MEMOIRS

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NOTE.

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MEMOIRS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF MANCHESTER.

I. *On the Composition of the Atmosphere.* By R. Angus Smith, Ph.D., F.R.S., F.C.S., President of the Society.

Read November 15th, 1864.

In this paper I have given, first, the inquiry into the composition of the atmosphere contained in a report to the Royal Mines Commission; and secondly, a subsequent inquiry, chiefly relating to specimens taken from various parts of Scotland, but including some from other countries.

When it was found necessary to compare the air of mines with the standard atmosphere, I found that, although I had read on the subject, I was unable to tell the composition of the air with great certainty and exactness. After examining many analyses, I came to the conclusion that the mean of Regnault's Paris analyses gave the oxygen very correctly as 20·96; and, after examining many specimens, I still consider that as a fair mean. But, lest this should be disputed, I took 20·9 as the number, which would sufficiently serve the purpose, and prevent any one from asserting that I was endeavouring to make out a case against the air underground. This number had already been taken by high authorities as indicating the amount of oxygen.

For my own satisfaction, however, it was needful to add greatly to the data from which to judge. Finding, too, that the numbers given by chemists varied exceedingly, I felt it necessary to inquire into the cause of the discrepancy.

I believe I have shown that the air of various places gives, from local causes, a difference in the amount of oxygen, and that this difference, although apparently very small, does in reality indicate important changes of quality, or, in other words, that the amount of oxygen is in reality an important guide in considering the purity of an atmosphere, although we must deal, not with percentages, but with parts in ten thousand.

Amount of Oxygen in Pure Air.

When Priestley discovered oxygen, he examined the air of various places, and found the amounts of this gas to differ to the extent of 6 per cent. He used as a test nitric oxide, which combines with the oxygen, after which both gases, viz. the combined oxygen and nitric oxide, are absorbed by water. At one time he obtained one-fifth of oxygen, which is nearly the exact amount; but he did not seize the idea forcibly.

Scheele found from 20 to 30 per cent., and others found still greater variations, until Cavendish showed, by 500 examinations of atmospheric air, that it had a nearly constant composition, and arrived at a mean of oxygen equal to 20·833 per cent.*

* See Dr. George Wilson's calculations in his Life of Cavendish.

Gay-Lussac and Humboldt, after many experiments, which gave from 20·9 to 21·2, settled on a mean of		21·0 p.c.
Gay-Lussac himself gave as a mean of the air from mountains and from Paris		21·49
De Saussure examined the air at Chambeisy and found		21·08 20·98 21·03 21·03 21·13 21·15 21·08 21·09 20·98 21·086 21·006 21·1 21·0 21·04
Mean		21·05
Berthollet found		21·05 p.c.
Thom. Thomson		21·0
Davy		21·0
Vogel found on the Baltic		21·59
Hermbstädt do.		21·5
Dalton, at Manchester		20·7
" "		20·8
" " (in a N.E. wind)		21·15
" "		20·9
" "		20·73
" "		20·85
" "		20·95
Doyère found	20·5	to 21·5
Regnault gave as the result of 100 specimens in Paris	20·913	20·999
9 from Lyons and around	20·918	20·966
30 " Berlin	20·908	20·998
10 " Madrid	20·916	20·982
23 " Geneva and Switzerland	20·909	20·993
15 " Toulon and Mediterranean	20·912	20·982
5 " Atlantic Orean	20·918	20·965
1 " Ecuador		20·960
2 " Pichincha, higher than Mont Blanc	20·949	20·981
Mean of all foregoing	20·949	20·988

Mean of the Paris specimens, 20·96.

Bunsen's analyses of air at Heidelberg are as follows:—

Oxygen.	Oxygen.
20·970	20·927 p. c.
20·963	20·919
20·927	20·880
20·914	20·921
20·950	20·892
20·906	20·840
20·943	20·859
20·927	20·925
20·934	20·940
20·928	20·937
20·911	20·952
20·889	20·953
20·928	20·964
20·927	20·960

Average ...... 20·924 ...... Lowest ...... 20·840

R. F. Marchand finds	20·9 to 21·03
Mean..................................	20·97
Graham gives	20·9
Liebig gives	20·9

Regnault has made the greatest number of analyses; no man can doubt his power of analyzing well, and he is famous, above all things, for his laborious accuracy. Probably his analyses represent most nearly the true composition of the atmosphere. The analyses made by Bunsen deserve equal respect: he is also a man famous for the minute accuracy of his details, and I would not for a moment put him second to Regnault or to any man; both stand before us as the best specimens of chemical investigators—both living, and in their prime. This is a strong argument for preferring their work to the work of chemists of a past generation. These two have greatly improved the methods of analysis. Still I prefer to take Regnault's results, because they are obtained by a more extensive inquiry. Perhaps the court of the laboratory where Bunsen obtained his specimens may have contained less oxygen than the purest air; at any rate, we cannot doubt Bunsen's accuracy, especially when he gives us these as model analyses.

Judging from all the analyses, I am more inclined to look on a very favourable specimen of air as proved to contain—

Oxygen	20·96
Nitrogen	79·00
Carbonic acid	·04
	100·00

But this is evidently subject to numberless changes. To be moderate, I have assumed 20·9 of oxygen; and Graham and Liebig have done so before me. This is done simply to be within the mark when speaking of the mines.

Analyses made by various Methods.

The earliest analyses were made with gases soluble in liquids. Nitric oxide united with the oxygen of the air; and the resulting compound was absorbed. But, taking all together, they agree with the latest results as well as can be expected. If we examine the results obtained by weighing, we find remarkable differences. It would be difficult to illustrate this difference more clearly than by the analyses of Lewy. When he used the balance he found—

Air of the German Ocean.

		Oxygen.
2nd August 1841	Covered	20·459
3rd do.	Fine	20·423
3rd do.	do.	20·450
4th do.	do.	20·432
22nd May 1842	Covered	20·884
22nd do.	Fine	20·911
23rd do.	Covered	20·893
24th do.	do.	21·010
24th do.	do.	20·839

Air of Guadaloupe.

			Oxygen in 100 vol.
Petit Canal	20 Nov. 1842...	Calm ...	20·839
do.	20 do. ...	Calm ...	20·821
do.	21 do. ...	Fine ...	20·848
do.	23 do. ...	Covered	20·929
do.	23 do. ...	Calm ...	20·667
Mangrove on the River Salée	27 do. ...	Calm ...	20·839
do. do.	28 do. ...	Fine ...	20·504
do. do.	28 do. ...	....	20·522
Petit Bourg	29 do. ...	Fine ...	20·802

When he used explosion by hydrogen, his results were as follows:—

Lewy's Analyses of the Air of the Atlantic Ocean. Carbonic acid in 1000 parts. (Annales de Chimie et de Phys., vol. xxxiv., 1852.)

1847.		Each a mean of 3 Analyses. Carbonic Acid per 1000.	Each a mean of 3 Analyses. Oxygen per cent.	
1 Dec.	Cloudy	0·4881	21·05170	We left Havre on the 25th November. The weather was wet, and remained so all the time we were in the Channel.
4 Dec.	Clear	0·3338	20·96321	
8 Dec.	A little cloudy	0·5497	21·05945	54 leagues from Madeira.
17 Dec.	Clear	0·5771	21·06030	30 leagues south of tropics.
18 Dec.	Do.	0·3346	20·96139	Middle between Africa and America.
18 Dec.	A little cloudy	0·5420	21·06099	
19 Dec.	Clear	0·3388	20·96074	Sea phosphorescent.
26 Dec.	Do.	0·5288	21·05889	
28 Dec.	Do.	0·5093	21·05686	
30 Dec.	Do.	0·5143	21·05789	
31 Dec.	Do.	0·3767	21·01114	Entering port of Santa Marta.

Air of New Granada.

Locality.	1848.	Weather.	Mean of three Analyses. Carbonic acid per 1000.	Mean of three Analyses. Oxygen per cent.	
Santa Marta ...	25 Jan.	Clear	0·4616	21·02379	
Mompox	7 Feb.	Do.	0·3147	21·04936	
Rio Magdalena	18 Feb.	Cloudy......	0·3259	21·03222	
Rio Magdalena	3 Mar.	Covered ...	0·4554	20·99826	
Honda	29 Mar.	Very cloudy	0·3226	20·99237	
Ambalema......	5 Apr.	Clear	1·1203	20·54833	Suffocating hot; wood burning near.
Esperanza	2 Aug.	Do.	2·4475	20·33075	Do.
Guaduas	2 Aug.	Cloudy......	0·3068	20·99691	
Santa Ana......	2 Aug.	Clear	1·2333	20·54479	Do.
Bogota	5 July	Do.	0·4994	21·03196	
Montserrat ...	8 July	Cloudy......	0·5215	20·98995	Rain beginning.

Lewy's Analyses of the Air of Bogota, 2645 metres above the Sea.

		Carbonic acid in 1000 parts.	Oxygen in 100 parts.	
1850.				
7 Mar.	Clear . .	0·3864	21·02099	Fine.
12 April	do. . .	0·3664	21·00382	After rain.
8 May	Covered	0·3609	20·99032	Wet.
9 May	do. . .	0·3824	20·99250	Wet.
15 June	do. . .	0·4192	20·99506	Less wet.
24 July	Clear . .	0·4249	21·01765	
19 Aug.	Cloudy .	0·5043	21·01411	
23 Aug.	Clear . .	0·4812	21·01826	Very fine.
1 Sept.	Cloudy .	0·6178	21·02434	
2 Sept.	Clear . .	0·7649	21·01700	
2 Sept.	Cloudy .	1·6291	20·96629	The upper part of Montserrat was covered with clouds, and a white veil descended from the mountain. Many persons became ill. Thinks the carbonic acid due to a wind called Las Quemas. After rain it disappeared.
2 Sept.	do. . .	1·7040	21·03011	
3 Sept.	Clear . .	1·5853	21·01976	
3 Sept.	Cloudy .	4·8963	21·03176	
3 Sept.	do. . .	4·9043	21·03197	
4 Sept.	do. . .	1·3261	21·02927	
4 Sept.	Covered	0·8648	21·00355	
8 Sept.	Cloudy .	1·2829	21·02097	
9 Sept.	do. . .	0·7512	21·03082	
10 Sept.	Clear . .	0·4583	21·03199	
12 Sept.	Cloudy .	0·4709	21·02689	
3 Oct.	do. . .	0·4751	21·02377	

It would appear as if he went in both cases slightly to excess; but it is not well to attempt to judge on this point.

Dumas and Boussingault together and Brunner obtain also less oxygen; they used weights; and they also are men standing, like Regnault and Bunsen, in the foremost rank.

	Oxygen per cent.	
Paris	20·810	Dumas & Boussingault.
Brussels	20·856	M. Stas.
Genève	20·784	M. Marignac.
Bern	20·757	M. Brunner.
Faulhorn	20·773	
Gröningen	20·793	Verver.
Copenhagen	20·811	

In looking over the analyses already presented, some of them means of hundreds, and the whole representing many years of labour, we see at once how many give the amount of oxygen to be above 20·9. As a rule those numbers which fall below 20·9 represent air from cities and less pure places or from high mountains, or they have been obtained by weighing the oxygen, a method which seems always to give lower results. Take the conclusion of Cavendish, wonderful at the time, that the average was 20·833, we are surprised at the accuracy of the man who used a method by which no one now seems able to obtain any reliable results. It cannot be supposed to take from the honour of Cavendish, if we add one-tenth of a per cent. to his figures after sixty years of scientific activity has made that apparatus the plaything of boys which in his time it required a philosopher to handle. He used acid liquids; and how easy it is to lose a fraction of a per cent. every man who has worked with gas must know and feel most keenly. If, however, any one shall object to this reasoning, we must put up against him Saussure, a man using the same method; and we find his average much higher, viz.

21·05. This also is strengthened by the labours of Gay-Lussac, Berthollet, Thomson, Davy, and Humboldt, all men to whom we must attend. Cavendish could not decide that the London air differed from that of the country. (See Dr. Wilson's Life of Cavendish.)

Dalton laboured long on gases; and although his thoughts were generally greater than his experiments when a theory could be found, few men could so persistently labour out the facts where no theory existed. On the composition of the air of Manchester he is moderate, and, it seems to me, just.

Dumas and Boussingault in conjunction, and also Brunner, gave results by weighing the oxygen. The air was conveyed in bottles, and the bottles themselves were filled by sending them free of air, and allowing the air for analysis to flow in. There are objections to this method. It is extremely dependent on the accuracy of the apparatus used, and, if I am at all right in my judgment, the tendency is to diminish the amount of oxygen; for although the specific gravity of nitrogen does not differ greatly from that of oxygen, it does differ, and is lighter. At any rate I believe the list of analyses given to be a fair representation of the work done. They may be taken as the evidence of the scientific world on the subject.

Regnault's analyses are very beautiful, and the uniformity of his results predisposes us strongly in their favour. Their number comes in to increase their importance.

Another reason why I admire the results of Regnault is one of a kind which affects all men, and which may be excused. They agree with my own, made in an open part of Manchester. His results are 20·913–20·99 for Paris; and all the numbers he made are above 20·9 until he comes to unwholesome places with putrid waters. A similar reason leads me to believe in the analyses by Bunsen, and those of Dalton made in Manchester, as I found like results when air from close places was examined. Those of Dr. Frankland are also corroborative.

It will be seen that I now consider 20·9 as representing air very inferior only, leaving out any reference to that from great heights. Even with this liberal allowance I found only 10·67 per cent. of the specimens of the air of mines capable of comparing with normal air.

Air deviating from the adopted Standard.

Air from Heights.

		Oxygen.
Dalton	Helvellyn	20·64
"	"	20·63
From a height of 9600 feet		20·7
" " 15,000 "		20·62
Brunner	Faulhorn	20·91
Boussingault	548 metres high	20·7
"	Santa Fé, 2643	20·65
Air brought by Green from a height of 11,300 feet		21·
Frankland	Chamounix	20·894
"	Top of Mont Blanc	20·963
"	Grand Mulêts	20·802
Berger	Jura and other mountains	20·3
	to	21·63
Configliachi	Simplon	21·
Mean		20·818

I leave out some under 20·2, and still we have a rather curious result—a lower number on the hills than on the plains. I do not profess to be fully satisfied with these results; we require a few hundred, or at least a few dozen analyses; still these statements are before us, and cannot be removed without much labour. The results of Frankland and Boussingault especially are striking, and require an explanation. Let us again compare results obtained by Dumas and Boussingault (Annales de Chimie, 1841).

Air of Paris.

Oxygen.		Oxygen.
By weight, 22·93	Calculated to volume*	... 20·729
" 23·06	"	... 20·856
" 23·03	"	... 20·828
" 23·01	"	... 20·810
" 23·00	"	... 20·802
" 23·00	"	... 20·802
" 23·08	"	... 20·826
" 22·07	"	... 20·864
" 22·89	"	... 20·701
Mean ...... 23·07		20·864

From the Faulhorn.

Oxygen.		Oxygen.
By weight, 22·96	Calculated to volume*	... 20·766
" 23·09	"	... 20·882
" 22·97	"	... 20·774
" 22·86	"	... 20·674
" 22·97	"	... 20·774
Mean ...... 22·97		20·774

Again we find a smaller amount on elevated places.

	Oxygen.
Brunner found	20·867
	20·750
	20·790
	20·842
	20·812
	20·837
	20·818

Although, for reasons given, I prefer the actual numbers obtained by Regnault, the comparative numbers in Paris and in the Faulhorn are equally valuable for the purpose of showing differences. These analyses were made by weighing.

Dr. W. A. Miller examined air collected during a balloon ascent, in August 1852, at a height of 18,000 feet, and also

* The results are calculated into volumes, taking 1·1057, the number taken by the analysts as the sp. gr. of oxygen.

a sample collected near the surface at the same time, with the following results :—

	Air 18,000 feet high.	Air near the earth.
Percentage of oxygen	20·88	20·92

Dr. Frankland* found at—

	Oxygen.	Carbonic acid.
Grands Mulêts	20·802	0·111
Summit of Mont Blanc.........	20·963	0·061
Chamounix.........................	20·894	0·063

He thinks it probable that the carbonic acid is generally, but not invariably, greater in the higher regions of the atmosphere. Messrs. H. and A. Schlagintweit found the carbonic acid to increase up to the height of 11,000 feet.

If the carbonic acid of the higher regions be really greater than in the best air below, and the oxygen less, it will probably be in part owing to the oxidation having been completed more fully. De Saussure considered it to be owing to the action of vegetation decomposing the acid and giving out oxygen at the surface. The organic matter will probably be entirely removed by thorough oxidation in great oceans of air. The process which converts oxygen into ozone would seem very well fitted for removing all organic matter. This, then, might lead to the existence of a smaller amount of oxygen in the air above, and the riddle would be solved. The diminution of the oxygen is probably a disadvantage—although to such a small extent is it so, that there is abundant compensation in the purification consequent on the removal of organic substances. We shall have, then, a distinct variety of air on mountains differing from that of the plains. In the one there would be more carbonic acid and less oxygen, with no organic matter—constituting mountain air; whilst the air of the plains would have more oxygen, less carbonic acid, and more or less organic matter.

* "On the air of Mont Blanc," Journal of the Chemical Society, for 1861.

It is exceedingly probable that the difference of composition is somewhat connected with the existence of water in the atmosphere; or it may be simply the result of separation caused by the weight of the gases—a result, certainly, which has been much discussed and is well known not to exist in proportion to the weight, and one by no means probable, considering the investigations of Graham: but that the physical cause may after all be struggling so as to make itself in some way felt, it is perhaps rather daring for us to deny absolutely. If the oxygen were diminished without the increase of the carbonic acid, it would be safest to advocate the latter reason. The reasons given in the previous paragraph agree best with the state of our knowledge. However, the discussion on this subject would be better postponed until we become absolutely sure of more facts.

Air of Impure Places.

Reliable analyses of the air of impure places are less numerous than those given; but decided results have been obtained, showing at least a deviation from the numbers found on analyzing fresh air.

Observer.		Oxygen.
Configliachi	Air of rice-fields	20·8
"	Crowded places	20·3
Regnault	Toulon harbour	20·85
"	Algiers	20·42
"	"	20·395
"	Bengal Bay	20·46
"	Do. over bad water	20·387
I found in the middle of Manchester, in a place closely surrounded and exposed to smoke		20·868
		20·808
		20·807
		20·613
		20·793
		20·179
	Average	20·652

The following results have been sent me by Dr. Frankland :—

Air from laboratory of Owens College, 1852.

	Oxygen.
November	20·883
December	20·898
Ditto	20·868
Ditto	20·876

M. Leblanc, in his researches into the composition of the air (Ann. de Chimie et de Ph. t. v., 3rd series, 1842, p. 248), gives the following analyses of inferior and impure air :—

(The amounts are given in weights, but have been calculated into volumes.)

	Oxygen by weight, per 1000.	Oxygen by volume, per cent.	Carbonic acid by weight, per 1000.
1. Conservatory of Equatorial Plants, Terre de Buffon, 6 o'clock in the evening	230·1	20·81	?
2. Seven o'clock next morning..	229·6	20·76	0·1
3. Chemical Theatre, Sorbonne, before lecture	224·3	20·28	6·5
4. Ditto after	219·6	19·86	10·3
5. Bedroom in morning	229·4	20·74	0·4
6. Hall of hospital three hours after shutting the windows	229·1	20·72	0·8
7. Same at 8 o'clock morning ..	227·2	20·54	2·8
8. Sleeping-room at the Salpêtrière, atmosphere sensibly bad	225·2	20·36	8·0
9. Another in a similar state ...	226·0	20·44	5·8
10. Salle d'Asyle, 116 children; smell bad also	227·1	20·53	2·7
11. Salle d'Ecole Primaire, 2d arrondissement...............	228·4	20·65	...
12. Same, incompletely ventilated; no smell	...	...	4·7
13. Same, quite closed; a sensation of heat, quickened respiration	...	...	8·7
14. Chamber of Deputies, in the ventilating-shaft; no smell.	...	...	2·5
15. Opera Comique, pit before end of play	...	...	2·3
16. Same theatre, centre boxes ...	...	...	4·3
17. Close stable, Ecole Militaire	225·5	20·39	1·05
18. Same stable, ventilated by vasistas	229·0	20·71	2·2

It was with these results before me that I began the inquiry; and although not expected to speak of the composition of the air generally, it seemed quite necessary to make some experiments, that it might be compared with the air of mines.

Having this great array of analyses, an abstract of the life-labours of many men, we seem to have before us proof sufficient of a decided variation in the composition of the air. The variation is really so great in some places, that we must admit some powerful local cause. In filthy places, and in marshy spots at a high temperature, the cause cannot be doubted; and how can we doubt the same change to occur in places that are badly ventilated? When carbonic acid increases, is it wonderful that oxygen should diminish? Notwithstanding all this, there is no firmly founded faith amongst scientific men regarding the subject. The evidence has not been brought fully before them, and probably some links may be wanting. Seeing the question in this state, I was desirous of throwing some light upon it, and indeed had already come to believe strongly in the variations, because of some which had been observed when making occasional analyses for the trial of new apparatus, or for instruction and trial of skill.

Under this impression, specimens of air were collected from the front of the laboratory and from behind, near an ash-pit, each at the same time; and the analyses, along with some others, are given in the following Table. Afterwards it will be seen that the carbonic acid of the same spot was also estimated; and the result is that not only is there a diminution of oxygen in the less pure spot, but the carbonic acid, although greater than in the pure air, is not sufficient to make up the vacancy left by the deficiency of oxygen, leading us to look for other gases also that tend to increase the impurity. This is an unexpected result, and, to my mind, one which has in it much value. But, like all other investigations, after leading us onward one step, it shows us that there is another to take.

If emanations arise from foul places, they must occupy space. They are mixed gases and vapours. When we use caustic soda to absorb the carbonic acid, perhaps we absorb some vapours also, and indeed I always find that in a eudiometer we are apt to obtain too much carbonic acid in cases where the quantity is small, unless extreme care is used.

Street and Suburb Air, Manchester, compared with Closet or Midden Air.

Time.	Air from closet or midden behind laboratory.	Air from front door of Laboratory.
1863.	Oxygen, vols. per cent.	Oxygen, vols. per cent.
Dec. 1.	20·80	20·90
,, 10.	20·85	20·96
,, 11.	20·79	20·98
,, ,,	20·72	20·90
,, 15.	20·87	20·90
,, ,.	20·76	20·02
,, 17.	20·59	20·96
,, ,,	20·85	20·78
,, ,,	20·90	20·83
,, 18.	20·21	20·91
,, ,,	20·58	20·92
,, ,,	20·74	20·87
,, ,,	20·40	21·02
,, ,,	20·77	21·00
,, 19.	20·99	20·83
,, ,,	20·70	20·98
,, ,,	20·82	20·88
,, ,,	20·46	21·01
,, ,,	...	20·87
,, 21.	20·56	20·92
,, ,,	20·79	21·02
,, ,,	20·64	20·88
,, ,,	20·94	20·91
,, ,,	20·67	21·01
,, 22.	20·53	20·96
,, ,,	20·71	20·92
1864.		
Feb. 26.	20·66	21·01
,, 24.	...	21·05
,, 20.	...	20·98
,, ,,	...	20·99
,, ,,	...	21·01
,, ,,	...	20·94
	Average 20·70	20·943

Compare also the specimens from the city of Perth, p. 24.

The meaning of these numbers may be further illustrated. Let us put together all the deviations from 21 per cent. in air from the front of the laboratory and from less pure places (the front may not, after all, give the best air), we have—

Good air.	From backs of houses and impure places.
·10	·20
·04	·15
·02	·21
·10	·28
·10	·13
·02+	·24
·04	·41
·22	·15
·17	·10
·09	·79
·08	·42
·13	·26
·02+	·60
·00	·27
·17	·01
·02	·30
·12	·12
·01+	·54
·13	·44
·08	·21
·02+	·36
·12	·06
·09	·33
·08+	·47
·04	·29
·08	
·01	
·05	
·02	
·01	
·01	
·06	
Average......... 0·065 (subtracting those with +).	Average......... ·293

This is a remarkable illustration, to my eye, and a conclusive proof.

The results are very distinct; there is nothing exaggerated about them. There are no great deviations to astonish us; and there are so many irregularities, that if two or three analyses only were made the effect would be bewildering or uncertain: by making numerous analyses we are able to show a steady diminution of oxygen, on one side, with occasional risings as the wind may blow here or there, and a steady rise of oxygen, on the other, with occasional fallings also, as the wind may chance to carry smoke or other gases.

The laboratory stands in an open space, which contains certainly a burial-ground in the centre, but is very much freer from the smoke of the town than the streets are; no manufactories exist beyond or between it and the country. The wind from west and south blows over many houses, but over no large chimneys.

The results clearly show a difference between the air of more and less pure places, and render the oxygen test more valuable than it hitherto has been supposed to be. Unfortunately so many analyses are required, that the test cannot be popular; but as one to be resorted to when the occasion warrants the labour it stands very clear. And indeed how can it be otherwise? We see putrid matter laid on the ground, and find it disappearing rapidly, and yet we are told that it is not accompanied by loss of oxygen; it is not credible, and the results given show it to be incorrect.

It may perhaps be said that, although some of the specimens contain less than 20·9 oxygen, if they came from a clear atmosphere, such air could not be considered very bad. This reasoning cannot hold. Analyses are after all subject to error, and the average is the only number on which we can rely. It may even happen that the small changes are caused by accidents which may give impurity. For example, take gusts of impure air even in the air of a street generally pure.

It is abundantly clear that whenever we leave the region of the uncontaminated or very little contaminated open air we obtain a diminution of oxygen, although that diminution is very small; this small loss is therefore a proof of impurity. In crowded rooms, theatres, cow-houses, stables, and laboratories it is easily proved, and that diminution is enough in decided cases to bring the figures various stages below 20·9.

Oxygen of the Air in wet and in dry foggy weather.

Continuing the subject and going further into detail:

In very wet weather in Manchester, and still before the laboratory, the following results were obtained:—

	Oxygen.
	20·90
	21·01
	21·01
	21·05
	20·96
	104·93
Average............	20·98

In dry foggy and frosty weather, when the smoke of Manchester had no exit from the town, the results were—

Near centre of town	20·90
	20·88
At laboratory	20·90
	20·96
At laboratory, afternoon	20·91
„ forenoon	21·01
„ afternoon	20·82
	146·38
Average	20·91

20·82 and 20·89 were found in a dense fog, such as has rarely visited Manchester. The eyes began to smart, and in walking on the pavement carters were met leading their horses into shops in the day-time—we can scarcely say in the daylight.

Thus we have certified, by experiment as well as the testimony of the senses, the inferiority of the air at certain times, and these senses seem to estimate on certain occasions an amount as small as 0·07; but, so far as we know, they do not estimate the loss of oxygen, but the corresponding increase of impurities. In the yard at the back of the laboratory the amount is less than before the laboratory, and is as follows:—

	Oxygen.
	20·80
	21·01
	20·94
	20·84
	21·09
Average	20·936

Thus we have the series—

In very wet weather, in front	20·98
At all times, an average of 32 experiments	20·947
Behind, in medium weather	20·936
In foggy frost	20·91
Over ash-pits	20·706

These results surprise me as I write. They come from analyses made some months ago, and without the hope of such a fine gradation of qualities. They seem also to show that we are exposed to currents of good air in the worst, and of bad air in the best atmospheres, in towns like Manchester. This is suggested also by that number, 21·01 or more, so curiously turning up in the analyses of most persons.

In Dwelling-rooms, &c.

If we go to dwelling-rooms, &c., we find the same diminution of oxygen where there is insufficient ventilation:—

Before the door of a house in a suburb of Manchester, the air gave of oxygen	20·96
In the sitting-room, not very close	20·89
In a very small room, with a petroleum lamp burning, a good deal of draught	20·84
After 6 hours	20·83
Pit of theatre, February 13th, 1864, 11.30 P.M.	20·74
Gallery, February 15th, 10.30 P.M.	20·63

In Cow-houses and Stables.

If, again, we enter cow-houses and stables, the same results are obtained. The following six are so uniform, they seem to be as good as thousands, and are obtained from the only specimens collected. I went in the morning after the cows had been milked and fed, and therefore after the air had been allowed to enter. The houses still had a close smell. The stables were badly closed, and one was open; the specimens were taken near to the horses, as far from the doors as possible, avoiding the direct breath of the animals. I cannot say they are fair examples of the air breathed by the horses or cattle alone; but they are very fair specimens of the kind of air breathed by those who work in or visit stables. Two of the stables were for cab-horses, the doors half open; a third was a gentleman's stable of four stalls, but there was only one horse; the door was shut. The air seemed good for a stable, and still the loss of oxygen is visible in the analysis :—

	Oxygen.
Cow-houses	20·70
,,	20·78
,,	20·75
Stables	20·82
,,	20·74
,,	20·74

In such places as have been last described it would not be pleasant to live, and in the atmosphere of the theatre we know how much desire of fresh air is produced. Yet none of these numbers are so low as 20·6, the number assumed as marking the beginning of very bad air. The temperature of the theatre in the pit was 78° F., and this is a common temperature in the mines. By taking Leblanc's analyses a somewhat different number might be arrived at; but even he finds 20·54 only after a hospital window had been shut all night, and 20·53 in a room with

116 children. Five of his analyses give numbers below 20·6. These are bad cases.

So far I had written on oxygen for the Mines Commission. Since that time many other specimens have been collected by myself and assistants.

Those from Scotland are sufficiently numerous to seek a place for themselves, and, at the risk of diminishing the clearness of the arrangement, they may be here introduced. I must also confess that I am unable to persuade myself to write a new and independent paper on the subject, having so lately completed the report; the method of merely inserting the latest matter must therefore be adopted.

Specimens from Scotland.

Mountainous Districts.

Top.	Oxygen.	Bottom.	Oxygen.
Ben Nevis	20·91	Ben Nevis	20·93
,,	20·96	,,	20·91
,,	20·94	,,	20·89
,,	20·88		
,,	21·01		
Lochin-y-gair (Balmoral)	20·94	Lochin-y-gair (Balmoral)	20·80
,,	20·95	,,	21·00
Ben Ledi	20·98	Ben Ledi	21·02
,,	20·97		
,,	20·97		
Ben Voirlich	21·01	Ben Voirlich	20·87
		,,	20·88
Ben-na-bourd	21·03	Ben-na-bourd	21·18
Ben Lomond	20·94	Ben Lomond	20·95
,,	21·08		
,,	20·91		
Ben Muich Dhu	21·00		
,,	21·07		
,,	21·02		
,,	20·99		
,,	20·93		
,,	21·01		
Ochill Hill	21·05		
,,	21·07		
Moncrieffe Hill	20·93		
Mean	20·98	Mean	20·94

Districts not Mountainous, or only partly so.

	Oxygen.	Means.	
Shore at Lossiemouth	21·05		
" "	20·95		
Mean	....	21·00	
Inverness, at Moray Frith	20·89		
" "	20·89		
" "	20·86		
Mean	....	20·88	
Inverness, behind the Town ...... (Inverness specimens taken in very clear weather.)	20·88		
Sea-shore, Oban	20·98		
Edinburgh, Prince's Street	20·99		
" "	20·92		
" Calton Hill	20·94		
Mean	....	20·95	
Aboyne	20·94		
"	20·95		
"	21·02		
Mean	....	20·96	
Aberdeen, sea-shore	21·05	....	Wind from sea, N.; evening.
" "	21·01		
" "	21·07		
Mean	....	21·04	
Errol, marshy ground	20·91	....	Windy and cloudy.
" "	20·96		
Mean	....	20·94	
Caledonian Canal (near Inverness)	20·88	....	Cloudy and windy, S.W.
Balmoral	20·88		
"	21·00		
Mean	....	20·90	
Tayn Inn (near Oban)	20·92		
" "	20·86		
Mean	....	20·89	
Braemar-on-the-Dee...........	21·18	....	Cloudy.
Huntley...........	21·03		
Mar Forest	21·04	....	Rain and sunshine.
"	21·02		
"	21·08		
"	20·88		
Mean	....	21·00	
Forest near Braemar	20·87		

Mean of the above, 20·959 or 20·96.

Some impurity rising from the water near Inverness has lowered the otherwise very high average of these analyses, 20·98.

Air from worst places in the City of Perth.

	Oxygen.	Mean.
Close, 70 South Street	20·87	
„ 44 Pomarium	20·92	
„ „	20·94	
„ „	20·93	
Weaver's Close, Pomarium	20·96	
„ „	20·94	
St. Paul's Close	20·96	
„ „ „	20·99	
Long Close, off George Street	20·94	
„ „	20·90	
Weaver's shop, 44 Pomarium	20·88	
„ „	20·93	
Close, 28 Watergate	21·02	
From a conduit, Athole Crescent	20·93	
„ „ „	20·95	
Close, 82 South Street	21·01	
From a conduit, Athole Crescent	20·84	
„ „ „	20·89	
Hewit's Close, 148 South Street	20·97	
„ „	21·00	
From conduit, Stormont Street	20·90	
Close, 44 Meal Vennel	20·90	
Mean		20·935

Scotch Analyses classified.

	Oxygen.
Mean of the sea-shore and the heath	20·999
Mean of the tops of hills	20·98
Mean of the bottoms of hills	20·94
Mean of all places not mountainous	20·978
Mean of inferior parts of a town (favourable, *i. e.* windy weather)	20·935
Mean of lower marshy, &c., places	20·922
Mean of the forests	20·97
Mean of all	20·959
	or 20·96

I conclude, therefore, that in order to obtain the mean, 20·96, it is needful to include very inferior air. It is, therefore, the mean composition of air as it is found in wholesome and less wholesome places, not the mean of the finest atmospheres.

It will be seen that here the sea-shore and open places still command the highest amount of oxygen, although the higher hills are not the most deficient—it may be, because in Scotland really high mountains do not exist, and also because, unlike the great ranges of the Alps and the Himalayas, the Scotch hills have much sea and little land from which to draw their supplies.

It may be remarked that the averages of the hills above and below, viz. 20·98 and 20·94, give exactly the number, 20·96, which was taken as a fair sample of air.

Marshy or confined Places, Switzerland, &c.

		Oxygen.	Means.
Aug. 1864.	Sion, Upper Valley of the Rhone, Switzerland (morning), over water, marshy grass	20·86	
"	Sion (morning) over water and brushwood	21·01 20·94 21·05 21·02 20·96 20·94 20·95 20·83 21·00 20·90	
	Mean	....	20·95
Sept. 1864.	Reddish, near Manchester, among brushwood	20·92 20·98 20·95 20·90	
	Mean	....	20·937
Aug. 1864.	Lauterbrunnen	20·94 20·97 20·95	
	Mean	....	20·953
"	Chamounix, Montanvert	21·03 20·99	
	Mean	....	21·01
"	Verdin, in the Sologne	20·97 20·90	
"	Vouzerou	21·01 20·90	
	Mean	....	20·95

As there is so much cretinism at Sion, and as goître is found in the whole valley, I thought it important to obtain some specimens of the atmosphere from the marshes themselves. The air was taken from the surface of the water or from the brushwood. The time was too favourable, as there was a considerable breeze; but I had not patience to wait, and, as it turned out, it would have been necessary to wait a long time. I consider the matter well worth further inquiry, and should be happy to make the analyses, if specimens were sent, as I may not soon be in the same spot again.

The specimens from the Sologne are very few. They were brought without any hope of a result. Had I known as much as I do now, more would have been brought. The book of Dr. Burdel, entitled 'Recherches sur les Fièvres Paludiennes,' excited a curiosity to see the district; but there seemed such a free air and such an open country, with such a dry sandy soil, that I doubted if anything like emanations could be found. I saw only few and small ponds. Seeing how careful the search must be, it is well not to be too confident until numerous analyses are made. Dr. Burdel does not believe in any difference of analysis, and seems to refer the unwholesome state to the action of the electricity of the atmosphere and the heat and cold. It is a courageous thing at the present time to refer any phenomenon to electricity; it has been overdone so much, that people now imagine it must never be done, and are afraid to mention it, thus deciding on the other side. It seemed to me when in Switzerland that the frequency of discharges on the mountains, with their absence in the valleys, was itself a proof of difference of condition in the two places. On plains the heavens and earth seem to equalize their electricity more uniformly; in these hilly regions it is done at certain high points. If the flow of this electricity is valuable to us, its loss is detrimental; and if the discharges are made violently from one point, instead of steadily and slowly from wide surfaces, the condition of these surfaces must be modified. On looking at Mont Blanc for a fortnight, and seeing nightly discharges from its summit, I was naturally led to look on the subject in this way, and to add this to Dr. Burdel's opinion, although I confess I do not understand the part he wishes the electricity to play. All I can see is, that there is a difference in these hilly regions. I do not pretend to see anything affecting animal life that can result from the difference; and in the Sologne there are not the mountainous ranges to account for any such differences.

Experiment has led me to form the following

TABLE of the Composition of the Air of pure and impure places, beginning with that containing most oxygen, although probably not the very richest specimen :—

	Oxygen.
N.E. sea-shore and open heath (Scotland)	20·999
Tops of hills (Scotland)	20·98
In a suburb of Manchester in wet weather	20·98
" " "	20·96
In the outer circle of Manchester, not raining	20·947
Low parts of Perth	20·935
Swampy places, favourable weather	20·922 to 20·95
In fog and frost in Manchester	20·91
In a sitting-room, which felt close, but not excessively so	20·89
In a small room with petroleum-lamp, well ventilated	20·84
Ditto, after six hours	20·83
Pit of theatre, 11.30 P.M.	20·74
About backs of houses and closets	20·70
Gallery, 10.30 P.M.	20·86
In large cavities in mines	20·77
In currents	20·65
Under shafts	20·424
In sumps	20·14
When candles go out	18·5
The worst specimen yet examined in the mine	18·27
Very difficult to remain in for many minutes	17·2

It has been shown that air in places where putrefaction may be supposed to be going on has been found by other observers to contain less oxygen than pure air. This inquiry puts the subject in a somewhat clearer light than hitherto, and shows us that those places containing impurities, and which are in or near all our houses, are also subjected to a diminution of oxygen. The diminution is not entirely made up by carbonic acid, and must be made up by other substances. This diminution is very sensible when it comes to 20·75, or even in some places 20·85, being equal to a removal of 0·2 to 0·1 of oxygen; so that it indicates more clearly in some cases than the carbonic-acid test does. These cases are probably such as allow for the absorption of oxygen into the soil or elsewhere. We do not require to seek deadly places for air with diminished oxygen; the air of every house is subjected to this diminution, which must of necessity be an indication of the amount of impurity existing in the air, although giving no clue to the quality of that impurity, which may be more or less innocent or noxious.

It is well known that oxygen over putrid substances is absorbed, whilst carbonic acid and other gases take its place. This reasoning does not touch the question, What is the effect of a loss of oxygen when no impurities take its place?—a condition little known. I wish particularly to say that it is probable that the objection to the air which has a little less oxygen than the normal amount may not arise from this fact itself. The loss of oxygen may only be an indication of the presence of a pernicious body.

Carbonic Acid of the Atmosphere.

Horace de Saussure first paid minute attention to the carbonic acid of the atmosphere, and showed its presence on the mountains of Switzerland as well as on the plains. He used lime-water. His results were published in his 'Voyages dans les Alpes' in 1796. His son Théodore in 1828 published a résumé of a much fuller inquiry, and in 1830 the complete account. He used a vessel of 34 litres in volume, and washed the air with baryta-water, collecting the carbonate of baryta precipitated. This is a laborious process; but, considering the great accuracy of the operator and the long experience which he gained, we may place the greatest confidence in his results. I am disposed to think that there may be a little excess in his results; but this will not affect the comparative amounts found at different times, and which form the most interesting part of the inquiry. He says*:—

"The quantity of carbonic acid in the open air in the same place is subject to almost continual change, equally with the temperature, the winds, the rain, and the atmospheric pressure. The observations which I have made since 1816 until the month of June of this year, in a meadow at Chambeisy, three quarters of a league from Geneva, indicate that the mean quantity of carbonic acid in volume which 10,000 parts of air contain is equal to 5, or more exactly to 4·9. The maximum of this gas is 6·2; the minimum is 3·7.

"The observations published ('Bibliothèque Univ.' vol. i.) show as maximum in the same place a greater proportion of acid; but it is probable that this excess was the result of the imperfection of the experiment.

"The augmentation of the average quantity of carbonic acid in summer, and its diminution in winter, are manifested at different stations,—in the country as in the city, upon the Lake of Geneva and upon a hill, in calm and disturbed air. According to an average of thirty observations made at Chambeisy, during seven years, with baryta-water, the quantity of carbonic acid in the months of December, January, and February, at midday, is to that of June, July, and August as 77 to 100.

"This ratio is not constant throughout every year. There are times which form exceptions, and in which the quantity of carbonic acid in summer is inferior to that in

* Annales de Ch. et de Ph. vol. xxxviii., 1828.

winter, or *vice versâ.* Thus after many years of observation, the mean quantity of carbonic acid in the month of January in 10,000 of air is 4·23; but the quantity of carbonic acid in the month of January 1828, which was extraordinary for the mildness of its temperature, rises to 5·1. The average quantity of carbonic acid in the month of August, taken in different years, is 5·68; but after an average taken from four observations (the results of which closely approximate) in the month of August 1828, which was singularly cold and wet, the quantity of carbonic acid at noon was only 4·45.

"The difference in the quantities of carbonic acid contained in the atmosphere in calm weather, during day and night, is one of the most remarkable results of my late observations. The following is the table of experiments which I have made, in open country, at noon and at eleven o'clock in the evening of the same day. (The Table in the original gives the quantities of carbonic acid in 10,000 parts, but for uniformity's sake they are here altered into percentages.)

	Noon.	Evening, 11 o'clock.
May 22nd, 1827	·0581	·0623
July 7th, „	.058	·062
Sept. 3rd, „	·0561	·0601
Nov. 6th, „	·043	·0486
May 31st, 1828.........	·0475	·0565
June 13th, „	·0506	·0583
June 26th, „	·0539	·0522
Aug. 1st, „	·0432	·0606
Aug. 12th, „	·0429	·0582

"It results from these observations, that the air contains in calm weather more carbonic acid during the night than during the day. The only exception to this result was on June 26th, 1828, during extremely violent wind, whilst all the other observations were made in calm weather or in slightly disturbed air. I have acquired sufficient experience in this kind of work to affirm that the general difference which is found in this table could not result from errors of observation.

"It remains for me to discover if this difference is maintained in the middle of winter, or when vegetation is inactive.

"The air taken at the middle of Lake Leman, opposite to Chambeisy, contains on an average a little less carbonic acid than the air taken a hundred toises from the bank. After eight observations, made at different periods, on the same days at noon the quantities of carbonic acid at the two stations are as 100 to 98·5; but the air of both places follows the same variations relatively to the seasons.

"The air of Geneva contains more carbonic acid than the air of a meadow at Chambeisy—almost in the ratio of 100 to 92, from six observations made at the same time at both stations. A greater purity in the air of the country could be foreseen. I cite this result only because the other eudiometrical experiments indicate no difference in the air of those two places, and as it shows the utility of the experiment by which this result was obtained."

At first Saussure was led to believe that rain increased the carbonic acid, but changed his mind on finding, on the contrary, that this acid increased in dry weather even with a freezing temperature.

For the Lake of Geneva and the neighbouring Chambeisy, his numbers are*:—

Date.	Time.	Carbonic acid at Chambeisy.	On the Lake of Geneva.
1826.			
Dec. 29	Mid-day	·0421	·0385
1827.			
May 22	ditto	·0540	·0502
July 2	ditto	·0523	·0578
Aug. 9	ditto	·0521	·0542
1828.			
Sept. 28	ditto	·0495	·0474
Jan. 19		·0491	·0446
July 7		·0481	·0441
Aug. 12		·0408	·0392
Aug. 26		·0422	·0410
Sept. 26		·0414	·0320
Sept. 26	Night	·0493	·0430
1829.			
Feb. 5	Mid-day	·0445	·0476
March 7		·0463	·0465
April 18		·0429	·0422
July 7	Night	·0534	·051
July 8	Mid-day	·0435	·0408
Oct. 13		·0354	·0342
Oct. 13	Night	·0416	·0368
	Mean	·0460	·0439

De Saussure found also more carbonic acid in the town of Geneva than outside at Chambeisy. The numbers are—

Date.	Time.	At Chambeisy.	In Geneva.
1827.			
Feb. 12	Mid-day	·0358	·0455
May 22		·0540	·0569
May 26		·0471	·0528
Aug. 9		·0453	·0476
1828.			
Jan. 28		·0426	·0427
Feb. 19		·0462	·0482
April 10		·0465	·05
July 25		·0390	·0445
July 1	Mid-night	·0407	·0385
Sept. 4	11 at night	·0441	·0439
Sept. 5		·0382	·0420
Oct. 1		·0414	·0423
Oct. 2		·0367	·0405
	Mean	·0437	·0468

* Ann. de Ch. et de Ph. vol. xliv., 1830.

In the day the carbonic acid at Chambeisy is 0·0445, and at night 0·0402; in Geneva during day 0·0485, and at night 0·0414. The difference for the country is 0·0043, and for the town 0·0073. The diminution at night is greater in the town, where less fuel is burnt and people are shut up.

One of the most curious results of De Saussure's inquiry is, that the carbonic acid on the mountains is actually greater than on the plains. It will be interesting to give these figures, as they are often referred to, and seldom seen. It may be remembered here that there was some reason to believe in a diminution of oxygen in mountain air to a minute extent. The increase of carbonic acid is a corroboration.

Name of mountain.	Height of mountain in metres.	Carbonic acid in the air of the mountain.	Carbonic acid in the air of the plain.
La Dôle	1267	·0461	·0474
Grand Salève-sur-Crevin	877	·0557	·0482
Hermitage (Petit Salève)	331	·0544	·0482
La Dôle	1267	·0491	·0446
Vasserode-sous-la-Dôle	908	·0481	·0446
Grand Salève-sur-Grange-Tournier	945	·0413	·0367* ·0359†
Col de la Faucille	963	·0443	·0414
ditto	963	·0454	·0415
ditto	....	·0369	·0387
ditto	....	·0360	·0322
ditto	....	·0422	·0355
ditto	....	·0395	·0315

He refers the difference to the rain below, and the moisture of the ground, and to vegetation, which diminishes the carbonic acid and increases the oxygen. He finds also that the mountain air does not change at night as the air below does. He finds a minute increase of carbonic acid arising from violent winds, and thinks this may arise from the upper mixing with the lower strata; his evidence on this point may be explained by the fact that

* At foot. † At Chambeisy.

he found a decrease on June 20th during a violent wind, probably from the same cause, namely mixing.

De Luna has lately examined the air of Madrid, with the following results*:—

Air of Madrid, outside the walls, during month of March. (1st series.)

Place.	Oxygen.	Carbonic acid per cent.
1	20·71	0·05
2	20·79	0·03
3	20·77	0·03
4	20·77	0·05
5	20·73	0·06
6	20·75	0·03
7	20·70	0·06
8	20·74	0·05
9	20·69	0·09
10	20·81	0·02
11	20·79	0·03
12	20·78	0·04

Air of Madrid, within the walls, during month of April. (2nd series.)

Place.	Oxygen.	Carbonic acid.
1	20·70	0·06
2	20·70	0·06
3	20·77	0·03
4	20·75	0·05
5	20·70	0·06
6	20·69	0·06
7	20·78	0·05
8	20·69	0·08
9	20·70	0·06
10	20·78	0·04
11	20·80	0·03
12	20·73	0·04

The amounts of oxygen are very small, and of carbonic acid very high. Solutions were used for the oxygen, and permanganate of potash for the organic matter.

* Estudios Quimicos sobre el aire atmosférico de Madrid, por D. Ramon Torres Mũnoz de Luna. Madrid, 1860.

Hospitals.

Rooms.	Oxygen.	Carbonic acid.
Hospital general	20·58	0·32
	20·50	0·38
	20·49	0·43
Hospital de la Princesa	20·65	0·27
	20·52	0·30
	20·60	0·29

The experiments of Mr. Lewy on the Atlantic Ocean and in America show a great irregularity. They are given on pages 6 and 7. We do not see clearly why there should be a rise in the carbonic acid from ·0333 to ·0577 at sea. The great inequalities on the land are interesting, and especially at Bogota, where meteorological influences interfere to render the amount great and diminish health.

Dalton had made experiments on the atmosphere with lime-water, but by no means such as are satisfactory. Mr. Hadfield, who had learned in his school, improved the process very much. He used a bottle of 471–498 cub. in., fitted with a cap and stop-cock, and filled it by means of a bellows. He tested the lime-water with sulphuric acid, before and after shaking with the air, or after it had stood for some days. He obtained 0·80 vol. per 1000. This is high as a constant result; but as he lived on the borders of a putrid canal at Cornbrook, it may not be too high. His results are found in the 'Memoirs of the Literary and Philosophical Society of Manchester,' 2nd series, vol vi., 1842.

