WEEKLY EVENING MEETING,

Friday, June 1, 1866.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Henry E. Roscoe, B.A. F.R.S.

On the Opalescence of the Atmosphere.

On a previous occasion (May 22, 1864) the speaker explained the principles of a method by the application of which we are able to gain some knowledge of the distribution of the chemically active rays on the earth's surface, and their variation from time to time. This method depends upon the comparison of tints gained by sensitive photographic paper when exposed to daylight; and it is evident that we must define the "chemical rays" to be all those which are able to produce a darkening effect upon chloride of silver paper. In order that such a mode of measurement should be possible, it is necessary, in the first place, that paper can be prepared of a uniform degree of sensitiveness; and secondly, that the relation between the several tints and the intensity of the light necessary to produce such tints should be known. These relations have been accurately ascertained, and the method is now so far perfected that the observations can be very easily and accurately made.*

The whole apparatus needed for these experiments is contained in a small box, and all the observations for a day can be made in the course of a few minutes.

Through the kindness of Mr. Balfour Stewart, determinations of the Chemical Intensity of Total Daylight have been carried on at Kew Observatory for the past year by Mr. T. W. Baker. The mean daily intensity can be readily obtained from the separate observations, and these, when plotted out as a curve, show the daily mean intensities for the year.

Daily Mean Chemical Intensities measured at Kew, April, 1865, *to April,* 1866.

For April, 1865	81·2	For October, 1865	29·2
„ May „	97·0	„ November „	12·8
„ June „	76·9	„ December „	6·9
„ July „	100·6	„ January, 1866	13·4
„ August „	82·5	„ February „	24·2
„ September „	110·2	„ March „	32·2

In Spring, 45·9 In Summer, 91·5 In Autumn, 73·9 In Winter, 11·0

(Light of the Intensity 1 acting for 24 hours taken as 1000.)

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* See 'Phil. Trans.,' 1865, p. 605: "The Bakerian Lecture."

It is seen that the condition of the sky and weather materially influences the chemical intensity of the month—thus June, 1865, was cloudy (2 days rain; 21 days cloudy; average amount of cloud, 5·5), and the mean intensity is 76·9; whereas September was a very bright month (0 days rain; 20 days cloudy; average amount of cloud, 2·5), and the chemical intensity reached 110·2.

If we compare the mean intensities for the summer and winter solstices and the equinoxes as measured at Owen's College, Manchester, we have,—

June 21	113
March and September 21 . . .	33
December 21	4·7

The above numbers show that the increase of chemical action from December to March is not nearly so great as that from March to June. This difference cannot be attributed to the common absorption exerted by the atmosphere, but may be explained as being the necessary consequence of a peculiar absorptive action which the atmosphere effects upon the chemically active rays, and to which the name of opalescence may be given.

It has frequently been stated* that the chemical intensity of light on snowy peaks and in tropical climates is much less than that in our own latitudes, and that photographers in Mexico have found it impossible amidst the glaring rays of a tropical sun to obtain a picture which in the gloomy atmosphere of England would need an exposure of only one minute. In order to ascertain the degree of truth attaching to these extraordinary statements, and to obtain some insight into the chemical intensity of tropical climates, the speaker was fortunate to be able to send his assistant, Mr. T. E. Thorpe, to Pará, on the Amazons (long. 48° 30′ W., and lat. 1° 28′ S.). The measurements there made have already furnished some very interesting results; in the first place, we find that the daily mean chemical intensities at Pará and at Kew on the same days are represented by the following numbers:—

	Kew.	Pará.	
April 4, 1866	19·7	260·0	or as 1 to 13·1
„ 7 „	9·3	320·0	or as 1 to 34·4
„ 13 „	45·7	326·0	or as 1 to 7·1

The curves for these days show the enormous variation of chemical intensity which occurs under a tropical sun during the rainy season. Every afternoon regularly, and sometimes at other periods of the day, the enormous thunder-clouds discharge their contents in the form of deluging rain, and the chemical action sinks to zero; then the storm passes over, and the chemical intensity again rises (see woodcut on opposite page). It is thus seen that any difficulties which a photographer may have in the tropics cannot be ascribed to an insufficient supply of the sun's chemically active rays.

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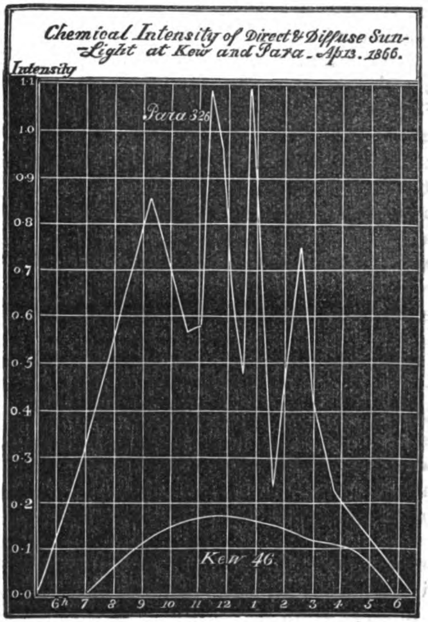
* See Golding Bird, 'Natural Philosophy,' 5th edition, p. 622.

The speaker desired, however, chiefly to direct attention this evening to some experiments which appear to throw light upon that much vexed question of the cause of the blue colour of the heavens and the ruddy tints of sunrise and sunset. Since the time of Leonardo da Vinci this subject has been a favourite ground for the display of meteorological speculations. Leonardo, and afterwards Goethe, believed that the blueness of an unclouded sky was due to the passage of the white light through the atmosphere containing finely-divided particles. Newton explained the blue colour of the heavens by the existence in the atmosphere of hollow very minute vesicles of water, upon which, as on a soap-bubble, the colours of thin plates become perceptible; and according as the thickness of the walls of these vesicles increased, so would the colour change from blue to yellow, orange, and red; and thus, by very frequent reflections, the various tints from sky-blue to sunset-red could be explained. Founded upon this theory Clausius has calculated the relative intensities of direct sunlight and the diffuse reflected light of the sky for varying altitudes of the sun.



Some physicists have assumed that the air itself has a blue colour, whilst others have admitted that if air be of a blue colour by *reflected* light, it must appear red by *transmitted* light.

Others again, in order to avoid the difficulty of explaining the great variety of sunset tints, have assumed these tints to be an ocular deception, or caused by the presence of clouds which receive and repeat the colour!

Many physicists have suggested that the atmosphere, being filled with small particles of floating solid matter, acts like an opalescent medium and transmits only red light; but it is to Brücke* that we are indebted for a complete statement and masterly investigation of this view of the subject. Forbes, again,† explains the phenomena in an entirely different manner; for he, observing that under certain circumstances aqueous vapour, or rather water in finely divided particles, is able to absorb the blue rays, and that the sun looked red when seen through a particular portion of a jet of escaping steam, attributes the sunset-red solely to the presence of water in this peculiar state of division.

In order to appreciate the value of these various opinions, it appears of special interest to obtain a knowledge of some quantitative facts respecting the intensity of the light transmitted directly from the sun, and that reflected by the air or particles in the atmosphere The possibility of making such measurements with respect to those parts of the sun's light which may be expected to show great differences in reflection and transmission, *viz.* the most refrangible portions, is rendered at once evident by the employment of the simple method of measuring the chemical intensity of light which has been above alluded to. The method employed consists simply in determining the chemical intensity of the total daylight (sunlight and diffused light), and immediately afterwards shading off the sun's direct rays by means of a small disc or sphere of metal, whose apparent diameter is only slightly greater than that of the solar disc seen from the position of the sensitive paper. In this way the chemical intensity of the total (direct and diffused) light is compared with that given off by the whole of the heavens alone, and the difference gives the chemical intensity of the direct sunlight.

Experiment soon proved that the relative intensity of the chemical light coming directly from the sun is very much less than we should ordinarily suppose, judging from the intensity of the visible light. Thus, at Owen's College, Manchester, it was found when the sun was 12° 3′ above the horizon, that of 100 chemically active rays falling on the horizontal surface, less than 5 were due to the direct sunlight, whilst 95 came from the diffused light of the heavens, even when the sky was unclouded. At the same instant, of 100 rays of visible light as affecting the eye, 60 came directly from the sun, and only 40 from the diffuse sky-light. This singular result was also observed at Cheetham Hill, by Mr. Baxendell, and at Heidelberg, by Dr. Wolkoff; indeed, at this latter station, it was found on several occasions that whilst the sun was shining brightly, it was totally devoid of chemical rays, the

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* 'Pogg. Ann.,' vol. lxxxviii. p. 363.

† "On the Colour of Steam under certain Circumstances, and on the Colours of the Atmosphere:" 'Edin. Transactions,' xiv. p. 371; 'Phil. Mag.,' xiv. xv. 3rd Series.

interposition of the small disc producing no diminution in the chemical action.

Thus, at altitudes from 0° 34′ to 12° 58′ on the following occasions, the sunlight was robbed entirely of its chemically active rays by passage through the atmosphere.

