to feel weak and nervous, and occasionally had a feeling of weakness and numbness in the left arm and leg.

The symptoms which occurred in this case are similar to those which in several recorded cases have resulted from an over-dose of camphor. Pereira says that 'in its power of causing stupor camphor agrees with opium, but it differs from the latter in its more frequently causing delirium and convulsions.' In the 4th edition of Pereira's 'Materia Medica' (vol. ii. part 1, p. 455), five cases are cited in which convulsions resulted from a large dose of camphor. In one case the dose was 2 scruples, in another 1 scruple. And in the case of three children, aged respectively 5 years, 3 years, and 18 months, about ½ a drachm caused convulsions, and in the infant a single attack of convulsions was followed by fatal coma.

It seems probable that while the convulsive attack in my patient was a direct result of the camphor entering the circulation and perhaps causing a defective blood-supply to the brain, the paralytic symptoms and the subsequent nervous derangement may have been due to an injury inflicted on the brain by the epileptic paroxysm.

I took the solution of camphor to Messrs. Bell in Oxford-street, and Mr. Gale did me the favour to analyse it. He reports it to be a saturated solution of camphor in alcohol—the proportion being an ounce of camphor to 1¼ ounce of spirit. So that this so-called homoeopathic preparation is stronger than the spiritus camphorae of the British Pharmacopoeia in the proportion of 7½ to 1.

There is some doubt as to the amount of this concentrated solution which my patient took. She believes that she took
only 25 drops, which would contain about 10 grs. of camphor. Mr. Drake remarked that the neck of the bottle was so thickly encrusted over with solid camphor from the concentrated solution that some pieces may have fallen into the wine-glass, and thus increased the dose. It is obvious that when the spirituous solution is mixed with water the camphor is precipitated in a state of very minute subdivision, and so in a condition more favourable for rapid absorption than when camphor has been taken in a state of coarse powder. The printed directions on the bottle are, 'Dose two to five drops on a lump of sugar, which may be repeated every half hour or oftener if necessary.' The patient in this case, probably thinking that a homoeopathic medicine was not likely to be poisonous, went much beyond the prescribed dose; but that a literal compliance with the printed directions on the bottle may be attended with serious consequences is shown by another case, the particulars of which have been recently given me by the patient himself.

The Rev. W. R., ret. 64, a highly intelligent energetic clergyman, in the spring of 1870, had a slight cold and hoarseness, for the relief of which he was advised to take three drops of the homoeopathic solution of camphor every five minutes for an hour. In order to ensure strict compliance with the prescription he sat down to write letters, while his daughter, watch in hand, remained by him and administered the dose at regular intervals. After taking the eighth dose he suddenly felt intense pain in the head, and, saying to his daughter, 'I can take no more of the medicine,' he went into another room, where, his wife seeing him, exclaimed, 'Why, you are as pale as death.' He vomited several times, then went to bed, where he remained for forty-eight hours, suffering all the time from the most intense headache. When he got up he felt a very severe pain in the spine, which made it difficult for him to maintain the erect posture. This pain continued for about a month, and it was two months from the time of taking the camphor before he was able to resume his duties in the church. He has since remained in his usual good health.

This gentleman informs me that since the occurrence of his own case, which excited much comment amongst his friends and acquaintances, he has heard of several instances in which very unpleasant and alarming symptoms have resulted from the employment of the same concentrated solution of camphor.
I am indebted to Mr. Delamark Freeman for a note of the following case, which I give in his own words:—

'A young lady, aged 19, about eighteen months ago, took, for an attack of diarrhoea, a tea-spoonful of homoeopathic solution of camphor in water. Soon after having taken it she was seized with a burning pain in the mouth, fauces, and the pit of the stomach, and she complained of feeling giddy, great dimness of vision, tinnitus aurium, numbness and tingling of the arms and legs, with loss of muscular power. When I saw her, which was very speedily afterwards, I found her in a state of insensibility, in which she remained for more than three hours. There was coldness of the surface, with a quick and feeble pulse, conjunctivæ insensitive to the touch, respiration feeble, tickling the soles of the feet produced no reflex action, but she had slight convulsions. Her breath smelt strongly of camphor. After administering an emetic of mustard she gradually became sensible, and had the appearance of a person awaking from a profound sleep; afterwards she had burning heat of the skin, full and quick pulse, great twitching of the eyes, much agitation, headache, giddiness, and inability to walk from loss of muscular power. Her mouth was in some places stained white by the flocculent deposit of camphor, and in other parts the mucous membrane was slightly red, swollen and painful. It was some days before the symptoms entirely passed away. I examined the solution of camphor, and found it to be a saturated spirituous solution of camphor, by far stronger than the ordinary spirit of camphor of the Pharmacopœia.'

Modern homœopaths admit that there are amongst them 'men of high dilutions, men of low dilutions, and men of no dilutions at all,' and it would appear that the disciples of Hahnemann have passed from the irrational and ludicrous extreme of infinitesimal dilutions to the dangerous extreme of the greatest possible concentration of active and poisonous drugs.

It is difficult to conceive that any useful purpose can be served by the employment of so concentrated a solution of camphor as that in question, the preparation being so hot and acrid that it cannot be taken, even in small doses, without being largely diluted. Then there is an obvious risk that this concentrated solution may be mistaken for the much weaker solution of the British Pharmacopœia—a mistake which, in spite of the printed directions on the bottle,
was probably made by both the young ladies who suffered so seriously for their error.

I have thought it right to bring forward these cases, in the hope that their publication may serve as a warning of the danger which attends the incautious employment of homœopathic concentrated poisons.

Appendix to Dr. G. Johnson's Paper on Poisoning by Homœopathic Camphor.

On July 2, 1874, I saw, in consultation with Dr. Wade of Chislehurst, Mr. C. P., act. 18, articled clerk. On June 27 Mr. P. had a slight diarrhœa, for which he took at 3 p.m. about a teaspoonful of 'Rubini's saturated solution of camphor' in water. Immediately after swallowing the dose, he went out to the cricket-field, a distance of about a quarter of a mile. Arrived there, he began to feel confused and giddy. For this he took a small quantity of brandy in water. In about half-an-hour after taking the camphor he suddenly fell down, and was violently convulsed. The bystanders said he appeared to be in an epileptic fit. Within about ten minutes from the seizure, Dr. Wade happened to be driving past and was called to him. The patient was then in a state of semi-consciousness, and was making ineffectual attempts to get upon his feet. Dr. Wade immediately had him put into his carriage and drove him home. He then gave an emetic of sulphate of zinc and common salt, which acted quickly, and caused the expulsion of a large quantity of precipitated camphor. After this the patient went to bed, and remained there until the following morning. During the five days since the attack he had felt weak, and had had an occasional sensation of giddiness; but in consequence, probably, of the speedy ejection of the camphor by the emetic, the after symptoms have been fewer than in the previous cases of poisoning by this preparation of camphor. With the exception of the slight diarrhœa, this youth was in good health, and had had no illness since the summer of 1868, when, for a few hours, he suffered from headache and delirium after being exposed bare-headed to the heat of the sun.

The chief anxiety of the patient's friends was to know whether the attack of convulsion was a result of the overdose of camphor. After my experience of similar cases, I
Dr. G. Johnson on Homœopathic Poisoning.

had no hesitation in saying that it was, and that a return of the convulsions need not be apprehended.

(From the British Medical Journal, Dec. 6, 1873.)

SIR,—I regret that I did not observe the notice of Dr. George Johnson’s communication to the Clinical Society on the above subject; had I seen it, I should have forwarded to him the following case, in addition to the three which he read. In September, 1871, I was staying at the Bell Alp, with Johann Jaun as guide. Jaun is an overland guide, and is well known as an active and vigorous man. His age is, perhaps, 28. One morning we started, about 3.30 a.m., for the ascent of the Schienhorn; and a London gentleman (whose name I forget), who, with his guide, intended to cross the Beich Grät, accompanied us on our way. When we had reached the névé of the Beich Glacier, Jaun, who had fallen back, came up to me and said that he was suffering so severely from diarrhoea that he feared he should be unequal to the ascent. As the morning was very fine, I was much disappointed, and mentioned the state of the case to my companion. He promptly brought forth a bottle of ‘homœopathic camphor,’ with which he had been armed by his own homeopathic attendant against diarrhoea. I caught at any chance of a cure, and gave my guide ten drops of it, which was double the dose ordered to my companion. As another loose motion occurred, I gave him a second dose of ten drops at most. I say ‘ten at most,’ because, in my slightly contemptuous estimate, I dropped it carelessly upon a bit of sugar. My own intention was to give him a liberal five drops. He may, however, have taken twenty drops in all. Soon afterwards he began to feel very ill, and in particular to suffer from a distressing giddiness (Schwindel). He had also some headache and nausea, and became so nervous that he feared to walk any further upon the snow. We were certainly upon a slope, but it was not very steep; and, before taking the medicine, he would have walked upon it without a thought. As it was, we had to deposit him in a cave for safety, and I gave him some ice-water and bathed his head. When quiet, he seemed better, and my companion and myself walked on with the other guide to the summit of the pass, and remained there until Jaun crawled up, about an hour and a half later, still feeling very giddy and nervous. He picked out the safest place in the rocks, and there fell off.

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into a sort of lethargy, from which I gently aroused him from time to time. He was quite unable to take food, but he drank some wine and water at intervals. As the morning was hot, I was able to remain about two hours longer on the summit, when Jaun felt able to go homewards; fortunately also a gentleman came up from Ried with two guides, so that we reached the Bell Alp without further difficulty. He grew better as we returned, and next day was quite well. There was no more diarrhoea after the second dose. When I read Dr. Johnson's cases, I see that we were lucky to come off so well, and that I was to blame in leaving Jaun behind without assistance. Without the other guide, however, we could not have proceeded, and my companion had a long walk before him.

I am, &c.,

T. Clifford Allbutt.

Leeds, December 1873.

VII.—Remarkable Case of Death from Meningeal Congestion, without Inflammation. By F. E. Anstie, M.D. Read November 14, 1873.

A YOUNG gentleman, aged nearly 13, came to me on Sept. 23, the end of his school vacation, and requested me to examine his neck, in which there were some swelled glands that had existed for a few days. The question to be decided was, whether these were cynanche parotidea—mumps—in which case he could not have gone back to school for fear of carrying contagion. I pronounced it certainly not mumps, but an ordinary swelling of cervical, not parotid, glands; possibly from cold. There was, however, no complaint of sore throat, or other catarrhal symptoms; but the lad looked a little pale. He was, however, particularly cheerful and animated; and I may say here that he was a remarkable lively and clever boy, taking the greatest interest both in work and play, and doing both well. He had just passed the vacation, mainly at the seaside, and had during that time been apparently in the best health and spirits.

I heard no more of the matter till the evening of Oct. 4, when the boy's father requested that I would see him in the course of the next day, as he was complaining of being out of sorts. On the 5th, accordingly, I saw him at his father's house, and learned the following story. He had gone back to school, and apparently had felt no inconvenience from his
Dr. Anstie’s Case of Meningeal Congestion.

neck; the glandular swellings had nearly disappeared. He had done his lessons with his usual ease. He had also sailed on the river, and had played football; after the latter, which was on the 29th, he had made some complaint of stiffness in the legs, but it came out clearly, on questioning him, that he had felt something of this before the game of football. His father, on a previous morning, had noticed that he walked down stairs with a noisy, thudding step, as if he walked on his heels. The sensation of stiffness in the lower limbs increased, and it was noticed that he got up-stairs slowly and with difficulty. Still he kept about, and managed to go to school till Saturday, Oct. 4, on the evening of which day his father spoke to me about him. On the occasion of my visit, on the 5th, I found that the family had been alarmed that morning by finding that the lad could not stand; on getting out of bed he at once sank down. Accordingly, I found him not at all able to stand; once seated, however, he could perform most movements of the lower limbs fairly enough, though rather slowly. I could not then discover that there was any decided interference with sensation in the lower limbs; but he owned to a slight tingling feeling both in his feet and also in his hands. There was not the slightest feverishness, either subjective or as tested by the thermometer; the face was pale, the eyes bright and clear, the tongue clean; there had been no sickness nor headache, nor the slightest loss of intelligence. The pulse was 85, regular, rather full, but also decidedly compressible. I own to having been very much puzzled to give either diagnosis or prognosis, with any confidence, in these circumstances. I could only tell the father that I leaned to the hopeful view that the stiffness and loss of power were merely functional, the result perhaps of muscular exertion accidentally coinciding with some other cause of delicacy, at present unexplained, and that the whole thing might pass away with a week or two of perfect rest. Meantime, he was ordered to take hypophosphite of soda, in 10-grain doses, three times a day, with calumba—a mixture which I have often found most beneficial in cases of mere nervous exhaustion. It happened, however, that the first or second dose was vomited, and the medicine was accordingly discontinued. My second visit was at 10 a.m. on the 6th. I found the patient in bed, and made a minute examination. As before, the face was pale, the eyes clear and full of intelligence; speech and swallowing perfect; the thermometer, most carefully applied, showed exactly
normal temperature. There was rather a weary look in the face, and I ascertained that he had slept very badly. I asked him particularly about this, and he said that he had no headache, nor feeling of mental wandering, nightmare, nor anything of that kind, but simply that his limbs soon stiffened into an uncomfortable posture, and he could not sleep till he got into a fresh position. I have said that his speech was perfect; I should have said it was, in fact, slow and a very little hesitating, but the father assured me this was natural to the boy: his articulation was very good. A fact that now arrested attention was the increased rapidity of the pulse, which was over 100. On examining the chest I could not help thinking that the cardiac impulse seemed diffused over a larger space than was normal, and thought I detected a certain amount of hypertrophy, although this idea was contradicted by the nature of the pulse at the wrist, which was compressible. There was no bruit. [It may be said here that the post-mortem disproved this supposed hypertrophy which, perhaps, I had been the more inclined to accept, because a brother of the patient had been quite accidentally discovered to have old standing heart disease]. On testing the power of the lower limbs, I found it slightly diminished; the movements which he executed were slower and more difficult than they had been the day before. As to the upper limbs, both deltoids seemed to have partially lost power, and indeed the right upper arm was wholly powerless; the grasping power of the hands was evidently weakened. Renewed examination showed that there was a difference between the two sides of the body, the right arm and (to a less extent) the right leg being weaker than the left. The numb and tingling sensations were complained of by the patient in increased degree; they were felt in all the four limbs. All these symptoms, however, are much more easy to appreciate accurately now than they were at the time; for example, it was not possible to be sure how far imperfection of movement might be due to stiffness rather than to actual paralysis; and I confess that the rapid irritably-acting heart made me think it quite possible that we had to do with a latent rheumatic pericarditis, in that pre-febrile stage which I have more than once seen to last for two or three days before the occurrence of any pyrexia or the development of any physical signs. I freely imparted to the father of the patient my deep anxiety and perplexity about the case. Being a medical man himself, he agreed with me as to the extreme obscurity of the case,
Dr. Anstie's Case of Meningeal Congestion.

and fully concurred in the opinion that, as we had no clear indications whatever for treatment, we should content ourselves with attempting to support the boy's strength; and this, indeed, required much attention, for not only was appetite almost nil, but the lad had vomited three or four times. He was fed with milk, beef-tea, and small quantities of solids; no stimulants were given, except a little champagne, which he scarcely tasted, and did not care to drink.

In the evening of that day (Oct. 6) matters seemed not much changed; but the boy complained of being dreadfully weary, and begged he might have something to make him sleep; and accordingly he was allowed a very mild opiate, which procured him some hour or two of repose. On the morning of the 7th I found him evidently worse. The legs were now truly, and almost completely, paralysed, as regards motion, though sensation was wonderfully little affected, and there was increased loss of power in the upper extremities. Another symptom which alarmed me very much was a loss of power in the sterno-mastoids; when he sat up, which he could do with comparatively little assistance, his head fell backward unless it was supported from behind. I now obtained the kind assistance of Dr. Andrew Clark, who made a most minute examination, which still further demonstrated the remarkably insignificant degree in which sensation of all kinds was affected, as compared with motor power. By this time the pulse had increased to 120; the temperature, very carefully taken, was 97.9°. Dr. Clark most minutely examined the chest, but could discover nothing wrong with the heart or lungs.

In consulting with me upon the case, Dr. Clark, while admitting the extreme obscurity of the symptoms, pointed out, by way of exclusion, the diagnosis to which we were almost necessarily reduced. The acute febrile diseases being out of the question, and there being an entire absence of the symptoms of meningitis either of the convexity or base of the brain, or of the spinal cord—as also, from the complete absence of pyrexia (and indeed from other circumstances), was myelitis—we were reduced to the supposition that the symptoms were produced by pressure within the spinal canal and at the base of the cranium. That pressure, it seemed most probable to Dr. Clark, was of the nature of vascular congestion, and unless relieved might go on to effusion either of blood, or of serum, or of inflammatory products. He recommended that an attempt should be made to effect a diversion
of blood from the part, and for this purpose prescribed saline diaphoretics and a calomel purge, to be taken at bed-time, and followed by a draught in the morning. It happened that all this treatment was frustrated; the mixture could not be retained by reason of the irritability of the stomach, and before the time came for administering the purgative the position of affairs had altogether changed. [It should be mentioned, by the way, that the bowels had been thoroughly evacuated on the morning of the day previous.]

About 7 o'clock, or 7.30, I saw the lad again and found him weaker; but besides this I was disturbed to find a considerable amount of mucus in the bronchial tubes, which the patient seemed powerless to expel. An hour and a half later the boy's father pointed out to me that there was visible loss of power in the respiratory muscles, both diaphragm and intercostals acting very imperfectly. It now seemed idle to do anything save to give such nourishment as might be possible, and ammonia and small doses of brandy to endeavour to restimulate the failing respiration. I need not say, however, that I felt the case to be hopeless; although it seemed difficult to connect the idea of inevitably impending death with a lad whose consciousness and intellect were as clear as my own, who followed all our movements with his eyes, and answered questions with perfect readiness. By 6 o'clock next morning it was evident that the lungs were becoming swamped, and from that time forward there is nothing to relate except the gradually increasing difficulty of breathing, falling temperature, and increasing lividity of features, till at about a quarter to 1 p.m. He then requested to be raised, and after this had been done became suddenly unconscious and died in about five minutes.

The post-mortem examination was made, nineteen hours after death, by Mr. Warrington Haward and myself, and occupied us during fully two hours. Cadaveric rigidity was moderate; the skull, and afterwards the spinal canal, were very rapidly and skilfully opened by Mr. Haward, before anything else was touched.

On removing the calvarium, which came away from the dura mater readily enough, and then slitting open the latter, it struck us that, even allowing abundantly for the inevitable effect of the mode of death—by gradual failure of respiration—there was more venous congestion than we had ever seen. This excess of congestion was not appreciated, however, till we had raised the brain and begun to divide the
nerves for the purpose of removing the brain. In particular it was noticed that the lower petrosal sinus was so tensely full, that when pricked it squirted blood almost like a living artery. It was distinctly remarked that while the superior longitudinal sinus was not more full of blood than it is frequently seen in cases of death from various indifferent diseases, the sinuses and great veins about the base of the cranium were extraordinarily full, and that this plethora was imitated by a similar condition of things in the meninges about the medulla oblongata. On dividing the cord, just below the medulla oblongata, and removing the brain, an extraordinary quantity of black blood kept pouring out of the spinal canal. The cord was removed with much care, but before taking it out we carefully inspected the condition of the membranes, which was very remarkable. The veins, both of dura mater and of pia mater, were so greatly injected as to form a black network of a truly wonderful appearance, such as neither I nor Mr. Haward, in all his practical experiences in post-mortems at St. George's Hospital, had ever observed; and the same remark was made about the medulla oblongata. I might, for all practical purposes, close the post-mortem record by saying that this was the only morbid appearance in the body. Even this condition of congestion did not extend to any other organs (except that, as was inevitable, there was considerable stasis in the lower and posterior parts of the lungs); and concerning the viscera and tissues of the body the only remark to be made is, that their condition was that of typical health. Returning to the nervous centres, I must justify my previous remark by saying that we spent a long time in their examination. As to the brain, there certainly was not a cubic inch of it anywhere that was not most carefully sliced and examined. Not the slightest lesion of nerve substance could be found, and there was rather less serum than usual in the ventricles. Nor could it be said that the grey matter was congested, nor that the number of bloody points on a cut surface of white substance was at all abnormal. The central ganglia, the crura cerebri, the cerebellum, the pons, and medulla oblongata, were all inspected with a similar result; and, finally, the spinal cord was also dissected with great minuteness. Not a trace either of softening or of induration could be found in the tissues of any of these organs; there was no effusion of serum, nor of blood, anywhere; no adhesions or anything to give the slightest hint of inflammatory processes; and, to sum up, Mr. Haward agreed with
Dr. Austie's Case of Meningeal Congestion.

me that the only pathological phenomenon was congestion, and that that congestion was essentially and exclusively meningeal. The solid tissues of all the nervous centres might have served for models of the healthy state. We spent a long time in investigating the state of the vessels, and especially looked out for any trace of arterial degeneration or obstruction, or of venous thrombosis, but nothing of the sort was visible. And we felt that our conjoint experience was sufficient to justify us in the assumption that as our eyes could see no appearances, even the very slightest, of changes in the nervous substance, it was not necessary to proceed to more minute examination of it; for it is not true, as some suppose, that nervous tissues which, when elaborately prepared, give microscopic evidence of change sufficient to account for rapidly fatal mischief, may be destitute of all naked eye morbid appearances. On the contrary, a competent and careful observer will always detect with the naked eye that something has been very much amiss. In this case the nervous tissues clearly were not the seat of any such change as would have rapidly cut short life.

Remarks.—Such cases as the above are, no doubt, very rare, and it may be remarked that there is little or nothing in the literature of nervous diseases which helps us either to account for their causation, or to diagnose their nature when we meet with them in practice—still less that will afford us any useful ideas respecting treatment. The older doctrines respecting spinal and cerebral congestion, and the older records respecting cases which are supposed to come under these denominations, are quite useless to the modern pathologist, because they rested either on mere conjecture unconfirmed by post-mortem experience, or, if post-mortems had been made, the appearances were ignorantly interpreted. It is now understood that a man must have examined a good many brains and cords, of persons dying under various diseases, before he is qualified to say what is pathological congestion and what is merely the effect of the manner of dying. In the present case there can be no doubt that decided pathological congestion did exist, and that it was limited, in a remarkable manner, to the meningeal vessels.

As regards the clinical history of cases of this kind, I have made diligent inquiry, I must say with very unsatisfactory results on the whole. From books I have gained absolutely nothing, except from the treatise of Dr. Radcliffe in vol. ii. of Reynolds's 'System of Medicine.' No fatal cases are
Dr. Anstic's Case of Meningeal Congestion.

detailed in Dr. Radcliffe's treatise, but he intimates that the disease has proved even rapidly fatal in not a few cases, and in private conversation with me he has referred to several instances in which death occurred in or about the fourth day from the appearance of any marked symptoms of illness. The description of the general character of the symptoms in his treatise corresponds very well with those which were present in the case brought forward to-night; but there is one rather notable exception. Dr. Radcliffe speaks of dull spinal pain; but there was not the slightest spinal pain in the case I have narrated. As regards one post-mortem appearance also, Dr. Radcliffe's description does not correspond with what was observed in the case before us. He speaks of an excess of cerebro-spinal fluid; but I am confident there was nothing of the kind in this case; and Mr. Haward quite agreed with this opinion. Dr. Radcliffe states, very decidedly, that the absence of changes in the nervous substance itself is a constant phenomenon in fatal uncomplicated spinal congestion. Finally, I may mention that Dr. Radcliffe tells me that cases which begin in the manner, and run the general course, which distinguished my case, are, in his opinion, quite hopeless from the first.

As regards etiology, I must confess myself entirely at fault. Dr. Radcliffe says that, in women, suppressed menstruation is a frequent cause of spinal congestion; and that the somewhat analogous instance of suddenly repressed haemorrhoids (to which males are as liable as females) is likewise a frequent exciting cause; but there had been no haemorrhoids in my patient. Excessive muscular exertion is also mentioned as a cause; but there was no reason to suppose that my patient had exerted himself in any way beyond his usual custom. And the influence of cold in repressing cutaneous circulation can hardly be credited with any influence here, for the weather was by no means specially cold at the end of last September.

As regards any treatment which could be brought to bear upon such cases as that now related, I fear the prospect is very poor. I cannot see much probability that any derivative measures, practically within our power, could be brought to bear at such an early stage as would allow them to be efficacious. On this point, however, I speak with the greatest diffidence, and I believe that Dr. Andrew Clark has had encouraging results in at least one case.

P.S.—I must add a special note in answer to certain con-
jectures that have been freely pressed upon me by friends. The glandular swellings in the neck were utterly unlike, in position and character, those of diphtheria, and there never was, from first to last, any affection of the throat or fauces. Nor was there any, even the slightest quantity, of albumen in the urine.

VIII.—Cases of Temporary Albuminuria, the results of Cold Bathing. By George Johnson, M.D. Read November 29, 1873.

Case I.

S. J., aet. 22, medical student, on June 10, 1873, about noon, while hot and slightly fatigued, bathed in the Marylebone tepid bath, and remained in the water actively swimming and diving for about a quarter of an hour. Soon after bathing headache came on, and in about four hours, the urine being high-coloured and scanty, was found to contain a quantity of albumen. The same evening, about 8 o'clock, a small trace of albumen was still present. On the following morning, after a good night, the headache was gone and the urine was free from albumen. During the interval between June 19 and 28 the urine was tested almost daily, and at different times of the day, and was always found quite normal. On June 28 he bathed again at the same place, and for about the same time, between 12 and 1 p.m., no cold bath having been taken in the interval. Headache again came on after the bath, and continued until bed-time. The urine was twice tested in the course of the afternoon, and found free from albumen; but at 9.30 p.m., two hours after dinner, the urine contained about $\frac{1}{6}$th of albumen. On the following morning, the 29th, headache gone; urine free from albumen; dinner at 1.15; two hours later albumen $\frac{1}{10}$th. At 6 and 9.30 p.m. urine free from albumen. From this time, for many days, every specimen passed was tested for albumen. On most days a trace of albumen was found at some period of the day until July 17, since which time no albumen has been found in any of the numerous specimens which have been examined. The time when the albumen was present was usually within two hours after the mid-day lunch or the evening dinner. The amount of albumen was very small
except on one occasion (July 7), when, after walking to church in the rain, a distance of about a quarter of a mile, the urine, at 1 p.m., contained \( \frac{1}{3} \)rd of albumen. At 2.15 there was a slight trace of albumen; at 4 and 10 p.m., the same day, not a trace remained. On five days only, between June 28 and July 17, was the urine free from albumen at all periods of the day. This gentleman is a water drinker, strong and active, has had the usual illnesses of childhood, not including scarlet fever, and (six years ago) a slight attack of dry pleurisy, from which he soon recovered.

**Case II.**

R. G. H., æt. 25, medical student, having heard of Case I., one day in July 1873 bathed for an hour in the Lambeth Baths. Bathing included swimming, diving, and standing occasionally on the edge of the bath. The weather was very warm, and the water unusually cold. The urine passed an hour after the bath contained \( \frac{1}{8} \)th of albumen. The urine was not again tested until three or four days afterwards, when it was found free from albumen.

The experiment has not been repeated.

This gentleman felt no inconvenience or discomfort after the bath which was followed by the temporary albuminuria.

**Case III.**

H. S. A., æt. 23, medical student, made a series of observations on himself in the months of Aug. and Sept. last. On Aug. 2 he tested his urine, and found it normal at 7.30 a.m. At 7.45 he bathed in the sea, remaining in the water about a quarter of an hour. When he reached home, within half an hour after bathing, his urine was found to contain about \( \frac{1}{6} \)th of albumen. The urine was not tested again until 4 p.m., when the albumen had disappeared. On Aug. 18 the urine was free from albumen at 7.45 a.m. At 8.15 bathed in the sea for a quarter of an hour. At 9.15 the urine was albuminous. At noon the urine still contained a trace of albumen. At 2 p.m. no albumen was found. Between 12 and 2 had active exercise in a canoe, and, the day being very hot, had perspired profusely.

Aug. 31.—At 7 a.m. urine normal; at 7.45 bathed for five minutes; after walking home, in about half an hour, urine not albuminous.

Sept. 17.—At 2 p.m. urine tested, and found normal. Bathed and remained in the water, which was very warm,
more than half an hour; afterwards the urine contained a
trace of albumen. Half an hour later the urine was free
from albumen.

Oct. 4.—At 12.30 was capsized out of a canoe into the
water. Swam on shore, about fifty yards, got into a boat,
and rowed home a distance of four or five miles; became very
warm with the exercise. After changing clothes the urine
was tested, and found free from albumen.

On the three occasions when the urine was found albu-
minous after bathing there was no feeling of chilliness,
fatigue, or weakness. The subject of this series of observa-
tions is in good health, and has never been seriously ill.

Four students, companions of the three whose cases are
before given, both before and after bathing for a considerable
time, found no albumen in the urine. One of these, R. B.,
about a year before, had suffered from the acute albuminuria of
scarlet fever, from which he completely recovered. When
bathing in the Lambeth Baths, in the month of July, on one
occasion for half an hour, and on a second occasion for an
hour, until he was very cold and his feet and hands blue, the
urine, half an hour after leaving the bath, was found quite
normal.

Another, R. B. M., on six occasions bathed for an hour,
and on another occasion for an hour and a half. The result
was that he felt chilled and tired, but no albumen appeared
in the urine.

Case IV.

V. B., æt. 16, was brought to me by his father on June 28.
The boy looked pale and complained of languor. There had
been no suspicion of renal disease, but some urine passed in
my room was of pale colour, and was found to contain 1/8th
of albumen. It was free from sediment, and contained no
casts. Seeking for the probable cause of the albuminuria, I
learnt that the only serious illness had been an attack of
diphtheria ten years before. It seemed not improbable that
the albuminuria might have existed from that time. There
was no dropsical symptom, nor had any such symptom ever
been noticed. I gave general directions as to diet and regi-
men, and prescribed the syrup of phosphate of iron to be
taken twice a day.

I did not see the patient again until Sept. 23, when the
urine contained a mere trace of albumen, discoverable by
adding nitric acid to the cold urine. Again I saw him on
Oct. 23, when the urine was found quite free from albumen. He declared himself to be quite well, and he had a fresh and healthy look. Finding that the urine had regained its normal character, I felt sure that the albuminuria could not have resulted from the diphtheria ten years ago; and on making more particular enquiries I learnt that until within a week of his first visit to me he had been for a month at the sea-side, where he had been in the habit of bathing daily, remaining in the water usually from half an hour to three quarters of an hour. He admitted that on several occasions he had felt fatigued and chilled, and on one occasion he had vomited after coming out of the water. There can, I think, be little doubt that the albuminuria in this case was a result of the repeated and prolonged immersion in cold water.

Cases of acute albuminuria with dropsy, resulting from exposure to cold and wet, are of frequent occurrence; and the facts here stated suffice to show that prolonged cold bathing, whether in fresh or in salt water, has in some healthy individuals the effect of inducing temporary albuminuria, while in others no such result follows prolonged immersion in cold water.

It is a matter of common observation that albuminuria which has once resulted from a chill is very apt to recur under the influence of renewed exposure to cold.

With regard to the first case here recorded, I suspect that his apparently more than ordinary liability to suffer from the influence of cold bathing may have resulted from imprudently prolonged sea-bathing three years before, when, for a period of six weeks, he bathed almost daily, and remained in the water often for an hour. He says that, at that time, he suffered no inconvenience from the prolonged immersion; but, nevertheless, it is probable that he may have had repeatedly recurring albuminuria unsuspected, and therefore undiscovered. His own impression, however, is that he was more readily disturbed by the bathing last summer, because he was then depressed by London air and hot weather, and hard work.

It will be seen that in that case, after the second bathing, the urine was found albuminous nearly every day for a period of three weeks, while in the fourth case the albuminuria continued more than three months from the probable time of its commencement, after repeated and prolonged cold bathing. There is an obvious risk that the frequent recurrence of albuminuria from the chilling effect of prolonged cold bathing, and the consequent repression of the cutaneous secretion,
might lead to permanent mischief and to structural degeneration of the kidney.

I have seen sufferers from Bright's disease who have sustained serious injury from the indiscreet employment of cold sitz baths in a hydropathic establishment; and I suspect that in some instances that method of treatment has been the originating cause of serious renal disease.


My sole object in bringing the following case under the notice of the Fellows of this Society is to institute inquiry into the proceeding illustrated by it, and to elicit a discussion upon its merits, not because the case itself has any independent interest, nor because my own experience is at all worth recording.

It so chanced, however, that, at the suggestion of my colleague, Mr. MacCormac, I was probably the first surgeon in England to apply this particular modification of an old method to an operation of any magnitude, and I have therefore taken some interest in watching its rapid adoption by most of the hospital surgeons in London, since its introduction at St. Thomas's Hospital by Mr. MacCormac in August of the present year.

I will first narrate as briefly as possible the case which is to serve as an excuse for introducing the subject to your notice, and then touch lightly upon the few points which seem to require an expression of opinion from those surgeons who have already a far larger experience of the method than myself.

Henry W., a single, delicate-looking man, aged 30, an undertaker's assistant, had been admitted into St. Thomas's Hospital, under the care of Mr. Croft, on June 19, 1873, and he came under my care during Mr. Croft's absence from town early in August.

There were symptoms of extensive disease of the right knee-joint, the whole limb, from the middle of the thigh to the toes, being oedematous, whilst there was considerable globular enlargement of the knee itself. The amount of
œdema was too great to render manipulation of the joint very instructive; but the signs appeared to me to indicate the presence of necrosed bone, suppuration in the joint, and an abscess to the inner side of the patella, which did not apparently communicate with the joint. There was, however, a peculiar grating felt on pressing the patella, for which I could not account, as the pressure caused no pain. Having regard to the obviously extensive local disease, and to the delicate state of the man's health, I was very unwilling to attempt excision of the knee, and I therefore suggested amputation, promising to make exploratory incisions into the joint first, with the view of saving the limb if possible.

I will not weary the meeting with many details; suffice it that, just before the patient was brought into the theatre, he peremptorily refused to allow his leg to be amputated. After chloroform was administered, Mr. MacCormac applied a broad elastic bandage tightly over the limb from the toes to near the groin. As soon as the patient was thoroughly insensible a stout elastic cord was tightly twisted round the thigh above the bandage, fastened with hooks, and the bandage removed. The limb appeared shrunken, and of a tallowy whiteness.

On exploring the joint I found that the patella was necrosed, lying in fragments in the midst of a thick-walled abscess, and that the apparently superficial abscess really communicated by a fine tortuous opening with the interior of the joint, which was full of pus, the synovial membrane being the seat of extensive pulpy change, and the cartilages irregularly eroded. After some doubt and consultation it was thought well to extend my incisions so as to excise the joint at once, as affording the next best chance to amputation; and I accordingly did so, removing thin slices from femur and tibia, the whole of the patella, and snipping away the pulpy tissues around as freely as possible. All this took much more time than is usually required for excision of the knee, so that fully thirty-five minutes elapsed before the elastic rope was removed, not a drop of blood having been lost up to this period. The free oozing which at once took place did not seem to be excessive, although it was sufficiently troublesome to cause much delay in completing the operation and arranging the limb on its splint in the usual manner.