Dr. Boswell Reid* made trials with various amounts of carbonate of lime in water, as indications of the carbonic acid absorbed. After passing the air through a solution, he compared the result with these trials, keeping the precipitates in bottles. This use of lime he called a carbono-

* 'Illustrations of the Theory and Practice of Ventilation,' by David Boswell Reid, M.D., F.R.S.E., &c.: Longman, 1844.

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meter. The results were comparative, but I do not find that they were quantitative. These precipitates change so much that they cannot be used for comparison after a few hours.

Pettenkofer lately took up the subject, using lime (now, I believe, baryta) to remove the carbonic acid from the air, and oxalic acid to test the solution. The bottles in which the experiments were made were dried with great care, and the solution of oxalic acid made very delicate. One cubic centimetre of this solution was made equal to a milligramme of lime, but it may also be made equal to a cubic centimetre of carbonic acid. The strength, however, must vary with the air. If the air is very bad, a stronger solution may be used. Indeed this is rather a difficulty in the process, as you cannot use one solution for all conditions, on account of its extreme delicacy. The carbonic acid saturates part of the lime, and the amount remaining is tested with oxalic acid to see how much is still uncombined. The point of neutralization is found by putting a drop of the liquid on a piece of turmeric paper.

The plan is very beautiful and complete. In principle it is exactly that of Hadfield's; but oxalic acid is used, and the experiment made more delicately.

He gives 0·5 per mille or ·05 per cent., as the amount in the air generally at Munich. This is above the number of Saussure, and both are above the numbers found here. Munich is 1690 feet, Geneva 1154 feet above the sea. In the Handwörterbuch der Chemie, under "Ventilation," Pettenkofer gives a summary of the amount of carbonic acid in dwelling-houses, as follows:—

In a dwelling-house, during the day, 0·054.

After a while it increased to 0·065, 0·061, 0·064, 0·068, 0·074, and 0·087. Mean 0·068.

In a bed-room at night, with closed windows, 0·230.

Partly open, 0·082.

He found the following amounts of carbonic acid per cent., on examining public places, hospitals, prisons, &c. :—

	·232	·143	·223
	·226	·307	·247
	·334	·261	·131
	·186	·278	·495
	·362	·429	·536
	·317		
Schools	·410	·567	·200
	·229	·558.	

Limit of Carbonic Acid in Dwelling-houses.

Dr. Reid's labours are much to be admired; he did much for the subject of ventilation in this and other countries. I am disposed, therefore, to have great respect for his opinion, that air containing between 0·1 and 0·2 per cent. of carbonic acid cannot be proved to be injurious —although certainty has not been arrived at. This, however, we know, that this amount of carbonic acid, at least when given out by human beings and at a temperature not cold, is very offensive, if not on account of the acid, still of its accompaniments.

Pettenkofer, after great attention to the subject, concludes that 1 per 1000 marks the limit of bad and good air, and that those who can plead for more have lost the refined use of their senses. He then inquires into the cause of this feeling, as there is neither a want of oxygen nor a great amount of carbonic acid, attributing the feeling experienced to the prevention of a proper flow of heat, and in part, very ingeniously, to the existence of such bodies as butyric and valerianic acids, which saturate a large volume, and so prevent further evaporation. In other words, he attributes the depressing feeling to organic matter, as I have also done, although lately obliged to give a large share of the blame to carbonic acid.

Pettenkofer finds also that a lowering of temperature ventilates a room more rapidly than opening a window.

In order to find the amount of fresh air which must be supplied to any dwelling, he explains that the carbonic acid of the expired air is about 4 per cent., or 40 per mille. The mean in the air is 0·5, and that of a good air for a room is 0·7. This gives a difference of 0·2. Then $\frac{40}{0\cdot2}=200$. If we must keep up the freshness of the air, we must add 200 times the volume of the air that is expired. If a man breathes out 300 litres in an hour, there must be added 60,000 litres of fresh air=2118·96 cubic feet. At the same time it is shown how much more rapid the change of air is when out of doors, where the air is consequently pleasanter, proving that even the apparently excessive amount obtained indoors was not all that was desired.

Pettenkofer mentions that it has been found necessary in some hospitals in Paris to use exactly this amount of 60,000 litres in order to prevent all smell.

If it were desired to keep the air at 0·6 per mille, the calculation would be $\frac{40}{0\cdot1}=400$ times. We cannot do better than adopt the figures for bad and good air given by Dr. Reid, or rather Professor Pettenkofer, as far as they will suit circumstances. Here we must put the amount of carbonic acid in the atmosphere at 0·4 at the most, instead of 0·5; and it is exceedingly probable that in many parts of England it will be found constantly less, as it certainly is frequently. This would make it possible to have a dwelling-room at 0·6, instead of 0·7. But practically we are not always favoured with such ventilation, even in England; and Germany seems to be much worse, judging from the analyses of Pettenkofer, &c. In some cases we may, with Dr. Reid, allow more carbonic acid. Dr. Reid was not exaggerating, although he was frequently blamed, when he insisted on supplying 600 cubic feet per hour for an individual; and we see in the 'Journal of the

Chemical Society' for 1858, that Dr. Roscoe found 1200 to be nearer the demand in a hospital. Dr. Arnott, a revered authority on ventilation, demands 1200. I shall return to this question.

Carbonic Acid of the Air in England.

It may be allowed first to quote a calculation of mine in the 'Chem. Soc. Journal,' 1859.

Allowing the air to go at the average rate of twelve miles an hour*, it will sweep over the three or four miles of Manchester three times an hour, or thirty-six times in twelve hours.

Taking the height of air affected to be 300 feet, as we must assume something, we have carbonic acid—

From coals	0·0091	per cent.
„ expired air	0·0002	„
Call the usual amount 0·06 ...	0·06	„
	0·0693	„

But call the usual amount 0·03, as it sometimes is, or even below it, we have—

From coals	0·0091
„ breathing	0·0002
„ usual air	0·0300
	0·0393 per cent. or 0·393 per thousand.

We see then that the combustion of coal and the breathing will produce carbonic acid amounting only to a third of that in the purest air generally found, or one-fourth of that in air having 0·04. It is certainly remarkable that the amount which I calculated as that which ought to exist should be found, by experiment, to be correct to the third decimal place, according to Dr. Roscoe. At the same time my own numbers are a little higher, and in extreme cases much higher.

Dr. Roscoe has found the carbonic acid to be as low as 0·027 per cent. outside of Manchester in wet weather, and

* Mr. Hartnup, F.R.A.S., finds it 12·62 at Liverpool.

near the sea 0·043—rather more than in Manchester. This favours the idea of oxygen coming from vegetation, or rather of carbonic acid being absorbed to such extent as to influence the experiments. I do not obtain quite the same.

Dr. Roscoe has put down for	Manchester	0·0392	per cent.
" "	the country............	0·0402	"
" "	Manchester district..	0·0394	"

That the amount should be less or even equal in Manchester is scarcely to be explained, especially as, even in Geneva, Saussure found more than in the country. Had the country places been mountainous, the observations would have been better understood.

At the same time I made some experiments which gave higher numbers, varying from 0·049 per cent. to ·15 per cent. These, with the exclusion of four which are very high, give an average of 0·0544. The four were taken under circumstances which make me believe they are correct; some places are continually exposed to gusts from chimneys, which must contain a high amount of acid.

Lately I undertook a much larger number of experiments, using Pettenkofer's method of analysis. The bottle used is of a different shape from Pettenkofer's, and the bellows are different, but the method is essentially the same. The bottle has a very wide mouth, so as to allow the hand to enter; and drying is performed by means of a pure linen cloth which has been washed in acid and distilled water. This saves a great deal of trouble; and repeated analyses have proved that neither from the hand nor the cloth does any hindrance to accuracy arise. The bellows pump is one that I have used for emptying the bottles of air for the permanganate test. The bottle and pump are figured in the first paper. When the bottle is cleaned and dried, the baryta is added, and the elastic cover is then put on.

The following results were obtained after nearly 200 experiments were made in Manchester and London :—

Carbonic Acid in the Air of the Suburbs of Manchester.

Place.	Per cent.	Wind.	Date.	Time.	Temp. Cent.	Bar.	Remarks.
				h m	°	in.	
Fields in Greenheys	0·0383	...	Feb. 22	...	...	...	
Higher Broughton	0·0350	E.S.E.	„ 24	...	0·6	29·9	
Old Trafford	0·0432	...	„ 25	...	3·0	29·98	
Buxton	0·0431	E.	...	11 0	...	...	Fine day.
„	0·0459	„	Mar. 19	...	...	...	
„	0·0467	„	„ 19	...	...	...	
„	0·0443	„	„ 19	1 0	...	...	
City Road, Hulme	0·0367	„	„ 8	3 0	4	29·2	Strong wind. Little snow.
Rusholme	0·0295	...	„ 11	...	5	29·2	Very windy. Hail storm.
Queen's Park	0·0308	...	„ 14	...	10	...	
Old Trafford	0·0340	W.	April 1	...	...	...	
„ over canal	0·0313	„	„ 1	...	...	...	When the hour is not given, it
„	0·0293	„	„ 2	...	...	...	is about the middle of the day.
„	0·0291	„	„ 11	5 50	...	...	
(14)	0·5172						
Mean	0·0369						

Carbonic Acid in Close Places.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.	Remarks.
				h m	°	in.	
Theatre Royal, pit	0·2734	......	Feb. 13	......	22	29·3	
" gallery	0·1358	......	" 15	10 50	29	29·7	
" "	0·1238	......	" "	"	"	"	
Queen's Theatre, pit	0·1026	......	" 16	......	26	29·7	
"	0·1019	......	" "	......	"	"	
Stables	0·0833	......	......	......	8·2	30·	4 horses had been there during the night, and 3 were still there.
"	0·0875	......	......	......	"	"	
Cellar of laboratory	0·0572	......	" 17	......	6·2	30·1	
Study, at table	0·1177	......	" 25	......	16	29·98	4 persons were in the room, with 3 gas lights and large fire.
" ceiling	0·1561	......	......	......	22	"	
Schoolroom	0·0970	......	Mar. 15	......	12	...	
"	0·0886	......	" "	......	...	...	
Brewery	0·1214	......	" 16	......	...	...	
"	0·180	......	" "	......	...	...	Near fermenting vat.
Mills	0·286	......	" 10	4 0	20	...	Taken in weaving-shed which contained 400 people.
"	0·283	......	" "	5 30	21	...	
"	0·296	......	" "	5 30	21	...	
"	0·300	......	" "	"	"	...	
(18)	2·8883						
Mean	0·1604						

Carbonic Acid in the Air of Manchester.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.	Remarks.
				h m	°	in.	
Churchyard, All Saints	0·0323	N.E.	Feb. 22	...	1	29·8	
Smithfield Market	0·0446	,,	,, 20	11 0	1·3	29·95	
,, ,, ...	0·0464	,,	,, 20	,,	1·3	,,	
Churchyard, All Saints	0·0437	,,	,, 18	...	5·2	30·18	
,, ,, ...	0·0521	,,	,, 18	...	,,	,,	
Back yard of laboratory	0·0539	...	,, 6	...	1·4	30·8	
,, ,, ...	0·0383	...	,, 6	11 15	,,	,,	
,, ,, ...	0·0473	...	,, 6	3 15	,,	,,	
,, ,, ...	0·0515	...	,, 6	3 35	,,	,,	
Philosophical Society	0·0677	...	,, 9	11 20	1	29·55	Fog, but not very dense.
,, ,, ...	0·0683	...	,, 9	5 15	,,	,,	,, ,,
Bishopsgate	0·0734	...	,, 9	4 30	,,	,,	Fog, very dense.
,, ...	0·0727	...	,, 9	,,	,,	,,	,, ,,
Back yard of laboratory	0·0523	...	,, 10	10 30	8	29·4	Little fog.
,, ,, ...	0·0541	...	,, 10	11 30	,,	,,	
Deansgate	0·0578	...	,, 10	2 30	...	...	Fog.
Small court off Berry Street	0·0604	...	,, 11	...	4	29·6	
Midden	0·0724	...	,, 12	5 5	5·4	29·38	
,, ...	0·0732	...	,, 12	,,	,,	,,	
,, ...	0·0805	...	,, 13	11 50	7·2	29·3	
,, ...	0·0837	...	,, 13	12 25	,,	,,	
Churchyard, All Saints	2·067	...	,, 19	...	1	30·18	
,, ,, ...	0·0447	...	,, 19	...	,,	,,	
,, ,, ...	0·0518	...	,, 19	...	,,	,,	
End of Market Street	0·0374	...	,, 23	...	2·8	29·85	

Carbonic Acid in the Air of Manchester (*continued*).

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.	Remarks.
				h m	°	in.	
End of Market Street	0·0321	...	Feb. 23	...	2·8	29·85	
Blackfriars Bridge	0·0495	...	...	...	...	...	Taken immediately over the water. Very bad smell. Fine day.
Near Printing Lane	0·0351	...	...	...	...	...	
Back yard of laboratory	0·0327	...	March 4	4 0	5·6	29·5	Rain.
„ „	0·0319	...	„ 4	5 30	...	„	
„ „	0·0391	S.W.	„ 7	10 15	9·5	29·	Very fine.
„ „	0·0358	„	„ 7	11 15	...	„	These experiments, Mar. 7, after 3 days' rain.
„ „	0·0343	„	„ 7	12 15	10·	„	
„ „	0·0311	„	„ 7	2 15	7·8	„	After more rain.
„ „	0·0311	„	„ 7	2 50	7·4	„	Rain.
„ „	0·0335	„	„ 7	3 15	7·2	„	„
„ „	0·0367	„	„ 7	4 15	6·4	„	Fine.
„ „	0·0333	„	„ 7	5 15	...	...	
All Saints Churchyard	0·0319	E.	„ 8	4 45	4·	...	A little snow; strong wind. Snowing fast.
Stockport, back street off Heaton Street	0·0415	N.E.	„ 9	2 30	4·	...	„ „ „
Stockport, Chester Gate	0·0351	„	„ 9	3 30	4·	...	„ „ „
„ near Grammar School	0·0355	„	„ 9	4 30	„	...	Very stormy.
Back yard of laboratory	0·0311	...	„ 11	4 50	5·	...	
River Medlock, a little above Brook Street	0·0351	...	„ 17	11 30	...	...	
Back street off Gray Street, Oxford Street	0·0383	...	„ 17	2 30	...	...	Rather windy.

River Medlock, where the water runs into a basin	0·0367	...	Mar. 17	4 15	...	...	
Canal bank, Canal Street	0·0375	...	„ 18	11 0	...	...	
Old Garratt, river-bank	0.0608	...	„ 22	11 40	...	...	
Granby Row, Crown Street	0·0520	N.E.	„ 22	4 0	...	...	
„ „	0·0544	„	„ 22	„	...	...	
Scholes Street, London Road	0·0408	„	„ 22	4 50	...	...	
Devonshire Street, All Saints	0·0343	E.	„ 23	...	...	...	
St. Mary's Gate, Exchange	0·044	W.	„ 24	...	...	...	
„ „	0·0375	„	April 1	11 0	...	...	
„ „	0·0344	„	„ 1	5 0	...	...	
„ „	0·0333	„	„ 2	11 0	...	...	
„ „	0·0346	„	„ 2	„	...	...	
„ „	0·0364	S.E.	„ 5	12 10	...	...	
„ „	0·0365	„	„ 5	„	...	...	
„ „	0·0389	„	„ 5	5 0	...	...	
„ „	0·0398	„	„ 5	5 0	...	...	
„ „	0·0317	N.W.	„ 11	...	...	...	
„ „	0·0329	„	„ 11	...	...	...	
Back yard of laboratory	0·0399	S.E.	„ 5	2 30	...	..	
„ „	0·0352	...	...	...	...	...	
„ „	0·0312	...	„ 6	...	...	...	
Ditto, over some mud	0·0358	...	„ 6	3 0	...	...	
„ „	0·0415	...	„ 6	„	...	...	
Ditto, over jar of mud	0·0433	...	„ 7	...	...	...	
„ „	0·0404	...	„ 7	...	...	...	
„ „	0·0501	...	„ 7	3 0	...	...	
„ „	0·0343	...	„ 7	...	...	...	
Yard	0·0353	...	„ 7	5 0	...	...	
„	0·0387	...	„ 7	„	...	...	

The following summary may be made of the Manchester results :—

	Average of Carbonic acid per cent.
In Manchester streets in usual weather	0·0403
During fogs	0·0679
About middens, of which there are thousands	0·0774
Average of all the town specimens	0·0442
Fogs excepted	0·0424
Fogs and middens excepted	0·0403
Where the fields begin	0·0369
In close buildings	0·1604
Minimum of suburbs	0·0291

When approaching the country the amount seems occasionally very low; probably the lower grounds, with much vegetation, are subject to variations below and above the standard, and such as are not found in exposed or bare regions.

There may be noticed in Manchester a tendency towards increase of carbonic acid as the day advances, as if at times the ventilation could not keep down the increase of acid : this is contrary to the results in a country place. There is less after rain, and less during high winds.

Carbonic Acid on the River Thames, April 1864.

Locality.	Wind.	1st deter.
		Carbonic acid per cent.
London Bridge:—		
City side	S.W.	·0354
Middle	,,	·0383
Surrey side	,,	·0383
Westminster Bridge :—		
City side	,,	·0313
Middle	,,	·0344
Surrey side	,,	·0298
Before the Houses of Parliament	,,	·0329
Lambeth Bridge:—		
City side	,,	·0329
Mean of 10 experiments	....	·0343

Carbonic Acid in the Open Places of London.

Locality.	Wind.	1st deter.	2nd deter.
Hyde Park	S.W.	·0334	·0334
The same	S.E., from City.	·0306	·0299
Regent's Park	N.E.	·0304	·0304
St. James's Park	"	·0285	
Duke of York's Column	"	·0285	·0280
Highbury			
Mean of 5 experiments	....	·0301	

Carbonic Acid in the Streets of London.

Locality.	Wind.	Carbonic acid present.	
		1st exp.	2nd exp.
Cheapside, Post Office end	S.W.	·0352	·0337
Outside the Exchange	"	·0398	
Newgate Street	"	·0413	
Oxford St., above Regent's Circus	E.	·0344	·0344
Lower Thames Street	"	·0428	
Small Alley, Smithfield	"	·0337	
Small Court, Smithfield	"	·0398	
Small Court, Upper Marsh, Lambeth	"	·0382	
New Cut, Lower Marsh, Lambeth	"	·0413	
Top of the Monument	"	·0398	·0405
Mean of 10 experiments	"	·0380	
Mean of 25 experiments in London	"	·0341	

With the aid of Dr. Bernays, of St. Thomas's Hospital, I have also obtained the amount of carbonic acid in close places in London.

Carbonic Acid in Close Places in London.

	Per centage by volume.
Chancery Court, closed doors, 7 feet from ground, March 3 ...	0·193
Same, 3 feet from ground	0·203
Chancery Court, door wide open, 4 feet from ground, 11.40 A.M., March 5	0·0507
Same, 12.40 P.M., 5 feet from ground	0·045
Strand Theatre, gallery, 10 P.M.	0·101
Surrey Theatre, boxes, March 7, 10.3 P.M.	0·111

Carbonic Acid in Close Places in London (*continued*).

	Per centage by volume.
Surrey Theatre, boxes, March 7, 12 P.M.	0·218
Olympic, 11.30 P.M.	0·0817
Same, 55.11 P.M.	0·1014
Victoria Theatre, boxes, 24th March, 10 P.M.	0·126
Haymarket Theatre, dress circle, 18th March, 11.30 P.M.	0·0757
Queen's Ward, St. Thomas's Hospital, 3.25 P.M.	0·040
Edward's Ward, St. Thomas's Hospital, 3.30 P.M.	0·052
Victoria Theatre, boxes, April 4	0·076
Effingham, 10.30 P.M., April 9, Whitechapel	0·126
Pavilion, 10.11 P.M., April 9 (Whitechapel)	0·152
City of London Theatre, pit, 11.15 P.M., April 16	0·252
Standard Theatre, pit, 11 P.M., April 16 (Strand)	0·320

Pettenkofer informs us that the air of Munich may be taken as containing about 0·05 per cent. of carbonic acid; with us it is certainly below this amount; and if raised to 0·05 by breathing, one would perceive it. May we conclude that such a small amount is imperceptible without organic emanations? Munich is very high; the air must sweep over the whole continent to come to it; it may wash up the carbonic acid, and, perhaps, oxidize the organic matter.

Air with a very small loss of oxygen is perceptibly deteriorated if its place is occupied with carbonic acid and exhalations from the person, although we are not able to say how far this is the case when carbonic acid alone is substituted for this small amount of oxygen.

On the Thames it is clearly seen that the open river is purer than the streets when the water is not putrid. It is purer above, at Westminster, than at London Bridge. London is freer from this gas than Manchester, although not equal to the parks in March and April, when the experiments were made. When the new sewers are complete, the difference will probably be perceptible.

These analyses indicate that a very minute amount of carbonic acid shows deterioration of air sufficient for the senses to observe. The senses observe a difference between Manchester and the outskirts. The difference is 0·0034 per cent. The senses observe it in London, where the difference between the streets and parks is 0·0040 per cent. They observe it also on the Thames and in wet weather. But they do not observe it in Munich, which has more carbonic acid than even these towns, and more than the New Cut or Lower Thames Street. The conclusion is, that carbonic acid in these small amounts is not that which annoys us. In some towns it is no doubt sulphurous acid, in others organic matter and gases from putrefaction.

It does not follow that we must therefore neglect carbonic acid; on the contrary, it ought to be examined minutely, so that not the smallest increase be allowed, if possible; not that we know certainly of any positive evil which it can do of itself in these small quantities, but because it almost always comes in bad company.

In the above analyses the air containing ·0774 is really worse than that containing ·1604 and even 0·3, because over the middens there is a little sulphuretted hydrogen. It is well, then, in such cases to use a double test. Indeed it is probable enough that other gases besides sulphuretted hydrogen, such as marsh gas and hydrogen, products of decomposition, are issuing from cesspools and middens. I should not say probable; it is really certain. These gases, including the carbonic acid, show the reason why less oxygen should be found in such places.

I believe these analyses are of importance in the inquiry into the state of the air of all places, as they teach us the meaning of a deviation from the normal amount of carbonic acid as well as of oxygen in the air. A deviation of 0·2 is painful to us when it is caused by simple want of ventilation. If it is accompanied with gases of putrefaction, it is much more hurtful, as some of these are very deadly.

These analyses teach us to be very careful not to allow the air to become deteriorated even to a very minute extent, and that a figure in the third decimal place is not to be despised; but they teach us more—namely, that in some places, such as high mountains, a slight increase of carbonic acid, such as is found in the third, or even to the length of 2 in the second place, is rather a proof that the oxygen of the air has done its work well and purified the atmosphere, and that this increase is probably owing to pure carbonic acid. It would be well to have those experiments of Schlagintweit confirmed, where 0·07 and 0·09 are found on high mountains. To conclude, we all avoid an atmosphere containing 0·1 of carbonic acid in crowded rooms; and the universal belief of civilized men is that it is not only odious but unwholesome. When men speak of good ventilation in dwelling-houses, they mean, without knowing it, air with less than 0·07 of carbonic acid. We must not conclude that, because the quantity of carbonic acid is small, the effect is small; the conclusion is rather that minute changes in the amount of this acid are of the highest importance.

Carbonic Acid in Scotland.

Having written so far, it was desired to throw more light on the subject by obtaining specimens from purely rural and hilly districts; and for this purpose Scotland was preferred. The uniformity in the numbers is something remarkable. There is no difference in the second decimal place, even in one instance, until we enter a town. I must therefore consider ·0336 per cent. as the amount of carbonic acid in the pure winds of the north of this island. Any amount above that is a deviation from purity. If we have any regard to the third place of decimals, we find there nothing to indicate a deterioration; and we can scarcely hope to rely on the fourth place. Still the results in the fourth place are not to be rejected: we find them the highest on the plains, less at 1000 and 2000 feet, and more again at 3000 to 4000 feet, making differences of three in a million, and so at 3000 feet beginning to increase, as other observers have stated to be the case at great elevations.

Mode of Reading the Numbers.

When we read of a fraction per cent., we imagine the amount to be very small; and if the fraction is in the second decimal place, we have no faith in it. We are quite wrong. The amount of sulphuric acid in the air runs into the fourth or fifth decimal place, and the actual sulphur in it still further removed, and yet a greater effect is produced on the atmosphere than is indicated by the amount shown in the oxygen or carbonic acid volumes. In speaking of these gases, we have been accustomed to disregard minute differences, and one-tenth of a per cent. of oxygen more or less is indifferently given or taken. It may, however, assist us if we disregard the decimal point, and in the oxygen volume with three decimal places read the number as full and parts in 10,000: we shall then find that the difference between 21·00 per cent. of oxygen and 20·90 is really 10 in 10,000 of the air and 0·47 per cent. of the oxygen. This amount in the case of some gases is simply intolerable; and although not perceptible as regards oxygen, it may not be less efficient. I conclude from these inquiries that we must pay attention to the number in the second place for oxygen: perhaps at a future time we may arrive at the third.

In the case of carbonic acid we must attend to the third even now, as I believe, and even to the fourth or one part in million. The numbers are all put down as fractions, and may be read as fractions per cent.; but they may all be read as whole numbers, disregarding the decimal point. Thus ·0314 will mean 314 in a million. Between this and ·0400 we have 86 in a million, which is no trifling amount even to the senses in the case of many gases, and we must learn that the effect of poisons on health is not in proportion to the effect on the sensations.

Let us consider what is meant by this 86 in a million. A room twice the size of one not unusual, or two 30 feet long, 24 wide, and 15 high, will contain 21,600 cubic feet, or 37,324,800 cubic inches. If we introduce 0·0086 per cent., we bring 3209 cubic inches of carbonic acid into the room, which will be considered a large amount if put together in vessels: it is nearly 12 gallons. During fogs there will be an addition of nearly five times the amount = 60 gallons, or 405 in a million. If we go to a very moderately close building, we add 1235 in a million, or 14 times the first amount, or 168 gallons. If we go to a room as close as a crowded theatre, take the number given for one in London 0·320, we add 2831 in a million, or 377 gallons. And when we come to the state of air in mines, we have various numbers, in a few instances rising to more than 2 per cent.—nearly a million cubic inches, which would be 578 cubic feet, out of the supposed room, which does not differ far from this in which I now speak [the Meeting-room of the Literary and Philosophical Society of Manchester].

In order to read off the amount in a million, there ought always to be four figures after the decimal point.

CARBONIC ACID—SCOTLAND.

Place.	Time.	Carbonic acid in 100 parts. At foot.	Carbonic acid in 100 parts. At top.	Mean.		Remarks.
Perth	Aug. 4, 1 P.M.	·0341	......	·0340		Windy and cloudy.
"	" "	·0340	......			
Moncrieffe Hill	Aug. 5, 12 noon	......	·0329	·0340		Windy and cloudy, S.W. wind.
" "	" "	......	·0361			
Kalifountain Hill	" 8 A.M.	......	·0331	....		Few clouds, sunset, N.W.
Kinnoull Hill	Aug. 6, 12 noon	......	·0315	·0315		Windy and cloudy, W.
" "	" "	......	·0315			
Birnam Hill	Aug. 8, 11 A.M.	......	·0300	·0300		Windy and clear, N.W.
" "	" "	......	·0300			
Ochil Hills	" 12 noon	......	·0352	·0347		Clear, N.
" "	" "	......	·0342			
				Foot.	Top.	
Ben Ledi	Aug. 10, 11 A.M.	·0342	·0323	·0335	·0327	Clear, N.W., at foot.
" "	" 5 P.M.	·0329	·0331			Cloudy, N.W. by N., at top.
Ben Voirlich	Aug. 11, 12 A.M.	·0329	·0321	·0329	·0320	Clear, N.W.
" "	" 6 P.M.	·0329	·0319			
Ben Muich Dhu	Aug. 17, 2 P.M.	·0346	·0357	·0344	·0356	At foot, cloudy, N.W.
" " "	" "	·0342	·0356			On top, thick mist and drizzling rain.
Lachin-y-gair	Aug. 18, 3 P.M.	·0346	·0337	·0344	·0336	On top, cloudy, N.W., as at the foot.
" "	" "	·0342	·0332			
Hill near Braemar	Aug. 20, 3 P.M.	·0346	·0327	·0344	·0330	Cloudy, N.; rainy.
" " "	" "	·0342	·0334			
Ben-na-bourd	Aug. 23, 8 A.M.	·0344	·0337	·0350	·0337	Cloudy, N.
" "	" 1 P.M.	·0357	·0337			
Ben Nevis	Aug. 30, 5 P.M.	·0346	·0322	·0346	·0327	At foot, rain.
" "	" 1 P.M.	......	·0333			On top, cloudy, N.W.
Ben Lomond	Sept. 2, 3 P.M.	·0350	·0344	·0350	·0339	At foot, rain, N.W.
" "	" 12 noon	......	·0334			At top, cloudy and misty, N.W.
Schehallion	Sept. 6, 10 A.M.	·0341	·0335	·0340	·0335	At foot, cloudy, N.W.
"	" "	·0340	......			At top, rain, N.W.
Mean		·0341	·0332			

Carbonic Acid.

Places not 1000 feet high.		Places between 1000 & 2000 ft. high.	
Name of Place.	CO_2 in 100 pts.	Name of Place.	CO_2 in 100 pts.
Perth	·0341	Birnam Hill	·0300
,,	·0340	Ochil Hills	·0347
Moncrieffe Hill	·0340	Braemar (Castletown)	·0344
Kalifountain Hill	·0331	Ballochbine Forest	·0332
Kinnoull Hill	·0315	Hill near Castletown	·0334
Errol (marshy ground)	·0314	Mar Forest	·0339
Foot of Ben Ledi	·0335	Braemar (on the Dee)	·0344
Foot of Ben Voirlich	·0329	Schehallion	·0335
Aberdeen	·0329		
Elgin	·0347		
Inverness (Moray Frith)	·0341		
Inverness (above the town)	·0341		
Caledonian Canal	·0341		
Loch Ness	·0329		
Foot of Ben Nevis (Ben-navie)	·0346		
Oban	·0348		
Foot of Ben Lomond	·0350		
Forest near Killiecrankie Pass	·0353		
Foot of Schehallion (Tummel Bridge)	·0340		
Mean	·0337	Mean	·0334

Carbonic Acid.

Places between 2000 & 3000 ft. high.		Places above 3000 feet high.	
Name of Place.	CO_2 in 100 pts.	Name of Place.	CO_2 in 100 pts.
Ben Lomond	·0339	Ben Muich Dhu	·0356
Ben-na-Courd	·0337	Ben Nevis	·0327
Ben Voirlich	·0320	Ben Ledi	·0327
		Lachin-y-gair	·0335
Mean	·0332	Mean	·0336

Mean of all the foregoing in Scotland .... ·0336.

Carbonic Acid.

Place.	CO_2 in 100 pts.	Forests.	CO_2 in 100 pts.
Braemar	·0346	Ballochbine Forest, near Castletown	·0340
"	·0342	Ditto, ditto	·0325
"	·0344	Ditto, ditto	·0327
"	·0344	Mar Forest, ditto	·0336
"	·0344	Ditto, ditto	·0337
Tummel Bridge	·0340	Ditto, ditto	·0343
		Forest near Killikrankie Pass	·0353
Mean	·0343	Mean	·0337

Carbonic Acid—close Places in Perth City.

Name of Place.	Time.	CO_2 in 100 pts.
Close, 148 South Street	Oct. 10, 11 A.M.	·0399
Over a conduit, Athole Crescent	Oct. 12, 7.25 A.M.	·0401
On North Inch, near the last	Oct. 12, 8 A.M.	·0352
Craigie	Oct. 12, 1 P.M.	·0324
Craigie	Oct. 12, 3 P.M.	·0332
Craigie	Oct. 12, 10 P.M.	·0573
Paul's Close, Newrow	Oct. 14, 8 A.M.	·0430
Close, 44 Pomarium	Oct. 17, 9 P.M.	·0508
Close, 82 South Street	Oct. 18, 11¼ A.M.	·0464
Close, 44 Pomarium	Oct. 18, 12.20 P.M.	·0353
Mean		·04136
Weaver's Shop, 56 Pomarium	Oct. 17, 9.40 P.M.	·2674

We must carefully study the numbers in the City of Perth. The whole week was windy; but still the amount is higher than round the city at some distance. Although there is much irregularity in the instance at Craigie, above the town the number is high. The analysis was repeated with the same result: the reason is not clear; chimneys may suggest one. On Kinnoul Hill and Errol the amount is lower than in any case, although the latter was wet and marshy. Such inquiries scarcely end. I must take the amount in the town on a calm day. There is, however, sufficient given to prove that, taking the oxygen and the carbonic acid together, there are indications which, although minute, may be found to correspond to great effects. The close places of the town have been of late very unhealthy.

I conclude by repeating that it is important to observe minute fractions in the amount of oxygen and carbonic acid in the air.

II. *Notes on Marine Shells found in Stratified Drift near Macclesfield.* By R. D. Darbishire, B.A., F.G.S.

Read November 29th, 1864.

In August last Mr. Sainter, of Macclesfield, requested me to examine a large quantity of fragments of shells which he and Mr. J. Lowe, under his directions, had collected from sand and shingle exposed during repeated cuttings made in the progress of the new cemetery-grounds now being formed on the north side of the town. I have since made several visits to the place, and from that mass of specimens, and my own smaller series, have been able to arrange the list which I now submit. I venture to think that it presents certain considerations of peculiar interest.

It is to be regretted that careful observations were not made during the progress of the works, and due notes taken of the particular beds and conditions in which different shells have occurred. Messrs. Sainter and Lowe have been very diligent in collecting such specimens as they could find, and in buying from the workmen,—the latter a practice which could scarcely fail (as indeed it did not fail) of producing a supply of showy specimens, including *Murex*, *Cypræa*, and *Pteroceras* of tropical origin, and other shells utterly strange to the English Drift. This accident is referred to because in some cases it would seem certain that shells from some recent British beach have been used to similarly profitable advantage. Spurious remains of the latter class, obviously introduced to meet the novel demand, while they are often more difficult to detect, throw suspicion upon some specimens which may after all be genuine.

There are, nevertheless, many undoubted fossils; and the list is larger than has yet been published of such remains in this neighbourhood. It is offered as a small contribution towards the multitude of observations on which at some future time a solution of the perplexities of the so-called "Drift" of the district may, it is to be hoped, be founded.

The beds in question are situated on the easterly side of the road from Macclesfield, past the Free Park, and on the north side of a small ravine which runs from the road, skirting the Cemetery Hill on its south-easterly declivity, to open into the valley of the Bollin. They were exposed chiefly on a south-easterly face along the ravine, but have been much defaced by terraces and ballast-tips, so that very little actual section can now be examined.

Situated at an elevation of between 500 and 600 feet above the sea-level, these beds show a vertical thickness of about 70 feet under the loam. They consist of fine (running) sand, fine and coarse shingle, and very coarse gravel of rounded pebbles, up to the size of a man's head or larger. I did not observe scratches on any of these pebbles. These materials are stratified, in general, horizontally, but exhibit considerable irregularity in the extension and levels of particular portions of successive strata. On both sections (north and south, and east and west) almost every layer shows various examples of false bedding, the whole presenting a characteristically marine aspect, and telling of the ebb and flow of currents, varying at short intervals, and probably not far from a coast-line. These sands rest upon a bed of marl with scratched boulders, the so-called Lower Boulder-clay of the Geological Survey. On the clay, in a shallow hollow at the bottom of the ravine, lies a bed of peat, over the surface of which a rill finds its way to join the Bollin.

As a trivial instance of the concurrence of remains, I may mention that during these cuttings there have been found, in the superficial loam, many of the small tobacco-pipe-heads, of Dutch manufacture, of the 17th century, and below them a silver coin of Edward the Second, and, in the same bed, in the lower part of the ravine, a curious very ancient weight, made of burnt clay, for sinking fishing-nets.

In the peat are found abundant hazel-nuts.

On no section, during several visits, was I able to find the shells *in situ* in quantity; but Mr. Lowe speaks of having observed the occurrence of fragments in layers.

In the following list the obviously spurious shells have not been noticed. A few species, particular specimens of which appear to be of doubtful authenticity, are marked in the 5th column with the letter D. It is, however, not impossible that some of these questionable remains may yet be genuine.

All the specimens are either much broken, even into small fragments, or much rolled and worn. A certain number may be put on one side as bearing the appearance of greater comparative freshness, having parted with less animal matter. These uniformly show signs of great attrition: they are noted in the 4th column. The specimens noted in the 3rd column present, as a whole, a facies of more complete fossilization, are often even friable and all, except only in the case of minute convolute shells particularly, broken up into little pieces.

The two groups have probably come from different beds, the first mentioned being from a newer deposit.

References :—a, abundant; c, common; f, frequent; r, rare; and v. r, very rare. D, some specimens probably spurious.

As some of the species are of peculiar interest, and identified from small fragments only, it may be worth while to state that none have been named except from remains showing undoubted and distinctive characteristics *.

	Species.	Frequency ? Older.	Frequency ? Newer.	Remarks.
1.	Pholas crispata, *Linn.* ...	v. r.		
2.	—— candida, *Linn.*	...	v. r.	Two questionable umbonal fragments, D.
3.	Mya truncata, *Linn.*	f.	r.	Hinge and other fragments.
† 4.	—— arenaria, *Linn.*	v. r.	...	One characteristic umbonal and hinge-fragment.
5.	Psammobia ferroënsis, *Chemn.*	f.	...	Many fragments, all very much worn and remarkably thick.
6.	Donax anatinus, *Lam.* ...	v. r.	...	One fragment, D.
7.	Tellina solidula, *Pult.* ...	c.	f.	Whole valves and fragments. Many very D.
8.	Mactra solida, *Linn.* ...	r.	r.	Valves and fragments.
9.	Lutraria elliptica, *Lam.*	v. r.	...	Characteristic hinge-fragments.
10.	Cytherea chione, *Linn.*...	f.	...	Umbonal portions & hinges of several individuals; also lateral fragments.
11.	Venus striatula, *Donovan*	r.	...	Fragments.
		...	v. r.	Valves, D.
12.	Artemis lincta, *Pult.* ...	r.	...	Hinge-fragments.
13.	Cyprina islandica, *Linn.*	c.	...	Hinge-fragments and others, small and all much worn.
14.	Astarte elliptica, *Brown.*	r.	...	Fragments.
15.	—— arctica, *Gray*	c.	...	Fragments.
16.	Cardium echinatum, *Linn.*	f.	...	Fragments.
		...	r.	Whole valves, D.

* While Mr. J. Gwyn Jeffreys's valuable manual is still incomplete, it appears most convenient to follow the nomenclature of Messrs. Forbes and Hanley's 'British Mollusca.'

† *Corbula nucleus*, Linn., has also occurred (older v. r.).

TABLE (*continued*).

	Species.	Frequency. ? Older.	Frequency. ? Newer.	Remarks.
17.	Cardium aculeatum (?), *Linn.*	v. r.	...	Two fragments, so named in Mr. Sainter's collection. May be of this species; but quære.
18.	—— rusticum, *Linn.*	r.	...	Several characteristic fragments.
19.	—— norvegicum, *Spengler.*	v. r.	..	One characteristic fragment.
20.	—— edule, *Linn.*	f.	...	Fragments.
		...	r.	Whole valves, D.
21.	Mytilus edulis, *Linn.*	f.		
22.	Modiola modiolus, *Linn.*	r.		
23.	Nucula	...	v. r.	A fragment.
24.	Arca lactea, *Linn.*	...	v. r.	Valves, apparently genuine.
25.	Pectunculus	v. r.		
26.	Pecten opercularis, *Linn.*	v. r.	...	Fragments.
27.	Ostrea edulis, *Linn.*	v. r.		
		...	r.	(Several fragments are very D.)
28.	Patella vulgata, *Linn.*	...	v. r.	
		...	r.	D.
29.	Dentalium entalis, *Linn.*	r.		
30.	—— abyssorum, *Sars*	...	v. r.	(J. G. Jeffreys.)
31.	Trochus cinerarius, *Linn.*	...	v. r.	One in Mr. Sainter's collection seems genuine, others D; *no fragments.*
32.	Littorina littorea, *Linn.*	...	v. r.	Fragments broken, rolled, and much worn; some D.
33.	—— rudis, *Donovan*	...	v. r.	D.
34.	—— littoralis, *Linn.*	...	v. r.	D.
35.	Turritella communis, *Risso*	a.		
		...	c.	
36.	Aporrhais pespelicani, *Linn.*	r.	...	Fragments.
		...	1	D.
37.	Natica nitida, *Donovan*	...	v. r.	D.
38.	—— monilifera, *Lam.*	...	v. r.	One young specimen.
39.	Murex erinaceus, *Linn.*	f.	...	Fragments.
		...	r.	Entire, much rolled, ? D.
40.	Purpura lapillus, *Linn.*	f.	...	Fragments.
		...	f.	Entire, much rolled, ? D.
41.	Nassa reticulata, *Linn.*	v. r.	...	Fragments.
		...	f.	Entire, much rolled, ? D.
42.	—— incrassata, *Müller*	v. r.		
43.	Buccinum undatum, *Linn.*	r.	r.	Fragments. Certain specimens D.

TABLE (*continued*).

	Species.	Frequency. ? Older.	Frequency. ? Newer.	Remarks.
44.	Fusus gracilis, *Lovén* ...	v. r.	...	One young, genuine. Another specimen, adult, in Mr. Sainter's collection, *may* be genuine; another, with epidermis, spurious.
45.	—— antiquus, *Linn.* ...	r.	...	Fragments, much rolled.
46.	Trophon clathratus, *Linn.*	f.	...	*Bamffius*, Donovan. Large and small.
47.	Mangelia turricula, *Montagu*	f.	...	Several, large and small.
48.	—— rufa, *Montagu*	r.		
49.	Cypræa europæa, *Montagu*	v. r.		
50.	Cliona (two species)	r.	...	In *Turritella*, and fragments of Bivalves.

Of the foregoing forty-nine species of marine shells, all are noted in Prof. E. Forbes's digested list of Pleistocene fossils of the British Isles (Mem. Geological Survey, vol. i.), except the following :—

Pholas candida (D.).
Cytherea chione.
Cardium rusticum.
—— aculeatum (?).
Arca lactea.
Littorina littoralis.
Dentatum abyssorum

(this last, perhaps, not identified by Prof. Forbes).

Of those which appear in that list, the following are noted by Mr. McAndrew (Geog. Distribution of Testaceous Mollusca in the South Atlantic and Mediterranean, 1854) as reaching their southern limit within the British seas :—

Cyprina islandica.
Astarte elliptica.
Astarte arctica.
Trophon clathratus.

And the following to extend southwards as far as the British Channel :—

Mya truncata.
—— arenaria.
Modiola modiolus.
Buccinum undatum.
Fusus gracilis.
—— antiquus.

The latter 10 species are in fact northern shells extending southwards. Of the remaining 32 species, the whole now range considerably southward of the British Isles, but, as a set, present a characteristically British aspect.

The remarkable feature of the Macclesfield list is therefore to be found in the 7 species not in Prof. Forbes's list, all the rest being already well known as members of the Pleistocene and recent faunas of these isles. Of these 7 not one appears in Mr. McAndrew's "List of Mollusca observed between Drontheim and the North Cape" (Annals of Nat. Hist., May 1856), nor in Danielssen's 'Zoological Notes of the Scandinavian coast,' 1857, except *Littorina littoralis*, a shell which, from its peculiarly littoral habit and its restriction to the fucus-herbage of the tidal rocks, may very well be absent from an extensive deposit of shingle.

The remaining 6 are all shells of species which at present reach their northern limit within the British seas extending on our western shores, from the Spanish province.

Cytherea chione,	Cardium aculeatum (?),
Cardium rusticum,	Arca lactea,

are characteristically shells of a Spanish or southern type. The *Cytherea* is not now found north of Caernarvon Bay, nor in the German Ocean: it is "essentially a southern species." The *Cardia* barely frequent the coasts of Devonshire and Cornwall. *Cardium aculeatum* is said to have been dredged off Bergen; but *C. rusticum* is not known east of the Channel. As a matter of historical geology, the *Cytherea* inhabited British seas during the Miocene period of the Coralline Crag, but has not occurred in the Red Crag. The *Arca* occurs in both beds; the *Cardia* in neither. Mr. J. Smith is quoted as having found *C. rusticum* in newer Pliocene beds in Worcestershire*.

* It may be worth while to describe the specimens of these species. *C. chione*, six umbonal fragments, three showing complete dentition, one less complete, and two half the hinge. Three fragments from the ventral region; all considerably worn. *C. rusticum*, several fragments, one showing poste-

On comparing the present list with that of the Caernarvonshire deposit on Moel Tryfaen, as the most elevated of known drift fossils, we have as yet found out of 60 species only 32 at Macclesfield, the excess of the Welsh list consisting of arctic forms. The 18 which occur in the present series, and not on Moel Tryfaen, consist of the 7 southern shells and 11 others which are all of British rather than arctic distribution.

The Macclesfield series is, therefore, distinguished from that of Moel Tryfaen by the absence of the northern, and presence of the southern shells.

Comparing the present list with a list of shells found in an estuarine deposit at Kelsey Hill, near Hull, somewhat fuller than that given by Mr. Prestwich (Geol. Journal, xvii. 443), we find 24 common to the two, and all the northern and all the southern absent from the Hull list. The remaining absentees are all species of shells now common in British seas, whose non-occurrence in that bed is not without explanation in the greater limitation of the series and the position of the deposit near the great river that supplied it with its Elephants' teeth and its *Cyrena fluminalis*.

This latter comparison, however, is interesting in that it affords evidence that the Cheshire beds were deposited in a sea that beat from the westward against the Derbyshire Hills after the period had commenced during which the physical conditions of the western sea have differed as they now do from those of the eastern. On the other hand, the presence of the three shells I have referred to (*C. chione*,

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rior lateral tooth, ligamental crevice, and a portion of adjacent surface, the others very characteristic fragments from the central and marginal regions of the valve. *C. aculeatum*, two fragments. These present, however, an appearance very similar to certain deeper-water forms of *C. echinatum*, and may be of that species. They differ, however, in the direction of *C. aculeatum* from the fragments of *C. echinatum* occurring along with them. *Arca lactea*, two almost perfect valves, apparently genuine.

C. rusticum, and *A. lactea*) in the Macclesfield list indicates a larger extension northwards into the basin of the Irish Sea, than now obtains, of the so-called southern or Spanish fauna. This might indeed be expected. A depression of the Welsh land, such as hoarded the shells of southern Spain in the recesses of the valley of the Mersey, would now heap the same species in the sources of the Dovey and far up the Severn valley.

It will be recollected, moreover, that a depression of 600 feet would leave only a few rocky islands on the east and south of what is now the mainland of Ireland, and probably carry some degrees further to the eastward that warmer influence which has been ascribed to the impact of the Gulf-stream on the zoologically speaking rich Atlantic shores of modern Ireland.

The tidal current alone of so weighty a mass of water would carry to or maintain the species of the warmer seas at their furthest extension, past the site of the present St. George's Channel.

I am not able to state what lands would be submerged by a similar depression of the country to the south-west of Macclesfield.

P.S.—I am tempted by the interest of an observation which I have just made, and which bears especially on one point in my remarks, to add the following:—

Mr. Green, of the Geological Survey, called my attention to a patch of gravel discovered by Mr. Prestwich some years ago, in which that gentleman had found fragments of shells at an elevation considerably greater than that of the Cemetery Hill—namely, about 1200 feet above the sea. This deposit I have just visited.

It appears in an escarpment on the north side of a brook, near the Buxton new road, about half a mile eastward of the first toll-bar out of Macclesfield. It is of precisely similar character to the cemetery-beds, except that the dry sand is somewhat clogged, and in places interleaved as it were, by a reddish clay, full of, and frequently laminated by, layers of very small scales of mica.

Under somewhat unfavourable meteorological conditions, I made out the height to be from 1120 to 1160 feet above the sea-level. At this spot I picked out from amongst fine shingle, stratified in beds of rounded but, so far as I noted, unscratched stones, fragments of 12 species, as follows :—

- Psammolia ferroensis.
- Tellina solidula.
- Mactra.
- Cytherea chione (a characteristic hinge-fragment, and another).
- Artemis lincta.
- Astarte arctica.
- Cardium echinatum.
- Cardium edule.
- Mytilus.
- Turritella communis.
- Fusus antiquus.
- Trophon.

The occurrence of the *Cytherea* in this bed at a height of 600 feet above the beds examined on the west of Macclesfield is very curious, and adds a formidable consideration to the many difficulties which seem as yet to delay the solution of the "Drift" problem.

The two beds appear to have been deposited under very similar conditions, and alike belong, I believe, to the beds which the Ordnance Geologists intercalcate between their higher and lower Boulder-clays. I understand that the higher clay has not been proved above Mr. Prestwich's patch.