Altitude.	Direct Sun.	Diffuse Light.
0° 34′	0·000	0·026
1 32	0·000	0·024
2 29	0·000	0·038
3 27	0·000	0·028
6 0	0·000	0·030
10 40	0·000	0·073
11 51	0·000	0·079
12 58	0·000	0·080

The same inactive condition of the sun at low altitudes has frequently been observed at Kew, Cheetham Hill, and Owens College.

The following numbers give the results of an extended series of observations made at Heidelberg, by Dr. Wolkoff, at Kew by Mr. Baker, at Cheetham Hill by Mr. Baxendell, at Owens College by myself, and at Pará (Brazils) by Mr. T. E. Thorpe

The last column gives the ratio of chemical intensity of sun to sky, the fraction of the action of the diffuse light which the direct sun exerts. Thus, the ratio 0·106 at Owens College means that if 1 represents the intensity of the chemical light from the diffused light of the whole sky, 0·106 was the intensity of the ray emanated directly from the sun.

Results of Observations at Heidelberg.

	Number of Observations.	Range of Altitude of Sun.	Mean Altitude of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
Group 1	10	0° to 15°	7° 15′	·048	·002	0·041
„ 2	19	15 — 30	24 43	·134	·066	0·472
„ 3	31	30 — 45	34 34	·170	·136	0·800
„ 4	22	45 — 60	53 37	·174	·263	1·511
„ 5	17	above 60	62 30	·199	·319	1·603

Results of Observations at Cheetham Hill.

	Number of Observations.		Mean Altitude of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	23	24	19° 30′	·064	·012	0·187
„ 2	22	22	25 31	·091	·019	0·208
„ 3	18	17	34 8	·104	·026	0·250

Results of Observations at Owens College.

	Number of Observations.		Mean Altitude. of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	33	34	17° 8′	·066	·007	0·106
„ 2	20	24	26 38	·074	·008	0·108
„ 3	4	5	54 12	·140	·043	0·308

Results of Observations at Kew.

	Number of Observations.		Mean Altitude of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	18	18	12° 55′	0·065	0·014	0·213
„ 2	8	8	21 8	0·072	0·030	0·416
„ 3	7	7	28 16	0·104	0·056	0·538
„ 4	6	6	41 23	0·135	0·107	0·792

Results of Observations at Pará (Brazils).

	Number of Observations.		Mean Altitude of Sun.	Insensity of Sky or diffused Daylight.	Intensity of Direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	20	20	42° 21′	·451	·168	·372
„ 2	25	25	62 49	·552	·277	·501
„ 3	25	25	77 20	·660	·267	·404

If we compare the numbers thus obtained by experiment with those calculated by Clausius on the theory of hollow vesicles impeding the passage of the rays, we shall notice a most remarkable difference between the experimental and calculated numbers.

Ratio of Chemical Intensities of direct Sunlight to diffused Light.

Sun's Altitude.	Calculated (Clausius).	Experiments.			
		Heidelberg.	Cheetham Hill.	Owens College.	Kew.
20°	0·491	0·350	0·19	0·10	0·36
25	0·896	0·480	0·20	0·11	0·47
30	1·320	0·650	0·23		0·57
35	1·690	0·820	0·26		0·65
40	2·032	1·00	—		0·75
50	2·634	1·37	—		
60	3·129	1·60	—		

Thus, whilst the theory requires that at an elevation of 20° the relation of diffuse light to sunlight was as 100 to 49·1, the experiments at Heidelberg showed a relation of 100 to 35, those of Kew of 100 to 30, at Cheetham Hill of 100 to 19, and at Owens College of 100 to 10, whilst the differences at higher altitudes becomes still greater. The Heidelberg observations were made on the summit of the Königstuhl, at an elevation of nearly 2000 feet above the sea level, and therefore at a position beneath which a very considerable portion of the densest air was situated; when the sun attained an altitude of 40°, the direct sun's rays exert the same amount of chemical action as the diffused light of a cloudless sky. At Kew Observatory, this point of equality is not nearly reached when the altitude of the sun is 42°. At Pará, under the equator, this difference between the chemical intensity of direct and diffuse sunlight becomes even more striking, for with an altitude of 77° the ratio of direct to diffuse is less than 0·5; that is, if 100 rays come from the diffused daylight only 50 come from the direct sunlight. This is certainly a very remarkable result. We thus see that the high tropical light curves are mainly caused, not by the increase of the chemically active rays in the direct sunlight, but by the enormous increase in the chemical activity of the diffuse light. It must, however, be borne in mind, that in the Pará observations the sky was not cloudless, and much light is reflected from the heavy cumuli; it is, nevertheless, remarkable, that under a tropical sun at an altitude of 80°, the diffuse daylight should exert a chemical action twice as great as the direct sunlight.

That the relation between the chemically active constituents of sunlight, direct and diffused, is quite different from the relation of the visible rays, can be easily ascertained. In some of the experiments made at Cheetham Hill, the shadow of a small disc was thrown on a horizontal surface of white paper, and careful estimations made of the relative brightness of the shaded and unshaded portions of the surface. A comparison of these results with those obtained at the same time for the chemical rays showed that when the sun's mean altitude was 25° 16′, the mean ratio of the chemical intensities of direct and diffuse light was 0·23 (or for 100 of diffuse light there was 23 of direct sunlight), whilst the ratio of the visible intensities was 4·0 (or for 100 of diffuse light there was 400 of direct sunlight). This shows that the action of the atmosphere was 1·74 times greater on the chemical than on the visible rays. Again, at Owens College, with a mean altitude of 12° 3′, the ratio of chemical intensity was 0·053, that of the visible intensity being 1·4; or the action of the atmosphere was 26·4 times as great upon the chemical as upon the visible rays.

How can we seek to explain this unexpected result—that the sun shining brightly, and casting a dark shadow, should at a height of 20° be capable of producing a chemical action of only $\frac{1}{10}$th of that produced by the diffuse light from the whole of a cloudless sky?

The explanation may be rendered plain by an experiment. Let us take a very slightly milky liquid—such as water containing $\frac{1}{10}$th grain of suspended sulphur in the gallon. So slight is the opalescence that we can scarcely detect it. Nevertheless, this minute trace of most finely-divided sulphur is sufficient to cut off the chemically active rays; the bright flash of carbonic disulphide in nitric oxide cannot explode the bulb when the opalescent solution is placed between it and the bulb; but the bulb explodes instantly when the light is allowed to pass through pure water.

We have here an exact imitation of the condition of the atmosphere as regards the chemically active rays. We see that light of a high degree of refrangibility cannot pass through the water containing the finely-divided sulphur; it is reflected back again by the particles of sulphur. So, too, the atmosphere is filled with particles which reflect the blue rays and transmit the red. What the exact nature of these particles may be, it is hard to say. We know, however, that the air is always filled with minute solid bodies. We see that in the sporules which are constantly present and cause fermentation and putrefactive decomposition. We see it also in the fact that particles of soda can always be detected in the atmosphere by spectrum-analysis. We notice these particles as motes dancing in the sunbeam, or in those grander paths of light which sometimes shoot up into the sky from a setting sun. The phenomenon may, perhaps, be caused by that finely-divided extra-terrestrial meteoric dust, which is, according to many physicists, constantly falling through the atmosphere to the earth's surface. These solid particles in the air *may* produce the above effects, and certainly do produce them; but we must remember that small particles of water are also able to transmit only red rays, and that, as Forbes has shown, the glorious ruddy tints of the setting sun are doubtless partly caused by aqueous vapour.

If the white beam of the electric lamp be passed through a tube 3 feet long, fitted with glass plates at each end, and filled with a scarcely visibly opalescent liquid, all the blue, green, and yellow rays will be completely cut off, and the immerging beam of light is deep red. Here indeed we have an artificial sunset. The finely divided sulphur reflects blue light and transmits red. If the visible light is diminished to one-third by means of opalescent sulphur, the chemically active rays are altogether cut off. The variation in the amount of this finely-divided matter, whether solid or liquid, in the air, will naturally produce variation in the tints of sunrise and sunset, and the presence at sunset of more aqueous vapour on the point of being condensed than at sunrise will explain the greater depth of colour in the setting than in the rising sun; the tints of dawn being, according to Mr. Brayley, those of evening in the reverse order.

In opal glass we have perhaps a still better illustration of the action of the atmosphere upon the chemically active rays. The opalescence of the glass is caused by the presence of very minute particles of bone-ash (calcium phosphate), or of arsenic trioxide, which are disseminated throughout the mass. By reflected light this glass appears white or blueish-white, by transmitted light it appears orange. If we place a bright source of white light behind the glass, we see that the direct rays are red, whilst the general diffused light reflected from the particles of the finely-divided matter in the glass is blueish-white.

That the size of the particles between which the light passes modifies the character of the transmitted ray scarcely admits of doubt.

This is most clearly exemplified in the beautiful phenomena of blue and ruby gold investigated by Mr. Faraday. Gold in thin plates reflects yellow, and transmits green light; but when suspended in a very fine state of division in water, it transmits blue, purple, or ruby light, according to the state of division in which it is precipitated.