During the evening of the same day there was some oozing of blood from the wound, but not enough to render it
necessary to touch the splint. There was no sloughing of skin. The discharge was—as was to be expected from the condition of the part—abundant, and has even now not wholly stopped, although there has long been firm union of the sawn bones. The temperature for some weeks averaged 99° in the morning and 100° in the evening, once only rising to 103°; so that the convalescence, although slow, has shown no sign of being affected in any way by the long-continued pressure kept upon the limb during the operation.

Before this Mr. MacCormac had applied the elastic bandage in two or three cases of necrosis; and on his shortly afterwards publishing an account of the plan in the 'Medical Record,' it was warmly taken up at almost all the metropolitan hospitals, and I am told that the constantly increasing demands for the necessary apparatus which come up from the country render it difficult for our instrument-makers to meet them with sufficient despatch.

All this shows that the ingenious suggestion of Prof. Esmarch has been a real boon to operators, and renders it the more important both to make the method widely known, and at the same time to discover, from our already large experience, whether there are any defects in the proceeding to be removed, or dangers to be guarded against. Amongst the points which immediately offer themselves for consideration are the following:

1. Is there any danger of sloughing following the use of the elastic bandage? and how long may the pressure be kept up with safety?

2. Is the subsequent circulation in the limb rendered inconveniently active on the removal of the elastic cord?

3. Is the plan available without chloroform? and would it then so far benumb the part as to render small operations painless as well as bloodless?

4. Under what circumstances may the method be advantageously avoided?

It has been reported in one of the medical journals that the edges of the flaps have sloughed in five out of seven amputations in which this method was employed at Guy's Hospital. If this be the case, and if it be proved that this sloughing was due to the pressure upon the limb during the operation, it will surely furnish a strong argument for caution in the use of the method. It must not be forgotten, however, that such untoward results are quite opposed to the hitherto recorded experience of other surgeons. But we
Mr. Arnott’s Case of Bloodless Operation.

still want careful notes of cases showing how far, if at all, the healing processes are delayed or interfered with by the strong pressure employed. For, although professional opinion as to the importance to the patient of loss of blood has been steadily receding from the old position assumed in the day of frequent and excessive blood-letting, it is probable that this last novelty of so-called ‘bloodless’ operations indicates the furthest swerving of the pendulum; and we may begin now to look for a gradual return to older notions, although the oscillations of practice in this as in other matters are likely to be constantly lessening.

Just at this moment it may be that Prof. Esmarch has given an unnecessarily strong jerk to the pendulum and somewhat unsettled the judgment of the profession, for we read of the adaptation of a complicated modification of this pressure by an eminent hospital surgeon to a case of removal of exostosis from one of the phalanges of the forefinger—not a very sanguinary business under any circumstances, and one in which light pressure on the brachial artery with the fingers of an assistant would surely sufficiently control all troublesome bleeding.

The circulation does not appear to be affected to any extent by the prolonged constriction; all that is seen, when the pressure is suddenly relaxed, is a vivid blush which travels down the released member and leaves it notably ruddier than its fellow for some time. This fact makes it necessary to plug very carefully any wound made for the removal of dead bone or tumour before the blood is again admitted to the part, or the amount of blood really saved will be after all insignificant.

It had probably occurred to many that the pressure caused by the elastic bandage and cord might so far paralyse the nerves of the part as to be productive of a convenient form of local anaesthesia. This, however, was found by Billroth, in one instance in which he tried it, not to be the case. In fact the proceeding itself is by no means free from pain. I have seen a man still tolerably insensible from chloroform spring up from the table in a spasm of pain caused by the sudden loosening of the elastic cord on the thigh towards the end of an operation for necrosis.

This pain, and the sudden engorgement of the capillaries accompanying it, would seem to indicate the wisdom of gradually relaxing the constriction instead of suddenly unhooking the cord, as at present practised.
As to those cases in which caution should be exercised in employing this method, surgeons have already pointed out the danger of exerting violent pressure over septic abscesses, the clot occluded rents in the veins about a compound fracture, and gangrenous parts; and it would also seem well to bear in mind the possible risk of apoplexy which would be incurred by suddenly overfilling the circulatory system of aged people with brittle arteries and feeble hearts. The subsequent effects of an increased blood supply to the viscera of a patient after one of these 'bloodless' operations remain to be carefully watched. In many—perhaps most cases, at all events, in town practice—the extra blood-supply to vital organs would be probably highly beneficial; but there are not a few instances in which the sudden distension of the vessels generally might be immediately dangerous, and in which the subsequent plethora in cases of large amputations, by causing visceral congestions, might seriously interfere with the recovery of the patient.

The real merit attaching to Prof. Esmarch's practice appears to be, as has been lately pointed out by Mr. Erichsen, that he has shown us how a method formerly used by other surgeons in exceptional cases of amputation, and notably by Mr. Clover in this country, may be simplified and profitably applied to almost every kind of operation upon the extremities. But the notion of bloodless operations is in itself so taking, especially to surgeons who cannot command unlimited assistance and appliances, that it is necessary that we should examine carefully into any possible risks and dangers to be encountered; and that, in order that these should be fairly understood, operators should make a point of publishing any accidents of the kind, if they occur. It is only by such means that a suggestion so really valuable can be secured against being as quickly laid aside as it has been suddenly taken up, on the occurrence of any unexpected mischief in an important case.
Mr. Wheelhouse's Case of Aneurism.

Aneurism of the External Iliac Artery cured by five hours' continuous Pressure with Lister's Abdominal Tourniquet applied on the Abdominal Aorta, while the Patient was under the influence of Ether. By Claudius G. Wheelhouse. Read December 12, 1873.

Richard Lister, publican, was admitted into the General Infirmary at Leeds on Sept. 26, 1873.

His family history is good, and he stated that he had always been a temperate and healthy man.

Examination elicits that in 1860 he contracted a chancre, which was followed by a suppurating bubo—a rash all over his body—and a sore throat.

He says that in 1861 he 'caught cold' in his right eye, and suffered a violent 'attack of inflammation,' and that in the same year Mr. Teale performed 'iridectomy.' He also says that about the end of 1862 he had an attack 'of inflammation of the kidneys,' and had bloody urine. There was no dropsy, but his general health was greatly deteriorated.

For this condition he was advised to take iodide of potassium and sarsaparilla, which soon cured his 'bloody urine,' and made a new man of him.

From that time, until the symptoms of aneurism made their appearance, he had enjoyed moderate health.

About twelve months ago he came under the care of Mr. C. V. Newstead, of Burley Road, Leeds, complaining of severe 'rheumatism' in the calf of the right leg; and on examination of the limb, a popliteal aneurism of about the size of an orange was detected. He had noticed about four months previously that on bending his knee to the full extent a sharp pain was produced, and that there was a tender spot behind the joint, but he had not perceived any swelling or pulsation.

A few weeks after he came under Mr. Newstead's care he strained his knee, whilst moving a heavy cask, and the pain became much aggravated.

In the early part of Jan. 1873 I was requested to see him with Mr. Newstead, and finding the popliteal aneurism then fully developed, I recommended 'compression' treatment.

On the 6th of the month continuous pressure was put...
upon the femoral artery for eight hours, by means of 'Porter's' femoral compressor.

The pain, although severe, was well borne; and on removal of the instrument the aneurism was found to be consolidated, and quite free from pulsation; there was no oedema of the leg, and the foot was warm and free from discolouration.

The limb was nevertheless wrapped in cotton wool, and the patient was kept in bed for a fortnight.

From that time the aneurismal tumour has been steadily diminishing in size, and is now (Sept. 1873) about the size of a pigeon's egg, but may still be easily felt in the popliteal space.

The leg does not appear to have suffered much from the obliteration of the artery, but he complains that it sometimes feels colder than the other one.

About two months previous to his admission into the Infirmary he found another tumour in the right iliac fossa of the same side, which was constantly pulsating, and which gave him great pain.

This tumour appears to have formed gradually, and no history can be elicited of any sudden strain, or feeling of giving way of the vessel, but his right leg has become colder than it used to be.

At this point I was again requested to see him by Mr. Newstead, and he was also seen with us by my colleague, Mr. Jessop.

We found a large pulsating and expansile tumour in the right iliac fossa, reaching from Poupart's ligament upwards to within two inches of the umbilicus. It extended in an outward direction almost to the spine of the ilium. It was about the size of a small cocoa-nut, hard and firm at the lower part, and softer in the upper portion. The pulsations and dilatations were found to be synchronous with the pulse in the left femoral artery.

The swelling appeared to be wholly connected with the external iliac artery, but to extend above and overlie the common iliac. Pressure cannot be made on the latter vessel sufficient to stop the beating, as the tumour is too much in the way; but it is easily controlled by pressure on the abdominal aorta, just above its bifurcation.

Under these circumstances, considering the difficulty there would be in placing a ligature upon the common iliac vessel, the great risk attendant upon such an operation, and the doubtful prospect of success from it, we determined to recom-
Cured by Compression.

nend a further trial of pressure upon the abdominal aorta under ether, and for this purpose we advised the patient to come into the hospital, to which he readily assented.

On Saturday, Sept. 27, he was first put under the influence of chloroform; and as soon as he was fairly unconscious, and his muscles were fully relaxed, ether was substituted. The administration of the anæsthetic was commenced at 1 P.M., and was kept up without intermission until 6 P.M.

During the whole time no unpleasant cardiac or pulmonary symptoms arose, and the patient was kept thoroughly unconscious and relaxed during the whole time.

In the five hours 25 ounces of ether were used. The ether was administered on sponges covered with impermeable cloth, and from time to time the anæsthetic was withdrawn, ordinary respiration was permitted, and the sponge was only replaced as its effect seemed to be passing away.

At the close of the operation the patient became conscious about ten minutes, and did not seem in the least disturbed; here was no after violence, and only very little sickness; indeed, he seemed to suffer less from it than did the administrators. When one sponge became frozen, another, previously warmed by immersion in hot water, was substituted.

As soon as the muscles were completely relaxed, Lister's large abdominal tourniquet was applied just over the umbicus, and was very slowly screwed down, until the flow of blood through the aneurism was arrested.

During the first half-hour the instrument slipped slightly twice, but was immediately re-adjusted.

About 2.30 P.M. the foot was found to be very cold, and was becoming a little blue.

At 3 P.M. I unscrewed the tourniquet a little; pulsation at once returned in the aneurism, and the pressure was immediately re-applied.

At 4 P.M. the blueness had extended up the leg, past the knee, and the left leg was also getting very cold.

At 5 P.M. the right limb was blue to the groin, and the left as far as the knee. The pressure was slightly relaxed, and during the relaxation the limbs somewhat improved. The tumour felt a good deal harder, but the pulsation had not entirely ceased.

At 6 P.M. the patient was 'black' in both limbs, and blue' as far as the tourniquet.

It was now therefore determined that the instrument must be removed, lest gangrene should follow, and this was accordingly done.
A quarter of an hour was taken in relaxing the pressure—a quarter turn of the handle being made every minute. The tumour was found to have ceased to pulsate, and to be firm and hard. The limbs were both swathed in cotton wool, and were tied together, and $\frac{3}{4}$th of a grain of acetate of morphia was injected under the skin of the arm.

During the whole continuation of the pressure slight pulsation could be felt in the left femoral artery. At 7 p.m. slight pulsation had returned in the aneurism but it felt quite firm and hard.

At 10.30 p.m. the pulsation had increased, but the tumour still remained firm; and the impulse was not so powerful and was much less distensile than before the commencement of the operation. The patient was restless and uneasy; another $\frac{1}{4}$th of a grain of morphia was injected.

The patient passed a restless night, and on the following morning the pulsation had returned with nearly its old force but the walls of the aneurism felt thicker and harder than before the pressure was applied. The beating, moreover, was more 'lifting' and less 'distensile,' and I ventured to predict that it would slowly cease altogether.

Two grains of opium were ordered, to keep the bowels at rest, and as little food as possible was given. The legs were both recovering their natural hue, and were becoming warm again.

Sept. 29 (morning).—The patient declared that he felt 'quite nicely;' the tumour was found to be again much harder, and pulsation could scarcely be felt in it.

(Evening).—Tumour very nearly fully reconsolidated, and warmth completely restored to both limbs.

30.—All pulsation had stopped, and the tumour was found hard and firm.

Nov. 14.—From this date there has been no return of pulsation; the aneurism has contracted, and has become as hard and as firm as a cricket-ball, and of about that size. The limb has completely recovered, and the patient is about again as usual.

For the administration of the anaesthetic in this case, and for the careful management of the tourniquet, I am indebted to our House Physician, Dr. Barfoot, and our House Surgeon, Mr. A. R. Dunnage; and to the latter I am also further indebted for this report of the case.

The annexed Table illustrates the variations in the radial pulse, and in that over the tumour, before and after compression was practised.
Table of Sphygmographic Tracings to illustrate Mr. Wheelhouse’s Case.

No. 1.—Right radial before Ether.

No. 2.—Right radial during Ether.

No. 3.—Tumour before Chloroform.

No. 4.—Tumour twelve hours after compression was discontinued.
XI.—A Case of large Femoral Aneurism, treated by Pressure, under Chloroform, maintained for fifty-two consecutive hours. By Barnard Holt. Read December 12, 1873.

Edward Thomas, ag. 33, married, was admitted into the Westminster Hospital Nov. 10, 1871, under the care of Mr. Holt, suffering from femoral aneurism, which occupied the greater part of scarpas triangle. Two-and-a-half years previously, whilst running after an omnibus, he fell in attempting to jump upon the step. Great pain was experienced in the upper part of the thigh, and on the following day a small pulsating tumour was detected, which gradually increased, having at the end of a year attained the size of a hen's egg. The leg and foot now began to swell, and as he experienced increased pain he consulted a surgeon, who informed him it was a bubo resulting from an injury, and that he must remain quiet. By rest the swelling subsided sufficiently to allow him to get about, but the tumour continued to increase; and the pain eventually becoming very severe, he consulted another surgeon, Mr. Jinks, who diagnosed the correct nature of the swelling, and sent him into the hospital under Mr. Holt. At the time of admission the aneurism was circumscribed, and occupied a considerable part of scarpas triangle. It pulsated forcibly, and a marked bruit could be heard. Pressure at intervals was tried for a week, but the pain being more than the patient could bear, and no effect being produced upon the tumour, it was determined to abandon the treatment until the skin, which had become somewhat sore, should resume its healthy condition. During this interval the aneurism became diffused, and extended upwards to the anterior superior spinous process, and downwards for some distance below the apex of scarpas triangle, the tumour occupying the whole of this space, overlapping Poupart's ligament above, internally overlapping the adductor muscles and externally pushing aside and overlapping the sartorius, the limb over the tumor measuring 25 inches. At its inner part was a conical soft elastic prominence, which was the last part to become condensed. On Sunday, Nov. 26, chloroform was administered, and pressure
was made upon the common and external iliacs alternately for twelve hours and a half, when, as neither pulsation or bruit could be detected, the pressure was removed, and the chloroform discontinued. The tumour was solid, and apparently cured. On the following morning both pulsation and bruit were again noticed, and it was evident the treatment had not been continued sufficiently long; the tumour was somewhat harder. Sickness now supervened, and continued for four days. A flannel bandage was applied round the limb, and as the patient had borne the chloroform well, Mr. Holt decided to again have recourse to pressure and continue it under chloroform so long as it was prudent.

On Dec. 9, at 6.30 p.m., the patient was again put under chloroform, and pressure commenced on the external iliac, and in half an hour was shifted to the common iliac. At 8.30 it was again changed to the external iliac; and at 9.20, being restless, it was removed to the common iliac, where it remained until 11.30, the pulse during this time ranging from 84 to 108. The chloroform was now suspended for half an hour to admit of his taking brandy and arrowroot; modified pressure was continued during this interval; at 12 the chloroform was resumed, and the pressure re-applied, shifting the tourniquets as appeared necessary.

10, 6.30 A.M.—Both tourniquets were removed. The tumour was hard, devoid of pulsation, but a slight bruit could be detected; the pressure was again applied.

8.30 A.M.—Chloroform suspended for half an hour, to admit of patient taking Liebig and brandy. Pulse, 104; temp. of limb, 98°.

9.15.—Chloroform resumed and pressure continued until 4 p.m., when it was again suspended for the administration of Liebig and brandy. When awoke the patient did not complain of sickness, but he desired to have the chloroform continued, as he experienced pain. Temp. of limb, 100°; pulse weak and 108. No pulsation, but slight bruit on outer side detected by Dr. Ogle, who at this time saw the patient.

With varying alterations of the tourniquets, and occasionally allowing the patient to resume consciousness for the purpose of taking food, the chloroform and pressure was continued for fifty-two hours; terminating on Dec. 11 at 10.31 p.m., when the aneurism was perfectly solid and devoid of either pulsation or bruit.

The limb, which was somewhat oedematous below the tumour, was carefully bandaged, and the horizontal position
was maintained. The tumour gradually subsided, and on Feb. 9, 1873, he left the hospital, walking well with the aid of one crutch.

Dr. Dupré, who examined the urine from time to time during the treatment, found chloroform but no sugar.

The case is interesting as showing that chloroform may in favourable cases be continued for a very considerable time, and that a very large aneurism of a most important artery may be cured where continued pressure can be maintained.

XII. — Case of Aneurism of the Superior Mesenteric Artery.

By George Pollock. Communicated by J. Warrington Haward. Read December 12, 1873.

WALTER W., æt. 40, was admitted into St. George’s Hospital Nov. 23, 1871, under the care of Dr. Barclay, who transferred him to Mr. Pollock’s care.

He had usually had good health, and lived temperately, but had had syphilis and gonorrhea. There was no cancerous history. He was a painter, and the bowels were usually costive. Six weeks before admission, while straining at stool, he felt a pain in the abdomen, and noticed a tumour near the navel. He continued to work, but the pain increased, and was most severe when he assumed the recumbent posture.

When admitted he was of a pale aspect and somewhat anxious expression. There was a pulsatory tumour the size of an orange, the centre of which was a little to the left of the umbilicus; it was freely movable, and the fingers could be thrust partially beneath it. The pulsation was synchronous with the heart’s systole, was decreased by pushing the tumour to the left side, and stopped by pressure upon the abdominal aorta. There was a distinct bruit to be heard over the tumour.

He was kept in bed for a week, without notable change in the symptoms; and on Nov. 28 chloroform was administered, and pressure applied to the abdominal aorta, as high up as possible, by a Lister’s tourniquet. This arrested the pulsation of the tumour and the femoral arteries. The tourniquet was screwed down gradually and carefully; and it was observed that as soon as much pressure was exerted upon the abdomen,
and before the pad reached the artery, that the man’s breathing became altered in character; it was shallow and embarrassed, but the pulse was not sensibly affected; when, however, the pad reached the aorta and the circulation was arrested, the rapid pulse instantly became quickened, feeble, and irregular. The pressure and anaesthesia were kept up for two hours and ten minutes, during which time the patient vomited several times, and had sweated profusely. At the end of that time he became so extremely faint, the pulse so very feeble and irregular, and the face so pale, that it was obviously neccessary to relinquish the pressure. No chloroform had been given for a quarter of an hour, though he remained quite insensible. As soon as the tourniquet was lowered he began to revive, and in a few minutes became sensible. The tumour appeared unchanged. Next day there was a large quantity of blood in the urine, and he complained of some abdominal pain, and was several times sick. At 2 p.m. ether was administered; he took the anaesthetic very well, and the pulse improved under its administration; but as soon as the tourniquet was screwed down the same symptoms were produced as occurred on the previous day. The pressure was kept up, however, for an hour and a quarter, when the extreme irregularity of the pulse and faintness rendered it necessary to relinquish it. He remained for several hours afterwards in a state of extreme faintness, from which he gradually recovered. After the second day’s pressure the tumour was decidedly more solid, but still present forcibly. There was blood in the urine for eight days. Pressure was subsequently made at intervals by a weight or tourniquet with sufficient force to diminish, but not to arrest, the circulation through the tumour. This pressure was managed by the patient, who became very expert in its application. The average time during which the pressure was made was an hour and a half, four times a day. He took iodide of potassium for a few days, but this was given up on account of producing iodism. He left the hospital May 22, with the tumour much more solid, but with the bruit and pulsation very distinct.

He was re-admitted Nov. 20, 1872. He was then much emaciated, and was unable to work. He had been in bed for some weeks, and had had but little food. The tumour had now increased, and extended from the margin of the left ribs to 3 inches below the umbilicus, and laterally from 1 inch to the right to 3 inches to the left of, and on a level with, the
umbilicus. He was kept at rest in bed, and given good diet, and remained without notable change till Dec. 20, when he rose from bed to get some water. On returning to bed he suddenly complained of pain in the abdomen, and immediately died.

Post-mortem.—The heart was found to be fatty, and the aorta and coronary arteries atheomatous.

The kidneys were granular, with adherent capsules and diminished cortices.

On the anterior surface of the left kidney, about its centre, and beneath its capsule, was a round tumour the size of a marble, containing stringy purulent matter, such as might result from the breaking down of a fibrinous clot. It had caused a corresponding depression on the surface of the organ.

The aneurism was of the superior mesenteric artery; it was fusiform in shape, and six inches long. It involved the aortic opening of the artery which formed part of the sac, and the branches of the artery were given off from the end of the sac. The dilated aortic just behind the sac had given way, and allowed the fatal extravasation of blood into the abdomen. The hepatic and other branches from the adjacent part of the aorta were atheromatous. The sac of the aneurism was nearly filled with laminated clot, there being only a small channel left through its centre, through which the blood still flowed.


G. D., æt. 15, was admitted into St. George's Hospital on Oct. 22, on account of growths springing from both irides.

He is a well-grown lad, somewhat slight and delicate-looking, but usually healthy, and with a good family history. He says that he is liable to colds, and that they affect his eyes; and that he has had enlarged glands in his neck. Three months ago he first noticed a small speck on the iris of the left eye, and this speck has steadily increased in size, and has lately begun to interfere with sight. A fortnight
ago he noticed two similar specks in the right eye, which seem to be increasing.

On examining the eyes, there was found to be a tumour, about the size and colour of a split pea, seated on the lower and inner quadrant of the left iris, extending to the margin of the anterior chamber, encroaching somewhat upon the pupil, and lying in contact with the inner surface of the cornea. The tumour was covered by a fine network of blood-vessels. The rest of the iris appeared healthy, the aqueous humour was clear, and the cornea generally transparent, but it was here and there dotted by small circular specks of opaque deposit. Vision was reduced to No. 8 of Jaeger's types, but with the aid of a convex glass of 24" he could read No. 1, showing that the defect was chiefly from interference with accommodation. There was a zone of fine injection round the cornea, and the presence and increase of the tumour were manifestly beginning to set up irritation.

In the right eye there were two small growths of a similar character, as large as pins' heads, springing from the outer part of the margin of the iris. This eye was in other respects healthy, and its vision was unaffected.

On Oct. 24 I removed the tumour from the left eye by making an opening into the anterior chamber on either side of it, and by uniting the two openings by means of a blunt-pointed knife. In this way a sufficient incision was made without wounding the tumour itself, which was drawn out by a blunt hook, and removed, together with the piece of iris on which it rested, by two or three snips with scissors. The specimen was handed to Mr. Warrington Haward for examination.

The wound left by the operation united readily, but the cornea became opaque at its posterior portion, where it had been in contact with the growth. Two or three days later a very slight and insidious iritis appeared, by which an adhesion was formed between the upper part of the pupillary margin and the anterior capsule, and by which the artificial pupil, left by the iridectomy, was slowly dragged down towards the cicatrix. This condition was treated by the local use of atropine, for which, at the suggestion of Mr. Bader, daturin was afterwards substituted. The irritation gradually subsided, and the corneal opacity has now much diminished. The sight, which was, of course, greatly impaired when the pupil was behind a patch of opaque cornea, has now improved in a degree corresponding with the clearing up of the opacity.
At the same time, however, a fresh tumour has appeared in the left eye at the outer margin, and the tumours in the right eye are steadily, although slowly, increasing. Mr. Haward reports that the growth removed was a round-celled sarcoma, and he has been good enough to bring some sections of it for exhibition to the members of the Society.

My reason for bringing forward the case at this early stage, and for exhibiting the patient, who is in attendance, has been the hope that I may receive some valuable guidance with regard to the course that should be pursued. It seems to me that not only vision, but life, are very seriously imperilled; and that almost the only prospect of saving life will be by removal of the eyes before vision is lost. As a first expedient I may perhaps remove the whole of the irides; but on all points of the case I shall hope to hear the opinions of the Society. I have not been able to make an exhaustive inquiry; but, as yet, I have not learnt that any like case has been recorded.*


Leah Comer, æt. 12, was brought to St. George's Hospital on March 5, 1872. She was a healthy-looking and well-nourished child, very intelligent, and presenting no trace, either in her person or history, of any kind of cachexia or morbid diathesis. Her left eye was projected downwards and outwards by a considerable swelling, which distended the upper eyelid. This swelling was of recent origin, and said to be increasing very rapidly. It was moderately firm, elastic, and the skin of the eyelid, which was freely movable, was traversed by distended veins. The displacement of the eye had produced double vision, but the sight of the displaced eye, when tested singly, was perfect, and the

* July 21, 1874. I have this day had an opportunity of seeing the patient again. There is very little visible increase of the growths; but vision, even of the right eye, is limited to the perception of large objects, and there are distended vessels emerging from the interior of both. The general health is excellent.—R.B.C.
ophthalmoscope showed no disturbance of the internal circulation. There was no history of injury, and no pain.

On the following day, March 6, the swelling was manifestly increasing, and the sight of the left eye was for the first time somewhat impaired, the patient not being able to read the smallest type easily. An incision was made through the skin, just below and parallel to the margin of the orbit, and the growth at once started into view. It was of a reddish-grey colour and moderately firm consistence, but broke down when grasped by forceps. It seemed to have no adhesions with the surrounding tissues, and was turned out and removed with great facility; the removal, as far as we could judge, being complete. The growth was handed to Dr. Whiphram for examination, and the wound was closed. Healing by primary union took place along its whole length, without the formation of a drop of pus; and in two or three days nothing but the scarcely visible cicatrix remained to indicate what had occurred. The position and movements of the eye were perfectly restored, and vision returned to the normal standard.

The child attended the hospital weekly to show herself, and all went well until the end of April, when the swelling returned in its former situation, and grew with very great rapidity. At the same time Dr. Whiphram reported that the original growth was an oval-celled sarcoma, of unquestionable malignancy; and by his kindness I am now enabled to ex-
hibit a section under the microscope, and also a drawing of the appearances. The patient was re-admitted into the hospital on May 2.

My own view was in favour of the immediate sacrifice of the whole contents of the orbit; but, on consultation with my colleagues, it was determined first to attempt another removal of the growth. On cutting down upon it in the line of the first incision, the growth again started into view but it was much softer than before, had no defined limits and seemed to infiltrate the surrounding parts. On passing my finger deeply into the wound, I felt a pulpy mass extending towards the apex of the orbit, and lost there among the normal tissues.

Under these circumstances the wound was closed for the time, and on the following day, having in the meanwhile explained the matter to the parents, I proceeded to deal with the growth by that combined use of the actual and potential cautery for which we are indebted to Mr. Lawson. The eye and eyelids having been first removed, the whole of the contents of the orbit were cleared out with scissors as rapidly as possible, and the cavity was dried with a hot iron. The ophthalmic artery bled very freely; but was at length stopped by pressure and the cautery. The whole cavity was then lined with Fell's paste, kindly supplied to me by Mr. Lawson for the occasion. Some morphia was injected under the skin, and the child removed to bed.

The shock of the operation was, of course, severe, and for some days the state of the patient gave us much anxiety. But she never had any symptoms of brain irritation; and soon began to show evidence of improvement. The bones of the orbit were thrown off by exfoliation, and she left St. George's for the Atkinson Morley Convalescent Hospital at Wimbledon, on July 3, just two months after the second operation. She has never since had any appearance of a return of the growth, and she is here to-night for the inspection of the members of the Society. I bring her case forward as a companion to those already recorded by Mr. Lawson; and also because I am not aware of any other instance in which this treatment has been employed for so young a subject.

Apart from any interest that may attach to the case, on account of this or any other personal peculiarity, it seems to me to furnish important evidence in favour of the growing belief in the originally and essentially local character of many
malignant tumours. Regarded in this light, it seems to show that the ordinary recurrence of sarcoma is not due to constitutional causes, but to imperfect removal of the primary growth, or, possibly, of the tissues in which it had its origin. I think we have evidence of the same character with regard to the most common form of intra-ocular cancer, glioma retinae; and it is manifest that the parts inclosed within the capsule of the eye—and, in a less degree, those included within the bony walls of the orbit—offer facilities for complete removal which are not to be found in any other portion of the body. In Dec. 1862 I removed an eye containing a glioma from a child three years old, who has ever since continued in perfect health, and whose people write to me, at my request, once every now and then, to tell me of his state. In this case, which has been published and often quoted, the character of the disease was determined, after removal, by Dr. George Johnson of King's College.

It is necessary to mention this element in the question, because the mere fact of the cure has been used to throw doubt upon the nature of the growth; and because, only a week or two ago, I removed an eye which I supposed to contain a cancer, but which was found to be filled by some growth of an uncertain nature, which had undergone caseous degeneration. But a very instructive example of the non-recurrence of glioma was furnished by a case that was treated, in succession, by my colleague, Mr. Spencer Watson, and by myself. The patient, then a male infant three months old, was brought to the South London Ophthalmic Hospital nearly four years ago, and came under the care of Mr. Watson. Both eyes were absolutely blind, and both retinae were studded over with white patches of a suspicious aspect. Mr. Watson asked me to see the case, and we advised that one eye should be removed for examination. This was done, the right eye being selected for the purpose; and it was submitted to the Committee on Morbid Growths of the Pathological Society. That committee reported that the patches were gliomata, and we advised the removal of the second eye; but to this, although the child had made a perfectly good recovery from the first operation, the parents would not consent. After the lapse of three years the child was brought to me at St. George's Hospital, the left eye being red and irritable. I found that there had been no recurrence of the disease in the right orbit, but that in the left eye it was just beginning to grow and increase. I
Mr. Carter's Case of Orbital Sarcoma.

strongly urged the immediate removal of the left eye, thinking there was still time to save life, but the parents refused to consent. A month later the eyeball was rapidly enlarging, and causing great pain. I again urged the operation, saying that, if too late to save life, it would at least prevent much misery. The parents again refused; but, three months later, worn out by the child's screams and sleeplessness, they brought him again, and said, 'Do what you think best.' The little patient was much exhausted, and the eyeball had become as large as a small orange; but as I had no certainty that the brain was invaded, I thought I would at least employ the orbit effectually. In order to save blood, I did this means of the galvanic cautery, and then lined the cavity with chloride of zinc paste. The child bore the operation well, but survived only a few days; and after death a large cancerous mass was found in the left hemisphere of the brain and the intracranial portion of the left optic nerve was infiltrated by a similar material. But two things were especially noticeable in the autopsy. First, that the application of the actual and the potential cautery to the inner side of the orbital wall had not produced any meningitis or other intracranial lesion. Secondly, that the right optic nerve had wasted into a mere fibrous cord, and that there was no recurrence of cancer in it, or in any of the structures contained in the right orbit. I venture to think it is almost sure, if both eyes had been removed at about the same time or before the growths took on a state of activity, that the child's life would have been preserved; and whatever is true of the eye must also be true, mutatis mutandis, of other structures of the body. I think that the cases I have related plead powerfully for the early and complete removal of cancer wherever such removal is practicable; and that they teach, very emphatically, the danger of delay. It is not within my province to discuss the methods of removal that may be available in other portions of the organism, but only to urge that removal, if effectually accomplished, affords a reasonable prospect of cure.
HEUMATISM is usually enumerated among the causes of aneurism, and excluding for the moment all speculations as to whether the rheumatic diathesis may or may not be favourable to the occurrence of more early degeneration of the great vessels, there are, I imagine, two ways in which aneurism of the aorta may possibly arise as the consequence of rheumatic fever, viz.:

1st. By extension of the rheumatic endocarditis to the commencement of the aorta, causing acute atheromatous disease there, with consequent softening and impairment of resistance to blood-pressure.

2nd. By chronic atheromatous disease, arising as the later result of the aortic regurgitant disease of the heart which has been left behind by the rheumatic fever.

It is with the view of testing the accuracy of these two propositions that I have ventured to bring before the Society to-night three cases of aneurism, which I will briefly relate.

I cannot say that I have myself ever seen post-mortem evidence of extension of rheumatic endocarditis to the aorta. Death directly from rheumatic fever is comparatively rare, and but few instances have fallen within my observation. Nor am I aware of any reference to the subject in current pathological works. Dr. Moxon has, however, kindly drawn my attention to a work on pathological anatomy, by Dr. Wilks and himself, now passing through the press, in which the following passages occur, which I may perhaps be allowed to quote:

In cases of aneurism in young persons there has often been a history of acute rheumatism. It was so in a case of axillary aneurism in a girl under Sir W. Gull's care; also in a case of aneurism of mesenteric artery. In these rheumatic cases there is always a probability that rheumatic carditis and embolism may have been the cause of the aneurism; but, we may ask, does it happen that along with rheumatic endocarditis there may also be a rheumatic endarteritis, weakening the affected spot and leading to dilatation and aneurism?
Dr. Powell’s *Cases of Aneurism,*

We have occasionally met with a patch of inflammatory softening of the aorta near, but not continuous with, aortic valves that were acutely inflamed from rheumatic fever, but in all instances but one there was reason to ascribe the aortitis to impact of vegetations attached to the valves. So that we know of but slight grounds for supposing that there is an arteritis directly rheumatic.

There thus seems to have been one case only about which these distinguished authors felt doubtful whether the aortitis was not an extension of the rheumatic disease.

The clinical inferences from the following case seem to be so strongly to favour the view that aneurism may arise in this way, that, with the kind permission of Dr. Reynolds, I have brought it forward this evening.

**Case I.**

John Milson, nat. 17, a grocer’s boy, had rheumatic fever at the age of ten years, and again at eleven, and has suffered from shortness of breath and palpitation since—these symptoms becoming urgent shortly before his admission into University College Hospital, under the care of Dr. Reynolds, Jan. 1873.

I pass round a diagram showing the main points of his case, as observed by myself on two occasions, Jan. 25 and July 2, 1873. I need only briefly enumerate these signs.

He is a thin, delicate-looking boy, with dark sallow complexion. The chest and extremities show the signs of early rickets, the chest being long and compressed laterally. The vessels of the neck visibly pulsate, and the radial pulses are sharp and dichrotous. The pulses and pupils are equal, the two sides, and there is no œdema of the extremities. He complains of no pain.