It is difficult to conceive of the deposit of a continuous bed of shingle 600 feet deep, with precisely similar fossils in its highest and lowest layers, and of the removal of the whole of this formation except a few patches of each layer lying within a space of 6 miles. It is more difficult to suppose that the cemetery-beds can be a redistribution of such portions of Mr. Prestwich's gravel as the wave of a retreating sea carried away while the land was rising. It is scarcely more easy to believe that the cemetery-beds and those of the higher land are merely portions of a deposit under similar conditions on a rising coast, the incline being not less than 600 feet in 6 miles. Can there have been a local alteration of level?

III. *On some Products derived from Indigo-blue.*

By EDWARD SCHUNCK, PH.D., F.R.S.

Read January 10th, 1865.

MY experiments on the formation of indigo-blue, an account of which I had the honour of presenting to this Society several years ago, led me to make some inquiries regarding the processes employed in tropical countries for the production of indigo from the various plants yielding that dye-stuff. I found that all the authors who have written on the subject agree in affirming that the process of fermentation, which is the one usually adopted for the purpose of extracting the colour from the plant, requires to be conducted with the greatest care, in order to yield a successful result. Unless certain precautions are adopted, a product of very inferior quality will be obtained; in some cases, indeed, the colouring matter is entirely lost. This will not be surprising to any one who considers that though indigo-blue, when once formed, is a very stable compound, the substance existing in the cells of the plant from which it originates, and which I have named *indican*, is decomposed with the greatest facility in various ways; that indigo-blue is only one of its products of decomposition, and may be formed or not, according to the nature of the process to which it is submitted. With this sufficiently obvious explanation I should have been inclined to rest contented, had I not acquired a knowledge of some other facts relating to indigo-blue, to which the same explanation cannot be applied, but which evidently belong to the same class.

It is well known to those dyers who employ the so-called woad-vat, in which the reduction of the indigo-blue is effected by the action of various organic matters, such as woad, madder, and bran, together with lime, that if the process be not carefully managed it may change its character entirely, the contents of the vat entering into a state of complete putrefaction—a change which results in the total destruction, or at least disappearance, of the colouring matter. Now this phenomenon, the reality of which cannot be doubted, though its nature has never been subjected to scientific scrutiny, cannot be explained in accordance with what is at present known regarding indigo-blue, which is considered by chemists to be a body of such a stable character as not to be decomposed by any except very potent agents, such as chlorine, bromine, and nitric acid. In no work on scientific chemistry is it stated that indigo-blue may be decomposed by any process of fermentation or putrefaction, in the same way as sugar or albumen.

In my experiments on indigo-blue I have generally employed for its reduction and purification the process of Fritzsche, which consists in acting on it with a mixture of alcohol, grape-sugar, and caustic soda. The colouring matter dissolves when the mixture is heated, and is again deposited on exposure to the atmosphere in crystalline needles. Now in performing this operation with very small quantities of indigo-blue and an excess of alcohol and grape-sugar, I found that the colouring matter did not make its appearance again on agitating the solution with air. The yellow colour of the liquid passed as usual through red to green; but, instead of the indigo-blue being precipitated, the whole became yellow or brownish-yellow, and the colouring matter disappeared entirely. In this way I had the mortification of losing a quantity of indigo-blue, which I had prepared with much labour from human urine, though the loss resulted, as it afterwards turned out, in some gain of information.

This fact was also difficult to account for, since it is usually supposed that by the combined action of reducing agents and alkalies indigo-blue merely takes up an atom of hydrogen and then dissolves, and, by the action of the atmospheric oxygen is again precipitated, unchanged and undiminished in quantity.

In order to ascertain on what the disappearance of the colouring matter in this case depends, I first dissolved a small quantity of indigo-blue by means of grape-sugar and caustic soda, using water as a solvent instead of alcohol; but though the indigo-blue was kept for a long time in solution, and heat was applied at the same time to assist the action, it made its appearance again on exposure to the air, apparently undiminished in quantity. In another experiment, in which alcohol was used as the menstruum and protoxide of tin as the reducing agent, the same result was arrived at. It was therefore apparent that the disappearance of the colouring matter was due to the combined action of the alcohol and the grape-sugar, not to the separate action of either. By the use of a great excess of these two agents, together with caustic soda and the long-continued application of heat to the solution, I succeeded in causing several grammes of indigo-blue to disappear entirely. I avoid the word *decompose*, because, as I shall show, the colouring matter is not decomposed, but enters into new forms of combination.

It now occurred to me that since, by the action of caustic alkalies on sugar, acetic and formic acids are formed, the effect produced by the grape-sugar in this process might in reality be due to the presence of one or both of these acids rather than to that of the sugar itself. My supposition was completely verified by experiment. On treating some pure indigo-blue with alcohol, to which an alkaline solution of protoxide of tin was added until it dissolved, then adding acetate of soda and digesting at a moderate heat, the indigo-blue after some time ceased to be deposited on exposure to the air, or even agitation; it had entirely disappeared. The same thing occurred when formiate of soda was employed in the place of acetate. It was evident, therefore, that in this process acetic or formic acid was capable of playing the same part as grape-sugar; and as the use of the latter might have tended to introduce complications, in consequence of the formation of secondary products, I ceased to employ it in my subsequent experiments. The object of the present communication is to give an account of the combined action of alcohol, acetate of soda, and caustic alkali on indigo-blue, and the products thereby formed.

At the commencement of the investigation I imagined that it was an essential condition that the indigo-blue should be in a state of solution; but I soon found that this was not necessary. The operation succeeds equally well if indigo-blue freshly precipitated or in fine powder be employed. The plan which I adopted was quite simple. Pure indigo-blue was introduced into a large quantity of ordinary spirits of wine, and, after being well agitated, the mixture was raised to the boiling-point. A quantity of pure acetate of soda, previously deprived of its water of crystallization, and a little solid caustic soda were then added, and the boiling was continued for several hours. A reduction of a portion of the indigo-blue took place in the first instance, as was evident from the deep red colour of the liquid. On agitating with air, this red colour disappeared for a moment, the indigo-blue being precipitated in powder, to be again dissolved on boiling the liquid; but after some time the liquid acquired a dark-brown colour, and deposited nothing on exposure or agitation. The process was then completed. There sometimes remained a residue of indigo-blue, which obstinately resisted the action of the boiling liquid; but, on pouring off the latter, and adding fresh materials, it generally disappeared rapidly. I found it advisable to employ only a small quantity of indigo-blue at a time, as the process is a slow one, and requires a great excess of alcohol and acetate of soda. The presence of caustic alkali I found to be quite essential, as no perceptible action took place without it; but the quantity required was not large. The stronger the alcohol, and, generally speaking, the freer from water all the substances employed were, the more rapidly was the process completed.

In order to obtain the products resulting from this process, I proceeded as follows:—The dark-brown alcoholic liquid containing them was first mixed with sulphuric acid until it had acquired a slightly acid reaction, and it was then evaporated. During evaporation, brown resinous masses were deposited; and on adding water, when the evaporation was nearly completed, a fresh quantity of resin-like matter was thrown down. The liquid filtered from this matter was still brown. It was evaporated to a syrup, which, after standing some time, became solid from the formation of crystals, consisting chiefly of acetate of soda. The whole mass of crystals was then dissolved in boiling alcohol, and tolerably strong sulphuric acid was added to the solution, until no more sulphate of soda was precipitated, care being taken to avoid an excess of the acid. The liquid, after standing some time, was filtered and evaporated, so as to drive off the acetic acid as well as the alcohol. When the evaporation was nearly completed, water was added, which threw down a large quantity of a brown pulverulent substance, as well as a little brown resin, which, after filtration, were added to the resinous matter previously obtained. The filtered liquid had lost much of its brown colour. I shall return to it presently.

The products insoluble in water obtained in this manner consist partly of resinous, partly of pulverulent substances. Among these products there are at least five distinct substances, which I have succeeded in separating from one another by the use of various solvents; but it is probable that small quantities of other substances closely resembling them are also formed at the same time. These bodies are all unfortunately amorphous, and possess very few characteristic properties. It is indeed only their origin and mode of formation which impart to them any interest; and I shall therefore refrain from adding to the already cumbrous mass of terms with which organic chemistry has to deal by inventing names for them, but shall simply distinguish them by the letters of the alphabet.

The process adopted for the separation of these substances from one another was as follows:—The whole of the mass insoluble in water was first treated with boiling water in order to remove all the sulphate and acetate of soda. It was then dried, finely pounded, and treated with successive doses of ether, as long as anything dissolved. The ethereal liquid, which had a rich reddish-brown colour, was filtered and evaporated, when it left a resin-like residue of the same colour. This residue was digested with weak caustic ammonia, which dissolved a great portion of it. The portion insoluble in ammonia was filtered off, washed, dried, and then treated with ether, which generally left a small quantity of brown powder undissolved. The filtered ethereal solution was evaporated, and the residue was dissolved in cold alcohol, which left behind a little resinous matter. The filtered liquid left on evaporation a brittle, brownish-yellow resin, which I assume to be an unmixed substance, and shall distinguish by the letter A. The matter dissolved by the ammonia was precipitated by acid in thick flocks, which, after being filtered off, washed, and dried, were treated with ether. The ether left some brown powder undissolved, which was separated by filtration. The liquid was evaporated, and the residue was treated again with ether, in order to separate a little more of the brown powder. The substance was then introduced into a hot solution of carbonate of ammonia, which, if not too concentrated, dissolved the greatest part of it, leaving only some brown powder behind. If, as sometimes happened, the solution of carbonate of ammonia was not sufficiently dilute, very little was dissolved by it, the greatest part of the substance sinking to the bottom of the vessel as a viscid resinous mass, which dissolved, however, almost entirely on pouring off the liquid and adding pure water. The addition of acid to the filtered solution produced a brown flocculent precipitate, which was filtered off, washed with water, and treated with cold alcohol. The filtered alcoholic solution left, on evaporation, a resinous body hardly to be distinguished in appearance from the preceding, and which I will denote by the letter B.

The matter insoluble in ether, constituting by far the larger part of the whole mass, was first treated with a little cold alcohol, to which it communicated a dark-brown colour. The filtered alcoholic liquid left, on evaporation, a brown resinous residue, which was not further examined, since it was sure to contain some of that well-known product of decomposition which is formed by the action of caustic fixed alkalies on alcohol, and which, being also resinous, I saw no prospect of being able to separate from any product derived from indigo-blue which might be mixed with it. The portion left undissolved by the cold alcohol was, after being dried, a brown powder, which consisted of three substances. In order to separate these from one another, the mixture was first subjected to the action of boiling dilute caustic soda-lye, in which one of the three was found to be insoluble. The alkaline liquid, which was of a dark-brown colour, was filtered, and the residue left undissolved was again treated with alkali in order to remove the whole of the soluble portion, and it was then treated with a boiling alcoholic solution of caustic soda, in which the greatest part dissolved with ease. The dark-brown solution was filtered and then mixed with an excess of hydrochloric acid, which precipitated the greatest part of the substance as a dark-brown powder. This was collected on a filter, washed with alcohol until all the acid and chloride of sodium were removed and dried. This body I will distinguish by the letter C. The caustic soda-lye contained the two other substances in solution, and it was accordingly mixed with an excess of acid, which produced an abundant brown flocculent precipitate. This was collected on a filter, well washed with water, and then treated with a boiling solution of acetate of soda, which dissolved part of it, thereby acquiring a brown colour. The liquid was filtered boiling hot, and the residue was treated with fresh solution of acetate of soda, the process being repeated as long as the boiling liquid acquired any colour. The residue left undissolved by the acetate of soda was treated with boiling alcohol containing a little ammonia, in which it dissolved with ease, forming a dark-brown solution, from which the greatest part was again precipitated on the addition of an excess of hydrochloric acid as a brown powder. This was filtered off, well washed with alcohol, and dried. This body may be denoted by the letter D. The substance held in solution by the acetate of soda was precipitated by sulphuric acid in brown flocks, which were filtered off, well washed with water, and then treated with boiling alcohol, in which they dissolved completely. The alcoholic solution deposited, on cooling, a brown powder, which was collected on a filter, washed with a little cold alcohol, and dried.

To this product I apply, for the sake of distinction, the letter E.

The acid liquid filtered from the mixture of substances insoluble in water still contained in solution a product of decomposition derived from the indigo-blue. It was evaporated until crystals began to appear on its surface, and it was then set aside and allowed to stand for some time, when a large quantity of crystals was gradually deposited. After separation from the mother liquor, these crystals appeared of a brown colour; but by recrystallization from boiling water and decolorization with animal charcoal, they were rendered white and pure. They were then found to have the properties and composition of anthranilic acid, the well-known product formed by the action of caustic alkalies on indigo-blue. The mother liquor of the crystals left, on evaporation, a thick brown syrup, which seemed to be a compound of anthranilic acid and acetic acid. On dissolving it in water, adding sulphuric acid to the solution and evaporating, I obtained a quantity of crystals, which were purified by crystallization, first from water and then from boiling alcohol. They differed in appearance from anthranilic acid, and consisted indeed of a compound of the latter with sulphuric acid. The same compound is obtained in place of uncombined anthranilic acid, if a great excess of sulphuric acid beyond what is required to unite with the free soda and that combined with acetic acid and the various products yielded by the process has been employed in the first instance. The sulphate, being more soluble in water than the free acid, does not crystallize so easily from the brown syrup which the liquid always leaves on evaporation, and hence it is advisable not to use an excess of sulphuric acid in the process above described for the separation of the anthranilic acid.

As regards their properties, the products insoluble in water present very little that is of interest. The body A is a brittle, amorphous, brownish-yellow resin, transparent in thin layers. At a temperature of 100° C. it becomes soft and semi-liquid. When heated on platinum foil, it burns with a bright flame, leaving much charcoal, which, on being heated, disappears without leaving any ash. It is decomposed by boiling nitric acid, yielding a product of decomposition in crystalline needles. It is quite insoluble in alkaline liquids, such as caustic potash, soda, and ammonia, even when a reducing agent, such as protoxide of tin, is added; but it is decomposed on being heated with dry soda-lime, giving off alkaline fumes having a peculiar penetrating odour. The body B can hardly be distinguished by its external appearance from A, with which it has also many properties in common; but it is easily soluble in caustic and carbonated alkalies, yielding yellow solutions, from which it is precipitated by acids in brown flocks. The compounds with baryta, lime, lead, silver, and copper prepared by double decomposition are brown or yellow, and insoluble in water. When treated with boiling nitric acid it behaves like A, yielding also a product of decomposition crystallizing in needles. The body C is a brown powder, which, on being heated, burns without previously melting; it is insoluble, like A, in watery solutions of alkalies, and very little soluble in alcohol alone, but easily soluble in an alcoholic solution of soda. D resembles C in most of its properties, but differs from it by its solubility in caustic and carbonated alkalies. E is a reddish-brown powder, soluble in alkalies, and more easily soluble in alcohol than C and D, but distinguished from the others chiefly by its solubility in acetate of soda.

The composition of these bodies is, however, a matter of some interest, since it is only from a knowledge of their composition that any light can be thrown on the nature of this curious process. I shall, therefore, proceed to give a short account of the results yielded by the analysis of these products which will lead to a few remarks regarding their mode of formation and probable constitution.

A.

Of this body I made two series of analyses, the specimens being prepared on different occasions. Unfortunately the results to which they led did not harmonize, though no difference could be detected in the external properties of the two specimens.

I. 0·3275 grm. dried at 100° C., and burnt with oxide of copper and oxygen, gave 0·9135 grm. carbonic acid and 0·2360 grm. water.

0·5390 grm., burnt with soda-lime, gave 0·2470 grm. chloride of platinum and ammonium.

II. 0·3290 grm. of the same gave 0·9165 grm. carbonic acid and 0·2335 grm. water.

These numbers lead to the formula $C_{62} H_{39} NO_8$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{62}	372	76·07	76·07	75·97
H_{39}	39	7·97	8·00	7·88
N	14	2·86	2·87	
O_8	64	13·10	13·06	
	489	100·00	100·00	

In order to explain the formation of a body of this composition in this process, it is necessary to assume that 1 atom of indigo-blue has combined with 8 atoms of alcohol, 3 ats. of acetic acid, and 2 ats. of carbonic acid, the whole losing 26 ats. of water, and forming 1 at. of the substance, since

$$C_{62} H_{39} NO_8 + 26\,HO = \underset{\text{Indigo-blue.}}{C_{16} H_5 NO_2} + \underset{\text{Alcohol.}}{8\,(C_4 H_6 O_2)} + \underset{\text{Acetic acid.}}{3\,(C_4 H_4 O_4)} + 2CO_2.$$

On the next occasion, though the method of preparation was exactly the same as that above described, the analysis of the substance led to different results, as the following details will show:—

I. 0·3810 grm. gave 1·0600 grm. carbonic acid and 0·2385 grm. water.

0·7280 grm. gave 0·5000 grm. chloride of platinum and ammonium.

II. 0·4015 grm. gave 1·1200 grm. carbonic acid and 0·2500 grm. water.

0·6235 grm. gave 0·4265 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{80}H_{44}N_2O_{10}$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{80}	480	75·94	75·87	76·07
H_{44}	44	6·96	6·95	6·91
N_2	28	4·43	4·31	4·29
O_{10}	80	12·67	12·87	12·73
	623	100·00	100·00	100·00

Though this formula differs widely from the first, it presupposes a similar mode of formation for the substance, the only difference consisting in the relative quantities of the elements uniting to produce it, as will be seen from the following equation:—

$$C_{80}H_{44}N_2O_{10}+28\,HO=2(\underset{\text{Indigo-blue.}}{C_{16}H_5NO_2})+9(\underset{\text{Alcohol.}}{C_4H_6O_2})+2(\underset{\text{Acetic acid.}}{C_4H_4O_4})+4CO_2.$$

It appears, therefore, that in both cases its formation was due to the union of indigo-blue with alcohol, acetic acid, and carbonic acid, accompanied by the loss of a certain proportion of water. The alcohol and acetic acid are ingredients employed in the process; but it is not easy to see whence the carbonic acid is derived. I think, however, it may originate in the formation of anthranilic acid. This acid, as is well known, is produced by the action of caustic alkalies on indigo-blue, which, taking up water and oxygen, yields anthranilic acid and carbonic acid, in accordance with the following equation :—

$$\underset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+2HO+4O=\underset{\text{Anthranilic acid.}}{C_{14}H_7NO_4}+2CO_2.$$

The oxygen in this case must be derived from water, the hydrogen of which, instead of being set at liberty, probably unites with a portion of the indigo-blue, forming reduced indigo, which dissolves in the caustic alkali. Hence the partial reduction and solution of the indigo-blue, which, as mentioned above, is observed at the commencement of the process. The carbonic acid does not, as might naturally be supposed, combine with the alkali, but unites *in statu nascenti* with alcohol, acetic acid, and a portion of the indigo-blue to form the body A. That it should do so in the presence of an excess of alkali is not more surprising than that acetic acid should, under the same circumstances, leave the base with which it is combined in order to form a perfectly neutral body—a fact of which there can be no doubt.

B.

Of this body also two series of analyses were made, the material being obtained at the same time as that of the two series of A. The first series yielded the following results :—

I. 0·3315 grm. gave 0·8925 grm. carbonic acid and 0·2465 grm. water.

0·5270 grm. gave 0·2645 grm. chloride of platinum and ammonium.

II. 0·3330 grm. gave 0·9015 grm. carbonic acid and 0·2445 grm. water.

0·5420 grm. gave 0·3190 grm. chloride of platinum and ammonium.

III. 0·3390 grm. gave 0·9195 grm. carbonic acid and 0·2530 grm. water.

0·5230 grm. gave 0·2625 grm. chloride of platinum and ammonium.

These numbers correspond with the formula $C_{52} H_{35} NO_8$, which requires

	Calculation.		Experiment. I.	II.	III.
C_{52}	312	73·41	73·42	73·83	73·97
H_{35}	35	8·23	8·26	8·15	8·29
N	14	3·29	3·15	3·69	3·15
O_8	64	15·07	15·17	14·33	14·59
	425	100·00	100·00	100·00	100·00

This body is therefore formed by the union of 1 at. of indigo-blue, 6 ats. of alcohol, and 3 ats. of acetic acid, 18 ats. of water being eliminated, as will be seen by the following equation :—

$$C_{52} H_{35} NO_8 + 18HO = \underset{\text{Indigo-blue.}}{C_{16} H_5 NO_2} + \underset{\text{Alcohol.}}{6(C_4 H_6 O_2)} + \underset{\text{Acetic acid.}}{3(C_4 H_4 O_4)}.$$

The second series of analyses made of this body gave the following results :—

I. 0·4420 grm. gave 1·0810 grm. carbonic acid and 0·2515 grm. water.

0·6550 grm. gave 0·3865 grm. chloride of platinum and ammonium.

II. 0·4610 grm. gave 1·1820 grm. carbonic acid and 0·2745 grm. water.

0·5085 grm. gave 0·3325 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{40} H_{23} NO_8$, which requires

	Calculation.		Experiment. I.	II.
C_{40}	240	70·38	69·86	69·92
H_{23}	23	6·74	6·62	6·61
N	14	4·10	3·70	4·10
O_8	64	18·78	19·82	19·37
	341	100·00	100·00	100·00

In this case the composition is to be explained by supposing that 3 ats. of alcohol and 3 ats. of acetic acid have combined with 1 at. of indigo-blue to form 1 at. of the substance, since

$$C_{40}H_{23}NO_8+12HO=\underset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+\underset{\text{Alcohol.}}{3(C_4H_6O_2)}+\underset{\text{Acetic acid.}}{3(C_4H_4O_4)}.$$

It appears, therefore, that in the case of this body, as in that of A, the composition may vary extremely, without any corresponding difference in external appearance and properties. The difference in composition in both cases is owing to the different proportion between the elements—indigo-blue, alcohol, acetic acid, and carbonic acid—of which they are composed. The two formulæ to which the analyses of B led, viz. $C_{52}H_{35}NO_8$ and $C_{40}H_{23}NO_8$, differ from one another by a multiple of CH, and they therefore represent homologous bodies.

C.

This body is formed in relatively small quantities, and I only obtained sufficient for one analysis, which yielded the following results :—

0·3600 grm. gave 0·9790 grm. carbonic acid and 0·1605 grm. water.

0·5435 grm. gave 0·5175 grm. chloride of platinum and ammonium.

Hence it is to be inferred that the formula is $C_{28}H_{11}NO_4$, which requires

	Calculation.		Experiment.
C_{28}	168	74·66	74·16
H_{11}	11	4·88	4·95
N	14	6·22	5·98
O_4	32	14·24	14·91
	225	100·00	100·00

This formula leads to the conclusion that the formation of the compound is due to the union of 1 at. of indigo-blue with 1 at. of alcohol and 2 ats. of acetic acid, and the elimination of 8 ats. of water, for

$$\underset{\text{Indigo-blue.}}{C_{28}H_{11}NO_4}+8HO=C_{16}H_5NO_2+\underset{\text{Alcohol.}}{C_4H_6O_2}+\underset{\text{Acetic acid.}}{2(C_4H_4O_4)}.$$

D.

This body is formed in abundance during this process, and I think it probable that its composition is always the same, as the following analytical results will tend to show:—

I. 0·3540 grm. of the substance, dried at 100° C., and burnt as usual, gave 0·9360 grm. carbonic acid and 0·1635 grm. water.

0·5630 grm. gave 0·5540 grm. chloride of platinum and ammonium.

II. 0·2325 grm., prepared on another occasion, gave 0·6030 grm. carbonic acid and 0·0985 grm. water.

0·5700 grm. gave 0·5435 grm. chloride of platinum and ammonium.

The formula with which these numbers most closely correspond is $C_{56}H_{24}N_2O_{10}$, which requires the following values:—

	Calculation.		Experiment. I.	Experiment. II.
C_{56}	336	71·79	72·11	70·73
H_{24}	24	5·12	5·13	4·70
N_2	28	5·98	6·18	5·98
O_{10}	80	17·11	16·58	18·59
	468	100·00	100·00	100·00

The deficiency of carbon in the second analysis may be partly due to the extreme difficulty with which the combustion of the substance is effected—a difficulty which is always experienced in the case of such bodies as become charred when heated without previously melting.

If the formula of C, $C_{28}H_{11}NO_4$, be doubled, it will be found to differ from that of D merely by 2 HO less. The formation of both bodies is therefore to be explained in the same manner. The body C may, indeed, be considered as the anhydride of D, the resemblance between the two substances, as regards their appearance and properties, being so great that it is only by their behaviour to caustic alkalies that they can be distinguished.

E.

Of this body I only obtained a quantity sufficient for two analyses, and I must, therefore, leave it doubtful whether its composition is uniform or not.

I. 0·4355 grm. gave 1·1050 grm. carbonic acid and 0·1770 grm. water.

0·5995 grm. gave 0·5740 grm. chloride of platinum and ammonium.

II. 0·4685 grm. gave 1·1890 grm. carbonic acid and 0·1930 grm. water.

0·5615 grm. gave 0·5560 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{28}\,H_{11}\,NO_6$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{28}	168	69·70	69·20	69·21
H_{11}	11	4·56	4·51	4·57
N	14	5·80	6·01	6·21
O_6	48	19·94	20·28	20·01
	241	100·00	100·00	100·00

If this formula be correct, it follows that 2 ats. of indigo-blue combine with 1 at. of alcohol and 5 ats. of acetic acid in order to form, after elimination of 14 ats. of water, 2 ats. of the body E, since

$$2(C_{28}\,H_{11}\,NO_6)+14HO=\underset{\text{Indigo-blue.}}{2(C_{16}\,H_5\,NO_2)}+\underset{\text{Alcohol.}}{C_4\,H_6\,O_2}+\underset{\text{Acetic acid.}}{5(C_4\,H_4\,O_4)}.$$

On comparing the formula of E with that of C, it will be seen that the former merely differs from the latter by 2 ats. oxygen, so that C + 2O = E.

Anthranilic Acid.

Though there could be no doubt, after an examination of the properties of the crystallized acid formed in this process, of its identity with anthranilic acid, still I conceived that its analysis, if not altogether indispensable, might prove of some interest. The results obtained were as follows :—

I. 0·3135 grm., dried at 100° C., gave 0·7035 grm. carbonic acid and 0·1500 grm. water.

0·4050 grm., burnt with soda-lime, gave 0·2890 grm. metallic platinum.

II. 0·1664 grm. gave 0·3720 grm. carbonic acid and 0·0780 grm. water.

0·5590 grm. gave 49 cc. of moist nitrogen at 7° C. and 759·2 millims. pressure, equivalent to 47·25 cc. dry nitrogen at 0° C. and 760 millims. pressure, or 0·0591 grm.

These numbers correspond with the formula $C_{14} H_7 NO_4$, which is that of anthranilic acid, as the following comparison of the composition with that required by theory will show :—

	Calculation.		Experiment. I.	Experiment. II.
C_{14}	84	61·31	61·19	60·97
H_7	7	5·10	5·31	5·20
N	14	10·21	10·13	10·58
O_4	32	23·38	23·37	23·25
	137	100·00	100·00	100·00

Since under ordinary circumstances this acid can only be obtained by the long-continued action of boiling concentrated alkaline lye on indigo-blue, its formation in this process, in which only a small quantity of caustic soda dissolved in a large quantity of alcohol was employed, is remarkable. There can be little doubt that its formation in this case is connected in some way with that of the other substances, and could not be effected by the mere action of a dilute alcoholic solution of caustic alkali on indigo-blue.

The experiments just described suggest a few general remarks on this process and the products to which it gives rise.

1. Though I have no doubt that the products, of the properties and composition of which I have just given an account, are distinct chemical compounds, still it might be objected that some of them were not free from an admixture of products of decomposition derived from alcohol alone, the action of caustic alkali on alcohol being a process not very well understood. In order to satisfy myself on this point, I took an alcoholic solution of caustic soda, boiled it for some time, and then evaporated it in contact with the air. The solution became brown; and on adding water and an excess of acid, after evaporation of the alcohol, I obtained a brown flocculent precipitate, which, being filtered off and washed, was dissolved in alcohol. The solution left, on evaporation, a dark brown resinous residue, which I found to be quite insoluble in ether. That portion of the products obtained in this process which was insoluble in water and ether, but easily soluble in alcohol and alkalies, was therefore certain to contain some of this resinous matter; and I therefore laid the whole of it aside, and gave up all further examination of it. It is certainly true that by the action of alkali on alcohol in closed vessels a totally different product is obtained—a product which differs from the other by its solubility in ether, and its total insolubility in alkalies, and shows a striking resemblance to the body A, which is also soluble in ether and insoluble in alkalies. Still, as my process was conducted in open vessels and not under pressure, I think it is not probable that any of this substance was formed*.

* According to Liebig, the colour which an alcoholic solution of caustic potash assumes in contact with the air is due to aldehyde-resin, the product of decomposition formed by the action of caustic alkalies on aldehyde. Weidenbusch (Annalen der Chemie u. Pharmacie, B. lxvi. S. 153), however, states that the true aldehyde-resin is almost insoluble in alkalies; and in consequence of the discrepancy in the accounts of this body, I requested Mr. A.

2. From what has been stated above, it follows that all the products, except anthranilic acid, are formed by a very simple process, which consists merely in indigo-blue combining with alcohol and acetic acid in various proportions, and yielding compounds in which none of the constituents as such can be detected. It is, therefore, not a process of decomposition, but rather a synthetical process, a building up of complex bodies from others of a simpler constitution. This is proved by the fact of water being eliminated during the process, whereas in all cases in which complex organic substances are decomposed into simpler ones water is absorbed. This elimination of water proceeds so far, that some of the products, notwithstanding that they are formed by the addition to indigo-blue of many atoms of alcohol and acetic acid (bodies having much less carbon and more oxygen), are found to contain even more carbon than indigo-blue itself, a great proportion of the water both of the alcohol and the acetic acid having been separated. Is it not possible that processes of a similar nature may go on within the cells of plants, the chief function of which is known to consist, chemically speaking, in the construction of complex bodies from others of a simpler composition? Is not the power residing in the vegetable cell which enables it to neutralize very potent chemical affinities somewhat of the same nature as that which, in this process, causes the acetic acid to leave the strong base with which

Mylius to make some experiments on the action of caustic alkalies on alcohol in sealed tubes. He obtained by this action a resin of a fine reddish-yellow colour, soluble in ether, but totally insoluble in watery solutions of alkalies. Its properties so nearly resemble those of the true aldehyde-resin, as described by Weidenbusch, and its composition differs so little from that of the latter, that it seems very probable that the two resins may be identical. If so, it follows that aldehyde-resin is certainly formed by the action of caustic alkalies on alcohol, but only under pressure in sealed tubes. The resin formed in open vessels in contact with the air is totally different. For further particulars regarding this peculiar action I must refer to the account of Mr. Mylius's experiments contained in the Proceedings of the Society, February 21st, 1865.

it is combined in order to unite with alcohol and indigo-blue, for which it cannot be supposed to have any strong chemical affinity?

3. The physical properties of these compounds do not seem to depend in any way on those of their constituents. Nevertheless it is to be observed that those containing the largest proportion of alcohol are insoluble in alkalies, whilst those in which the indigo-blue preponderates are the least soluble in alcohol and ether.

4. No law or rule can be detected determining the number of atoms of alcohol and acetic acid which are capable of uniting with the indigo-blue. Were the series more extensive, it is probable that some such law might be found to prevail. It may be remarked, however, that all the products insoluble in water, with one exception, contain either 8 or 10 equivalents of oxygen (assuming the formula of C to be doubled), as will be seen from the following tabular view of their formulæ:—

$$\text{A}\ldots\ldots \begin{cases} C_{62}\,H_{39}\,N\,O_8 \\ C_{80}\,H_{44}\,N_2\,O_{10} \end{cases}$$

$$\text{B}\ldots\ldots \begin{cases} C_{52}\,H_{35}\,N\,O_8 \\ C_{40}\,H_{23}\,N\,O_8 \end{cases}$$

$$\text{C}\ldots\ldots\ldots C_{56}\,H_{22}\,N_2\,O_8$$

$$\text{D}\ldots\ldots\ldots C_{56}\,H_{24}\,N_2\,O_{10}$$

$$\text{E}\ldots\ldots\ldots C_{28}\,H_{11}\,N\,O_6$$

5. Regarding the rational formulæ or probable internal constitution of these compounds I hardly venture to indulge in any speculations. They might be considered as conjugated compounds—compounds of which organic chemistry affords so many examples; and it might consequently be possible to obtain from them, by decomposition, some of the simpler bodies which are known to have entered into their composition. I have, however, been unable to discover any facts in favour of this view. Neither indigo-blue nor any of its products of decomposition can be obtained from them by any means which I have tried. In one experiment

which I made for this purpose, and which consisted in subjecting the body D to the action of caustic soda, I obtained neither anthranilic acid nor acetic acid, as might have been expected. By evaporating the alkaline solution to dryness, and heating the residue to incipient fusion, the substance was partly converted into a black humus-like matter, insoluble not only in water and alcohol, but also in alkalies. The alkali was supersaturated with sulphuric acid, and the liquid was distilled, when a trace of what I suppose to be formic acid passed over. The liquid yielded no anthranilic acid.

In this respect these compounds resemble some of the secondary products which are formed during the decomposition of indican by acids, and from which no indigo-blue can be obtained, though they must be supposed to contain the elements of that body and of various organic acids, such as formic, acetic, and propionic acids. Indeed, the resemblance between the two series of compounds extends also to their physical properties. For instance, the body A resembles indifulvine, one of the products derived from indican, both being brownish-yellow resins insoluble in alkalies. B is very similar to indiretine; and D is so like indifuscine, that the two can hardly be distinguished from one another. There may, in fact, be some analogy in the composition of the two last-named bodies. Indifuscine may, as I have shown on a former occasion, be considered as a compound of indigo-blue and propionic acid minus water, as may be seen by the following equation:—

$$\underset{\text{Indifuscine.}}{C_{44}H_{20}N_2O_{10}} + 2HO = \underset{\text{Indigo-blue.}}{2(C_{16}H_5NO_2)} + \underset{\text{Propionic acid.}}{2(C_6H_6O_4)}.$$

In like manner D may be supposed to contain the same elements combined in a different proportion, since

$$\underset{\text{D.}}{C_{56}H_{24}N_2O_{10}} + 10HO = \underset{\text{Indigo-blue.}}{2(C_{16}H_5NO_2)} + \underset{\text{Propionic acid.}}{4(C_6H_6O_4)}.$$

Analogies such as these, unsupported by experimental proof, may be only fanciful. Nevertheless they may prove of some use in facilitating the classification of facts. At all events, the circumstance of indigo-blue yielding, by the combined action of alcohol, acetic acid, and alkalies, bodies so closely resembling the products obtained along with indigo-blue in the decomposition of indican seems to afford a striking confirmation of the view which I have taken regarding the composition of these products.

There is another point of view from which these bodies may be considered. They may be represented as substitution products of indigo-blue, one or more of the atoms of hydrogen in the latter being replaced by one or more organic radicals. For instance, the body C may be looked upon as the hydrate of a compound, in which one atom of the hydrogen of indigo-blue is replaced by phenyl ($C_{12}H_5$), since

$$C_{28}H_{11}NO_4 = C_{16}H_4(C_{12}H_5)NO_2 + 2HO.$$

In order to obtain some confirmation for this hypothesis I took some of the body D, of which I had a considerable quantity, and which only differs from C by containing more water, and subjected it to the action of hydriodic acid and phosphorus in a sealed tube. By the action of the nascent hydrogen I expected that indigo-blue might possibly be regenerated, but the experiment led only to a negative result; for though the tube was heated in the water-bath for several days, the substance, on its being opened, was found to be almost unchanged, a small part only having been converted into a resinous matter easily soluble in alcohol. A similar negative result was obtained when an amalgam of sodium was employed as a source of hydrogen. After these failures I felt but little encouragement in making further experiments in this direction; and this part of the snbject must, therefore, be left in its present state of obscurity.

6. The occasional disappearance of the indigo-blue in the woad-vat, in consequence of mismanagement, now

admits of an explanation, which will probably be allowed to be the correct one. By the fermentation of the sugar contained in the madder and other materials employed, alcohol is generated, which in its turn may yield some acetic acid; and, alcohol, acetic acid, and a base (lime) being present, nothing further is required for the development of the process above described. By neutralizing a portion of the lime when necessary, the danger of losing colouring-matter is to some extent obviated; but I would venture to suggest, as a means of rendering it still less, the avoiding all materials containing much sugar or starch—substances which might, by their decomposition, lead to the formation of alcohol.

When, in the process above described, formiate of soda is employed instead of acetate of soda, exactly the same phenomena are observed. The indigo-blue gradually disappears, and a dark brown alcoholic liquid is obtained, which is found to contain bodies closely resembling those formed by means of acetate of soda. By operating on a tolerably large quantity of material, I was enabled to ascertain the presence in this liquid of anthranilic acid, and of three products corresponding to, and having the same physical properties as, the bodies B, D, and E. They were separated from one another by the same means as the latter, the first being a brownish-yellow resin, easily soluble in alcohol and ether, as well as in alkalies; the second, a brown powder, soluble in alkalies, but soluble with difficulty in alcohol and ether; whilst the third was a reddish-brown powder, distinguished by its solubility in a boiling solution of acetate of soda—a property which afforded a ready means of separating it from the others. No compounds insoluble in alkalies, and corresponding to the bodies A and C, were formed with formiate of soda. The analysis of the compound resembling B yielded the following results:—

0·2960 grm. gave 0·7565 grm. carbonic acid and 0·1920 grm. water.

0·3615 grm. gave 0·1915 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{48} H_{29} NO_{10}$, which requires

	Calculation.		Experiment.
C_{48}	288	70·07	69·70
H_{29}	29	7·05	7·20
N	14	3·40	3·32
O_{10}	80	19·48	19·78
	411	100·00	100·00

The substance, it will be seen, is formed by the union of 6 ats. of alcohol, 4 ats. of formic acid, and 1 at. of indigo-blue, accompanied by the separation of 20 ats. of water, since

$$C_{48} H_{29} NO_{10} + 20\,HO = \underset{\text{Indigo-blue.}}{C_{16} H_5 NO_2} + 6(\underset{\text{Alcohol.}}{C_4 H_6 O_2}) + 4(\underset{\text{Formic acid.}}{C_2 H_2 O_4}).$$

The composition of the substance corresponding to E was also determined, the results being as follows:—

0·1960 grm. gave 0·4500 grm. carbonic acid and 0·0820 grm. water.

0·3635 grm. gave 0·3250 grm. chloride of platinum and ammonium.

These numbers correspond with the formula $C_{26} H_{11} NO_8$; but the only formula which can lead to an explanation of the manner in which the substance is formed, though it gives a calculated composition not agreeing quite so well with the experiment, is $C_{26} H_{13} NO_8$, which requires

	Calculation.		Experiment.
C_{26}	156	63·15	62·61
H_{13}	13	5·26	4·64
N	14	5·66	5·61
O_8	64	25·93	27·14
	247	100·00	100·00

Assuming this to be the correct formula, its formation would take place in accordance with the following equation:—

$$\underset{\text{Indigo-blue.}}{2(C_{26}H_{13}NO_8)+10HO=2(C_{16}H_5NO_2)}+\underset{\text{Alcohol.}}{3(C_4H_6O_2)}+\underset{\text{Formic acid.}}{4(C_2H_2O_4)}.$$

It will be seen that the same law regarding the number of atoms of oxygen prevails here as in the case of the bodies before described, this number being either 8 or 10.

If in this process ordinary alcohol is replaced by methylic alcohol, the same effect is produced, provided acetate of soda is employed; but a mixture of methylic alcohol, formiate of soda, and caustic soda does not act in the same manner on indigo-blue, which remains unchanged, however long it may be left in contact with the boiling liquid. It appears, therefore, that one of the two agents, ethylic alcohol or acetic acid, is quite essential. One of the two may be replaced by an homologous body; but when both are so replaced, the indigo-blue remains intact.

IV. *On some Physiological Effects of Carbonic Acid and Ventilation.* By R. Angus Smith, Ph.D., F.R.S., F.C.S., President of the Society.

Read January 24th, 1865.

In a report on the air of mines and confined places, there was given a chapter on the action of the pulse when carbonic acid accumulated in the air. It is proposed to repeat that chapter, and to supplement it with additional experiments. The experiments, when not otherwise explained, were made in an air-tight lead chamber described in the report alluded to. It may be well first to show the amount of carbonic acid exhaled. This will be done by giving the amount per cent. in the air of the chamber. This experiment was the beginning of the inquiry. I expected that the amount of carbonic acid exhaled would diminish, and with it the amount of strength in the muscles; but these points could not be reached by the methods employed. The amount of oxygen used is for the time the same, although there is less in the atmosphere. I shall not pretend to say how the health is affected further than this, that a change is observed in the respiration and the pulse. I must leave physiologists to tell what mischief this will ultimately cause; but I cannot doubt that the circulation is diminished, and that the lungs endeavour to compensate for this by more rapid action. How much each person can bear of this change will depend on circumstances which, it appears to me, physiologists cannot estimate.

Table B.—*One Person in the close Chamber.*

	Carbonic acid. 1st day.	Carbonic acid. 2nd day.
After 20 minutes	0·18 per cent.	0·19 per cent.
„ 40 „	0·32 „	0·36 „
„ 1 hour	0·49 „	0·49 „
„ 80 minutes	0·62 „	0·64 „
„ 100 „	0·74 „	0·75 „
„ 2 hours	0·88 „	0·89 „
„ 140 minutes	1·06 „	1·03 „
„ 160 „	1·20 „	1·22 „
„ 3 hours	1·25 „	1·34 „
„ 200 minutes	1·52 „	1·48 „
„ 220 „	1·54 „	1·60 „
„ 4 hours	... „	1·81 „
„ 260 minutes	... „	1·98 „
„ 280 „	... „	2·10 „
„ 5 hours	... „	2·25 „

I have not had time to attend to the full explanation of each experiment, and some require a continuation of the inquiry; but this last must not be passed over without special notice. The amount breathed every hour is the same—no matter whether there be 0·04 or 2 per cent. of carbonic acid in the air, and no matter although there be 20·94 or only 18·8 of oxygen. This is strongly corroborative of the views taken by Liebig, but other circumstances tend on the other hand to show that this state of things is kept up at the sacrifice of the comfort, to say the least, of other vital functions. There must of course be a limit, but I was afraid to go farther than I went. In one experiment the breathing was changed from 16 inspirations per minute to 22, the pulse fell from 76 to 55, whilst it was so weak that it was difficult to find. My assistant was in the chamber this time; I requested him to attend to his pulse and breathing, as on another occasion when there was still more carbonic acid in the air, namely, 3·9 per cent., my breathing rose up to 26 inspirations, and my pulse became so weak as to cause alarm. This has happened so regularly that it must be put down as the result of poisoning with carbonic acid. On one occasion there was a comparatively large amount of oxygen in the room, viz. 20·1. The carbonic acid had been driven in upon fresh air, and no oxygen removed. Even here the pulse was weak, although the breathing was not very difficult, and the candles burnt moderately well.

The conclusion is, that in the air containing an increased amount of carbonic acid, this gas alone, even without the other hurtful ingredients, such as organic matter, begins to poison, and men exposed to it are really gasping for breath without knowing it. All the other hurtful conditions contribute their powerful aid.

As I came on this result at the end of the inquiry into the composition of the air of mines, it is not easy to do it justice. We learn much from it. We learn that the blood can take its oxygen out of very impure mixtures; but we learn also that some functions are meantime suffering greatly. It is, to my view, a most important thing to show that with an amount of oxygen not less than is found in the air of some mines, and an amount of carbonic acid actually less, such extraordinary changes should result in the functions of a healthy man. We want no other experiment than this to prove great evil arising from impure air, either in mines or elsewhere.

In order to obtain similar results in a shorter time, five persons entered the lead chamber, expecting to have in one hour the same results that were obtained by one person in five hours. The figures are here given ; it is seen that they are not exactly the same as previously. Time causes us to yield, although we may struggle against the evil influences for an hour or so. The effects are not exactly such as were expected. The pulse begins to be irregular very soon, and certainly when the air contains 0·4 per cent. of carbonic acid, in three cases 0·2. It rises and falls, but at last begins to fall. In all cases, however, it becomes very weak, as in the first experiment.

With the younger it rose rapidly at first, and seems to indicate the more rapid struggle for life with the more advanced; it was a steady determination not to be changed by external circumstances, although they gradually caused a change at last.

These figures will probably induce many others to continue the inquiry.

May it not be useful to lower the pulse in this method in some cases? If so, must the experiment be tried with pure carbonic acid? And how much was due to the carbonic acid, and how much to organic matter? All these are interesting questions. Meantime the question is so far answered, that we see the effects due to the want of ventilation.

TABLE C.—*Beats of the Pulse. Five Persons in the Chamber. Observations every 5 minutes.*

	A.	B.	C.	D.	E.	Temp.
To begin	60	78	84	70	73	68° F.
After 5 minutes	60	70	90	70	72	
" 10 "	59	76	90	75	72	
" 15 "	72	74	91	74	70	
" 20 "	70	74	89	74	72	
" 25 "	79	77	91	71	74	
" 30 "	74	81	89	70	71	
" 35 "	78	79	87	74	68	72° F.
" 40 "	73	76	89	76	70	
" 45 "	70	70	90	73	72	
" 50 "	74	72	89	72	71	
" 55 "	70	73	89	72	70	
" 60 "	66	73	88	73	72	
" 65 "	66	74	88	72	70	
" 70 "	69	73	86	71	69	
" 75 "	70	70	85	71	70	
" 80 "	73	70	86	70	69	
" coming out 5 minutes	66	68	89	68	68	
" 3 hours	63	74	84	74	73	
" "	61	75	85	74	73	

Number of Respirations.

	A.	B.	C.	D.	E.
Normal	20	15½	22	20	20
After 33 minutes	24	16½	25	20	25
" 58 "	23	17	25	22	24½
On coming out after 5 minutes	20	16	23	19	21

After 5 minutes.

Organic matter not pleasant.

After 15 minutes.

A. Pulse stronger and quicker.
B. Irregular pulse, but strong.
C. Weaker, and already difficult to feel.
D. Same to the feeling.
E. Much weaker.

After 25 minutes.

A. Stronger.
B. Irregular.
C. Irregular and weak.
D. Irregular.

After 45 minutes.

Organic matter less sensible than at first to the majority.

D feels air to be bad.

After 50 minutes.

A can scarcely feel his pulse; several attempts made to count it. Still feels quite well.

B begins to fell head uneasy.

C feels his heart beat more than usual.

D. Pulse weak.

E. Pulse very weak.

Here every one was observed to be sighing, although all were cheerful.

After 6 minutes.

B. Flushed.

C and D. Headache began slightly.

The effect of company was considerable in preventing the lowering of the pulse by keeping the mind cheerful.

But the experiment, Table F, shows that even when quite alone the pulse did not lower when the air was pure.

This experiment differs from that of Table D. The impure air was formed five times more rapidly, and the results were not so perceptible. It would appear that we can resist for a short time when we cannot resist for a long time.

The irregularity of most of the pulses is apparent.

A was the youngest, being about 17, and having a naturally low pulse; his was raised.

B was about 21 years old; his pulse went lower, then higher, then finally lower.

C, about 24; his pulse went higher, then sank to nearly its usual point, but he was the most affected in sensation.

D, 27; his pulse went higher and then lower.

E, 47; his pulse went lower, higher, and lower, but he felt no discomfort; forehead began slightly to warm.

It is remarkable that the breathing increased in all cases, and that it went back to its normal amount very rapidly.

TABLE D.—*One Person in the Lead Chamber. Respiration and Beats of the Pulse taken every* 10 *minutes.*

Time.	Pulse.	Respiration.	Temperature, Celsius.	Carbonic acid in the same periods.
h. m. 10 55	73	15·5	18°·2	0·04
min. After 10	73	16	18°·2	0·114
„ 20	72	16	18·2	0·187
„ 30	71	17	18·4	0·261
„ 40	71	16	18·4	0·335
„ 50	70	16	18·5	0·408
„ 60	68	16	18·6	0·482
„ 70	67	16·5	18·7	0·556
„ 80	67	17	18·8	0·629
„ 90	66	17	18·9	0·703
„ 100	65	18	19·0	0·777
„ 110	65	18·5	19·0	0·850
„ 120	64	19	19·0	0·924
„ 130	63	19	19·2	0·997
„ 140	62	19·5	19·1	1·071
„ 150	62	20	19·1	1·145
„ 160	62	20	19·1	1·218
„ 170	61	20	19·1	1·292
„ 180	60	21	19·1	1·366
„ 190	60	22	19·2	1·439
„ 200	59	23	19·2	1·513
„ 210	58	24	19·4	1·587
„ 220	57	24	19·4	1·661
„ 230	57	24	19·4	1·734

TABLE E.—*When the door was opened.*

Time.	Pulse.	Respiration.
After 10 minutes	59	22
„ 20 „	59	19·5
„ 30 „	60	19
„ 40 „	60	18
„ 50 „	60	17

TABLE F.—*Sitting quiet for an hour in the Lead Chamber in pure air.*

Time.	Pulse.	Respiration.
$4^h\ 50^m$	75	17
After 10 minutes	76	17
" 20 "	76	17
" 30 "	76	17
" 40 "	77	17
" 50 "	76	17
" 60 "	76	17

From this we learn that the same quiet condition in pure air produced no change.