The blue, purple, and ruby solutions all contain metallic gold in suspension, as Mr. Faraday has most conclusively shown, and yet they transmit totally different rays.

Hence we may fairly suppose that the varying size of the reflecting particles may aid in producing the widely differing sunset tints, from deep ruby-red to yellow and even *blue;* for we are not without several well-authenticated cases in which the sun has been seen to be *blue.* Thus, in the year 1831, a blue sun was noticed over a great part of Europe, as also in America.

We have seen that the light transmitted by finely-divided sulphur is red—it is, however, singular that blue sulphur can be formed. If we add ferric chloride to solution of sulphuretted hydrogen, we get a transient but very splendid purple tint; and we may ask ourselves whether this can be due to the size of the particle. If we heat sulphuretted hydrogen water up to 200° C. the gas decomposes, sulphur being deposited, and the solution attains a deep blue colour. Can this possibly be due to the minute division, almost approaching solution, which the sulphur attains? We find that on cooling the colour disappears, sulphur is deposited, and the liquid becomes milky. If we dissolve sulphur in sulphuric trioxide (anhydrous sulphuric acid), no chemical action that we know of occurs, and we get a magnificent deep-blue colour. Can this again be due to the minute division of the sulphur, thus permitting the blue rays alone to pass?

Finally, it is interesting to learn that both the analogues of sulphur, selenium and tellurium, yield magnificently coloured liquids when acted upon by sulphuric trioxide. Selenium in this state yields a deep olive-green solution, and tellurium a magnificent ruby-red colour.

Can these colours likewise be caused by the reflection or absorption of one kind of light and the preferential transmission of another kind by finely-divided particles?

The ruby-red gold liquid and ruby-red gold glass are both as transparent, and the one is apparently as truly a liquid as the red solution of tellurium. Yet we know that finely suspended metallic gold is the cause of this red tint. Are we acting contrary to analogy in supposing that the colour of this red liquid is caused by the particles of finely-divided tellurium, or that of these blue and green liquids by the particles of sulphur and selenium?

The speaker felt that he was here entering upon debateable ground, that, namely, of the cause of the colour of natural bodies; it was with much diffidence that he brought forward these examples of coloured solutions, and he did so only because they forced themselves on to his notice in the consideration of the plainer and now somewhat better understood phenomenon of the Opalescence of the Atmosphere.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1866.

WILLIAM POLE, Esq. M.A. F.R.S. in the Chair.

Edward Beanes, Esq. C.E. F.C.S.
Robert C. L. Bevan, Esq.
were *elected* Members of the Royal Institution.

John Hogg, M.D.
was *admitted* a Member of the Royal Institution.

The Special Thanks of the Members were returned to Sir Henry Holland, Bart. the President, for his Eighth Annual Donation of £40 to "the Donation Fund for the Promotion of Experimental Researches" (*see* page 151).

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM
Asiatic Society, Royal—Journal, Vol. II. Part 1. 8vo. 1866.
Astronomical Society, Royal—Monthly Notices, 1865–6. No. 6. 8vo.
British Architects' Institute, Royal—Sessional Papers, 1865–6. Part III. No. 1. 4to.
Editors—Artizan for May, 1866. 4to.
  Athenæum for May, 1866. 4to.
  British Journal of Photography for May, 1866. 4to.
  Chemical News for May, 1866. 4to.
  Engineer for May, 1866. fol.
  Horological Journal for May, 1866. 8vo.
  Journal of Gas-Lighting for May, 1866. 4to.
  Mechanics' Magazine for May, 1866. 8vo.
  Pharmaceutical Journal for May, 1866.
  Practical Mechanics' Journal for May, 1866. 4to.
Franklin Institute—Journal, Nos. 480, 481, 442, 483. 8vo. 1866.
Geographical Society, Royal—Proceedings, Vol. X. No. 3. 8vo. 1866.
Geological Society—Quarterly Journal, No. 86. 8vo. 1866.
Horticultural Society, Royal—Proceedings, 1866. No. 4. 8vo.
Jones, H. Bence, M.D. F.R.S. Hon. Sec. R.I.—Third Report of the Cattle Plague Commissioners. fol. 1866.
Leeds Literary and Philosophical Society—Annual Report, 1864–5. 8vo.
  Catalogue of the Library. 8vo. 1866.
Macpherson, John, M.D. M.R.I. (*the Author*)—Cholera in its Home. 8vo. 1866.
Mechanical Engineers' Institution, Birmingham—Proceedings, August, 1865. Part 3. 8vo.
Meteorological Society—Proceedings, No. 24. 8vo. 1866.
Photographic Society—Journal, No. 169. 8vo. 1866.
Royal Society of London—Proceedings, Nos. 82, 83. 8vo. 1866.
Sidney, Rev. Edwin, M.A.—Constantinus, R.: Lexicon Græco-Latinum. fol. 1592.
United Service Institution, Royal—Journal, Appendix to Vol. IX. 8vo. 1865.

Vereins zur Beförderung des Gewerbsfleisses in Preussen—Verhandlungen, Sept.-Dez. 1865. 4to.
Wilson and Beadell, Messrs.—Portrait of Sir Heny Holland, M.D. D.C.L. F.R.S. President R.I. 1866.
Yorkshire Geological and Polytechnic Society—Report for 1864-5. 8vo. 1866.

WEEKLY EVENING MEETING,

Friday, June 8, 1866.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair,

Edward Frankland, Ph.D. F.R.S.
PROFESSOR OF CHEMISTRY, R.I.

On the Source of Muscular Power.

What is the source of muscular power? Twenty years ago, if this question had been asked, there were but few philosophers who would have hesitated to reply, "The source of muscular power is that peculiar force which is developed by living animals, and which we term the *vital force!*" but the progress of scientific discovery has rendered the view implied in such an answer so utterly untenable that, at the present moment, no one possessing any knowledge of physical science would venture to return such a reply. We now know that an animal, however high its organization may be, can no more *generate* an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive drive a train without fuel. All that such an animal can do is to liberate that store of force, or *potential energy*, which is locked up in its food. It is the *chemical change* which food suffers in the body of an animal that liberates the previously pent-up forces of that food, which now make their appearance in the form of *actual energy*—as heat and mechanical motion.

From food, and food alone, comes the *matter* of which the animal body is built up; and from food alone come all the different kinds of *physical force* which an animal is capable of manifesting.

The two chief forms of force thus manifested are *Heat* and *Muscular motion* or *mechanical work*, and these have been almost universally traced to two distinct sources—the *heat* to the oxidation of the *food*, and the *mechanical work* to the oxidation of the *muscles*.

This doctrine, first promulgated, the speaker believed, by Liebig, occupies a prominent position in that philosopher's justly celebrated 'Chemico-Physiological Essays.'

In his work entitled 'Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, Braunschweig, 1842,' Liebig says, "All experience teaches that there is only one source of mechanical power in the organism, and this source is the transformation of the living parts of the body into lifeless compounds. . . . This transformation occurs in consequence of the combination of oxygen with the substance of the living parts of the body." And again, in his 'Letters on Chemistry, 1851,' p. 366, referring to these living parts of the body, he says, "All these organized tissues, all the parts which in any way manifest force in the body are derived from the albumen of the blood; all the albumen of the blood is derived from the plastic or sanguineous constituents of the food, whether animal or vegetable. It is clear, therefore, that the plastic constituents of food, the ultimate source of which is the vegetable kingdom, are the conditions essential to all production or manifestation of force, to all these effects which the animal organism produces by means of its organs of sense, thought, and motion." And again, at page 374, he says, "The sulphurized and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organized structures, and the producers of force; the latter support the respiratory process, they are *materials for respiration.*"

This doctrine has since been treated as an almost self-evident truth in most physiological text-books; it has been quite recently supported by Ranke;* and, in his lecture 'On the Food of Man in relation to his Useful Work, 1865,' Playfair says, page 37, "From the considerations which have preceded, we consider Liebig amply justified in viewing the non-nitrogenous portions of food as mere heat-givers. . . . While we have been led to the conclusion that the transformation of the tissues is the source of dynamical power in the animal." At page 30 he also says, "I agree with Draper and others in considering the contraction of a muscle due to a disintegration of its particles, and its relaxation to their restoration. . . . All these facts prove that transformation of the muscle through the agency of oxygen is the condition of muscular action." Finally, in a masterly review of the present relations of chemistry to animal life, published in March last,† Odling says, page 98, "Seeing, then, that muscular exertion is really dependent upon muscular oxidation, we have to consider what should be the products, and what the value of this oxidation." . . . And again, page 103, "The slow oxidation of so much carbon and hydrogen in the human body, therefore, will always produce its due amount of heat, or an equivalent in some other form of energy; for while the latent force liberated by the combustion of the carbon and hydrogen of fat is expressed *solely in the form of heat*, the combustion of an equal quantity of the carbon and hydrogen of voluntary muscle is expressed *chiefly in the form of motion.*"

Nevertheless, this view of the origin of muscular power has not escaped challenge. Immediately after its first promulgation, Dr. J.

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* 'Tetanus eine Physiologische Studie.' Leipzig. 1865.

† 'Lectures on Animal Chemistry.'