The heart’s apex beats at the sixth space, a little outside the nipple. Its impulse is thrusting and now strongly hearing, although at the first observation it appeared more as if the heart were bodily propelled downwards and forwards, its apex being pushed between the sixth and seventhe ribs. Cardiac dulness not appreciably increased. Over the second and third cartilages and the intervening space an impulse is felt with each cardiac systole, accompanied by a marked thrill. The impulse is at its maximum at the second space, where it is visible to the eye. It has lately extended into the subclavicular regions and to the fourth cartilage, and has become more distinctly expansive, and there is now over the region
of impulse a perceptible bulging. There is dulness on gentle percussion from the sterno-clavicular articulations to the third space, and from the sternal margin to the mid-clavicular line, the dulness being absolute from the second to the third cartilage, including the point of maximum impulse and thrill. There is but little appreciable hardness on percussion.

A systolic murmur is heard most loudly at the second space, and is conducted in all directions, but most distinctly to the right and upwards. Succeeding to this murmur, in the same situation, a faint churning sound was in January and July audible over only a very limited area. It has now disappeared. A fine high-pitched and prolonged diastolic bruit, observed by both Dr. Reynolds and myself to be distinct from the churning sound, is audible directly over the aortic valves, and downwards to the ensiform cartilage.

At the right supra-spinous fossa the respiration is bronchial and the murmur is much more audible than on the opposite side. There is no alteration in the respiration at the bases.

This boy has never received any injury, nor strained himself in any way. He is now following a light occupation as shopboy, feeling tolerably well, but very short of breath.

In this case, then, we have the signs of aortic regurgitation—murmur, sharp pulse, and locomotion of vessels—and, in addition, those of aneurism of the first portion of the aorta, viz. impulse, thrill, and double murmur over the vessel, and tubular respiration at the supra-spinous region.

From the history, and particularly from the youth, of this patient, we can scarcely doubt, I think, that the aneurism has arisen as the direct consequence of rheumatism—as the result, in fact, of rheumatic endarteritis.

It is possible that the aortic regurgitant disease has been an important auxiliary cause of the dilatation; in more advanced age it may, as I shall endeavour to show, become an important cause of aneurism; but, with the elastic and better-nourished vessels of youth, it is contrary to experience that such a cause could be sufficient alone to occasion aneurism.

I think the absence of much dulness, and particularly of hardness on percussion, the presence of thrill, and the nature of the impulse and murmurs in this case, favour the probability that the aneurism is of the globular rather than the sacculated kind. At any rate its communication with the aorta must be very wide.
The following cases are the most striking that have come under my notice, which lead me to think that aortic regurgitation may, in advanced or middle life, be an efficient cause of aneurism.

Case II.

Thomas Brown, æt. 37, a bricklayer, first came under my notice in March last, at the Brompton Hospital, suffering from pain in the centre of the sternum and dyspnoea, much increased on exertion, and occasionally occurring in severe paroxysms. He was a man of steady habits, and, with the exception of an attack of ague and diphtheria, his health had been good until 1862, when he had an attack of rheumatic fever. He states that for three months after this attack he suffered severely from palpitation, and that he continued ill for two years. Four years before he came under my observation he had attended as an out-patient, under the care of my colleague, Dr. Tatham (Jan. 1869), who noted the existence of a loud diastolic bruit over the aortic valves and conducted downwards to the ensiform cartilage, also a slight systolic aortic bruit. In the following March he had an attack of dyspnoea, which he does not distinctly describe, but which lasted four hours. In April his general health had improved, and, not having had any return of the pain or dyspnoea, he left off hospital treatment.

When I first saw him, in March 1873 (four years later), he presented the following signs:

The heart's apex beat at the sixth space, 2 in. below the nipple. Impulse strong, and diffused to the epigastrium. Cardiac dulness slightly increased. There was audible at the apex a slight systolic bruit, apparently generated there, and also a bruit (systolic) which increased in intensity as the stethoscope was passed towards the aortic region, where it was loud and rough, and conducted still onwards towards the shoulder: opposite the second and third cartilages it was accompanied by an impulse very perceptible to the ear, slightly also to the hand, and attended with a distinct thrill. The impulse was also appreciable to the eye. At the third space, close to the sternum, a high-pitched musical diastolic murmur was most audible, and conducted most distinctly towards the ensiform cartilage, where its musical character became nearly lost, and the blowing sound of the murmur was more distinct. Over the second and third cartilages there was some questionable defect in resonance, but deep per-
ussion was not employed. There was considerable fulness in upper abdomen, below the normal hepatic region. There were no pressure symptoms, but at the right posterior apex there was slight but decided dulness on percussion, with tubular respiration, and here the systolic and diastolic bruits were both audible, and much more distinctly so than on the other side.

The only point of importance in the family history of this patient is his statement that his mother, aged 53, ‘dropped down dead with heart.’

This patient was admitted into the Brompton Hospital in June, and was seen by me in Dr. Cotton’s absence. My note (June 4) was as follows: ‘Heart’s apex at upper border seventh rib in nipple line. Impulse increased. Upper margin of dulness fifth rib; dulness extends to right of sternum and over precordial region, with some diffused impulse. Distinct and visible impulse at third rib space close to sternum, attended with thrill. At this point a loud rough systolic bruit, with fine musical diastolic bruit; towards the ensiform cartilage the musical bruit is superadded to a rough, blowing sound. Systolic murmur is audible over right upper scapula. Pulse aortic; arteries visible; no pressure signs, but liver slightly depressed. Urine contains no albumen.’

**Case III.**

M. A. Wiggins, æt. 48, a married woman, came under my notice at the Brompton Hospital in May 1873. She had had rheumatic fever seventeen years ago, and had been ill for five years with pains in the chest and dyspnœa, and with some cough and loss of flesh. She came under the care of my colleague, Dr. Chas. T. Williams, in Feb. 1870 (3½ years ago), presenting the signs of double aortic disease. She was repeatedly examined by Dr. Williams, and sphygmnographic tracings of the pulse taken, but nothing more discovered. The following are the brief, but instructive, marginal notes made upon the paper from time to time.

- Feb. 17, 1870.—Sphyg. examination, Dr. Sanderson.
- March 17.—Urine, no albumen.
- Oct. 31.—Cough troublesome at night; ankles swell; dyspnœa; pulse, 92, regular.
- Nov. 17.—Physical signs as before; urine, no albumen; no oedema.
- Dec. 15.—Chest examined; still double murmur.
Jan. 2, 1871.—Worse.
19.—Urine scanty; pain in passage; sphyg. observation.

March 23.—Murmurs very loud.
April 20.—(She was seen by me). Urine, no albumen; pale.

Jan. 1, 1872.—Double aortic bruit, and probably mitral; diagnosis, cardiac disease; grave prognosis.

Feb. 27, 1873.—Impulse decided in aortic region, where there is a low-pitched expansive bruit; finer diastolic bruit; systolic thrill (commencing aneurism).

May 29.—Cough bad at night; no expectoration; aching pain, with tightness, in the precordial region; states breath to have been bad one month.

Some dulness and impulse in aortic region. Bruit and thrill as before; bruit audible in right supra-spinous fossa.

Nov. 27.—On examining the patient to-day with Dr. Williams and Dr. Farquhar, the signs were found to be much the same, but there appears to be some extension of the aneurism downwards and towards the right nipple. This is in accordance with Dr. Sibson's experience that aneurisms affecting the first portion of the aorta usually manifest physical signs first at the second space, and that then signs advance in a downward direction.

I will here venture to add an account of a fourth case which has come under my notice too recently to be shown to the Society, but which nevertheless is so similar in its character to the latter two cases, that I think it may usefully be included in the series.

Case IV.

Clara Merritt, æt. 30, came under my notice as an outpatient at the Charing Cross Hospital, May 8, 1874.

History.—Father subject to gout; died of apoplexy at the age of 64. Mother asthmatical; died of bronchitis and dropsy. Patient has been married nine years; has had no family; never miscarried. Her husband is a steady man, an envelope-maker.

Sixteen years ago patient had rheumatic fever, not followed by any heart symptoms. Six years ago, however, she had 'rheumatism at the heart and congestion of the lung,' as her medical man is said to have informed her, and she has suffered from cardiac pains and palpitation since. Between the first and second attacks of rheumatic fever she suffered from rheumatic pains in the joints, and 'for years' she has
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complained of swelling of the feet towards evening. From childhood she has had a cough, never haemoptysis, but was subject to occasional epistaxis before the second attack of rheumatism. She had small-pox at the age of two years.

For about two years she has been conscious of a blowing noise in chest (referred as to time and situation to the seat of systolic bruit over aorta).

This patient was admitted into one of Dr. Green's beds in the hospital, and I examined her with the House Physician, Mr. Taylor, who was kind enough to take down the following notes at my dictation:

Present State.—May 29, 1874. A thin dark-haired woman, with a slightly puffy face, pitted by small-pox and marked by acne-rosacea. The arterial system generally is not marked by disease. The arteries visibly pulsate; no difference in radial pulses. There is some deformity of the small joints from old rheumatic arthritis. The urine, carefully examined on two occasions, has been found free from albumen. The heart's apex beats at the fifth space in nipple line. Impulse heaving. From the first to the third right cartilages and from mid-sternum to the line of the right nipple a purring systolic thrill is perceptible to the hand, very intensely so at the first and second spaces close to the sternum, where also an undulating impulse is distinct both to eye and hand. There is dulness on percussion over region of impulse. The cardiac dulness extends from the third to the fifth left cartilages, and for two fingers’ breadth to the right of the sternum.

A very loud rough systolic bruit is heard in the aortic region. This murmur is loudest over right upper half of chest, is audible all over the front, and is distinctly perceived by the patient. It is also heard in both interscapular regions. At mid-sternum, and increasing in intensity towards the ensiform cartilage, is a finer bruit, diastolic in time.

Sibilant rhonchus is heard generally over the chest with respiration, and some coarse crackling rhonchus over the right upper chest, in front a slight crackle or friction, evidently of pulmonary origin, being heard over the diseased aorta at the end of each systolic bruit. There are no signs of pressure present.

In this case, too, we have evidence of dilated aneurism of the aorta—dulness, thrill, impulse, bruit—in addition to aortic regurgitant disease.

Remarks.—It is quite possible that the rheumatic inflammation may have extended in these latter three cases to the
commencement of the aorta, and may have left behind some permanent damage to the nutrition of the wall of the vessel there, which in later years caused it to yield before the blood-pressure. I am inclined, however, to take a more mechanical view of the way in which the rheumatic fever has in them been the remote cause of aneurism, viz. that the atheroma and dilatation are the consequences of the aortic valve lesion left behind by the rheumatism.

If we reflect upon what must be the effect of aortic regurgitant disease upon the aorta and the heart, we shall note first of all—what is indeed a matter of daily clinical observation—that this is the disease of all others that leads to great hypertrophy of the left ventricle, and at the same time in this disease the aortic orifice becomes as a rule more patent than natural. Hence we have the volume of blood more rapidly and forcibly propelled against the first portion of the aorta than is natural. But with the recoil of the great vessel the blood flows not only onwards but also backwards through the valves; hence the aorta is more abruptly and completely emptied than is natural. We have thus the aorta at one moment in a comparatively empty and flaccid condition, deprived of that column of blood which should equally support and distribute the force of its recoil; at the next moment this flaccid vessel is suddenly stretched by an undue volume of blood sent with increased force from the hypertrophied ventricle. We may see the effect of this sudden filling and sudden collapse of the main vessel even at its second and third branches, in the water-hammer pulse and the throbbing carotids. The effect of the increased shock upon the aorta is I think necessarily to strain it, and to tend to produce in it those athermatous changes which have been so clearly shown by Dr. Moxon and others to be occasioned by local strain. Further, the same cause continuing to act upon a weakened vessel, may lead—and I think, in the cases above related, has led—to the production of aneurism.
XVI.—A case of Double Facial Paralysis, with Paralysis of Four Extremities; General Anaesthesia; Imperfect Paralysis of Respiration and Deglutition; Paresis of the Bladder; Recovery under Anti-syphilitic Treatment. By Thomas Buzzard, M.D. Read March 13, 1874.

The following case is one of recovery from a condition of the gravest possible character. The patient, who has been at his work for the last eight months, is in attendance for the examination of the Society.

William H., æt. 45; bootmaker, married; four children; was admitted into the National Hospital for the Paralysed and Epileptic, under my care, on Jan. 8, 1873. The following notes were then taken:

The patient has his right lower extremity paralysed and wasted, the foot being in a condition of equino-varus, from an attack 'when he was cutting his eye-teeth.' On this account he has always been lame, and forced to help himself along with a stick; but he has worked hard at his employment.

Present Condition generally.—He cannot stand, and has but little power of moving his arms; nor can he shut either eye; and his face is without expression. Cutaneous anaesthesia is more or less generally marked throughout the trunk and extremities. His respiration and deglutition are greatly impeded.

Present Condition in detail.—The eyes remain permanently open. On being asked to close them, the eyeballs are turned upwards, so that the cornea on each side is concealed by the upper lid; but there is no power, or only the very slightest, of approximating the lids. He cannot wink or frown. If the conjunctiva covering the sclerotic be touched with the finger-point, avoiding other structures, the touch is said to be felt, but there is no reflex action of the orbiculares. The orbital fissure of the left eye is somewhat the widest.

There is no perceptible strabismus, nor has there been any. The eyeball on each side is freely moved in all directions. The left eyeball appears unusually prominent. My colleague, Dr. Gowers, by carefully testing the eyes, found that there
was slight diplopia, referable to impairment of power of the
right external rectus muscle.

The sight is perfectly good, and the ophthalmoscope shows
nothing abnormal.

As regards the lower half of the face, this does not wear
the mask-like aspect seen in cases of confirmed labio-
glosso-laryngeal paralysis. There is some power of moving
the muscles about the mouth and lips, though this is much
enfeebled. The patient can open and shut his lips, and
purse them up to a certain extent, in an effort to whistle,
but fails to accomplish this process. The saliva does not
escape from his mouth. The cheeks bag. Food, such as
bread, taken into the mouth, is managed with difficulty, as
he cannot get it between his molars, except occasionally.

Swallowing is greatly impeded. He cannot swallow solids
at all, and fluids can only be taken with a spoon. They have
on several occasions nearly come through his nose.

The muscles of mastication act normally. The movements
of the tongue appear to be normal.

The left side of the soft palate hangs lower than the right.
On touching the soft palate with a feather, the uvula is
drawn up somewhat sluggishly, and the muscles of the palate
fail to contract with any energy. Tickling the posterior
wall of the pharynx causes a tendency to retch, but much
less than in health.

The taste is asserted by the patient to be perfect.

All over the face the sensibility of the skin is somewhat
impaired. A touch is not felt as in health. The patient
observed this first about the lower part of the face, where
the passage of the razor produced an unusual sensation.

The sense of smell and of hearing is perfect.

The speech is altered, according to his friends' account;
they describe it as thick. The letters 'b,' 'p,' and especially
've,' are imperfectly pronounced; the word 'the' is clearly
expressed.

The speech is not exactly nasal, as in cases of diphtherial
paralysis. Its character is perhaps a compound of the nasal
with that which comes from speaking with a pebble in the
mouth.

There does not seem to be any impairment of the sterno-
mastoid and trapezius muscles. The head is freely turned
to either side, and the shoulders are vigorously shrugged.
The movements of breathing are very shallow, and occasion-
ally supplemented by forced sighing inspiration. There
is but little movement of the diaphragm. Owing to the distress of breathing he cannot lie down, and has much difficulty in narrating his history.

The grasp of the hands is almost nil. The arm and forearm on either side can be moved, and the latter slowly flexed upon the former, but not against the slightest resistance from another. There is no wasting of the muscles. A general deficiency of cutaneous sensibility is observed in the upper extremities; and this is most marked perhaps in the fingers, but as these are horny from the effects of toil, it is difficult to be positive on this point.

The right lower extremity has been atrophied since infancy. There is very slight power of voluntary contraction in the anterior muscles of the thigh on either side, and least upon the right.

There is no power of voluntary muscular contraction in either leg.

A touch can be felt all over the legs, but the cutaneous sensibility is a good deal impaired.

There are no reflex contractions on tickling the sole of either foot.

The patient describes a sensation as of a tight band around his belly, between the ribs and the pelvis.

The cutaneous sensibility appears to become rapidly improved just above the lower margin of the thorax, but in no part of the trunk does it seem to be quite perfect.

There are no pains in his extremities, and there have been none, nor have there been any involuntary contractions of the arms or legs.

A sense of numbness and weight is complained of in each leg, and occasionally a 'throbbing runs down the left thigh and calf.' For the first two or three weeks of his illness he had 'pins and needles in his legs.'

No abnormality is to be found in the spinal column, and deep percussion gives no sign of tenderness in it at any point.

The expulsive action of the bladder is impaired to a slight extent.

The condition of the lower bowel is normal.

Reproductive power is in abeyance.

Electrical Examination.—The muscles about the mouth contract to an intermittent battery current (Stöhrer, 8 cells), which is insufficient to produce any effect whatever on the corresponding muscles of a healthy man. They do not
respond to the strongest induction current which can be borne.

The muscles of both arms evince a greatly diminished excitability to faradism. There is no increased reaction in them to the intermitted battery current.

In the muscles of both legs the excitability to faradism is entirely absent. In the left thigh it is very much diminished, but in the right thigh there is only the slightest trace. In no part of the upper or lower extremities is there any increased excitability to the intermitted battery current. This is only to be found in the muscles of the face.

The urine contains no albumen and no sugar. The lungs are healthy; respirations 18 in the minute.
The heart-sounds are normal; pulse 76, weak.
Temperature, 99° Fahr.
There is no headache, and no intellectual confusion.
There has been no vomiting.

History of the Attack.—On Tuesday, Dec. 10 (a month before I saw him), the patient walked after breakfast to his shop, a distance of a quarter of a mile, and set to work boot-making. Soon after beginning he felt a sense of numbness in the finger-ends of both hands, and a difficulty in picking up minute objects. The first symptom, indeed, that he noticed, was that he could not pick up the 'brads' used in his work. He went on working, however, and towards evening walked home, feeling very weak in the legs. He went to bed as usual, and slept five or six hours.

Wednesday.—Got up and walked to his shop, feeling his legs still more weak. During this day 'a feeling of numbness began in his seat, and gradually got down his thighs, and into the calves of his legs.' He worked, however, although the fingers were very numbed. At the close of the day he walked part of the way home with the help of a stick, but for the remainder was forced to lean upon a friend. In the evening he acted as president of a club, and wrote memoranda.

Thursday.—Had difficulty in standing, but managed with great effort to walk to his shop and commence work. In the afternoon he felt so ill that he was obliged to leave off work and go home. He was languid and prostrated, but managed to 'crawl home on foot,' halting several times on the road.

Friday.—His legs could scarcely support him, and his fingers were much numbed. He was unable to go to work.
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Saturday.--Was confined to his chair, being unable to stand. His hands and arms by this time were in a state of 'tingling numbness,' as high as the elbows, and felt very heavy.

About a week after the beginning of his illness he began to have a sensation of a tight band round the belly, which afterwards extended to the short ribs.

The left arm held out best for the longest time, but after admission and a week's treatment the right arm improved most. Altogether his lower extremities have been much more affected than the upper.

For the first week of his illness he could use a knife and fork for his food. A little later he would lean his fore-arm on a table, and, with his face lowered, would contrive to convey his food (fluid, or bread sopped in fluid) into his mouth by means of a spoon held between the first two fingers of the right hand. Later still, he was obliged to be fed by another.

His speech began to get thick about a week after the commencement of his illness, and from that time there was also constantly increasing weakness in his limbs. The difficulty of swallowing began a week later, and the shortness of breath was noticed by him at the same time. He was not aware, until I called his attention to the fact, that he was unable to close his eyes. The upward roll of the globe, by bringing the pupil behind the upper lid, on his attempting to close the eyelid, had served to prevent the deficiency being observed, since the light was shut out by this movement.

At no period of his illness had he noticed any feverish symptoms.

Previous History.—His general health has always been good, and he has worked, as his hands indicate, very laboriously. He has been of temperate habits. He has had no blow, nor wound of any kind; no diphtheria nor sore throat. He is marked with small-pox.

On examination a slight induration of glands is found in the right groin. He had a chancre fourteen years ago, with a bubo, the size of an egg, hard and prominent. This was lanced at a hospital, and blood flowed. On poulticing it matter appeared a few days afterwards, and it was open for six weeks. He does not think he took any mercury. The affection was not followed by any sore throat or eruption of the skin, and he has never had any nodes.
Progress of the Case.—The condition of this man was so grave that, although he presented himself as an out-patient, I at once admitted him into the hospital, fearing, from the degree to which respiration was involved, that he would die upon the road if I allowed him to return home. He was put upon a water-bed, and ordered eggs, wine, and beef-tea, with 10-grain doses of iodide of potassium three times a day. Within twenty-four hours he had improved, so that he felt much less distress of breathing and increased strength. Day by day there was still further improvement. On Jan. 16, eight days after admission, he could lie down and sleep at night, could close the right eyelid one-fourth of the normal extent, could swallow bread and butter in addition to beef-tea and wine, but as yet nothing more solid. I now found the muscles on the front of the left leg responding, although sluggishly, to a strong induced current. On Jan. 20 the right eye could be shut completely, and the left to three-quarters of its normal extent of closure. He could eat meat, and his features showed a trace of a smile for the first time. His right arm had increased considerably in strength. There were still no reflex contractions on tickling the sole of the foot. He no longer complained of difficulty in breathing. By the end of January both eyes could be closed, and in a few weeks more all the facial muscles had quite recovered. At the end of February his limbs were stronger, but he could not stand. He complained of numbness in the arms and legs. Early in March he could extend the leg against firm pressure, and his other symptoms had improved in a corresponding degree. On May 6 he began walking, leaning on an attendant, and on May 13 with the aid of a stick and crutch. On May 21 he was discharged, able to walk and use his arms, but still somewhat weak. All symptoms of paralysis had then disappeared.

Since that time he has presented himself occasionally as an out-patient, rather because I wished to keep him under observation than that he had need of medical advice. He gained strength, so that on July 23 he said that he was nearly as well as he had been before the attack, and that he had been able to resume his employment. Since then his only complaint has been a little numbness in the arms and legs, and slight remaining tightness in the precordial region. When I last saw him, a few days since, he told me that this symptom had disappeared.

Treatment.—Besides the measures already described, the
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treatment consisted in the administration of iodide of potassium, at first in 10-grain doses three times a day, increased at intervals to 20, 30, 40, 50, and eventually to 60 grains at a dose, three times daily. The increase was regulated by his progress. If, after improving for several days whilst taking a certain quantity, his progress appeared to come to a standstill, the dose was enlarged, and this always appeared to give a fresh impetus to his recovery. About the end of February, when he had been taking 60-grain doses, and had ceased to show any marked daily improvement, I ordered him to be injected subcutaneously with a solution of mercury of the kind devised by Dr. Staub of Paris. The following is the formula for this, which is called a solution of the alkaline chloro-albuminate of mercury.

No. 1.—Hydrargyri perchlor. 1.25 gramme; ammonii chloridi, 1.25 gramme; sodii chloridi, 4.15 grammes; aquæ distill. 125 grammes. Dissolve and filter.

No. 2.—Dissolve the white of an egg in 125 grammes of distilled water. Filter.

Mix the two solutions, and filter. One gramme of this solution contains 5 milligrammes of mercury.

The medium dose is one centigramme daily, in two injections. Reducing the weights to the English measure, I calculate that ten minims will contain nearly $\frac{1}{10}$th of a grain of the mercurial salt.

I began by injecting 5 minims of this solution, and gradually increased this dose to 10, 15, 20, and 25 minims. The latter quantity was injected daily from April 19 to May 21. A little local irritation followed each injection. There was smarting pain for about half an hour, and a small lump appeared at the spot, but no abscess ever occurred. No salivation was produced. The patient continued to improve whilst this treatment was followed.

When the muscles began to respond to the induced current, this was applied regularly three times a week.

Remarks.—My first impression on seeing this patient in the consulting-room was, that I had to do with one of those cases of acute ascending paralysis, dependent upon rapid and extensive softening of the cord, of which one now and then sees examples, and which are remarkable for their terrible fatality. Observing the degree to which the respiratory movements were embarrassed, I thought that it was only a question of the time during which the man could live. One point, however, speedily threw doubt upon this idea of the
nature of his disorder. The double paralysis of the face was a symptom which I had never known to occur in such cases, the course of which is very uniform. With or without slight preceding pains and formication, there is observed a gradually increasing weakness in the lower extremities, often so trivial that the patient attempts to 'walk it off.' In the course of a few hours or days the weakness has progressed to a paralytic state of the legs and thighs, then of the abdominal muscles, and those of the ribs, the upper extremities being afterwards gradually involved. The breathing becomes diaphragmatic, aided by the extraordinary muscles of respiration. Frequently there is paralysis of the rectum and bladder, and sometimes—quite at the last—deglutition may be involved. At this point, but usually before this, death ensues. There is great tendency in such cases to the production of rapid and extensive sloughing of the skin over the sacrum and hips.

In the case under observation there were no nutrition disturbances of this kind; and there was, moreover, evidence, in the double facial paralysis, of lesion of a higher portion of the cerebro-spinal axis than is reached in examples of acute myelitis. This may be because, in such cases, death anticipates the involvement of this portion.

It is evident, from the results of this case, that it was not an instance of this kind of lesion. The idea of myelitis must be at once dismissed, as incompatible with the patient's rapid and complete recovery. We need not, therefore, dwell longer upon this point.

There remain three conditions which it is necessary to consider, in coming to a conclusion as to the probable seat and nature of the lesion.

1. Was the case one of diphtherial paralysis? Apart from the fact, about which the patient was carefully questioned, that there had been no preceding sore throat nor abrasion about the body, the symptoms differed considerably from those of the paralysis which occasionally follows diphtheria, and sometimes other acute disorders—as typhus. Under such circumstances there is a striking constancy in the character of the symptoms, and especially in their order. The paralysis nearly always begins in the velum palati, and is marked by a peculiar nasal voice and difficulty of swallowing. Next there is always, I think, trouble in accommodating the eye for near objects. Less frequently some of the external muscles of the eye are attacked, causing diplopia.
Then comes incomplete paralysis of the extremities, with more or less anaesthesia. The condition generally is one rather of paresis than of paralysis. I have never met with an instance of complete double paralysis of the facial nerves as a consequence of diphtheria. In the case described there was complete paralysis of the lower extremities before any disturbance took place in the act of deglutition, and there was never any difficulty in accommodating the eyes.

2. Lesion of one side of the pons Varolii is well known to produce hemiplegia on the opposite side of the body, accompanied by facial paralysis on the side of the lesion. We may consider, therefore, that lesion involving both sides of the pons might produce double facial paralysis, and double hemiplegia, part of the phenomena observed in this case. A large tumour, for example, might occupy this position, and moreover by slightly compressing the right sixth nerve, leaving the left sixth intact, account for the imperfect paralysis of the right external rectus muscle which was observed. The only tumour capable of disappearing rapidly under iodide would be of syphilitic character, and there may therefore have been a gummatous tumour in this situation. But I do not think that this alone would suffice to explain the paralysis of the intercostal muscles and that of the diaphragm. I feel certain, too, that it fails to explain the marked sense of constriction—the feeling as of a tight band around the belly—of which this man complained. If I except a somewhat similar symptom which I have occasionally heard described by chloro-anæmic patients, I have never known this sensation except as a result of lesion about the spinal cord. To explain the occurrence of these symptoms, it is necessary to suppose that another gummatous tumour was situated in the upper part of the spinal column, and this may possibly have been the case.

3. Inflammation of the membranes in the region of the pons Varolii and spinal cord, producing thickening at different points, might give rise to all the symptoms remarked in this case. Such inflammation could not, in this instance, be the result of cerebro-spinal meningitis of epidemic character, from which the case evidently differed in numerous respects, and notably in the absence of fever. So also the history of the case precludes the idea that it was one either of idiopathic or of tubercular inflammation of the soft membranes. There was neither fever, nor pain of any description, nor muscular contractions, nor any affection of...
the sensorium. There was no sign, indeed, of the irritative
phenomena which would have resulted from the presence of
inflammatory exudation under the arachnoid.

On the whole, it seems most probable that the dura mater
lining the basilar process of the occipital bone, and a certain
portion of the vertebral canal at its upper part, was the seat
of the lesion, and that the affection had its origin in the
syphilitic infection which this man had incurred fourteen
years previously.

In describing syphilis of the dura mater, Virchow dis-
tinguishes two kinds, according as the affection is seated in
the external or in the internal layer of the membrane. The
affections of the external layer, he says,* nearly always pre-
sent themselves under the form of pachy-meningitis externa,
and this belongs to the same category as external syphilitic
periostitis, since it may determine exostoses, hyperostoses,
and circumscribed gummy tumours. It is this condition
which often gives rise, by the pressure of the resulting swell-
ing, to paralysis of one or more cranial nerves, so commonly
observed in the sequel of syphilis. It may be circumscribed
or diffuse. When diffuse it may extend over large sur-
faces, involve the seats of passage of many nerves (this is
especially common about the sella turcica), and produce
paralysis, whilst the organs contained in the cranium present
an appearance of perfect integrity. It may be associated—
but this is by no means always the case—with more or less
grave affection of the osseous tissue in relation with the dura
mater. When the external layer of the dura mater is
affected, the internal layer often presents a simple chronic
inflammation which causes thickenings, adhesions to the pia
mater, and even inflammatory modifications in the adjacent
parts of the brain.

Internal pachy-meningitis presents itself frequently under
a gummatus form, though still more often it is of simple
character. The gumnata are solid and caseo-fibrous; they
are ordinarily rounded, lying flat on the dura mater, or pene-
trating deeply into it. They are often surrounded by in-
flammatory disturbance giving rise to haemorrhagic products,
and causing at a later period adhesions to the pia mater.
They may involve and cause obliteration in arteries.

It is of course impossible to say to which of these con-
ditions the lesion in the present case is to be referred, but

to one or other of them I should think the symptoms were probably due. As a result of the thickening of the dura mater there would be pressure upon the pons Varolii and spinal cord—a pressure which would not necessarily be equal at all parts, and thus the escape of some nerves, whilst others suffered, would be explained. There was evidence of compression of the fifth and seventh pairs of nerves, and of the sixth on the right side. There was no distinct proof that the glosso-pharyngeal was involved. The difficulty of swallowing might of course be dependent upon lesion of the pharyngeal plexus formed by branches from the glosso-pharyngeal and vagus, or of the superior and external laryngeal branches of the latter. But, on the one hand, the preservation of the sense of taste; and, on the other, the fact that, neither in the circulation nor in the stomach, were there any signs of disturbed action of the vagus, seems to make it more probable that the impediment to swallowing was due to other sources. The partially paralysed state of the palate may probably have been sufficient to cause this embarrassment, coupled as it was with impaired sensibility of the upper part of the pharynx, parts supplied by branches from Meckel's ganglion, itself an emanation from the superior maxillary division of the fifth, to which the portio dura contributes some motor fibres in the Vidian nerve. The ninth pair of nerves showed no evidence of lesion.

The paralysis of the diaphragm and of the intercostal muscles was probably dependent upon compression of the phrenic, and of the anterior roots of certain of the nerves in the dorsal region of the cord.

The lapse of time (fourteen years) since this man incurred syphilis, and the absence of any of the more usual signs of suffering from the constitutional affection, may be thought by some to throw great doubt upon the supposition of this attack being referable to that disorder. But I have frequently seen, and have described several cases (Vide 'Lancet,' February and March 1873) in which nervous disorders of well marked syphilitic character occurred in persons who had never previously presented any symptoms of constitutional syphilis, and in whom an interval of many years had elapsed since the primary infection. This is a point to which attention has been drawn by more than one writer.

It may be interesting here to refer to a case which I have lately met with, occurring in the practice of Prof. Wagner, which presents a striking similarity to that which I have
described, and in which, moreover, more palpable symptoms of constitutional syphilis were present at the time of the patient's being attacked by general paralysis, and afforded the corroborative evidence which is absent in my case. The case is related by Dr. O. Bayer,* and is called 'Cure of an Acute Ascending Spinal Paralysis under Anti-syphilitic Treatment.'

The patient, a military man, aged 35, had contracted a chancre in 1862, which was followed five months afterwards by a throat affection which subsided, after many weeks' duration, under non-specific treatment. A year later he had a papulo-pustular eruption, for which he was rubbed in. Ere two years had passed he had a recurrence of throat affection, which was treated exclusively by mercury. In August 1866 he complained of stiffness in the neck, and at the same time there appeared, in the neighbourhood of the spinous processes of the last cervical and first dorsal vertebrae, a swelling at first the size of a groschen, and after a fortnight as big as a two-thaler piece, tolerably flat and compact to the feel, with the skin over it a little reddened. The part was slightly sensitive to movements of the head, and still more so to pressure with the finger. The patient could not explain the origin of the swelling, which had been treated as rheumatic. About six months after its appearance the swelling began to give way and decrease in size, and in May 1867 had entirely disappeared.

In the meantime, in December 1866, an elevation had formed on the left parietal bone, near its anterior angle. It was as big as a groschen at first, somewhat flat and compact, and tender on pressure. It then increased to the size of a thaler, and remained unchanged at the beginning of the severe illness to be described.

On May 5, 1867, the patient, in mounting his horse, perceived a weight and clumsiness in his legs, although when he left his dwelling-place he had felt nothing of this. During his ride of one hour he felt as usual. In dismounting there was again heaviness and uncertainty in both legs, and now he retained this feeling; nevertheless the patient took a short walk, in which he was aware of more weariness than was common to him. Next morning the condition was somewhat worse; the legs could not be lifted out of bed without trouble, which was still more marked in standing and in

walking, but especially in ascending stairs. His steps were taken as though with very tired legs. This weakness increased in the course of the afternoon, so that the patient mostly remained lying on the sofa. In spite of a quiet night, on the next morning the legs could only be lifted with striking difficulty, and it required especial exertion for him to raise himself in bed. To leave the bed was impossible; in other respects the patient had nothing to complain of. During the following day nearly complete paralysis of both legs declared itself. To rise in bed was impossible; active movement of the upper extremities was more and more difficult; till, finally, the arms could only be slightly lifted, and with the hands also light objects could not be laid hold of. Urination was laborious, and was obliged to be aided by pressure upon the hypogastric region. The action of the bowels was retarded, and required purgatives; the appetite vanished. Otherwise nothing abnormal was evident, and neither cramps nor spontaneous contractions were to be observed. Indifferent treatment only had been employed.

At this stage the patient was transferred to the treatment of Prof. Wagner. The results of the objective examination were as follows:—

Body long, of strong bone build; considerable emaciation; skin very pale.

Temperature, pulse, and respiration normal.

In different parts of the body the following abnormalities:—

The swelling on the head as above described.

Deep clefts in the tonsils, especially the left.