Experiments B, C, and D, on the beats of the pulse, seem decisive. The air affects the pulse when the ventilation is such that the amount of carbonic acid reaches 0·18. The question of carbonic acid and organic matter, viz., which is the most hurtful, must be decided by other experiments. My belief is that much is due to the carbonic acid, because the progress of the pulse downwards is so regular, and I believe that the organic matter does not increase so regularly. This may not be true at the temperature given, and is another point to be ascertained.

But leaving out all the details, the great broad fact remains that carbonic acid and other emanations from the person diminish the circulation, and hasten the respiration, and that the effect is perceptible when the per-centage of carbonic acid reaches 0·18, or say one-fifth of a per cent. certainly. If, however, we do not wish to infer too much from one beat of the pulse, let us, for rough practice, say ¼ per cent.

EFFECT ON THE PULSE AND BREATHING

Artificial carbonic acid being inhaled along with the organic exhalations of the body.

1 *per cent. of Carbonic Acid.*

	Pulsations.
	68.
After 5 minutes	68.
„ 12 to	68, 70, 70, 70, 69, 70.
„ 22 to	70, 70.
„ 30 to	68, 68, 66.
„ 34 to	65, 65, 66, 66, 66.
„ 42 to	67, 68.
„ 51 to	66, 66.
„ 60	64, 63, 63, 63, 63, 63, 63.

2 *per cent. Carbonic Acid.*

	Pulse.	Inspirations.
		18
After 5 minutes	64	19
„ 10 „	66	19
„ 15 „	65	20
„ 20 „	64	20
„ 25 „	63	21, pulse very weak.
„ 30 „	62	21
„ 35 „	63	21
„ 40 „	64	21¼
„ 45 „	63	22
„ 50 „	62	22½
„ 55 „	60	23
„ 60 „	61	23
„ 65 „	60	23¼
„ 70 „	60	23¼
2 minutes after coming out		68.

Here the pulse was very much affected even in the number of beats, but the effect was observed principally in its great weakness: it sometimes tried to recover its number, but this was not observed to take place with regard to the strength.

3 *per cent. Carbonic Acid.*

	Pulse.	Inspirations.	
	67	17	
After 10 minutes	67	21	Acidity perceptible
„ 15 „	65	21	to the smell.
„ 20 „	63	22½	
„ 27 „	62	23	

Here the pulse became so weak that it was difficult to count the beats. There was also a very unpleasant feeling. The door was opened, and two other young men entered. Of course a good deal of carbonic acid was removed, but not more than from ¼ to ½ per cent. In ten minutes the pulse of the eldest, B., fell from

79	Inspirations rose from 18
to 75	to 22
Unpleasantness felt.	

Here, as in the experiment recorded previously, the pulse of A. rose.

At first it was	63	Inspirations	21
It rose to	69	rose to	25

A.'s pulse very feeble. There is always a slight rise at the beginning. This rise was very decided in the case of A. It always results in a fall, and would no doubt have done so in this case had A. remained longer. This, however, would not have been safe, as, even in these two minutes, his pulse was almost imperceptible, and he could not count it himself.

In the above cases the persons who breathed sat in the lead chamber, and of course the organic matter from their bodies escaped into the air around them. Still we know that the organic matter would not produce these effects without the carbonic acid, simply because when we remain in the chamber much longer without pouring in carbonic acid the pulse does not become so weak, whilst the organic matter is of course accumulated to an extent much greater than it could have been with artificial carbonic acid.

Whilst I gave abundant credit to the organic matter for doing evil, I could not refuse to blame the carbonic acid; but as a friend was still dissatisfied with the argument if applied to smaller amounts of carbonic acid, the following experiments were made. In them the organic matter is entirely excluded. For the first, in which 1 per cent. of carbonic acid was mixed with the air, several aspirators of flexible material were filled with the mixture, and the air was inhaled from the aspirators by the mouth, whilst it was exhaled from the nostrils. The carbonic acid was made from bicarbonate of soda, and passed through a solution of bicarbonate of soda to remove mineral acids.

With 1 per cent. of Carbonic Acid.

	Pulse.		Pulse.
	66	After 14 minutes	66
After 2 minutes	67	,, 16 ,,	64
,, 4 ,,	67	,, 18 ,,	64
,, 6 ,,	68	,, 20 ,,	64
,, 8 ,,	67	,, 22 ,,	63
,, 10 ,,	68	,, 26 ,,	63
,, 12 ,,	67		

In this experiment the difficulty of supplying air was felt to be considerable; and the aspirations having become less agreeable and regular, they were not counted.

In order to remove all difficulty, the lead chamber was charged with the mixture to be breathed, and the operator sat outside inhaling the air through a wide tube with ease. Of course a similar amount of air entered, and this was supplied through some small chinks, which were not carefully filled up. The change taking place in the air of the chamber from this latter cause only would scarcely be perceptible in half an hour, and then it would be against the success of the experiment. The uniformity of results is therefore very remarkable.

With 0·5 *per cent. Carbonic Acid.*

		Pulse.	Inspirations.
		76	17
After	5 minutes	76	
,,	10 ,,	76	
,,	15 ,,	75	20
,,	20 ,,	73	
,,	25 ,,	71	
,,	30 ,,	71	22
,,	35 ,,	71	
,,	40 ,,	71	24

With 0·25 *per cent. of Carbonic Acid.*

		Pulse.	Inspirations.
		70	17
After	5 minutes	72	
,,	10 ,,	73	
,,	15 ,,	72	19
,,	20 ,,	70	
,,	25 ,,	69	
,,	30 ,,	69	21

Here a disturbance is seen at once, more fully in the breathing. Owing to the mode of draining the reservoir of air breathed, it would not have been fair to proceed further.

With 0·1 *per cent. of Carbonic Acid.*

		Pulse.	Inspirations.
		73	18
After	5 minutes	72	
,,	10 ,,	73	
,,	15 ,,	73	19
,,	20 ,,	72	
,,	25 ,,	71	
,,	30 ,,	72	
,,	35 ,,	73	19
,,	40 ,,	73	
,,	45 ,,	72	19
	Average	72·4	18·75

Here there is a disturbance perceptible of two on the pulse; and I may say that the experiment is scarcely fair after 25 minutes. The disturbance on the inspirations is more uniform. It is, for example, more perceptible than in the next case.

Pure air was breathed in the same position as in the previous cases, D. sitting outside the lead chamber, which had been well ventilated. This experiment was made in order to ascertain the influence of breathing through a tube, as it was feared lest some mechanical difficulties might have interfered with the value of the operations. The result shows that no such difficulties occurred. There is a little diversity of one above and one below the average of the pulse, and the breathing is a little lower in one case, instead of being resolutely higher as in every other case given, even when so little as one-tenth of a per cent. of carbonic acid was used.

Ordinary Atmosphere in the Lead Chamber; breathing through the tubes as before.

	Pulse.	Inspirations.
	74	18
After 5 minutes	74	
„ 10 „	75	
„ 15 „	74	18
„ 20 „	74	
„ 25 „	75	
„ 30 „	74	18
„ 35 „	74	
„ 40 „	74	
„ 45 „	73	17
„ 50 „	74	
„ 55 „	74	
„ 60 „	75	18
Average	74·1	17·8

In a report on the air of mines I discussed questions relating to the absorption of oxygen and poisoning by carbonic acid, quoting several opinions of eminent chemists. The important point is this: How can the blood be influenced by a diminution in the amount of oxygen to the extent of 0·1 or even 1 per cent? Liebig says, "In a closed space 8 feet long, 9 feet high, and 8 wide a man could not breathe 24 hours without uneasiness." This is equal to living about 5 hours and a half in my lead chamber; and in that time, by sitting quietly, we may avoid uneasiness; but the air will be very bad, candles will scarcely burn, some will go out, and any person entering suddenly will be very unwell. The sensations are gradually affected, and nothing striking is observed; the senses are diminishing in power. If we look at the important total acts, the circulation of the blood and the respiration, we find that death has begun, so to speak, and the life is going out as quietly as the candle.

Nearly all the usual experiments on breathing in impure air have been made violently and not with small amounts of impurity, and during a very long interval of time; a rabbit has been killed in a few minutes, and the same air has been breathed until it has attained its maximum impurity. Liebig says, "Lavoisier and Seguin found that the carbonic acid of respired air, when again inspired, may be raised to 10 per cent., but not beyond that amount, even when respiration was continued, which it could be only for a very short time. This proportion of carbonic acid may be regarded as the limit at which life is endangered in man."

We can scarcely look on this experiment as sufficient. I became distinctly faint in 4 per cent. of carbonic acid, the others around me were very uncomfortable in even less; one fainted in 2 per cent., which did not affect my senses. We can, when in very good condition, bear 4 per cent. for a quarter of an hour at least, so that life is not endangered suddenly; but I am disposed to think that no one could live long in such air. When hours are spoken of, the danger to life of any amount less than this is not immediate when the person is healthy. The constant lowering of the pulse, even in much less impure air, must have a gradual effect on the vitality; which effect will be seen in some persons in a few hours, in some after days, and in others perhaps years. It is probable that to live during the whole 24 hours of the day in any air containing above 1 per cent. would bring results on the health very rapidly; but no men are exposed to this, so far as I know; the usual exposure is only for three or four hours, seldom during the whole working time; and even with this the pulse is kept permanently low, as will be seen in Dr. Peacock's report.

Now comes the question, If the oxygen of the air is taken up by the blood by chemical affinity, why should the presence of carbonic acid affect it, and therefore why should it be a matter of importance whether the amount of oxygen be small or great?

1st. The absorption cannot be wholly chemical; it must, to some extent, follow the physical laws of absorption, if we may so call them. In this case the amount absorbed will be in proportion to the bulk of the two gases presented to the liquid. The smallest increase of either gas will make a difference. I entered on this more fully in a former paper.

2nd. If the absorption is purely chemical, knowing as we do that the work of absorption must be done rapidly, the amount absorbed must still depend on the amount presented.

3rd. In either case it will require a certain amount of oxygen to drive out the carbonic acid.

If blood contains 10 per cent. of oxygen and 5 of carbonic acid, add one per cent., or one-tenth, or one-hundredth per cent. of oxygen more, and a certain amount of carbonic acid will be removed.

Viewing blood as a liquid like water, this would be the case, I suppose, if we gave it time. Viewing it as a chemical solution, it would be still more the case. If we add oxygen to protocarbonate of iron in water, the carbonic acid is driven out in proportion to the rapidity with which the oxygen is absorbed, and of course the oxygen is absorbed with greater rapidity if the liquid contains less carbonic acid.

If, again, we view the blood and the membranes rather as porous bodies, we have the question still more clearly answered; and there are reasons why we should believe the action somewhat to resemble the action of these bodies. Whenever charcoal, a porous body, is filled with one gas and is put into another, a certain amount of the first is driven out with great force; the result is not a mere mixture taking place quietly, but an instant forcible diffusive and absorbent action. If we view the carbonic acid as driven out by the oxygen, taking any of the three views, the actual amount of the gases present must be of the greatest importance.

The amount of carbonic acid in the lungs is always considerable. If the air inspired has more or less oxygen, the proportions are first changed in the lungs, then the act of absorption takes place, when the proportions must again be changed. We must remember that we breathe every three seconds, so that the change in the lungs is made rapidly; and the absorption will also be rapid, although the chemical changes taking place in the blood may be slower.

I must be careful in speaking of such subjects; but I trust I do not go farther than is legitimate for a chemist.

If we consider the effect of even one beat of the heart in a minute in a mechanical point of view, we need not be surprised at a change of result in the health. If the amount of blood sent by the heart is three ounces, we have, for every beat of the pulse lost per minute, a diminished circulation of many gallons of blood per day; for every beat less, the corresponding amount is taken from the circulation. But even this is not the whole difference because the beats become excessively feeble. The blood seemed to require to remain in the lungs rather longer, in order to obtain its oxygen, whilst the breathing supplied air more rapidly; so that in some cases there were found to be an addition of nearly one-half the number of inspirations. In one case especially the pulse was raised, not sunk; and in most cases it was raised a little for a short time at first, as if an inferior blood were endeavouring to do equal work by moving more rapidly.

Medical men have objected to the argument that any evil result can arise from these effects, saying that man is formed so as to resist such influences, and is not so weak as to be confined within such small limits. When the ground gives way under a man he cannot resist, he can generate no force contrary to gravitation, except a few movements or leaps from the ground itself if the sinking is not too rapid. When the heart ceases beating man cannot resist, as he needs the beating heart itself to generate his power. If the heart is feeble, he may breathe fast to supply it rapidly with oxidized blood, and to a certain extent succeeds; but he must take this compensation force from some place. I cannot pretend to give an opinion on the result of an unnatural slowness of pulse, and an unnatural rapidity of breathing; but that they are evil omens is true, or we have long been deceived. In mines and such places the evil is exaggerated, because the exertion required to climb the ladders leads to an increased activity of the heart.

If the gas by which the oxygen in air is diluted were insoluble, the result might be very different, and we might probably remain in air with less than 10 per cent. of oxygen. In one condition, namely, in high regions, something similar to this occurs; the amount of oxygen is diminished by rarefaction. But even if no rarefaction took place, we could breathe in air having much less oxygen than in the worst metal mines we know, if the carbonic acid was removed. This Liebig states, and so far I have proved it, that by removing the carbonic acid by lime from air in which breathing was uncomfortable, the whole seemed quite fresh; candles also burned better. This I have elsewhere described. Nitrogen and some other gases, marsh gas for example, not uniting chemically, and not being altered to a great extent mechanically, but above all not being driven out from any compound in the blood, either by the addition or otherwise of oxygen, do not produce effects so violent as carbonic acid.

Whatever the explanation be, my conclusion from the experiments is, that the smallest diminution of oxygen in the air breathed affects animal life, if its place is supplied with carbonic acid.

V. *Further Observations on the Permian and Triassic Strata of Lancashire.*

By E. W. Binney, F.R.S., F.G.S.

Read March 21st, 1865.

Introductory Remarks.

In previous memoirs, published in the Transactions of the Society,* I have given what information I possessed in a fragmentary state, just as I obtained it, of the Permian strata of Lancashire and the north-western counties of

* Transactions of the Manchester Literary and Philosophical Society, vol. xii. (2nd series), vol. xiii. (2nd series), vol. ii. (3rd series), vol. iii. (3rd series).

Westmoreland, Cumberland, and Dumfries, as well as the north-western corner of Yorkshire. I took sections where I was fortunate enough to obtain them; but I made no attempt to lay the strata down continuously on a map, my materials being far from sufficient for such a purpose.

By looking at a geological map of the county of Lancaster, the observer will find a great gap between the Permian beds of Grimshaw Delph, Bradley Brook, and Skillaw Clough, to the north-west of Wigan, and the sections described by me at Rougham Point near Cartmel, and Stank near Ulverston. The lower Coal-Measures from Harrock Hill can be traced pretty well towards Chorley, and thence to near Withnell; and then the millstone grit runs to Hoghton, through Salmesbury and Alston, and across the country, not very well seen, to Griesdale, Scorton, Cleveley, Ellel, Ashton, near Abbey Lighthouse on the Lune, over the mouth of that river to Robshaw point, and on to Heysham. The country forming the western boundary of the above line is a low district, a good deal covered up with drift, and affording few natural sections to show clearly the relation of the carboniferous to the Permian strata. The district probably may afford some sections if carefully investigated; but up to this time it has been quietly dismissed by colouring it red for Trias.

In this communication I intend to give a little more information, which I have lately obtained in a line from Hoghton Tower to Fleetwood, at Roach Bridge, Salmesbury, and Alston, also at Cockersand Abbey, south of the mouth of the Lune, and Robshaw Point and Heysham to the north of the same river; having first made a few remarks on some singular red sandstones, hitherto classed as Trias, in the neighbourhood of Whiston and Rainford, near St. Helen's, and laid down as such on the maps of the Geological Survey, as well as a soft red sandstone, classed as Lower Permian by Mr. Hull, near Manchester.

On the Hard and Soft Sandstones of the Knowsley, Whiston, St. Helen's, and Manchester Districts.

No doubt it is a very difficult matter to determine with absolute certainty where the Trias strata end and the Permian begin when there are no organic remains to guide us, and we have to trust to a bed of red marl or a deposit of red sandstone. In my several memoirs published on this subject, so far as South Lancashire was concerned, the red marls and limestones of Newtown and Bedford are assumed to be the uppermost Permian deposits found. It is quite true, as stated in my third memoir, "Some of the sections near Manchester, especially that seen in the valley of the Irk, in Cheetham and Newtown, would apparently show that the red marls containing limestones and fossils of the genera *Bakevellia, Schizodus,* &c., passed into the overlying Trias;" but as a whole it was assumed from other facts that the red sandstone of the Trias was unconformable to the underlying permian beds. In a paper published by Sir R. I. Murchison and Professor Harkness, printed in the 'Quarterly Journal of the Geological Society' for May 1864, as well as in my last memoir, the thick red sandstones of St. Bees are described as Permian and not as Trias, and were traced down, as Professor Sedgwick had previously followed them, into Furness, near Hawcoat and Barrow. Anyone who sees the red sandstones, much used for building purposes at Shawk, Maryport, and St. Bees, and compares them with that at Hawcoat, will not be able to distinguish the one from the others. It is only from their physical characters that we can compare these sandstones; for up to this time, so far as my knowledge extends, no organic remains have been met with in them. Now, this is bringing a Permian red sandstone above the Newtown and Bedford red marls and limestones, and introduces, for the first time, an Upper Permian sandstone into Lancashire; and this rock runs into the Trias so regularly that it will be very difficult to separate it by any well marked boundary from the lower soft sandstone or pebble-beds of the Trias, as laid down and described in the maps and memoirs of the Geological Survey.

It is pretty clear, if some of these Permian and Triassic sandstones are to be classed by their physical characters alone, that certain of the latter rocks, as laid down by the Geological Survey in the Huyton, Croxteth, and Knowsley districts, will probably have to be put into the Permian, for no one can tell the red flaggy sandstone of Knowsley Quarry from the Hawcoat and St. Bees sandstones, and it must be taken as Permian, just as the Hawcoat rock is identified with that at St. Bees.

The Triassic beds in South Lancashire, as seen near Liverpool, according to Mr. Hull, are as follows* :—

Formation.	Division.	Subdivision.
New Red Sandstone	Keuper	1. Red Marl, with beds of Upper Keuper Sandstone.
		2. Lower Keuper Sandstone, or Water-stone, with a Base of Breccia, or Conglomerate.
	Bunter	1. Upper Red and Mottled Sandstone.
		2. Pebble-beds.
		3. Lower Red and Mottled Sandstone.

Next, as seen near Manchester, where the same author classes the bunter as composed of

1. Upper red and mottled sandstone.
2. Pebble-beds.

It will be seen from the above classification that the lower soft red mottled sandstone of Liverpool is left out at Manchester altogether, the lowest member of the Trias there being the pebble-beds. There certainly is the Collyhurst or Vauxhall sandstone, which would pass very well for the lower soft red; but the Newtown fossils found above it

* Manchester Geological Society's Transactions, vol. ii. p. 23.

clearly cut off that rock from the Trias, and establish it with the Permian beyond all question.

I have not made any division of the bunter portions of the Trias. No doubt they are useful in different places, and have sometimes to be varied with the districts to which they are applied. In the north, about Carlisle, up to this time only one bed of soft red sandstone without pebbles has come under my notice. But at Sutton, as previously alluded to, there is a soft red sandstone without pebbles resting on Permian red marls, which cover a conglomerate lying on Permian red sandstone. Similar sandstones, in the same position, are seen near the canal at Bedford, below Messrs. Hampson and Co.'s Print-works at Clayton Bridge, Manchester, and near Messrs. Brocklehurst's Lime-works at Ardwick, near Manchester. There is also a soft red sandstone, apparently dipping, under the pebble-beds of Heaton Mersey, near Stockport, well seen on the banks of the Mersey from Stockport to Fogg Brook, which would well pass for the lower soft sandstone of the Trias; but for some reason with which I am unacquainted, the gentlemen connected with the survey prefer (I am informed) to class this sandstone underlying the pebble beds with the Permian rather than the Trias.

It appears that throughout the western part of Cheshire and the adjoining county of Flint, as well as in West Lancashire, where there are few, if any, Permian strata exposed, the Geological Survey has always had a lower soft red and mottled sandstone; but when the east part of Lancashire is reached, and undoubted Permian beds found, this supposed lowest member of the Trias disappears.*

* It was once suggested by me that it was possible the permian red marls and limestones might have thinned out to the west, and thus caused some difficulty in identifying this soft sandstone, which is hard to distinguish from the Collyhurst sandstones, but such a solution was not entertained by Mr. Hull. Manchester Geological Society's Transactions, vol. ii. p. 33.

The soft yellow and variegated sandstones of Whiston, Croxteth Park, and Huyton, all resting unconformably on coal-measures, described by Mr. Hull, are evidently of the same age as the Rainford and Grimshaw Delph beds. Unfortunately in no instance have any red marls been yet found lying either above or below them. I have described the two latter as Permian, whilst Mr. Hull thinks they are the lower red and mottled sandstone of the Trias. But with respect to the Knowsley Quarry, it so much resembles the St. Bees and Hawcoat Upper Permian sandstones that, if they were found in Furness, Sir R. I. Murchison and Professor Harkness would, without doubt, claim them as Permian.

It is many years since I first saw the Knowsley Quarry, and I then in my note-book remarked that these sandstones, especially that belonging to Mr. Littler, could not be distinguished from the Upper Permian sandstones of the neighbourhood of Dumfries, which I had just returned from examining. Now, if they can be proved to immediately overlie the coarse-grained, false-bedded, soft red and mottled sandstones of Whiston and Croxteth Park, both the latter as well as the former will have to be classed as Permian rocks, according to the present geological nomenclature of the north-west of England; and I think that the new sections I now describe at Roach Bridge, Cockersand Abbey, and Robshaw Point tend to confirm this view.

In all the quarries of Lancashire where the Trias sandstones have been wrought, I have never seen so hard and thin-bedded a stone as that found at Knowsley. It was formerly used for paving-sets in Liverpool; and large quantities of it were broken for road-metal purposes, for which I have seldom known a Trias rock used. Some of its beds also afforded fine-grained flags, with faces as smooth as any Permian sandstone I have ever seen in the neighbourhood of Dumfries. A good example of pebble-beds is seen at Kirkby Rough; but this rock bears no resemblance to the stone at Knowsley Quarry, and the two stones cannot well be classed as the same from their characters.

On the Soft Red Sandstone at Ardwick.

Mr. Hull, in his memoir previously quoted, and the map accompanying it, brings in by a fault a piece of Permian sandstone between the Lime Works and Ardwick Bridge, Manchester. When Mr. Mellor some years since kindly took me down the pit and showed me this sandstone, it certainly did appear very much like the Permian soft red sand of Collyhurst; but I saw no evidence of any fault, further than that it rested on the eroded beds of the upper coal-measures of Ardwick, and was of course unconformable to them. In my last memoir I described this sandstone as Trias, and I see no reason for altering my opinion. I had seen the rock some years before near the weir above Ardwick Bridge, and traced it to the latter place. On the dip it has been bored through at Mr. Buchan's, Messrs. Gallimore's, Hoyle and Son's, and Leese and Co.'s Works, and the red marls and limestones found under it. In position, it occupies the place of Mr. Hull's soft and mottled sandstone; and its characters and position warrant its being classed as that rock, rather than with the Vauxhall sandstone, so far as I have been able to ascertain.

General Description of the District North of Preston.

As it is desirable to attempt to connect the Permian deposits of the south and west of Lancashire with those in the north of the county, as seen at Rougham Point, near Cartmel, and Stank, near Furness Abbey, I give the result of some of my late examinations. The only section hitherto seen by me near Preston is one in the Ribble, below that town, and appears to be a portion of the pebble-beds of the Trias. It extends up the valley of the Darwen to the weir above Bannister Hall, where the soft and variegated sandstones, apparently pebble-beds, are seen; and, after a distance of one-third of a mile, soft yellow variegated and red sandstones, at the base of which a conglomerate (Permian) rests unconformably on what appears to be limestone-shale.

The Ribble, between Walton and Lower Brockholes Bridge, does not afford any evidence, so far as I saw, of the underlying strata until we reach the latter place, where a soft red sandstone, apparently the pebble-beds of the Trias, makes it appearance, and is seen all the way past Samlesbury Chapel and Lower Hall to near Barton's Boat, where it rests unconformably on Lower Carboniferous strata.

Near Cockersand Abbey, on the south side of the mouth of the Lune, west of the town of Lancaster, below high-water mark, is a small patch of what appears to be Permian sandstone.

To the north of the last-named place, across the Lune, at Robshaw Point, the same soft red sandstone makes its appearance on the beach covered by the tide, and appears to be a continuation of that rock seen to the south, but much better exposed. With these exceptions, no further evidence has yet been obtained of any Permian beds until we reach Rougham Point. From this last-named place to Stank is a portion of Morecambe Bay, and a low sandy district, of which little or nothing is known. At the old magnesian-limestone quarry at Holebeck, near Stank, described by me many years since*, Mr. Bolton, of Sedgwick Cottage, near Ulverston, informs me that a bore-hole had been made which showed blue shale to the depth of 150

* Transactions of the Manchester Literary and Philosophical Society, vol. viii. (2nd series), p. 423.

feet to occur under such limestone. What is the age of this shale I have no means of determining; but, from the old attempts near the place to sink for coal, most probably they will be found to be of Carboniferous age. If this be so, it proves the absence of the Robshaw Point sandstone there.

Section from Fleetwood to Roach Bridge.*

Distance, 25 miles.

In running over the country from the Irish Sea near Fleetwood to the millstone-grit hills near Hoghton, very little evidence of the underlying rocks can be obtained, owing to the thick covering of drift which envelopes the district between the first-named place and Preston. Here the river Ribble has excavated through the beds of drift, so as to expose some of the underlying strata. At Fleetwood, by the bore lately made there by Her Majesty's Government in their search for water, 409 feet of Keuper marls were penetrated. Further eastward at Poulton Breck many years since, in a bore made for coal, the same strata, with the overlying drift-deposits, were penetrated 537 feet. About Kirkham I am not aware what Triassic beds have been met with; but I should suppose the upper portions of the Bunter would be found on going through the drift, and that these strata extend to Preston, where, in the bed of the Ribble, a little above the East Lancashire

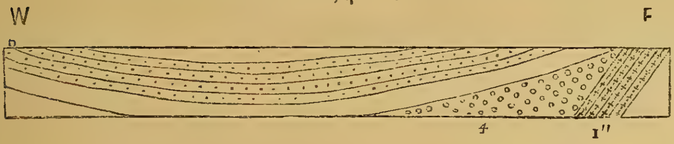
* In this and the following sections illustrating the present memoir, the references will be the following:—

Trias	7.	Upper Red Marls and Waterstones.
	6.	Upper New Red Sandstone, Bunter.
Permian	5.	Red Marls, with gypsum and conglomerate.
	4.	Lower New Red Sandstone. At Roach Bridge it contains a bed of fine conglomerate.
Carboniferous	1′	Lower Coal-measures.
	1″	Mountain-limestone series.

Railway Bridge, the pebble-beds make their appearance. They consist of a soft red sandstone, containing rounded pebbles of white and brown quartz, and dip at a considerable angle to the S.S.E. Only a small portion of the rock is exposed, so that no other dip could be obtained, which on the whole, if it could be seen more extensively, most probably is more to the west.

Section from Preston to Roach Bridge.

Distance, 4 miles.



On following the river up to Walton, little evidence of the Trias is to be seen, so far as I observed; but on tracking the Darwen a couple of miles to the turn of the river, above the Bannister Hall Print Works, a soft red sandstone, much bedded, makes its appearance, which dips W.S.W. at an angle of 9°. After following this rock up the river, rounded pebbles of brown and white quartz are seen in it, as well as small pockets of red marl. At the weir the same sandstone appears in great force, and the river flows over it and forms a cascade. A bed of micaceous shale, of a red colour, 1 foot 6 inches in thickness, divides the rock. The dip is to the west, at 18°. Nothing is seen in the river for about half a mile, and there is space for a great deposit of a lower soft red sandstone (Trias) or Permian red marls; but no evidence is to be had of the strata until the most westerly houses of Roach Bridge are reached. At Roach Bridge, in Samlesbury, an interesting section is seen on the banks of the river. The strata consists of a soft yellow sandstone, ripple-marked and false-bedded, some of the lower portions of which have formerly been used for building purposes, but with little success, as the perishing walls testify. The dip is to the west, at an angle of 12°. On proceeding up the river, the yellow sandstone changes into a rock of a dark red soft sandstone of considerable thickness, and then the same becomes divided by bands of red shale. Next comes a bed of fine conglomerate, 6 inches in thickness, consisting of fine pebbles of red and white quartz, well rounded, some of them being of the size of a small pea, cemented together by a red calcareous paste. The rock effervesces briskly when treated with sulphuric acid. Under the conglomerate occurs a bed of red lumpy shale, 2 feet in thickness, and then a few feet of variegated white and purple clays. All these beds dip to the west, at an angle of 12°. Some 10 feet of ground is not seen; and then appears a bed of black laminated shales, containing large rounded nodules of limestone, full of *Goniatites*, dipping to the south at an angle of 40°. They appear like limestone shales from their general characters, but they may be a bed of shales belonging to the millstone-grits; the *Goniatites* would not enable me to speak with certainty.

These soft, red, yellow, and variegated sandstones, with their thin bed of calcareous conglomerate, appear to me to be of Permian age, and they rest quite unconformably on the edges of the black shales.

A little to the south of Roach Bridge the millstone-grits make their appearance. In Hoghton, at a quarry known by the name of the Hollies, they are seen in the form of a hard gritstone of a red colour, the upper part of the rock, to the depth of 2 feet, being of a greenish-brown hue, and much decomposed. It dips to the W.N.W., at 20°.

The following is a rough estimate of the thickness of the Triassic and Permian strata, as seen in the Darwen section:—

		ft.	in.
Trias (pebble-beds)	about	350	0
Space not seen	about	400	0

		ft.	in.
Soft, red, yellow, and variegated sandstones	about	400	0
Bands of sandstone, parted by red shale,	about	30	0
Conglomerate (calcareous)		0	6
Red lumpy shale		2	0
Variegated shales		5	0

After these Lower Carboniferous strata disappear, beds of millstone-grit are seen at Wildbottoms and Owlet Holes. Further up the stream, near to Sir W. Hoghton's corn-mill, below Feniscowles Hall, a red sandstone, containing no remains of plants, makes its appearance, and dips to the S.S.E. at an angle of 12°. This stone, from its appearance, has sometimes been taken for a Permian or Triassic rock rather than a carboniferous deposit; but it unquestionably belongs to the lower coal-measures. On its rise it is succeeded by the latter strata, dipping to the north-west at an angle of 12°; and, above the bridge, small seams of coal have been wrought.

So far as my examination of the valley of the Darwen has proceeded, I have never been able to trace any Triassic or Permian deposits to the east of the black carboniferous shales containing *Goniatites* seen above Roach Bridge.

Section from Preston to Samlesbury, opposite Alston Hall.

Distance, 5 miles.

The bed of the river Ribble, from Walton to the bridge at Lower Brockholes, affords little evidence of the underlying strata. At the last-named place, however, a soft red sandstone, like the Trias in the Ribble below Preston, makes its appearance, dipping to the west at a small angle, and continues in the bed of the river to Salmesbury Chapel, below which place it appears as a soft red sandstone, much bedded, and without pebbles, and dips W.S.W. at an angle of 9°. It occupies the river-course up to Mr. Swift's house at Salmesbury Lower Hall, where it dips W.S.W. at 8°. It is also seen in a small stream, called Besser Brook, below Mr. Fisher Armitstead's house. After leaving Lower Hall I found pebbles in the stone, but not abundantly, of brown and white quartz, one of an oblong shape, 2 inches in diameter, and small pockets of red marl. On reaching the side of the wood there, Mr. Armitstead showed me a small quarry which he had opened for getting building-stone that he had used in the erection of his farm-buildings. It was soft when first quarried, but became harder on exposure to the atmosphere. In this quarry was a bed of red marl, 6 inches in thickness, used by the farmers for marking their sheep. The red sandstone can be seen in the river, past the turn below Mr. Barton's farm-house, and then for a short distance, less than 100 yards, on the rise of the strata, till and clay are seen until you reach the ferry called Barton's Boat, where occurs a bed of dark blue shale and fine-grained sandstones, dipping to the south at an angle of 29°. These appear to extend up the river, past Alston Hall on towards Ribchester, and no tidings could be had of any more red rock in that direction. The soft red sandstone in the Ribble appeared in every respect like those previously described near Bannister Hall, and seems to belong to the pebble-beds of the Trias; but I did not see anything like the red and yellow sandstones and conglomerate of Roach Bridge, previously described. The Lower Carboniferous rocks at Barton's Boat, by their dip, appeared to me to be a continuation of the Lower Carboniferous strata seen in the Darwen, and previously described; but the rocks lying on it appeared to be Trias rather than Permian. From the dip of the strata below Preston and

Salmesbury, there appears to be a synclinal axis, similar to what is seen in the Ribble and Darwen, between Preston and Roach Bridge, and previously described.

These pebble-beds are of great thickness; and the pebbles, although only in the middle portion of the rock, and found very sparingly, undoubtedly do occur. It is probable that the lower portions of the red sandstone seen in the Darwen at Bannister Hall Weir, and below Barton's farm in Salmesbury, may belong to the lower soft and mottled red sandstone. There is ample space for that rock.

From the neighbourhood of Salmesbury up to near Scorton I have not carefully examined the country to say with certainty what it consists of; but I have heard of no sections showing the relation of the Triassic and Permian beds to the millstone-grits or limestone shales.

In a valley to the south of Scorton, known by the name of Griesdale, is a bed of black shale containing large nodules of ironstone, some of them of a rich red colour. They, as well as a coarse gritstone seen in the same neighbourhood, dip a little west of south, at an angle of 35°. These beds appear to belong to the millstone-grit series, and can be traced at intervals through Scorton, Cleveley, and Ellel. In Cleveley I have seen that rock in the form of a soft reddish-brown sandstone of coarse grain, which dipped slightly west of south, at an angle of 16°.

Further north, in Ashton, at a place called the Outer Park Quarry, the millstone-grit is seen as a fine-grained sandstone, of a beautiful white colour, quite soft when first taken from the quarry, especially the upper beds of it, and much used as a building-stone. It is one of the most clear and pure silicious grits that has ever come under my notice, and appears to be well suited for glass and china manufactures, and for making cores for iron castings. Some of the beds contain pebbles of white quartz, and traces of

Sigillaria and other coal-plants are found in it. The dip is to the E.S.E., at an angle of 12°.

In a quarry in an adjoining field to the Outer Park Quarry, the stone, evidently a millstone, is of a reddish colour, and dips to the south at 14°.

These millstones appear to run under the country to the Abbey Light, at the mouth of the Lune, and then across that river to Robshaw Point* and on to Heysham; but on their west side they do not, to my knowledge, afford any sections showing their relation with Permian or Triassic rocks, except at Cockersand Abbey and Robshaw Point.

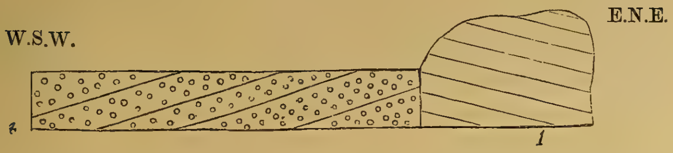
A little to the south of Cockersand Abbey, near to the mouth of the Lune, below high-water mark, is seen a soft sandstone of a dark red, variegated by patches of a yellowish-brown colour, very false bedded, and containing no pebbles. There it dips to the south at an angle of 6°.

On the beach just below the Abbey the dip was due west at 15°, and nearer the Abbey still it dipped N.W. at 15°. This variety of dip may arise from the extraordinary false bedding, making it extremely difficult to get the true dip of the rock. I took it to be a soft Permian sandstone, similar to that formerly described by me under the conglomerate at Rougham Point near Humfray Head; but although I saw numerous large pieces of the latter rock lying on the beach below the Abbey, I found none *in situ.* I was informed that the Abbey Light-house was built on a fine-grained, light-coloured sandstone, something like the Ashton stone; but I did not see it myself. This appears probable, as the Permian sandstone ranges across the mouth of the Lune, and makes its appearance on the beach below high-water mark, on the north side of the mouth of that river, at Robshaw Point, the cliff being of millstone-grit.

* This is the name of the place, given to me by a man living near it.

Robshaw Point Section.

Distance, ½ mile.



This is seen on the north of the Lune, about 2 miles south of Heysham. The cliff from that village to near Robshaw Point is composed of millstone-grit, of dark red colour*. Near Heysham it is traversed by a vein of sulphate of barytes and red oxide of iron, running in a direction from N.E. to S.W. The dip of the strata there is W.N.W., at an angle of 23°. As you approach Robshaw Point, the millstone is not so coarse in grain, has a good deal of false bedding in it, and is parted by layers of red stone and red shale. The dip in the cliff is to the south, at an angle of 11°. Below the cliff, on the beach, a piece of millstone is seen dipping W.S.W. at 20°. A little to the south of this point, about 350 yards, is seen a patch of soft red sandstone, coarse in grain, but containing no pebbles, mottled with brown and white colours, having traces of black oxide of manganese in it, and very false-bedded. Near to high-water mark it dips to the W.S.W. at 35°; but at its extreme west, about 300 yards towards low-water mark, its dip is only 22°. Its range is W.N.W. and E.S.E. No trace of a conglomerate could be seen on its dip, the uppermost beds disappearing in the sand. On its W.N.W. range it likewise disappeared in the sand, and could not be traced nearer than 350 yards to the patch of millstone on the beach previously alluded to.

Altogether about 120 yards in thickness of this Permian sandstone is exposed; and, from its range and dip, it appears

* This stone is coloured as Trias in the Geological Map of the Geological Society.

to be a continuation of the same sandstone before described at Cockersand Abbey, and most probably ranges across Morecambe Bay to Rougham Point, near Humfray Head. In all its characters it exactly resembles the soft red Permian sandstone of that place, and it rests unconformably on millstone-grit, like the same rock there does on mountain limestone. Although the overlying Permian conglomerate, as seen at Rougham Point, is not met with, as previously stated, *in situ*, it lies about as boulders in considerable quantities on the shores north and south of the mouth of the Lune, both at Robshaw Point and Cockersand Abbey.

Concluding Remarks.

The sections of Rougham Point, Robshaw Point, Cockersand Abbey, and Roach Bridge all show a soft variegated red and yellow sandstone, of Permian age, resting unconformably on Carboniferous rocks, in a similar manner to what is seen at Grimshaw Delph, Rainford, Croxteth Park, and on the Manchester and Liverpool Railway at Whiston, and in all probability will be proved to be of the same age.

The country, owing to the thick covering of drift, is very difficult to examine; but it is to be hoped that all the facts ascertained by boring for coal, and in other subterranean searches, will be recorded and published, so as to give us some further materials to reason on.

The Bannister Hall and Roach Bridge section, if the lower beds are proved to be of Permian age by the occurrence of red shales or marls in the covered-up beds (which I think most probably may be the case), will show us a thin bed of calcareous conglomerate, at the base a soft red sandstone, instead of at the top of that rock, as seen at Cheetham Weir Hole, near Manchester. This is not unlike a conglomerate bed previously described by me in my last memoir, as occurring in a similar position on the banks of the Esk immediately above Canobie Bridge. All our investigations in the Permian deposits lead us to expect great changes and variations in the beds, and to be prepared to accept sections such as they are found, without predicting what they should be in a new district. At some future time there may be materials for placing them on a map provisionally; but at present it is only in my power to collect a few disjointed sections, and preserve them as guides for future workers.

After the great change which has of late years been made in the classification of the rocks formerly known as new red sandstone found north of Manchester and Liverpool, considerable portions of Trias having been transferred to Permian, so that when we reach Carlisle little has been left of the former rock, in my opinion probably less than it is fairly entitled to.

The arbitrary lines of division in the Carboniferous and Permian formations, and the passage of the one into the other, and the latter into Trias, although doubtless at some places well-marked and distinct, and admitting of no doubt; at other points, pass one into the other so gradually as to render it hard to tell where one ends and the other begins, when there are no organic remains to guide us, but simply beds of red sandstone and red marl.

All passage-beds require careful examination; and when we have to trust to the composition of the beds alone, without the aid of fossils, our views ought to be expressed in anything but dogmatic language. Doubtless we have much yet to learn of the highest Coal-Measures and the lowest Permian beds, which is not rendered easier by Mr. Scott's discoveries of unconformable Coal-Measures in the Coal Brookdale district, and Professor Ramsay and Mr. Aveline's description of apparently unconformable Coal-Measures near Rotherham in Yorkshire. Added to this, when we have a great thickness of unproductive Coal-

Measures to investigate, which nobody will, if he knows it, explore more than once, we have not anything like the chance of knowing barren Coal-Measures like a rich and profitable series of workable beds.

All mining engineers dislike what they term "red ground," and do not trouble themselves much with investigating strata containing little or no coal, and their investigation is left to the geologist. Even many of the latter dislike examining red clays and sandstones, usually very barren of organic remains, and not at all enticing to collectors of pretty specimens.

The occurrence of a series of strata lying between the uppermost Coal-Measures yet noticed and the lower soft sandstone of Collyhurst, and generally unconformable to both such Carboniferous and Permian strata, is now pretty well proved in some districts, as at Whitehaven, Astley, Allesley, Moira, and other places. I think a series of Coal-Measures, from which Mr. Edwin Baugh, of Bewdley, has obtained a valuable collection of coal-plants, will also come into this division, as was supposed would be the case by my esteemed friend Dr. Geinitz, who says in his 'Dyas'* "he had not obtained any certain information as to the existence in England of a lower 'Rothliegende,' or in general of any lower division of the Dyas. From the observations previously made in these pages, it appears that the absence of the hornstone porphyry ('Felsitporphyr' proper) just in those districts of England has a bearing upon this question. It is repeatedly remarked that the existence of most of the porphyries are linked to the lower period of the Dyas, and, *vice versâ*, that the formation of the lower 'Rothliegende' stands in close relation to the porphyries."

"The existence in England of the lower 'Rothliegende'

* "Dyas or Permian Formation in England" (translation from 'Dyas,' &c., of Dr. Geinitz), Transactions of Manchester Geological Society, vol. iii. p. 144.

which is so general in Germany, and there forms such an imposing bed, might perhaps be most easily traced in the neighbourhood of Kidderminster. It is at least indicated by the presence, in a reddish sandstone, of *Walchia piniformis*, Schl., which I saw in the collection of Mr. G. E. Roberts, together with some less distinct fossils. The exhaustive collection of specimens from the upper portion of the Coal-Measures of Wribbenhall, near Bewdley, made by Mr. Edwin Baugh, will no doubt greatly contribute to the more exact determination of the palæontographical relations between the Coal-Measures and the Dyas in this district."

In my former communications I have noticed at some length, in my description of the upper Coal-Measures of Canobie and the Whitehaven sandstone, these beds, ana compared them with a similar sandstone found in the same position at Astley, near Manchester, and Moira, near Ashby-de-la-Zouch. Mr. H. H. Howell, F.G.S., in his excellent paper on the Geology of the Warwickshire Coal-Field*, describes at length the Spirorbis-limestone with great care, and shows its value as a datum-line in the upper coal-field. When my last paper was printed, I had not seen Mr. Howell's communication, or I should have noticed it. He had previously named this limestone Spirorbis, and I, not knowing the circumstance, had done the same. Of course the name belongs to him, and not to me. This much, however, I will say, that I have never read a more carefully prepared and useful description of upper coal-measures than is contained in his memoir. It remains for other geologists to give us similar descriptions of the higher division of the western coal-fields, and then we are likely to know where the Carboniferous strata end, and the Permian begin, which is probably not so clear at present as is to be desired.

* Memoirs of the Geological Survey of Great Britain.

The Allesley sandstone, from which it is believed the beautiful specimens of fossil wood now in the Warwick Museum came from, so far as it has come under my observation, will have to be classed as of the same age as the Whitehaven, Astley, and Moira sandstones, and which in Germany would be called Lower Rothliegende. As I have previously stated, some of the sandstones in the neighbourhood of Bewdley, as Dr. Geinitz supposed, will most likely have also to be added. With the assistance of Mr. E. Baugh, I have obtained evidence of the occurrence of the Spirorbis-limestone at Prizeley and Gibhouse, near Cleobury Mortimer; but as yet I have not been able to prove that Mr. Baugh's plant-beds, found in the cutting at Cundalls, near Bewdley, are Carboniferous strata lying above this limestone, although it is probable such may be the case. All the geologists in the district about Bewdley will render good service to science by satisfactorily determining this point.

VI. *On the Plumules or Battledore Scales of* Lycænidæ.

By John Watson, Esq.

[Read before the Microscopical Section, January 16th, 1865.]

Having on a former occasion drawn your attention to the plumules of some genera of Pieridæ with the intention of showing that they serve for the identification of species in that family of the Lepidoptera, I now, with the same aim, request your attention to the microscopic examination of the (so-called) Battledore Scales of some genera of the Lycænidæ, which exhibit similar generic and specific alliances and differences, and answer the same purpose of identification.

The name "Battledore" is not appropriate. Looking upon them as flat surfaces, it would be so with many; but they are more or less globose or cylindrical, and have manifest rotundity. I prefer the name of plumules, given to their congeners among the Pieridæ.

They are most beautiful microscopic objects, and interesting in a physiological sense, displaying how variously and marvellously creative power has worked in these minute organisms, always with the same end in view.

The especial function of the plumules of the Pieridæ was suggested in my former paper to be that of air-vessels, giving buoyancy to the insects; and these Lycæna scales, by their balloon shape, are eminently fitted for this service, and even in a greater degree render it probable that certain Lepidoptera possess at least two kinds of scales, performing different offices in the economy of the insects. These plumules are attached to the wings by an apparently hollow peduncle. They show striæ-like ribs, suitable for binding, strengthening, and distending or contracting their balloon-like forms; these ribs are more or less beaded or articulated, by which different scales are bound or bent in various ways. The end opposite to that of insertion is closed or covered with apparently ciliary apparatus; and they lie in rows between and under the ordinary scales, which may therefore be elevated or depressed at the pleasure of the insects by the regulated inflation of the plumules. They differ in separate species in every conceivable way—in form, in the number and articulation of the ribs, in transparency, in size, and in the length and shape of the peduncle; and among them are found some very anomalous forms, as in the plumules of the Pieridæ. In that family, *Pieris agathina* possesses an abnormal and unique form; and so, in the Lycænidæ, *Lycæna bætica* resembles it in this peculiarity.

And as in the Pieridæ, so in the Lycænidæ, it is only on the males that these scales are found; it is probable that this mark of virility may indicate the comparative vigour or age of the insects. On some individuals of the same species they are much more abundant than on others; and, again, in some species they are plentiful, and in others scarce; and in some individuals of all species it is difficult to find them at all. On newly caught specimens, however, they are most easily found.

It is the genus *Lycæna,* with its neighbour *Danis,* which affords the scales now submitted to your inspection. The upper-side of the wings of the males is generally bright blue, but at all events with more or less blue irrorations; the males of certain species, however, are brown. The females are generally brown; but even when they have blue surfaces I have found no plumules on them, while on the males which have any tinges or reflections of blue these scales are present. No species, however, the males of which are brown, yields these scales; and yet it is not in these peculiar scales that the pigment or colour-reflection resides. To a very eminent lepidopterist I lately wrote, asking if he was aware of any other physiological difference between the blue and brown species. His reply was, "that he could not think there was any other difference, but that it was a most interesting fact that when these males imitate the females in colour they lose another male characteristic."

Mr. Sidebotham has, with habitual industry and kindness, drawn figures of a large number of these scales; and they are now placed before you. These drawings are as truthful as beautiful; the slides were mounted from insects in my own cabinet, and I believe reliance may be placed on the correctness of the nomenclature and habitat. These slides and insects are also here for your inspection.

Nos. 1 *a,* 40, 42 *a,* 46, and 50 belong to the genus *Lycæna.* Nos. 47, 48, 49, 52, and 53 belong to the genus *Danis,* according to the arrangement of Doubleday, Westwood, and

Hewitson, in their 'Genera of Diurnal Lepidoptera,' the text-book of general diurnal lepidopterists. The undersides of the insects of this genus are unlike those of *Lycæna,* but have a family resemblance of their own. In constituting this a genus, our authors say of it, "With general characters of *Lycæna,* it appears very (perhaps too) close to *Lycæna.*" This is said without reference having been made to other than the usual tests. The scales have certainly a peculiarity of their own.

The remarkable figures 38 to 41 on Plate II. cannot fail to attract attention; and at first sight it would be thought that they can have no relation to the others, and that the insects could have no natural affinity; but similar differences (inconsistencies, if you will) exist in the Pieridæ, and it does not appear that insects nearly allied in other respects are always furnished with similar plumules. It is, however, possible that similarity in this respect may hereafter influence entomologists in their arrangements.