R. Mayer wrote,* "A muscle is only an apparatus by means of which the transformation of force is effected, *but it is not the material by the chemical change of which* mechanical work is produced." He showed that the 15 lbs. of dry muscles of a man weighing 150 lbs. would, if their mechanical work were due to their chemical change, be completely oxidized in 80 days, the heart itself in 8 days, and the ventricles of the heart in 2¼ days. After endeavouring to prove by physiological arguments that not one per cent. of the oxygen absorbed in the lungs could possibly come into contact with the substance of the muscles, Mayer says, "The fire-place in which this combustion goes on is the interior of the blood-vessels, the *blood* however—a slowly-burning liquid—is the oil in the flame of life. . . . Just as a plant-leaf transforms a given mechanical effect, *light*, into another force, *chemical difference*, so does the muscle produce mechanical work at the cost of the chemical difference consumed in its capillaries. Heat can neither replace the sun's rays for the plant, nor the chemical process in the animal: every act of motion in an animal is attended by the consumption of oxygen and the production of carbonic acid and water; every muscle to which atmospheric oxygen does not gain access ceases to perform its functions."

But Mayer was not the first to conceive this view of muscular action. Nearly 200 years ago, a Bath physician, Dr. John Mayow,† distinctly stated that for the production of muscular motion two things are necessary—the conveyance of combustible substances to the muscle by the blood, and the access of oxygen by respiration. He concluded that the chief combustible substance so used was fat. A century before Priestley isolated oxygen, Mayow was aware of its existence in the air, in nitre, and in nitric acid; he knew that combustion is supported by the oxygen of the air, and that this gas is absorbed in the lungs by the blood, and is absolutely necessary for muscular activity.

For two decades this doctrine sank into oblivion; and it is only within the last two years that it has been again advanced, chiefly by Haidenhain,‡ Traube, and, to a limited extent, by Donders.‖

Experimental evidence was, however, still wanting to give permanent vitality to the resuscitated doctrine; for although the laborious and remarkable investigations of Voit § and of Edward Smith¶ point unmistakably in the direction of Mayow and Mayer's hypothesis, yet

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* 'Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel,' 1845.

† 'De Motu musculari,' 1681. Mayow was born in 1645, and died 1679.

‡ 'Mechanische Leistung Wärmeentwickelung und Stoffumsatz bei der Muskelthätigkeit,' 1864.

‖ As this is passing through the press, the speaker has become aware that Messrs. Lawes and Gilbert advocated this doctrine in 1852, and repeatedly since; their opinions being founded upon experiments on the feeding of cattle.

§ 'Untersuchungen über den Einfluss des Kochsalzes, des Kaffeés und der Muskel-bewegungen auf den Stoffwechsel,' p. 150. Munich, 1860.

¶ 'Phil Trans.,' 1861, p. 747.

the results of these physiologists were not sufficiently conclusive to render the opposite view untenable. This want of data of a sufficiently conclusive character has been supplied by a happily conceived experiment undertaken by Fick and Wislicenus in the autumn of last year, and described in the 'Philosophical Magazine,' vol. xxxi. p. 485. In the application of these data, however, to the problem now under consideration, one important link was found to be wanting, *viz.* the amount of actual energy generated by the oxidation of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words:—"The question now arises what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question, for neither the heat of combustion of muscle nor of the nitrogenous *residue* (urea) of muscle is known." Owing to the want of these data, the numerical results of the experiment of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle combustion than they otherwise would have been, whilst similar determinations, which have been made by Edward Smith, Haughton, Playfair, and others, are even liable to a total misinterpretation from the same cause.

The speaker stated that he had supplied this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen. Availing himself of these data he then proceeded to the consideration of the problem to be solved, the present condition of which might be thus summed up:—It is agreed on all hands that muscular power is derived exclusively from the mutual chemical action of the food and atmospheric oxygen; but opinions differ as to whether that food must first be converted into the actual organized substance of the muscle, before its oxidation can give rise to mechanical force, or whether it is not also possible that muscular work may be derived from the oxidation of the food, which has only arrived at the condition of blood and not of organized muscular tissue.

The importance of this problem can scarcely be overrated; it is a corner-stone of the physiological edifice, and the key to the phenomena of the nutrition of animals. For its satisfactory solution the following data require to be determined:—

1st. The amount of force or actual energy generated by the oxidation of a given amount of muscle in the body.

2nd. The amount of mechanical force exerted by the muscles of the body during a given time.

3rd. The quantity of muscle oxidized in the body during the same time.

If the total amount of force involved in muscular action, as measured by the mechanical work performed, be greater than that which could possibly be generated by the quantity of muscle oxidized during the same time, it necessarily follows that the power of the muscles is not derived *exclusively* from the oxidation of their own substance.

As regards the first datum to be determined, it is necessary to agree upon some unit for the measurement of mechanical force. The unit most commonly adopted is that represented by the lifting of a kilogram weight to the height of one metre. The researches of Joule and Mayer have connected this standard unit with heat;—they prove that the force required to elevate this weight 425 times will, when converted into heat, raise the temperature of an equal weight of water 1° C. If this weight were let fall from a height of 425 metres, its collision with the earth would produce an amount of heat sufficient to raise the temperature of 1 kilogram of water 1° C. The same heating effect would also of course be produced by the fall of 425 kilograms through 1 metre. This standard of force is termed a *metrekilogram;*[*] and 425 metrekilograms are equal to that amount of heat which is necessary to raise the temperature of 1 kilogram of water through 1° C. If then it be found that the heat evolved by the combustion of a certain weight of charcoal or muscle, for instance, raises the temperature of 1 kilogram of water through 1° C., this means, when translated into mechanical power, 425 metrekilograms. Again, if a man weighing 64 kilograms climbs to a height of 1,000 metres, the ascent of his body to this height represents 64,000 metrekilograms of work; that is, the labour necessary to raise a kilogram weight to the height of 1 metre 64,000 times.

In order to estimate the amount of actual energy generated by the oxidation of a given amount of muscle in the body, it is necessary to determine, first, the amount of actual energy generated by the combustion of that amount of muscle in oxygen, and then to deduct from the number thus obtained the amount of energy still remaining in the products of the oxidation of this quantity of muscle which leave the body. Of these products, urea and uric and hippuric acids are the only ones in appreciable quantity which still retain potential energy on leaving the body, and of these the two latter are excreted in such small proportions that they may be considered as urea without introducing any material error into the results.

These determinations were made in Lewis Thompson's calorimeter, which consists of a copper tube to contain a mixture of chlorate of potash with the combustible substance, and which can be enclosed in a kind of diving-bell, also of copper, and so lowered to the bottom of a suitable vessel containing a known quantity (2 litres) of water. The determinations were made with this instrument in the following manner:—19·5 grams of chlorate of potash, to which about ⅛th of peroxide of manganese was added was intimately mixed with a known weight (generally about 2 grams) of the substance whose potential energy

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[*] I follow the example of the Registrar-General in abbreviating the French word *gramme* to gram.

was to be determined, and the mixture being then placed in the copper tube above mentioned, a small piece of cotton thread previously steeped in chlorate of potash and dried was inserted in the mixture. The temperature of the water in the calorimeter was now carefully ascertained by a delicate thermometer; and the end of the cotton thread being ignited the tube with its contents was placed in the copper bell and lowered to the bottom of the water. As soon as the combustion reached the mixture a stream of gases issued from numerous small openings at the lower edge of the bell and rose to the surface of the water—a height of about 10 inches.

At the termination of the deflagration, the water was allowed free access to the interior of the bell, by opening a stopcock connected with the bell by a small tube rising above the surface of the water in the calorimeter. The gases in the interior of the bell were thus displaced by the incumbent column of water, and by moving the bell up and down repeatedly a perfect equilibrium of temperature throughout the entire mass of water was quickly established. The temperature of the water was again carefully observed, and the difference between this and the previous observation determines the calorific power or potential energy, expressed as heat, of the substance consumed.

The value thus obtained is, however, obviously subject to the following corrections:—

1. The amount of heat absorbed by the calorimeter and apparatus employed, *to be added.*
2. The amount of heat carried away by the escaping gases, after issuing from the water, *to be added.*
3. The amount of heat due to the decomposition of the chlorate of potash employed, *to be deducted.*
4. The amount of heat equivalent to the work performed by the gases generated in overcoming the pressure of the atmosphere, *to be added.*

Although the errors due to these causes to some extent neutralize each other, there is still an outstanding balance of sufficient importance to require that the necessary corrections should be carefully attended to.

The amount of error from the first cause was once for all experimentally determined, and was added to the increase of temperature observed in each experiment.

The amount of heat carried away by the escaping gases after issuing from the water may be divided into two items, *viz.* :—

a. The amount of heat rendered latent by the water which is carried off by the gases in the form of vapour.

b. The amount of heat carried off by these gases by reason of their temperature being above that of the water from which they issue.

It was ascertained that a stream of dry air when passed through the water of the calorimeter, at about the same rate and for the same period of time as the gaseous products of combustion, depressed the temperature of the water by only $0^{\circ}{\cdot}02$ C.