On the chest, belly, and spine small white scars, which the patient referred to the previously existing exanthem. The upper extremities were only moved with much trouble and feebly, at the best, the fingers being scarcely able to grasp anything. The sensibility was not impaired.

The extended legs could not be lifted. In the knees feeble flexion was alone possible, in the left somewhat better than the right; yet the least pressure prevented this. Both feet could be moved a little to and fro. The toes of the right foot were capable of being moved very slightly, those of the left actively. The sensibility in the foot, leg, and thigh was considerably weakened; in the left somewhat less than in the right.

The patient was not in a condition to raise himself in bed.

All passive movements could be readily accomplished.
The sensibility of the skin in parts of the body not especially named remained normal.

The conditions represented remained stationary under treatment for a week and a half.

From May 22 the sensibility in both lower extremities improved gradually, but evidently from one day to another; the power of movement of the toes also increased. At the same time the appetite improved.

In the night of May 30—31 the patient experienced severe cardialgia, which disappeared on the evening of the 31st; the loss of appetite connected therewith vanished after some days, whilst the swelling on the head manifestly lessened.

The paralytic phenomena in the legs and arms diminished uninterruptedly; the urine also could be slightly discharged; the sensibility, meanwhile, had become pretty normal. On June 5 numerous hæmorrhages of the size of lentils showed themselves on the toes of both feet. No trace of salivation. Mercurial eczema was apparent in different places after some days.

On June 10 the hæmorrhages had to a great extent disappeared. The improvement of the paralysis had made such progress up to this day that the patient could lift his arms, and also exercise some pressure with his hands. He could eat without assistance, and could raise himself, although with some trouble, in bed. The knees could be bent with some strength; the bladder and rectum acted spontaneously; nothing more was to be seen of the swelling of the head.

After many fruitless attempts, on June 21 the patient stood freely on the ground; with the aid of an attendant he could also move a little forwards.

A week later the patient walked briskly with the help of a stick.

On July 1 he no longer required the stick, and in the course of this month acquired strength so rapidly that, at the end of it, he had completely recovered.

The treatment consisted in the rubbing in of mercurial ointment. When the hæmorrhages appeared the body was washed, and the inunction was stopped for a few days. Iodide of potassium was likewise given in the beginning, and stopped on account of disorder of the stomach, which was treated with bismuth, soda, and morphia. In the first two and a half weeks in June short applications of faradization were also made daily to the muscles of both lower extremities.

Strong, but easily digested, nourishment was allowed from
Dr. Cayley's Case of Hemoptysis.

the commencement. Careful rinsing of the mouth was especially observed during the inunction.

In this case the affection did not involve so high a portion of the cerebro-spinal axis as in mine; but if we imagine an extension upwards so as to involve the dura mater in relation with the pons Varolii, we shall find, I think, the key to all the additional symptoms which occurred in the patient whom I now present to the Society.

XVII.—A Fatal Case of Hemoptysis. By W. Cayley, M.D. Read March 27, 1874.

A., a police constable, age 20, was admitted into the Middlesex Hospital under my care, on Oct. 14, 1873.

His family history, as far as it could be ascertained, was this:—His father was living and a strong healthy man; his mother was also alive, but was very sickly and delicate. She was not, however, subject to any chest affection. He had lost a brother and two sisters; one of the latter had died of consumption, as also had two of his uncles.

He stated that he himself had always been strong and healthy. He had been in the police force for two years, and had never been disabled from performing his duty. These statements were fully borne out by his appearance, which was that of a well-made healthy young man in the prime of youth.

Nine days before his admission he received a blow with the fist below the right breast, from a drunken woman whom he was taking into custody. He did not attach much importance to this at the time, but afterwards he complained of pain in the site of the injury.

The same evening, while eating his supper, he suddenly felt sick and then coughed up about half-a-pint of florid blood. He had no return of the haemoptysis for five days, nor did he suffer from cough; but during this time he felt unwell, complained of pain at the seat of the blow, and was unable to return to his duty.

Two days before his admission, five days after the first attack, the haemoptysis returned, but less profusely, and it continued to recur in small quantities until his admission.

On admission the patient, though, as I have stated, a
healthy looking young man, was in a depressed condition. On
making any exertion, such as sitting up in bed, he coughed
up small quantities of bright frothy blood. He complained
much of pain at the site of the blow, but a careful examina-
tion failed to detect any injury either of the soft tissues or
of the ribs.

His pulse was 108; respirations, 32; temperature, 99½°. There
was slight impairment of resonance under the right clavicle; and here a little loose crepitation, due no doubt to
the presence of blood in the bronchial tubes, was audible.
Over the lower part of the chest in front, and in the flank,
a fine grazing friction was heard. The expansion of this
side of the chest was somewhat diminished, but there was
good resonance down to the base of the lung.

He continued to bring up small quantities of blood for
some hours, and then the haemoptysis ceased.

Oct. 15.—When I saw him in the middle of the day he
was apparently better, his pulse was only 76, his temperature
99°. He had brought up no more blood; but when I some-
what incautiously made him sit up in bed to examine the
back of his chest he again expectorated a small quantity; this,
however, now was black and clotted, as if it had been lying
for some time in the bronchial tubes. He still complained of
pain in the same part, and the physical signs were not mate-
rially altered.

He continued much in the same condition, still spitting up
at intervals small quantities of blood, but not in sufficient
amount to cause alarm till Oct. 17, when he became worse.
His pulse rose to 100, his temperature to 103°, and he com-
plained much of pain and oppression of breathing. There
was now decided dulness at the base of the right lung.

18.—The following note was made:—Pulse, 130; resp., 60;
temp., 104½°. He is very prostrate, and sweating profusely.
Tongue coated, red at tips and edges. He has expectorated
a little blood in the night, but not since. There is dulness
and feeble tubular breathing over the lower half of the right
back.

At 6 p.m. his pulse was 124; temp., 105°. At 9 p.m. pulse,
132; resp., 72; temp., 104½°. He was still perspiring pro-
fusely, and occasionally spitting up small quantities of blood.

Oct. 19.—In the morning his pulse was 132; resp.,
48; temp., 101½°. He felt much better, the perspirations
had ceased, but he still continued to expectorate small quan-
tities of blood. At 1 p.m. the haemoptysis returned with
Dr. Cayley's Case of Hæmoptysis.

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great violence; he brought up torrents of bright red blood; he became blanched, livid, his respiration gasping, and he died soon after 9 p.m.

A post mortem examination showed the following appearances:—

There was no external mark of injury and no fracture of the ribs.

On opening the chest an abscess was discovered situated in the attachment of the diaphragm to the cartilages of the ribs on the right side in front. This abscess, which held about half-an-ounce of creamy pus, was invested by the pleura, but did not actually communicate with its cavity, and the cartilages of the ribs were not laid bare by it. The right pleural cavity contained two pints of fluid, of which the upper part was nearly clear while the lower consisted of thick pus. The lower lobe of the right lung was carniﬁed, and the pleural surfaces were smeared with soft yellow lymph. There was an old ﬁbrous band of adhesion at the apex of the right lung, and embedded in the upper lobe near the apex was an opaque yellow caseous nodule, evidently of old date, about the size of a hazel-nut; round this the lung was deeply reddened by inﬁltration of blood. Scattered through the upper and middle lobes were groups of very minute grey miliary granules of quite recent origin. The bronchial tubes on both sides contained some clotted blood. The left lung was quite normal.

There were old ﬁbrous adhesions between the liver, spleen, diaphragm, and the surrounding parts. Scattered over the peritoneal surface of the liver were several minute grey tubercle granulations, and in the adhesions between the spleen and stomach was a small circumscribed collection of pus. The other viscera were quite healthy.

Such was the history of this case. I have not dwelt on the treatment, for unfortunately it exercised no inﬂuence on the course of the disease. When ﬁrst admitted he was given large doses of ergot, afterwards sulphuric acid and tinure of opium. When the hæmoptysis again became profuse, recourse was again had to ergot, leeches were applied to the side, he was kept in a recumbent position, and ice was freely administered. But on looking back at the case I think the patient would have had a better chance of being snatched from impending death if he had been bled freely from the arm, and if the effusion had been sucked out of the pleura by the aspirator as soon as it had formed. For the immediate
cause of his death seemed to be the blocking up of his bronchial tubes with blood, assisted by the embarrassment of respiration produced by the pleuritic effusion.

Having, however, come to the conclusion that the case was one of acute tuberculosis, I felt disinclined to any very heroic remedies; and when the patient was bringing up blood in such quantities as even to give rise among the spectators to the suspicion that he might have ruptured an aneurysm, paracentesis for a moderate pleural effusion seemed altogether beside the mark. Neither do I think any mode of treatment could have been of more than temporary benefit, for the eruption of miliary tubercles in the peritoneum shows that dissemination had already begun, and the patient would soon have succumbed to acute miliary tuberculosis.

A more profitable subject for speculation is, I think, the connexion between these different lesions. And not only are many points of clinical and pathological interest involved in this inquiry, but also a very grave medico-legal question.

Several plausible hypotheses may be put forward. First we may consider that the patient—though of the tubercular diathesis, as shown by his family history and the marks of obsolete disease seen in his lungs after death—was in perfect health at the time he received the blow, but that he possessed that peculiar vulnerable condition of the bronchial capillaries to which Niemeyer ascribes the production of bronchial hemorrhages. The concussion of the blow gave rise under these circumstances to the attack of hæmoptysis, which came on shortly after its infliction. The infiltration of blood into the tissues of the lung caused more or less inflammatory irritation, and this determined the eruption of the tubercles, and these the subsequent hæmoptysis. The abscess and the pleurisy were also the effect of the blow, but did not stand in any causal connexion with the tuberculosis.

Or we may adopt another hypothesis. We may consider, as in the former case, that the first attack of hæmoptysis was caused by the concussion of the blow. But the blow also caused the abscess; this, by its close contiguity, set up the pleurisy; the pleurisy by infection caused the tuberculosis, and this the subsequent hæmoptysis.

It is now, I believe, generally acknowledged that pleurisy, especially when the effusion is prevalent and allowed to remain long in the chest, very frequently excites tuberculosis, and that those only too common cases of pleurisy in
which the patient ultimately dies tubercular, and which formerly were considered to be tubercular at their outset, are so no longer. The tuberculosis in such cases is now looked upon as secondary to the pleurisy.

But if the blow caused the abscess, the abscess the pleurisy, the pleurisy the tuberculosis, the tuberculosis the haemoptysis, and the haemoptysis the death of the patient, it is clear that, though the chain is a longer one, the person who gave the blow is legally responsible for all the secondary consequences.*

But though chronic suppurative pleurisy is a frequent cause of tuberculosis, I am not aware that this effect has ever been ascribed to acute pleurisy, and it may, I think, be argued with equal plausibility that these different lesions stand in causal connexion with one another; that the patient, who clearly had the tubercular diathesis, happened to be attacked by tuberculosis and haemoptysis at the time when he accidentally received a blow; and that though the blow may have caused the abscess and the pleurisy, these lesions had nothing to do with the tuberculosis and the haemoptysis, though they may have exercised an unfavourable influence on the progress of the disease.

XVIII.—A Case of Blood-cyst of the Hand. By J. Warrington Haward. Read March 27, 1874.

JOSEPH K., aged 43, was admitted into St. George's Hospital, under the care of Mr. H. Lee, Oct. 30, 1873. He was a labourer, married, and in good health. His father and a sister died of dropsy. There was no history of cancer in the family.

Three years before admission he began to feel a tingling sensation in the right forefinger, for which he could assign no reason. This continued, with varying intensity, for a year, at the end of which time he noticed swelling of the metacarpo-phalangeal joint of the right thumb. This swelling diminished, but did not disappear, when the joint was kept at rest, and remained in much the same state till August 1873, when it began to increase rapidly, and to spread over the ball of the thumb; it also became more

* The Coroner's jury brought in a verdict of death from natural causes.
painful. He states that about Oct. 15 a surgeon punctured
the tumour, and that only blood escaped.

When admitted to the hospital, there was a tumour the
size of a hen's egg upon the palmar aspect of the right hand,
extending from the wrist to the middle of the palm, and
covering the ball of the thumb. The tumour was firm, and
the skin over it very tense and red; it gave an indistinct
sense of fluctuation. There was considerable oedema of the
hand, and the movements of the thumb, and first and middle
finger, were much restricted. A dull throbbing pain ex-
tended from the tumour up the radial side of the fore-arm.

Nov. 6.—Ether was administered, and the hand and fore-
arm bandaged after Esmarch's method. Mr. Lee then made
an incision into the tumour, and turned out a fleshy mass of
somewhat gelatinous appearance, containing in its interior a
number of smaller masses of similar material and a consider-
able quantity of old blood-clot. There remained in the
wound a kind of capsule, or wall of condensed tissue, the
removal of which was not attempted. On removing the
restraining pressure from the limb there was free general
oozing of blood from the wound, which was controlled by the
application of lint soaked in solution of sulphate of copper,
and by bandaging.

The membrane lining the wound gradually sloughed away
and was removed, and the part granulated and was nearly
healed when he went home on Dec. 3. He had then consi-
derably regained the use of his fingers.

For the above notes I am chiefly indebted to Mr. Howard,
clinical clerk to Mr. Henry Lee. I was requested by Mr.
Lee to examine the tumour, and found it to consist of a
somewhat oval-shaped mass of a pale yellow colour, semi-
transparent, of about the consistence of gelatine. In the
centre of this tumour was a cavity containing numerous
small, round, and irregular-shaped masses of similar mate-
rial, besides some old blood-clot. The wall enclosing this
cavity was from 1/4 to 3/4ths of an inch thick, and from its
inner surface were irregular projections. The microscope
showed the walls of the cyst and the loose masses contained
therein to be composed of similar elements, viz. spindle-
shaped cells with oval nuclei, closely placed in a very scanty
intercellular material.

The clinical histories of cases of blood-cyst exhibit such
various characters, that the prognosis in this disease must at
present always be considered doubtful. For whereas there
are some cases wherein the removal of the cyst has resulted in complete recovery, there are, on the other hand, others in which operation has been followed by the recurrence of disease presenting decidedly malignant characters.

These differences probably depend chiefly on the constitution of the cyst-wall.

In the first place many cysts containing blood may, of course, have their origin in simple serous cysts, into which haemorrhage has taken place, as haematoceles, or the case related by Mr. Flower.* Secondly, such collections may result from violence, causing the rupture of a blood-vessel. Such probably was the history of the case related by Dr. Buchanan,† in which there was a simple collection of blood among the muscles of the calf.

Thirdly, such extravasations may be owing to the spontaneous rupture of vessels from vascular disease; as in the case related by Dr. Payne,‡ of a blood tumour among the muscles of the thigh, in a man who died with heart disease, meningeal apoplexy, and haemorrhagic infarcta in the spleen.

Fourthly, there is another class of such cases which has a close association with malignant disease, and is apt to result unfavourably; and these are they which have their walls composed of sarcomatous tissue, such as the case related above.

A case is related by Mr. Lawson§ of a blood-cyst of the thigh, for which the limb was amputated. The tumour was examined by Mr. Arnott, who found it to consist of material resembling that of a recurrent fibroid tumour. Four years afterwards, recurrence took place in the stump, and the patient died with cancer of the lung and mediastinal glands. Mr. Lawson removed another similar tumour, which was examined by Mr. Arnott, in 1872.|| In this case there was no recurrence of the disease ten months afterwards; but in the same paper Mr. Lawson mentions the case of a lady in whom the puncture of a blood-cyst on the back was followed by the growth of medullary cancer from the wound.

A remarkable tumour was also exhibited at the Pathological Society last year by Mr. Holmes.¶ This was a large

* 'Pathological Transactions,' vol. xi. p. 237.
† Ibid. vol. vii. p. 363.
‡ Ibid. vol. xxiii. p. 237.
§ Ibid. vol. xviii. p. 272; xxiii. p. 239.
|| Ibid. vol. xxiv. p. 207.
¶ Ibid. vol. xxiv. 213.
blood tumour removed from the leg, and consisted of a thin cyst containing a large quantity of blood-clot. An examination of this by Messrs. Marcus Beek and Henry Arnott revealed, however, a thin layer of sarcomatous tissue in the cyst-wall—a condition which would certainly not have been suspected from a naked-eye examination. This patient recovered, the operation having been performed in March 1873.

The only other case to which I will allude is that of a blood-cyst in the museum of St. George's Hospital,* which was removed from the groin of a young woman by Mr. Hewett. It is a suggestive fact that from this patient a fibro-plastic tumour had been removed from the poplitial space of the same limb four and a half years before.

The practical teaching, I apprehend, of these cases therefore is, that in operating on cases of blood-tumours we should be careful to remove the growth very completely, and even then should give a guarded prognosis.

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XIX.—*Description of a Bracketed Splint for Excisions of the Knee, or for Compound Fractures.* By John H. Packard, M.D. Communicated by G. W. Callender. Read March 27, 1874.

ONE great point (perhaps the great point) to be aimed at after excisions of the knee is to keep the limb at absolute rest until the union of the sawn surfaces of the bone is an accomplished fact. In order to secure this, provision must be made for changing the dressings without removing the limb from the splint. It is very desirable that the discharge, which is sometimes very profuse, should be prevented from soiling the splint, and therefore that access should be had, not merely to the wound, but to the whole circumference of the limb at the part operated on. This latter point is not attained in any of the forms of apparatus of which descriptions have met my eye, although most of them are complicated and expensive, and require a skilful mechanic for their construction.

* See 'Pathol. Trans.' vol. xvii. p. 19.
I beg therefore to call the attention of British surgeons to a splint devised by me in 1869, and since used repeatedly, not only by myself, but by other gentlemen in this city. A brief notice of it was published in the Proceedings of the College of Physicians of Philadelphia ('American Journal of the Medical Sciences' for July, 1870), with a woodcut. I have, however, modified it to advantage since that time.

The essential idea of this splint is that of a solid support for the whole limb, the middle portion, or that corresponding to the knee, being removable, the other portions being firmly bracketed together. I trust that its real simplicity will not be lost sight of in the unavoidable length of my description. I have had it made as follows:

A large sheet of coarse paper is placed beneath the diseased limb, an outline of which is traced upon it, and the part corresponding to the knee marked. The outline should be a little larger than that of the limb; by it is now cut out a piece of inch board. On the outer side the outline should extend up as far as the trochanter, on the inner side as far as
the perineum; the fold of the buttock should also be traced, to form the line of the upper edge of the splint.

When the knee is stiffly flexed by disease, the splint must of course be made upon measurements, instead of upon an outline traced in the manner mentioned. The former plan is more difficult, but with care the proper dimensions can be accurately obtained. If the surgeon has to entrust the handwork to a carpenter, it is much better for him to make the outline himself for the latter's guidance, and not trust to mere directions.

The piece of board thus cut out is slightly hollowed from side to side, so as to afford an extremely shallow trough, as it were, for the limb to rest in. Its upper end is bevelled off so that there shall be no edge to press against the skin, and a somewhat deep hollow may be made corresponding to the heel.

To make the removable portion, the part of the splint corresponding to the knee may either be simply sawed out, the saw being carried obliquely, as in Fig. 2, a, or it may be sawed out carrying the saw perpendicularly, and the sawn edges then fitted with a tongue and groove, as in Fig. 2, b.

At the part of the splint just below where the heel will come, a hole may be mortised lengthwise, to receive the tenon of a foot-piece, to be keyed at any point by a wooden pin.

Next, as to the brackets. Any blacksmith can make them in a short time; they should be in the form of an arch (see Fig. 4), so shaped as to admit of free manipulation beneath them, and fastened each by three screws to the side of the thick part of the wooden splint above, and by three to the side of the leg part below. Each bracket should be about nine inches in height, and it is well to have them flared out a little.

A third iron arch, to go over the foot, being screwed at either side of the lower end of the wooden splint, may be used, but is not absolutely necessary, as the foot-piece ought to be high enough and steady enough to keep off the pressure of the bed-clothes.

In order to keep the middle or knee portion firmly in place when the splint is applied, a small bolt or a hook should be placed on either end of its lower face, with a catch on each of the other portions.

Strips of soft thick leather are next tacked to each portion of the splint, one strip at either side, as in Fig. 3. They may either be pierced for a lacing, or have pairs of tapes
attached to tie across. In one case I had straps and buckles instead of tapes; but I think the tapes are better.

Before operating, the surgeon sees that each portion of the splint is duly padded with cotton-battling (cotton-wool) or with folded flannel. I have had the padding so graduated as to provide for the due angulation of the limb, as this gives the patient subsequently a much less limping gait than if the bones are allowed to unite in a perfectly straight line. Upon the middle or knee portion of the splint, on the padding, is laid crosswise a strip of oiled silk of proper width, and on this the lint soaked in laudanum, lead water and laudanum, carbolic acid solution, or whatever application may be preferred.

The operation having been concluded, the limb is laid upon the splint, the foot secured to the foot-piece, the dressings adjusted so as to cover the knee, and then the leathers are brought up on either side so as to keep the limb steady. I have generally added small pasteboard or stiff leather splints at the front of the leg and thigh, over which the fastenings of the side pieces are secured. The foot may be raised to any desired height by placing pillows or other supports beneath the apparatus. The splint, as applied, is seen in Fig. 4.

In order to change the dressings, let the foot be raised and firmly supported, so that access is easily had to the under side of the splint. The tapes or other fastenings of the middle part of the splint are next loosened, and the catches undone. Next, the surgeon takes hold of the oiled silk at the inner side of the knee with one hand, while with the other he pulls gently on the movable part of the splint, an assistant if necessary aiding him by gently pushing on its inner side. Thus the shell, as it were, is carefully and steadily removed, leaving the dressings exposed, so that they can be taken off, and fresh ones applied; the tapes are then passed under the knee, the shell accurately adapted, drawn and pushed until it slides into its place, when the catches are fastened, and the tapes tied above as before.

The removal and replacement of the shelf are aided materially, when the tongue and groove arrangement is adopted, by oiling the surfaces which come in contact. It is rendered much more difficult if either the discharges from the wound or the superabundant liquid of the dressing should flow down and soak into the wood, causing it to swell. Hence it is important to have the removable portion fully wide enough, that is, a sufficient space between the thigh and leg portions of the splint.
Mr. Callender on the Treatment of Neuralgia.

The application of this apparatus to compound fractures of the lower extremity needs no special explanation. Its cost is very small—not more than two or three dollars—and very many surgeons would find no difficulty in making it themselves, except the brackets, which any blacksmith can hammer out at short notice. By the addition of outriggers it can be swung, if thought desirable, in the ordinary way.


In this case a very painful affection was treated by stretching the median nerve.

W. K., æt. 41, was admitted into Darker Ward, St. Bartholomew’s Hospital, Jan. 20, 1874. He was a bricklayer by occupation, was married, and had three children. His family history was good.

I am indebted to Mr. Bryant, under whose care he lay in Guy’s Hospital, for the following statement respecting the case:—In the year 1867 he fell and sustained a compound fracture at the wrist, and a second fracture about the middle of the fore-arm. Twelve months after the accident several pieces of bone were removed, and eight months later the diseased parts were amputated (in St. George’s Hospital). The cicatrix and surrounding parts became very tender, and a discharge continued. In 1869 the tenderness was especially on the inner and upper and on the outer and lower sides. He felt the index, middle and ring fingers, and complained of an itching sensation which he referred to the palm of the hand; the stump jumped, as he termed it, more especially at night, the pain being then excessive and extending up towards the back of the ear and to the corner of the left eye. On July 13 an amputation was practised at the elbow-joint. On Feb. 7, 1871, Mr. Bryant removed a neuroma from the stump, and on July 2, 1872, he cut out a portion of the median nerve.

During the year 1873 the patient again applied for admission into Guy’s Hospital, but appears to have migrated to St. Bartholomew’s, where he was seen in consultation, but it was not thought feasible to relieve him by operation. The trouble in his arm becoming greater, he again came up from the country, and was admitted into one of my wards.
His general health was fairly good, but he was worn from want of sleep, his nights being much disturbed through the pain from which he suffered, despite the free use of sedatives, injected or taken in the ordinary way. P. 66, T. 98-5°, R. 20. Urine natural. The left arm, from the shoulder to the elbow (where amputation had been performed), was glazed, of a dusky colour, and was cold. It was the seat of pain in the course of the median nerve, and this pain was increased on pressing the end of the nerve, which felt swollen and was hard. In the course proper to this nerve were two scars, at which points portions of the nerve had been removed by operation between the present end of the nerve and the cicatrix of the amputation wound. In addition to these symptoms, which were referable directly to the condition of the peripheral portion of the nerve, the patient stated that he had suffered from a sense of tingling in the entire arm, a sense which for some months had been superseded by that of intense pain (causalgia). The tingling and aching had extended to the neck, and he further complained of these symptoms as infesting the side of the face, and the back and side of the head—about the distribution, evidently, of the branches of the cervical plexus; the action of the diaphragm was not interfered with, but when the median nerve in the stump was suddenly struck he had increased pain about the shoulder, and the muscles (more especially the pectorals) became violently cramped.

Two sets of symptoms were thus indicated: the first, or peripheral, referring to an altered condition of the median nerve in the stump; the second, or central, pointing to the irritation of the spinal cord,* caused probably by ascending hyperplastic neuritis; the reflex symptoms thus induced showing themselves in the pain diffused about the neck, the side of the head and face, in the impaired nutrition of the arm, in the tingling and hyperesthesia, and lastly in the cramp of the pectoral and other shoulder-muscles.

Premising that the patient was free from rheumatism,

* Such an association of central symptoms, with an altered condition of a nerve-trunk, has been recognised since it has been shown by the cases recorded by Flaubert and by Le Bref that inflammation of the spinal cord may follow an injury to the brachial plexus, and since the more recent researches of Dr. Brown-Sequard have illustrated other changes produced in the spinal cord by the peripheral irritation of nerve-trunks.

and that general treatment and local applications and injec-
tions had failed to relieve him, and apart from re-amputation
of the arm—a method of treatment from which no good result
could now be anticipated, that is if we were to judge from
past experience—the following operations suggested them-

selves for consideration:—

(1) Excision of the end of the nerve.
(2) Division of the nerve-trunk.

Both of these were counter-indicated by the evidence of
neuritis complicated with irritation of the spinal cord. There
were no signs to enable us to judge where the nerve should be
divided, so as to cut it above the range of the neuritis, and
there is no evidence on record to show that the neuritis by
such a procedure could be prevented from being re-kindled
in the central end of the nerve.*

(3) Handling and stretching the affected nerve.

In the year 1870 I suggested † an operation for cutting
away all thickened tissues from around a nerve, so as to free
it from pressure, in cases in which great pain persists in con-
sequence of compression or irritation of a large nerve-trunk,
such as the ulnar; and I then showed, by illustrative cases,
that there was no risk of hurting the nerve by detaching it
from its surroundings. Since then I have had no oppor-
tunity, however, of treating an irritated nerve-trunk until
this case came under my notice; and now, in deciding to try
the effect of isolating and stretching the median nerve, I had
the advantage of the experiences obtained by Billroth and
Nussbaum. ‡

* When neuritis (hyperplastic) or degeneration (Wallerian) affects a nerve-
trunk, and the limits of the affection are unknown, the relief obtained has been for
the most part but partial, as in Dr. Nott's case of excision of part of the great
ischiatic near its point of exit from the pelvis, and in the very interesting case
reported by Drs. Sands and Seguin, in which portions were cut out from the bra-
chiac plexus, the nerves of which were found matted together, and the condition of
neuritis probably extended further on the central side, as the success of the opera-
tion was not radical although the patient was much relieved. Cases in which
division of a nerve-trunk has been followed by cessation of the neuralgia are
those from which symptoms of neuritis are absent or are distinctly localised.
Nerves entangled in cicatrices or in fractures, or compressed by tumours, have
often been divided, or portions have been cut out, and complete relief has followed
the operation. See Drs. Sands and Seguin, 'Archives of Scientific and Practical
Medicine,' No. 1, Jan. 1873; Mitchell 'On the Nerves,' p. 285; Schmidt's
'Jahrbiicher,' Bd. exxxv. p. 220, exxii. p. 218, exxiii. p. 298; Dr. E. Graf, 'Wiener
Med. Wochenschrift,' Ap. 19, 1873. Dr. Nott's case was one of traumatic neuralgia
after gunshot wound. Three amputations preceded the removal of the portion of
nerve.
‡ Professor Billroth reports a case ('Archiv für Klinische Chirurgie,' Wien, Bd.
Mr. Callender on the Treatment of Neuralgia.

On Feb. 7 the patient was removed to the operating theatre, and was placed under the influence of chloroform. An incision was then made over the nerve, and this when exposed was detached from the surrounding tissues, with the exception of its extremity, which was left as fixed after the last operation. The nerve-trunk was considerably increased in size. After its isolation it was seized with a vulsellum, and was then drawn down from the plexus. I used very considerable force in thus stretching it, but it was firmly held, and yielded but little.

The nerve, which had been but slightly bruised by the grip of the instrument, was then carefully relaid in the wound, which was washed with carbolic acid lotion, drained, and closed with silver sutures.

Feb. 8.—The patient had slept soundly without morphia, and had been entirely free from pain since the operation. P. 76, T. 98°4', R. 18.

He convalesced without an unfavourable symptom, and on Feb. 13 was up and about the ward. The night of the 17th, however, he slept badly, shivered, and felt sick, his temperature rising from 98°6' to 101°5', and the following day to 103°. These symptoms ushered in an attack of erysipelas, which, as it had no direct bearing on the results of the operation, need not be further referred to.

April 6.—He was free from pain, and the arm had lost its dusky, purplish colour, and its ordinary nutrition seemed to be re-established.

The patient is now (two months after the operation) well, and following an ordinary occupation.

In this, as in other cases where we are largely dependent upon the statement of the patient for our knowledge of his symptoms, it is necessary to consider how far such state-
ments can be trusted. After the amputation performed at Guy's Hospital, it appears that 'private matters being urgent, the stump is so far recovered that he has gone out on his own responsibility,' and it further appears that in 1873 his conduct after accepting an order for admission into the hospital was not satisfactory. It is certain, however, that he submitted to two amputations, and to three later operations, because of the pain of which he complained. And of his symptoms, the rigid cramp of his pectoral and other muscles, and the evidence of mal-nutrition of the stump, were such as could not be simulated, and the same may be said of the improved nutrition of the parts which followed the last operation. Nor does it seem to me likely that this man could have so thought out the symptoms he described as to fit them in, as they did from first to last, with those characteristic of neuritis causing spinal-cord irritation. These are reasons which lead me to conclude, as I had done from general observation of his conduct, that his complaints of pain were truthful.

This treatment of certain forms of neuralgia by nerve-stretching deserves further trial. At present the mode of action is uncertain. The stretching may act by paralysing the nerve, by breaking down adhesions and constrictions along its track, or (as Dr. Nussbaun suggests) by altering the relations of the nerve-fibres and by improving their nutrition. It is, however, clear that the operation may be practised without fear of inducing local or other serious troubles.


J.S., set. 45, was admitted into Darker Ward, St. Bartholomew's Hospital, Jan. 8, 1874.

In September 1872 he punctured the little finger of the left hand with a splinter of bone, while cutting decomposed meat. This was followed by thecal abscess and diffuse suppuration along the flexor tendons, with much inflammation of the hand and arm. After free incisions in the hand and fore-arm, letting out large quantities of matter, he gradually recovered, and after staying rather more than three months
in St. George's Hospital, he left with considerable constriction of the flexor tendons. Gradually the contracted fingers, and then the entire hand, became the seat of continued pain, at first tingling, then burning in character. With this there was great tenderness on touching the parts, even in the lightest way.

While thus suffering he had been under observation as an out-patient, first at St. George's, and subsequently at St. Bartholomew's Hospital, and every kind of treatment short of an amputation or other cutting operation had been tried for him. At last application was made to me to admit him, in order that the arm might be amputated. In avoiding this operation I met with little encouragement, as it was considered that nothing short of this procedure could relieve him, and doubts were entertained as to whether even this operation was likely to have a satisfactory issue.

Jan. 23.—On carefully looking into the symptoms of the case it was evident that we had to deal with a trouble which was limited to a peripheral affection of certain nerves. It arose from the entanglement of nerve-fibres in the contracted and indurated tissues of the ulnar side of the hand, and of those involving the median branch to the ring-finger, and probably the fibres to the end of the index-finger. By sympathy with these the pain was diffused over the entire hand, and was followed up in the course of the ulnar and median trunks. The implication of the ulnar was below the separation of the deep muscular branch, as there was no wasting of the muscles of the ball of the thumb. There was no evidence of central irritation, no diffusion of pain (both pain and malnutrition being limited to the area supplied by the affected nerves), no cramp or stiffness of muscles, and no affection, more especially, of the muscles supplied by the median—those of the thumb, for example. The hand was cold, of a dusky colour, the surface of the skin was glazed and devoid of hairs, the opposite (right) hand being freely covered with them on its dorsal aspect. But this condition of impaired nutrition was limited to the hand, not ascending beyond the wrist, and being rather less marked on the back of the thumb than elsewhere.

With such symptoms it seemed fair to infer that the peripheral irritation of certain nerve-fibres was the sole cause of the patient's intense suffering, and the nerves affected were most probably those lying in the parts mentioned as having borne the stress of the suppuration, leading to the
subsequent contraction and induration. These considerations seemed to justify an attempt to relieve the patient by removing the diseased structures, which was effected by amputating the last phalanx of the index-finger, and by cutting away the little and ring-fingers, with parts of the corresponding metacarpal bones. It seemed best thus to sweep away the diseased textures rather than to attempt the section of various small nerves—operations difficult to practice in the midst of thickened tissues, and which if performed would have left certain parts deprived of all nerve supply, and consequently liable to a persistence of the mal-nutrition of the tissues with its prospective discomforts.

As the result of the amputation the patient was at once relieved of his pain, and is now, more than three months later, in good health. The part of the hand left to him is useful, nutrition is natural, and it may be noticed that the hairs have again appeared on the dorsal surface.

It will be observed that, compared with the case of neuralgia in a stump, the neuralgic condition was here essentially different. In the first case the affection had become central, there being distinct symptoms of spinal cord irritation; in the second it remained purely peripheral. The treatment required was manifestly that of amputation in the peripheral affection, whilst it was equally clear that a further amputation offered no certainty of relief in the instance in which central mischief had been established.

My object in bringing these cases before the Society is to direct attention to the desirability of very carefully sifting the symptoms in cases of neuralgia before determining upon a plan of treatment, or before abandoning certain cases as irremediable.

XXII.—A Case of Recurrent Tumour of the Breast.
By J. Warrington Haward. Read April 10, 1874.

MARY G., æt. 64, was admitted into St. George's Hospital Oct. 18, 1873, under the care of Mr. Hewett.

She said that thirteen years ago she had a tumour removed from the right breast by Sir James Paget, and that after three years it recurred and was again removed. Four years ago the tumour had again grown to a great size, and the whole breast was then removed. About the beginning of 1873 she noticed a small tumour at the outer end of the
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...& in this gradually increased. She did not lose flesh, and felt in pretty good health. When admitted she was pale, but fairly nourished. There was a large, rather hard tumour covering the right mammary and infra-clavicular region, ulcerated on its surface, and bleeding slightly. No enlarged glands could be felt in the axilla.