The scales represented by figures 1 to 37 and 42 to 53 on Plates I., II., and III. are from insects inhabiting various localities all over the world, each with certain geographical limits. Most Butterflies have a rather narrow range of habitat, as is the case with other animals; and some are confined to very strait localities. For example, *Morpho Ganymede* is known only as inhabiting a certain district of Bogota, the family Acræa is found almost only in Africa, *Ageronia* only in Brazil; but *Pyrameis cardui* ranges over the whole world, and *Vanissa Antiopa* is excluded only from Africa. Now *Lycæna bætica,* figs. 38, 39, and 40 on Plate II., is also a cosmopolite, inhabiting all the localities of those represented in Plates I., II., and III. In the 'Diurnal Genera,' before referred to, its habitat is given as "Southern Europe, Java, South Africa, Mauritius, Madagascar, Africa, India." It has also been found in

Great Britain; and I may add to the list Australia, the insect from which fig. No. 40 was taken having been captured by Mr. Diggles at Moreton Bay. Figs. 38 and 39 are also from Moreton Bay, and mere varieties of the same insect.

Collectors are now receiving from Australia insects previously known as appertaining only to the Indian archipelago; and it is remarkable that while this island has an insect fauna of its own, it should also possess the insects of neighbouring though distant lands, and yet that its peculiar fauna, animal and vegetable, should be distinct.

The insect whose scale is shown by No. 41 has been lately named by Felder *Dipsas lycænoides*; it is questionable whether it should be placed in the genus *Dipsas*. It is also from Moreton Bay, and evidently allied to *bætica*. The beaded or articulated appearance at the upper end is very singular. The insect has an evident affinity with *bætica*, but in no other instance have I found any scale approaching these.

The points desired to be insisted upon as useful in this investigation are—

1. That these plumules are always identical in different individuals of the same species; and therefore mere geographical or other varieties may be detected by this test; and that

2. In species nearly allied, so closely as to make them difficult of distinction, these scales will be often found very different, forming very certain and unquestionable divisions; while, on the other hand, species of easy separation in other physiological peculiarities have sometimes almost identical plumules.

Microscopists have seen in some of the Foraminifera exquisite forms of flasks and decanters; and in these plumules no one can fail to observe their elegance, beauty, and applicability to industrial-art purposes for forms and engravings of wine-glasses and goblets.

The following is a list of the names and habitats of the insects to which the drawings have reference:—

LYCÆNA.

1. Alexis. Europe.
2. Icarius. „
3. Dorylas. „
4. Damon. „
5. Adonis „
6. Acis. „
7. Ægon. „
8. Cœlestina. „
9. Corydon. „
10. Orbitulus. „
11. Theophrastus. India.
12. Alsus. Europe.
13. Euphemus. „
14. Melanops. „
15. Unknown, and not named.
16. Sebrus. Europe.
17. Argus. „
18. Unknown, and not named.
19. Do.
20. Optilete. Europe.
21. Hylas. „
22. Cassius. Brazil.
23. Unknown. India.
24. Telicanus. Africa.
25. Unknown.
26. Do.
27. Do.
28. Methymna. Cape Town.
29. Ælianus. India.
30. Elpis. „
31. Celeno. „
32. Erebus. Europe.
33. Kandarpa. India.
34. Argiolus. Europe.
35. Unknown.
36. Pseudargiolus. Canada West.
37. Unknown. Australia.
38. „ „
39. „ „
40. „ „
41. Dipsas lycænoides. Australia.
42. Lycæna cardia. India.
43. —— Lacturnus. „
44. —— Aratus. „
45. —— Cneius. „
46. Unknown. „
47. Danis Hylas.
48. —— New species.
49. —— „
50. Lycæna Alexis.
51. —— ? new species.
52. Danis, new species.
53. —— Sebæ.

VII. *Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes.*— Part I. By J. C. Dyer, Esq., V.P.

Read October 17th, 1865.

1. *On Lace-making by Machinery.*

The manufacture of lace, or "bobbin-net," at and near Nottingham was conducted by hand-working on *cushions* or "lace-frames" by women or young persons, and a large lace-trade had been established in that place when, about the beginning of this century, this method of forming the lace by *hand-work* was superseded by machines driven by power. The change was brought about by the inventions of the late Mr. John Heathcoat, who afterwards became, and for many years was one of the Members of Parliament for Tiverton.

In his early life, Mr. Heathcoat had been engaged in making the *frames* used in the stocking-weaving and by the bobbin-net makers, in which employment he had carefully observed the process of forming the meshes of the lace by the workers on the *cushions*, and he conceived it possible to perform the like movements for guiding the threads to form the lace-meshes by means of mechanical instruments to be driven by rotative power. His first experiments were made as follows, viz., he procured some common twine, or packing-threads, and stretched them in a plane across his room, at the proper distances apart, to form the *warp* of the fabric to be wrought; then, by means of common *pliers*, he passed the bobbins charged with thread between the cords, and delivered them into the jaws of other pliers on the opposite side of the warp, and then, giving a slight sideways motion to the pliers, the bobbins were returned back between the next two cords, and so on, moving from side to side to tie or form the *meshes*.

With this rude apparatus Mr. Heathcoat found that the knots, or intersections of the threads, could be formed of the *same kind* as those made on the cushions by the hand-workers.

The next step was to contrive the form of *bobbins* that would pass back and forth between the threads, placed at the requisite distances apart to form the lace. For this purpose *metallic disks* were made, with grooves turned in their peripheries to contain the threads to be carried successively through the warp for weaving the lace.

The machine for working the bobbins consisted of two bobbin-carriers, one on each side of the warp, mounted and arranged so as to *deliver* and *receive* alternately the bobbins of thread from one side of the warp to the other. The bobbins, or *thin disks,* being placed in proper recesses in the carrying-frames, the required motions were given to them for conducting back and forth between the threads of the warp the *bobbin-threads,* and for transferring the bobbins to the opposite carrier-frame, to be again passed back through the warp. In this way the required number of bobbins, being duly arranged in their carrier-frames, were conducted back and forth (from the upper to the under side of the warp, and *vice versâ*) to weave the lace.

I do not mean to describe here the many ingenious movements employed to effect the operations above mentioned, and which made them obey continuous rotative action, as they are set forth at large in the specifications of Mr. Heathcoat's patents.

The great saving of labour effected by the patent machines rendered the bobbin-net trade unprofitable as it had been carried on upon the old system of working, consequently many hands employed therein were thrown out of work, and much local distress prevailed in that neighbourhood, which led to the "*Nottingham Riots*," "*Lace-frame Breaking*," and other outrages that took place about fifty years ago. Among these acts of violence was the entire destruction of Mr. Heathcoat's establishment for lace-making with his patent machinery. The great success of the inventions had excited such extensive and bitter hostility against him and his partners in trade, that it appeared advisable, even for his personal safety, to leave that part of the country, and re-establish his works at some place quite disconnected with the cotton trade. Accordingly he removed to Tiverton, Devonshire, where he erected a large lace-making establishment for using his new machines in safety. These works have been continued to the present time, and from time to time greatly extended, and successive new inventions and improvements have been introduced, the fruits of his fertile genius and enterprising spirit, which have made them eminently successful.

Whilst engaged in devising changes and improvements in his lace-machines about forty-five years ago, Mr. Heathcoat came to Manchester for the purpose of examining the movements of my *wire-card making machines*, and by a careful analysis of them he was enabled to apply several of those movements to facilitate the improvements then making in his lace machinery.

It mostly happens that the first effects of labour-saving inventions are to cause derangement and distress among those employed in the trades for which such inventions are brought into use, and this in proportion to the real efficiency of the new machines in reducing the cost of labour in working them. But the temporary evils thus arising are always counterbalanced by the advantages to grow out of extending those trades, and thus affording employment for a greater number of hands than could ever be employed by the superseded plans of working, and also by the employment of more capital at increased profits in the trades so improved and extended. These are the benefits conferred upon *Society* and *Nations* by men of original genius and great mental powers, such as were eminently displayed by the late John Heathcoat. The *old bobbing-net trade* giving support to a small circle of working people and petty dealers at Nottingham, has now become extended into a great branch of national industry, filling the world with all kinds of plain and gaudy *lace-netting*, from the "mosquito net" to the most elegant and fantastic adornment of beauty and fashion.

"It is a vulgar error" to suppose that the several valuable machines now employed in our cotton and other manufactures were originally conceived and brought into successful operation at some one time by their respective inventors.

The entire history of all important inventions goes to disprove this assumption, and to show that the most valuable mechanical inventions in use have been patiently worked out from the simple conception of a *new principle of action*, to be substituted for the practice then in use. This, as above shown, was the origin of the method conceived and pursued with such marvellous success by Mr. Heathcoat in giving to his new lace-frames a thousand-fold power over the old ones worked by hand, for making plain and figured lace.

We are not to limit our estimate of the value of Mr. Heathcoat's inventions to their application in giving such a vast extension to this one branch of textile manufacture. His simple but clearly original thought was that of passing the threads of the *woof* through those of the *warp*, then delivering them into conductors on the opposite side, there to be repassed and delivered into the former conductors, the movements being under mechanical control in place of hand-working. This *original process* being realized in lace-making, opened a new vista to other inventors, affording them both instruction and incitement in their mechanical labours, and must have led to the application of the same principle—that of passing threads back and forth through warps, on which the beautiful embroidering machines are constructed.

The acquired knowledge of each generation is the fountain from whence new lights flow to their successors; so it is no derogation from the merits of those who contribute to the general stock by extending the limits of "useful knowledge," although the germs of such extensions may be found in former practice, for it is wisely said, "there is nothing *new* under the sun;" yet in the usual sense I consider the *invention* of the embroidering machine, as now in operation at Messrs. Houldsworth, to embrace so many original and beautiful movements, as to entitle the authors of them to rank among the most talented mechanicians of our time. Again, I consider another most valuable and ingenious invention, also of foreign origin*, to be based upon the same idea—that of passing from one to another set of holders and conductors the cotton in the *combing machines* now so extensively employed in separating the short and coarse from the long staple in carding fine cotton.

2. *On Wire-card making by Machinery.*

In the North American Colonies and States the manufacture of woollen and cotton fabrics for domestic use, especially of the coarser sorts, had become a regular branch of industry in the winter months, when out-of-doors labour

* The embroidering and the combing machines were first patented in France, and were each in a very imperfect state when brought to this country. They were taken up by Mr. Henry Houldsworth, by whose eminent talents and ingenuity they have been successively simplified and rendered practically valuable machines.

was mostly suspended, the *carding, spinning,* and *weaving* by the ancient hand-machines being done in the farm-houses mostly by grown-up members of the families. These fabrics are called "*domestics,*" to mark them as distinct from the finer and more ornamental kinds imported for "best dresses" and other decorations.

The *hand-cards* then in use were mostly obtained from England, though a portion of the coarser sorts were made in the several localities. About the close of the last century a change in the carding process was brought about by the introduction of the "cylinder carding engines" from England, by which the hand-carding was superseded, and the preparation for spinning more perfectly effected. Upon this change, in place of importing the hand-cards, a demand arose for the kind of wire-cards especially adapted for covering or clothing the cylinders of the new engines, and these could only be obtained from England. But the uncertainty and delays in importing these machine-cards to replace the worn-out or damaged cards on the cylinders had at times led to the "stoppage of the carding-mills," and thereby created an urgent demand for *domestic*-made machine cards.

In this state of the wire-card trade Mr. Amos Whittemore (then residing near Boston, and having a small trade in hand-card making) began his experiments for constructing a MACHINE to make cards by continuous rotative power. His first step was to examine the movements required to form the wire staples, or "*card-teeth,*" as they were set in the sheets of leather to form the hand-cards. The complete *card* being formed with wire *staples,* the legs passed through the sheets of leather, and the *flat crowns* of the staples pressing against the leather, and the legs bent forwards to a slight angle with the sheet, so that when set in rows their points would successively take hold of the fibres to be carded, and arrange them evenly along the face of the card in loose sheets or layers, to be "*doffed*" or removed for spinning.

The successive motions to form and set the teeth in the leather, to complete the card, are as follow :—

(1) The feeder, to draw the wire from a reel (in lengths about $1\frac{1}{4}$ inch) to form the staples.

(2) To hold the wire in the middle between the *staple-bar* and the *presser*.

(3) To cut this wire off (from the supply coil) always in equal lengths.

(4) The severed wire being held against the stapler-*bar*, the stapler-*wings* are advanced so as to push forward the ends of the wire *to form the staples*.

(5) Two piercers or steel points are then advanced, and pierce holes through the leather, just opposite the points of the staples, and then they are drawn back, and descend out of the way of the staple points.

(6) The staple-bar and wings then advance with the staple, the points of which are guided by the wings into holes of the leather.

(7) The staple-bar then rises above the crown of the staple, and the *presser* advances and forces the crown home against the leather.

(8) On the opposite side of the leather are placed the "*crookers*," or knee-benders, having loops or "eye-openings" just above a fixed steel edge or bar, and the *legs* of the staple are passed through these eyes, so that as they descend the wires are "*crooked*" or bent to the proper angle to form the carding surface.

(9) All of the above-named parts return to their former positions, for repeating the same motions for making and setting the card-teeth, and then the sheet of leather (stretched in a frame) is *moved upwards*, just enough to set the next row of teeth, and *sideways* the proper distance apart for the crowns of the staples, and so on continually

to form the wire-cards of such sizes and kinds as are required for *carding-engines.*

In the old system of card-making several rude machines were made (1) for cutting the wires into the required lengths for the teeth; (2) for bending them into staples and giving the knee-bend; and (3) for piercing the holes in the sheets of leather; but the *setting the teeth in the leather* was the work of children, in which a large number were employed in and near Halifax, which had become the principal seat of the trade; and this *card-setting* was very bad for the eyes, sometimes even destroying the sight of the children so employed, and otherwise injuring them through the long hours and the very low wages paid for this work. Apart, therefore, from the saving of labour to be effected by a machine that would complete the cards without any hand-work, it would allow those children to be *sent to school,* instead of being thus injured by improper confinement at a kind of work unfitting them for other employment when grown up. However, the direct stimulus to Mr. Whittemore was, as above stated, to supply a pressing want of the time. When we look to the many delicate and distinct movements required for making and setting the card-teeth in rapid succession, so as to render the machine practically successful, as against the old plan of working, it must be seen that he had undertaken a task of no small difficulty, requiring high *inventive powers* and an indomitable *will* to realize the object in view. Still, as he had the idea of a complete machine matured in his mind, he persevered amidst great difficulties in his experiments, until he constructed a *working model,* on which his American patent was obtained.

Having thus found it practicable to perform the operations required in due succession by the rotation of a shaft, viz. (1) the feeding, (2) holding, (3) cutting off, and (4) bending the wires into the staples or teeth, (5) piercing the sheets of leather, (6) passing the staples through it, and (7) pressing their crowns home to the sheet, (8) crooking the teeth to the knee-bend, and then (9) advancing the leather sheet to receive the next row of teeth, — these complex and curious motions were produced by a series of "cams," or excentric pieces of steel fixed on a shaft and turned by a winch.

The *invention*, therefore, of "making wire-cards by machinery" was thus accomplished by AMOS WHITTEMORE, to whom the honour of this *invention* is solely due.

In this machine, as in that of Mr. Heathcoat for lace-making, a new principle of action had been applied by Mr. Whittemore to produce and govern the movements for making wire-cards; namely, the "wedge-pressure," consisting of a series of "cams," or excentric curves revolving on a driving-shaft, and giving the alternate movements to the traversing parts of the machine in their exact order for making and setting the card-teeth, as before explained. Although in the old "*iron-forging*" and the "cloth-fulling mills" the trip-hammers and the cloth-beaters were worked by radial levers or short arms from the driving-shafts, yet they were used merely for *elevating* the hammers and beaters, and not for guiding their motions, as the work was done, in both cases, by *impact*, viz., by the falling of the weights thus raised by the levers on the driving-shafts. On the other hand, the *cam* motions in Mr. Whittemore's machines were guided and determined in their due order of succession to produce the nine distinct movements at each revolution of the driving-shaft, as before described.

It will be shown further on that this new and very important application of projecting curvilinear levers called "cam pieces," fixed on driving-shafts, has since been extensively adopted for giving "intricate and regulated motions" in many other machines that have been invented and patented within the last fifty years.

Without casting any slight upon the application of Mr. Whittemore's *cam motions* by subsequent inventors, or calling in question the merits of the machines in which they are used exactly in the same way as in the wire-card making, it is but fair to say that, besides *inventing* his own machine, he thereby became the pioneer and guide to the pursuits of other able mechanicians in their labours for the advance of mechanical science.

Mr. Whittemore, like most other inventors, met with many obstacles to bringing his machine into a "good working state" after it was *invented,* so far as regards the mechanical principles on which it was founded; and many years elapsed thereafter before any profit could be realized from his patent card-making works.

His path had been beset with both mechanical and pecuniary difficulties, many of the former not having been anticipated to the extent that arose in practice. Mr. Whittemore had adapted his machine for setting in plain rows the kind of cards before nailed on boards for *hand-carding,* whilst those required for the new carding engines were of various kinds, widely differing from the former sorts, such as *plain, ribbed,* and *twilled* in the setting, and in *sheets, tops,* and *fillet* cards of various lengths and breadths to suit the different sized engines. To meet the calls for these (already supplied by the hand makers in England) Mr. Whittemore had to devise many changes in the construction of the machine, to adapt it for making the several kinds of cards suited for the new cylinder carding, such changes taxing his inventive powers to meet the unlooked-for demands.

In this incomplete state of the patent machine his brother, Mr. William Whittemore, joined him to form a Company in Boston for patent card-making, and for bringing the patent machines into more extensive use in America, and also for introducing it into England.

With this latter view the Company sent out to me one of the fillet card-machines (in the year 1811) to be patented in this country, for the joint account of the Company and myself. I need not describe the state in which the invention was thus "communicated" to me, seeing that a very full and clear *specification* of the machine, with elaborate drawings of all its separate parts, may be seen as enrolled at the Patent Offices.

In the following year (1812) a patent card-making Company was formed in London, under the direction of Mr. Henry Higginson (of Boston, then an American banker in London). After having a number of machines built in Birmingham, Mr. Higginson had them removed to *Manchester*, where the first patent wire-card-making works were established on account of the joint patentees.

In the course of constructing and putting the machines into operation, several alterations and improvements in their working parts were suggested, both by Mr. Higginson and myself, and by our joint labours the machine was made far more simple in several of its movements, whereby it could be worked with much greater safety and speed than in its previous state. The business was then extended to about thirty machines, with their needful appendages, and the experiment seemed to afford good hopes of success, when unfortunately the factory, with all the machines and stock in trade, were destroyed by fire in January 1814.

We had omitted to ensure the works, so that, when burnt down, it was an entire loss to the Company of something over £6000. Under this discouraging state of the concern, the parties in America were unwilling to concur in rebuilding the works, preferring to sell out their interests in the English patents, which, having more confidence in the prospect of success, I finally purchased, and then became the sole proprietor of the patent; and Mr. Higginson having left and returned to Boston, I commenced building the machines for re-starting the works on a large scale; and in doing this I made several other changes and improvements, which are set forth in the specification of my second patent, dated in 1815.

I had from time to time made many changes in the different parts of the machine, and especially those to form the *twill fillets* and for *controlling the inertia* of the rapidly alternating movements in it, which so far increased the speed and safe working of the machine as to supersede entirely the original model machine as it came to me from America.

Some time after completing these improvements, I sent to Mr. Whittemore the working parts, or "head work," of one of my machines for him to adopt, if thought useful, in the card-making works of the Company with which he was connected, and which had been removed to New York; and in return I was much gratified by receiving the assurance that he, and the parties concerned with him, had highly appreciated the improved construction so transmitted by me, and that great advantages would result from bringing them into use in their works.

It is now over thirty years since I relinquished the card-making business in Manchester, which was subsequently transferred to Mr. James Walton (a gentleman highly distinguished as an able mechanician, and the author of several original and valuable inventions), by whom several further improvements were made in the card-machine, some of them rendering it still more safe and rapid in working.

I may here observe, in conclusion, that several important and well-known inventions of our own times will rank higher in the annals of science than this of Mr. Whittemore, especially those concerned with the *vast moving forces* employed in large engineering works, the control of which forces requires a profound knowledge of the principles of action and reaction on which they depend; still I feel assured that "the perfect witness of all-seeing Jove" will assign to this invention of Mr. Whittemore the rank of being one of the highest exertions of *inventive genius* (as applied to *the rapid succession of complicated motions to effect given results*) that are anywhere to be found in successful practice in our times.

The *cam*, or curvilinear wedge, action produced by revolving shafts, as we have seen, was *first employed* by Mr. Whittemore for guiding a series of regulated motions in this card-machine; but since that machine was brought into use, many other inventors have availed themselves of the same cam action for producing similar motions in other valuable machines now in general use, among which may be mentioned that for making the eyes or shanks of metal buttons. Several of the motions in this machine very closely resemble those in the card-machine.

Again, the machine for making "*wire reeds,*" used in weaving (the invention of Capt. Wilkinson), which was greatly improved in its construction and rendered of value in practice by the late Mr. Richard Roberts, who built the machines for a patent reed manufactory in Manchester—this and the pin-making machine invented by Mr. L. Welman Wright, by which the wire is cut into proper lengths, and the pins *pointed* and *headed* and completed by the motions given by the driving-shaft—both of these inventions are evidently based on that of the card-machine.

3. *On Cutting Furs from Pelts.*

The next invention I have to notice will be of interest, on account of its having proved, in an eminent degree, the parent source whence a great many other inventions have since sprung, several of which will be pointed out below,—namely the machine for cutting furs from pelts by revolving cutters arranged *spirally,* to act against a fixed *straight edge* in the way of *shears.* In the year 1810 the model of a fur-cutting machine was forwarded to me in London by a Company in Boston, which had a patent for it in America, with instructions for me to obtain the English patent, and introduce it to the notice of the hat-manufacturers of this country, by whom it was supposed the machine would be largely used, as it had been by the same trade in America. In transmitting the model to me it was stated to have been the invention of a Mr. Bellows, a shopkeeper of Boston, of whom I had never before heard; and not having then or since received any direct communication from or concerning him as the inventor, I have no means of forming any opinion as to his being entitled to the honour of originating this curious and important invention. I accordingly obtained the English patent; and for the construction of the machine and the principle of its action I refer to my specification, wherein they are fully set forth and explained—the purpose of this notice being rather to avoid the mechanical details, and speak of the *principle* of *shearing* by means of a series of spiral cutters so fixed as to revolve round a common axis, their cutting edges moving in the line of a cylinder, so that one cutter shall come into contact with the fixed straight cutter, just as the preceding spiral cutter shall pass the other end of the former: thus the revolving cutters will come into successive action with the fixed straight edge; and it will be obvious that any *fibrous* or other yielding substance, being brought into contact with the straight edge, will be taken hold of between it and the revolving cutters, and thus cut or sheared off from the surfaces to which the fibres were before attached, such as furs from pelts, and many others to be noticed further on.

When the patent was obtained I had a machine built, of full working-size, and got it put into operation at the large hat-making works of the Messrs. Hicks in the Borough. Some months after, those gentlemen reported that the machine appeared to work to *their* entire satisfaction, but not to their workmen's! These latter had a sort of monopoly in the art of cutting the furs by hand-shears or knives, and they would not stand having the machine-cutting continued in the works. It finally appeared that the cost of cutting the furs by hand was too small an item in the general expenses of hat-making to justify any interruption of the works for the small saving to be effected by the patent machine, which therefore they declined to adopt in their establishment. Upon the conclusion of this experiment I was led to view the fur-cutting patent as an entire failure, and that to submit to lose all that had been expended on it were better than to press it further on the trade. If, therefore, the *invention* had not subsequently fructified in other hands, and for other purposes, I should never have thought it worth while to bring it into public notice beyond that unsuccessful trial.

In the foregoing account we have seen that the *inventor* of this fur-cutting machine was "unknown to fame" at the time of its transmission to England; and since then I have not received any further information concerning his being in any wise distinguished as a mechanician; yet his *name* should be recorded in the history of modern inventions, as an honoured contributor to the advance of practical mechanics, if he be the real *author* of this application of the *spiral cutters* in successive contact with a *straight one* to perform the work of shearing fibres from the surfaces to which they are attached. I should here observe that the original machine was adapted to cut the pelts or skins into narrow slips from the furs, rather than the latter from pelts; but it soon became evident that this principle of shearing was equally applicable, and would prove of greater value, when applied *to shear the fibres off from surfaces without injuring them.*

About six months after my specification of the patent appeared in the 'Repository of Arts,' I found that a patent had been taken out for *chopping straw, roots,* &c., by employing the same action of revolving spiral cutters against a fixed straight cutter; and this patent had a great run as a useful agricultural implement, which, as I was informed at the time, yielded a large profit to the inventor; and, in the ordinary acceptation of the term, it was *an invention.* But the principle of it was evidently derived from the fur-cutting machine; and doubtless, if I had then possessed the art and mystery of *claiming* under patents for *one purpose* their application to many others, I might have then forestalled all of the subsequent uses of the *revolving spiral cutters* which were successively patented, besides that for straw-cutting—namely two or three patents for shearing the naps from cloths: some of these were very ingenious and valuable machines, but they are direct copies from the fur-cutting patent. This same principle has been applied in various ways since in preparing *dye-stuff,* paper-making, and others; but without stopping to notice them separately, it will suffice to mention the *lawn-mowing machines,* now much in vogue, which are from the original fur-cutter.

VIII. *On the Amount of Carbonic Acid contained in the Air above the Irish Sea.* By T. E. Thorpe, Assistant in the Private Laboratory, Owens College. Communicated by Professor H. E. Roscoe, F.R.S., &c.

Read November 28th, 1865.

The determination of the amount of carbonic acid contained in the atmosphere over the land has been made the subject of investigation by many experimenters; and from the results obtained by Théodore de Saussure, Brunner, Boussingault, Angus Smith, and others, we are acquainted with the exact proportion of this gas contained in the atmosphere under varying circumstances of situation and weather.

But hitherto the influence which, *à priori*, must necessarily be exercised by large bodies of water on the proportion of carbonic acid in the atmosphere has scarcely been sufficiently studied. The fact that a considerable influence is exercised has certainly been noticed; but beyond the incomplete results of one or two observers, we have no numerical data from which to judge of the extent of this influence, and we therefore know but little of the changes in the comparative amount of the atmospheric carbonic acid as effected by the waters of the ocean. Dr. Roscoe therefore suggested that I should undertake some experiments on this subject, and kindly placed the necessary time and apparatus at my disposal. I may here be allowed to express my thanks for the kindness and for the advice and assistance I have received from him during the prosecution of these experiments.

It was noticed long ago by Vogel (Ann. of Phil. vi. 1823, and Journ. de Pharm. t. vii. p. 461) that air collected over the Baltic, about half a mile from the coast at Doberam, contained so little carbonic acid, that baryta-water was scarcely rendered turbid by it. A repetition of the experiment made in 1822 by the same observer in the Channel, two leagues from Dieppe, by emptying a bottle of distilled water and testing the air with baryta solution, gave a perfectly similar result.

Krüger, at Rostock (Schw. xxxv. 379), also observed that the air did not render lime-water turbid when the wind came from the north, the direction of the Baltic, but that a considerable turbidity was produced by a wind blowing from the opposite direction, namely from the land.

Théodore de Saussure (Ann. de Chimie et de Phys. xliv. 1830) noticed that the air collected near the surface of Lake Sêman generally contained less carbonic acid than air taken at Chambeisy, half a league distant.

The difference, however, is but slight: the means of eighteen determinations, made at different seasons of the year and at various times of the day, gave for the air of the lake 4·39, for the air of the land 4·60 in 10,000 volumes of air, or as 95 to 100.

Watson (Brit. Assoc. Reports, iv. 1835, and Journ. für Prakt. Chemie, iii. 75, 1835) likewise found from determinations made near Bolton, that winds from the seaward contained less carbonic acid than when blowing from the land. Thus, on Nov. 6th, 1833, at 2 P.M., after much rain, and with a very strong west wind, the air contained 3·614 vols. in 10,000 of air, and on Dec. 6th, in precisely similar circumstances as to wind and rain, exactly the same amount. Southerly and easterly winds from the land gave numbers varying from 4·196 to 4·730 in 10,000 vols. of air. These determinations were made by absorbing the carbonic acid in a known volume of air by means of a measured quantity of lime-water of known strength. After the expiration of a week, during which the vessel was frequently agitated, the residual lime-water was neutralized by sulphuric acid of a certain strength; and from the amount of that acid required before and after absorption, the proportion of carbonic acid in the air was easily calculated.

A few experiments made in a similar manner by Colonel Emmet (Phil. Mag. xi. 1837) at Bermuda, about the end of September and beginning of October 1836, gave as a mean 1·25 vol. of carbonic acid in 10,000 vols. of air—a result considered, however, by Dalton, from whom the communication was received, to be only approximative, since the lime-water had not remained for a sufficient length of time in contact with the air. Emmet observed qualitatively, by means of lime-water, the invariable presence of carbonic acid in the atmosphere above the sea during the voyage out from England to Bermuda; but the quantity apparently fluctuated, the film of carbonate forming sometimes more rapidly than at others.

These old observations are, however, scarcely to be trusted as regards quantity, owing to the inaccurate nature of the methods employed. We now have to notice more recent and reliable determinations.

It appears from the experiments of Morren (Ann. de Chimie et de Phys. xxxiii. 12, 1844), made near St. Malo, on the French coast of the Channel, on the nature of the gases which sea-water holds in solution at different periods of the day and during the various seasons of the year, that the alteration in the composition of these dissolved gases may possibly cause a sensible alteration in the composition of the atmosphere immediately above the sea. The air contained in sea-water consists of variable quantities of free carbonic acid, oxygen, and nitrogen,—the changes in the relative proportion of these gases depending (1) upon alteration of temperature, affecting the relative amounts of the dissolved gases in accordance with the laws of gaseous absorption, and (2) upon the variations in intensity of direct and diffused solar light, producing a corresponding effect upon the vitality of sea-plants and animals, and hence altering the composition of the dissolved gases.

These conclusions of Morren are substantially confirmed by Lewy's experiments (Ann. de Chimie et de Phys. xxxv. 17, 1846) on the composition of the dissolved gases in the water of the Channel at Langrune (Calvados); but the two chemists differ somewhat in their statements of the extent of the variations in the composition of the dissolved gases. The limits of variation for the oxygen, according to Morren, are from 31 to 39 in 100 volumes of the gases; according to Lewy from 32·5 to 34·4. As the same quantity of gas, however, is not always evolved from the same volume of sea-water, but sensibly varies during the day, it is better, in stating the differences between the results of the two observers, to compare the absolute amounts evolved from the quantity of water employed in the experiments. Thus, according to Morren, the amount of oxygen varies from 29·7 c.cm. to 53·6 c.cm. in 4·5 litres of sea-water, the amount always taken for experiment; whilst Lewy finds from 23 c.cm. to 29·1 c.cm. only per 4·45 litres. In regard to the carbonic acid, the variations, according to Morren, are from 7·5 c.cm. to 21·0 c.cm. in 4·5 litres of water; according to Lewy, from 10·6 c.cm. to 17·5 c.cm. in 4·45 litres.

Morren's determinations were made in the months of March, April, and May; those of Lewy in August and September; but this circumstance will hardly serve to explain the disagreement in the above results, since, as the latter chemist points out, it is scarcely possible that the difference in the seasons can exercise so great an influence.

Both chemists, however, agree in attributing the observed differences in the composition of the dissolved gases in great part to the influence of direct and diffused solar light on the microscopic animalcula which, according to Ehrenberg, are always present in sea-water—basing their conclusions on the examination of the water left by the recession of the tide in the hollows of the rocks or shore, in which infusoria develope with great rapidity, and in which consequently under favourable circumstances, all these phenomena are eminently exhibited.

If it is possible that the composition of the air above the sea in our latitude can be sensibly altered by this phenomenon of the variation in the nature of the gases in solution in sea-water, it is reasonable to expect that the atmosphere above the tropical oceans would manifest to a much larger extent variations in the relative amounts of carbonic acid and oxygen, since infusoria exist, as is well known, in enormous quantities in these oceans, and the composition of the air in their waters must necessarily undergo rapid variation, and a consequent evolution of the dissolved gases occur. Some experiments by Lewy on the composition of the air of the Atlantic Ocean (Ann. de Chimie et de Phys. xxxiv. 14) tend to confirm this opinion. At the instance of the French Academy, Lewy collected air at different times during a voyage from Havre to Santa Marta. On subsequent analysis, the air collected during the day appeared to be sensibly richer in carbonic acid and oxygen than air collected in the night. On comparing the means of each series, we have, in 10,000 volumes of air, for

	The day (mean of 7 expts.).	The night (4 expts.).
Carbonic acid........	5·299	3·459
Oxygen	2105·801	2097·412

This variation appeared to increase in proportion as the middle of the ocean was approached; and in air collected on December 18th, 1847, at about equal distances from Africa and America, the widest difference was observed:—

Hour.	Temp. of air.	Temp. of sea.	Weather.	Lat. & long. W. (Paris).	Distance of continent	Means of 3 expts.	
						CO_2.	Oxygen.
	C.			° ′ ° ′	leagues.		
3 A.M.	21°·5	not given	Fine breeze, unclouded.	21 45 41 3	435	3·346	2096·139
3 P.M.	24°·0	24°·5	Do. slightly clouded...	21 9 42 25	412	5·420	2106·099

The extent of this variation, as seen from the above Table, is 2·074 for the carbonic acid, and 9·960 for the oxygen in 10,000 volumes of air,—without doubt a very appreciable difference.

This remarkable phenomenon may doubtless be accounted for, without any reference to the direct action of infusoria, by the heating effect of the sun on the sea-water, and the consequent disengagement during the day of gas proportionately rich in carbonic acid and oxygen. During the night, on the other hand, as this source of action is removed, the disengagement may be assumed not to occur; and, following Lewy, one may perceive that this difference would become more appreciable and easier to trace in air at great distances from any continent than in air collected nearer the coasts, and consequently liable to be mixed with the air of the land.

The specimens of air for the above experiments were collected in glass tubes of about 100 c.cm. capacity, in the manner prescribed by Regnault in his "Instructions" to the sailors and others who aided him in collecting the air for his memorable research on the composition of the air at different parts of the globe. The analyses were executed eighteen or twenty months after, with the eudiometric apparatus of Regnault and Reiset; and the results are the means of three separate determinations of each specimen of air. The precision of the results in the case of carbonic acid is somewhat remarkable when we consider the difficulty generally experienced in accurately noting contractions so minute as the absorption of the carbonic acid from a small volume of atmospheric air, and when we remember the fact pointed out by Regnault in the investigation above mentioned (Ann. de Chimie et de Phys. xxxvi. 1852), that air which has remained for any great length of time in glass tubes invariably exhibits a notable diminution in the amount of carbonic acid, since the glass absorbs a portion of this gas.

The kind permission of the Honourable Board of Trinity House has enabled me, during the vacation of last summer, to make some additional experiments in this direction, on board the Bahama-bank light-vessel, situated in the Irish Sea, lat. 54° 21′, and long. 4° 11′, seven miles W.N.W. of Ramsey, Isle of Man, and consequently nearly equidistant from the nearest shores of England, Scotland, and Ireland. The ship is placed to mark the proximity of a dangerous bank, by which, for the greater part of the day, a strong current, setting in from the southward, flows through the north channel, and thence into the Atlantic.

These experiments were made in the early part of August, at the same periods of the twenty-four hours namely, about 4 A.M. and 4 P.M., or nearly the times of minimum and maximum temperature.

Pettenkofer's method of analysis was adopted, with the improvements in the practical details suggested by Angus Smith (Proc. Lit. and Phil. Soc. Manchester, 1865). This method is in principle similar to the one employed by Watson and Emmet, but admits of far more delicacy and precision in practice. Baryta- is substituted for lime-water, and oxalic for sulphuric acid. The solution of oxalic acid for these experiments was so made that one cubic centimetre of it corresponded to one milligramme of carbonic acid; it thus contained 2·864 grammes of pure crystallized oxalic acid per litre. Twenty-five cubic centimetres of the baryta solution were originally made to correspond to about twenty-eight of oxalic acid; but of course the exact strength of the baryta-water was ascertained previously to each experiment.

The bottles were generally filled with the air by means of bellows; but sometimes, when the wind was strong, it sufficed to hold them up for a minute or two in such a manner that the air could circulate freely within. The baryta-water remained in contact with the enclosed air for three-quarters of an hour to one hour, during which time the bottles were frequently agitated. Although even this is longer perhaps than is actually required for the complete absorption of the carbonic acid, still, for the sake of conclusiveness, in experiment 4 the bottles were allowed to stand for three hours, and in experiment 13 for six hours, before the solutions were tested. The capacities of the two bottles which served for all the experiments were 4815 c.cm. and 4960 c.cm. The burette was Mohr's modification, for which a table of calibration had been constructed by weighing and interpolating in the ordinary way.

As an example of the process and mode of calculation, take the experiment where the baryta-water remained in contact with the air for six hours:—

Aug. 17th; time, sunrise; bar. 753·1 millims. Temperature: dry bulb 13°.9 C., wet bulb 12°.8 C.; wind W.S.W., fresh. Cloudy, amount of cloud (overcast = 10) 9; nature of cloud, cirrostratus. Temperature of sea-water, 15°.0 C.

50 c.cm. of baryta-water before experiment were equal to 55·18 c.cm. oxalic acid. After the expiration of the six hours, 25 c.cm. of the baryta-water required 26·29 of oxalic acid solution for neutralization; therefore the 50 c.cm. originally taken would require double this quantity, namely 52·58 c.cm.; and $55{\cdot}18 - 52{\cdot}58 = 2{\cdot}60$. But as one cubic centimetre of oxalic acid solution is made equal to one milligramme of carbonic acid, this number corresponds to 2·6 milligrammes of carbonic acid in the amount of air taken

for experiment. The capacity of the bottle was 4815 c.cm.; from this is to be subtracted the volume of air displaced by the baryta-water, namely 50 c.cm., and we have 4765 c.cm. as the amount of air containing 2·6 milligrammes of carbonic acid. We have now, with the temperature and pressure of the air, and weight of one litre of carbonic acid at the standard temperature and pressure (namely 1·966 gramme), all the data from which to calculate the proportion in volumes of carbonic acid in 10,000 volumes of air. The calculation stands thus:—

$$\frac{760 \times 2{\cdot}6 \times 286{\cdot}9 \times 10{,}000}{753{\cdot}1 \times 273 \times 4765 \times 1{\cdot}966} = 2{\cdot}94 \text{ in } 10{,}000 \text{ vols. of air.}$$

The fact that the various meteorological changes influence to such a remarkable extent the nature and amount of the gases dissolved in sea-water renders it necessary, in any investigation on the constitution of the atmosphere over the sea, to take particular account of these meteorological changes. Accordingly the temperature and pressure and degree of humidity of the air, direction and force (estimated, Beaufort's system) of wind, amount (estimated, overcast = 10) and nature of clouds, and general appearance of the day, together with the temperature of the sea-water and amount of sea-disturbance (1 to 9), were noted at the time of each experiment.

The following Table shows the results of those observations, together with the amount in volumes of the carbonic acid in 10,000 volumes of air. All the experiments which were made are here given. The hours of observation, as before stated, were 4 A.M. and 4 P.M.

TABLE OF RESULTS.

No.	Aug. 1865.	Night or Day.	Bar. Mm.	D. Bulb.	W. Bulb.	Temp. of Sea.	Direction and Force of Wind.	Sea Distrb. (1 to 9).	Amount & Nature of Clouds. (Overcast = 10.)		Carbonic Acid.		Remarks.
											1st Ex.	2nd Ex.	
1.	4th.	D.	762·5	16·4° C.	11·1° C.	16·0° C.	N.W. by W. v. light	calm	cirrus	1	2·66	3·07	Day very fine and clear.
2.	4th.	N.	762·0	13·9	12·9	15·0	S.W. by S. light br.	1	cirro-cumuli	9	2·92	3·05	
3.	5th.	D.	761·2	16·1	14·4	15·0	S.W. by S. light br.	1	cirro-cumuli	9	3·08	3·21	Not much sun.
4.	6th.	N.	753·4	14·2	13·3	15·0	S.W. by W. light	1	cirro-cumuli	9	3·30	3·22	Baryta-water exposed 3 h.
5.	7th.	D.	757·5	17·2	15·1	15·6	N.W. light	1	cirro-cumuli	6	3·20	3·15	Sunny; very fine.
6.	7th.	N.	760·2	13·6	12·2	15·0	N.N.W. moderate	3	cirro-cumuli	8	3·06	3·19	
7.	8th.	D.	761·0	18·3	13·1	16·0	N.N.W. light br.	1	cirrus	1	3·32	3·02	Fine and sunny.
8.	8th.	N.	758·7	13·3	12·2	15·0	S.W. by W. light br.	1	cirrus	7	2·93	3·10	
9.	9th.	D.	756·4	15·0	...	15·0	S. by W. moderate	1	cirro-cumuli	7	3·09	3·23	Rain.
10.	10th.	D.	749·3	15·0	13·9	14·5	S. by W. fresh	4	nimbus	9	3·11	3·11	Very wet; rain all day.
11.	11th.	N.	750·5	13·4	11·9	14·5	S.W. by W. strong	5	nimb.&cirro-cu.	7	3·09	3·10	Very windy and much rain from the 11th to the 16th.
12.	16th.	D.	752·3	14·7	12·8	15·0	N.W. by W. light	2	cirro-stratus	8	2·93	2·95	
13.	16th.	N.	753·1	13·9	12·8	15·0	W.S.W. fresh	4	cirro-stratus	9	3·12	2·94	Baryta-water exposed 6 h.

Day, mean of 14 determinations .... 3·086
Night „ 12 „ .... 3·085

In comparing these results with the following determinations of the carbonic acid contained in land air, it is seen that the air of the Irish Sea contains a much smaller proportion of carbonic acid than the air of the neighbouring land. The most extensive observations on the land air have given as means:—

Observer.	Locality.	No. of Expts.	Vols. in 10,000 of air.
Th. de Saussure,	Chambeisy,	104	4·15
Boussingault,	Paris,	142	3·97
Verver,	Groningen,	90	4·20
Roscoe, 1st ser.,	London & Manchester,	108	3·97
,, 2nd ser.,	Manchester,	53	3·92
Smith,	ditto,	200	4·03
	General mean of land air		4·04
	Mean of 26 expts. on sea air........		3·086

It would also appear that no difference is discernible in the amount of carbonic acid in the air of day and night over the Irish Sea. On the other hand, from Saussure's observations a decided difference may be traced between day and night air on the land—a conclusion subsequently confirmed by several experimenters.

In noting the above mean, 3·08, and the apparent identity in the amount of carbonic acid in the air of day and night over the sea, it should be borne in mind that July and August are in general the hottest periods of the year (these months were unusually hot this year, 1865), and that consequently all the influences may be supposed at work which would tend to increase the relative amount of carbonic acid, and render appreciable any difference in the air of night and day.

The conclusions therefore to be drawn from these experiments are:—

1. That the influence of the sea in our latitudes in abs-

tracting the carbonic acid from the atmosphere is not so great as the old experiments of Vogel and others would lead us to suppose.

2. That the sea in our latitudes does not act in increasing the amount of carbonic acid in the air above the ocean, as found by Lewy over the Atlantic near the equator.

3. That the differences observed in the air of night and day by Lewy on the Atlantic, are not perceptible in the air above the Irish Sea.

4. That in the month of August 1865, the mean quantity of carbonic acid in the atmosphere of the Irish Sea was 3·08 in 10,000 volumes of air.

In conclusion, I beg to acknowledge the kind attention which I received from Captain Temple, and from his crew, during my stay on board his ship.

IX. *Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes.*— Part II. By J. C. Dyer, Esq., V.P.

Read December 12th, 1865.

On the Employment of Steel for Multiplying Engravings.

At the beginning of the present century (upon the death of Washington, and in commemoration of that event) Mr. Jacob Perkins (then a silversmith at Newbury Port, near Boston) undertook to make and supply copies of a "Washington Medal;" and as they were likely to command a large sale if *speedily* brought out, it occurred to him that this might be effected in a summary way, by the process of transferring the engraved design from prepared steel dies.

The medal (an eagle, motto, &c.) being engraved or sunk on a die of *softened steel,* the die was then hardened and served to stamp or impress the design, in *relief,* upon another softened steel die, which latter being hardened, was employed as a stamp to transfer or raise the figures on the silver, rolled very thin, to form the medals.

By this process Mr. Perkins was enabled to supply a vast number of the medals, from which he derived a considerable profit, as also great credit for his success, and encouragement to extend his new transferring process to other branches of engraving.

His next application of the principle was to the printing of *bank-notes,* with very elaborate engravings, to discourage or prevent forgeries, by reason of the cost and difficulty of imitating them by the hand-engraving of the forgers. For this purpose Mr. Perkins procured some cast-steel plates (about half an inch in thickness), and after making their surface smooth and level, he subjected them to the *decarbonizing process* (explained in his patent), by which the surfaces, to the depth of about $\frac{1}{16}$ of an inch, were converted into *very soft and pure iron.* On these were then engraved, by hand, the letters and designs for the bank-notes, and the entire surface of the plate was covered with minute *letters* or *figures,* to render the bare labour of counterfeiting them a great hindrance, as well as that of the difficulty of imitation. The steel plates so prepared and engraved were next *recarbonated* by his process of cementation with animal carbon, and then they were hardened and tempered for use.

But instead of printing the notes from these plates, they were used as dies for making others to print with. Thus he prepared a cast-steel cylinder (its circumference equal to the length of the plate), which in like manner was decarbonated at the surface, and then mounted in an apparatus adapted for turning it over the engraved plate, under a very great pressure (effected by compound traversing levers), whereby the letters and figures engraved on the hardened plate were taken up in "relief," or *raised* on the surface of the soft cylinder. This cylinder being then hardened and tempered, was used to transfer, by means of the same traversing pressure, the entire work upon its surface to any number of copper plates for printing the notes—as in the case of prints; where a great many were wanted, then the cylinder was used to transfer the figures on it to softened steel plates, which, being then hardened, served to give a vast number of impressions before it became "the worse for wear."

The experiment by two or three banks of having their notes printed from transferred engravings "on Perkins's patent process" was so far successful, that subsequent forgeries were wholly upon the other banks, whose notes contained merely the requisite letters and some small design or figure to distinguish them, which work could be readily imitated, with slight labour or skill, by the common graving tool in the hands of the lowest class of artists. This, therefore, led to an extended demand for the application of Mr. Perkins's plan of transferring engravings of very difficult execution by hand, and of great cost to the forger if so executed. To supply this demand from many banks, Mr. Perkins greatly extended his works, and was in a good way to obtain a fair return of profit, as well as of honour, for his invention, the practical value of which had thus been *proved*. But his path was then beset by opponents, who professed to have made the same discoveries, and to have performed the like process of transferring engravings before the date of the patent to Mr. Perkins, and on which pretence they proceeded to make bank-notes in opposition to, and in disregard of his patent right; and as this right could only be sustained by expensive lawsuits, he became entangled with troubles and conflicts, which for a long time impeded his labours, and greatly restricted his means of completing the many other schemes then conceived by him, and waiting to be matured.

In the year 1809 Mr. Perkins communicated to me the entire details of his method of preparing the steel dies and transferring engravings for the prevention of forgeries, and for multiplying engraved designs, with a view to having the invention patented in England for our joint account. From the success attained in America, he anticipated the adoption of his system by the banks in this country, especially as it was susceptible of great improvements in the style of work to be printed on the notes, from the high state of the graphic arts in London, as compared with what could be formed in America. Accordingly I took out patents, and minutely specified the method of carrying the invention into effect. I then obtained a very elaborate and beautiful design, from the classic pencil of the late Mr. Robert Smirke, P.R.A., and had it engraved by Raimbach on *prepared steel,* to be transferred to plates for printing bank-notes. But after many fruitless efforts to induce the banks of England and Scotland to take up the plan for their notes, I was obliged to give up the task as quite hopeless at that time; nor could I then induce the booksellers to adopt the transferring system for illustrating works for the press, for which it was so well adapted, as it has since proved to be.

The saying that "there is a time for all things," seemed to apply in this case, and to show that the time had not arrived for exciting any general or strong interest on the question of the "bank-note forgeries," then so extensively practised, owing to the slight labour or ingenuity required to counterfeit the "one pound notes" in general circulation; wherefore the frequent hanging of men for a feat so easily performed was suffered to continue for many years, without any loud calls upon *the Bank* to take some steps to remedy, or at least to check, an evil so extensive and so disgraceful to the Bank, as being the *source* of the paper circulation of the whole kingdom. If any excuse can be offered for such neglect, it may be said that, amidst the vicissitudes of the pending war, and the embarrassments consequent on the transition from war to peace, which for many years caused great disturbances in the circulating medium and in the general interests of commerce and industry, it was difficult to awaken and concentrate public attention upon this great scandal, of relying solely upon the *gallows* for preventing forgeries.