By placing a delicate thermometer in the escaping gases, and another in the water, no appreciable difference of temperature could be observed. Both these items may therefore be safely neglected.

The two remaining corrections can be best considered together, since a single careful determination eliminates both. When a combustible substance is burnt in gaseous oxygen, the conditions are essentially different from those which obtain when the same substance is consumed at the expense of the combined or solid oxygen of chlorate of potash. In the first case the products of combustion, when cooled to the temperature of the water in the calorimeter, occupy less space than the substances concerned in the combustion, and no part of the energy developed is therefore expended in external work, that is, in overcoming the pressure of the atmosphere. In the second case, both the combustible and the supporter of combustion are in the solid condition, whilst a considerable proportion of the products of combustion are gases. The generation of the latter cannot take place without the performance of external work, for every cubic inch produced must obviously, in overcoming atmospheric pressure, perform an amount of work equivalent, in round numbers, to the lifting of a weight of 15 lbs. to the height of one inch. In performing this work the gases are cooled, and consequently less heat is communicated to the water of the calorimeter. Nevertheless, the loss of heat due to this cause is but small. Under the actual conditions of the experiments detailed below, its amount would only have increased the temperature of the water in the calorimeter by 0°·07 C. Even this slight error is entirely eliminated by the final correction which we have now to consider.

It is well known that the decomposition of chlorate of potash into chloride of potassium and free oxygen is attended with the evolution of heat. If a few grains of peroxide of manganese, or better, of peroxide of iron, be dropped into an ounce or two of fused chlorate of potash which is slowly disengaging oxygen, the evolution of gas immediately proceeds with great violence, and the mixture becomes visibly red hot, although the external application of heat be discontinued from the moment when the metallic peroxide is added. The latter remains unaltered at the close of the operation. It is thus obvious that chlorate of potash, on being decomposed, furnishes considerably more heat than that which is necessary to gasify the oxygen which it evolves. It was therefore necessary to determine the amount of heat thus evolved by the quantity of chlorate of potash (9·75 grams) mixed with one gram of the substance burnt in each of the following determinations. This was effected by the use of two copper tubes, the one placed within the other. The interior tube was charged with a known weight of the same mixture of chlorate of potash and peroxide of manganese as that used for the subsequent experiments, whilst the annular space between the two tubes was filled with a combustible mixture of chlorate and spermaceti, the calorific value of which had been previously ascertained. The latter mixture was ignited in the calorimeter as before, and the heat generated during its combustion effected the complete decomposition of the chlorate in the interior cylinder, as was proved by a subsequent examination of the liquid in the calorimeter, which contained no traces of undecomposed chlorate. The following are the results of five experiments thus made, expressed in units of heat, the unit being equal to 1 gram of water raised through 1° C. of temperature:—

	Units of Heat.
1st experiment	340
2nd „	300
3rd „	375
4th „	438
5th „	438
	5)1891
Mean	378

This result was confirmed by the following experiments:—

1. Starch was burnt, firstly, in a current of oxygen gas, and secondly, by admixture with chlorate of potash and peroxide of manganese.

Heat units furnished by one gram of starch burnt with 9·75 grams chlorate of potash	4290
Heat units furnished by the same weight of starch burnt in a stream of oxygen gas	3964
Difference	326

2nd. Phenylic alcohol was burnt with chlorate of potash, and the result compared with the calorific value of this substance as determined by Favre and Silbermann.

Heat units furnished by one gram of phenylic alcohol burnt with 9·75 grams chlorate of potash	8183
Heat units furnished by one gram of phenylic alcohol when burnt with gaseous oxygen (Favre and Silbermann)	7842
Difference	341

These three determinations of the heat evolved by the decomposition of 9·75 grams of chlorate of potash, furnishing the numbers 378, 326, and 341, agree as closely as could be expected, when it is considered that all experimental errors are necessarily thrown upon the calorific value of the chlorate of potash.

The mean of the above five experimental numbers was, in all cases, deducted from the actual values read off in the following determinations.

It was ascertained by numerous trials that all the chlorate of potash was decomposed in the deflagrations, and that but mere traces of carbonic oxide were produced.

Joule's mechanical equivalent of heat was employed, *viz.* 1 kilogram of water raised 1° C. = 423 metrekilograms.

The following results were obtained:

Actual energy developed by one gram of each substance when burnt in oxygen.

Name of Substance dried at 100° C.	Heat Units.					Metre-kilograms of force. (Mean.)
	1st Experiment.	2nd Experiment.	3rd Experiment.	4th Experiment.	Mean.	
Beef Muscle purified by repeated washing with ether . . .	5174	5062	5195	5088	5103	2161
Purified Albumen . .	5009	4987	..	..	4998	2117
Beef Fat	9069	..	..	..	9069	3841
Hippuric Acid . . .	5330	5437	..	..	5383	2280
Uric Acid	2645	2585	..	..	2615	1108
Urea*	2121	2302	2207	2197	2206	934

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by the oxidation of muscle within the body, because, when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water; the nitrogen being, to a great extent, evolved in the elementary state; whereas, when muscle is most completely consumed in the body, the products are carbonic acid water and urea; the whole of the nitrogen passes out of the body as urea—a substance which still retains a considerable amount of potential energy. Dry muscle and pure albumen yield, under these circumstances, almost exactly one-third of their weight of urea, and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual energy developed by one gram of each substance when consumed in the body.

Name of substance dried at 100° C.	Heat units. (Mean.)	Metrekilograms of force. (Mean.)
Beef Muscle purified by ether . .	4368	1848
Purified Albumen	4263	1803

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* The speaker showed the combustibility of urea by burning it upon asbestos in a jar of oxygen gas.

We have thus ascertained the first of our three data, *viz.* the amount of force or actual energy generated by the oxidation of a given amount of muscle in the body; and we now proceed to ascertain the second, *viz.* the amount of mechanical force exerted by the muscles of the body during a given time. For this purpose we have only to avail ourselves of the details of Fick and Wislicenus's conclusive experiment already referred to, and which consisted in the ascent of the Faulhorn in Switzerland from the lake of Brienz. This mountain can be ascended by a very steep path from Iseltwald, which was of course favourable for the experiment, and there is an hotel on the summit which allowed the experimenters to pass the following night under tolerably normal circumstances. The following is their own description and estimate of the amount of work performed in the ascent.*

"Let us now inquire how much work was really done by our muscles. One item necessary for the reply is already at hand, *viz.* the height of the summit of the Faulhorn above the level of the lake of Brienz multiplied by the weight of the body; the former reckoned in metres, the latter in kilograms. The weight of the body with the equipments (hat, clothes, stick) amounted to 66 kilograms in Fick's case, and 76 in Wislicenus's. The height of the Faulhorn above the level of the lake of Brienz is, according to trigonometric measurements, exactly 1956 metres. Therefore Fick performed 129,096 and Wislicenus 148,656 metrekilograms of muscular work."

But in addition to this measurable external work there is another item of force "which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0·64 metrekilogram† for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5·5 hours of the ascent an amount of work which may be estimated at 25,344 metrekilograms, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labour of respiration. One of us has shown, however, in the second edition of his 'Medical Physics' (p. 206), that Donders' well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0·63 metrekilogram. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5197 metrekilograms. If we add this, and the number representing the work of the heart, to

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* 'Phil. Mag.' vol. xxxi. p. 496, 1866.

† 0·43 is here assigned as the work of the left, and 0·21 as that of the right ventricle.

the external work performed by Fick, we obtain a total of 159,637 metrekilograms. If we suppose that Wislicenus's respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, *i. e.* 7 : 6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 metrekilograms.

" Besides these estimated (and certainly not over-estimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the muscles as if work were being performed which did not undergo this transformation. In order to make this point yet clearer we may take into consideration that the whole work of the ascent, only existed temporarily as work. On the following day the result was reversed; our bodies approached the centre of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

" Let us observe, besides, that in an ascent it is not only those muscles of the leg specially devoted to climbing which are exerted, the arms, head, and trunk are continually in motion. For all these movements force-generating processes are necessary, the result of which cannot, however, figure in our total of work, but must appear entirely in the form of heat, since all the mechanical effects of these movements are immediately undone again. If we raise an arm, we immediately let it drop again, &c.

" There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. We may conceive of a tetanized muscle as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active but it performs no work, and therefore all the force produced is liberated in the form of heat."

Thus the total amount of measured and estimable work performed in 5·5 hours in the experiments before us was 159,637 metrekilograms for Fick, and 184,287 metrekilograms for Wislicenus. This is our second datum.

The third, *viz.* the amount of muscle oxidized in the body during the performance of this work has been carefully determined by the same experimenters, as well as the rate of muscle consumption before and after the ascent. For the details of these determinations the speaker referred his hearers to the 'Philosophical Magazine' for 1866, vol. xxxi. page 488; but the following is a condensed summary of the results:—

Ascent of the Faulhorn.