Oct. 23.—In the absence of Mr. Hewett, Mr. Rouse removed as much of the tumour as was possible, but was unable to eradicate it, as the chest walls were found to be involved. The bleeding, which was very free, was controlled by the actual cautery, and styptic lint.

She was freely stimulated, but rapidly sank after the operation, and died next day. The above notes are derived from those in the Hospital Case-book, taken by Mr. Rowland, Surgical Registrar.

I made a post-mortem examination twenty-four hours after death. The body measured 5 ft., and was well nourished. The hair was black.

On the right mammary region, and extending as far as the left margin of the sternum, was a large sloughing wound. On removing the dead tissue from this, an oval growth was seen upon the surface of the thorax, extending from the first to the fifth rib, and limited laterally by the axillary fold, and the left margin of the sternum. This growth had invaded the intercostal muscles, and in the second intercostal space projected slightly into the pleural cavity, pushing the parietal pleura before it. A linear cicatrix extended from the edge of the tumour a short distance into the skin of the axilla. The glands and other tissues of the axilla were natural. The parietal pleura over the growth in the second right intercostal space was of a purple colour. The pleurae were otherwise natural.

All the other organs of the body were natural. The new growth was soft and easily broken down, chiefly of a pale red colour (about the tint of a fatty muscle), interspersed with irregular areas of yellow and rusty-brown colour. The yellow portions were rather firmer than the rest. Microscopically it was seen to consist chiefly of fusiform cells with oval nuclei, imbeded in a scanty, dimly granular matrix. Besides these cells, however, were oval and round cells, and large mother-cells, containing a dozen or more oval nuclei with nucleoli. These last cells were of irregular shapes and dimly granular, and resembled in all respects those described as myeloid. (See Drawing.)
The growth was very vascular, and the myeloid cells were more numerous in some parts than in others.

An examination of the fragments of tumour removed during life shows them to consist of precisely the same elements as those just described.

I have unfortunately been unable to obtain any account of the tumours previously removed from this patient, but the clinical history of the case is quite that of a recurrent fibroid tumour. It is remarkable, however, that in the last growth, at any rate, there were cells present, which oblige us to place it among the class of myeloid tumours. Cases of myeloid tumour of the breast are extremely rare. I am aware of only two others.

Sir James Paget has been so kind as to give me the notes of a case of myeloid tumour of the breast occurring in a member of a cancerous family, in whom, five years after removal of the tumour, a hard cancer of the breast appeared.

The following is an abstract of Sir James Paget's notes:—Emma N., aet. 45, a pale anaemic nervous woman, admitted
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into St. Bartholomew's Hospital under Mr. Stanley, Dec. 11, 1847. 'She is unmarried, and has had no children. Though pale, she has no certain appearances of general ill-health, and makes no complaint except of a tumour in her left breast. This tumour is situated in the middle of the outer half of the breast, clear of the nipple, which is not retracted. The tumour feels of oval, somewhat nodulated form, flattened slightly on the surface next the skin, and there also slightly nodulated. It lies deep in the substance of the gland, and moves freely, though apparently not without some of the gland immediately around it. It is hard and incompressible, as hard as some hard cancers that lie deep in the substance of the mammary gland. It is but little painful, only now and then pricks and shoots a little. The axillary glands are somewhat swollen, but not hard. The patient knows of nothing to which to ascribe the tumour. She has generally had good health, has always menstruated regularly and does so still. She first found the tumour four months ago; she felt a pricking pain in her breast, and on examination found a small lump, which enlarged rather quickly.'

Mr. Stanley removed the tumour, with the portion of gland around it.

'It proved to be a regularly circumscribed oval tumour, about $1\frac{1}{2}$ in. x 1 in., imbedded in the substance of the deepest part of the mammary gland, and very slightly adherent to it, yet certainly not infiltrated in it. It was hard and tough; invested with a very thin capsule, if with any; it cut very smooth and firm, and after some time the cut surface became

**Microscopic appearances of Sir J. Paget's myeloid tumour.**
slightly concave. Near its outer surface, or upon it, was a small cyst full of synovia-like fluid. 'The section presented a surface of uniform texture and consistence, smooth and without appearance of lobules or fibres. Its basis-colour was greyish, with a dim tinge of yellowish-green or cats' eye, shining but not translucent, and this was irregularly and rather distantly blotched and spotted with bloody, deep crimson, and rust-coloured patches, as if some undefined portions were very vascular. This character it presented throughout, but its peripheral parts were more blotched than its central parts. The substance of the gland just around the tumour was uniformly indurated.' The microscopic characters were those of a myeloid tumour, and some of the parent-cells sketched by Sir James Paget precisely resemble those that I have drawn from the tumour in the case first related.

The wound healed well and quickly, but in March 1853, five years after the operation, the patient was again admitted into the hospital, and this time with a hard cancer of the breast. 'Directly under the distal end of the scar of the former operation (a well-formed scar) one felt a small hard irregular lump, just like a scirrhous cancer.' This she had noticed six months.

The whole breast was removed; and the tumour, which was connected with the outermost border of the gland and the inferior surface of the scar, was a true hard cancer. The wound healed well, and her health improved.

In February 1854 Sir James Paget again saw her. She had then a very pallid, sallow, and cachectic appearance, and a cancerous tumour of the right breast. This was removed with the surrounding gland substance by Mr. Stanley, but having been lost, was not examined. In May 1856 she again appeared with another tumour of the right breast, and a recurrence of the growth in the scar of the left breast. Both growths were removed, that from the right breast being well-marked medullary cancer, that from the left as well-marked scirrhous cancer.

She recovered favourably from the operation. In February 1857 she had another growth in the right breast, but was subsequently lost sight of. While the patient was under observation her sister, a few years younger, came under treatment for a scirrhous cancer of the breast, which was removed. The disease recurred after eleven months, and she died cancerous after two years.
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The great-grandfather of these sisters was said to have had cancer of the face.

Sir James Paget remarks: 'Thus we have a case of myeloid tumour in the breast in a patient with family tendency to cancer, and, five years after its removal, a hard cancer formed near the place of the operation-wound. The connection between the two is perhaps only a distant one, yet it may be noticed that so rare a thing as a myeloid tumour in the breast, should happen in one prone to cancer.'

The only other case of myeloid tumour of the breast of which I know is one mentioned by Sir James Paget in his 'Lectures on Pathology.'* It occurred in a woman 50 fifty years old, and was of about nine month's duration. The characters of the tumour were a good deal obscured by suppuration. It was removed, and eighteen months afterwards the patient returned with an ulcerated tumour of the axilla, which resembled cancer, and had formed six months after the operation. With this she soon died.

The cases I have related show, then, that we may have a tumour with all the clinical characteristics of recurrent fibroid, containing myeloid cells; and secondly, a myeloid tumour followed, after removal, by a recurrent growth having all the characters of cancer.

I cannot say whether, in the case of Mary G., the first tumours contained myeloid cells or not; but the course of the second, which was myeloid from the first, is very suggestive of the affinities of these tumours, especially when we consider how often (as Sir J. Paget has pointed out) recurrent non-cancerous tumours occur in members of cancerous families.

Moreover there are a sufficient number of cases on record† of myeloid disease showing malignant characters to justify this association, and to show that it is an error to assert, as Mr. Gray and others have done, that myeloid tumours are perfectly innocent. It would be difficult to produce a case showing more distinctly malignant characters than such an one as that related by Mr. Henry in the 'Pathological Transactions,'* in which a myeloid tumour of the humerus was removed by amputation through the shoulder-joint, and was followed by recurrence in six weeks, and death in three months; myeloid growths being found in the recurrent tumour and in the lungs. Or than that related by Dr. Wilks

† 'Med. Chir. Trans.' vol. xxxix.
Mr. Lee’s Case of Traumatic Stricture of Trachea.

("Path. Trans." vol. x. p. 244), of a man whose leg was amputated by Mr. Cock for a myeloid tumour of the fibula, in whom recurrence took place after two years in the stump, and was followed by death from pleurisy occasioned by the growth of secondary myeloid tumours on the surface of the lung.

So that the lesson which these cases seem to me to teach is this: that if we would make our definitions of diseases practically useful, we must beware of making them too exclusively anatomical; the significance of the anatomical characters needs to be determined by the light of the clinical history; and I suppose that the more we know of the natural history of morbid growths, the less inclined shall we be to base our prognosis of the disease simply upon their microscopical characters.

For these reasons, although the case I have related has much of pathological interest, it seemed to me not unfit for the attention of the Clinical Society.

XXIII.—Case of Traumatic Stricture of the Trachea relieved by Operation. By Henry Lee. Read April 24, 1874.

A YOUNG gentleman, who had for several days been drinking freely, cut his throat on the 10th of February, 1873. He had used a carving-knife, with which he had made a transverse wound immediately below the cricoid cartilage. He had then taken a penknife and made an extensive jaggy wound lower down into the trachea on the right side. There was considerable loss of blood at the time, but the vessels were fortunately secured in time to prevent any excessive haemorrhage. The rings of the trachea had evidently been cut in more than one place, so that the wound in it remained open. Both wounds, however, healed readily, and were almost closed in the beginning of March.

In the middle of the night of the 12th of March I was startled with the information that my patient was suffocating. I found the information was but too true. He was breathing with the greatest difficulty and exertion, every inspiration being accompanied with a peculiar whizzing sound. The skin was bathed in perspiration, and the skin of the face was livid. He had already partially lost his consciousness. With the assistance of Mr. Rouse I re-opened the wound on the right side of the trachea, and introduced
tracheotomy tube. The breathing was now performed
without difficulty, and the patient was again comparatively
comfortable. After the lapse of a few days he was removed
short distance from town, and discontinued the tracheo-
tomy tube. On March 30 the symptoms of suffocation
gain appeared, and I had to repeat the former operation.
On each of these occasions I have no doubt my patient would
have died had not surgical relief been at hand. He had
suffered so much from these attacks, that there was now no
longer any occasion to urge him to wear his tube continually.
There was, however, a great disposition of the parts to con-
tract; and when he withdrew the tube for the purpose of
leansing it, there was often difficulty in replacing it. By
placing his finger on the opening of the tube he could speak
with an effort, but tolerably distinctly. He now returned to
his home in the country.
I saw my patient again Nov. 14. He had now en-
tirely lost his voice, and could make noises only with his
lips. These sounds his intimate friends could sometimes
interprett, but to others they were quite unintelligible. He
had not spoken for six months. I was informed that after
he left London the tendency to contract which I had noticed
continued; there was often great difficulty about the tube,
and power of speaking, which had gradually disappeared.
I had then the advantage of Mr. Hewett's opinion upon the
case, and I determined to try a fresh operation to relieve
the obstruction which it was evident existed in the trachea,
above the point at which the tube was inserted. Mr. Rouse
again favoured me with his assistance, and on Dec. 19 the
patient, being under the influence of chloroform given
through the opening in the trachea, I made a longitudinal
incision in the median line over the lower part of the thyroid
and the cricoid cartilages. After waiting a little for the
bleeding to stop, I made a free incision into the membrane
between the cartilages. Through this I was enabled to put
a tube, but found it impossible to pass it down into the
trachea. I then still further enlarged the opening, partially
dividing the thyroid and the cricoid cartilages. I now en-
deavoured to pass tubes of different sizes, but in vain. Even
a No. 7 elastic catheter would not pass. The wound made
was sufficiently large to allow me to introduce my little
finger, and I could feel that the larynx terminated in a cul-
de-sac about the lower margin of the cricoid cartilage. A
director was now passed from the lower wound in which

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the tracheotomy-tube had been used into the larynx, a probe-pointed bistoury was passed along it, and what remained between the two openings was divided as nearly in the median line as possible. This was, however, so firm that it required very considerable force. It was very difficult to keep the patient under chloroform during these successive steps of the operation, and consequently he struggled a good deal. Sometimes hemorrhage also delayed the completion of the operation, which under the circumstances was necessarily long. A large tube, without any opening in its convex part, was introduced into the wound in the median line. This had the effect of enabling the patient to breathe freely, and probably prevented any blood from passing into the trachea. At all events, there was no further trouble from this source. The day after this operation there was some swelling of the parts, and this was followed by some suppuration. On the 23rd, all swelling and irritation having subsided, I could see a portion of the anterior rim of the cricoid exposed. This I removed with a pair of probe-pointed scissors, taking a slice from each side of the incision I had made at the previous operation. A tracheotomy-tube of a peculiar construction, made on purpose, was inserted, and the patient bore its presence very well. It consisted of a longer tube which passed downward like an ordinary tracheotomy-tube, and within that was a shorter tube which passed upward. This shorter tube separated from the other rather in front of the point where the opening generally exists on the convex part of an ordinary tube. The external orifice of the tube was furnished with a valve which would allow the air to enter, but not to pass out. The patient was directed to keep very quiet, in order that the tube might not be disturbed. The next morning, as the nurse entered the room, she was terribly frightened by the patient saying to her, in a voice as loud as natural, though somewhat rough, 'Good morning, nurse.' She, of course, had never heard him speak before. After a couple of days the tube with two limbs was removed, and the condition of the parts at the back were for the first time brought into view. It was now seen that the posterior part of the upper ring of the trachea on the left side approached very closely to the anterior part of the lower margin of the cricoid. The first wound that the patient made in his throat must have passed between these parts, and they must have been drawn together in the process of cicatrisation. The interval between the upper margin of the
The upper ring of the trachea as now seen, and the lower margin of the anterior part of the cricoid which had been removed, must have been less than a quarter of an inch. On the 26th this upper ring of the trachea, which had evidently been the principal cause of the stricture, was seized with a pair of forceps, and in great part removed with a pair of scissors. Some haemorrhage now prevented a good view of the parts, but the remainder of the ring which projected into the tracheal tube was removed in the same way without difficulty on the following day.

The upper part of the wound healed much more rapidly than the lower, probably on account of the latter having been kept open so long by artificial means. On Jan. 5 the wound was so far closed that by placing his finger over the opening the patient could speak without any tube. His voice was natural, with the exception of being rather gruff. At this time the upper part of the wound had healed, and the opening through which he wore the tube in the trachea was below the former seat of stricture. A short tube was therefore made, curved to a right angle, with a large opening on its convex part, and a valve at its external orifice. This was worn with its inner opening upward without inconvenience, and the patient was able to go about his business as usual.

The patient was again seen on July 25. He had suffered no inconvenience since the last report, but there was evidently some tendency to contract at the point where the upper ring of the trachea had been removed.

XXIV.—On the Treatment of Cystic and Fibro-cystic Bronchocele. By Morell Mackenzie, M.D. Read April 24, 1874.

During the last six years* 87 cases of cystic and fibro-cystic bronchocele have come under my care; and in addition to these cases many others have been treated in the manner here recommended, by my colleagues at the Hospital for Diseases of the Throat, and by other professional friends.

Of my 87 cases, 68 were cystic, and 19 fibro-cystic; of the

* This paper was written at the end of the year 1873.
cystic cases 54 were cured, 11 were considered too slight to require treatment, and in 3 instances cardiac disease rendered it undesirable to employ radical treatment. Of the fibro-cystic cases 10 were cured, 4 greatly benefited, and 1 died whilst in 3 cases the disease was so slight that it did not exist for interference, and 1 patient discontinued attendance during the treatment.

In the cystic cases the cyst was first punctured and emptied with a trochar at its most dependant part; a drachm or tv (according to the size of the cyst) of a watery solution of perchloride of iron (3i) was then injected with a glass syringe, and the canula plugged, the iron being left in the cyst. After seventy-two hours the plug was removed, and the iron solution withdrawn. The plug was then re-inserted, and poultices of linseed-meal kept constantly applied for a few days (sometimes for ten days or a fortnight) immediately over the cyst. In a few days suppuration was set up, and the plug was then permanently removed, the canula, however, being allowed to remain until the secretions became limited in amount and thin in consistency.

One injection is generally sufficient; but if the first injection be too quickly removed the process has to be repeated three or four times, at intervals of two or three days; and the injection should be repeated from time to time, as long as the discharge contains much blood or hæmatin.

In order to avoid injecting air, I now use a syringe terminating in a long arm or beak, tapering to a point, which is placed at a right angle to the cylinder of the syringe. The piston of the syringe being provided with a stop (near to its proximal extremity) cannot pass down to the end of the cylinder, and the syringe cannot be entirely emptied. In injecting, the conical or tapering point of the syringe passes almost horizontally into the canula placed in the wound, the cylinder of the syringe being nearly perpendicular. In this way, any air that is present in the syringe must remain at its upper part, and cannot possibly be injected into the cyst.

The duration of treatment is from three weeks to four months, according to the size of the cyst, the usual time being from six to eight weeks.

In the fibro-cystic cases the cysts are first cured in the manner described, and the fibrous structure afterwards treated by subcutaneous injections of iodine. A few cases, one of which is reported here, were in part treated with caustic darts.
The following cases, and the accompanying photographs, are sufficient to illustrate the treatment:

Cystic Goitre of four years' duration, cured in one month by injections of iron.

K. L., æt. 21, was admitted into the Hospital for Diseases of the Throat Sept. 4, 1869, on account of a cystic goitre of four years' duration, the throat measuring 16½ inches. She was treated by injections of iron, and discharged cured Oct. 6, the neck measuring 12 inches.

Cystic Goitre cured in one month by injections of iron.

Jemima K., æt. 33, the wife of a soldier, suffering from cystic goitre of seven years' duration, was admitted into the Hospital for Diseases of the Throat July 31, 1871. Treated by injections of iron, and discharged cured August 31.

Cystic Goitre reduced in three months by injections of iron.

William L., æt. 22, admitted into the Hospital for Diseases of the Throat Nov. 16, 1871, suffering from cystic goitre of four years' duration. The base of the neck over the tumour measured 21 inches. He was treated by injections of perchloride of iron. Suppuration was soon established; but the case proved a tedious one, and he was not considered convalescent till the middle of February, when the neck measured 15 inches. The patient was kept in town for seven months longer. During the treatment, notwithstanding the extensive suppuration, he gained 11 lbs. in weight.

Cystic Goitre of several years' duration cured by injections of iron, after the failure of a seton.

Miss S., æt. 60, was sent to me by Sir Ranald Martin in June 1872, on account of a bronchocele of several years' standing. On examination, a cyst about the size of an orange was found in the front of the neck, in the situation of the isthmus of the thyroid gland; a seton was lying in the cyst, and Miss S. informed me that it had been there for nine months, and had produced no effect on the tumour. She was treated by injections of iron, suppuration was established in a few days, and the case was completely cured at the end of five weeks.
Fibro-cystic Goitre of nineteen years' duration cured by injections of iron.

Mary W., æt. 49, was admitted into the Hospital for Diseases of the Throat July 2, 1872, suffering from a cystic goitre, the neck measuring 22 inches. The cyst was treated with injections of iron, and the fibrous tissue subsequently attacked by injections of iodine. She was discharged cured Oct. 9, the throat measuring 13 inches.

Cystic Goitre of four years' duration cured by injections of iron.

Mrs. M., æt. 27, was sent to me by Mr. Buxton Shillitoel in July 1873, on account of a cystic goitre. The disease commenced four years previously, when the lady was living in a goitrous district at the foot of the Simplon; she afterwards went to South America, unfortunately again to a district where bronchocele was endemic. The goitre rapidly developed, her breathing became embarrassed, and she was obliged to return to Europe. She now came under my observation, and I found her suffering from cystic enlargement of the thyroid gland, the neck measuring 19 inches. She was treated by injections of iron, and cured at the end of five weeks.

Cystic Goitre of seventeen years' duration cured by injections of iron.

Emma H., æt. 28, was admitted into the Hospital for Diseases of the Throat Nov. 13, 1871, on account of a cystic goitre of seventeen years' duration, the neck measuring 16 inches. She was treated with injections of iron, and discharged cured Dec. 22, 1871, the throat measuring 12 inches.

The following case illustrates the progressive shrinking, which often goes on for some time after treatment has been discontinued.

Fibro-cystic Goitre of twenty years' duration cured by injections of iron and caustic darts.

Edith M., æt. 50, was admitted into the Hospital for Diseases of the Throat Sept. 28, 1870, suffering from fibro-cystic goitre, the neck measuring 20½ inches. The cysts were injected with iron, and the fibrous structure attacked
with caustic darts. The patient was discharged greatly relieved on Jan. 29, 1871, the throat then measuring 16 inches. Her condition at that time was portrayed in the 'Lancet' of May 7, 1872. In December 1873, without having undergone any further treatment, her neck was found to have contracted to 13 inches, and the cicatrices to have almost disappeared.

The following is the only fatal case that I have met with.

**Fibro-cystic Substernal Goitre, treated by injections of iron. Death from entrance of air into a vein.**

Mr. M. C., a Lincolnshire farmer, æt. 37, consulted me in June 1872, on account of a swelling at the lower part of the neck, accompanied with shortness of breath.

On examination a fibro-cystic substernal goitre was found to be present; but the neck only measuring 16½ inches, and the symptoms not being urgent, I did not think that any treatment was necessary. A year later he consulted me again, and the neck having increased an inch and a quarter, and the dyspnea being at times considerable, treatment was recommended.

Accordingly, on Oct. 31, 1873, the cyst was tapped, and a small quantity of sero-sanguineous fluid was drawn off. Perchloride of iron was then injected, but before the injection had been completed a sucking noise was heard in the wound, and the patient expired in a few seconds.

The mode of death was so instantaneous, and the sucking noise following the injection was so characteristic, that the entrance of air into a vein, and its passage to the heart, can alone explain the fatal occurrence.

Unfortunately a proper autopsy was not permitted, but a partial examination was made by Dr. Lowe, of Lincoln.

The following is the Report which he kindly forwarded to me:—'The superficial veins were large and turgid with very black fluid blood. The tumour was oblong in shape, and extended downwards beneath the manubrium sterni. Its substance was very friable, and full of black blood converted into a solid state, of cheesy consistence. The large veins, lying at the posterior surface of the swelling, were distended with blood of the same character as that contained in the tumour.' (The condition of the heart was not ascertained.)

I have no doubt that in this unfortunate case air was
actually injected into a vein—not sucked in, as it occasionally is in operations on the neck.

To prevent the recurrence of such an accident, I now use the bent syringe already described, and with it I believe it to be impossible to inject air.

The following are the conclusions at which I have arrived:

(1) That any cystic goitre which has attained the size of a hen's egg requires to be actively treated, even when the symptoms are not urgent.

(2) That smaller cysts, which give rise to serious dyspnœa or dysphagia, likewise require to be treated.

(3) That the conversion of the cyst into a chronic abscess is the safest and most certain mode of treatment.

(4) That suppuration is best set up by injections of the perchloride of iron, as the disposition to haemorrhage is thereby effectually controlled.

(5) That injections of iodine (in cystic goitre) are dangerous, because often followed by sloughing.

(6) That there is a risk in the treatment by injections of iron, from the occurrence of too profuse suppuration, when the cyst has been allowed to attain too large a size before treatment.

(7) That all operations on the neck are attended with the danger of air entering a vein and causing sudden death.

(8) That this risk is in proportion to the development of the veins, and the propinquity of the tumour to the heart.

(9) That in pure cystic goitre the chance of this occurrence is so slight that it may be dismissed from consideration.

(10) That in certain kinds of fibro-cystic goitre, viz. those in which some of the original gland-substance is contained in the cyst, especially in substernal fibro-cystic goitre, the risk is at its maximum.

(11) That the extirpation of cysts is always attended with great danger from haemorrhage.

(12) That extirpation is, nevertheless, justifiable where (the symptoms being urgent) the cyst has obtained an enormous size, and has a capacity of several pints, but is not directly connected with the trachea or œsophagus.

(13) That extirpation is justifiable where such a cyst has already burst, and the patient is in danger from an exhausting discharge.

(14) That extirpation may also be employed for the
removal of a small but distinctly pedunculated cyst, having for instance a capacity of two or three ounces, provided that there be no large vessels in its peduncle.

XXV.—Paralysis of the Radial Nerve from an unusual mode of Lead-poisoning. By J. Althaus, M.D. Read April 24, 1874.

A CHEMIST, set. 41, married, of full habit and in tolerably good health, consulted me on Nov. 5, 1873, for a paralytic affection of the left hand, from which he had been suffering for a month past. He gave me the following history:—He was walking in the country in April last, and, on getting over a freshly-planted hedge, ran a black-thorn into the middle of his left thigh. This accident was followed by erysipelas and sloughing of the inner portion of the thigh, and he had to take to his bed a fortnight afterwards. He was laid up for about four months, after which he commenced to go about again, although the sore was then not nearly healed. In the commencement of October last he suddenly noticed that he was unable to use his left hand. Stimulating liniments were used for restoring the lost power, but without effect, and the patient then came to London to consult me.

On examining the left upper extremity, it appeared that there was full control over the muscles of the shoulder and the arm, and that the paralysis was confined to the extensor muscles on the back of the fore-arm, viz.:—the extensores communs digitorum, carpi radialis and ulnaris, indicis et digit minimi proprius, abductor and extensores pollicis longus et brevis; that is, all muscles animated by the radial nerve. Extension of the wrist, the first phalanges of the fingers, abduction of the thumb, and the lateral movements of the wrist towards the radial and ulnar side, were impossible; while flexion of the wrist and fingers, supination and pronation of the hand, and extension of the second and third phalanges, could be freely performed. The paralysed muscles felt flabbier than their fellows on the right side, but there was no wasting, for on measuring the two limbs, their size appeared to be the same. There was no anaesthesia of the skin. On faradising the radial nerve at the lower third of the humerus, just midway between the insertion of the del-
Dr. Althaus' Case of Lead Paralysis.

toid and the external condyle, and on directly faradising the paralysed muscles, no response was obtained in them, even if a current of great power was used; and the electro-muscular sensibility appeared to be considerably diminished. The triceps, the supinators, flexors, pronators, and the lumbrical muscles, contracted well under the influence of a current of moderate power, and their electro-muscular sensibility was normal. On applying a continuous current of forty cells of Becker-Muirhead's battery to the radial nerve and the muscles animated by it, the usual sensation of pricking and heat was felt in the skin, but no contraction took place; and it was only on increasing the galvanic power used to sixty pairs of plates that a feeble and sluggish response was obtained.

The peculiar electric phenomena which I have just mentioned are only met with in paralysis due to lead-poisoning. It is true that we occasionally meet with palsy of the muscles animated by the radial nerve from exposure to wet and cold, and also from injury. But in the rheumatic form of this palsy the farado-muscular contractility is either normal or only slightly diminished, and the supinator muscles suffer just the same as the extensors; and in traumatic paralysis not only motion but also sensation is lost, and the supinator muscles are affected to the same degree as the extensors. As therefore in the present case the farado-muscular contractility of the paralysed muscles was entirely lost, and the supinators were healthy, I could exclude rheumatic paralysis; and as there was no anaesthesia of the skin and no paralysis of the supinators, I could exclude traumatic paralysis; so that no doubt existed in my mind that the influence of lead had to be accused as productive of the paralysis in this instance. It is true that it appeared *prima facie* unaccountable how the patient could have got the lead into his system; and on being questioned on this subject, he denied that he had ever in his business or elsewhere handled lead or come under the influence of it. I nevertheless persisted in my opinion, and suggested to him various ways in which lead might have entered the system, such as sleeping in a newly painted bedroom, drinking beer*early in the day from the tap of a public-house, taking snuff packed in lead-foil, &c. All these queries received a negative reply; but suddenly the patient said that he had used an immense quantity of lead-ointment for dressing the sore on the thigh, viz. as much as an ounce of the unguentum plumbi subace-
tatis compositum, three times daily. This ointment contains one part of the solution of the sub-acetate of lead in $5\frac{3}{4}$ of wax and-almond oil; which, at the rate of three ounces per diem, would amount to about half an ounce of the solution of subacetate of lead applied daily to a wound having then a circumference of ten inches by six. The dressing with this ointment had been continued for a month, and sufficient lead could therefore have been conveyed to the absorbents for exercising a decided influence on the system. It is a well-known fact that lead is absorbed even by a surface not denuded of its cuticle; and in the present case absorption must have been considerably facilitated by the highly vascular condition of the sore, and by the length of time during which the lead ointment, which was spread on lint, remained on the surface of the wound.

The diagnosis of the case having once been settled, no difficulty was experienced in determining upon a plan of treatment. I prescribed fifteen grains of iodide of potassium to be taken daily, for removing the lead from the system; and held out the prospect of an eventual application of galvanism to the paralysed muscles, for restoring the use of the hand.

The patient presented himself again three weeks afterwards, when he reported himself as feeling generally much better and stronger, but the condition of the hand remained unchanged. There was still no power of extension, and the faradic excitability of the affected nerve and muscles was entirely gone. I now applied the continuous current to the radial nerve, inducing catelectro-tonus in the same, and followed this up by intermittent galvanisation of the paralysed muscles, combined with voltaic alternatives. There was an immediate improvement, and two more applications were sufficient to restore the use of the limb. The faradic excitability of the paralysed muscles returned pari passu with their voluntary motion.

This case possesses several features of clinical interest. It shows in the first instance that an external medicinal application of lead, provided this affects a surface of some extent and is continued for some time, may cause palsy of a limb. It is a very remarkable fact, that the paralysis should have been confined to the wrist of the same side where the poisoning took place. In by far the largest majority of cases of lead-palsy the right hand suffers, and if the left be also affected, it is generally much less so than the right.
This is evidently owing to the right hand being much more brought into contact with lead, in those trades in which lead-palsy is chiefly observed, viz. amongst painters and compositors; while in the present case the left wrist suffered, from lead having been exclusively used on the left side. The inference seems therefore justifiable that lead-palsy is more owing to local absorption than to a general infection of the system with the metal.

The diagnostic value of faradisation was very conspicuously displayed in this case, as there was a total absence of other symptoms of lead-poisoning, such as a blue streak on the gums, dyspepsia, colics, obstinate constipation, &c., which rendered the diagnosis difficult; yet the faradic test showed at once that the case was one of lead-palsy.

The therapeutical use of the continuous current in such conditions was likewise very evident, inasmuch as three applications of it were sufficient to restore to the influence of volition muscles which had been completely paralysed, and which had not shown the slightest sign of amelioration under the influence of iodide of potassium. The galvanic treatment was rapidly successful in this case, because it was resorted to early. If, as is almost invariably done, electricity is not employed until a high degree of atrophy of the paralysed muscles has set in, and if then faradisation only is used, under the erroneous impression that it will do as well as the continuous current, the treatment must be more tedious and less successful. It is true that patients improve even under such circumstances, but they rarely regain complete power over their limbs. The therapeutical lesson to be drawn from the present case is, therefore, that we should use the continuous current early in all cases of lead-palsy, so as to prevent wasting of the muscles, and to restore voluntary motion.

Another question upon which the case I have related appears to throw some light, is that of the actual seat of the paralysing lesion in lead-palsy. Opinions differ considerably on this point amongst the best observers. Some are inclined to believe that the palsy is owing to a general cachexia induced by blood-poisoning. Now there can be no doubt that sanguification is considerably altered in many cases of saturnine infection, which is probably owing to the circumstance that the salts of lead neutralise the digestive power of the gastric juice. Deposits of chloride of lead are formed in the stomach, whereby part of the digestive ferment is rendered insoluble. At the same time the arterioles are constricted,
so that too little blood is conveyed to the parts, and in the later stages of the complaint there is actual wasting of the gastric glands. The same thing occurs in the intestines, and the formation of chyle is therefore impeded. But although the impaired sanguification resulting from these pathological changes sufficiently explains the general saturnine cachexia from which so many workers in lead suffer, it is impossible to believe that this is the actual cause of the paralysis. In the case which I have just narrated, there was no general cachexia; neither dyspepsia, nor colics, nor obstinate constipation; on the contrary, the appetite and digestion had always been good, and the general health, although somewhat feeble, was yet on the whole satisfactory. Nevertheless there was complete paralysis of an entire group of muscles; and it is therefore necessary to assume that, apart from the systemic effects of the metal, there is a special and immediate action of the same upon the nerves and muscles. The point we have to decide is, therefore, which part of the system was at fault? Was it an affection of the nervous centres, of the radial nerve, or of the muscular substance itself?

That the central nervous system suffers in consequence of saturnine poisoning is unquestionable; for epilepsy, eclampsia, and general tremor, are not unfrequently traceable to it; nevertheless I do not believe that in the present case, or indeed in other cases of lead-palsy, the nervous centres are at fault. My chief reason for this opinion is to be found in the striking electrical phenomena we meet with in lead-palsy. It may be laid down as a general rule that, where paralysis is owing to central disease, the faradic contractility of the paralysed muscles is either normal or only slightly diminished; while if it be owing to disease or injury of a peripheral nerve or plexus of nerves, farado-muscular contractility is lost. Thus, for instance, we find, on examining a case of hemiplegia owing to corpus striatum haemorrhage or softening, normal excitability of the muscles, even as late as two or three years after the attack; while in the case of contusion or other injury, say of the median or sciatic nerve, the faradic excitability of the muscles is entirely gone within a fortnight after the accident. When therefore total abolition of the electro-muscular excitability is encountered, we have the strongest reason for suspecting a peripheral seat of the palsy. Another circumstance speaking for the peripheral origin of lead-palsy is, that its later stages are invariably characterised
by a high degree of muscular atrophy, which indeed often sets in within a few months from the first appearance of, or even simultaneously with, the palsy. The same thing is observed in injuries to motor nerves, which are always followed by wasting of the muscles animated by them, and which commences generally in the third week after the receipt of the injury. The transverse stripes then become indistinct, crowds of nuclei make their appearance, and amyloid degeneration follows. In most diseases of the nervous centres causing paralysis, on the other hand, there is little or no wasting in the paralysed muscles, even where the affection has lasted for years.

The last question we have to consider is, whether lead-palsy may be owing to an affection of the muscular substance itself. Henle and Gusserow have asserted that lead has a special affinity to muscular fibre, and more especially to the unstriped fibre-cell; while Heubel states that muscular tissue absorbs lead only with difficulty, and that nervous matter takes it up most readily. The appearances observed in the present case would certainly lead me to side with the latter opinion. It is a well-known fact that in truly muscular affections the faradic and galvanic excitability of the fibres is only lost in exact proportion to their degree of wasting; and that, where even a feeble remnant of healthy fibres still exists, this will contract under the influence of faradisation. There is total loss of this faculty only when the entire substance of the muscle has been destroyed. In the present case, however, there was total loss of excitability without atrophy, and we are therefore led to the conclusion that lead acted specially upon the radial nerve, probably constricting, by its astringent action, the arterioles of its sheath, and thereby interfering with its function to such an extent as to cause palsy of a number of muscles which are under its influence.