From such considerations it will be obvious that my feeble voice (with those of others who had proposed different plans for the prevention of forgery) could not avail, even for getting *any trials made* to test the efficacy of "Perkins's transferring process," and I therefore gave it up as a hopeless task, and submitted to the dead loss of the money already expended for the patent, and on the experiments I had made to establish the practical working of the transferring process, and its general application for cheaply multiplying elaborate engravings.

It has been above shown that Mr. Perkins's *invention* was *not* for engraving on *steel plates for printing*, nor for engraving on steel at all, but rather for engraving on *iron* of homogeneous structure. It was found that all wrought iron is more or less *fibrous*, which rendered it unfit to receive delicate engraved work, hence it was necessary to employ cast steel, which being decarbonated to a proper depth, gave a uniform surface of *pure iron*. On this surface the letters and designs were then engraved, and the surfaces were reconverted into steel for use; that is, to be employed as dies for transferring, or for printing direct from them, as before mentioned.

This restatement of the nature of the invention seems called for, since it appears that many persons have supposed that "the invention of Perkins" was merely the substitution of steel in place of copper for engraving upon. In fact, such a bare substitution of the one metal for the other would be no invention in any fair sense of the word. But the methods of obtaining the soft iron surfaces to receive the work on them, the converting of these surfaces back into steel, and the transferring the engravings to other plates for printing, comprise together a series of novel processes, which will confer lasting honour upon the name of Jacob Perkins as the original author of them.

It was some years before the troubles of the "transition period" had so far subsided that public attention could again be drawn to the growing evils attending the circulation of a paper currency, so easily and extensively imitated by the bank-note forgers. In the meantime Mr. Perkins had removed to Philadelphia, and formed a company there for carrying on the engraving and printing business upon his patent system on a large scale.

Having made known to him the better prospect then apparently opening for the adoption of his plans for preventing forgery in England, I recommended his coming over himself to explain them, and aid the artist here in putting the system into working order. Accordingly, in the year 1820, Mr. Perkins came to England, and, being over sanguine of success, brought with him a large staff of able artists, mechanics, &c.; but unluckily he could not bring any *money* to aid in commencing the intended London works on his new system. He and his partners had assumed that in England capital could always be obtained for conducting any really useful works, if proved to be safe and profitable in practice. Now this matter of *proof* was the question, and proved to be a bar to success with the *monied class*; so that to me alone (not of that class) they had to look for the current expenses of their entire mission, and this I could only bear for a few months.

Besides his printing and transferring apparatus, Mr. Perkins brought over several other new and interesting inventions and discoveries, both mechanical and philosophical, which created much inquiry in the scientific and artistic circles of London. Among the mechanical novelties was the "Geometrical Lathe," a triumph of skill in forming, by cutting or etching, minute intersecting lines on cylinders for printing delicate shades of gradual tints on grounds of paper or cloth.

The new plans for engraving designs, and the many improvements he had made in the machinery for transferring by steel dies, were patented as improvements on the former patent, which had then some four years to run.

After some time the late Mr. Charles Heath, of high artistic celebrity, was induced to join Mr. Perkins, and by a purchase of a share in the patents, became an active partner in the engraving and printing works, which were then commenced in Fleet Street, London, and have since been continued by their successors.

It was found here, as in America, that to make an invention of importance, and to secure it to its author by patent, is not enough to protect him in its exercise in peace and quietness. When at length Perkins's steel engraving had been taken up by several banks and publishers, and was beginning to yield him some fame and profit, other artists soon appeared to compete with him for both, claiming to have done the same work long before him!

If the "transferring process" described in the patent of 1810 had in fact been practised before that time by the parties who, ten years later, denied his right as the original inventor, how came it to pass that no practical application of their plans for effecting the same process, nor any announcement of such, had ever come before the public until *after* those of Perkins had begun to bear fruit?

At this distance of time it were bootless to dwell on those adverse claims that served to impede the labours of Mr. Perkins in his untiring efforts to prove the value of his new system for making and perpetuating exact copies of beautiful designs for printing on bank notes, or for illustrating books.

Besides the printing on paper, as above described, Mr. Perkins's system for transferring designs and patterns has been very extensively applied (since he led the way in 1809) to calico-printing, and other ornamental fabrics, wherein his processes were directly copied by parties to whom he had minutely explained them in London. I had witnessed these communications, and warned Mr. Perkins of the danger of making them so loosely, but without effect. It seems "not worth while" to dwell on these cases now, or to name the parties so acting at that time.

In later years we have seen the transferring process employed to a vast extent in many other departments of the graphic art, such as post-office and receipt stamps and other prints, required in greater numbers than could be taken from other than steel plates or stamps.

When any important discoveries in physical science are made they never die, whatever may chance to their authors. The new facts, when placed before the public, go forth like seeds cast upon fertile soil, yielding the fruits of continual progress (in the arts of civil society) among the families of men who seek improvement. It seems only just, then, that each generation should transmit to the next some record of the names of those contemporaries to whose genius and talents the nations are indebted for such useful discoveries. Wherefore, in addition to the four distinguished inventors, whose names* I have brought (in former papers) to the special notice of the Society, I have (in the present one) aimed to place that of Jacob Perkins as a worthy contributor to the advance of those branches of art to which his original inventions have been applied.

APPENDIX I.

In tracing the progress of steel engraving, I had no thought of giving an account of the life and general researches of Mr. Perkins in the physical sciences, some of them having been long ago published and duly appreciated. Yet it may not be out of place here to refer to such of his other discoveries as became suggestive of the many improvements since made in kindred branches of knowledge.

His "Experiments on the Compressibility of Water," made some time before he left America, were for ascertaining the truth of the old Florentine doctrine, that water was a "non-elastic" body; which doctrine, founded on the Florentine experiments, was still taught in the schools and elementary works, and generally accepted. But the "Perkins's experiments" clearly established the elastic nature of water, and showed that it, like air, was compressible in volume, directly as the compressing forces. At that time Mr. Perkins fully believed this to be a *new discovery,* as he had never heard of the experiments of CANTON, made some fifty years before. Though not strictly a "new discovery," the experiments of Perkins were of high scien-

* These are, (1) *Robert Fulton,* who first succeeded in "*practical* steam navigation."

(2) *William Eaton,* inventor of the "self-acting mule."

(3) *John Heathcoat,* inventor of lace-making.

(4) *Amos Whittemore,* inventor of wire-card making.

tific value, because the compressing forces employed by Canton and by him were so widely different. Mr. Canton had applied the pressures from half an atmosphere, or from $7\frac{1}{2}$ lbs. to 30 lbs. per square inch, while Perkins used pressures from 50 to 400 atmospheres, from 750 lbs. to 6000 lbs. per inch. The same *rate of compression* appeared in all his experiments; and as this was the same as that shown by Canton, they may together be taken as proving the law of equal compression by equal forces. It will most likely be established that the same law will apply to all *solid bodies* as well as to liquids and vapours, unless the solids be disrupted by the forces. In short, that all known bodies are elastic will ere long become an admitted property of ponderable matter may be safely predicted; thus disposing of the supposed division of material ponderable bodies into elastic and non-elastic.

The apparatus employed by Mr. Perkins was, first, a cast-iron cylinder, the sides and bottom 3 inches thick, with a moveable top of equal strength. This, filled with water, had a force-pump, as in the hydraulic press, to measure the forces applied by the leverage and size of the induction pipe.

Second, a small brass cylinder, with a piston fitted to slide in it, water-tight; the cylinder half an inch in diameter, and of the length to have a column of ten inches long under the piston when drawn up to the end. The piston-rod was graduated into divisions of 100 to the inch, and a sliding-ring fitted on it, so as to be pressed up on the rod as this was forced down upon the enclosed water, thus marking the descent in $\frac{1}{100}$ parts of an inch. The brass cylinder being under the same water-pressure inside and outside, was not subject to any strain to alter its capacity.

Third, when the external pressure was removed, the water in the brass cylinder would of course expand to its former length, raising the piston, and marking the greatest compression effected.

Fourth, in each trial the diminished bulk of the water corresponded with the increased force applied; and it was found that under the pressure of 100 atmospheres the bulk of water was reduced one part in a hundred, and this rate, as before mentioned, was the same as had been shown by the experiments of Canton.

Some time after the experiments of Perkins had been repeated and publicly shown in London, a course of similar experiments were made by Professor Œrsted, who employed a stout glass vessel, and adopted a column of mercury to give the pressure, having the like inside instruments to mark the results. These being the same as those shown by Canton and Perkins, go to confirm the said law of compression.

APPENDIX II.

On Perkins's Steam-Gun.

Among the interesting inventions of Mr. Perkins were those for substituting high-pressure steam in place of gunpowder for small arms and artillery, so as to discharge projectiles with far greater rapidity and destructive effect from batteries on sea and land, than could be done by any system of gunnery then in use. This scheme attracted great public attention at the time it was brought out (in 1821).

To carry this object into effect, Mr. Perkins had devised a plan for subjecting water to a more intense heat than could be done by any of the boilers then known. He employed a great number of strong iron or copper tubes, placed near together, with their ends fastened into iron plates, and these fastened to other end-plates, formed cavities at each end of the tubes for receiving the water (by a force-pump at one end), and for emitting the steam at the other end.

This tubular boiler, with its induction and eduction chambers, was fixed in the middle of a furnace, for the heat to act directly on the *water in the tubes,* the flame circulating around them before passing off; thus, as Mr. Perkins phrased it, "the water could be made red-hot, and flash into steam of force exceeding that of gunpowder."

He had then a gun-barrel fixed, with the breech end open just opposite the valve opening from the steam-chamber, and a moving apparatus for conducting the balls in rapid succession into the space between the end of the barrel and the outlet for the steam, so that by having the balls brought between the breech and the steam-valve, the latter, opening at the same time, allowed the steam to issue and propel the balls through the gun in rapid succession, and with a force equal to the elastic pressure of the steam; and this, of course, might be continued as long as the heat from the furnace could maintain the required pressure.

By experiment he found that from fifty to a hundred balls a minute could be shot forth to the end of the trial ground (some hundred yards, I believe), and made to strike a target with a force nearly or quite equal to those projected by ordinary gunpowder. With these means of rapid supply of balls and steam, by having a number of guns, say ten, so fixed to exits of steam from one furnace and tubular boiler, it is obvious that some 500 to 1000 balls might be discharged per minute, which would constitute a very formidable battery, compared with the most rapid firing before known.

These experiments were witnessed by the Duke of Wellington and many other eminent men, who took great interest in them. The causes that prevented their adoption in the public service may admit of the following explanation:—On *ships of war*, the danger from fire, and the inconvenience of having such a furnace with its apparatus placed on deck in a position for using the steam-gun effectually, were serious objections. Again, the time required for getting up the steam, in cases of sudden encounter with an enemy, might compel a surrender before a shot could be thrown from the steam-battery; and a yet more serious objection to the plan arose from the injury to the tubes (containing the water) from the unequal heating in the furnace. The requisite heat being very intense softened some of the tubes, so that they gave way, and allowed water to escape into the fire. Although the quantity of water so escaping was too small to cause explosions, yet it could deaden the fire, reduce the supply of steam, and diminish the force of the projectiles, or entirely suspend them if many of the tubes were thus damaged. The leaking tubes could be easily replaced, and the boiler made again effective in a short time, yet "such pauses for preparation" in the midst of battle must be fatal to the suspended arm; wherefore the famous "steam-gun experiments" resulted in a loss of the heavy sums expended on them, and produced to the author nothing beyond some transient and barren fame.

The after interest, however, attached to the plan of using tubular boilers, arose from this scheme serving to suggest *the reversed use of the tubes*, viz., by employing them as flues for the furnace, to convey the heat through them to the water surrounding them in an outer boiler; the water thus heated outside instead of inside the tubes by the flame passing through them, could not injure the tubes by outer pressure; nor could the heat, so passing through them, cause any partial injury, as in the former case.

It appears, then, that Mr. Perkins's invention was not barren to the outer world, since the use of tubular boilers by him led to their extended and beneficial employment in railway engines and steam-boats, and were, I believe, first employed by Stephenson, a few years after the steam-gun experiments had been put *hors-de-combat*; and it would be safe to foretell, that this description of boiler will ultimately be extended to all high-pressure steam-engines.

NOTE.—Although it is needless to describe the process of case-hardening, so generally known, it may be well to explain that of decarbonating the steel plates for engraving; this process is as follows:—The prepared steel plates are placed in a cast-iron box, and covered about an inch deep with an oxide of iron, prepared by subjecting iron-filings to alternate wetting and drying until they are mostly converted into red oxide. Over this covering a clay luting is placed, so as to exclude the air, and the box is then placed in a furnace and kept at a red heat for about sixty hours, when the oxide in contact with the steel will have taken up the carbon from its surface to the depth of about a sixteenth of an inch, and thus convert the surface into pure iron, as mentioned in the text.

Any other oxide in the form of powder, in which the affinities are weaker than those of the oxide with carbon, might be employed in lieu of iron-filings. It is not improbable that this fact of bringing oxygen in contact with cast iron in a melted state may have suggested the Bessemer process of purifying cast iron.

X. *Questions regarding the Life-History of the Foraminifera, suggested by examinations of their dead shells.* By THOMAS ALCOCK, M.D.

[Read before the Microscopical Section, October 16th, 1865.]

THE specimens of Foraminifera which have suggested the following remarks were obtained from an extensive deposit of calcareous sand on the shore of Dogs Bay, near Roundstone. They are found in excellent condition, most of them being not at all worn and rarely broken; and their abundance may be understood from the fact that fully three-fourths of the whole mass of the sand consists of their shells. Though it may be generally true that dredging is the only satisfactory way of getting good material for examination, a deposit like this certainly forms an exception, and deserves thorough investigation; already it has yielded a greater number of distinct forms than are described by Prof. Williamson from the whole extent of the British Seas; seventy-four of them agree with forms figured in his 'Recent Foraminifera of Great Britain,' but the remainder are either very decided and remarkable varieties, or they are perfectly distinct from any there described.

This sand was first brought under my notice four years ago by Mr. Thomas Glover, who observing the great number of small shells of Mollusca it contains, brought away a supply of the material with the intention of picking them out at leisure, and gave some of it to me for the same purpose. After having worked for some time at these small Mollusca, which are distinguishable without the aid of a microscope, and having obtained in this way many beautiful and interesting species, I separated the finer from the coarser parts by means of a sieve, and gave the finer material a special examination. The extraordinary richness of this material soon became evident, and it is sufficient to say that daily examinations of it from that time to the present have not yet exhausted its novelties. Since I obtained the first sample, Mr. Glover has favoured me at various times with further supplies from this place and from neighbouring localities; Mr. Darbishire has also furnished me with a large stock of the Dogs Bay sand, and I received from the late Mr. Parry some remarkable material from the same place, obtained, however, under peculiar circumstances, having been skimmed from the surface of pools left by spring tides. In this case the sand of the shore had become thoroughly dry by exposure to the sun and air during the interval of the low tides, and when again covered by water, the lighter and more perfect of the Foraminifera floated on the surface, while broken specimens and the heavier kinds sank, so that in this way a selection was made naturally, like that resulting from the plan recommended by Prof. Williamson for obtaining specimens from samples of sand which are too poor to be worth examination in the ordinary way.

This naturally selected sample has furnished some very interesting results, by supplying great abundance of specimens of certain varieties comparatively rare in the rough sand; but at present I have seen in it only two marked forms which have not been met with in the other samples; these are two varieties of *Lagena*, namely, *Lagena crenata*, described and figured by Messrs. Parker and Jones in the 'Transactions of the Royal Society' as from Swan River, Australia, but not hitherto recorded, so far as I am aware, as recent British; and *Lagena antiqua*, plate IV. fig. 3, a form which is very distinct in character from the other varieties; it is opaque white, and appears finely granular in texture; its surface is without any raised markings, and at the base of the neck there is a projecting collar.

My present intention, however, is not to describe varieties which may have hitherto escaped notice, but to lay before you certain conclusions respecting the life-history of some of the Foraminifera which I believe may be fairly drawn from an examination of their dead shells. This is a subject which appears to me not to have received all the attention it deserves, and I am satisfied that these empty shells are capable of affording still more information than they have yet given, since from their nature they retain permanently the impression of any passing condition of the animal at the time of their formation. In the first place, it is evident that the soft and yielding body of the Foraminifer acts as a mould upon which the shell is formed, and that it must remain still and without change of shape while the process goes on; it follows, therefore, that this formation of shell, so far as the foundation layer is concerned, must be looked upon as a single act, and probably one requiring no great length of time. But if the shell be moulded on the surface of the animal, as I conclude it must be, it is clear that from the first the animal will completely fill it, and whatever growth afterwards takes place must be continued outside. This is evidently the case in the many-chambered Foraminifera, such as *Rotalina*, where an additional chamber is formed from time to time to protect the new growths; but what provision is made where the perfect shell always consists of only a single chamber? The Dogs Bay sand, and especially Mr. Parry's sample, contains in great abundance two forms of Foraminifera which I believe give some information on this subject; these are *Orbulina universa* and *Globigerina bulloides*,—the former consisting of a hollow sphere, with many small perforations, and frequently a single larger one; the latter of a graduated series of small spheres attached together, and arranged in a helix-like form. These objects are interesting as being the prevailing shells in deep-sea dredgings, and they have been noted as abundant in the bed of the sea under the Gulf Stream;

but on the shores of this country they have not hitherto, so far as I am aware, been found anywhere in plenty, though they occur scantily in many localities; and their unusual abundance on this part of the west coast of Ireland may probably be due, therefore, to the influence of the Gulf Stream. Another point of interest about these two forms is, that they always occur together; and this fact, with a general agreement in their character, has led to a suspicion, long since entertained, I believe, by Prof. Williamson, that there is some very close relationship between them. I find the *Orbulina*, or single-sphered form, of very different sizes, the largest being as much as six times the diameter of the smallest, with every possible intermediate gradation; and, considering that the shell is needed for protection and support, but can only be made of the exact size of the body on which it is moulded, while that body will continue to grow, I cannot avoid the conclusion that, in this case, as often as a larger shell is required the animal must withdraw itself entirely from the old one and cast it off. The coating of sarcode, therefore, which is usually found on the outside of the shell, is more than a mere result of the coalescence of the bases of the pseudopodia, for it consists of the whole additional growth of the animal since the shell was formed, and will continually increase in quantity, until at last a new one is required. I have one specimen where a large and perfect globe has the segment of another globe of similar size attached to one side of it, and I can account for this appearance in no other way than by supposing that instead of the animal having cast its shell as usual, the external sarcode has in this case collected itself into as much of a sphere as the circumstances would allow, on the outside of the original globe, and has there covered itself with a supplementary shell.

But a consequence of this view—that the single-chambered Foraminifera cast their shells at intervals to form new ones—is, that they must occasionally be freed for certain periods from the restraint of the shell, and be in a condition to effect that spontaneous division which is so striking a feature in the Rhizopoda. The specimens of double shells which I have observed, including six or eight of *Orbulina*, four of *Lagena*, two of which are represented on Plate IV. figs. 4, 5, and about half a dozen of different varieties of *Entosolenia*, are all of them of medium size, the pair in each case being together about equal to one large specimen of the same species; and these indicate, I believe, that here the process of self-division was arrested by the formation of the shell, probably to be again attempted with better success after the next change of shell. I have seen plenty of proof in *Truncatulina lobata* that fresh individuals may be formed by portions of the sarcode being cast off from full-grown animals, and afterwards covered by a shell; for specimens are not at all uncommon consisting either of a single chamber or of a group of two or three chambers, perfectly agreeing in their general character with the shell of *Truncatulina*; but I have seen no proof that this happens with the single-chambered forms, though I have one curious specimen of a *Lagena* (Plate IV. fig. 6) with a second very small one attached to its mouth, which might at first sight be looked upon as a proof that an off-shoot was here being detached, but before it could get free had become fixed to the parent by a premature formation of its shell. No specimens, however, anything like so small as this, have been found loose in the sand, which they ought to be if this were at all a usual mode of increase; and the interpretation of the specimen is, I believe, that the animal, instead of withdrawing itself from its shell, either to form a new one of a larger size or to divide, has taken the unusual course of adding a supplementary chamber, just as in the instance before given of the *Orbulina* with a segment of another globe added to its surface.

But there are other specimens of *Orbulina* found occasionally in the Dogs Bay sand which have a very special interest. D'Orbigny divided the shells of the Foraminifera primarily into two groups, according as they were formed of one or of many chambers, the former being called monothalamous and the latter polythalamous shells; but this division, though apparently so natural, has proved unsatisfactory, its effect being in many cases to separate widely apart forms which are closely related; but in no case is it more clearly shown to be untenable than in *Orbulina* and *Globigerina*, which are proved, I believe, by the specimens now to be described, to be simply different states of one and the same species. That this is the case was announced in 1858 by L. F. Pourtales in 'Annals and Mag. of Nat. Hist.,' his specimens illustrating the fact having been obtained from dredgings in the Gulf Stream. Dr. Carpenter mentions this announcement, but, after stating that he had himself looked in vain for appearances like those described, gives reasons why, as he believes, the observations are not likely to be correct; these reasons, however, are not unanswerable, and they lose their weight altogether in face of the specimens themselves, which show most indisputably the perfect *Globigerina* inside the sphere of the *Orbulina* (Plate IV. fig. 1). Pourtales explained this appearance by supposing that the *Globigerina* is the young of *Orbulina*, and is developed within it, until at last the parent-shell breaks to allow its escape; but there are many specimens from Dogs Bay which will not admit of this explanation, though they suggest a different one, and this equally applicable to them all. In the peculiar specimens to which I allude (Plate IV. fig. 2), the external appearance is that of a sphere made irregular by several rounded projections, or portions of smaller spheres; but when they are viewed by transmitted light, they are seen to consist of a collection of spheres of graduated sizes, the largest nearly, but not quite, enclosing all the others. In the ordinary *Globigerinæ*, the chambers are formed in succession, as they are needed to protect additional growths of the animal, the sarcode collecting from the outer surface of the already formed shell into the globular shape peculiar to the species, and placing itself in advance of the other chambers; but in the examples now under consideration this sarcode retains its position as a coating over the whole surface of the existing shell, and *there* acquires its shell-covering; the only difference, therefore, between these globes with an irregular surface and those having the *Globigerina* completely enclosed within the outer sphere, is dependent on the greater or less amount of sarcode requiring to be covered by this last chamber. In both cases alike the next enlargement needed will involve the withdrawal of the animal from the shell and the formation of an entirely new one, which will then appear in the characteristic form of *Orbulina*, as a simple hollow sphere.

XI. *On Air from off the Mid-Atlantic, and from some London Law Courts.* By R. Angus Smith, Ph.D., F.R.S., &c., President.

Read February 20th, 1866.

As my friend Mr. Alfred Fryer was going to the West Indies and America, I made up a box of tubes to hold specimens of air, adding also apparatus for its collection. He has brought me back some of the tubes filled; the rapidity of his movements prevented him from obtaining many. As Mr. Fryer is known to be a skilful experimenter, we may be sure that the specimens are well preserved.

The air from off the Atlantic is seen to contain more oxygen than any of the others. I did not expect that with such a small number any average could be obtained that could be usefully compared with other results; but we find here that the amount of oxygen is almost identical with that found by me in the air on the sea-shore and open heaths of Scotland, and the amounts found by others in places where the best air was obtained. In other words, this air stands in the first class as regards oxygen, and we could expect nothing less.

The amount of carbonic acid could not be taken with confidence in the small quantity of air at command.

Perhaps in making these experiments we ought to be more careful in following the movements of nitrogen also. We are apt to allow that this gas makes way for the others, and undergoes no change itself. Of the three important gases of the air, this is the least liable to change. If the nitrogen remains constant, or nearly so, and the oxygen varies as much as I believe proved, is it only to make room for the constant quantity of 200, 300, or 400 of carbonic acid in a million, as always found? By more careful observation we should add to our ability to say if the oxygen is found in the same condition as oxygen gas usually is, or if it is condensed, either by being combined or changed allotropically; we are in fact groping about for some powerful oxidizing agent which most persons allow to exist in the air in varying amounts more powerful than ordinary oxygen, although acting as oxygen. We are much in the condition of Hooke and Mayow when looking for oxygen, they found what they called a nitro-aërial principle in the air; in other words, that which is the characteristic of nitre. The question whether the powerful agent alluded to is ozone or some other thing is a minor one, although of the greatest interest. Although the amount is small, we can perceive it in breathing with the greatest ease; the effect is produced on the external senses, and then on the spirits, and on the state of the whole mind. With some persons the exhilaration is greater than suits the health in other respects.

When we look at the analysis of the air from the island of Antigua we find some loss of oxygen; this corresponds to the outer circle of Manchester during dry weather, but not quite equal to the same in wet weather. In Antigua the morning was showery when the specimens were taken —April 11th, 1865, at 9 A.M.

It is interesting and important to know that we can trace these small changes. It is probable that to them in part is due the character both of body and mind, not merely found in races, but in sections of the same race, separated perhaps by a hill or a stream, or raised from the ground by a difference sometimes of a few feet only, although at times hundreds or thousands.

Oxygen per cent. in some Specimens of Air.

18 ft. above water. Fine day. 2.30 P.M. Lat. N. 43° 5′, Long. W. 17° 12′.	St. John's, Antigua. April 11th, 1865, 9 A.M. Showery morning.
21·0100	20·9600
21·0000	20·9100
20·9700	21·0000
Mean 20·9900*	Mean 20·9500

Law Court, Feb. 2nd, 1866.	Law Court, from the lantern. 4.30 P.M., just as the Court was closing.
20·6400	20·5000
20·6700	20·4800
Mean 20·6500	Mean 20·4900

During a visit to London a scientific friend called my attention to a law court which was badly, or rather in no-

* May be read 209,900 in a million, and so with the others.

way ventilated; and as I was very ready to please him, as well as desirous of increasing my list of analyses, I collected specimens.

The court was extremely warm and unpleasant at the moment of entering, and even after some minutes it was not to be voluntarily borne; I therefore did not attempt to penetrate the mass of people, but took specimens of air when perhaps eight feet from the door. On coming out, the feeling of relief was remarkably pleasant. This feeling, as elsewhere explained, is usually accompanied with a restoration of the normal action of the heart, and a calmer respiration.

The amount of oxygen in places not mountainous is given by me as 20·978—an average from many analyses. London always stands well in examinations of air, and the parks will contain about 20·9800, and sometimes more, judging from the carbonic acid of which estimations have been made, leaving out the oxygen. We have then 209,800 of oxygen in a million, but in the law court only 206,500, or a loss of 3300 in a million. Examining the Tables to which I have already alluded, we find no place above ground with such a small amount of oxygen, except the gallery of an extremely crowded theatre at half-past ten at night, when the whole evening had been spent in spoiling the atmosphere, and those places at the backs of our houses, which we are not expected to name, much less to inhabit. Although by analysis these places were as bad as the court, in reality they were less so, as the court temperature was very high, and the organic matter from perspiration in proportion. The deleterious effects of this we are not yet able to judge of, the other we can to some extent measure. I say deliberately, that this court where I took the air was worse than the middens alluded to.

The warmer air rises, and that at the ceiling is generally the worst. This, however, depends upon circumstances;

if it has time to cool, from the height and space being great, the carbonic acid may be arrested before reaching a great height.

If from a space filled with warm air in which many persons have breathed we fill a flask and weigh it, we shall find that, unless the carbonic acid is unusually great, the weight is less than the weight of the same bulk of air taken before it was warmed by human beings. If we shut up the space and allow it to cool to its first temperature, and weigh a similar bulk of air, we find that it is really heavier than it was at first. Fortunately the warmth raises the air above us, and it seeks an exit away from our lungs; so that air rendered in this way impure is made lighter, but as soon as it cools, it is heavier than at first and falls down. To ventilate well, the air must be removed before it cools, and the heating, cooling, and ventilating must work in harmony. It is not easy to bring these agents to act so.

The air raised into the lantern above the court was inferior to that below, and contained only 20·4800 of oxygen, being a loss of 5000 in a million. Nature never seems to offer us air with a loss of even 1000 in a million. Comparing healthy places with healthy, the difference is about 200, and perhaps this indicates a similar difference of vital principle in a climate.

I need scarcely say that I found no such loss of oxygen in the mills of Manchester, or in any other inhabited place above ground during the day. If we seek air similarly degraded, we must descend the shafts of mines, and there we find oxygen removed in some places to a much greater extent. As an average, however, the currents in a metalliferous mine gallery contain 20·6500 of oxygen, exactly the amount in the court, and the air under the shafts 20·424, almost exactly the amount in the lantern. I certainly am anxious to see legislation in favour of miners; but this is a circumstance rather adverse to my hopes.

The organic matter, apart from the carbonic acid, is a subject still requiring much labour. On the windows in the upper part of the court there were streams of liquid; the hot vapour rising up in the court was cooled on the large surface of glass, and two or three ounces of the liquid were readily obtained. Many years ago I had examined this liquid, and found that it nourished microscopic vegetables and animalcules; when working with mine air, it was found that it had a strong smell of perspiration. In this case there was no mistaking the odour. The liquid was not purely from the air of the court, it had washed the windows, and of course taken dust with it and some soot. It would have been much better if the condensation had been effected in a purer vessel by means of ice, which I hope to do. Nevertheless this liquid showed its origin with distinctness.

It contained a considerable amount of organic matter, some of it of a fatty kind. When examined with a microscope, it showed numberless floating bodies; most of them looked like separate cells, some of them with central spots, indicating organic structure; but I was not able distinctly to identify any as being exactly similar to any either in the 'Micrographic Dictionary' or Pritchard's volume.

I do not suppose that any of these germs came from the breath or perspiration of individuals in court, but I mean to say that they would find nourishment there, and might probably increase. The existence of these germs in that place, although not taken from the air, shows them to have been carried by the air, and if so, capable of entering our lungs, depositing themselves in all places, and carrying out their character, whether that be for good or evil. It will be readily seen that merely to refill a room with air is not ventilation where much of this matter exists. It can be removed only by abundant streams of air acting for a long time. If the house is shut up, the matter grows till the air is so filled as to smell musty, although there may be a full measure of oxygen present; but if it is to be purified, the wind must blow and oxidize at the same time that it carries away mechanically. And if it removes, it must remove it to a spot. It is difficult to escape the presence of these minute bodies.

One plant seemed to me to resemble the yeast plant, and one to resemble that found during the formation of vinegar; but neither I nor Mr. Dancer could on trial prove any fermentation. When treated with permanganate of potash, 150 grains took 0·6 cub. centim. of a solution; some days afterwards 0·7 without acid; with acid the same amount took 1·1 cub. centim. of permanganate, and after ten days 1·9. It was undergoing some chemical action. The commonest observation would lead any one to say with Bacon, "out of question it is man's sweat putrefied."

XII. *On Minimetric Analysis.*

By R. Angus Smith, Ph.D., F.R.S., &c., President.

Read April 4th, 1865.

Tests for Carbonic Acid and of Ventilation.

Although the only impurity in air is not carbonic acid, as a rule the best chemical test for ventilation of rooms rendered impure by exhalations from the person is the presence and quantity of this gas. It will be seen in a former paper that baryta- and lime-water were tried for a long time in various ways, and after various stages became accurate in the able hands of H. Saussure; simple, and in theory completely accurate, in the hands of Mr. Hadfield; and at last, by the greatest refinement, were used as scientific instruments by Pettenkofer.

It is not pleasant to speak of the history of discoveries, as we so often find that much that is discovered is so for the second time and not for the first; but it is extremely improbable that Pettenkofer knew of Hadfield, and it is very probable that Hadfield, whom I long knew, could not have carried out the refined experiments of Pettenkofer. Besides, the use made of the instrument by the Munich professor is more important than the instrument itself.

It was one of my duties, in connexion with the Royal Mines Commission, to examine into the subject of tests, in order to find a simple method of determining the value of the air in mines. I saw clearly that my test for oxidizable matter was valueless in such places, and Pettenkofer's could not be used comfortably, or at least would not be used. More simplicity was required. There must be little to carry, little to do, and little to think of. Nothing better than baryta or lime suggested itself. The comparison of precipitates of lime, as Dr. Boswell Reid recommended, failed long ago, because the precipitates changed in physical appearance; but his mode of keeping the extent of the precipitate in the memory did not exactly fail, and was to be considered correct or otherwise, according to the memory, and according to the frequency of the experiment.

Equal quantities of baryta-water were poured into two bottles; air was blown into them from the lungs until a decided precipitate formed, equal in both cases. The amount of precipitate was estimated by testing the amount of baryta still in solution. When this was done several times by two persons, the results were almost absolutely the same. Next day, these same two performed the experiment, relying on the memory of the precipitate of the previous day; and the results were that the oxalic acid required was 23·7 cub. centims., 23·2, and 23·2. The difference in one case is 0·0005 gramme of carbonic acid, as every cub. centim. of the oxalic acid solution was equal to 0·001 gramme of carbonic acid. This was repeated times without number, and served as a basis for a new mode of using the baryta- and lime-water test. To this method of analysis I have given the name *Minimetric*. We ascertain the smallest amount of air required to produce a precipitate of a given density.

The same method can be employed to determine hydrochloric acid, sulphuric and sulphurous acids, sulphuretted hydrogen, &c.

Estimation of Carbonic Acid by Minimetric Analysis.

1*st*. *For Definite Amounts of Carbonic Acid.*—If we shake a bottle containing 644 cub. centims. or 23 ounces of common air, we obtain a precipitate such as that described above. Now, if air containing twice as much carbonic acid were to be put into the bottle, the precipitate would be twice as great, but we could not ascertain its value by the eye. We cannot even make a probable approach to it. If, however, we used a bottle just half the size of the first, the air being still twice as bad as the first specimen, we should have a precipitate exactly the same, because in fact the amount of carbonic acid would be exactly the same. If the air were four times as bad, we should then use a bottle four times smaller, and obtain a precipitate also exactly the same as the first; and so on down to the smallest dimensions. I go here in the belief that, although we cannot approach at all closely when endeavouring to obtain the comparative value of two precipitates, *we can retain in the memory with great exactness the character of one precipitate of a given density.*

If, then, we wish the air of a place to be kept at any one given state of purity, we should require only to have a bottle corresponding to the amount of carbonic acid, and the trial could be made at once. This plan would not suffice for estimating the amount in any given air; it would estimate only one amount; but it would show clearly when there was more and when less.

When it was found so easy to remember a certain bulk of precipitate, it became important to know what bulk would be the most easily remembered. Must it be a minute quantity, such as a chemist would call a trace, or must it be a quantity such as we should call milky? Neither suffice. The first is too small for certainty; the second has no translucency, or so little that we cannot judge of the amount that lies behind. The quantity will be expressed most clearly by saying that the liquid is turbid and still translucent; but not so that you could read through it. Any one may obtain it exactly by shaking a clear 23-ounce bottle with half an ounce of baryta-water in air containing 0·04 per cent. carbonic acid; and this may easily and frequently be done to aid the memory. To be more precise, it is a precipitate obtained by throwing down baryta with 0·2515 cub. centim. of carbonic acid, or 0·00224 gramme carbonate of baryta freshly precipitated in half an ounce of liquid. In Table I. all the information actually necessary is given. Column 2 is for fine measurements in cubic centimetres, indicating the amount of air which will contain the carbonic acid necessary for producing the precipitate of baryta when the proportion is according to any number in the first column. Column 3 is the same number, with the addition of 14·16 cub. centims., or half an ounce, which is the space occupied by the liquid. This, then, gives the size of the bottle to be used. The fourth column also gives the size of bottle to be used, the numbers being avoirdupois ounces; fractions are not in all cases given, and are not required so minutely as they are given in some.

TABLE I.—To be used when the point of observation is the precipitate described, page 190. Half an ounce of baryta water, containing about 0·08 gramme baryta.

Air at 0° C., and 760 millims. bar.

Carbonic acid in the air, per cent.	Volume of air, in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois.
0·03	838	853	30·
0·04	629	644	23·
0·05	501	516	18·
0·06	419	434	16·
0·07	359	374	13·
0·08	314	329	12·
0·09	279	294	10·
0·10	251	266	9·
0·11	228	243	8·55
0·12	209	224	7·88
0·13	193	208	7·32
0·14	180	195	6·86
0·15	167	182	6·40
0·16	157	172	6·05
0·17	148	163	5·74
0·18	139	154	5·42*
0·19	132	147	5·17
0·20	125	140	4·92
0·21	119	134	4·71
0·22	114	129	4·54
0·23	109	124	4·36
0·24	104	119	4·19
0·25	100	115	4·04
0·26	96	111	3·90
0·27	93	108	3·80
0·28	90	105	3·70
0.29	87	102	3·59
0·30	84	99	3·48
0·40	63	78	2·74
0·50	50	65	2·28
0·60	42	57	2·00
0·70	36	51	1·79
0·80	31	46	1·61
0·90	28	43	1·51
1·00	25	40	1·40
2·00	12	27	0·95

Perhaps in some cases it may be found more convenient to use those sizes of bottles which do not give any precipitate or milkiness when half an ounce of baryta-water is

* This size of bottle gives no precipitate in air with 0·04 per cent. carbonic acid.

shaken up with the air in them. The sizes corresponding to various percentages of carbonic acid are given in Table II.

TABLE II.—To be used when the point of observation is "*no precipitate.*" Half an ounce of baryta-water, containing about 0·08 gramme baryta.

Air at 0° C., and 760 millims bar.

Carbonic acid in the air, per cent.	Volume of air, in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois.
0·03	185	199	7·06
0·04	139	154	5·42
0·05	111	125	4·44
0·06	93	107	3·78
0·07	79	93	3·31
0·08	70	84	2·96
0·09	62	76	2·69
0·10	56	70	2·46
0·11	51	65	2·29
0·12	46	60	2·14
0·13	43	57	2·01
0·14	40	54	1·90
0·15	37	51	1·81
0·20	28	42	1·48
0·25	22	36	1·29
0·30	19	33	1·16
0·40	14	28	1·04
0·50	11	25	0·89
0·60	9	23	0·83
0·70	8	22	0·78
0·80	7	21	0·75
0·90	6	20	0·72
1·00	5·5	19·7	0·70

In order to use this Table, first in its application to ordinary circumstances in life, we may assume that a bottle holding 5·42 ounces will not give any precipitate in the air around houses if we live in a tolerably fair atmosphere. To try the experiment the bottle must be very wide-mouthed, so that we can put into it a rod covered with clean linen, and rub the sides dry and clean; we must then fill it with the air of the place, either by blowing in air with a bellows, or putting a glass or caoutchouc tube into the bottle, and inhaling the air out of the bottle, so that fresh may enter. No way is more exact than this, if care is taken not to breathe into the bottle. This care is not at all difficult to take; and no amount of apparatus can be more accurate than this method, if done intelligently. If the slightest amount of breath goes into the bottle, the process of rubbing clean and drying must be undertaken anew.

When the bottle is filled with the air of the place to be examined, add the half ounce of baryta-water, put on the stopper, and shake. If there is no precipitate, the air is not worse than 0·04 per cent. When it is desired to ascertain if it really contains as much as 0·04, then a bottle holding 7·06 ounces must be used.

Having ascertained that the air around contains no more than 0·04, it may be decided that a sitting-room shall not be allowed to contain more than 0·06, 0·07, or 0·10 per cent. If the first, then a bottle holding 3·78 ounces is taken; if the air does not contain above 0·06 per cent., there will not be any precipitate in the liquid. If it is allowed to contain 0·10 per cent. (1 per thousand), and on some evenings many houses will contain this, then a bottle of 2·46 ounces is enough.

If in workshops 0·25 per cent. is allowed, then a bottle holding 1·29 ounce is enough.

This plan does not enable us to make an analysis of air. The person to whom the care of the atmosphere would be committed would have only one bottle of the proper size, and would only require to see that the air never gave any precipitate with that size of bottle. The order might be given for any required purity, and by this test an uneducated man could tell when the amount of carbonic acid was too great.

For a private house the rule would be not to have the air above 0·07 at most; better to have less.

The baryta-water need not be of any particular strength;

a weak solution is sufficient. The strength used is given; but the precipitate does not differ when the water is stronger. If, however, the water should be extremely weak, several times weaker than the above, there is a difference. The carbonate of baryta dissolves in the water to a very perceptible extent. The first precipitate made in baryta-water by oxalic acid also, although very white at the surface where there is much acid and before mixing, disappears on shaking to a perfectly transparent and brilliant liquid. I speak, however, of very weak solutions; a solution five times weaker than the one given as an example, would be incorrect on account of its weakness.

Hitherto baryta has been spoken of; and it may well be asked why lime should not be preferred. The same precipitate to all appearance may be got with lime-water. Tables III. and IV. are constructed for lime-water, on exactly the same principles as the former ones. It will be seen that lime is so soluble or so transparent that it requires three times as much space or air from which to collect its equivalent of carbonic acid needful to produce the required opacity. This is, of course, an objection. Still, lime is to be had everywhere, and lime-water has not the poisonous properties ascribed to baryta-water.

TABLE III.—To be used when the point of observation is the precipitate described, page 194. Half an ounce of lime-water, containing 0·0195 gramme lime.

Air at 0° C. and 760 millims. bar.

Carbonic acid in the air.	Volume of air, cub. centims.	Size of bottles to be used, cub. centims.	Size of bottle, in ounces avoirdupois.
0·03	2566	2581	91·
0·04	1925	1940	68·
0·05	1540	1555	55·
0·06	1283	1298	46·
0·07	1100	1115	39·
0·08	963	978	34·
0·09	856	871	31·
0·10	770	785	28·
0·11	700	715	25·
0·12	642	657	23·
0·13	593	608	22·
0·14	550	565	20·
0·15	513	528	18·59
0·16	481	496	17·42
0·17	453	468	16·48
0·18	428	443	15·60*
0·19	405	420	14·78
0·20	385	400	14·08
0·21	367	382	13·45
0·22	350	365	12·85
0·23	335	350	12·32
0·24	321	336	11·83
0·25	308	323	11·37
0·26	296	311	10·95
0·27	285	300	10·56
0·28	275	290	10·21
0·29	266	281	9·89
0·30	257	272	9·58
0·40	193	208	7·32
0·50	154	169	5·95
0·60	128	143	5·03
0·70	110	125	4·40
0·80	96	111	3·90
0·90	85	100	3·50
1·00	77	92	3·22
2·00	38	53	1·86

* This size of bottle gives no precipitate in air with 0·04 per cent. carbonic acid.

Table IV.—To be used when the point of observation is "*no precipitate*." Half an ounce of lime-water, containing 0·0195 gramme lime.

Air at 0° C., and 760 millims bar.

Carbonic acid in the air, per cent.	Volume of air, in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois.
0·03	571	584	20·63
0·04	428	443	15·60
0·05	342	356	12·58
0·06	285	299	10·57
0·07	245	259	9·13
0·08	214	228	8·05
0·09	190	204	7·21
0·10	171	185	6·54
0·11	156	170	6·00
0·12	143	157	5·53
0·13	132	146	5·15
0·14	123	137	4·82
0·15	114	128	4·53
0·20	86	100	3·52
0·25	69	83	2·92
0·30	57	71	2·51
0·40	43	57	2·01
0·50	34	48	1·71
0·60	29	43	1·51
0·70	25	39	1·36
0·80	22	36	1·25
0·90	19	33	1·17
1·00	17	31	1·10

It is worth observing that the proportion of lime and baryta are nearly as their atomic weights. Perhaps more minute observation would make it quite the same. It was supposed that lead might give a similar proportion; but the texture of the precipitate was entirely different, the particles much larger. This prevented it being used in a similar way, and obstructed the theory as well as the practice so far.

One of the main advantages of this process is that it requires no weighing and no measuring, and we may almost say no thinking; this idea is, perhaps, more fully carried out with the lime than with the baryta-water. Lime-water may be prepared of the same constant strength so closely that we may neglect the difference; with baryta we are apt to make the solution unnecessarily strong, and so waste it; but the experiment will still be the same. Lime-water is common; but baryta-water could also be prepared cheap, if there were a demand for it.

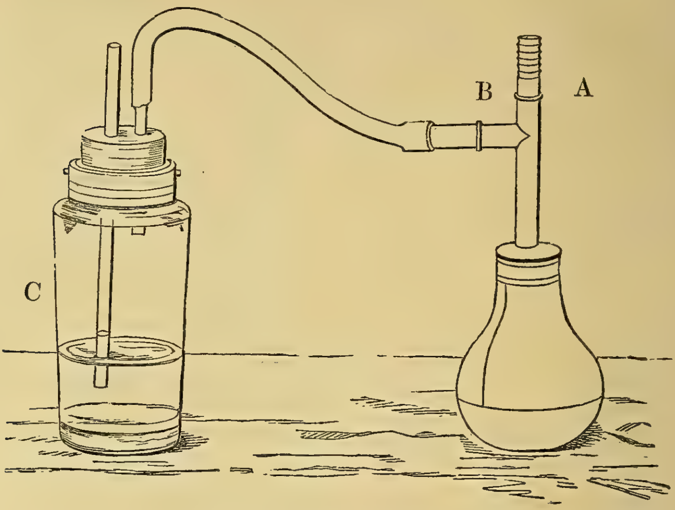
The use of this method would enable us to distinguish air in this manner:—We could say, This is 6-ounce air, that is 4-ounce air, that is 2-ounce air (meaning that 6, 4, or 2 ounces of it cause a precipitate in baryta-water); and any person would understand it, and prove it easily; whilst vials, so often seen in cottages, might be converted into scientific instruments for sanitary purposes.

2nd. *For all amounts of Carbonic Acid, the bulk of air varying.*—If it be desired to ascertain the amount of carbonic acid in air of which we know nothing, the following less simple apparatus is proposed:—

An elastic india-rubber ball may be made to contain any given amount; let us suppose two ounces. When we press it in the hand we can drive out the whole air, or at least nearly the whole; we let it go, and it fills again. If a tube be fitted to it we can drive the air which has gone into the ball through baryta- or lime-water, and obtain the precipitate before spoken of at the beginning. It is preferable, however, to cause the air to pass through the baryta- or lime-water before entering the ball; and this can easily be done. If the air be pure, it will require so many more balls to be emptied; if impure, a smaller number.

If a ball with a simple tube is used for blowing into a liquid, that liquid is drawn up as soon as the ball expands, and many fillings cannot be made without inconvenience. If, however, a valve be placed between the liquid and the ball, there can be no return. This is the case in the tube

A in the annexed figure: connected with the ball, there is a valve such as is used in air-pumps, preventing all entrance of air, but allowing it to pass out. Then, again, on tube B there is a valve of the same kind, preventing all egress of air, but allowing it to enter. The little instrument is therefore in reality an air-pump and a condenser. The air passes in at B, and goes out at A.



It is well to use always the same amount of solution, which may be, as in the previous method, half an ounce. If this liquid is put into an open vessel there is an escape of the carbonic acid, which ought to be retained, and also a collection of some from the atmosphere, which ought not to be absorbed. It is better, therefore, to use a bottle as at C, with a small entrance-tube.

The bottle should have the same capacity as the ball + the space required for the liquid. Previously to commencing the experiment, it must be filled with the air of the place: this is done by one or two pressures of the ball. The liquid and cork are then put in their places, and the whole shaken up. That counts for one ballful of air. The ball is then emptied by pressure, and allowed to fill itself again through the bottle. The fresh air gives up most of its carbonic acid in passing through the liquid; but as a little remains unabsorbed, the bottle is well shaken, so that the liquid may absorb all the carbonic acid. The operation is repeated until the desired precipitate is obtained. The number of ballfuls being counted, on referring to a Table such as the following, the percentage of carbonic acid is at once obtained:—

With baryta-water. Air at 0° C., and 760 millims. bar.

Number of strokes of the finger-pump, or number of ballfuls of air.	Per cent. of carbonic acid indicated in the air.	Actual amount of carbonic acid in the air of the ball, in cub. centims.
1	0·444	0·2515
2	0·222	0·1257
3	0·148	0·0838
4	0·111	0·0629
5	0·088	0·0503
6	0·074	0·0419
7	0·063	0·0359
8	0·055	0·0314
9	0·049	0·0279
10	0·044	0·0251
11	0·040	0·0229
12	0·037	0·0209
13	0·034	0·0193
14	0·032	0·0180
15	0·029	0·0167

This Table is constructed for a ball of 2 ounces capacity; but of course any size of ball may be used, and a table constructed to suit it. For very bad air (above 0·07 per cent.) it is found advisable to use a ball of half the size.

We might call the apparatus a finger-pump, if no better name is suggested.

In using this ball it is well to observe the method by which the points of the fingers press into the centre. If this is followed, the whole of the air may practically be driven out. The ball is thus divided into two parts: one part is pressed between the palm of the hand and the finger; the other is pressed between the surface of the nails and the first joint of the thumb.

This apparatus requires a little experience to produce confidence. Assistants have tried it along with Pettenkofer's method and obtained remarkably accurate analyses, with fewer errors. I have not yet used it in scientific investigations, and scarcely even practically, although with observing persons it is worthy, I believe, of all confidence, and especially if used daily, as in certain proposed inspections would be the case.