	Fick.	Wislicenus.
	Gram.	Gram.
Amount of Nitrogen secreted in Urine per hour before ascent	·63	·61
Weight of dry Muscle corresponding to Nitrogen	4·19	4·05
Amount of Nitrogen secreted per hour during ascent . . .	·41	·39
Weight of dry Muscle corresponding to Nitrogen	2·70	2·56
Amount of Nitrogen secreted per hour during 6 hours after the ascent	·40	·40
Weight of dry Muscle corresponding to Nitrogen	2·63	2·63
Amount of Nitrogen secreted per hour during the following night	·45	·51
Weight of dry Muscle corresponding to Nitrogen	3·06	3·39
Total amount of Nitrogen secreted during ascent	3·31	3·13
Ditto during 6 hours after ascent	2·43	2·42
	5·74	5·55
Weight of dry Muscle corresponding to Nitrogen secreted . . During ascent	20·98	20·89
During 6 hours after ascent	16·19	16·11
	37·17	37·00

The results of these determinations add a new link to the chain of experimental evidence, that muscular exertion does not necessarily increase the excretion of nitrogen through the urine. From mid-day before the ascent (August 29th, 1865) to the following evening at seven o'clock (August 30th) both gentlemen abstained from all nitrogenous food. During these thirty-one hours they had nothing in the way of solid food except starch, fat, and sugar. The two former were taken in the form of cakes. Starch was made up with water into a thin paste, which was then made into small cakes and fried with plenty of fat. The sugar was taken dissolved in tea. In addition to this there was the sugar contained in the beer and wine, which were taken in quantities usual in mountain excursions. It was doubtless owing to this absence from food containing nitrogen that the amount of this element secreted through the urine, declined tolerably regularly from the 29th of August till the evening of the 30th. Even in the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this is probably to be sought for in the circumstance that during the period of abstinence, the secretion of nitrogen was carried on at the expense of tissues, and now these tissues required reparation.

It is perhaps scarcely worthy of record that during the ascent neither of the experimenters perspired perceptibly, since it has been proved by Ranke that no appreciable amount of nitrogen leaves the system in the matter of perspiration; and as Thiry has also shown that no nitrogen is got rid of by respiration, it follows that in addition to the nitrogen contained in the urine, the only other mode of exit for this element is through the fæces. Now the proportion secreted through the fæces has been estimated by Ranke at about one-twelfth of that in the urine; but inasmuch as all experiments on the subject tend to show that this alvine nitrogen is, as voided, a constituent of un-oxidized compounds, that is, of compounds that have not yielded up their force, it has no claim upon our attention.

There is still another circumstance which requires to be taken into consideration before we proceed to apply our three data to the solution of the problem before us. It is this:—Is it possible that at the termination of the ascent of the Faulhorn there might be a considerable quantity of the nitrogenous products of decomposition retained in the body? Considering the physiological effect of the retention of urea in the system, as exemplified whenever the secretion of urine is interrupted, it is difficult to imagine the possibility of any considerable quantity of urea being retained in the system of a healthy man. It is, however, otherwise with creatin, another of the products of the metamorphosis of tissue; for it has been repeatedly shown that a muscle which has been hard worked contains more creatin than one that has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be ·14 per cent. (Gregory), and that in other ox-flesh only ·06 per cent. (Staedeler). Now the muscles which extend the leg in walking, and which do the essential work in ascending, have been estimated by Weber to weigh in both legs 5·8 kilograms, and if we assume that before the ascent these muscles contained ·06 per cent. of creatin, whilst after the ascent the percentage had increased to ·14 per cent., then the amount of creatin thus exceptionally retained would amount to 4·64 grams, which would be derived from 8·4 grams of muscle.

The speaker had been unable to determine the calorific effect of creatin, and consequently the actual energy developed by the transformation of muscle into creatin; for, although he was kindly furnished with an ample supply of this material by Dr. Dittmar, yet all attempts to burn it in the calorimeter were fruitless. Even when mixed in very small proportions with chlorate of potash and other combustibles of known value, the mixture invariably exploded violently on ignition. Although actual determination thus fails us, there can be no doubt that the transformation of muscle into creatin and other non-nitrogenous products must be attended by the liberation of far less actual energy than its transformation into urea, carbonic acid, and water. To be convinced of this, it is only necessary to compare (under equal nitrogen value) the formulæ of muscle, creatin, and urea, remembering at the same time that the nitrogen probably possess no thermal value, and that each atom of oxygen destroys approximately the thermal effect of two atoms of hydrogen.

	Comparable formulæ.	Powerful or unburnt matter.
Muscle . . .	$C_{24} H_{37} N_6 O_7$	$C_{24} H_{23}$
Creatin . . .	$C_8 H_{18} N_6 O_4$	$C_8 H_{10}$
Urea . . .	$C_3 H_{12} N_6 O_3$.	$C_3 H_6$

Thus it is evident that the amount of creatin exceptionally retained in the system could not greatly affect the result of the experiment as regards the possible amount of actual energy derivable from the metamorphosed tissues during the ascent; firstly, on account of the small quantity of creatin so retained, and, secondly, because creatin still contains about one-third of the potential energy of the muscle from which it is derived. But as this point cannot be experimentally demonstrated the speaker followed the example of Fick and Wislicenus, and made a very liberal allowance on this score. He allowed, as they had done, that the whole of the nitrogen secreted during the six hours after the ascent was exceptionally retained in the system *as urea* during the ascent. This is equivalent to an admission that the muscles of the legs contained at the end of the ascent eleven times as much creatin as was present in them before the ascent. In the above tabular statement of results provision has been made for this allowance by adding together, on the one hand, the amounts of nitrogen secreted during the ascent and six hours after it, and, on the other, the weights of dry muscle corresponding to these two amounts of nitrogen.

Having thus far cleared the ground, let us now compare the amount of measured and calculated work performed by each of the experimenters during the ascent of the Faulhorn, with the actual energy capable of being developed by the maximum amount of muscle that could have been consumed in their bodies, this amount being represented by the total quantity of nitrogen excreted in each case during the ascent and for six hours afterwards.

	Fick.	Wislicenus.
Weight of dry Muscle consumed	Grams. 37·17	Grams. 37·00
Actual energy capable of being produced by the consumption of 37·17 and 37·00 grams of dry Muscle in the body	Metrekilograms. 68,690	Metrekilograms. 68,376
Measured work performed in the ascent (external work)	129,096	148,656
Calculated circulatory and respiratory work performed during the ascent (internal work)	30,541	35,631
Total ascertainable work performed . . .	159,637	184,287

It is thus evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived from the oxidation, either of their muscles, or of other nitrogenous constituents of their bodies, since the maximum of power capable of being derived from this source even under very favourable assumptions is, in both cases, less than one-half of the work actually performed. But the deficiency becomes much greater if we take into consideration the fact, that the actual energy developed by oxidation or combustion cannot be wholly transformed into mechanical work. In the best constructed steam-engine for instance, only $\frac{1}{10}$th of the actual energy developed by the burning fuel can be obtained in the form of mechanical power; and in the case of man, Helmholtz estimates that not more than $\frac{1}{5}$th of the actual energy developed in the body can be made to appear as external work. The experiments of Haidenhain, however, show that, under favourable circumstances, a muscle may be made to yield in the shape of mechanical work as much as one-half of the actual energy developed within it, the remainder taking the form of heat. Taking then this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by 2 the above numbers representing the ascertainable work performed, in order to express the actual energy involved in the production of that work. We then get the following comparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necessary for the performance of the work executed in the ascent of the Faulhorn.

	Fick.	Wislicenus.
	Metrekilograms.	Metrekilograms
Actual energy capable of being produced by Muscle metamorphosis	68,690	68,376
Actual energy expended in work performed .	319,274	368,574

Thus, taking the average of the two experiments, it is evident that *scarcely* $\frac{1}{8}$*th of the actual energy required for the work performed could be obtained from the amount of muscle consumed.*

Interpreted in the same way, previous experiments of a like kind prove the same thing, though not quite so conclusively. To illustrate this I will here give a summary of three sets of experiments: the first, made by Dr. E. Smith, upon prisoners engaged in treadmill labour; the second, by the Rev. Dr. Haughton, upon military prisoners engaged in shot drill; and the third, adduced by Playfair and made upon pedestrians, piledrivers, men turning a winch, and other labourers.

TREADWHEEL EXPERIMENTS.

A treadwheel is a revolving drum with steps placed at distances of 8 inches, and the prisoners are required to turn the wheel downwards by stepping upwards. Four prisoners, designated below as A, B, C, and D, were employed in these experiments, and each worked upon the wheel in alternate quarters of an hour, resting in a sitting posture during the intervening quarters. The period of actual daily labour was 3½ hours. The total ascent per hour 2160 feet, or per day 1·432 mile. The following are the results :—

Treadwheel Work.—(E. Smith.)

	Weight in Kilograms.	Ascent in Metres.	Days occupied in Ascent.	External work performed in Metrekilograms.	Total Nitrogen evolved.	Weight of dry Muscle corresponding to Nitrogen.
					Grams.	Grams.
A	47·6	23,045	10	1,096,942	171·3	1101·2
B	49	23,045	10	1,129,205	174·5	1121·7
C	55	20,741	9	1,140,755	168·0	1080·1
D	56	20,741	9	1,161,496	159·3	1024·3

In these experiments the measured work was performed in the short space of 3½ hours, whilst the nitrogen estimated was that voided in the shape of urea in 24 hours. It will, therefore, be necessary to add to the measured work, that calculated for respiration and circulation for the whole period of 24 hours. This amount of internal work was computed, from the estimates of Helmholtz and Fick, to be as follows ;—

Internal Work.—(Helmholtz and Fick.)