Paralysis of the muscles animated by the radial nerve is a very annoying affection, as it renders the hand in a great measure useless. It is true that the median and ulnar nerves are there to supply motive power to the flexors, pronators, and other muscles of the extremity; but the radial nerve animates some muscles which are constantly required in the occupations of daily life, and the inaction of which renders the entire limb practically of little use. Foremost amongst these are the long abductor and the extensors of the thumb, the paralysis of which does not allow the patient to grasp
anything with the hand, and to write or draw, when the nerve of the right side is affected. The second in importance is the extensor carpi radialis, which approaches the hand to the front part of the body, and which is constantly used in dressing and eating. Paralysis of the other muscles animated by the radial nerve is not of such immediate practical importance; yet it may, if the case is not properly attended to, become a serious drawback in the later stages of the complaint. If these muscles remain paralysed, they gradually waste away, and this leads in its turn to contraction of the flexor and pronator muscles, with consequent deformity of the hand. Thus the whole limb may ultimately become crippled. It is therefore fortunate that we possess, in the continuous galvanic current, a remedy by means of which we are able to relieve very readily most cases of paralysis of the radial nerve.

XXVI.—History of a Case of Palmar Aneurism, with Notes on the Treatment of Hæmorrhage from the Palmar Arch. By W. H. Cripps. Read May 8, 1874.

So few cases of aneurism, affecting the arterial branches passing through the palm of the hand, are recorded in the recent surgical reviews, that this particular form of disease must be more rare than at first sight might seem probable.

Since the treatment of such cases has always been recog-

as a subject of some difficulty, a few notes upon one which has recently come under my observation may not be without their value.

On Oct. 28 the patient, æt. 35, ran the blade of an ordinary penknife into the palm of the hand. The wound was not more than a quarter of an inch wide, and from the position of the hand at the time of the accident must have extended upwards and inwards, passing the first inter-osseus space towards the middle line of palm. Blood immediately poured out in a full stream; a piece of cork bound tightly over the wound arrested the hæmorrhage.

No inconvenience was felt till the fourth day after the accident, when the patient experienced considerable pain.
The compress was removed, and the external wound had completely healed.

During the next three days the hand was painful and swollen, and on the eighth day after the injury the pain and swelling had considerably increased.

On the tenth day the whole hand was swollen and very painful. In the first inter-osseous space a tumour, equal in size to a walnut, had appeared, extending more on the back than into the palm of the hand.

The tumour pulsated strongly, and a loud bruit could be heard.

Direct pressure on the tumour caused a nearly total subsidence of the swelling, while pressure on the brachial stopped pulsation and caused the swelling to diminish.

The patient, a strong healthy man, was unable to recollect that he had ever suffered from abnormal bleeding in consequence of slight wounds, except upon one occasion, when the extraction of a tooth was followed by profuse hæmorrhage for three or four days.

On Nov. 4 the hand and fore-arm, having been bandaged according to Esmarch's method (a process after which the tumour almost entirely disappeared), the posterior or dorsal portion of the sac was laid open for about two inches. The sac appeared to be well defined, and contained but a small quantity of coagulated blood. No open vessel could be seen, but upon the return of blood to the limb free hæmorrhage occurred. Two small vessels were tied, but blood continued to well up freely from the deeper portions of the wound in the neighbourhood of the first and second metacarpal bones. As no bleeding-point could be discovered, a plug of lint was inserted into the wound and the hand bandaged. All bleeding stopped.

The patient was kept in bed, and on the fourth day after the operation the plug was gently removed, the wound looking clean and healthy.

At ten o'clock on the night of the 9th, five days after operation, the patient was awakened by the sensation of blood flowing from the wound.

His wife wound three or four coils of indiarubber tubing tightly round the wrist, which stopped the bleeding. The patient had, however, lost about a pint of blood.

An hour later all dressings were removed, and the elastic tube taken off. There was then no bleeding. The clot was turned out and a fresh lint plug introduced, and a mode-
rately tight bandage placed over the hand, from the fingers to the fore-arm.

At 1 a.m. bleeding recurred, and was controlled, but not completely stopped, by pressure on the radial and ulnar arteries. This pressure, after an hour, causing some pain and not completely arresting hemorrhage, was relaxed, and once again the plug of lint adjusted to the wound, and the hand and fore-arm bandaged to a splint. The bleeding again stopped for a short time, and then once more began to ooze through the dressings; all dressings were removed, and the wound was thoroughly sponged with cold water. The hand was raised, the fore-arm bent, and the wound exposed to the air.

At 3.30 the bleeding recommenced. The wound was now carefully cleaned with small sponges, while the brachial artery was compressed. On relaxing pressure blood welled up in a free stream from deep in the palm.

At this point, having to the best of my judgment at the time used every effort unsuccessfully to stop the bleeding by means of position and pressure, I tied both the radial and ulnar arteries about 2½ inches above the wrist. By this means the bleeding was completely arrested, the wounds were dressed with oiled lint, and the hand wrapped in cotton-wool.

The patient did well for two days. On the night of the 12th bleeding commenced afresh from the original wound; the bleeding was not so free as on the former occasion, the blood dropping from the dressings after they had been saturated, to the extent of half an ounce per minute. This was stopped by a tourniquet on the brachial. The patient now became restless, with a temperature of 101°, and was removed to St. Bartholomew's Hospital. Digital pressure on the brachial artery was then resorted to in the following manner:—The patient was placed on his back, with the wounded arm bandaged from the fingers to the elbow, the forearm being bent to rather more than a right angle, raised nearly perpendicular, and supported by pillows. The artery was very superficial, and moved freely under the finger; it was compressed in the lower third. The pressure was kept up for seven full days, until the evening of the 20th—an hour being the utmost time for which pressure could be kept up by one person without fatigue.

For the first four days the patient bore the pressure well,
sleeping comfortably, under the influence of morphia; but during the latter part of the time he became restless, the arm being tender and painful where compressed. On the pressure being discontinued, at the end of the seventh day the arm was placed on an angular splint, and slung by weight and pulley from the top of the bed. During this time the wound in the palm showed some tendency to heal by granulation; but on the evening of this day (the eleventh after ligature of the arteries) the ulnar began to bleed. This was, however, stopped by a cork compress over the artery below the wound. On the removal of this compress, after forty-eight hours, a small dark pulsating swelling was found in the wound which was laid open, and the distal end of the ulnar artery tied with carbonised catgut.

The patient now became very anxious, with a rising temperature. He had been placed on light diet, and for the last few days had been taking 7-min. doses of the liquid extract of ergot three times a day, also 20-min. doses of tinct. ferri perchlor. On the 27th, eighteen days after tying the arteries, and twelve days after commencement of digital pressure, bleeding recommenced from the original wound in the hand. The wound looked slightly inflamed, the cavity being filled with dark blood coagula.

After a consultation amongst the surgeons of the hospital, it was decided to ligature the brachial artery above the sup. profunda. The artery was accordingly tied by Mr. Thomas Smith just below the teres major, two silk ligatures being passed round an inch apart, and the artery divided between them. When divided both ends of the cut artery retracted out of view. A slight attack of erysipelas, which soon passed off, was the only bad symptom.

The ligatures from the distal end came away on the 16th, and that from the proximal on the following day. A fortnight later the wound in the hand had closed, and thirty-one days after the ligature of the brachial artery the patient was able to leave the hospital.

The patient was under the care of Mr. Thomas Smith, to whose kindness I am indebted for the opportunity of bringing the case before the Society. I was requested by Mr. Smith to see the patient, owing to his suddenly being called to the country.

The history of this case may serve to throw some light upon the treatment, first, of traumatic aneurisms in the
palm of the hand; and, secondly, of bleeding resulting from wounds of the palmar arch.

The results of experience have left little doubt as to the treatment of traumatic aneurisms of the usual diffused character. An opening of the tumour and ligature of the vessel at the wounded point has in most cases proved practicable, it being obvious that blood effused into parts surrounded by soft and yielding tissue has but little tendency to become circumscribed.

But the form of aneurism by which the palm of the hand is most commonly affected is that known as the circumscribed traumatic variety, where, although no sac properly so-called exists, the blood is held within certain limits by a firm layer of plastic material effused from the surrounding tissues. That an aneurism is of this character may be recognised by the circumscribed nature of the tumour, and by the fact that it contains blood, which can by means of direct pressure be returned into the vessel from whence it had escaped.

That pressure should be first tried in a circumscribed traumatic aneurism is generally admitted. This failing, it would then be a question as to the desirability of opening the sac, or performing the Hunterian operation. This would be settled by the situation of the tumour, and the chance of finding the vessel at the wounded point.

Now where an aneurism exists in the palm, the real nature of which it may be impossible exactly to determine, what is the probability of our being able, by means of opening the tumour, to find the wounded vessel?

The structure of the palmar fascia would appear to permit an aneurism to enlarge only in one of two directions, either towards the ball of the thumb and first inter-osseous space, or towards the ulnar side of the palm. If a pulsating tumour in either of these situations be laid open, the wounded vessel may be, and probably is, situated at a considerable distance from that part of the sac laid open, so far indeed that the dissection requisite to expose the bleeding point would, in the complicated structures of the hand, be exceedingly hazardous.

In this state of things the treatment required would be precisely analogous to that necessary where bleeding is the result of a recent wound of the palmar arch, for which purpose I will endeavour to show, by reference to a few reported cases, that pressure may be an effectual and a simple remedy.
If this be so, however, it would seem that pressure applied through the medium of the unbroken skin after the tumour had been emptied, combined with pressure on radial and ulnar, would be as effectual as if applied to the raw surface in the exposed sac.

Mr. Erichson, in his work on Surgery, says that he has laid open an aneurism of the superficial palmar arch, and tied both ends of the vessel with success.

He also refers to a case mentioned by Roux; but in this case the patient died of bleeding, notwithstanding the ligation of radial and ulnar.

Mr. Holmes, in a lecture delivered at the College last year, mentions three cases of palmar aneurism. One of these was cured by direct pressure; both the others died of haemorrhage, after the sac had been opened. Although not so stated, it is to be presumed in these two cases the arteries could not be secured in the wound. This was clearly so in one of them at St. Thomas’s Hospital, for on that occasion the brachial was tied.

Fischer, in his Tables, gives five cases of palmar aneurism; three of these were cured by pressure, one by injection, one by ligature.

In the "Lancet" of June 26, 1867, Mr. Sydney Jones, of St. Thomas', mentions the following case:—

'A narrow blade of glass having entered the palm some two inches through the annular ligament, was withdrawn by the patient herself.

'The wound completely healed in ten days.

'Mr. Jones saw her on June 18, four weeks after the injury.

'On the outer side of the ball of the little finger there was a pulsating swelling the shape of a large walnut. The skin was thin, red, and shining over the surface. Much pain was complained of in the palm and along the ring and little fingers; the movements of these fingers were also much impaired.

'On pressure the sac was readily emptied, and the thumb might be passed without difficulty deep into the palm and beneath the annular ligament. Pressure on the ulnar above the wrist diminished, but did not completely control the pulsation. When combined with pressure on the radial all pulsation was arrested. Pressure on the brachial likewise arrested pulsation. Forced flexion of the elbow did not
seem to have sufficient control over the pulsation to trust to that treatment alone.

'By means of a tourniquet light pressure was kept on the brachial. A bandage applied from the hand upwards, and cork pads placed on radial and ulnar above the wrist, and the fore-arm flexed at the same time. A day or two later a cork pad was placed over the aneurism in addition. All pulsation had ceased in eight days, and the tourniquet on brachial removed, but the pads were kept on the radial and ulnar sometime longer. Pulsation did not return, and she left the hospital a fortnight later.

'When seen six months afterwards no trace of aneurism to be discovered, and the movements of the fingers not in the least impaired.'

An almost precisely similar case as regards treatment and result is recorded by Mr. Bainbridge of Tooting, in the same journal.

Professor Syme stopped pulsation in a traumatic aneurism in ball of thumb in twenty-four hours by pressure on radial. Pulsation never returned, and the patient completely recovered.

L. Parker, of Queen's Hospital, Birmingham, mentions two cases of palmar aneurism cured by pressure.

Liston cured a traumatic aneurism in ball of thumb by ligature of brachial.

These seventeen cases are the only ones I find alluded to in the text-books of surgery or medical journals after a pretty extensive search. I have arranged them thus:—

<table>
<thead>
<tr>
<th>Method</th>
<th>Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pressure</td>
<td>9</td>
</tr>
<tr>
<td>Ligature</td>
<td>2</td>
</tr>
<tr>
<td>Injection</td>
<td>1</td>
</tr>
<tr>
<td>Sac opened</td>
<td>5</td>
</tr>
</tbody>
</table>

Total 17

The result in the last five cases being as follows:—

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cured</td>
<td>1</td>
</tr>
<tr>
<td>Died of bleeding</td>
<td>3</td>
</tr>
<tr>
<td>Case brought before the Society</td>
<td>1</td>
</tr>
</tbody>
</table>

Total 5
Now in wounds of the palmar arch, undoubtedly the artery, if possible, should be tied at the wounded point.

This may be done if the original wound is sufficiently free to admit of the vessel being easily found.

Butcher of Dublin, and Liston, are amongst surgeons of authority who have recommended enlarging the wound to search for the wounded vessel.

Astley Cooper, Roux, Abernethy, and Skey, all recommended ligature of both arteries above the wrist—simple means having failed—rather than enlarging a small wound in so complex a structure as the palm of the hand.

These eminent surgeons seem to have trusted little to pressure; with the exception of Butcher, to whom the thanks of the profession are due for a most instructive case published by him in the ‘Dublin Medical Press,’ July 14, 1852.

Now, sir, there are a large number of cases on record where obstinate bleeding has been effectually stopped by the Hunterian operation. But what is of far more interest, there are a certain number of cases recorded in which all haemorrhage was arrested by simpler means, namely, by pressure. Moreover, the success of this would appear to be directly in proportion to the care and skill displayed in its adjustment.

Dr. Arnott, from his own experience, records one case of primary and three of secondary haemorrhage occurring from the palmar arch arrested by pressure.

They are all somewhat similar. As a typical case illustrating Arnott’s method of treatment, I will select one recorded by him in the ‘Lancet’ (Oct. 30, 1858).

The man wounded his palm by falling on to an iron rake. An abscess formed and was evacuated. On the eighth day, on Arnott being hastily summoned, he found the patient had fainted, and was lying on the floor in a pool of blood.

He says, ‘I applied a graduated compress to the wound in the usual way, and was about to leave the house, but was recalled by the patient telling me blood was coming through the dressings. It soon began to drop on the floor briskly.

‘I reapplied the tourniquet, undid the wound, did it up afresh more effectually, and waited half an hour without further haemorrhage occurring. He passed the night pretty comfortably. Next morning at eight the bleeding recurred.

‘The case now assumed a serious aspect, and something
summary was needed. Ligature of the brachial I had been taught to regard as the one thing needful.

'The operation was proposed, but the patient declared he would rather die than submit to it.

'It then occurred to me to attempt to obstruct the radial and ulnar trunks by compression. The fingers were accurately bandaged, and the wound filled with a graduated compress, and the radial and ulnar obstructed by continuing the bandage upwards and adjusting over them two halves of a cork covered with lint. The tourniquet was gradually loosened, but retained ready for immediate reapplication.'

All haemorrhage was from that time completely controlled, and the man recovered without a bad symptom.

'Showing the care and caution in the adjustment of a compress, and illustrating an important feature in its application, I will read an extract from the case I have before alluded to by Butcher of Dublin.

'The wound was inflicted by a penknife thrust deep into the palm. The patient, on admission, was blanched and excited. On drawing the wound aside, the bleeding orifice could not be seen at all.

'The artery being compressed, each finger was separately rolled with a narrow bandage from its extremity to its articulation with the metacarpal bone. The thumb was similarly treated. Then even flat compresses of folded lint were adapted to every part of the palm of the hand, excepting to the extent of a half-crown piece, the centre of which was the wound.

'This space was left for special pressure, applied in the following way:—The finger being cautiously removed from the divided artery, a very small dorsal of lint was passed into the wound so as accurately to take its place; over this another and another, each larger than the former one, until a well-formed conical compress not only filled the wound with its apex but was elevated by its base a few lines above the surrounding tissue.

'When so adjusted, not the slightest oozing appeared. A roller was put on to maintain the compress in the palm and carried up to the wrist, when oblong compresses of lint were placed over the ulnar and radial arteries and retained with such force as to moderate but not to arrest the amount of blood circulating in the vessels. The roller was continued up to the elbow; outside this a splint was placed on the front
of fore-arm and palm, and padded in such a way that the
gfingers were flexed upon it in order to slacken the palmar
fascia and permit the compressing pad to lie in undisturbed
contact with the artery.'

Butcher lays great stress on thus relaxing the palmar
fascia and permitting the compress to lie in more direct con-
tact with the deeper structures.

This will prevent the blood becoming extravasated into
the fine reticular tissue lying between the fascia and sub-
jacent vessels.

Butcher states that had he found blood oozing through
this compress applied so carefully he should have enlarged
the wound by passing a curved director under the fascia,
and after lifting it from the parts beneath slit it up on the
director.

The cases recorded by Arnott and Butcher so successfully
treated by pressure in the first instance may well be con-
trasted with the following celebrated case, when pressure was
defferred till a last resource. ('Lancet,' 1855.)

The patient wounded the palm on its ulnar side with a
knife on Jan. 17. Free haemorrhage occurred. After bleed-
ing had recurred two or three times, a bleeding orifice was dis-
covered and tied in the wound. Twenty days later he was
admitted into St. Bartholomew's, and Skey tied the radial
and ulnar at the wrist. On Feb. 12 the ligature came away
from the proximal end of ulnar, and haemorrhage fol-
lowed. He then tied the ulnar in the middle of fore-arm.

On the 16th haemorrhage returned with violence from the
ulnar. The brachial was tied below the inferior profunda.
On the 18th the last operation having failed to arrest haemorrhage, the axillary was tied—five arteries had now
been tied in ten days. On March 1, twelve days after last
operation, a sudden burst of bleeding from the axillary left
the patient almost moribund. Skey now considered it too
late to amputate, the man being so reduced from loss of blood.

Looking now entirely to saving the man's life, and disre-
garding the chance of gangrene, he bandaged the arm tightly
from the hand to the shoulder.

No further bleeding took place after this, and the man
completely recovered, with a useful arm.

In the case forming the subject of this paper the writer
gave what he considered a fair trial to pressure, but had he
previously been aware of Butcher's paper he would have
given it one more trial before tying the arteries, although
from the subsequent history of the case it would seem improbable that that course would have resulted successfully.

Pressure was applied after the manner described by Mr. Erichsen, that is with the hand straightened. But since so much stress is laid by Butcher upon the application of pressure with the hand bent, it is to be regreted that that method was not tried.

It is possible that others may draw from the cases noted a conclusion different to my own, and in drawing such a conclusion they may be assisted by facts and experience of which I have no cognizance.

It would appear to me, however, that the foregoing cases tend to the following conclusions:—

It is better, except as a last resource, to avoid opening traumatic aneurisms of the palm, since pressure appears to be particularly successful in a large proportion of these cases.

In the case of bleeding from the arteries of the palm, well adjusted pressure not only on the wound, but also on the radial and ulnar arteries, should be tried, and further that the arm should be carefully bandaged from the fingers to the shoulder. This pressure failing, the brachial artery might be tied.

If the compression of the radial and ulnar arteries fail to arrest the bleeding, it would appear unlikely that any advantage would be gained by their being ligatured, for these arteries can be effectually stopped by pressure, and their accompanying veins are so small, that slight obstruction to the return of venous blood, by means of such pressure, would occur; and should bleeding continue whilst these arteries are compressed, it would seem probable that there existed abnormally large interossei or arteria comes nervi mediani.

It is not desirable to compress the brachial artery for long, since such pressure cannot be kept up without interfering with important veins, in which case the stagnation of blood interferes with those reparative changes on which alone the occlusion of the wounded vessel must depend.

Pressure on the brachial could never have had a more thorough and careful trial than in the case reported; but it signally failed in permanently arresting the haemorrhage.
XXVII.—Description of the Sarcotome, an Instrument for painlessly cutting through the soft Tissues of the Body.
By W. Ainslie Hollis, M.D., Cantab. Communicated by G. W. Callender, F.R.S. Read May 8, 1874.

This instrument weighs somewhat less than an ounce and a half, and consists of the following parts:

1. A hollow metal cylinder about 3 inches long, and with a diameter of a quarter of an inch, closed at one end by a short rod and having a series of graduations along its barrel. The other extremity is wormed externally to receive a screw, and is perforated by a narrow opening about half an inch long in the direction of the long axis of the cylinder.

2. A helically coiled spiral spring of hardened steel; the spring is of such size as to pass readily into the cylinder, where its free extremity touches the closed end of the tube. To the other end of the spiral a small metal disc is fitted at right angles.

3. A ring of metal about an inch broad, and of such diameter as to pass freely over the cylinder. To the outer surface of this ring is affixed a metal cross-bar with a screw-nut in the centre.

4. To the rod at the end of the cylinder is fitted a metal cup, the free extremity of which consists of a curved metal ring so arranged that one opening is terminal, the other lateral.

5. Lastly, a ring of metal with milled edges and wormed internally as a screw, fits on the male-screw of the cylinder. This ring is called the 'screw-ring.'

When arranged for use, the spring is placed within the cylinder and the disc is connected by means of a small screw, which passes through the lateral openings in the cylinder, to the outer band of metal. The spring is then forced up as high as it will go into the body of the cylinder by means of the 'screw-ring.'

After passing a waxed thread or other ligature around the parts to be severed, the surgeon puts both its ends through the terminal hole in the cap and out again at the lateral one. The cap is then fitted on the cylinder, the ligatures are passed two or three times in a figure of 8 loop around the cross-bar of the sliding tube and are fastened by the central screw-nut. The screw-ring is next removed,
and the whole force of the spring (amounting in this instrument to a pressure of 11 lbs.) is directly exerted upon the ligature. During the course of an operation it may be necessary to re-adjust the ligatures occasionally; in such a case the spring is compressed by the 'screw-ring,' the ligatures are released from the screw-nut, tightened, refixed, and the spring is again released by the removal of the screw-ring.

XXVIII.—Case of Pyæmia, the Cause of which remained undiscovered. Termination by Thrombosis of the Veins of one Leg.* By John W. Ogle, M.D. Read May 8, 1874.

HENRY P., æt. 20, a somewhat delicate-looking man, who had nevertheless enjoyed generally good health, and been temperate as to habits, was admitted into St. George's Hospital Jan. 14 last.

He had never had syphilis, gout, or rheumatism. It appeared that about four or five days previously he had been affected by pain in the back and abdomen and head, with shivering, and a degree of sickness. These symptoms had continued ever since. There had been no delirium.

On admission there was some tremor of the muscles of the face and hands, and slight pain in the head. The tongue was very furred along the centre, its edges being indented, and the bowels were confined. The pulse was 110 per minute, and feeble, but regular. The cardiac sounds and impulse were quite natural, as were also the sounds connected with the action of the lungs. There was no cough. There was no diarrhœa, but some discomfort was caused by pressure upon the abdomen.

No eruption was found on examining the skin, and no albumen detected in the urine.

Temperature in the evening 105°.

Strong beef-tea and milk were given, and the effervescing citrate of ammonia salines every four hours; also castor-oil with a little laudanum.

On the morning after admission the temperature was 103°.

* The chart of the daily temperature and pulse is given at the end of the case.
The bowels had been opened and the evacuations were not over light in colour, nor in any way noticeable, having certainly no appearance of 'fever' stools. The patient had slept for two or three hours. The urine was much charged with lithates.

On the 17th the pulse was 96 per minute. There was no tenderness of the abdomen. The urine, which was acid, was found to contain a small amount of albumen, in addition to the lithates.

Port wine was ordered, in addition to other things, on the 16th.

The bowels remained confined until opened by castor-oil on the 20th, at which time no spots or eruption had appeared on the surface of the body. The pulse was 86, quiet and regular, in the morning.

At 2:30 p.m. a distinct rigor came on, and during its occurrence the temperature was 103°.

On the 22nd the patient said he was much better; the tongue was somewhat cleaner, but still much furred.

On the 23rd the pulse was 78 and quiet, and the patient felt hungry.

On the 24th (i.e. fourteen or fifteen days since the symptoms of illness were supposed to have set in, and ten days since admission into hospital) the patient said he still felt much better. The tongue was almost clean, and he had had two evacuations from the use of castor-oil. The abdomen was free from any tenderness, and no albumen existed in the urine. The pulse had, however, risen to 112, and he complained of slight pain in the right shoulder. The temperature had risen to 104·2°.

Jan. 26.—The pain in the right shoulder was still more complained of, and when examined the joint was ascertained to be tender to the touch, and to present unnatural roundness and fulness. The tongue was a little more furred. The salines which he had taken were continued, with 3 grains of quinine added to each dose. Pulse 104. Temperature above 104° morning and evening.

28.—Much pain and loss of power in the hands were complained of. No particular action or acid smell of the skin was noticed. Pulse 99. Pain in shoulder as before. Temperature in morning nearly 103°, and above 102° in evening.

29.—Tongue more furred. Temperature nearly 105° in morning, and above 102° in the evening.

30.—The pain in the shoulder was much less, but that in
the wrists was more severe, and some fulness and swelling of the wrists had come on. Profuse sweats had existed in the night, and in the course of the morning a severe rigor came on, leaving him complaining of cold. Pulse at this time 61, and regular. Six ozs. of brandy were given, in addition to the wine. Temperature in the morning between 96° and 97°, and in the evening 105·8°.

Feb. 1.—Pulse regular, 70. Wrists very painful and quite stiff. Continuing with the 3-grain doses of quinine.

4.—Had a rigor in the night. Much pain in the left wrist, to which the belladonna liniment was applied with benefit. Temperature in the morning above 101°, and in the evening above 102°.

6.—Had a rigor in the night and again at mid-day. Pulse 100, irregular. Temperature about 101° both morning and evening.

7.—Had another rigor. Pain and stiffness of the wrists the same. Temperature in the morning about 104° and in the evening about 99°.

8.—Bathed in extremely acid sweats. Temperature in the morning about 100° and in the evening nearly 102°.

13.—There had been no return of rigors, but perspiration had been profuse. The pain in the wrists, especially the left one, continued, and this joint was much swollen, and evidently the seat of some effusion. No grating, however, between the bones was detected. In the morning the temperature was about 104°, and in the evening above 105°.

15.—Another slight rigor occurred in the morning. Pain in the left wrist very acute; somewhat relieved by poultices. Some degree of pain in the left shoulder.

17.—Countenance very anxious; otherwise the same. Pulse 120. Temperature in the morning above 100° and in the evening a little above normal.

20.—No additional rigors since the 15th. Mr. Pick saw the patient with me, and like myself considered the case to be one of pyæmia. The left hand and wrist were fixed in a splint.

21.—Another rigor took place. The patient was put on bark and quinine.

On the 24th the temperature was between 98° and 99° in the morning, and above 105° in the evening; and on the 27th it was above 106° in the morning, and above 103° in the evening.
28.—The wrist free from pain; the temperature a little above 98° in the morning, and above 99° in the evening.

March 1.—Wrist still free from pain, and capable of being easily moved. Pain, however, in the region of the left hip-joint and upper part of the thigh was complained of. In the morning the temperature was a little above 98°; and almost as low as 97° in the evening.

4.—The pain had greatly increased in the thigh, which was much swelled. Temperature a little above 98° in the morning, and a little above 100° in the evening.

7.—The left thigh was exceedingly painful, and the thigh was swelled down to the knee and very tender when touched. No part of the skin was, however, at all reddened. The tongue was brown and dry in the centre, and very red at the edges. Appetite good, and he ate the mutton-chop and took the generous diet and stimulants which had been allowed him. Temperature in the morning above 101°, and in the evening above 102°.

14.—An abscess appeared to be forming in the left hip-joint, and evident fluctuation existed. Diarrhoea also had come on, requiring chalk and opium and opiate enemata. The hands and shoulders were now free from pain. Temperature in the morning above 100°, and nearly 102° in evening.

Fluctuations in the temperature after the above date will be seen by the appended Temperature Chart.

On the 25th Mr. Pick talked of the necessity for opening the abscess, which apparently was beginning to 'point.' This, however, gradually subsided, and became less painful; and by degrees the sweating subsided. Nourishing diet with wine and supporting treatment were continued, and by degrees the patient improved, got stouter, and the left hip and thigh gradually diminished in size and the pain deserted him. Soon afterwards he wished to leave the hospital, and was detained against his will, as he observed 'only a little stiffness remained.'

At the present time (May 8) he is looking stout and well; and were it not that the left wrist is rather thickened and tender on pressure, and the left thigh and leg larger than the other, presenting a degree of hardness in the neighbourhood of Poupart's ligament, he would have the entire appearance of health. Measurement shows that the left thigh at the upper part, and also the left calf, are about an inch thicker than the corresponding parts of the opposite
DR. OGLE's CASE OF HENRY PRICE \textit{Æt} 20 ADMITTED JANUARY 14th 1874.

\begin{itemize}
\item \textbf{Pulse Temp.}
\item \textbf{Temp.}
\item \textbf{Fever}
\item \textbf{Diarrhea}
\end{itemize}

\textbf{Day of Discharge:} 3.25
Dr. Ogle's Case of Pyaemia. 143

limb. No enlargement of veins at any part of the surface of the leg is noticeable. I have detained the patient in the hospital in order to prevent any excessive use of the leg or any accident to it which might tend to produce detachment of any of the plugs which no doubt occlude the veins of the limb.

Comments.—The fluctuations in the character and course of the above case at the outset were specially worthy of note: viz. the resemblance at first to continued fever, then the suspicion of rheumatic fever, prior to the establishment of symptoms such as rigors, and certain temperature-changes indicative of pyaemia.

The obscurity as to the origin of the case was another point of much interest; for neither could it be made out that there had been any injury or even abrasion of the skin, or mucous membrane, nor had there been any history of suppuration either deep or superficial; nor had there been any gout, rheumatism, or syphilis, &c.; nor was there any history of fever during which ulceration of the intestine or plugging of veins might have occurred and given rise to septicemia. A comparison of dates shows that many weeks elapsed, after the setting-in of the disease, before the affection of the thigh began; a trouble which was evidently accompanied by occlusion of the veins of the part, as indicated later on by the swelling of the limb, and the hardness in the neighbourhood of Poupart's ligament. The question naturally arose, whether the case should be considered as one of 'spontaneous' pyaemia—a question which does sometimes, though not often, suggest itself, as was well exemplified in a case which occurred at St. George's Hospital in 1864, as related in the 'Lancet' for that year.*

From the accompanying chart of the temperature and also (for some days) of the pulse, it will be seen what variations the thermometer showed; also how far the pulse was from corresponding in frequency with the temperature; having been at one time only 61 when the thermometer stood at 105°.

* See Vol. ii. p. 690 (December 17th). The case was under the care of Dr. Pitman. After death, pus was found in the joints as well as in the substance of the heart and kidneys. The reporter also refers to a doubtful case of Dr. Fincham's, which occurred in Westminster Hospital, and which was suspected of having been spontaneous in origin.

The idea of the contagiousness of consumption is very old, and the belief in it is rather general in southern countries, while it has few adherents amongst the medical men of England and Germany, and scarcely more in France. The communication of the disease by the ordinary intercourse, nursing included, between the sick and the healthy, is probably exceptional, though I do not by any means regard it as impossible. The facts communicated by Villemin and others in France are important; and Drysdale has likewise, a few years ago, related several instances of probable communication.

The nature of the communication of the disease on which I seek your impartial discussion, viz. that from the tainted husband to the healthy wife, seems to me to possess peculiarities of its own.

More than twenty years ago my attention was forcibly directed to it by two striking instances. Since then I have followed up the subject, but have hesitated in giving publicity to my views, partly on account of the difficulty of a satisfactory explanation, in accordance with our general views on consumption; and partly, also, from the fear of causing distress to some persons. Some recent experiences, however, induce me to bring the question before the Clinical Society.

With your permission I shall first give an abstract of my personal experience. I have the history of 68 persons, male and female, who, with a more or less pronounced consumption taint, have married healthy partners. One or several of the partners of 10 out of these 68 cases became consumptive. The question, however, takes a different aspect if the originally tainted husbands and wives are considered separately. Of the 68 persons 39 were husbands, 29 wives. Only one of the husbands of the 29 wives became diseased, while the wives of 9 out of the 39 husbands became affected. These 9 husbands lost 18 wives, viz. 1 lost four wives, 1 lost 3, 4 others lost 2 each, and 3 only 1 each.

In placing the outlines of the cases before you, I shall confine myself, to save time and space, to the shortest descriptions.
Case I.

J. had lost his mother, two brothers, and one sister, from consumption; had twice himself had hæmoptysis when about 20 and 21 years old; became afterwards a sailor; felt perfectly well after his 25th year; married when 27—

1. A lady, belonging to a healthy family, who enjoyed herself good health till towards end of her third pregnancy, when she began to cough and emaciate. She died from consumption after her third confinement.

2. After a year he married again an apparently healthy woman who, after the first year of married life, began to cough, had hæmoptysis, and died from galloping consumption.

3. The third wife belonged to an exceptionally healthy family, having both her parents, four brothers, and two sisters living; and in good health. She was 25 when she married, and continued to enjoy good health until her second pregnancy, when she began to cough and become feverish; had twice hæmoptysis; and when I first saw her, seven weeks after the second confinement, she had extensive affection in the upper portion of both lungs, hectic fever, and profuse perspiration. A month later she had serious pulmonary haemorrhages and died soon after, about eighth months from the appearance of the first symptoms.

The post-mortem examination manifested the signs of pneumonic phthisis combined with tuberculosis, to use the late Dr. Addison’s expression, who had seen the case with me.

4. The fourth wife, whom I likewise attended, had no indication of phthisis in her family, and was at the age of 23, when she married, in perfect health. About thirteen months later, three months after her first confinement, which had passed off quite well, she began to cough, had pyrexia, which was soon followed by marked signs in the right and afterwards left apex, by hæmoptysis and slight pleuritic effusion. On a journey to Melbourne she temporarily improved, but had severe haemorrhage at Melbourne, and died soon after her return to England, nine months after the beginning of her illness. The post-mortem examination showed extensive pneumonia and tubercular affection in both lungs, and also tubercles in the intestines, on the spleen and liver.

On two occasions, in 1854 and 1857, after the death of the vol. VII.
third, and during the illness of the fourth wife, I had the opportunity of examining J. His general health was perfect, and he had, he assured me, no cough, but brought up a slight amount of mucus every morning. The upper part of the left side of the chest was flattened, the precussion over the same region less clear than on right; inspiration indistinct, with occasional rhonchus and prolonged expiration. He did not marry again, thinking it 'certain death' for the woman of his choice; continued in good health in his seafaring occupation until 1869, when, through a severe fracture, he was laid up for several months; he then began to cough, the formerly healthy right apex became affected, and consumption regularly developed itself, leading to death in 1871, when the post-mortem examination disclosed as well the cicatrized condition of the former seat of the disease, as also the recent affection.