For practical purposes the state of the barometer may be neglected.

3rd. The bulk of Air fixed, the Lime-Water varying.—There are many ways of ascertaining the amount of carbonic acid in the air with great precision. An experimenter may make any one of them perfect, if he will only continue to use it until familiarity ensues. Lime-water may be used according to Table V.

TABLE V.—Neutralization with lime-water*. Capacity of bottle, 50 ounces.

Air at 0° C., and 760 millims. bar.

Carbonic acid in the air, per cent.	Quantity of lime-water required.	Carbonic acid in the air, per cent.	Quantity of lime-water required.
	grs.		grs.
0·03	120	0·15	599
0·04	160	0·20	798
0·05	200	0·25	998
0·06	239	0·30	1197
0·07	279	0·40	1596
0·08	319	0·50	1995
0·09	359	0·60	2394
0·10	399	0·70	2793
0·11	439	0·80	3192
0·12	479	0·90	3591
0·13	519	1·00	3990
0·14	559		

* The lime-water used is the ordinary lime-water diluted with 9 volumes of pure distilled and fresh boiled water.

Here it is proposed that a bottle of 50 ounces capacity should have attached to it a flexible ball filled with a known quantity of lime-water. A little is squeezed into the bottle, and shaken about until it becomes neutral; again a little more; and when there is no more carbonic acid to render more lime neutral, the operation ceases. But how are we to know when the liquid is neutral? Of the many substances tried for this, perhaps turmeric was the best—a little bit of turmeric paper floating on the liquid itself. One of my assistants, Mr. Clement Higgins, tried the turmeric in this way, and became very familiar with its use. The operation, however, was slow, and not satisfactory to me, although it can be made excellent with patience and attention.

The plan is not correct for large quantities of carbonic acid, because the liquid takes up so much of the vessel. This could be avoided by emptying it into another elastic ball; but I have not cared to employ it so.

The solution used by this method was lime-water ten times diluted. The manufacture of lime-water is a very good method of obtaining a pretty exact strength of a liquid without weighing. The lime-water which has plenty of lime at the bottom, remains much the same. There is a little change occasionally; it would be well to determine the exact cause, and we might perhaps be able to start from the point of saturation, even for the most exact researches. The following results were obtained:—Four bottles of lime-water took of oxalic acid—

	Cub. centims. solution.			
	1st.	2nd.	3rd.	4th.
April 7th, 12° C	19·5	19·6	19·5	19·6
After two days, 6° C	19·4	19·3	19·3	19·4
" " 8°·4 C	20·0	19·8	20·1	19·9
" " 15° C	19·9	20·05	20·1	20·1
" " 13° C	19·9	19·9	19·9	19·9
	19·9		19·9	19·9

This shows that, for the precipitate on the plan of Tables I. and III., lime-water is more than necessarily constant, and even enough for Table V., and may be used in some cases to save the purchase of a balance.

Some years ago I proposed rosolic acid as an agent for ascertaining neutrality. It shows the neutral point with extreme sharpness when oxalic or any liquid acid is used; and we can tell to a small drop when to cease pouring. If all caustic solutions were coloured with it, it would help to point them out, as well as serve instead of litmus or turmeric. I did not obtain such successful experiments as I could have wished when trying it with carbonic acid lately. The last traces of colour are difficult to remove; I hope to bring it on further some day. It is curious that lime- or baryta-water take up carbonic acid much less readily when rosolic acid is present. The latter portion of the experiment is affected chiefly when the solutions become weak. It is as if the resinous consistence of the acid repelled the gas. When a liquid is used, such as oxalic acid in solution, the rapidity of action is very great, the same resinous quality causing the rosolic acid to shrink into its shapeless and colourless state with great suddenness.

It may, however, be remarked that weak solutions of baryta and lime take up carbonic acid very slowly of themselves, although rosolic acid renders the absorption still slower.

Manganates and Ferrates as Tests for Carbonic Acid.

In a former paper I mentioned that the carbonic acid of the air was capable of being estimated by observing its action on manganates. The green manganate was prepared in the usual way, or by adding caustic alkali to the so-called permanganates, which, however, from this action appear rather as bi-manganates. The amount of acid required to convert the green to red may be found readily by a solution of test acid, and the equivalent for carbonic acid calculated. The plan already described, with a ball from which the manganate is pressed, suits the test very well when the ball contains nothing which affects the salt. If the ball is objected to, we may adopt another method. This plan consists simply of a graduated tube like a pipette fitted into the cork of the bottle; a small quantity is allowed to flow down when required; and when the liquid begins to become purple, there being no more carbonic acid to render it red, the experiment is finished: the amount used is read on the graduated tube.

At Mr. Hutchinson's alkali works I obtained from Mr. Powell a salt of ferric acid accidentally formed. It was analyzed by Dr. Roscoe. This salt was expected to be still more sensitive to organic matter than chameleon. It was sensitive, certainly; but it decomposed rather rapidly in the light. It, however, can be used both for carbonic acid and oxidizable matter, in the same manner as the manganate. The action on light, however, is not quite sufficient to make the salt a good photometer. The objection to it for carbonic acid is that it does not keep well.

The aspirator may be used for drawing the air through any of these solutions, and either the ball apparatus or two small bottles. Even one bottle may be used with great safety, if it is covered from the air, and the speed not very great; and by using Mr. Dancer's apparatus the exact measurement of the air may be obtained. This proposal appears like a return to Dr. Reid's carbonometer, made somewhat more convenient and elegant, and exactly quantitative.

There may be varieties of tastes; but I expect the first and second methods to be very much used; they are very simple. It is quite possible to add many other methods and modifications, but I know of none so simple as these.

XIII. *Catalogue of Binary Stars, with Introductory Remarks.* By Alfred Brothers, F.R.A.S.

Read October 14th, 1866.

From the time of the publication of the first catalogue of double stars by Sir William Herschel in 1782, the search for such objects has been one of unabated interest to both professional and amateur astronomers; and the result has been the compilation of many other valuable catalogues.

The number of double and triple stars discovered by Sir William Herschel and other observers down to the present time amounts to many thousands; but of these, comparatively few have been proved to be binary systems, as distinguished from stars optically double. That such physical connexion might exist was suggested by Michell in 1767; but the honour of establishing the fact was reserved for Sir William Herschel, who appears not only to have ascertained the truth of the supposition beyond doubt, but also to have invented the instrument by which some of his measures were taken*. These early measures of Herschel, and those of Struve, Sir J. Herschel, Argelander, Smyth, Dawes, Secchi and others, form the foundation on which all the subsequent observations are based. They are contained, however, in works not generally accessible to the observer; and I have no doubt that there are many amateurs who, like myself, have felt the inconvenience of having to refer to several sources for information relative to the positions and distances of the binary stars, and who would be glad to possess a more complete list than is usually found in elementary works on astronomy. I have therefore been induced to extend the plan of a catalogue

* Phil. Trans. vol. lxxi. p. 500.

which I had originally prepared for my own use only, and have included the results of the most recent observations of several eminent astronomers, which they have been kind enough to send me for the purpose.

This catalogue is intended to show at one view the measures of the late Admiral Smyth, taken at the Bedford Observatory during the years 1830 to 1842, and from 1843 to 1858 at the observatory of the late Dr. Lee, at Hartwell; also, on the authority of Smyth, some of the measures of the same objects by observers previous to 1830, including Sir William Herschel's epochs from 1778 to 1802, Struve's from 1819 to 1836, and some others. In the columns, following the name of Admiral Smyth, will be found the names of the Rev. W. R. Dawes, the Rev. R. Main, Mr. Knott, the Baron Dembowski, Mädler, the Rev. Father Secchi, Mr. Morton, and Mr. Talmage, with their measures of position and distance. The names of the observers are arranged as nearly as possible in the order of the dates of observation; so that a glance from the first name to the last on the list, under each particular star, shows at once the changes which have taken place during a period of about 90 years, and in some instances of more than a century.

In the cases of stars which are known to have passed their perihelion, the dates are given: these dates are derived from various sources; and it must be remarked that some computors give epochs which do not agree with those inserted. The orbital periods are stated on the authority of Admiral Smyth, and must in almost every instance be taken as approximations only.

The objects are arranged in the order of right ascension, and the places of all those which have been observed by Smyth are reduced to the epoch 1865·0 by using his precessions. There are, however, many objects in the catalogue which were not observed by Smyth. In all these exceptional cases the date (1860) is inserted, and the authority quoted is Secchi. The magnitudes of all the stars taken from the 'Cycle' are those given by Smyth, and in many instances do not agree with the observations of Dembowski and Secchi. In all other cases Secchi's magnitudes have been adopted.

The colours are stated on the authority of Smyth, Webb, Main, and Secchi. It may be observed that the aperture of the telescope used probably influences the observer's estimate of colours, as I find from information received from Mr. Webb that, while using a refractor of 3·7 inches aperture, his colours are often quite different from those recorded as seen with one of 5·5 inches aperture, which was used for all his observations after 1863.

The measures of Dembowski will be found in the 'Astronomische Nachrichten,' Nos. 1473 to 1475, and Nos. 1572 to 1574; and I have given the means of his measures for the years. Those of Mr. Dawes are derived from a list furnished by him; and the same remark applies to those of Mr. Knott. The 'Radcliffe Observations' for 1861–1863 have furnished the particulars of Mr. Main's observations; and to Professor Secchi I am indebted for his list of 1321 double stars measured at the Observatory of the Roman College, and also for a separate list by which the measures of some of the objects are brought down almost to the present date.

The measures of Mr. Morton are derived from Lord Wrottesley's "Catalogue of the Positions and Distances of 398 Double Stars," contained in vol. xxix. of the 'Memoirs of the Royal Astronomical Society of London.' Mr. Romberg's measures are taken from the 'Leyton Astronomical Observations' for 1862–64; those of Professor Kaiser will be found in No. 9 of vol. xxvi. of the 'Monthly Notices;' and the measures by Mr. Talmage were made at Mr. Barclay's Observatory at Leyton, and supplied for the purpose of this catalogue.

As some confusion has arisen in designating the star Flamsteed's "51 Libræ," I have, at the suggestion of the Rev. W. R. Dawes, inserted it as "*ξ Scorpii*" (=*Fl.* 51 *Libræ*), as it undoubtedly belongs to Scorpio, which constellation otherwise has no star marked ξ, while Libra has three stars indicated by that letter. The remarks of Mr. Dawes respecting this star are interesting, and I venture to quote his own words; Mr. Dawes says:—"The interest which attaches to ξ Scorpii $\frac{A+B}{2}$ and C is so exceedingly small compared with that which belongs to A and B as a binary system, that I can only account for the close and rapidly revolving pair being omitted by any of the observers to whose lists you refer by the very probable fact that their telescopes were not competent to show it as a double star. Its low situation, in these latitudes, increases the difficulty of observing it successfully with telescopes which could divide it well." Respecting Dembowski's observations on this triple star, Mr. Dawes also remarks:—"Of $\frac{A+B}{2}$ and C he gives, 1863·14, P=70°·46, D=7·154, and, for 1865, P=71°·02, D=7·112. Whatever weight may attach to these results, they rather go to negative the supposition of C's having any orbital movement in respect of A and B. Indeed, notwithstanding the regular decrease of Struve's angles, I have thought the motion so doubtful as to require observations at long intervals only; and I find I have not measured the star (C) since 1840·56, P=69°·45, D=7·43. Herschel II. obtained P=70°·93, D=7·07 in 1831·38, and at the Cape P=77°·2 in 1834·35. These, compared with Dembowski's more recent results, throw some doubt on former deductions. But no list of binary stars should be without A : B, the variation of angle having amounted to nearly 160° in 40 years, and about 150° since I began to observe it."

Respecting the star 61 Cygni, Powell states (Mem. R. Ast. Soc. vol. xxxii. p. 95), "Surely the movements of the components cannot be of an orbital nature;" and Mr. Dawes also says, in a letter of 8th September 1866, "The motion observed in 61 Cygni is not *orbital*, but *rectilinear*, arising from difference of proper motion." On such authority perhaps the star ought to have been omitted; but it is one of Smyth's binaries, and for that reason, as well as for the special interest attaching to it, it is retained.

The star ζ Herculis was observed to be single from about 1863 to 1865; but the following remarks, contained in the letter from Mr. Dawes above quoted, may be of some interest:—"From the extreme difficulty, or perhaps impossibility, of seeing ζ Herculis in the least elongated in 1863, I imagined it could not be otherwise than 'single' to telescopes of ordinary dimensions in 1865. I find, however, that the small star has made more haste to emerge from its concealment than I supposed to be probable; .... but near the end of October I received a letter from Alvan Clark, informing me that with one of his exquisite object-glasses of 7 inches aperture he had seen ζ Herculis double. He gave me no particulars of angle or distance, and I confess I thought it likely there might be some mistake. On August 31st I turned an 8-inch refractor on to ζ Herculis, and instantly saw the small star perfectly detached from the large one."

μ^1 Herculis.—Of this star I have not found any other measures than those by Mr. Dawes in 1858 and 1864, and by Mr. Knott in 1865; but as the change of the angle of position amounts in 17 years to about 20 degrees, the binary character of the object may, perhaps, be considered established. The measures by Alvan Clark, as published in 1856, are not complete.

π Cephii.—The first measures of this object appear to have been made by Admiral Smyth in 1843; and it is evident from Mr. Knott's measures in 1865 that the star has changed its angle of position 34 degrees, and in distance ·65″ in 22 years, and, consequently, must be added to the list of binaries.

P. III. 98, 145 Leonis, η Orionis, and some others are inserted on the authority of only two observers; but they appear to be binary systems, and deserve attention.

Since the date of the 'Cycle' the number of binary stars has been greatly increased; and several observers have favoured me with the names of stars which have changed their angles of position sufficiently to entitle them to a place amongst that class of objects; and they are accordingly inserted.

To the gentlemen referred to I take this opportunity of returning my best thanks.

With few exceptions, I have not included in this catalogue any object which has shown a change of position of less than 10 degrees in 30 years. There are many stars which may eventually prove to be binary; but their motions are extremely slow, and require examination at long intervals. Of the few stars forming the exception to the rule adopted, I may name Sirius, Antares, and a few others as interesting objects, and deserving close attention on the part of those observers who have telescopes of sufficient aperture to measure the angles of position and distances of such difficult objects.

D. 5 and A. C. 5 refer respectively to the lists of double stars discovered by Mr. Dawes and Alvan Clark.

I do not attach any importance to my own measures; and the following are given merely to show that the work done with a telescope of 5 inches aperture may be useful in this department of astronomy, and that the possessors of such, and even smaller telescopes, may do good work by taking advantage of favourable states of the atmosphere.

The objects selected are those only which are conveniently situated at this season of the year :—

Star.	Epoch.	Position.	Distance.
		°	"
36 Andromedæ	1866·760	349·03	1·29
P. I. 123 Piscium	1866·760	24·40	Wedged.
ε Arietis	1866·769	197·40	Separated.
ε Bootis	1866·725	325·70	2·59
44 "	1866·769	238·60	
δ Herculis	1866·766	180·60	19·34
61 Cygni	1866·701	111·01	18·87
ζ Aquarii	1866·766	337·40	3·58

The first three stars on the above list may be considered good tests for the telescope; but in addition I may say that on several occasions I have seen the companion to δ Cygni; and, still further to test the matter, I requested a friend, who also could see the star, to place the position-line upon it, and a comparison of the angles showed that we had both seen the same object.

With the following information before him, the amateur who has not access to more recent epochs than those contained in Smyth's 'Cycle,' may probably feel greater interest in his work, and more certain of his own measures; for the pleasure of observing and recording observations is greatly enhanced when we feel that our results approach the accuracy of the more practised "astrometer."

Of the 155 objects which this catalogue contains, 69 appear to have direct, and 86 retrograde motion.

It should perhaps be stated that some of the measures for 1866 depend on a small number of observations, and may be subject to correction.

Cephei 316. Σ 2.

R. A. (1860), $0^h\ 1^m\ 30^s$.
Dec. N. 78° 56′.
Magnitudes, $a = 5\frac{1}{2}$, $b = 6$.
Colours: Secchi, 1857, yel., green.

	Epoch.	Position.	Distance.
		°	″
Struve	1830·85	341·50	0·81
Mädler	1838·63	338·14	0·69
Dawes.........	1839·67	336·15	0·70
Secchi.........	1857·52	324·97	0·38
Talmage	1865·76	295·56	...

Cephei 318. Σ 13.

R. A. (1860) $0^h\ 8^m\ 18^s$.
Dec. N. 76° 10′.
Mags. $a = 6$, $b = 6\frac{1}{2}$.
Colours: Secchi, 1857, both white.

Struve	1831·50	124·02	0·53
Mädler	1840·82	118·01	0·52
Secchi.........	1857·52	102·28	0·69
Dembowski ..	1863·00	103·56	0·50

η Cassiopeæ. Σ 60.

R. A. $0^h\ 40^m\ 52^s$. Dec. N. 57° 6′ 2″.
Mags. $a = 4$, $b = 7\frac{1}{2}$.
Colours: Smyth, 1843, p. whi., purp.; Webb, 1850, yel., p. garnet; Main, 1861, white, lilac.

Struve.........	1819·80	80·12	10·80
Smyth.........	1830·91	87·80	9·80
"	1854·17	108·30	7·70
Dawes.........	1854·00	109·60	7·91
Secchi	1857·14	112·84	7·85
Dembowski ..	1865·18	125·66	6·72
Knott	1865·69	125·59	6·75

Orbital period about 700 yrs. (Smyth).

36 Andromedæ. Σ 73.

R. A. $0^h\ 47^m\ 36^s$. Dec. N. 22° 53′ 53″.
Mags. $a = 6$, $b = 7$.
Colours: Smyth, 1843, br. oran., yel.; Webb, 1850, Main, 1861, both yel.

Herschel......	1830·78	307·04	0·90
Struve.........	1832·44	307·80	0·84
Smyth.........	1835·92	315·70	1·10
"	1852·83	335·80	1·30
Dawes.........	1859·83	340·28	1·19
Main	1861·80	329·00	1·10
Dembowski ..	1865·25	345·44	1·21
Knott	1865·69	344·85	1·35
Talmage......	1865·77	346·48	1·06
Secchi.........	1866·05	349·51	1·31

P. 0. 251 Piscium.

R. A. $0^h\ 52^m\ 29^s$. Dec. N. 0° 13′ 3″.
Mags. $a = 8$, $b = 9$.
Colours: Smyth, 1838, p. orange, bl.

	Epoch.	Position.	Distance.
		°	″
South	1825·17	296·27	18·87
Smyth	1832·98	299·80	18·40
"	1838·03	301·80	18·50
"	1852·81	305·10	18·80

42 Ceti. Σ 113.

R. A. $1^h\ 12^m\ 54^s$. Dec. S. 1° 14′ 4″.
Mags. $a = 6$, $b = 8$.
Colours: Smyth, 1834, br. wh., white; Dembowski, 1863, both white.

Smyth.........	1834·84	332·80	1·20
Struve.........	1836·91	334·30	1·77
Smyth.........	1857·97	344·60	1·30
Dawes.........	1854·51	338·47	1·16
Secchi.........	1856·48	339·75	1·16
Main	1861·90	357·43	0·90
Dembowski ..	1863·03	343·03	1·27

P. I. 123 Piscium. Σ 138.

R. A. $1^h\ 28^m\ 59^s$. Dec. N. 6° 57′ 15″.
Mags. $a = 6\frac{1}{2}$, $b = 8$.
Colours: Smyth, 1843, yel., p. white; Webb, 1855, yellow; Main, 1862, deep yellow.

Struve.........	1830·23	20·00	1·46
Smyth.........	1832·86	19·80	1·50
"	1853·91	26·30	1·50
Dawes.........	1853·81	29·38	1·26
Secchi.........	1857·89	29·10	1·45
Main	1862·02	30·28	1·24
Dembowski ..	1863·17	28·69	1·57

P. 1. 209 Piscium. Σ 186.

R. A. (1860), $1^h\ 47^m\ 0^s$.
Dec. N. 0° 59′ 0″.
Mags. $a = 7\frac{1}{2}$, $b = 7\frac{1}{2}$.
Colours: Secchi, 1857, both white.

Struve.........	1831·12	64·72	1·23
Secchi.........	1857·92	267·52	0·42
Dawes.........	1863·85	85·15	0·30

There is evidently an error of 180° in one of these measures. Smyth in 1833 has P = 62·9, D = 1·5.

α PISCIUM. Σ 202.

R. A. 1^h 55^m 3^s. Dec. N. 2° 6′ 39″.
Mags. a = 5, b = 6.
Colours: Smyth, 1846, p. gr., blue.

	Epoch.	P.	D.
		°	″
Herschel ...	1779·80	337·23	5·12
Smyth	1834·92	334·70	3·60
"	1846·92	331·40	3·50
"	1852·03	329·50	3·50
Dawes.........	1853·99	328·10	3·42
Secchi.........	1856·16	327·87	3·35
Jacob	1858·05	326·82	3·19
Morton	1858·70	326·46	3·51
Main	1861·90	329·36	3·15
Dembowski ..	1863·95	326·28	3·13
Knott	1865·47	325·76	3·23
Talmage......	1865·76	324·90	3·48

γ ANDROMEDÆ. Σ 205.

R. A. 1^h 55^m 37^s. Dec. N. 41° 40′ 55″.
A : B.
Mags. a = 3½, b = 5½.
Colours: Smyth, 1843, ora., em. gr.

Struve	1830·02	62·44	10·33
Dawes.........	1832·94	64·05	10·63
Smyth.........	1843·33	61·60	11·00
Secchi.........	1858·58	62·79	10·30
Main	1861·85	62·24	9·58
Dembowski ..	1862·96	62·97	10·42
Romberg ...	1863·13	62·65	10·32
Dawes.........	1863·86	64·20	10·47
Morton	1859·76	63·11	10·34
Talmage......	1865·81	62·70	9·47
Knott	1865·67	63·43	10·36

B : C. b = 5, c = 6·3.
Colours: Smyth, p. yel., smalt blue.

Struve.........	1842·72	126·36	...
Smyth.........	1852.99	115·00	0.50
Mädler	1855·02	119·42	...
Secchi.........	1858·99	108·52	0·45
Dawes	1863·86	107·70	0·59
Romberg ...	1863·99	107·58	0·60
Dembowski ..	1865·64	105·40	0·50
Knott	1865·67	107·07	0·59
Talmage......	1865·76	106·35	0·58

259 ANDROMEDÆ. Σ 228.

R. A. (1860), 2^h 5^m 6^s.
Dec. N. 46° 50′.
Mags. a = 7, b = 7.
Colours: Secchi, 1857, both yellow.

	Epoch.	P.	D
		°	″
Struve	1831·46	262·14	1·08
Mädler	1836·61	267·18	1·03
Secchi.........	1857·62	286·86	0·99
Dembowski ..	1862·96	286·50	0·90

Σ 257 PERSEI.

R. A. (1860), 2^h 15^m 12^s.
Dec. N. 60° 55′.
Mags. a = 7, b = 8.
Colours: Secchi, 1857, yellow, white.

Struve.........	1830·53	164·93	·0.60
Mädler	1837·38	170.90	0.56
Secchi.........	1857·48	183·34	0.40
Dembowski ..	1863·13	183.50	...

ι CASSIOPEÆ. Σ 262.

R. A. (1860), 2^h 17^m 35^s.
Dec. N. 66° 46′.
A : B.
Mags. a = 4½, b = 7.
Colours: Secchi, 1857, both yellow.

Struve.........	1829·66	276·68	1·86
Mädler	1831·64	275·33	1·88
Dawes..... ...	1848·09	265·50	2·24
Morton	1856·03	265·40	2·01
Secchi.........	1857·49	266·88	1·83
Dembowski ..	1862·99	265·87	1·92
Romberg ...	1863·95	268·77	1·72

Σ 278 CASSIOPEÆ.

R. A. (1860), 2^h 25^m 36^s.
Dec. N. 68° 41′.
Mags. a = 8, b = 8½.
Colours: Secchi, 1857, both white.

Struve.........	1830·77	82·05	0·43
Secchi.........	1857·93	67·67	0·40

114 ARIETIS. Σ 305.

R. A. 1860, 2^h 39^m 36^s.
Dec. N. 18° 46′.
Mags. a = 7, b = 8.
Colours: Secchi, 1858, both white.

Struve.........	1830·95	330·87	1·58
Dawes...	1853·89	322·48	2·33
Secchi.........	1857·89	322·23	2·56
Dembowski ..	1863·09	321·78	2·52

ε ARIETIS. Σ 333.

R. A. $2^h\ 51^m\ 29^s$.
Dec. N. 20° 47′ 57″.
Mags. $a = 5$, $b = 6\frac{1}{2}$.
Colours: Smyth, 1863, p. yel., whitish; Dembowski, 1863, white.

	Epoch.	P.	D.
		°	″
Struve.........	1830·16	188·87	0·54
Smyth	1835·77	193·50	0·50
„	1853·08	200·10	1·00
Dawes.........	1854·00	195·55	1·01
Secchi.........	1856·57	196·73	0·87
Dembowski ..	1862·99	194·72	0·80
Knott.........	1866·64	199·06	1·14

7 TAURI. Σ 412.

R. A. 1860, $3^h\ 26^m\ 0^s$.
Dec. N. 23° 59′.
A : B.
Mags. $a = 6\frac{1}{2}$, $b = 6\frac{1}{2}$.
Colours: Secchi, 1856, both white.

Struve.........	1830·38	269·92	0·69
Smyth	1833·21	265·00	0·70
Mädler	1839·76	265·50	0·55
Dawes.........	1846·91	259·92	0·65
Secchi.........	1856·35	256·78	0·42
Dembowski ..	1863·08	257·90	...
Knott.........	1864·93	240·80	0·50
Talmage......	1865·71	261·97	...

P. III. 98 ERIDANI.

R. A. $3^h\ 29^m\ 52^s$.
Dec. N. 0° 8′ 49″.
Mags. $a = 6\frac{1}{2}$, $b = 9$.
Colours: Smyth, 1845, yel., p. blue.

South	1824·02	225·12	5·81
Smyth.........	1834·93	231·80	5·90
„	1845·81	235·90	6·00

49 CEPHEI. Σ 460.

R. A. (1860), $3^h\ 46^m\ 48^s$.
Dec. N. 80° 18′.
Mags. $a = 5\frac{1}{2}$, $b = 6\frac{1}{2}$.
Colours: Secchi, 1857, both white.

Struve.........	1830·89	352·56	0·88
Mädler	1836·76	356·75	0·87
Secchi.........	1857·90	10·79	0·72
Dembowski ..	1862·95	15·61	0·70

Σ 511 CAMELOPARDI.

R. A. (1860), $4^h\ 6^m\ 12^s$.
Dec. N. 58° 2′ 36″.
Mags. $a = 6$, $b = 7$.
Colours: Secchi, 1858, yel., p. green.

	Epoch.	P.	D.
		°	″
Struve.........	1829·52	320·00	0·54
Secchi.........	1858·01	302·02	...
Dembowski ..	1863·61	294·07	...

TAURI 230. Σ 535.

R. A. 1860, $4^h\ 15^m\ 42^s$.
Dec. N. 11° 0′.
Mags. $a = 7$, $b = 8$.
Colours: Secchi, 1857, both white.

Struve.........	1831·34	353·88	1·94
Dawes.........	1854·29	342·73	2·06
Secchi.........	1856·51	345·01	1·54
Morton	1860·01	345·58	1·60
Dembowski ..	1862·99	342·38	1·73

2 CAMELOPARDI. Σ 566.

R. A. (1860), $4^h\ 28^m\ 6^s$.
Dec. N. 53° 14′.
Mags. $a = 5\frac{1}{2}$, $b = 8\frac{1}{2}$.
Colours: Secchi, 1858, yellow, blue.

Struve.........	1829·79	311·40	1·58
Mädler	1834·96	309·22	1·55
Smyth.........	1836·28	308·70	1·70
Secchi.........	1858·92	300·55	1·73
Dembowski ..	1863·24	299·53	1·68

Σ 577 AURIGÆ.

R. A. (1860), $4^h\ 32^m\ 48^s$.
Dec. N. 37° 14′.
Mags. $a = 7\frac{1}{2}$, $b = 8$.
Colours: Secchi, 1857, both white.

Struve.........	1829·57	278·88	1·58
Mädler	1835·81	274·19	1·64
Secchi.........	1857·65	267·99	1·63
Dembowski ..	1862·77	265·50	1·62

η ORIONIS. D 5.

R. A. $5^h\ 17^m\ 26^s$. Dec. S. 2° 31′ 48″.
Mags. $a = 4$, $b = 5$.
Colours: white, purplish.

Herschel ...	1781·99	single?	
Dawes.........	1848·04	87·00	1·00
„	1866·95	86·12	0·95
Knott.........	1863·12	88·92	0·94
„	1866·94	89.83	1·02

Σ 932 Geminorum.

R. A. 6h 26m 24s. Dec. N. 14° 51′.
Mags. a = 8, b = 8½.
Colours: Secchi (1857), both white.

	Epoch.	P.	D.
		°	″
Struve.........	1830·53	341·70	2·42
Mädler	1837·51	339·56	2·53
Secchi.........	1857·16	334·38	2·04
Dawes.........	1859·15	332·07	2·56
Dembowski ..	1863·41	333·27	2·26

12 Lyncis. Σ 948.

R. A. 6h 34m 18s.
Dec. N. 59° 34′ 26″.

A : B.

Mags. a = 6, b = 6½.
Colours: Smyth, 1839, white, ruddy.

Herschel......	1780·68	181·23	1·50
Struve.........	1831·10	153·70	1·53
Smyth.........	1832·96	154·30	1·60
,, ...	1839·27	149·50	1·60
Dawes.........	1848·22	143·35	1·69
Smyth.........	1852·96	143·70	1·50
Secchi.........	1857·30	142·27	1·68
Morton	1858·24	140·15	1·55
Main	1862·31	136·18	1·53
Dembowski ..	1863·05	138·57	1·72
Kaiser*	1866·28	316·50	1·64

* This measure is evidently too large by 180°.

A : C. c=7½, bluish.

Herschel......	1780·68	302·33	9·38
Struve.........	1831·10	304·20	8·67
Smyth.........	1832·96	305·10	8.60
Dawes.........	1833·13	304·06	8·88
Smyth.........	1852·96	306·30	9·00
Secchi.........	1857·30	305·50	8·66
Morton	1858·24	305·90	8·56
Main	1862·31	303·58	8·97
Dembowski ..	1863·06	305·77	8.67

Orbital period about 700 years.

α Canis Majoris (Sirius).

R. A. (1860) 6h 39m 13s.
Dec. S. 16° 31′ 47″.
Mags. a = 1, b 10.
Colours: a white, b ... ?

Bond	1862·10	85·15	10·37
Dawes.........	1864·22	84·86	10·00
Lassell	1864·20	80·15	9·67
Struve, O. ...	1864·22	74·80	10·92
Knott	1866·08	77·15	10·43
Secchi.........	1866·28	71·31	10·10

38 Geminorum. Σ 982.

R. A. 6h 47m 1s. Dec. N. 13° 20′ 56″.
Mags. a = 5½, b = 8.
Colours: Smyth, 1849, light yellow, purple; Dawes, yellow, blue.

	Epoch.	P.	D.
		°	″
Herschel......	1781·99	179·54	7·95
Struve.........	1829·24	174·88	5·73
Smyth.........	1836·10	171·80	6·00
,, ...	1849·19	170·20	5·60
Fletcher	1851·89	168·87	6·25
Morton	1854·14	168·58	5·99
Dawes.........	1854·17	167·49	5·86
Secchi.........	1856·11	169·30	6·13
Powell	1861·12	165·30	6·07
Dembowski ..	1863·02	166·30	6·13
Main	1863·14	167·48	5·96
Romberg ...	1863·11	167·28	6·36

Σ 1037 Geminorum.

R. A. (1860), 7h 4m 6s.
Dec. N. 27° 27′ 36″.
Mags. a = 7, b = 7½.
Colours: Secchi, 1856, both white.

Struve.........	1830·42	332·67	1·11
Mädler	1841·80	331·10	1·33
Dawes.........	1848·17	324·67	1·32
Secchi.........	1856·67	323·07	1·24
Dembowski ..	1863·20	318·10	1·22

α Geminorum. Σ 1110.

R. A. 7h 25m 59s.
Dec. N. 32° 11′.
Mags. a = 3, b = 3½.
Colours: Smyth, 1843, white, p. wh.; Webb, 1854, white; Main, 1863, yellow.

Herschel......	1778·27	302·47	5·16
Smyth.........	1830·95	258·80	4·70
,, ...	1849·17	248·10	4·90
Secchi.........	1855·82	245·13	5·36
Main	1863·14	239·00	5·39
Dembowski ..	1863·03	241·66	5·38
Knott	1865·04	239·71	5·42
Dawes.........	1865·31	241·45	5·68
Talmage......	1866·13	237·36	5·37

Perihelion passage, 1855 (H).

Σ 1157 Monocerotis.

R. A. (1860), $7^h\ 47^m\ 30^s$.
Dec. S. 2° 26′.
Mags. a = 8, b = 8·3.
Colours: Secchi, 1856, both white.

	Epoch.	P.	D.
		°	″
Struve.........	1831·20	267·27	1·59
Secchi.........	1856·47	256·44	1·30
Dembowski ..	1863·11	256·77	1·29

85 Lyncis. Σ 1187.

R. A. (1860), $8^h\ 0^m\ 42^s$.
Dec. N. 32° 37′.
Mags. a = 8, b = 7½.
Colours: Secchi, both white.

Struve.........	1829·50	71·00	1·61
Mädler	1837·69	68·25	1·62
Secchi.........	1858·21	61·65	1·75
Morton	1860·28	55·28	1·83
Dembowski ..	1863·15	56·28	1·83

ζ Cancri. Σ 1196.

R. A. $8^h\ 4^m\ 28^s$.
Dec. N. 18° 3′ 14″.
A : B.
Mags. a = 6, b = 7.
Colours: Smyth, 1843, yel., orange; Webb, 1849, yellow.

Herschel......	1781·90	3·28	1·06
Smyth.........	1832·23	28·30	1·30
"	1853·17	322·70	0·90
Fletcher	1853·30	321·06	1·10
Secchi.........	1857·28	303·92	0·77
Dembowski ..	1864·15	255·02	0·50
Dawes.........	1865·30	243·42	0·63
Knott	1866·26	233·63	0·78
Secchi.........	1866·28	234·62	0·40

Perihelion passage, 1853.

A : C. c = 7½.

Herschel......	1781·90	181·44	8·05
Smyth.........	1832·23	149·40	5·40
Fletcher	1852·49	143·68	4·84
Smyth.........	1853·17	144·10	4·80
Dawes.........	1854·07	140·47	5·05
Dembowski ..	1865·17	139·72	5·46
Secchi.........	1866·28	140·70	5·61

P. VIII. 13 Cancri. Σ 1202.

R. A. (1860), $8^h\ 5^m\ 54^s$.
Dec. N. 11° 16′.
Mags. a = 8, b = 10.
Colours: a white, b ... ?

	Epoch.	P.	D.
		°	″
Struve.........	1829·55	335·93	2·35
Dawes.........	1848·24	328·57	2·22
Secchi.........	1856·17	325·27	2·06
Dembowski ..	1863·12	327·45	2·50

ε Hydræ. Σ 1273.

R. A. $8^h\ 39^m\ 38^s$.
Dec. N. 6° 54′ 53″.
Mags. a = 4, b = 8½.
Colours: Smyth, 1843, p. yel., purp.; Webb, 1857, yel., ruddy; Main, 1863, yellow, pale purple.

Struve.........	1830·60	195·58	3·33
Smyth.........	1837·11	198·40	3·40
Dawes.........	1851·32	208·48	3·44
Fletcher	1852·96	208·52	3·57
Main	1863·17	200·53	3·40
Dembowski ..	1863·13	212·90	3·47
Secchi.........	1865·27	215·60	3·47
Talmage......	1866·14	213·83	3·87

Orbital period about 450 yrs. (Smyth).

157 Lyncis. Σ 1338.

R. A. (1860) $9^h\ 12^m\ 12^s$.
Dec. N. 38° 46′ 42″.
Mags. a = 6½, b = 7.
Colours: Secchi, both white.

Struve.........	1829·53	121·14	1·76
Mädler	1838·10	127·10	1·69
Dawes.........	1854·20	134·64	1·65
Morton	1854·24	137·30	1·92
Secchi.........	1856·30	137·18	1·63
Dembowski ..	1863·28	140·95	1·59
Talmage......	1866·10	142·48	1·71

ω Leonis. Σ 1356.

R. A. $9^h\ 21^m\ 13^s$. Dec. N. 9° 38′ 36″.
Mags. a = 6½, b = 7½.
Cols.: Smyth, 1843, p. yel., greenish.

Herschel......	1783·26	110·54	0·40
Smyth.........	1832·11	160·00	0·50
"	1843·14	193·00	0·30
Dawes.........	1854·23	346·23	0·55
Secchi.........	1856·42	0·99	0·35
"	1866·29	32·92	0·30

Perihelion passage, 1849.

P. IX. 161 SEXTANTIS. Σ 1377.

R. A. (1860), $9^h\ 36^m\ 6^s$.
Dec. N. 3° 16′.
Mags. a = 8, b = 10.
Colours: Smyth, 1834, yellowish white, blue.

	Epoch.	P.	D.
		°	″
Struve	1830·24	142·20	3·31
Smyth	1834·26	145·00	4·00
Mädler	1836·41	140·61	3·37
Secchi	1856·27	129·45	3·11

φ URSÆ MAJORIS. O Σ 208.

R. A. $9^h\ 41^m\ 10^s$. Dec. N. 54° 49′.
Mags. a = 5, b = 5½.
Colours:

Struve, O.	1842·35	8·45	0·52
Dawes	1854·28	25·89	0·40
Secchi	1857·34	30·60	0·30
Struve, O.	1866·42	45·90	0·24

8 SEXTANTIS. A C 5.

R. A. $9^h\ 45^m\ 6^s$.
Dec. S. 7° 24′ 0″.
Mags. a = 6, b = 6½.
Colours: Dawes, both white.

Dawes	1854·20	50·54	0·55
,,	1860·34	38·21	0·50

γ LEONIS. Σ 1424.

R. A. $10^h\ 12^m\ 30^s$.
Dec. N. 20° 31′ 35″.
Mags. a = 2, b = 4.
Colours: Smyth, 1843, bri. orange, greenish yel.; Webb, 1849, yellow, greenish yel.; Main, 1862, yellow, greenish.

Herschel	1782·71	83·30	3·00
Struve	1832·75	103·46	2·50
Smyth	1833·20	102·50	2·80
,,	1843·18	107·20	2·80
Fletcher	1853·21	108·43	3·00
Main	1862·35	107·19	3·10
Dembowski	1863·28	109·29	2·85
Secchi	1865·04	110·29	3·18
Dawes	1865·40	110·30	3·17
Knott	1866·21	110·49	3·21
Talmage	1866·20	111·44	3·17

LEONIS 145. Σ 1426.

R. A. (1860), $10^h\ 13^m\ 12^s$.
Dec. N. 7° 8′.
A : B.
Mags. a = 8, b = 7½.
Colours: Secchi, both pale yellow.

	Epoch.	P.	D.
		°	″
Struve	1832.26	256·77	0·62
Dawes	1854·16	263·28	0·88
Secchi	1856.15	271·78	0·65

Σ 1457 SEXTANTIS.

R. A. (1860), $10^h\ 31^m\ 24^s$.
Dec. N. 6° 28′.
Mags. a = 7½, b = 8.
Colours: Secchi, 1856, both p. yell.

Struve	1829·55	287·85	0·71
Mädler	1837·53	299·51	0·74
Dawes	1850·78	302·75	0·92
Secchi	1856·24	307·55	0·76
Morton	1857·28	312·58	0·60
Dembowski	1863·20	309·83	0·91

Σ 1516 DRACONIS.

R. A. (1860), $11^h\ 5^m\ 42^s$.
Dec. N. 74° 15′.
Mags. a = 7, b = 7½.
Colours: Secchi, 1856, both white.

Struve	1831·54	298·70	9·93
,,	1835·56	301·67	8·42
Dembowski	1854·55	8·35	2·70
Secchi	1856·29	29·55	2·61
Morton	1860·21	56·33	3·24
Dembowski	1863·35	70·05	4·14

ξ URSÆ MAJORIS. Σ 1523.

R. A. $11^h\ 10^m\ 59^s$.
Dec. N. 32° 17′ 39″.
Mags. a = 4, b = 5½.
Colours: Smyth, 1843, white; Webb, 1848, white; Main, 1863, white.

Herschel	1780·33	143·47	3·50
Struve	1827·26	229·30	1·82
Smyth	1835·37	180·20	1·90
,,	1851·31	123·50	2·90
Fletcher	1857·40	111·25	2·99
Dawes	1860·32	105·21	2·88
,,	1864·50	93·96	2·42
Dembowski	1864·83	91·96	2·23
Secchi	1864·38	92·88	2·40
,,	1865·51	89·88	2·53
,,	1866·30	86·55	2·25
Kaiser	1866·45	87·80	2·08

Perihelion passage, 1816.

ι LEONIS. Σ 1536.

R. A. $11^h\ 16^m\ 53^s$.
Dec. N. 11° 16′ 36″.
Mags. a = 4, b = 7½.
Colours: Smyth, 1843, pale yellow, light blue; Webb, 1849, pale yell., pale blue; Main, 1862, yell. blue.

	Epoch.	P.	D.
		°	″
Struve	1827·28	97·00	2·30
Smyth	1836·40	90·50	2·40
”	1853·29	81·30	2·50
Main	1862·25	73·54	2·72
Dembowski	1863·23	76·70	2·51
Secchi	1865·27	76·55	2·88
Dawes	1865·40	72·13	2·81
Talmage	1866·31	75·58	3·17

VIRGINIS 191. Σ 1647.

R. A. 1860, $12^h\ 23^m\ 30^s$.
Dec. N. 10° 49′.
Mags. a = 7½, b = 8.
Colours: Secchi, 1856, both white.

Struve	1830·07	202·04	1·18
Mädler	1835·06	204·23	1·20
Dawes	1850·66	211·72	1·23
Secchi	1856·35	211·67	1·19
Dembowski	1863·24	212·90	1·39

γ VIRGINIS. Σ 1670.

R. A. $12^h\ 34^m\ 50^s$.
Dec. S. 0° 42′ 34″.
Mags. a = 4, b = 4.
Colours: Smyth, 1843, silvery whi., pale yell.; Webb, 1851, yellowish white; do. 1863, pale yell.; Main, 1863, white.

Mayer	1756·00	144·22	6·50
Herschel	1780·06	130·44	5·70
Struve	1825·32	277·92	2·37
Smyth	1831·38	74·90	1·60
”	1837·21	265·40	0·60
”	1843·33	191·60	1·90
Fletcher	1858·38	170·01	3·56
Smyth	1858·39	169·90	3·80
Main	1863·30	164·35	4·05
Secchi	1864·41	165·54	4·28
Dawes	1865·42	164·02	4·37
Dembowski	1865·43	164·10	4·04
Knott	1865·45	164·33	4·33
Secchi	1866·31	164·28	4·39
Talmage	1866·36	162·40	4·51
Main	1866·44	164·92	4·34

Perihelion passage, 1836.

35 COMÆ BERENICIS. Σ 1687.

R. A. $12^h\ 46^m\ 39^s$.
Dec. N. 21° 58′ 49″.
A : B.
Mags. (Secchi), a = 5½, b = 8½.
Colours: Smyth, 1843, pale yell., lilac; Secchi, 1856, yellow, blue.

	Epoch.	P.	D.
		°	″
Struve	1829·99	25·39	1·43
Smyth	1834·38	30·00	1·00
”	1843·32	42·00	1·50
Secchi	1856·40	41·45	1·31
Morton	1856·34	50·13	1·32
Dawes	1860·34	47·68	1·44
Dembowski	1863·26	50·33	1·30
Knott	1865·31	52·87	1·31

42 COMÆ BERENICIS. Σ 1728.

R. A. $13^h\ 3^m\ 26^s$.
Dec. N. 18° 14′ 34″.
Mags. a = 4½, b = 5.
Colours: Smyth, 1842, both p. yell.; Dembowski, 1863, both white.

Struve	1827·83	*9·50	0·64
”	1836·41	190·20	0·30
Smyth	1842·50	*5·00	0·30
Secchi	1856·96	192·45	0·47
Morton	1856·28	190·44	0·65
Dembowski	1863·23	189·10	...
Dawes	1864·43	193·41	0·36
Secchi	1865·53	193·95	0·25

* These measures should perhaps be + 180.

P. XIII. 127 VIRGINIS. Σ 1757.

R. A. $13^h\ 27^m\ 23^s$.
Dec. N. 0° 22′ 38″.
Mags. a = 8, b = 9.
Colours: Smyth, 1843, pale white, yellowish; Secchi, 1856 white.

Struve	1825·37	10·00	1·60
Smyth	1842·52	37·90	1·70
”	1852·38	51·70	2·00
Secchi	1856·88	52·94	1·84
Dawes	1860·34	54·31	2·31
Dembowski	1863·32	59·04	2·00

Orbital period about 240 years, Smyth.

25 Canum Venaticorum. Σ 1768.

R. A. (1860), $13^h\ 29^m\ 54^s$.
Dec. N. 37° 11′.
Mags. a = 5·7, b = 7·6.
Colours: Struve, white, blue.

	Epoch.	P.	D.
		°	″
Struve	1831·51	76·50	1·07
Mädler	1839·25	71·30	1·06
Dawes	1854·43	36·26	0·35
Secchi	1856·48	25·75	...
Dawes	1865·44	round	...

Σ 1785 Bootis.

R. A. (1860), $13^h\ 42^m\ 48^s$.
Dec. N. 27° 41′.
Mags. a = 7, b = 7½.
Colours: Secchi, 1856, both white.

Struve	1830·12	164·43	3·48
Mädler	1840·85	172·10	3·47
Secchi	1856·36	185·97	3·24
Morton	1859·29	185·24	2·89
Main	1863·31	191·15	2·78
Dembowski	1864·97	192·41	2·60

Σ 1819 Virginis.

R. A. (1860), $14^h\ 8^m\ 18^s$.
Dec. N. 3° 47′.
Mags. a = 7½, b = 8.
Colours: Secchi, 1859, both white.

Struve	1830·39	84·90	0·98
"	1836·43	76·12	1·12
Dawes	1843·34	61·92	1·02
Secchi	1859·45	39·45	1·00
Dembowski	1863·01	32·35	1·29

Σ 1830 Bootis.

R. A. (1860), $14^h\ 11^m\ 12^s$.
Dec. N. 57° 19′.
Mags. a = 8½, b = 9.
Colours: Secchi, 1860, white, blue.

Struve	1830·89	264·00	4·84
Mädler	1838·19	267·60	5·11
Secchi	1860·06	278·23	5·30

π Bootis. Σ 1864.

R. A. $14^h\ 34^m\ 22^s$.
Dec. N. 16° 59′ 57^s.
Mags. a = 3½, b = 6.
Colours: Smyth, 1836, both white.

	Epoch.	P.	D.
		°	″
Herschel	1779·72	96·28	6·17
South	1822·05	97·53	6·90
Struve	1830·32	99·20	5·83
Smyth	1836·51	99·30	6·00
Secchi	1856·79	100·93	5·97
Morton	1857·34	100·36	6·13
Main	1861·31	100·80	6·18
Romberg	1863·27	101·82	6·01
Kaiser	1866·45	100·60	5·73

Σ 1876 Libræ.

R. A. (1860), $14^h\ 39^m\ 0^s$.
Dec. S. 6° 48′.
Mags. a = 8, b = 8.
Colours: Secchi, 1856, both white.

Struve	1832·33	51·73	1·18
Secchi	1856·87	60·84	1·00
Dembowski	1863·39	65·87	1·20

ε Bootis. Σ 1877.

R. A. $14^h\ 39^m\ 5^s$.
Dec. N. 27° 38′ 40″.
Mags. a = 3, b = 7.
Colours: Smyth, 1838, pale orange, sea-green; Webb, 1850, light yell., greenish; Main, 1862, orange, greenish; Secchi, 1855, yell., blue; Struve, 1856, yellow, blue; Dembowski, 1865, yellow, blue.

Herschel	1779·67	301·34	4·00
Struve	1829·39	320·58	2·64
Smyth	1831·46	321·60	3·20
"	1848·54	322·10	2·80
Secchi	1855·37	323·58	2·60
Main	1862·39	324·24	2·60
Dembowski	1864·81	324·70	2·71
Dawes	1865·48	325·50	2·92
Secchi	1865·48	324·70	3·29

Orbital period about 980 years, Smyth.

ξ Bootis. Σ 1888.

R. A. $14^h\ 45^m\ 9^s$.
Dec. N. 19° 39′ 48″.
Mags. $a = 3\frac{1}{2}$, $b = 6\frac{1}{2}$.
Colours: Smyth, 1842, oran., purp.; Webb, 1850, clear yellow, reddish purple; Main, 1862, straw-colour and reddish.