	Work performed.	Actual energy required.
	Metrekilograms.	Metrekilograms.
Circulation of the blood during 24 hours, at 75 pulsations per minute	69,120*	138,240
Respiration for 24 hours, at 12 respirations per minute	10,886	21,772
Statical activity of muscles	not determined	not determined
Peristaltic motion	„ „	„ „
	80,006	160,012

Taking this estimate for internal work, the average results of the treadwheel experiments may be thus expressed :—

Treadwheel Work.

Average external work per man per day	119,605 mks.
Average nitrogen evolved per man per day	17·7 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day	114 „
Actual energy producible by the consumption of 114 grams of dry muscle in the body	210,672 mks.
Average actual energy developed in the body of each man, *viz.*—	
External work . . . 119,605 × 2 = 239,210 mks.	
Circulation . . . 69,120 × 2 = 138,240 „	
Respiration . . . 10,886 × 2 = 21,772 „	
	399,222 „

In these experiments the conditions were obviously very unfavourable for the comparison of the amount of actual energy producible from muscle metamorphosis, with the quantity of actual energy expended in the performance of estimable work; since, during that portion of the twenty-four hours not occupied in the actual experiment, a large amount of unestimable internal work, such as the statical activity of the muscles, peristaltic motion, &c., was being performed. Nevertheless, these experiments show that the average actual energy developed in producing work in the body of each man was nearly twice as great as that which could possibly be produced by the whole of the nitrogenous matter oxidized in the body during 24 hours. It must also be remarked that the prisoners were fed upon a nitrogenous diet containing six ounces of cooked meat, without bone; a diet which, as is well known, would favour the production of urea.

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* Since making use of this number, I find that Donders estimates the work of the heart alone, for 24 hours, at 86,000 metrekilograms, a figure which is higher than that above for the combined work of circulation and respiration.

Shot-drill Experiments.

The men employed for these experiments were fed exclusively upon vegetable diet, and they consequently secreted a considerably smaller amount of nitrogen than the flesh-eaters engaged in the treadwheel work. The other conditions were, however, equally unfavourable for showing the excess of work performed, over the amount derivable from muscle metamorphosis.

In shot-drill, each man lifts a 32 lb. shot from a tressel to his breast, a height of 3 feet; he then carries it a distance of 9 feet, and lays it down on a similar support, returning unloaded. Six of these double journeys occupy one minute. The men were daily engaged with—

Shot drill . . .	3 hours.
Ordinary drill . . .	1½ „
Oakum picking . .	3½ „

The total average daily external work was estimated by Haughton at 96,316 metrekilograms per man.

The following is a condensed summary of the results of these experiments:—

Military Vegetarian Prisoners at Shot Drill.—(Haughton.)

Average external work per man per day		96,316 mks.
Average nitrogen evolved per man per day		12·1 grams
Weight of dry muscle corresponding to average nitrogen evolved per day		77·9 „
Actual energy producible by the consumption of 77·9 grams of dry muscle in the body		143,950 mks.
Average actual energy developed daily in the body of each man, *viz.* External work, 96,316 × 2 = ...	192,632 mks.	
Internal work	160,012 „	
		352,644 mks.

Owing chiefly to the vegetable diet of these prisoners, the result is more conclusive than that obtained upon the treadwheel, the amount of work actually performed being considerably more than twice as great as that which could possibly be obtained through the muscle metamorphosis occurring in the bodies of the prisoners.

Playfair's Determinations.

In these determinations the number 109,496 metrekilograms was obtained as the average amount of daily work performed by pedestrians, pile-drivers, porters, paviours, &c.; but, as the amount of muscle consumption is calculated from the nitrogen taken in the food, the conditions are as unfavourable as possible with regard to the point the speaker was seeking to establish; for it is here assumed, not only that all the nitrogen taken in the food enters the blood, but also that it is converted into muscle, and is afterwards oxidized to carbonic acid, water, and urea.

The following are the results expressed as in the previous cases:—

Hard-worked Labourer.—(Playfair.)

	Work performed.	Actual energy required.
Daily labour (external work)	109,496 mks.	218,992 mks.
Internal work	80,006 „	160,012 „
	189,502 mks.	379,004 mks.
Actual energy capable of being produced from 5·5 oz. (155·92 grms.) of flesh-formers contained in the daily food of the labourer .	..	288,140 mks.

Thus, even under the extremely unfavourable conditions of these determinations, the actual work performed exceeded that which could possibly be produced through the oxidation of the nitrogenous constituents of the daily food by more than 30 per cent.

We have seen, therefore, in the above four sets of experiments, interpreted by the data afforded by the combustion of muscle and urea in oxygen, that the transformation of tissue alone cannot account for more than a small fraction of the muscular power developed by animals; in fact, this transformation goes on at a rate almost entirely independent of the amount of muscular power developed. If the mechanical work of an animal be doubled or trebled there is no corresponding increase of nitrogen in the secretions; whilst it was proved on the other hand by Lawes and Gilbert, as early as the year 1854, that animals, under the same conditions as regarded exercise, had the amount of nitrogen in their secretions increased twofold by merely doubling the amount of nitrogen in their food. Whence then comes the muscular power of animals? What are the substances which, by their oxidation in the body, furnish the actual energy, whereof a part is converted into muscular work? In the light of the experimental results detailed above, can it be doubted that a large proportion of the muscular power developed in the bodies of animals has its origin in the oxidation of non-nitrogenous substances? For whilst the secretion of nitrogen remains nearly stationary under widely different degrees of muscular exertion, the production of carbonic acid increases most markedly with every augmentation of muscular work, as is shown by the following tabulated results of E. Smith's highly important experiments regarding the amount of carbonic acid evolved from his own lungs under different circumstances.*

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* Phil. Trans. for 1859, p. 709.

Excretion of carbonic acid during rest and muscular exertion :—

	Carbonic acid per hour.
During sleep	19·0 grams.
Lying down and sleep approaching	23·0 ,,
In a sitting posture	29·0 ,,
Walking at rate of 2 miles per hour	70·5 ,,
,, ,, 3 ,, ,,	100·6 ,,
On the treadwheel, ascending at the rate of 28·65 feet per minute	189·6 ,,

It has been already stated as a proposition upon which all are agreed, that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations, the speaker believed, prove conclusively, firstly, that the non-nitrogenous constituents of the food, such as starch, fat, &c., are the chief sources of the actual energy, which becomes partially transformed into muscular work; and secondly, that the food does not require to become organized tissue before its metamorphosis can be rendered available for muscular power; its digestion and assimulation into the circulating fluid—the blood—being all that is necessary for this purpose. It is, however, by no means the non-nitrogenous portions of food alone that are capable of being so employed, the nitrogenous also, inasmuch as they are combustible, and consequently capable of furnishing actual energy, might be expected to be available for the same purpose, and such an expectation is confirmed by the experiments of Savory upon rats,* in which it is proved that these animals can live for weeks in good health upon food consisting almost exclusively of muscular fibre. Even supposing these rats to have performed no external work, nearly the whole of their internal muscular work must have had its source in the actual energy developed by the oxidation of their strictly nitrogenous food.

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue ; the latter, like every other part of the body, requiring a continuous change of substance, whilst the chief function of the non-nitrogenous is to furnish by their oxidation the actual energy which is in part transmuted into muscular force.

The combustible food and oxygen coexist in the blood which courses through the muscle, but when the muscle is at rest there is no chemical action between them. A command is sent from the brain to the muscle, the nervous agent determines oxidation. The potential energy becomes active energy, one portion assuming the form of motion, another appearing as heat. *Here is the source of animal heat, here the origin of muscular power!* Like the piston and cylinder of a steam-engine, the muscle itself is only a machine for the transformation of heat into motion; both are subject to wear and

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* 'The Lancet,' 1863, pages 381 and 412.

tear and require renewal, but neither contributes in any important degree by its own oxidation to the actual production of the mechanical power which it exerts.

From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. The speaker had therefore made careful estimations of the calorific value of different materials used as food, by the same apparatus and in the same manner as described above for the determination of the actual energy in muscle, urea, uric acid, and hippuric acid.

The results are embodied in the following series of tables, but it must be borne in mind that it is only on the condition that the food is digested and passes into the blood, that the results given in these tables are realized. If, for instance, sawdust or paraffin oil had been experimented upon, numbers would have been obtained for these substances, the one about equal to that assigned to starch, and the other surpassing that of any article in the table; but these numbers would obviously have been utterly fallacious, inasmuch as neither sawdust nor paraffin oil is, to any appreciable extent, digested in the alimentary canal. Whilst the force values experimentally obtained for the different articles in these tables must therefore be understood as the maxima assignable to the substances to which they belong, yet it must not be forgotten that a large majority of these substances appear to be completely digestible under normal circumstances.