I may here mention that, having had the advantage of consultation in the illnesses of the third and fourth wives with Dr. Addison, as well as Dr. Hughes (of Guy's Hospital), I discussed the question of infection through the husband with them. They both regarded the occurrence as very strange, but hesitated in accepting the view, because the nature of the disease was against the idea of infection—the more so as the husband might be regarded as cured.

Case II.

W., belonging to a consumptive family, having lost his father and two sisters from consumption, had haemoptysis and other pulmonary affections at the age of 19; was sent three successive winters to Venice, and afterwards considered himself quite well. He married at the age of 26—

1. A perfectly healthy young woman, belonging to a healthy family. She began to cough towards the end of the first pregnancy, had haemoptysis soon after the confinement, and died four months later of consumption.

2. He married again two years later, in 1852, a young lady at 21, herself in perfect health, but not quite free from family disposition. She continued in good health during the first pregnancy, and also afterwards till towards the middle of the second pregnancy, when she had several 'inflammatory attacks on the chest,' and afterwards two attacks of haemoptysis, after which she never rallied, but died about three months after the second confinement from 'rapid phthisis.'
3. He married a third time, after a short interval. The lady was healthy and strong; both her parents, four sisters, and one brother, all in good health. About three months after the second confinement she had an 'inflammation on the lungs,' from which she did not entirely recover. When I saw her for the first time, seven months after the beginning of this 'inflammation,' both lungs were affected, the right more extensively than the left; there were every day two attacks of high pyrexia, frequent perspirations, diarrhoea, and great emaciation. Tubercular meningitis supervened soon after I had first seen her, and death occurred about eight to nine months after the first signs of illness.

The post-mortem examination showed extensive disease of the right side, with general miliary tuberculosis. Uterus free.

The husband's general health appeared to be good. He was able to walk long distances, but suffered from dyspepsia when engaged in-doors. The supra- and infra-clavicular regions on the right side were flattened, slightly dull on percussion, respiratory murmurs almost absent, with occasional dry rhonchus.

He suffered from severe hypochondriasis with self-reproach for several months after the death of his wife. He then recovered, lived much in the open air, but had typhoid fever in Germany, after which pleuro-pneumonia came on in the right lung, and death from consumption six months after the typhoid fever.

Again I had, in the treatment of the third wife, the benefit of Dr. Addison's opinion. His former view regarding the impossibility of infection was to some degree shaken, but ultimately he thought it was more likely to be a curious coincidence, though he was not quite certain.

Case III.

Z., without family history. Had twice haemoptysis when about 19; was then ill for many months, but recovered, he said, completely, being exposed to every kind of weather from morning till night without harm. He married when 23 years old—

1. A strong and healthy woman, æt. 20, without family predisposition. She fell ill after the first confinement, and died within five months from 'rapid consumption.'

2. He married again about two years later a healthy person, æt. 27, belonging to a remarkably healthy family. Eighteen
months after marriage, three after her first confinement, she began to cough and emaciate. The cough never altogether ceased. She became again pregnant two months later; and when I first saw her, she was in the eighth month of pregnancy, very emaciated, a picture of the last stage of consumption, the greater part of the left side being affected, and exhibiting the signs of cavities and infiltration, the right lung being likewise diseased, though not to the same degree; profuse perspiration, diarrhoea, hectic fever. She was confined a month later, and died three weeks afterwards. The post-mortem examination manifested various conditions of recent subacute phthisis, viz. grey infiltration of different colour and consistency, fresh cavities of irregular shape, without any lining, scattered yellow and grey tubercles. Uterus free from tubercles.

Z., the husband, considered himself well, but the upper part of the right side was somewhat flattened. The resonance on percussion was decidedly imperfect as well in the supra- and infra-clavicular as in the supra-spinal region. The inspiration over the same localities was indistinct, the expiration prolonged. Six years later, in 1865, after his return to Germany, he had again severe hæmoptysis, and died in 1866. I have no report of the post-mortem examination.

Case IV.

A., with consumptive family tendency, had pulmonary affection at the age of 22. Spent two winters at Madeira; felt afterwards well. Contracted syphilis when 29; considered himself cured when he married, âœ. 32—

1. A healthy girl, âœ. 22, belonging to a healthy family. She manifested signs of syphilitic affection six months after marriage, and was confined of a dead child some months later. No more symptoms of syphilis were observed after 1854, and she had a healthy child in December 1855. In May 1856 she began to cough. During the same summer she became much emaciated, had often diarrhoea, and was always feverish. When I saw her first, in November 1856, there was extensive affection of the upper parts of both lungs, high fever, diarrhoea, profuse perspiration, and excessive emaciation. Subsequently she had two attacks of hæmoptysis, and died in February 1857, i.e., nine months after the commencement of the cough.
The husband had signs of old disease in both apices, but more so in the left, viz., flattening, dulness on percussion, and mucous rhonchus. He had also occasional cough in the morning, and now and then the expectoration was tinged with blood; but he had no fever, and the general health and appearances were perfectly good.

He married again, in 1859, a lady æt. 32. The wife remained well, but had no children; while he himself, after several years of hard sedentary work, began to develop the signs of active consumption in 1865, and died in 1866. The post-mortem examination manifested tense slate-coloured, contracted tissues studded with cretaceous masses in both apices, and fresh grey infiltration, with numerous yellow and grey tubercles in other parts of the lungs.

Case V.

C., a member of a consumptive family, was himself considered consumptive from his 19th to his 20th year, after which period he felt well, and married when 25—

1. A healthy girl, æt. 22, who became consumptive during her third pregnancy, had three times haemoptysis, and died soon after the third confinement, the illness having lasted about seven months.

2. He married again after a short interval a perfectly healthy lady, æt. 21, quite free from family disposition. His wife remained well for about 14 months, viz., till towards the end of her first pregnancy, when she began to cough. The cough increased after the confinement and was attended with rapid emaciation, frequent profuse perspirations, and constant feverishness. When I saw her, four months after the beginning of the cough, the right lung was extensively affected, and the upper part of the left was not quite free. She died seven months after the beginning of the disease. The post-mortem examination disclosed numerous small irregular cavities in both lungs, tubercles in the lungs and intestines. Uterus free.

C., the husband, had distinct dulness and mucous rhonchus at the supra- and infra-clavicular regions of both sides. The consumptive symptoms became more active in 1861. He went to Australia and died there, without having married again.
Case VI.

R., belonging to a very consumptive family, had serious haemoptysis at the age of 20, and to a less degree a year later. After having spent three summers at a whey-cure establishment in Appenzell, he felt quite well, and married when 24 years old—

1. A healthy girl of 18, who had abortion with much loss of blood six months after marriage; then began to cough, and died consumptive fourteen months after marriage.

2. Two years later he married again, a healthy lady, æt. 18, belonging to a healthy family. She had her first child eleven months after marriage, her second fifteen months later. Five months after the second confinement she began to cough. Loss of appetite, feverishness, frequent perspirations, accompanied the cough. Emaciation soon manifested itself; and when I first saw her, in June 1857, five months after the beginning of the cough, she offered a type of florid phthisis. Both lungs were affected; there were only short remissions in the almost continuous fever; laryngeal affection had supervened. She followed an irresistible longing to return to her home in Germany (Mannheim), where she died in August of the same year, seven months after the commencement of the cough.

The husband had slight dulness with occasional rhonchus over the third and fourth intercostal spaces. Had only rarely a slight cough, was well nourished, and appeared to all his friends perfectly healthy. He did not marry again. In 1862 he became more ill, and died from acute symptoms of pneumatic consumption (after three months’ illness) at Baden-Baden, under the care of Dr. Guggert.

Case VII.

W., belonging to a family with consumptive tendencies, ‘suffered from the lungs’ from his 17th to 19th years. Felt afterwards well, and married at the age of 29—

1. A healthy young lady, æt. 23 (without family disposition), who began to cough towards the end of the second pregnancy, and died soon after the second confinement from galloping consumption.

2. He married again at the age of 33, a lady æt. 24, who had always enjoyed good health and belonged to a healthy
family. She remained well until about fifteen months after her marriage and two after first confinement. She then began to cough, to lose her appetite, and to become feverish. Hæmoptysis occurred three months after the first symptoms of the illness; diarrhœa and excessive perspirations succeeded. In January 1865, when I saw her, viz., seven months after the commencement, both lungs were considerably affected, the emaciation was extreme. In February 1865 she had another severe attack of hæmoptysis, and died in March.

The post-mortem examination disclosed fresh cavities in both lungs, and in short the phenomena of a subacute case of phthisis. The abdominal and pelvis organs were free from disease.

The husband, who at this time considered himself perfectly well, had distinct flattening and dulness corresponding to the left apex. He did not marry again, and died of consumption in 1869.

Case VIII.

G., a member of a consumptive family, wintered, on account of his lungs, from his 19th to 21st year, in the south of France, and spent afterwards three years at the Cape. After the 24th year he had no marked symptoms. He married at the age of 30, in 1861—

1. A healthy lady, æt. 26, who remained well till January 1864, some months after her second confinement, when she began to cough and emaciate. In the autumn of 1864 considerable local disease was discovered, and a winter at Mentone was recommended. When I saw her, in May 1865, the patient had all the symptoms of advanced consumption, and she died in June of the same year.

The husband had marked flattening, some dulness, and occasional rhonchus over the apex of the left lung; but he had rarely any cough, and his general health was good.

2. He married again in 1867. His wife had remained well when I saw her in 1869, but she had no children, and the husband died of a fresh outburst of phthisis in 1871, the apex of the left lung containing some cretaceous masses embedded in fibrous slate-coloured tissue, the lower part of the left and the right lung giving evidence of the more recent disease.
CASE IX.

S., again a member of a consumptive family, had hæmoptysis in his 18th 19th, and 20th years; spent several summers (from 1858 to 1861) at Heiden, Gais, and Weissbad, for the milk and whey cure. He felt afterwards well, but was rather short-breathed when he married in the autumn of 1867, aged 27—

1. A healthy lady, aet. 21, belonging to a healthy family. She was confined in March 1866, and remained well till August of the same year, when she began to cough and emaciate. In March 1867, when I first saw her, the local symptoms were most marked in the upper portion of the left lung, but the right apex was likewise affected. There was a total loss of appetite, frequent diarrhœa, profuse perspiration, and she had almost daily two attacks of pyrexia. Hæmorrhage occurred in April, and she died in the same month.

The post-mortem examination disclosed the phenomena of advanced pneumonic phthisis with tubercles. Uterus free.

The husband had old quiescent affection of both apices, and some amount of emphysema in the lower parts, especially of the left lung. He married again—

2. In 1868, a healthy lady, aet. 31, who had children in 1869 and 1870, and remained in good health.

The husband began to cough much in the winter of 1871, and went, in 1872, to the Cape, where he enjoys better health.

The first question which appears to offer itself is whether the preceding cases support the probability of the communicability of consumption from husband to wife. In my limited field of observation, of the 39 diseased husbands, the wives of 9 of them became consumptive after marriage; but as several married repeatedly, I must take the number of marriages between diseased husbands and healthy wives as my basis, and this would show that, in 51 such marriages, 18 wives became consumptive after marriage. In comparing this with 51 marriages between healthy husbands and wives, we certainly do not find such a proportion of consumption amongst the wives.

The number, however, on which my experience is based is so small that no inference can be drawn from it with any degree of certainty; for what occurred to me may have been to a great degree, or even entirely, accidental. It is on this account that I venture to ask for the greater experience of
the members of the Society, and I hope that a better proportion for the wives will be obtained as regards the chances of immunity. I fear, however, that some risk does exist, though perhaps much smaller than would appear from my own experience.

2. Assuming the possibility of communication, the question would arise—How, or under what circumstances, does it seem to occur?

The fact that in 29 marriages between consumptive wives and healthy husbands, only one husband became consumptive, while in 51 marriages between consumptive husbands and healthy wives 18 wives became consumptive, is such as can scarcely be explained by the ordinary means of intercourse. I admit that the wife runs greater risk than the husband through nursing her husband much more closely, and through being much more in the atmosphere of the sick room; but this consideration does not explain the great preponderance of infected wives in the present case, for the husbands were, with scarcely any exception, regarded as in good health; not one of them, at all events, was confined to his house, and still less to the sick room.

A much more likely cause would be given in the seminal fluid, either by impregnation and infection through the fcetus, or by the mere absorption of the fluid.

In discussing the subject with Dr. Southey, he thought the infection through the fcetus as the most probable source of communication. On examining the preceding notes I find that of the 18 wives who died consumptive, 16 had borne children to their husbands, one had had no child, but a miscarriage; about one my notes are imperfect on this point. Of the 33 wives who remained free—viz. 30 who were their husbands' first and only wives, and 3 who were successors to wives deceased from consumption—14 have children, 10 are childless, about 9 I have no information.

These data are evidently inadequate for drawing an inference, but as far as they go they seem to indicate that those wives who do not become pregnant are more likely to escape infection.

I may here mention that the uterus, in three cases, is described as free from disease, while in the others my notes do not specially mention the organ. There seems to be no reason to assume that the uterus was first affected, and that the consumption was secondary to a local affection of the womb, in the same sense as general miliary tuberculosis is often secondary to local cascous affections.
3. It has been suggested to me that possibly the infecting husbands were syphilitic, and that the deaths were caused by a kind of syphilitic phthisis; but I find that only one of the husbands had been decidedly syphilitic, and this husband’s first wife was the only one who manifested indubitable signs of syphilis; while about four of the husbands I am as much as sure that they never were syphilitic. Besides, the character of the disease of the wives and the post-mortem appearances were not in favour of the syphilitic view.

4. A remarkable point in the diseases of the wives is their great rapidity, the duration not exceeding eighteen months in any, and being below twelve months in five of them. A high degree of pyrexia, great tendency to haemorrhage, to profuse perspiration, and to diarrhoea, were much more prominent symptoms than in the average cases of phthisis. The post-mortem examination, where it could be made, manifested extensive soft grey infiltration or pneumonic softening, numerous small irregular, newly-formed cavities, yellow and grey tubercles.

5. In strong contrast with this rapid course of disease in the infected wives, the affections of the husbands were quite chronic, stationary and apyretic, apparently the remains of former attacks. In all, however, the phenomena were on examination sufficiently manifest to allow the diagnosis of chronic phthisis, and in seven out of nine there was a marked family tendency. All except one died subsequently from phthisis, though some only after a long interval.

You will have observed that, in at least two of the cases, the husbands had at last formed the idea that in re-marrying they would expose the wives to great danger. This suspicion, however, seems to be exceptional; and I know that quite recently re-marriage was recommended by medical men to a widower with a phthisical taint who had lost three wives from rapid consumption, and to another who had lost two; and I am sure the medical advisers would not have acted in this manner had they had any idea of the communicability of phthisis from husband to wife.

It therefore appears to me desirable that this question should be discussed without prejudice, and that the fear of unwillingly giving pain should not stand in the way; for though it is a noble object of medicine to save pain, physical and moral, we have another duty even higher, which is to elicit truth.
XXX.—Case of Loud Musical Cardiac Murmurs, probably produced by Rupture of Aortic Valve. By Burney Yeo, M.D. Read May 22, 1874.

The patient presented to the society was a widower, 45 years of age—a railway clerk—who had applied as an out-patient to the Brompton Hospital, complaining of a loud 'singing' noise in his chest, which he first heard about a fortnight previously, on lying down in bed.

He stated that he had suffered from winter cough for fifteen years, and had also, during the last eighteen months, been troubled with rheumatic pains in the limbs. He had suffered from no other illness, and had never been laid up.

On being questioned he remembered that three weeks ago, while coming down some stairs one of his legs gave way, and he slid rapidly down a flight of stone steps, holding on all the time to the iron railing, and finally fell violently on his back at the bottom of the flight. It was soon after this accident—he does not remember if it was the same night—that on going to bed he heard this curious 'singing' noise in his chest, which kept him from sleeping.

On auscultating this patient's chest a loud musical bruit is to be heard over the whole pericardial region. At the apex it is heard as a bruit distinctly preceding the impulse—it becomes louder towards the base, and is loudest over the second right costal cartilage. It is diastolic, but is followed by a much fainter systolic murmur. The bruit, which had a remarkable musical tone, is very intense, and can be heard over every part of the chest, along the course of the vessels, in the head and the upper extremities. In a quiet room it may be heard distinctly at three feet from the patient's body. There is evident cardiac hypertrophy—the apex beats about two inches below and an inch and a half outside the nipple line. There are also signs of slight pulmonary emphysema, the respiratory sounds are fuller, the expirations somewhat prolonged, and the long resonance a little exaggerated, and there are patches of dilated capillaries along the costal margins.

The sudden observation of this murmur by the patient himself, immediately or soon after an accident in which
great muscular effort was put forth, the situation of maximum intensity of this murmur, its peculiar musical tone, and its wide diffusion, all point to a traumatic lesion of the aortic valve as its cause.

The patient has suffered no pain in the cardiac region, nor has he complained of faintness or any great difficulty of breathing. It is therefore inferred that the lesion is not an extensive one.

XXXI.—Case of Favus of the Scalp (Tinea favosa).
By Dyce Duckworth, M.D. Read May 22, 1872.

T. B., aged 12 months, a large, ill-tended, rickety baby, the youngest of four children, was brought to St. Bartholomew's Hospital, suffering from whooping-cough.

Dr. Church, who first saw the case, detected the presence of favus upon the scalp, and referred the child to the skin department of the hospital.

The mother stated that the disorder had begun upon the head three months previously. She first noticed 'a small red spot, which became covered with a white head.' Other similar spots appeared subsequently.

There were found several dirty yellowish masses of favus crust scattered over the head. Some were as large as a shilling, others not bigger than split peas. Some of the masses were a little depressed in the centre. There were several small bald patches upon the head, and some red stains, as of recent favus-scars, upon the face.

Careful cross-examination elicited nothing as to contagion. The other children were reported to be free of the disorder. A brother of the child was brought and carefully examined. There was not, it was stated, any similar case in the house. No cat was kept by the family, and the only cat in the lodging-house I found to be free of favus.

This is the sixth case of favus which has come under my observation during the last four years. Of these four were instances of favus of the body, three of them in the persons of two brothers and a sister. The disorder is extremely rare in London, and generally, I believe, in England. In Edinburgh and Glasgow it is of much more common occurrence. Out of about 5,000 cases of skin disease of all kinds seen
during the last four years at St. Bartholomew's Hospital, I have only met with six cases. Dr. M'Call Anderson, of Glasgow, met with no fewer than 156 in 10,000 dispensary cases. Professor Sanders, of Edinburgh, informs me that he believes the disease is now less common in Edinburgh than was formerly the case. One of my patients was a boy recently come from Germany, where the disease is frequently seen.

In the above case the nature of the crusts was readily detected, both by their aspect and by microspical examination of them and of the entangled hairs.

The treatment consisted of poulticing, followed by the application of parasiticides.


T., a large stock farmer, æt. 37, of medium stature and build, married; had always been accustomed to much out-door exercise, and, up to the date of this report, Dec. 13, 1872, had been a total abstainer from alcoholic drinks. He had always been healthy till about seven months previously, when, in May, 1872, he began to suffer severely from dryness of the mouth, excessive thirst, listlessness, debility, and great fatigue on exertion.

The patient also began to fall off rapidly in flesh, so that he lost a stone and a half in the course of a few weeks. His urine increased in quantity, to about six or seven pints daily. He did not suffer much from loss of sleep. His illness continued to progress in intensity during the succeeding three months, until the following August, when his medical attendant ascertained that he was suffering from diabetes mellitus. By the advice of this gentleman, and a physician consulted in the case, both of whom considered the disease to be of a severe form, the patient was now placed on a restricted diet, consisting of mutton, beef, gluten bread, wheat-meal biscuits (four or five of the latter daily), bran bread, about two pints of skim-milk, and two eggs daily; he was also allowed tea without sugar.

Under this regimen the more distressing symptoms of
the disease were considerably mitigated; but the specific gravity of the urine continued to range so high as from 1035 to 1040, the daily quantity being from four to five pints.

The patient first consulted me on Dec. 13 following (1872), when I found him in the condition just described; but he now complained of suffering much from cold, especially when in bed at night, having much difficulty in getting himself into a comfortable degree of warmth. He had, moreover, a thin, wasted, pale appearance. The urine passed in my presence had a specific gravity of 1040 on cooling, and was of the usual pale straw colour, without a deposit. On examination it was found to contain 25·340 grs. of sugar to the ounce. The instrument used for estimating the quantity was the polarising saccharimeter of Soleil, improved by the manufacturer, Duboscq, of Paris.

On the day following, Dec. 14, the patient was placed under the skim-milk treatment. Seven pints were taken daily during the first three days, and subsequently eight pints, divided into four meals. No other food was allowed, and no medicine prescribed.

On Dec. 20—the seventh day of the treatment—the patient was greatly improved. Urine, 100 oz.; sp. gr., 1020; it contained 6·345 gr. of sugar to the oz., being a diminution of 19 gr. to the oz.

On Dec. 27—the fourteenth day of the treatment—the quantity of urine was 100 oz.; sp. gr., 1012; sugar, 0·792 gr. (about \( \frac{3}{4} \) gr.) to the oz.

Two days later, Dec. 29, being the sixteenth day of the treatment, not a trace of sugar could be detected in the urine; its sp. gr. was 1020; quantity, 110 oz.

On Jan. 1, 1873, the urine amounted to 1012 grs., with a sp. gr. of 1012, and was quite free from sugar.

The patient now felt quite well, and free from every symptom of the disease. His strength had greatly improved, and the feeling of chilliness, especially during the night, had entirely left him, so that he felt quite comfortable after getting into bed. The daily allowance of skim-milk was now increased to nine pints, five or six of which were taken in the natural liquid state, and from three to four were made into curd by the essence of rennet. This exclusive regimen was strictly adhered to until Jan. 19; and as sugar had now been absent from the urine for a period of twenty-two days (the patient having being under a purely skim-milk regimen
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thirty-eight days, or five weeks and three days), the daily allowance of skim-milk was reduced one pint, and he was allowed, in addition, one meal of half a pound of mutton-chop, with greens, in the middle of the day. Under this change of diet the daily quantity of urine continued about 100 oz., while its sp. gr. increased and ranged from 1018 to 1020, but continued quite free from sugar.

On Feb. 14 the urine was still free from sugar; sp. gr., 1020. The regimen of the patient now consisted of six or seven pints of skim-milk daily, with mutton or beef, and green vegetables for dinner. His strength had now greatly increased, and he had regained 8lb. in weight, and this at the end of the second month of the treatment.

On April 9 he had gained a stone in weight, and felt much stronger and better than he had ever been since prior to the commencement of his illness. He was now in excellent health and spirits. The sp. gr. of the urine was at this date 1020, and it was quite free from sugar. His diet consisted of about six or seven pints of skim-milk daily, with plenty of solid animal food (especially beef and mutton) and green vegetables.

On Feb. 17 of the present year (1874), nearly ten months after the previous date, and fourteen months after the commencement of the treatment, the patient still continued quite well, his urine being free from sugar. He had increased considerably more in weight, and for a considerable period previously he had been taking ordinary bread to his meals regularly, without any ill effect. In short, he is now living under very slight restrictions as to diet, but still abstains from alcoholic drinks.

Remarks.—This was a case of confirmed idiopathic diabetes, in which, under a restricted diet (excluding in a great measure vegetable sugar and starch), persevered in for four months, the patient was passing at the end of this period at least from 2,500 to 3,000 grains of sugar in his urine daily; this regimen having produced but slight amelioration in his condition further than partially relieving some of the more distressing symptoms. But under the skim-milk treatment the glucosuria was entirely removed within sixteen days, together with all the symptoms of the disease; this effect being followed by the restoration of the health and strength of the patient and the recovery of his flesh. He, moreover, lived by rule afterwards, excluding vegetable sugar and starch, and also fatty matter, from his food as much as pos-
sible, for several months, but taking during this period a large quantity of milk-sugar, or lactine, daily as a constituent of skim-milk. The result being that, within a year after his recovery, he resumed an almost ordinary mixed diet without experiencing a return of the disease.

Such are the chief facts of the case. But it presents another clinical feature of the utmost importance in relation to the treatment pursued, namely, that the patient had evidently passed the primary stage of the disease in which there is mal-assimilation of the amylaceous or saccharine and oleaginous principles, only, of the food, into the advanced and most formidable phase in which the albuminous alimentary compounds also undergo saccharine metamorphosis: the result being rapid emaciation and the development of a cachectic condition under which so many grave secondary affections are prone to originate, and in which the disease soon ends fatally if not arrested. It is in this advanced stage of the malady that the superior efficacy of the skim-milk treatment becomes evident when even a strictly nitrogenous solid diet exercises but little or no control over it, and the quantity of sugar secreted in the urine.

If the stage of diabetes in which sugar is formed from the albuminous constituents of the food is not too far advanced, and no secondary affection necessarily fatal has become developed—such as pulmonary disease, or great and general emaciation, in which the heart is involved, does not exist, or if the ravages inflicted on the organism generally are not irreparable—the disease can usually be arrested, and the patient restored to a condition of health.

But even should the disease be too far advanced, and the injury inflicted by it and its complications be of too serious a nature to permit of complete recovery, the most remarkable improvement can generally be effected by the treatment; the disease becoming greatly subdued or checked in its progress, and all its distressing symptoms speedily relieved; so that the patient recovers a feeling of comfort and comparative health, instead of living, as previously, in a state of intense suffering and misery. In contributions already published, I have recorded instances of this description; but none of these are more strikingly illustrative than the case recorded by Dr. W. J. Fyffe, Surgeon-Major and Assistant Professor of Medicine, Netley College.*

* See 'Army Medical Report' for 1871, p. 273. (Appendix No. ix. 'Army Blue Book.)
This case, to which I can only briefly refer, was that of a soldier, æt. 26, treated by Dr. Fyffe, in Netley Hospital. He was suffering from severe diabetes, and when admitted was passing fifteen pints of urine daily, sp. gr. 1030, and containing a large quantity of sugar; he was emaciated and very weak. The patient was placed on a diet composed principally of fish, fresh mutton, bran biscuits, and a small quantity of milk. On this restricted diet, with occasional variations, he was kept for three months. The mineral acids were prescribed, and opium occasionally administered; but there was no improvement. The quantity of urine at the best of this period remained undiminished, and the patient had become so weak that he could not leave his bed.

On July 14 he was placed by Dr. Fyffe on the skim-milk treatment. Four pints of skim-milk were taken at first, daily, and within twenty-four hours the quantity of urine fell from thirteen to nine pints; the skim-milk was now increased to eight pints daily, and finally to nine pints divided into four meals. Four days after the treatment was commenced the quantity of urine passed had fallen to four pints fifteen ounces, and at the end of nine days to three pints, being a diminution of more than eight pints per day. This was on July 24, and on Aug. 18, the treatment being continued, he was passing only two pints seven ounces of urine. The patient was then dismissed from the hospital. The sugar was not entirely removed from the urine, but its quantity was enormously diminished.

According to Dr. Fyffe's report, the improvement in the condition of the patient was most remarkable. To quote his own description:—'The general health of the patient improved immensely after he had been taking the milk for a few days; he felt stronger and better than he had done for months; he lost the feeling of lassitude and malaise which he had before experienced; he was able to leave his bed and to walk out for exercise in the hospital grounds; the intense thirst which is so distressing in these cases was allayed. His nights were quiet and peaceful; instead of having to empty his bladder five or six times during the night, he had only to do so once. The voracious appetite which he could scarcely satisfy, even when taking large quantities of animal food, no longer consumed him; his weight increased about 7 lbs., and his spirits, which were of the lowest, became bright and cheerful; and he acknowledged the comfort and benefit he had received from the skim-milk treatment.'
Dr. Fyffe has since written to inform me that he has found the treatment equally beneficial in two other cases in Netley Hospital, and I gladly avail myself of his testimony to show how much good can be achieved by it even under circumstances the most desperate and hopeless. Dr. Fyffe states that two facts have impressed themselves upon his mind after observing the effects of the treatment. 'First—The rapid diminution of the quantity of urine passed in twenty-four hours. Second—The rapid improvement in the general condition of the patient.'

A very remarkable instance of the efficacy of this treatment is recorded in the 'Transactions of the Medical Society of the State of Pennsylvania, for 1872, * in the Report of the President for the Schuylkill County Medical Society. This was kindly forwarded to me by Dr. Halberstadt of Potsville, Pennsylvania. 'A case of diabetes, confined to bed in an apparently hopeless condition from extreme emaciation and debility, was treated by skim-milk as proposed by Donkin. In ten days the quantity of urine was reduced from thirty pints per diem to the normal quantity, and the specific gravity from 1050 to 1023, with a corresponding general improvement in every particular. The patient took seven pints every twenty-four hours in four meals, and, abstaining from everything else, continued it for four weeks, gaining fifteen pounds in weight, when a slight deviation from this diet was immediately followed by a return of the glycogenic symptoms. The milk treatment alone was then continued for four months, when it was found that broiled beef and mutton with bran bread could be taken without detriment, and at the end of five months no trace of sugar could be discovered. At this period his condition was such as to enable him to endure considerable fatigue and embark in business—a matter which, before he began the exclusive milk regimen, he and his friends had for ever abandoned in his case.'

There is one most important fact, of which I have satisfied myself by numerous and carefully conducted experiments; namely, that, in severe and far advanced cases of diabetes, in which the sugar cannot be completely removed from the urine, but is only reduced to a minimum (to a trace or a grain or two to the ounce) by a strict skim-milk regimen, persevered in for some length of time, the addition of solid nitrogenous animal food, such as beef and mutton, to this

regimen invariably produces a decided, or even a very large, increase of sugar in the urine, and arouses the disease to fresh activity. How can this effect be explained?

The juventia and the tardentia of disease often throw important light on obscure pathological questions. Now, if it is a clinical fact—which I have long endeavoured to demonstrate by carefully conducted and recorded experiments—that a pure skim-milk regimen removes the sugar from the urine completely, and puts an end to diabetes in a very large percentage of cases, and in a very considerable number of the remaining proportion reduces the urine-sugar to a minimum, with a corresponding degree of improvement in the health of the patient, what interpretation, in a pathological point of view, are we to give of this fact? Does it not go very far to show that diabetes is not a neurosis—some affection of the nervous system, whether cerebro-spinal or sympathetic—acting on the liver through the medium of the circulation, and causing an increased production of glycogen, or its downward metamorphosis into glucose, or diabetic sugar; for this is the theory of the disease, which at present has found very general acceptance; even although it has never been demonstrated that glycogen (a purely physiological product) is secreted by the liver at all in this disease, or that its existence under such a condition is more than hypothetical. This theory of the pathology of diabetes originated, as is well known, in the experiments of Bernard on the lower animals. But this distinguished physiologist has now come to perceive that a temporary glucosuria from lesions of the nervous system (even though the latter may be permanent) is a condition altogether different from genuine idiopathic diabetes. That I may not misrepresent him, I shall quote from his recent lectures on the subject.* "I do not pretend to believe that we have yet arrived at a complete explanation of diabetes; on the contrary, we know less about it than we thought we did. Form whatever opinion one pleases of this disease, call it a constitutional dystrophy or otherwise, these are still but empty words, behind which we seek to hide our ignorance of the real cause. Physiology has shown us to-day that we had a false idea of the cause of diabetes; it has shown us that it is not a physiological symptom, glycaemia or glucosuria, the mechanism of which is perfectly

* ‘London Medical Record,’ p. 741, 26th November, 1873.
well known to us, that we must attack; it is a more profound cause that we are obliged to seek.'

I do not agree with Bernard that the solution of the question is to be found in experimental physiology, but rather in careful clinical observation and experiment. Looking at diabetes from a clinical point of view, embracing its progress and the effect of treatment, I have come to regard it as a disease of mal-nutrition and mal-secretion, producing diabetic sugar—a new formation or secretion foreign to the organism; in fact a morbid growth differing from other diseased heterologous products of a malignant character, such as cancer and tubercle, in one essential particular, namely, that the product or growth developed by the disease is a crystalloidal substance, which, instead of accumulating or growing, as it is termed, like colloidal malignant formations, amongst the tissues where they originated, and leading to their ultimate destruction, is subject to the laws of osmosis, with a strong affinity for water, its endosmotic equivalent. Therefore, as a crystalloid, it passes into the current of the circulation as rapidly as it is formed, leaving the tissues in which it is formed or generated intact and uninjured. Hence the difficulty of detecting the locality of its origin.

Further, I believe this crystalloid glucose to be a diseased secretion of the liver-cells, taking the place of their healthy secretion, glycogen, which is a colloidal substance formed and stored up in them for some ulterior use in the animal economy.

The most characteristic pathological feature of diabetes is its power of misappropriating the proximate principles of food, converting them into an unassimilable substance incapable of oxidation—namely, diabetic sugar, which is cast out of the body by the kidneys as a useless, injurious, foreign substance. At first there is partial and then complete mal-assimilation of amylaceous and saccharine vegetable substances; next there is saccharine metamorphosis of fatty matter; and, last of all, the albuminous compounds of the food suffer the same fate, and are converted into sugar and excreted, until a period arrives when a very small proportion of these latter, even, is left to nourish and warm the body. Thus the late Prof. Griesenger found, by careful experiment on a diabetic patient, restricted to a rigorous meat diet, that only two-fifths of the albumen consumed in the food remained available for all the purposes of nutrition, the rest being converted into diabetic sugar.
These data readily explain the gradually increasing and, at length, extreme emaciation observed in diabetes, accompanied not by pyrexia, but an abnormally low temperature of the body; the latter condition being due to the complete absence from the food of the oxidisable products of the assimilation of the carbonaceous compounds of the food and the great deficiency of those derived from the albuminous. Hence we can readily understand the fact shown by the recent investigations of Pettenkofer and Voit, that in diabetes there is a diminished consumption of oxygen and a correspondently decreased production of carbonic acid by the process of respiration.

XXXIII.—Case of Tumour in the Left Hemisphere of the Cerebellum, with Smaller Tumour in the right half of the Medulla Oblongata. By Thomas Buzzard, M.D. Read May 22, 1874.

HENRY C., æt. 10, was brought to the National Hospital for the Paralysed and Epileptic on Dec. 17, 1873, on account of fits.

He was a well-proportioned lad, with an intelligent face, and looked extremely ill. Pale, with a hectic flush upon his cheeks, very large pupils, and an appearance of extreme weakness, he gave one the idea of a patient scarcely convalescent from a serious illness. He walked with difficulty, his gait being tottering and rather of the character belonging to general feebleness than to any form of paralysis. His arms also were weak, and the grasp of each hand was much diminished in power—that of the left being notably the weakest.

Family History.—His father and mother are both in good health. His mother has lost three brothers from phthisis.

Personal History.—His general health has been good until two years ago, when he had scarlatina followed by dropsy, and since that time he has never regained his usual condition. One year ago he began to suffer from attacks of pain of spasmatic character in the waist, just below the ribs. The
paroxysms, which would only last about five minutes, were so severe that he would leap from his chair.

Six months since he first complained of frequent attacks of pain in the head. This was very severe, and would seize him through the temples, but especially at the vertex and occipital region, thence extending down the back of the neck. These attacks have continued ever since.

Six or eight months ago he first had a fit. It began with 'snorting and fretting;' then the arms were stretched out rigidly, and he fell back insensible for a few minutes. He did not bite his tongue, but always vomited at the end of the fit. After the fit there has always been great pain in the head and eyes, and he has lost his sight for some minutes. Occasionally he has described diplopia.

There is nothing to be discovered abnormal in the condition of the heart or lungs. The urine contains no albumen. The cutaneous sensibility is everywhere unimpaired.

On examining the eyes with the ophthalmoscope I found in both the appearances belonging to descending optic neuritis. The papilla in each case was swollen and very prominent, of a greyish-white colour, and flocculent appearance, its outline lost, its vessels tortuous, and in portions of their course obscured by exudation. These changes were most marked in the left eye.

The patient was admitted into the hospital.

Progress of the Case.—On Jan. 12 the following observations were noted. Pupils, both large. The right acts readily to light; the left not at all.

The left eye is usually turned a little out; the movement upwards and inwards is slightly defective. The sense of smell and of hearing unaffected.

The action of the masseter muscles equal and good. Sensation in the face unaffected.

Taste said to be good. He can taste sweets and bitters perfectly; certain things, however, oranges, for instance, have a peculiar taste, which he cannot compare to anything.

Tongue deviates a little to the right. Palate moves equally.

Arms.—Neither very strong, but the right decidedly stronger than the left, which is very weak.
Legs.—Left much weaker than the right. Can walk, but slowly and hesitatingly. No throwing about of limbs.

Whilst under observation in the hospital he had no convulsive seizures, but on two or three occasions what were described as 'slight faints' occurred. He grew progressively weaker in his limbs, and his sight failed more and more, the pupils becoming enormously dilated. On Jan. 24, 1874, he vomited early in the morning for two hours, before he had taken any food, and attacks of vomiting of this description were afterwards repeated every week or so for two or three mornings together. He suffered much from pain about the coronal suture, especially at night. No symptoms of genital irritation were observed at any time.

Electrical Examination.—On Feb. 23 I found that farado-muscular excitability was entirely lost (at least to the strongest current which could be borne) in all the muscles of his upper and lower extremities, as well as in those of the abdomen. There was no increase of excitability in them to the interrupted battery current. Cutaneous sensibility was apparently perfect. Tickling the soles of the feet was distinctly felt as tickling, but there was no reflexion.

It is noted that on March 9 'he can still stand alone, but cannot take a step.'

On March 23 his skin was observed to be hot and the temperature in his mouth was found to be 100.2°. During the next eleven days the thermometrical observations were as follows:

| March 24 | 100. | March 30 | 101.2 |
| March 26 | 99.2 | " 1 | 101.3 |
| March 27 | 100.1 | " 2 | 101.2 |
| March 28 | 100.3 | " 3 | 101.4 |
| March 29 | 100.4 |

Towards the end of March it was noticed that his tongue, the deviation of which had become more marked, was wasted in its right half. Up to April 2 his intelligence had remained perfect, except that latterly there had been a little wandering at night. He now became more delirious, and, from this time till April 7, when he died, was quite insensible, and at times noisy. In the intervals he complained much of pain down his back. He had no return of convulsions, but became comatose for some hours before death.

His treatment consisted in small doses of iodide and bromide of potassium and bichloride of mercury.
The ophthalmoscopic appearances were carefully recorded from time to time by Dr. Gowers, who has been kind enough to give me the following notes of his observations.

Jan. 1.—With the right eye he can read No. 1 Jaeger, and can name colours accurately. With the left he can only count fingers and cannot recognize colours. The field of vision in the right eye is of normal extent, but the sight is dim in the peripheral portion. In the left eye sight is lost over the right half, but he can see fingers pretty well on the left.

Jan. 12.—Ophthalmoscope. The left disc is lost under a pale prominence about half as wide again as the disc itself; the centre almost white, stippled with greyish red. Edges greyish red, passing gradually into the red fundus.

Vessels of about the normal size and proportions, veins half concealed by lymph at the place of their emergence. Double contours distinct on the surface of the swelling, but on the slope down its side they are lost, and at the edge of the swelling the veins themselves are lost or almost lost to view, reappearing at a little distance from it, and in most cases presenting at the foot of the swelling a more or less considerable curve. One or two small twigs disappear abruptly at the side of the prominence, to reappear at some distance laterally from the spot at which they were lost.

Of three large arteries only one can be seen in front of the disc; the rest appear in the fundus, about the distance from the disc at which the veins regain their double contour. The artery on the disc has a double contour and a fair width, but its origin, in the centre is concealed.

Two or three small vessels exist on the swelling, but do not extend far beyond its margin.

There is much pigment about the yellow spot, and in the centre a small area (somewhat in shape like a \( \sim \)) which is free from pigment and rather paler than the rest of the fundus.

Midway between this pigment and the disc is a curious group of white spots like pin-heads, but some smaller. They are very white; the larger ones have part of their outline indistinct. They appear to be behind the level of a small retinal vessel which passes between them.

The retina about the disc presents a striation more distinct than normal.

Right disc similar, but the amount of swelling less. The large arteries are all visible on the surface of the swelling. The obscuration of the veins as they pass on to the retina is less.
No white spots can be seen on the fundus, and there is no accumulation of pigment about the macula lutea, as in the other eye. Sight of right eye very dim. He can only count fingers; same degree of sight even to periphery of field of vision. He cannot read No. 20 Jaeger.

The colours yellow, blue, or green, are all named accurately, but red is called brown.

The left eye is now insensible to light.

Jan. 17.—Right eye cannot now count fingers; there is qualitative perception of light only.

Ophthalmoscopic appearances similar except that in the left eye the arteries can now all be seen upon the prominence, but have there not more than one-third of the diameter which they have on the retina.

Feb. 2.—Swelling of discs less. On the outer part of the left the edge of the choroid can now be seen. The white spots on the retina are still visible, but all are smaller than before, and many of them paler.

From this time the subsidence of the swelling progressed, until by the middle of March both discs were clear in outline, the surface grey, with some whitish lymph about the vessels. There was a little atrophy of the adjacent choroid. In the position of the white spots some minute white points were still to be seen. The pigmentation around the macula lutea continued. The veins of about the normal size, the arteries much smaller than normal.

About the beginning of March he lost even qualitative perception of light in the right eye.

The condition of his discs underwent no material alteration to the time of his death.

**Autopsy.**—A very small quantity of miliary granulations is found in the apex of each lung, and a cheesy mass, the size of a large pea, in the middle of one lung. The abdominal viscera are healthy.

**Contents of the Cranium.**—The convolutions of the cerebrum are somewhat flattened. The lateral ventricles are moderately distended with fluid.

**Superior Surface of the Cerebellum.**—The right hemisphere is normal. The superior veriform process projects about half an inch above the level of the right hemisphere, the whole of it being prominent, and the prominence extends over the inner half of the left quadrate lobe. Anteriorly it stops short within an inch of the anterior edge. About half an inch of the anterior portion of the superior veriform process is normal.
The centre of the prominence is concave for about a square inch, and over that space the tentorium is firmly adherent. The prominence is caused by the presence of a tumour in the substance of the left hemisphere, which, on being pinched up, is found to be firm and about the size of a Maltese orange. The surface, save as regards the adhesion of the membranes, is unaltered.

_Inferior Surface of the Cerebellum._—The left half of the cerebellum is distinctly more prominent than the right, the difference being most marked in the amygdala. The right is normal to the feel. The left is rendered prominent by the hard mass in its interior.

_Medulla Oblongata._—The right half, as far down as the decussation, is twice the width of the left. The increase is caused by enlargement of that part which lies between the median line in front and the place of apparent origin of the eighth nerve behind. From the centre of the prominence the right hypoglossal nerve emerges, very much reduced in size. The prominence extends right up to the pons, the lower margin of which it somewhat encroaches upon and obscures. To the feel it seems to enclose a body about the size of a hazel-nut.

_Pons Varolii_ normal.

The chiasma, circle of Willis, and interpeduncular space, are obscured by a quantity of yellowish lymph beneath the arachnoid, out of which the third nerves emerge, apparently unaltered. No tubercle is to be seen here.

The optic nerves appear larger than normal.

The tumour in the cerebellum, when exposed, is found to be hard, nodulous, generally spherical, invested by a delicate capsule, and it forms a prominence in the roof of the fourth ventricle. On section it is yellow-green-grey and cheesy—evidently tubercle. The adherent membranes on its surface are more than a line in thickness, dense and hard.

_Microscopically_ the tumour in the cerebellum and that in the medulla oblongata are both found to be made up of shrivelled nuclei, with a few larger cells containing one or more nuclei, and scattered oil-globules.

_Remarks._—On the admission of the patient into the hospital the case was diagnosed as one of tumour of the cerebellum. The grounds for this diagnosis were the pains in the head extending from the vertex down the occipital region, the attacks of vomiting, the peculiar paresis of all four extremities, not accompanied by paralysis of any cranial
nerve, the convulsive seizures, the associations with these symptoms of double optic neuritis; to which may be added, on the negative side, the absence of psychical disturbance. As regards the nature of the tumour, this was diagnosed as tubercle, from the fact of three maternal uncles of the patient having died of phthisis.

In the course of the disease the elevation of temperature which has been described caused a confident opinion to be held that signs of tubercular meningitis would be discovered after death. The paralysis and progressive wasting of the right half of the tongue made it equally sure that the right hypoglossal nerve would be found involved in some way in the lesion. The result showed that this had actually been brought about by the pressure of a separate tumour in the medulla oblongata.

I would especially call attention to the extent to which this patient's sight was retained under circumstances in which it could hardly be expected to survive. Even when the ophthalmoscope showed extremely coarse and well marked changes in the right optic disc, the boy could read the smallest type with this eye and name colours accurately.

Another point of interest lies in the fact that the greatest want of power in the patient's limbs was exhibited on the left side of the body. The tumour, although it occupied a position nearly in the median line, was developed in the left hemisphere of the cerebellum. It is known that in cases of disease of the cerebellum want of power in the limbs has been observed sometimes on the side of the body opposite to and sometimes on that corresponding to the cerebellar lesion. In the Société Anatomique of Paris a case was recently brought forward by M. Cassy in which a tumour developed in the right cerebellar fossa, within the dura mater and compressing the cerebellum and pons, caused symptoms of right hemiplegia.* At first sight the present case would appear to be another example of a cerebellar tumour affecting the motive power of the limbs on the same side. But it must not be forgotten that here there was a second tumour occupying the right half of the medulla oblongata above the decussation of the pyramids, and capable therefore of explaining by itself the motor difficulty of the left limbs. Under these circumstances, no inference as to the crossed or direct influence of cerebellar lesions can be drawn from the present case.

* 'Le Progrès Medical,' March 28, 1874.
XXXIV.—Case of Acute Rheumatism attended by Cerebral Symptoms without Hyper-pyrexia. Recovery. By Edward Headlam Greenhow, M.D. Read May 22, 1874.

T., police constable, at 23 years, was admitted into the Middlesex Hospital under my care, Nov. 29, 1873. Both his parents were dead; four brothers and two sisters were alive and healthy. There was no discoverable evidence of the existence of insanity, epilepsy, or any other disease of the nervous system in the family, but his father had suffered severely from rheumatism.

Previous History.—Ten years previously the patient had received a kick in the mouth, which was followed by unconsciousness of some days duration, and two years before his admission he had suffered from an attack of rheumatic fever, which confined him to bed for two months. He stated of his own accord, when his history was being taken, that his head had been much affected by the rheumatic fever, and that he could not remember what had taken place during the time of his illness. He had been quite free from rheumatism after his recovery until the commencement of his present illness.

Present Illness.—Began on Tuesday, Nov. 23, with headache, sickness, and loss of appetite. Next day he experienced pain in the left ankle, and during the following days other joints became successively affected.

State on Admission.—Pulse, 96. Temperature, 102.4°. Tongue coated with white creamy fur. Both ankles, right hand and wrist, swollen and painful on movement; left hand and wrist much swollen and painful on pressure; sweat profuse, and very sour. A faint systolic murmur was heard at the apex, and a soft friction-sound at the base of the heart one inch above and a little inside the nipple. Urine, sp. gr. 1025, acid; not albuminious.

Ordered to take effervescent draughts of citrate of potash with an excess of alkali, and to have a plaister, composed of equal parts of ointment of iodide of potassium and extract of belladonna, applied over the precordia.

As he passed a sleepless night and his bowels were confined, he was ordered on the day after admission to take ten
grains of Dover's powder and two of calomel at bed-time, and a compound senna draught the following morning. On subsequent days he took the Dover's powder at bed-time without the calomel.

Progress of Case.—Dec. 2. Highest temperature, 103°. Pulse, 96. He was perspiring profusely, but the only pains this day were in the left wrist and in the knuckles of the right hand, which were considerably swollen and tender. Tongue furred, bowels rather loose. The systolic murmur was still audible at the apex of the heart, but the friction-sound could no longer be heard. The whole trunk was covered with a bright red papular rash.

4.—Temperature from 102° to 103°. Pulse, 90 to 96. Patient was quite free from pain, but still sweating profusely. The cardiac murmur continued as before. He complained of low spirits.

5.—Had slept well and continued free from pain, but was bathed in sour perspiration. The murmur at the apex of the heart was louder and rougher than before, and a soft murmur was now again audible at the left border of the sternum, an inch above the nipple. The temperature rose on this day to 103.3°, the highest point it reached during the illness, but fell again on the following morning to 102°, which it only once subsequently exceeded.

7.—Sweating continued, but there were no pains. The knuckles of the left hand were slightly swollen, and there were choreal movements of the fingers of the same hand. Patient had been delirious in the night; his manner was confused, his pupils were large; tongue dry in centre.

Dover's powder to be omitted. Ten grains of chloral hydrate to be taken at bed-time, and the dose to be repeated every two hours until sleep was induced.

8.—Had slept indifferently; was incoherent and wandering; had much jactitation of body generally, constant movement of fingers of left hand, tremor of right hand, and subsultus. When told to take hold of anything with the left hand, was unable to do so without choreal movements. Tongue dry and fissured; bowels much purged.

A draught containing diluted sulphuric acid, decoction of logwood, and tinctures of catechu and opium, was ordered to be given as occasion might require, to check the diarrhoea. In the evening he was very delirious, and sang incoherently; his eyes were bloodshot. At midnight he was found asleep after the chloral draught.
Dec. 9.—Had slept only two hours, and been very delirious all night, but could answer questions rationally this morning, though his manner was odd and abstracted. Chorea movements in fingers of left hand continued, and when the hand was turned on to its back there were constant twitching movements of the hand and fingers, and the forefinger became flexed towards the palm. Eyes bloodshot, pupils small. He still sweated, but had no pains; passed stools and urine in bed. Was ordered to continue the chloral draught, and to take ten grains of bromide of potassium every four hours.

10.—Slept four hours last night; when awake was constantly delirious, with distinct hallucinations. In addition to the choreal movements of the left hand and arm, and tremor of the right hand, there was now also frequent rolling of the head from side to side. The sweating continued, and indeed persisted throughout the illness, although there was no return of rheumatic pains and no rise of temperature above 102°. Pulse was full and bounding, but very compressible. Ordered half an ounce of port wine every three hours; and, one drachm of hemlock-juice to be added to the bromide of potassium (draught) taken every four hours.

12.—Slept three and a half hours after the chloral draught last night, but when he awoke was delirious, with hallucinations. At 10 a.m. on this day was clearer in intellect than he had been for several days, but at the mid-day visit was found to be again incoherent.

13.—Had slept for four hours without waking, and when awake had been very quiet and much less actively delirious, though still wandering in mind, and casting his eyes about with a look of suspicion. Chorea movements less violent; tremor of right arm extreme; restless movement of head from side to side. Eyes still bloodshot; pupils dilated. First sound of heart prolonged and rough at the apex.

14.—Had slept for six hours after taking one dose of chloral, and when awake had been very quiet and almost free from delirium, but still had delusions and a sly suspicious look. The choreal movements and tremor of hands were obviously diminishing, and he took his food freely.

16.—The bromide and hemlock-juice were this day directed to be given only every six hours.

18.—Decided improvement. Chorea movements and mental symptoms daily decreasing.

The improvement continued steadily, but after the delirium had left him he remained for some days incoherent and
childish in manner. On Dec. 22 the bromide and hemlock-juice were discontinued. A few days later he became quite rational, and able to answer questions pertinently; the tremor and choreal movements had altogether ceased, and he complained of hunger. He continued to perspire freely a few days longer, but was discharged quite convalescent on January 6, 1874, at his own request.

Remarks.—Cases of rheumatic fever, accompanied by cerebral symptoms and hyper-pyrexia, have been recorded in the ‘Transactions of the Clinical Society,’ by Drs. Hermann Weber, Murchison, Burdon-Sanderson and Southey, and also by myself.* In the case which I have now read there was a marked resemblance to all those cases in the train of cerebral symptoms, but there was at the same time a marked absence of the hyper-pyrexia which has been supposed by some authorities to give rise to them. The general aspect of the case, the tremor, subsultus, jactitation, and delirium, so forcibly reminded me of my previous cases of rheumatic fever attended by hyper-pyrexia, that I anticipated a rise of temperature in this case also, and made arrangements for placing the patient in a cool bath so soon as the thermometer should reach 105·5°; but, as the history of the case has shown, the highest temperature during the illness was 103·3°. It appears to me, therefore, that this case is an important one, first as confirming the opinion expressed by Dr. Weber, in which I fully concur, that the hyper-pyrexia, in cases of this class, is not the cause but the effect of the brain paralysis; and secondly, as showing that it is an effect which is not invariably produced.

With the one exception of the absence of high temperature, this case presented the same general symptoms as most of the cases already referred to. There was the same subsidence of the arthritic pains coincident with the accession of cerebral symptoms, the same noisy, violent delirium, and the occurrence of a form of flux. In Dr. Southey’s case, and in two of Dr. Weber’s, this flux took the form of a profuse flow of urine. In Dr. Weber’s other case, and in my own former case, as well as in the present one, it took the form of diarrhoea. In both my cases profuse sweating persisted throughout the illness, notwithstanding the subsidence of the rheumatic pains, and in the present case it even greatly exceeded in amount the copious sweating usual in rheumatic fever.

*‘Transactions of the Clinical Society,’ vols. i., v. and vi.
The only symptom in the case which was not observed in any of the cases already published in the Society's 'Transactions,' nor in either of two unpublished cases of the same kind which have been under my care, was the choreal affection of the left hand.

It may, perhaps, be supposed that the blow on the face received by the patient ten years before, and followed by unconsciousness, had caused brain-mischief which led to the occurrence of the cerebral symptoms during his two attacks of rheumatic fever. But, independently of the fact that the precise similarity of the cerebral symptoms in this case to those observed in other cases of the same class gives no countenance to the supposition, it seems to me untenable, not only on account of the patient's immunity from any symptoms of cerebral disturbance during the eight years that elapsed before his first attack of rheumatic fever, but still more on account of his perfect recovery during the interval between his first and second attacks, and again immediately after the second, the history of which I have just related.


Case I.

A SERVANT attended the hospital, stating that she had suffered from a sore throat for five days. I found enlargement and redness of the right tonsil, with thirst and dryness of the throat, and considerable pain in swallowing.

The crypts of the gland were dilated with thick yellow cheesy secretion, which when removed was found to be fetid, and to be composed mainly of large multinuclear cells like pus or mucous, and therefore different from the ordinary secretion of a healthy gland, which contains no cells.

The local conditions were accompanied by constitutional disturbance, the temperature being $100^\circ\text{F}$, the pulse 100, and the tongue white furred. The patient recovered in three days under the treatment of a purge, and a gargle of chloride of zinc, gr. iiij to $\frac{1}{2}$j, used about a dozen times a day.

This affection—which I would term acute tonsillar
catarrh, or acute catarrhal tonsillitis, because the mucous surface only, and not the parenchymatous tissue, is attacked by the inflammation—is of extremely common occurrence. It is very frequently mistaken for ulceration of the tonsil, the collection in the crypts bearing a resemblance to ulcers. When people remark that they have been suffering from ulcerated throat, I believe it to have been almost invariably catarrhal tonsillitis, for in my experience ulcers of the throat are extremely rare, except as a result of syphilis. I may say, equally rare as catarrhal tonsillitis is common.

The causes of this affection are those of other catarrhal affections, exposure to cold, damp, or draught; and the prognosis is highly favourable, for it never runs on to suppuration, but on the contrary yields readily (as a rule in three or four days) to the mildest treatment—a purgative and an astringent or stimulant gargle.

Case II.

The second case is that of a young man about nineteen years of age, both whose tonsils had been enlarged so far back as his memory would carry him.

The glands nearly closed upon each other in the central line. Both ears were deaf. The voice was thick and nasal. Respiration was noisy, and during sleep was so much interfered with as to become a painful and anxious annoyance. From time to time one or other tonsil became inflamed, producing dysphagia and all the symptoms of acute catarrhal tonsillitis. Both tonsils were at once removed and all symptoms disappeared.

This case is a type of a common affection, which I would look upon as chronic catarrhal tonsillitis.

Pathology.—All the enlarged tonsils that I have examined had the appearance of having undergone subacute inflammation. In some there was distinct proliferation of the fibrous tissue element. In these the parenchymatous tissue was not only plain to vision, but existed at the expense of the crypts and their connected channels, forming a greater portion of the gland structure, and giving to it a somewhat intense hardmess. Such a condition would be the result of chronic parenchymatous tonsillitis.

In the majority of hypertrophied tonsils it appeared that the mucous and secreting tissues had been chiefly affected—a condition accompanying chronic catarrhal tonsillitis.
crypts and their channels formed large open spaces, occupied by, and in fact dilated by, rounded nodular elevations upon the surface. The lining surface of the crypts and channels was covered with hypertrophied villous processes or papillae. These processes are present in all tonsils, healthy or unhealthy, though I have not hitherto seen a description of them. In healthy tonsils they are slightly conical or even throughout their length, and rounded at their free extremity. They are covered with epithelium, and contain within them a plexus of blood-vessels. When inflamed they increase twice or threefold in size, and appear as a soft velvet pile, becoming distinctly visible to the naked eye. If catarrhal hypertrophy has existed for any length of time, the parenchymatous tissue becomes secondarily affected, and by proliferation increased in thickness.

The two cases above described are types of a disease, the one acute and the other chronic, which may be recognised as an inflammation of the secreting surface of the tonsil, and may be designated tonsillar catarrh, or catarrhal tonsillitis, a disease totally distinct from inflammation of the parenchymatous structure of the gland in its causes, in every symptom, in its treatment and results.

Comparing the acute forms of the two diseases the following differences occur:

<table>
<thead>
<tr>
<th>Catarrhal</th>
<th>Parenchymatous</th>
</tr>
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<tbody>
<tr>
<td>A mucous inflammation.</td>
<td>A fibrous inflammation.</td>
</tr>
<tr>
<td>Three or four days duration</td>
<td>Two to four weeks.</td>
</tr>
<tr>
<td>under treatment.</td>
<td></td>
</tr>
<tr>
<td>Cause—Exposure, draught, damp, &amp;c.</td>
<td>Often neighbouring inflammation, cutting wisdom teeth, &amp;c.</td>
</tr>
<tr>
<td>Great depression, often profuse perspiration.</td>
<td>High fever, with hot dry skin.</td>
</tr>
<tr>
<td>Pulse small and quick.</td>
<td>Strong hard, as in fibrous inflammation.</td>
</tr>
<tr>
<td>Never goes on to abscess.</td>
<td>Commonly runs on to abscess.</td>
</tr>
<tr>
<td>Both tonsils affected.</td>
<td>More often one tonsil affected.</td>
</tr>
<tr>
<td>Lacunae filled with masses of morbid secretion resembling ulcers.</td>
<td>Often covered with lymph, but no collection of secretion in lacunae.</td>
</tr>
<tr>
<td>Little or no oedema around.</td>
<td>Extensive oedema.</td>
</tr>
<tr>
<td>Treatment—Tonics and stimulants from the first, with astringent gargles.</td>
<td>Antiphlogistics and depressants never gargles.</td>
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CHRONIC CATARRH OF TONSIL.

Papillae found upon the lining membranes of the Crypts & Channels of Tonsils.

1 Inch Power.

Surface View

The Hypertrrophied lining membrane of the Crypts projecting through the Lacunae
Natural size

Internal View of Crypts

Lining membrane of Crypts, & Channels Hypertrrophied
Natural size

To illustrate M'Nerter's Case.
XXXVI.—Cases of Sterility after Lithotomy. By W. F. Teevan. Read April 10, 1874.

Case I.

SAMUEL N., hall-keeper, æt. 44, was cut for stone, by the lateral method, twenty years ago. The patient was married when he was 27, his wife being his junior by four years. She has had neither child nor miscarriage. The man states that he has no emission during connexion. He has suffered from incontinence of urine ever since the operation, is in feeble health, and the testicles, which are small and flabby, hang down in a very pendulous scrotum.

Case II.

Robert P., a painter, æt. 47, was operated on by the lateral method when he was 31 years old, the calculus extracted being about the size of a pigeon's-egg. The patient was married when he was 28 (i.e. three years before the operation), his wife being his junior by two years. She has borne him four children, the youngest being born three months after the operation, since which period she has had neither child nor miscarriage. The patient says that he has no emission during coitus, and although he is in feeble health his testes are firm and well developed. He sought my advice for the recto-vesical fistula which remained after the operation.

Case III.

Henry H., a shoemaker, had lateral lithotomy performed on him when he was two years old. He married when he was 25, his wife being his junior by one year. She has never had a child nor a miscarriage. He has perfect erections, but he has no emission when he has connexion, and he has never had a nocturnal emission in his life. The patient is in good health, and has well developed testicles. He sought my advice on account of the incontinence of urine, from which he had suffered ever since the operation. I unexpectedly cured him, in a fortnight, of his incontinence by giving him drachm doses of the tinct. ferri. sesq. three times a day.
Case IV.

John B., æt. 45, shipwright, was cut by the lateral operation when four years old. He married when he was 23 years of age, his wife being slightly his junior. She has never had a child nor a miscarriage. The patient is in good health, and has firm but rather small testes. He has never had a nocturnal emission in his life, and has no emission during coitus.

It would appear that all these patients, who were operated on by the lateral method, were injured by the operation. The stone did not seem to be large in any case. What was the cause of the affliction? To me the answer seems clear. The operation, as usually performed, involves a laceration of the floor of the prostatic urethra,* and as a result the ejaculatory ducts are torn across, or their orifices are plugged with inflammatory exudation in the process of healing. Occasionally the prostatic splits in its roof, and hence the ducts escape any injury.


The remarks I have to make upon the above morbid state are drawn from the observation of five cases. Of two of these I am able to give many details, but the three others were only seen by me on one or two occasions.

Case I.

Miss B., after the cessation of the catamenial period, became insensibly more and more languid, with general increase of bulk. This change went on from year to year, her face altering from oval to round, much like the full moon at rising. With a complexion soft and fair, the skin presenting a peculiarly smooth and fine texture was almost porcelainous in aspect, the cheeks tinted of a delicate rose-purple, the cellular tissue under the eyes being loose and

* See communication by me, 'Path. Soc. Trans.' vol. xvii. p. 186.
folded, and that under the jaws and in the neck becoming heavy, thickened, and folded. The lips large and of a rose-purple, alæ nasi thick, cornea and pupil of the eye normal, but the distance between the eyes appearing disproportionately wide, and the rest of the nose depressed, giving the whole face a flattened broad character. The hair flaxen and soft, the whole expression of the face remarkably placid. The tongue broad and thick, voice guttural, and the pronunciation as if the tongue were too large for the mouth (cretinoid). The hands peculiarly broad and thick, spade-like, as if the whole textures were infiltrated. The integuments of the chest and abdomen loaded with subcutaneous fat. The upper and lower extremities also large and fat, with slight traces of œdema over the tibiae, but this not distinct, and pitting doubtfully on pressure. Urine normal. Heart’s action and sounds normal. Pulse, 72; breathing, 18.

Such is a general outline of the state to which I wish to call attention.

On the first aspect of such a case, without any previous experience of its peculiarity, one would expect to find some disease of the heart leading to venous obstruction, or a morbid state of the urine favouring œdema. But a further inquiry would show that neither condition was present; nor, when minutely studied, is the change in the body which I have described to be accounted for from either of these points of view.

Had one not proof that such a patient had been previously fine-featured, well-formed, and active, it would be natural to suppose that it was an original defect such as is common in mild cretinism. In the patient whose condition I have given above, there had been a distinct change in the mental state. The mind, which had previously been active and inquisitive, assumed a gentle, placid indifference, corresponding to the muscular languor, but the intellect was unimpaired. Although there was no doubt large deposit of subcutaneous fat on the extremities, chest, and abdomen, the mere condition of corpulency, obesity, or fatness, would not in any way comprehend the entire pathology.

It is common to see patients with a very superabundant accumulation of fat in the subcutaneous adipose tissues, and on that ground more inactive, without the change in the texture of the skin, in the lips and nose, increased thickness of tongue and hands, &c., which I have enumerated. The change in the skin is remarkable. The texture being pecu-
lierly smooth and fine, and the complexion fair, at a first hasty glance there might be supposed to be a general slight oedema of it, but this is not confirmed by a future examination, whilst the beautiful delicate rose-purple tint on the cheek is entirely different to what one sees in the bloated face of renal anasarca. This suspicion of renal disease failing, any one who should see a case for the first time might suppose that the heart was the faulty organ, and that this general change in the features and increase of bulk were owing to venous congestion. But neither would this be confirmed by an exact inquiry into the cardiac condition.

I am not able to give any explanation of the cause which leads to the state I have described. It is unassociated with any visceral disease, and having begun appears to continue uninfluenced by remedies.

Case II.

P. M., æt. 40, a married woman, having had five children, and living in good circumstances, came under my observation in 1866, complaining of general languor.

Heat was normal. Pulse, 60. Catamenia too profuse. There had been gradual and general increase of bulk. The features had become broad and flattened, the skin was peculiarly fair and fine and soft, with a very delicate rose-bloom on the cheeks. The cellular tissue about the eyes was thrown into folds, giving the impression, when cursorily looked at, of being oedematous. The eyes were bright, the lips were thickened, and of a light rose-purple. Tongue large, the speech guttural, and, as in the former case, as if the tongue were rather unwieldy. The sounds and impulse of the heart were normal, breathing was normal, urine normal. In fine, there was no discoverable change in any of the viscera, and the morbid state complained of seemed to be some primary change in the integuments, the muscles, and the nervous tissues of the cerebro-spinal system. This change continued to advance, so that in 1873 I made the following notes:—

' Tongue large; false teeth cannot be worn, as tongue bitten by them. Lips large, thick, of a light rose (venous) tint. Features broad. Tissue under eyes loose, suggesting oedema. Fine delicate rose-tint on cheeks. Hair soft. Neck thick. Skin and subcutaneous textures lying in resisting folds. Hands broad and spade-like, the textures suggesting oedema, but not pitting. Much subcutaneous fat on chest, abdomen,

The following is from a letter written by me on this case March 7, 1873, and fairly expresses my views of it at that time, which was seven years after my first observation of it.

'Ve believe it to be a rare form of constitutional disorder, without any internal visceral disease, but characterised by great inaptitude to spontaneous exertion both of mind and body. The deposit of fat and the changes in the skin and connective tissues correspond to a languid condition of the venous circulation, but without any tendency to oedema, or any sign of cardiac defect.

'No doubt, under the stimulus of external circumstances, there is a response of mental activity which seems to prove that the mind requires but an exertion of the will to work up to its normal level. Though this be theoretically possible, I doubt if it be practically so in this state. The peculiar condition of the nervous system will, I believe, be best understood by reference to the external condition of the frame; for although I do not think the nervous centres have undergone any discoverable anatomical change, nor is there any evidence that the intellect is materially injured, I believe the nervous power is upon the whole lessened, and hence have arisen the changes in the temper, and the attacks which have been described to me.

'The best suggestions I can make are to let events take their course very much, maintaining the strength by simple regimen and fresh air, and by the occasional or more or less continuous use of such remedies as quicken the peripheral venous circulation. Hot-air bath, or warm bath, frictions, &c., but the general good effect will, I think, be limited.'

To those about such a patient the whole morbid condition is likely to be attributed to indolent habits, and the apparent incapacity for exertion to be deemed dependent upon mere inertness of the will. No doubt extreme circumstances have a distinct influence upon these as upon other patients, but I believe the disinclination to mental or muscular activity is largely pathological.

There is certainly a degree of habitual and mental indifference, though this may under occasional circumstances be obviated, since the intellect seems to be unimpaired. It
will be noticed that I have designated this state *cretinoid*. My remarks are rather tentative than dogmatical, my hope being that once the attention of the profession is called to these cases, our clinical knowledge of them will in proportion improve. That the state is a substantive and definite one, no one will doubt who has had fair opportunity of observing it. And that it is allied to the cretin state would appear from the form of the features, the changes in the lips and tongue, the character of the hands, the alteration in the conditions of locomotion, and the peculiarities, though slight, of the mental state; for, although the mind may be clear and the intellect unimpaired, the temper is changed.

In an interesting Paper* on sporadic cretinism occurring in England, my friend Dr. Fagge has given a case which began as late as the eighth year, in a subject previously healthy and well developed; and he states that in this case the physical configuration was alone manifested, or at any rate that any change in the mental powers was doubtful; and he adds it may therefore be interesting to speculate as to what character would be present should the disease, if that be possible, arise still later in the course of adult life.

In the same paper we find that 'in the report of the Sardinian Commission it is stated that, according to information received from medical men practising in infected districts, and according to all those who have written on this degeneration, there is no example in which, after the seventh year, a healthy child has become a cretin.' And the Commission further quote with approval the statement of Maffei (who practised for a long time where cretinism was endemic, and who therefore had good opportunities of observing it), 'that the period within which cretinism may commence is limited by the fourth year of life. . . It must indeed be mentioned that Rösch has recorded two cases in which the disease is said to have begun respectively at five years of age, and between seventeen and eighteen years.'

It is to be borne in mind that these statements are applicable only to endemic cretinism, and therefore the objections from the experience of those who have observed only the endemic cases will be of less value.

The occasional occurrence of cretinism in children of healthy parents, and living in healthy districts in this country, is now well known. But our experience as to its

development at different periods of childhood is of the most limited kind. The whole information on the point is contained, I believe, in Dr. Fagge's Paper, and is illustrated by the second case given.

In the cretinoid condition in adults which I have seen, the thyroid was not enlarged; but from the general fullness of the cutaneous tissues, and from the folds of skin about the neck, I am not able to state what the exact condition of it was. The supra-clavicular masses of fat first described by Mr. Curling, and specially drawn attention to by Dr. Fagge as occurring in cases of sporadic cretinism in children, did not attract my attention in adults. The masses of supra-clavicular fat are not infrequent in the adult, without any associated morbid change whatever.
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