Actual Energy developed by One Gram of various Articles of Food when burnt in Oxygen.

NAME OF FOOD.	Heat Units.		Metrekilograms of Force.		Per cent of Water.
	Dry.	Natural Condition.	Dry.	Natural Condition.	
Cheese (Cheshire) . . .	6114	4647	2589	1969	24·0
Potatoes.	3752	1013	1589	429	73·0
Apples	3669	660	1554	280	82·0
Oatmeal.	...	4004	...	1696	...
Flour.	...	3941	...	1669	...
Pea-meal	...	3936	...	1667	...
Ground Rice	...	3813	...	1615	...
Arrowroot	...	3912	...	1657	...
Bread Crumb	3984	2231	1687	945	44·0
Ditto Crust	...	4459	...	1888	...
Beef (lean).	5313	1567	2250	664	70·5
Veal „	4514	1314	1912	556	70·9
Ham „	4343	1980	1839	839	54·4
Mackerel	6064	1789	2568	758	70·5
Whiting.	4520	904	1914	383	80·0
White of Egg	4896	671	2074	284	86·3
Hard-boiled Egg	6321	2383	2677	1009	62·3
Yolk of Egg	6460	3423	2737	1449	47·0

Actual Energy developed by One Gram of various Articles of Food—continued.

NAME OF FOOD.	Heat Units.		Metrekilograms of Force.		Per cent. of Water.
	Dry.	Natural Condition.	Dry.	Natural Condition.	
Gelatin	4520	...	1914	...	...
Milk	5093	662	2157	280	87·0
Carrots	3767	527	1595	223	86·0
Cabbage	3776	434	1599	184	88·5
Cocoa Nibs	...	6873	...	2911	...
Beef Fat	9069	...	3841	...	...
Butter	...	7264	...	3077	...
Cod-liver Oil	...	9107	...	3857	...
Lump Sugar	...	3348	...	1418	...
Commercial Grape Sugar.	...	3277	...	1388	...
Bass's Ale (alcohol reckoned)	3776	775	1599	328	88·4
Guinness's Stout „	6348	1076	2688	445	88.4

Actual Energy developed by One Gram of various Articles of Food when oxidized in the Body.

NAME OF FOOD.	Metrekilograms of Force.	
	Dry.	Natural Condition.
Cheshire Cheese	2429	1846
Potatoes	1563	422
Apples	1516	273
Oatmeal	...	1665
Flour	...	1627
Pea-meal	...	1598
Ground Rice	...	1591
Arrowroot	...	1657
Bread Crumb	1625	910
Lean of Beef	2047	604
Ditto Veal	1704	496
Ditto Ham, boiled	1559	711
Mackerel	2315	683
Whiting	1675	335
White of Egg	1781	244
Hard-boiled Egg	2562	966
Yolk of Egg	2641	1400
Gelatin	1550	...
Milk	2046	266
Carrots	1574	220
Cabbage	1543	178
Cocoa Nibs	...	2902
Butter	...	3077
Beef Fat	3841	...
Cod-liver Oil	3857	...
Lump Sugar	...	1418
Commercial Grape Sugar . .	...	1388
Bass's Ale, bottled	1559	328
Guinness's Stout	2688	455

Weight and Cost of various articles of Food required to be oxidized in the Body in order to raise 140 *lbs. to the height of* 10,000 *ft.*

External work = ⅛th actual energy.

NAME OF FOOD.	Weight in lbs. required.	Price per lb.		Cost.	
		s.	*d.*	*s.*	*d.*
Cheshire Cheese	1·156	0	10	0	11½
Potatoes	5·068	0	1	0	5¼
Apples	7·815	0	1½	0	11¾
Oatmeal	1·281	0	2¾	0	3½
Flour	1·311	0	2¾	0	3¾
Pea-meal	1·335	0	3¼	0	4½
Ground Rice	1·341	0	4	0	5½
Arrowroot	1·287	1	0	1	3½
Bread	2·345	0	2	0	4¾
Lean Beef	3·532	1	0	3	6½
„ Veal	4·300	1	0	4	3½
„ Ham boiled	3·001	1	6	4	6
Mackerel	3·124	0	8	2	1
Whiting	6·369	1	4	9	4
White of Egg	8·745	0	6	4	4½
Hard-boiled Egg	2·209	0	6½	1	2½
Isinglass	1·377	16	0	22	0½
Milk	8·021	5*d.* per quart.		1	3½
Carrots	9·685	0	1½	1	2½
Cabbage	12·020	0	1	1	0¼
Cocoa-nibs	0·735	1	6	1	1¼
Butter	0·693	1	6	1	0½
Beef Fat	0·555	0	10	0	5½
Cod-liver Oil	0·553	3	6	1	11¼
Lump Sugar	1·505	0	6	1	3
Commercial Grape Sugar	1·537	0	3½	0	5½
Bass's Pale Ale (bottled)	9 bottles.	0	10	7	6
Guinness's Stout	6¾ „	0	10	5	7½

Weight of various articles of Food required to sustain Respiration and Circulation in the Body of an average Man during 24 *hours.*

NAME OF FOOD.	Weight in oz.	NAME OF FOOD.	Weight in oz.
Cheshire Cheese	3·0	Whiting	16·8
Potatoes	13·4	White of Egg	23·1
Apples	20·7	Hard-boiled Egg	5·8
Oatmeal	3 4	Gelatine	3·6
Flour	3·5	Milk	21·2
Peameal	3·5	Carrots	25 6
Ground Rice	3·6	Cabbage	31·8
Arrowroot	3·4	Cocoa-nibs	1·9
Bread	6·4	Butter	1·8
Lean Beef	9·3	Cod-liver Oil	1·5
„ Veal	11·4	Lump Sugar	3·9
„ Ham, boiled	7·9	Commercial Grape Sugar	4·0
Mackerel	8·3		

These results are in many instances fully borne out by experience. The food of the agricultural labourers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients, in fact these dumplings frequently contain no fruit at all. Egg and bacon pies and potato pies are also very common *pièces de résistance* during harvest-time, and whenever very hard work is required from the men. The speaker well remembers being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway: they consisted of thick slices of bread surmounted with massive blocks of bacon, in which mere streaks of lean were visible. Dr. Piccard states that the Chamois hunters of Western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them, as provisions, nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. They doubtless find that in fat and sugar they can most conveniently carry with them a store of force-producing matter. The above tables affirm the same thing. They show that ·55 lb. of fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or peameal or of 3¼ lbs. of lean beef. Donders, in his admirable pamphlet 'On the Constituents of Food and their Relation to Muscular Work and Animal Heat,' mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks, "Many insects use during a period in which very little muscular work is performed food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen.

We thus arrive at the following conclusions:—

1. The muscle is a machine for the conversion of potential energy into mechanical force.

2. The mechanical force of the muscles is derived chiefly, if not entirely, from the oxidation of matters contained in the blood, and not from the oxidation of the muscles themselves.

3. In man the chief materials used for the production of muscular power are non-nitrogenous; but nitrogenous matters can also be employed for the same purpose, and hence the greatly increased evolution of nitrogen under the influence of a flesh diet, even with no greater muscular exertion.

4. Like every other part of the body, the muscles are constantly being renewed; but this renewal is not perceptibly more rapid during great muscular activity than during comparative quiescence.

5. After the supply of sufficient albuminized matters in the food of man to provide for the necessary renewal of the tissues, the best

materials for the production, both of internal and external work, are non-nitrogenous matters, such as oil, fat, sugar, starch, gum, &c.

6. The non-nitrogenous matters of food, which find their way into the blood, yield up all their potential energy as actual energy; the nitrogenous matters, on the other hand, leave the body with a portion (one-seventh) of their potential energy unexpended.

7. The transformation of potential energy into muscular power is necessarily accompanied by the production of heat within the body, even when the muscular power is exerted externally. This is, doubtless, the chief and, probably, the only source of animal heat.

[E. F.]

WEEKLY EVENING MEETING,

Friday, June 15, 1866.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Tyndall, Esq. LL.D. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, R.I.

Experiments on the Vibrations of Strings.

1. I lay hold of one end of this India-rubber rope, the other end of which is fixed to the ceiling, and by a jerk raise a protuberance upon it. The protuberance runs along the rope to its fixed end, is there reflected, and reversing itself, returns to my hand. In this case, where the points of the rope rise in succession to form the protuberance, we have an example of a *progressive* wave or undulation.

2. After the first wave I now send a second, so that it shall meet the reflected wave on its return. The foremost ends of both waves now meet in the centre of the rope; they there neutralize each other, and the two halves continue to swing with an apparently motionless point called a *node* between them.

3. I now stop the rope, send a wave forward, and then another wave so quickly after it that this second wave shall meet the first at one-third of the length of the rope from its fixed end. At that point a node is produced. But I have already sent a third wave after the second. The second wave being reflected at the node, meets this third one and a second node is formed. The whole rope is now divided into three vibrating parts, separated from each other by two nodes.

4. By properly timing the impulses imparted to the rope I can divide it into four, five, six, ten, and even twenty vibrating parts,