	Epoch.	P.	D.
		°	″
Herschel......	1780·28	24·07	3·42
Struve.........	1829·46	334·11	7·22
Smyth.........	1831·53	332·10	7·30
„	1842·42	322·90	6·90
„	1852·38	316·80	6·50
Dawes.........	1854·46	311·98	6·26
Secchi	1856·88	310·05	6·01
Main	1862·33	305·53	5·68
Dembowski ..	1864·91	301·58	5·44
Secchi	1865·77	300·82	5·41
Talmage......	1866·48	298·53	5·09

Perihelion passage, 1779.

44 Bootis. Σ 1909.

R. A. $14^h\ 59^m\ 21^s$.
Dec. N. 48° 10′ 51″.
Mags. $a = 5$, $b = 6$.
Colours: Smyth, 1842, white, grey; Main, 1862, yellowish; Webb, 1865, pale yellow, tawny.

Herschel......	1781·62	60·06	1·50
Struve.........	1832·24	234·01	2·86
Smyth.........	1839·62	235·30	3·50
„	1847·45	236·20	4·10
Fletcher	1851·47	237·95	4·26
Dawes.........	1854·74	237·77	4·58
Secchi.........	1856·40	238·80	4·55
Main	1862·42	238·30	4·61
Dembowski ..	1863·31	239·53	4·75

1 Coronæ Borealis. Σ 1932.

R. A. (1860), $15^h\ 12^m\ 18^s$.
Dec. N. 27° 21′.
Mags. $a = 6$, $b = 6\frac{1}{2}$.
Colours: Secchi (1856), both white.

Struve.........	1830·28	273·85	1·62
Mädler	1839·52	278·47	1·53
Dawes.........	1854·40	284·07	1·36
Secchi.........	1856·40	285·37	1·14
Dembowski ..	1863.28	290·27	1·18

η Coronæ Borealis. Σ 1937.

R. A. $15^h\ 17^m\ 37^s$.
Dec. N. 30° 46′ 44″.
Mags. $a = 6$, $b = 6\frac{1}{2}$.
Colours: Smyth, 1842, white, golden yellow; Webb, white, yell.; Dembowski, 1863, white.

	Epoch.	P.	D.
		°	″
Herschel......	1781·61	30·41	1·00
Struve.........	1826·76	35·16	1·08
Smyth.........	1832·63	57·20	0·80
„	1842·58	151·30	0·50
„	1852·43	246·80	0·50
Secchi.........	1858·51	359·19	0·53
Dembowski ..	1863·03	19·04	0·81
„ ..	1865·49	27·40	1·02
Dawes.........	1865·44	27·52	1·07
Secchi.........	1865·50	26·26	0·79
„	1866·53	33·13	1·12

Perihelion passage, 1830.

μ^2 Bootis (P. XV. 74). Σ 1938.

R. A. $15^h\ 19^m\ 25^s$.
Dec. N. 37° 49′ 18″.
Mags. $a = 8$, $b = 8\frac{1}{2}$.
Colours: Smyth, 1842, greenish wh.; Secchi, 1856, white, blue.

Herschel......	1782·68	357·14	1·50
Struve.........	1829·73	324·05	1·25
Smyth.........	1832·31	321·40	1·30
„	1842·52	306·10	0·80
„	1853·60	255·00	0·50
Secchi.........	1857·44	231·33	0·48
Romberg ...	1863·63	195·80	0·75
Knott	1864·41	193·64	0·50
Dawes.........	1865·46	190·07	0·48
Dembowski ..	1865·13	186·29	0·50
Secchi	1866·57	180·30	0·30

Perihelion passage, 1849.

δ Serpentis. Σ 1954.

R. A. $15^h\ 28^m\ 21^s$.
Dec. N. 10° 59′ 32″.
Mags. $a = 3$, $b = 5$.
Colours: Smyth, 1842, bright white, bl. white; Main, 1862, both white.

Herschel......	1782·99	227·12	3·00
Struve.........	1802·10	208·50	...
South	1821·33	199·13	3·05
Smyth.........	1842·35	196·20	2·80
„	1851·32	196·50	3·00
Secchi	1855·88	195·52	3·06

[*Continued.*

δ SERPENTIS (*continued*).

	Epoch.	P.	D.
		°	″
Morton	1857·40	193·40	3·37
Main	1862·33	190·16	2·96
Dembowski ..	1863·43	192·20	3·19
Knott.........	1865·41	189·95	3·32
Kaiser	1865·54	192·30	3·18
Dawes	1865·55	191·22	3·24
Talmage ...	1866·45	189·80	3·42

γ CORONÆ BOREALIS. Σ 1967.

R. A. $15^h\ 37^m\ 4''$.
Dec. N. 26° 43′ 30″.
Mags. a = 4, b = 6½.
Colours: Smyth, 1842, flushed wh., uncertain; Secchi, 1857, yellow, purple.

Struve	1826·75	111·05	0·72
Smyth	1839·69	225·00	0·30
,, ...	1848·37	295·00	0·50
Secchi	1857·51	289·32	0·36
Dawes	1858·96	281·46	0·47
Dembowski ..	1862·56	292·90	wedg'd
Secchi.........	1866·51	round.	

ξ SCORPII (= Fl. 51 LIBRÆ). Σ 1998.

R. A. $15^h\ 56^m\ 57^s$.
Dec. S. 10° 59′ 55″.

A : B.

Mags. a = 4½, b = 5.
Colours: Smyth, 1842, bright white, pale yellow; Webb, 1856, yellow, yellowish green.

Herschel......	1782·36	7·58	1·50
Smyth.........	1834·42	6·60	1·40
,, ...	1842·56	23·50	1·20
,, ...	1846·49	24·90	1·00
Secchi.........	1855·54	53·60	0·47
Dembowski ..	1864·95	152·02	sep.
Dawes.........	1865·54	156·91	0·57
Secchi.........	1866·51	161·00	0·4 ±

A : C. c = 7½, grey.

Herschel......	1780·39	88·37	6·38
Smyth.........	1834·42	-6·10	7·20
Dawes.........	1840·56	69·45	7·43
Smyth.........	1846·49	68·10	7·00
Secchi.........	1855·54	70·47	7·50
Main	1861·42	72·80	6·93
Dembowski ..	1865·38	71·02	7·11
Secchi*	1865·49	249·66	7·10

* Evidently should be −189°.

49 SERPENTIS. Σ 2021.

R. A. $16^h\ 7^m\ 0^s$.
Dec. N. 13° 53′ 37″.
Mags. a = 7, b = 7½.
Colours: Smyth, 1839, pale white, yellowish; Main, 1862, both whi.

	Epoch.	P.	D.
		°	″
Herschel......	1783·18	291·33	2·50
South	1823·28	311·57	4·21
Struve.........	1832·70	316·41	3·20
Smyth.........	1839·29	318·10	3·30
Dawes.........	1849·44	321·27	3·3 ±
Smyth.........	1854·58	323·00	3·20
Morton	1855·48	322·41	3·65
Secchi.........	1856·01	332·35	3·45
Main	1862·37	323·43	3·53
Dembowski ..	1864·80	324·60	3·53

Orbital period, about 600 years.

Σ 2026 HERCULIS.

R. A. (1860), $16^h\ 7^m\ 48^s$.
Dec. N. 7° 44″.
Mags. a = 8½, b = 9½.
Colours: Secchi, 1856, both yellow.

Struve.........	1830·94	345·92	2·54
Mädler	1838·05	342·02	2·39
Secchi.........	1856·56	325·60	1·78
Dembowski ..	1865·39	326·10	1·50

σ CORONÆ BOREALIS. Σ 2032.

R. A. $16^h\ 9^m\ 37^s$.
Dec. N. 34° 12′ 12″.

A : B.

Mags. a = 6, b = 6½.
Colours: Smyth, 1843, white, smalt-blue; Webb, 1856, yellow, bluish; Secchi, 1865, white, blue.

Herschel......	1802·74	11·24	...
Struve.........	1827·02	89·21	1·31
Smyth.........	1839·67	145·10	1·60
,, ...	1852·25	176·80	2·20
Secchi.........	1857·61	183·57	2·42
Dembowski ..	1864·94	191·24	2·79
Dawes.........	1865·38	191·48	3·08
Secchi.........	1865·81	192·45	2·97

Perihelion passage, 1826.

A : C. c = 11, dusky.

Smyth.........	1830·76	90·00	43·30
,, ...	1852·25	90·00	46·30
Secchi.........	1855·61	88·25	49·45
Dembowski ..	1862·60	88·35	51·04
Secchi.........	1866·62	87·10	...

α Scorpii (Antares).

R. A. $16^h\ 21^m\ 7^s$.
Dec. S. 26° 7′ 50″.
A : *a*.
Mags. *a* = 1, *b* = 8.
Colours: Secchi, red, blue.

	Epoch.	P.	D.
		°	″
Dawes........	1848·02	273·17	3·45
Secchi	1856·40	273·48	2·99
Smyth........	1857·40	270·00	3·50
Morton	1858·34	275·52	3·29
Powell	1861·09	271·90	...
Dawes........	1864·43	275·71	3·67
Dembowski ..	1865·56	270·43	2·99
Secchi	1866·00	272·90	2·92

λ Ophiuchi. Σ 2055.

R. A. $16^h\ 24^m\ 6^s$.
Dec. N. 2° 16′ 53″.
Mags. *a* = 4, *b* = 6.
Colours: Smyth, yellowish, smalt-blue; Webb, yellowish, tawny.

Herschel......	1783·18	75·30	0·50
Struve........	1825·51	331·80	0·84
Smyth........	1834·48	351·20	1·00
,, ...	1842·50	1·40	1·10
,, ...	1853·25	15·50	1·20
Dawes........	1854·14	14·00	1·31
Secchi........	1857·50	19·88	1·33
Dawes........	1860·36	19·58	...
Secchi	1864·58	22·18	1·25
Dembowski ..	1865·49	25·26	1·51

Perihelion passage, 1798.

ζ Herculis. Σ 2084.

R. A. $16^h\ 36^m\ 12^s$.
Dec. N. 31° 50′ 41″.
Mags. *a* = 3, *b* = 6.
Colour: Smyth, 1842, yellowish wh., orange; Dembowski, 1863, *a* yell.; Secchi, 1858, yellow, violet.

Herschel......	1782·55	69·18	1·00
Struve........	1826·63	23·24	0·91
Smyth........	1835·68	190·00	0·50
,, ...	1842·57	136·90	1·20
,, ...	1852·53	83·80	1·30
Secchi	1858·47	54·59	1·06
Dembowski ..	1863·49	342·45	wedg'd
Secchi	1865·55	86·±	single
Dawes........	1866·81	229·22	0·83

Perihelion passage, 1825.

Σ 2106 Ophiuchi.

R. A. (1860) $16^h\ 44^m\ 30^s$.
Dec. N. 9° 39′.
Mags. *a* = 6·2, *b* = 7·8.
Colours: Secchi, 1856, yell. wh., bl.

	Epoch.	P.	D.
		°	″
Struve........	1827·31	337·50	1·01
Secchi	1856·45	328·40	0·84
Dembowski ..	1863·53	321·30	0·50

Herculis 167. Σ 2107.

R. A. (1860), $16^h\ 46^m\ 18^s$.
Dec. N. 28° 54′.
Mags. *a* = $6\frac{1}{2}$, *b* = $8\frac{1}{2}$.
Colours: Secchi, 1856, yellow, blue.

Struve........	1829·91	148·63	1·12
Mädler	1842·10	161·70	1·03
Dawes........	1852·85	176·36	1·16
Secchi	1856·59	175·43	0·97
Dembowski ..	1865·08	189·28	0·93

P. XVI. 270 Ophiuchi. Σ 2114.

R. A. (1860), $16^h\ 55^m\ 18^s$.
Dec. N. 8° 39′.
Mags. *a* = 7, *b* = 8.
Colours: Smyth, 1832, both white; Morton, 1859, whi., bluish white.

Struve........	1830·97	135·40	1·34
Smyth........	1832·41	137·00	1·50
Dawes........	1847·63	143·38	...
Secchi	1856·84	145·25	1·25
Morton	1859·30	147·38	1·31

Herculis 210. Σ 2120.

R. A. (1860), $16^h\ 59^m\ 6^s$.
Dec. N. 28° 17′.
Mags. *a* = $6\frac{1}{2}$, *b* = 9.
Colours: Secchi, 1856, yellow, blue.

Struve........	1833·25	3·80	3·44
Mädler	1842·30	345·48	2·73
Dawes........	1848·55	322·72	2·31
Morton	1855·61	298·50	2·57
Secchi........	1856·75	290·45	2·48
Dembowski ..	1865·09	272·89	2·98
Talmage	1866·44	272·94	3·56

μ DRACONIS. Σ 2130.

R. A. $17^h\ 2^m\ 33^s$.
Dec. N. 54° 39′ 7″.
Mags. $a = 4$, $b = 4\frac{1}{2}$.
Colours: Smyth, 1839, both white; Main, 1862, both white.

	Epoch.	P.	D.
		°	″
Herschel	1781·73	232·22	4·35
Struve	1832·22	205·06	3·23
Smyth	1839·53	200·30	3·30
"	1854·58	190·70	3·00
Morton	1854·67	*8·22	2·92
Secchi	1857·50	188·37	2·74
Dawes	1859·73	185·47	2·83
Dembowski	1863·14	*2·42	2·63
Main	1862·41	...	2·84
Secchi	1865·59	181·84	2·79

Orbital period, about 600 years (Smyth).

* These measures should evidently be +180.

36 OPHIUCHI.

R. A. $17^h\ 7^m\ 1^s$.
Dec. S. 26° 23′ 28″.
Mags. $a = 4\frac{1}{2}$, $b = 6\frac{1}{2}$.
Colours: Smyth, 1842, ruddy, pale yellow; Webb, 1854, both golden yellow.

Mayer	1780·00	360·00	13·00
South	1825·17	228·28	5·20
Smyth	1835·33	221·40	5·00
Dawes	1841·59	219·26	4·78
Smyth	1842·46	216·60	4·90
"	1857·39	213·80	4·60
Secchi	1857·55	211·30	4·29
Main	1862·43	212·47	4·22

δ HERCULIS. Σ 3127.

R. A. $17^h\ 9^m\ 29^s$.
Dec. N. 25° 0′ 3″.
Mags. $a = 4$, $b = 8\frac{1}{2}$.
Colours: Smyth, 1839, greenish wh., grape red; Main, 1862, straw-coloured, reddish; Webb, 1865, pale yellow, lilac.

Herschel	1779·61	162·28	33·75
Struve	1829·77	173·42	26·11
Smyth	1839·62	175·10	24·50
Secchi	1857·50	177·97	21·92
Main	1862·38	177·59	20·02
Dembowski	1863·14	179·39	20·50
Knott	1866·72	179·61	20·18

ρ HERCULIS. Σ 2161.

R. A. $17^h\ 19^m\ 2^s$.
Dec. N. 37° 16′ 23″.
Mags. $a = 4$, $b = 5\frac{1}{2}$.
Colours: Smyth, 1839, bluish white, pale emerald; Main, 1862, both bluish white.

	Epoch.	P.	D.
		°	″
Herschel	1779·66	300·21	2·97
Struve	1830·35	307·22	3·60
Smyth	1839·74	308·90	3·70
"	1853·39	310·50	3·50
Dawes	1854·73	308·83	3·91
Morton	1855·65	309·45	3·84
Secchi	1856·60	309·71	3·83
Main	1862·37	309·50	3·61

Σ 2173 OPHIUCHI.

R. A. (1860), $17^h\ 24^m\ 12^s$.
Dec. S. 0° 58′.
Mags. $a = 6$, $b = 6{\cdot}8$.
Colours: Secchi, 1858, yell., orange.

Struve	1830·84	323·80	0·62
"	1836·71	single	...
Dawes	1848·45	158·45	1·11
Secchi	1858·56	325·92	0·84

μ^1 HERCULIS. Σ 2220.

R. A. (1860), $17^h\ 41^m\ 0^s$.
Dec. N. 28° 48′.
B : C.
Mags. (Knott), $b = 10{\cdot}5$, $c = 10{\cdot}7$.
Colours: Knott, bluish white.

Dawes	1858·77	59·91	2·02
"	1864·43	77·59	1·81
Knott	1865·43	79·61	1·83
Dembowski	1865·44	81·98	1·20

τ OPHIUCHI. Σ 2262.

R. A. $17^h\ 55^m\ 43^s$.
Dec. S. 8° 10′ 36″.
Mags. $a = 5$, $b = 6$.
Colours: Smyth, 1842, pale white.

Herschel	1783·37	331·60	oblong
Struve	1836·62	199·90	0·44
Smyth	1842·52	227·00	0·90
"	1855·34	238·80	1·10
Dawes	1854·67	238·05	1·22
Dembowski	1863·05	244·57	1·40
Knott	1863·59	246·84	1·20
Secchi	1866·70	248·05	1·60

Orbital period, about 130 years (Smyth).

70 Ophiuchi. Σ 2272.

R. A. $17^h\ 58^m\ 37^s$.
Dec. N. 2° 32′ 30″.
Mags. $a = 4\frac{1}{2}$, $b = 7$.
Colours: Smyth, 1842, pale topaz, violet; Webb, 1850, yell.-orange; Main, 1861, bright yell., reddish; Dembowski, 1863, yell., rose-col.

	Epoch.	P.	D.
		°	″
Herschel......	1779·77	90·00	3·59
"	1804·41	318·48	2·56
Struve.........	1819·63	168·42	4·66
South	1825·56	148·18	4·76
Smyth.........	1835·56	130·60	5·97
"	1842·55	122·40	6·64
"	1852·44	114·90	6·50
Dawes.........	1859·72	109·33	6·24
Main	1861·46	107·30	5·89
Dembowski ..	1863·06	104·96	5·66
Secchi.........	1863·51	104·07	5·28
"	1865·41	102·69	5·40
"	1866·60	101·13	5·27

Perihelion passage, 1806.

α Lyræ.

R. A. $18^h\ 32^m\ 20^s$.
Dec. N. 38° 39′ 15″.
Mags. $a = 1$, $b = 11$.
Colours: Smyth, 1843, p. sapphire, smalt-blue.

Herschel......	1792·32	116·14	42·99
South	1822·87	132·07	42·11
Dawes.........	1830·42	135·03	42·46
Smyth.........	1837·51	137·90	42·70
"	1843·34	140·30	43·40
Secchi.........	1857·55	148·70	45·50
Dembowski ..	1865·63	150·12	46·15

ϵ^1 Lyræ 4. Σ 2382.

R. A. $18^h\ 39^m\ 51^s$.
Dec. N. 39° 31′ 43″.
A : B.
Mags. $a = 5$, $b = 6\frac{1}{2}$.
Colours: Smyth, 1842, yell., ruddy; Webb, 1849, yellow, tawny.

Herschel......	1779·83	33·55	3·54
Struve.........	1831·44	26·06	3·03
Smyth.........	1842·59	20·60	3·20
"	1853·71	19·70	3·00
Secchi.........	1856·23	22·42	3·07
Dawes.........	1859·73	19·32	3·06
Main	1861·45	20·23	3·06
Dembowski ..	1863·09	19·35	3·04

ϵ^2 Lyræ 5. Σ 2383.

C : D.
Mags. $c = 5$, $d = 5\frac{1}{2}$.
Colours: Smyth, 1842, both white; Webb, 1849, both yellowish wh.; Main, 1861, both white.

	Epoch.	P.	D.
		°	″
Herschel......	1779·83	173·28	3·50
Struve.........	1831·44	155·10	2·57
Smyth.........	1842·59	150·90	2·60
"	1853·71	148·10	2·50
Secchi.........	1856·06	148·40	2·57
Main	1861·45	152·39	2·43
Dembowski ..	1863·09	143·97	2·47
Dawes.........	1865·75	144·86	2·56

Smyth (Spec. Hart. 1853) considered that the relationship between ε 4 and 5 Lyræ was not yet established, but that they have a common movement in space.

Σ 2402 Serpentis.

R. A. (1860), $18^h\ 43^m\ 0^s$.
Dec. N. 10° 32′.
Mags. $a = 8$, $b = 8\frac{1}{2}$.
Colours: Secchi, 1856, both white.

Struve.........	1830·20	197·67	0·74
Mädler	1838·83	204·31	0·69
Secchi.........	1856·64	213·41	0·89

P. XVIII. 274 Antinoi. Σ 2434.

R. A. $18^h\ 55^m\ 48^s$.
Dec. S. 0° 53′ 56″.
A : B.
Mags. $a = 9·2$, $b = 9$.
Colours: Smyth, 1838, both white.

Struve.........	1822·67	149·06	26·09
"	1831·57	147·02	25·56
Mädler	1835·53	145·78	25·45
Smyth.........	1838·59	146·80	25·60
Secchi	1856·93	138·87	24·48
Dembowski ..	1864·66	136·85	24·29

B : C. $c = 16$, blue.

Struve.........	1831·57	80·50	1·93
Smyth.........	1838·59	85·00	2·00
Secchi.........	1857·12	68·70	1·73
Dembowski ..	1864·66	69·60	2·79

Σ 2455 Vulpeculæ.

R. A. (1860), $19^h\ 0^m\ 54^s$.
Dec. N. 21° 58′.
Mags. $a = 7\frac{1}{2}$, $b = 9\frac{1}{2}$.
Colours: Secchi, 1857, both white.

	Epoch.	P.	D.
		°	″
Struve	1828·77	144·47	4·92
Mädler	1839·29	136·60	4·41
Morton	1855·66	124·20	3·98
Secchi	1857·29	123·01	3·70
Dembowski	1864·96	115·53	3·53

P. XIX. 108 Draconis. Σ 2509.

R. A. (1860), $19^h\ 15^m\ 30^s$.
Dec. N. 62° 57′.
Mags. $a = 6\frac{1}{2}$, $b = 8$.
Colours: Secchi, 1857, purple, green.

Struve	1832·30	353·00	0·52
Secchi	1857·42	340·28	0·68
Dembowski	1862·98	343·78	0·80

δ Cygni. Σ 2579.

R. A. $19^h\ 40^m\ 45^s$.
Dec. N. 44° 48′ 8″.
Mags. $a = 3\frac{1}{2}$, $b = 9$.
Colours: Smyth, 1842, pale yellow, sea-green; Secchi, 1856, yellow, violet; Dembowski, 1865, white, blue.

Herschel	1783·72	71·39	2·50
Struve	1826·55	40·39	1·91
Smyth	1837·78	30·90	1·50
"	1842·56	25·60	1·80
"	1852·69	14·70	1·50
Dembowski	1865·02	350·76	1·57
Dawes	1865·38	349·62	1·67
Secchi	1865·54	348·45	1·46
Knott	1866·68	348·31	1·70

Perihelion passage, 1860.

O Σ 400 Cygni.

R. A. $20^h\ 5^m\ 30^s$.
Dec. N. 43° 33′ 42″.
Mags. $a = 7\frac{1}{2}$, $b = 8\frac{1}{2}$.
Colours:

Struve, O.	1844·83	336·90	0·65
Dawes	1853·89	320·55	0·65
Struve, O.	1861·62	316·70	0·62

Σ 2696 Delphini.

R. A. (1860), $20^h\ 26^m\ 36^s$.
Dec. N. 4° 58′.
Mags. $a = 8$, $b = 8\frac{1}{2}$.
Colours: Secchi, 1856, both white.

	Epoch.	P.	D.
		°	″
Struve	1831·06	298·92	1·06
Mädler	1838·27	302·80	0·99
Secchi	1856·61	310·26	0·72

λ Cygni. O Σ 413.

R. A. (1860), $20^h\ 41^m\ 54^s$.
Dec. N. 35° 55′.
Mags. $a = 6$, $b = 7$.
Colours: both white.

Struve, O.	1842·66	122·35	0·65
Secchi	1859·61	101·48	0·65
Dawes	1860·81	96·51	0·72
"	1866·99	92·51	0·69

4 Aquarii. Σ 2729.

R. A. $20^h\ 44^m\ 16^s$.
Dec. S. 6° 7′ 45″.
Mags. $a = 6$, $b = 8$.
Colours: Smyth, 1834, pale yellow, purple; Secchi, 1856, both yellow.

Herschel	1782·68	351·30	0·30
Struve	1825·59	25·00	0·81
Smyth	1834·69	45·00	0·50
Dawes	1854·75	101·70	0·3±
Secchi	1856·81	107·86	0·30

ε Equulei. Σ 2737.

R. A. $20^h\ 52^m\ 20^s$.
Dec. N. 3° 46′ 46″.
A : B.
Mags. $a = 5\frac{1}{2}$, $b = 7\frac{1}{2}$.
Colours: Smyth, 1838, white, lilac: Dembowski, 1862, both white.

Smyth	1838·83	290·00	0·50
Secchi	1855·87	287·44	0·81
Dawes	1861·57	285·43	0·96
Dembowski	1862·64	283·87	0·60
Knott	1863·66	287·11	1·01
"	1865·68	288·08	1·07
Secchi	1866·70	290·25	1·06

[*Continued.*

ε Equulei (*continued*).

A : C. $c = 7\frac{1}{2}$, blue.

	Epoch.	P.	D.
		°	″
Herschel......	1780·59	84·21	9·37
South	1823·58	79·21	12·37
Smyth..........	1833·77	77·60	10·70
,, ...	1838·83	78·10	11·20
Secchi.........	1855·87	73·93	10·55
Dembowski ..	1862·64	76·17	10·83
Secchi.........	1866·70	73·13	10·55

61 Cygni. Σ 2758.

R. A. $21^h\ 0^m\ 41^s$.
Dec. N. 38° 3′ 54″.
Mags. $a = 5\frac{1}{2}$, $b = 6$.
Colours: Smyth, 1839, both yellow; Webb, 1850, both deep yellow; Main, 1861, both yellow.

Bradley	1753·80	35·24	19·63
Mayer.........	1778·00	50·58	15·24
Herschel......	1780·72	53·32	16·08
Piazzi	1800·00	69·18	18·20
Bessel	1812·30	79·07	16·74
Struve.........	1819·90	83·02	15·20
South	1822·90	84·41	15·43
Dawes.........	1830·66	90·20	15·70
Smyth.........	1839·69	96·30	16·30
,, ...	1848·07	99·80	16·40
,, ...	1853·80	103·70	17·00
Secchi.........	1855·99	105·93	17·94
Main	1861·85	107·42	17·89
Dembowski ..	1865·15	110·64	18·55
Knott.........	1866·72	111·69	18·76

Orbital period, about 540 years (Smyth).

A. Z. XXIV. 11. A. C. 19.

R. A. (1860), $21^h\ 10^m\ 50^s$.
Dec. N. 63° 49′ 48″.
Mags. $a = 7\frac{1}{4}$, $b = 7\frac{1}{4}$.
Colours: both pale yellow.

Struve, O. ...	1843±	Single.	
Dawes.........	1860·12	246·25	0·89
,, ...	1866·83	244·53	0·98

20 Pegasi. Σ 2799.

R. A. (1860), $21^h\ 22^m\ 24^s$.
Dec. N. 10° 28′.
Mags. $a = 6\frac{1}{2}$, $b = 7\frac{1}{2}$.
Colours: Secchi, 1856, pale yellow.

	Epoch.	P.	D.
		°	″
Struve.........	1831·82	332·88	1·35
Mädler	1835·81	332·90	1·39
Dawes.........	1854·74	320·31	1·18
Secchi.........	1856·27	320·70	1·23
Dembowski ..	1863·09	317·57	1·44

P. XXII. 33 Pegasi. Σ 2877.

R. A. (1860) $22^h\ 7^m\ 42^s$.
Dec. N. 16° 30′.
Mags. $a = 6\frac{1}{2}$, $b = 9\frac{1}{2}$.
Colours: Secchi, 1857, yellow, blue.

Struve.........	1828·95	316·45	7·63
Mädler	1836·57	322·40	7·83
Secchi.........	1857·35	337·45	8·56
Dembowski ..	1863·67	342·20	8·99

ζ Aquarii. Σ 2909.

R. A. $22^h\ 21^m\ 53^s$.
Dec. S. 0° 42′ 37″.
Mags. $a = 4$, $b = 4\frac{1}{2}$.
Colours: Smyth, 1842, very white, white: Webb, 1851, both p. yell.

Herschel......	1779·70	18·21	4·56
Piazzi	1800·00	0·00	3·±
Struve.........	1820·92	358·18	4·40
Smyth.........	1831·83	356·00	3·60
,, ...	1842·59	348·90	2·70
,, ...	1852·81	346·90	3·20
Main	1861·78	340·43	3·28
Dembowski ..	1863·14	339·04	3·52
Secchi.........	1866·70	347·83	3·38
Knott	1866·71	337·01	3·64
Dawes.........	1866·99	336·33	3·32

Orbital period, about 750 years (Smyth).

37 Pegasi. Σ 2912.

R. A. $22^h\ 23^m\ 9^s$.
Dec. N. 3° 44′ 54″.
Mags. $a = 6$, $b = 7\frac{1}{2}$.
Colours: Smyth, 1839, both white; Secchi, 1857, both white.

Struve.........	1831·12	112·63	1·16
Smyth.........	1839·60	118·90	1·10
Dawes.........	1854·54	118·53	0·91
Secchi.........	1857·09	117·56	0·74

Orbital period, about 500 years (Smyth).

Σ 2934 Pegasi.

R. A. (1860) $22^h\ 35^m\ 6^s$.
Dec. N. 20° 42'.
Mags. $a = 7\frac{1}{2}$, $b = 9$.
Colours: Secchi, 1856, both white.

	Epoch.	P.	D.
		°	"
Struve	1830·07	187·83	1·22
Mädler	1838·19	182·21	1·20
Secchi	1856·86	168·23	1·10
Dembowski	1863·82	164·70	1·21

π Cephei.

R. A. $23^h\ 3^m\ 36^s$.
Dec. N. 74° 39' 29".
A : a.
Mags. A = 5, a = 10.
Colours: Smyth, 1843, deep yellow, purple.

Smyth	1843·77	330·00	1·80
Knott	1865·71	5·97	1·15

o Cephei. Σ 3001.

R. A. (1860), $23^h\ 12^m\ 54^s$.
Dec. N. 67° 21'.
Mags. $a = 6$, $b = 8\frac{1}{2}$.
Colours: Secchi, 1858, yellow, blue.

Struve	1832·84	174·97	2·35
Mädler	1839·55	178·19	2·39
Secchi	1858·43	187·15	2·47
Morton	1858·61	186·51	2·60
Powell	1861·01	184·00	...
Main	1862·57	182·16	2·28

P. XXIII. 69 Aquarii. Σ 3008.

R. A. $23^h\ 16^m\ 47^s$.
Dec. S. 9° 12' 0".
Mags. $a = 8$, $b = 8\frac{1}{2}$.
Colours: Smyth, 1834, both flushed; Secchi, 1856, yellow, blue.

	Epoch.	P.	D.
		°	"
South	1824·80	274·04	7·98
Struve	1830·89	273·33	7·54
Smyth	1834·79	272·10	7·50
Mädler	1837·54	271·16	7·11
Secchi	1856·86	265·57	6·11
Morton	1859·91	263·57	5·58
Dembowski	1863·08	262·88	5·59

Σ 3062 Cassiopeæ.

R. A. (1860), $23^h\ 58^m\ 54^s$.
Dec. N. 57° 39'.
Mags. $a = 6\frac{1}{2}$, $b = 7·3$.
Colours: Secchi, 1857, yell., orange.

Herschel	1782·65	320·70	...
Mädler	1825·81	36·70	1·25
Struve	1833·71	108·57	0·55
Morton	1856·90	247·57	1·32
Secchi	1857·60	253·39	1·25
Dawes	1863·86	265·61	1·40
Dembowski	1865·18	269·84	1·37
Knott	1865·71	269·95	1·43
Talmage	1865·98	271·38	1·23

Perihelion passage, 1837.

APPENDIX.

The discrepancies found in the measures of the following objects render it doubtful whether they should be classed with the binaries, although in many instances the difference in the angles of position amounts to several degrees; but, owing either to the closeness of the stars or their minuteness, it is difficult to measure them.

Since the date of Struve's Catalogue, all the objects in the following List have been measured by Father Secchi and the Baron Dembowski, and some of them by Mr. Dawes and other observers: but for comparison the epochs of Struve and Dembowski are considered sufficient.

The right ascensions, declinations, and magnitudes are given for the year 1860 from Secchi's Catalogue.

Σ 44 ANDROMEDÆ.

R. A. 0^h 30^m 36^s.
Dec. N. 40° 13′.
Mags. a = 8½, b = 9·3.

	Epoch.	P.	D.
		°	″
Struve.........	1829·82	258·83	7·86
Dembowski ..	1865·09	263·20	8·66

Σ 208, 10 ARIETIS.

R. A. 1^h 55^m 42^s.
Dec. N. 25° 16′.
Mags. a = 6, b = 8½.

Struve.........	1833·05	25·17	1·98
Dembowski ..	1863·07	33·92	1·43

Σ 234 CASSIOPEÆ.

R. A. 2^h 7^m 6^s.
Dec. N. 60° 42′.
Mags. a = 8, b = 8·7.

Struve.........	1831·55	239·23	0·83
Dembowski ..	1863·45	231·43	0·70

Σ 295, 84 CETI.

R. A. 2^h 34^m 6^s.
Dec. S. 1° 17′.
Mags. a = 6, b = 10.

Struve.........	1831·90	334·62	4·85
Dembowski ..	1863·97	324·73	4·63

Σ 367 CETI.

R. A. 3^h 6^m 48^s.
Dec. N. 0° 12′.
Mags. a = 8, b = 8.

Struve.........	1831·72	281·40	0·95
Dembowski ..	1864.01	257·10	0·50

O Σ 97 TAURI.

R. A. 4^h 58^m 5^s.
Dec. N. 22° 53′ 6″.
Mags. a = 6½, b = 7½.

	Epoch.	P.	D.
		°	″
Struve, O. ...	1849	Separated	
Dawes.........	1866	Single	

Σ 728, 32 ORIONIS.

R. A. 5^h 23^m 18^s.
Dec. N. 5° 50″.
Mags. a = 5·2, b = 6½.

Struve.........	1833·96	207·75	1·04
Smyth.........	1839·20	206·20	1·00
Dembowski ..	1863·33	192·24	...

Σ 749 TAURI.

R. A. 5^h 28^m 30^s.
Dec. N. 26° 50′.
Mags. a = 6½, b = 6·6.

Struve.........	1829·48	203·45	0·67
Dembowski ..	1862·98	186·44	0·60

Σ 963, 14 LYNCIS.

R. A. 6^h 40^m 42^s.
Dec. N. 59° 40′.
Mags. a = 6, b = 6.

Struve.........	1830·88	51·51	0·89
Dembowski ..	1863·44	59·53	0·70

Σ 997, μ CANIS MAJORIS.

R. A. 6^h 49^m 0^s.
Dec. S. 13° 48′.
Mags. a = 5, b = 8½.

Struve.........	1831·20	343·53	3·22
Dembowski ..	1864·09	337·20	2·76

Σ 1216 Hydræ.

R. A. 8^h 14^m 12^s.
Dec. S. 1° 9′.
Mags. a = 7, b = $7\frac{1}{2}$.

	Epoch.	P.	D.
		°	″
Struve.........	1831·24	115·17	0·45
Dembowski ..	1863·35	151·13	...

Σ 1306, σ^2 Ursæ Majoris.

R. A. 8^h 58^m 0^s.
Dec. N. 67° 41′.
Mags. a = 6·3, b = $9\frac{1}{2}$.

Struve.........	1832·14	263·55	4·58
Dembowski ..	1863·19	253·51	3·25

Σ 1316 Hydræ. (A : B.)

R. A. 9^h 0^m 54^s.
Dec. S. 6° 34′.
Mags. a = 7, b = $11\frac{1}{2}$.

Struve.........	1832·88	146·33	6·78
Dembowski ..	1864·84	138·40	6·74

Σ 1348, 110 Hydræ.

R. A. 9^h 17^m 6^s.
Dec. N. 6° 57′.
Mags. a = $7\frac{1}{2}$, b = $7\frac{1}{2}$.

Struve.........	1831·02	334·30	1·09
Dembowski ..	1863·15	328·15	1·66

Σ 1357 Hydræ.

R. A. 9^h 21^m 30^s.
Dec. S. 9° 23′.
Mags. a = 7, b $10\frac{1}{2}$.

Struve.........	1831·20	51·40	7·54
Secchi.........	1856·27	59·58	7·60

Σ 1781 Virginis.

R. A. 13^h 39^m 6^s.
Dec. N. 5° 49′.
Mags. a = 7, b = 7·8.

Struve.........	1856·39	246·56	0·99
Dembowski ..	1864·75	251·77	1·10

Σ 1788, P. XIII. 238 Virginis.

R. A. 13^h 47^m 42^s.
Dec. S. 7° 22′.
Mags. a = $6\frac{1}{2}$, b = 7·3.

	Epoch.	P.	D.
		°	″
Struve.........	1831·38	54·04	2·36
Dembowski ..	1864·85	67·70	2·36

Σ 1825, 121 Bootis.

R. A. 14^h 10^m 6^s.
Dec. N. 20° 47′.
Mags. a = 7, b = 8.

Struve.........	1830·66	185·70	3·44
Dembowski ..	1864·47	178·80	3·89

Σ 1837, P. XIV. 70.

R. A. 14^h 17^m 6^s.
Dec. S. 11° 18′.
Mags. a = 7, b = 8·6.

Struve.........	1829·83	326·87	1·49
Dembowski ..	1865·07	314·15	1·34

Σ 1863 Bootis.

R. A. 14^h 33^m 18^s.
Dec. N. 52° 10′.
Mags. a = 7, b = 7·2.

Struve.........	1830·14	109·75	0·65
Dembowski ..	1864·37	95·23	...

Σ 1865, ζ Bootis.

R. A. 14^h 34^m 30^s.
Dec. N. 14° 20′.
Mags. a = $4\frac{1}{2}$, b = 5.

Struve.........	1830·47	309·17	1·18
Dembowski ..	1864·78	303·25	1·02

Σ 1883 Bootis.

R. A. 14^h 41^m 54^s.
Dec. N. 6° 33′.
Mags. a = 7, b = 7·4.

Struve.........	1830·37	271·96	1·23
Dembowski ..	1863·28	262·70	0·80

Σ 1934 Bootis.

R. A. $15^h\ 12^m\ 24^s$.
Dec. N. 44° 18′.
Mags. a = 8·2, b = $8\frac{1}{2}$.

	Epoch.	P.	D.
		°	″
Struve	1830·88	45·13	5·29
Dembowski ..	1864·88	38·10	6·05

Σ 1957 Serpentis.

R. A. $15^h\ 29^m\ 18^s$.
Dec. N. 13° 23′.
Mags. a = 8, b = 9.

Struve.........	1831·10	163·12	1·41
Dembowski ..	1863·51	155·70	1·53

Σ 2165 Herculis 281.

R. A. $17^h\ 20^m\ 48^s$.
Dec. N. 29° 35′.
Mags. a = $7\frac{1}{2}$, b = $8\frac{1}{2}$.

Struve	1832·16	45·72	6·71
Dembowski ..	1864·57	51·17	7·10

Σ 2199 Draconis.

R. A. $17^h\ 36^m\ 0^s$.
Dec. N. 55° 50′.
Mags. a = 7, b = $7\frac{1}{2}$.

Struve.........	1830·94	116·37	1·66
Dembowski ..	1863·06	101·45	1·65

Σ 2289 Herculis 417.

R. A. $18^h\ 3^m\ 54^s$.
Dec. N. 16° 27′.
Mags. a = $6\frac{1}{2}$, b = $7\frac{1}{2}$.

Struve.........	1829·96	243·12	1·20
Dembowski ..	1862·95	234·33	1·24

Σ 2437 Sagittæ.

R. A. $18^h\ 55^m\ 48^s$.
Dec. N. 18° 58′.
Mags. a = $7\frac{1}{2}$, b = 7·8.

Struve.........	1839·79	80·84	1·08
Dembowski ..	1863·06	71·47	0·80

Σ 2454 Lyræ.

R. A. $18^h\ 59^m\ 24^s$.
Dec. N. 30° 11′.
Mags. a = 8, b = 9.

	Epoch.	P.	D.
		°	″
Struve.........	1831·50	203·97	0·75
Dembowski ..	1865·32	225·97	1·26

Σ 2525, Cygni 22.

R. A. $19^h\ 21^m\ 6^s$.
Dec. N. 27° 2′.
Mags. a = 7, b = $7\frac{1}{2}$.

Struve.........	1830·43	255·90	1·33
Dembowski ..	1865·22	240·84	0·60

Σ 2544 Aquilæ. (A : B.)

R. A. $19^h\ 30^m\ 24^s$.
Dec. N. 8° 0′.
Mags. a = 7·2, b = $9\frac{1}{2}$.

Struve.........	1828·99	218·40	1·14
Dembowski ..	1864·21	208·90	1·20

Σ 2556 Vulpeculæ.

R. A. $19^h\ 33^m\ 24^s$.
Dec. N. 21° 55′.
Mags. a = 7, b = 7.

Struve.........	1829·83	188·40	0·56
Dembowski ..	1864·91	167·72	...

Σ 2576 Cygni.

R. A. $19^h\ 40^m\ 18^s$.
Dec. N. 33° 17′.
Mags. a = 7·6, b = 7·8.

Struve.........	1831·80	318·80	3·59
Dembowski ..	1863·35	308·85	3·27

Σ 2744 Aquarii.

R. A. $20^h\ 55^m\ 54^s$.
Dec. N. 0° 59′.
Mags. a = 6·3, b = 7·1.

Struve.........	1830·16	190·54	1·52
Dembowski ..	1863·24	177·55	1·50

Σ 2746 Cygni.

R. A. $20^h\ 55^m\ 0^s$.
Dec. N. $38°\ 31'$.
Mag. $a = 8$, $b = 8{\cdot}7$.

	Epoch.	P.	D.
		°	"
Struve.........	1830·82	276·25	0·87
Dembowski ..	1863·33	283·70	0·80

Σ 2804, Pegasi 29.

R. A. $21^h\ 26^m\ 30^s$.
Dec. N. $20°\ 6'$.
Mags. $a = 7{\cdot}1$, $b = 8$.

Struve.........	1831·62	316·90	2·90
Dembowski ..	1864·87	324·52	2·75

Σ 2928 Aquarii.

R. A. $22^h\ 32^m\ 6^s$.
Dec. S. $13°\ 20'$.
Mags. $a = 8$, $b = 8{\cdot}3$.

Struve.........	1830·82	327·70	4·69
Dembowski ..	1863·11	319·35	4·38

Σ 2944, P. XXII. 219. (A : C.)

R. A. $22^h\ 40^m\ 36^s$.
Dec. S. $4°\ 57'$.
Mags. $a = 7$, $c = 8$.

Struve.........	1833·01	157·32	55·64
Dembowski ..	1862·68	146·67	50·67

Σ 2976 Piscium. (B : C.)

R. A. $23^h\ 0^m\ 36^s$.
Dec. N. $5°\ 51'$.
Mags. $b = 9\frac{1}{2}$, $c = 9{\cdot}9$.

	Epoch.	P.	D.
		°	"
Struve.........	1828·43	177·68	15·88
Secchi.........	1857·41	183·23	16·31

Σ 3046 Ceti.

R. A. $23^h\ 49^m\ 30^s$.
Dec. S. $10°\ 16'$.
Mags. $a = 8$, $b = 8{\cdot}3$.

Struve.........	1830·15	232·20	2·51
Dembowski ..	1863·92	241·05	2·90

Σ 3050, Andromedæ 37.

R. A. $23^h\ 52^m\ 18^s$.
Dec. N. $32°\ 57'$.
Mags. $a = 5{\cdot}7$, $b = 6{\cdot}3$.

Struve.........	1832·65	191·03	3·78
Dembowski ..	1864·84	199·52	3·17

Σ 3107, Ophiuchi.

R. A. $16^h\ 51^m\ 6^s$.
Dec. N. $4°\ 8'$.
Mags. $a = 8$, $b = 8\frac{1}{2}$.

Struve.........	1831·83	112·30	1·59
Dembowski ..	1864·53	104·37	1·32

XIV. *On Mosses new to Britain.*

By G. E. Hunt, Esq.

Read before the Microscopical and Natural History Sections, November 12th, 1866.

The present Paper records the species of mosses that have been identified in Britain since the publication of Wilson's 'Bryologia Britannica,' 1855. The number there described is about 450, and the additions amount to about 75 species. Doubtless much more yet remains to be done, for whilst some previously little-understood groups have been thoroughly examined (such, for example, as *Campylopus* and *Orthotrichum*) some have hardly been studied—as *e.g.*, *Amblystegium*, many species of which genus may be expected yet in Britain. Of all the groups, however, perhaps the one in the most confused and unsatisfactory state is that group of the *Hypna* called the *Adunca*, the species of which apparently *each* run through a series of forms similar outwardly in each species, according to the stations in which they grow. The various groups of mosses are intimately connected, showing that there is a gradual progression onwards between them; indeed there are few genera that have not close connecting links with those lower and higher than themselves. Thus *Phascum* is linked to *Gymnostomum* by the *Phascum rostellatum* and *G. squarrosum*, both placed by Dr. Schimper in a section *Hymenostomum*; *Gymnostomum* to *Weissia* through *Weissia viridula*, which sometimes is without a peristome. The *Trichostoma* gradually merge into *Tortula* through *Trichostomum rigidulum* &c., which Lindberg places in *Tortula* on account of the inclined teeth of the peristome; and *Tortula* passes back into *Pottia* (a genus without a peristome) through *Tortula cavifolia*, a species which Dr.

Schimper has lately discovered to have the peristome of a true *Tortula*, but so fragile that it has until lately escaped notice, falling off almost invariably with the operculum. It was formerly called *Pottia cavifolia*, var. *gracilis*. *Pottia*, again, merges, through *Pottia minutula* and *Anacalypta Starkeana* (which, indeed, may prove to be one species), into the latter genus. In many also of the mosses fruit is very rare; and this makes the task of discrimination more difficult, and, in the case of new species, more uncertain, until after long and careful comparison. A great amount of light, however, has been thrown on the subject through Dr. Schimper's invaluable publications; and through the liberality of our continental friends, specimens (which are certainly far more valuable than even the best descriptions) are more easily obtainable than formerly. Of the 75 species considered to be well-authenticated as correct and new, 20 are mentioned in 'Berkeley's Handbook,' published in 1863.

Andreæa crassinervia, Br. Nearly allied to *A. rupestris*, L. (*A. Rothii*, Wils. Bry. Brit.). Common on rocks. The question rather seems to be, where in Britain the true *A. rupestris* occurs.

Andreæa falcata, Sch. Allied to the preceding. Discovered on rocks at Cryb-d-Yscil, Snowdon, by Dr. Schimper, June 1865. In Scotland, on hills near Callendar, by Mr. A. M'Kinlay.

Andreæa alpestris, Sch. In Scotland, with the preceding, by Mr. A. M'Kinlay.

Sphagnum recurvum, P. Beauv. (*S. Mougeotii*, Sch.). Allied to *S. cuspidatum*, but distinguished from it by its branch-leaves, recurved when dry, and elliptical, not attenuated towards the apex. It usually grows out of the water, whilst *S. cuspidatum* is almost submerged. The two species, however, in the same circumstances, retain all their specific characters. Common in bogs throughout Britain, but fruiting less freely than *S. cuspidatum*. *S. laricinum*, Spruce, is a variety of this species, and not of *S. contortum* as formerly supposed. A form intermediate between the two occurs abundantly on Carrington Moss.

Sphagnum curvifolium, Wils. MS. Allied to *S. subsecundum*, but differing in the cortical layer of the stem having two or three rows of cellules (whilst there is only one row in *S. subsecundum*), in the absence of marginal pores to the leaves, and the entire acute leaves. A more brittle plant than its ally, discovered by Mr. Wilson in Cheshire, whose description I copy. I believe it to be very abundant near Portree, in Skye.

Ephemerum tenerum, Br. and Sch. Weald of Sussex, Mr. Mitten.

Seligeria tristicha, Brid. On calcareous rocks, Blair Athol. Berkeley's Hand-book of Mosses, 1863.

Seligeria calcicola, Mitt. (*S. subcernua*, Lindb.; *Gymnostomum paucifolium*, fide Carruthers in 'Journal of Botany'). Allied to *S. pusilla*, but with the leaves of a brighter green, wider at the base, more acute above, nerve narrow. On chalk, Sussex Downs, Mr. Mitten.

Dicranella curvata, Hedw. Llanberis, North Wales, Mr. Wilson.

Dicranum longifolium, Hedw. Ben Lawers, July 1866, Dr. Stirton.

Dicranum viride, Lindb. Staffordshire, Mr. Bloxam; identified by Mr. Wilson.

Dicranum trichodes, Wils. Probably a *Blindia*, but, I think, distinct from our British species *Blindia acuta*, to which, however, it is allied. Rocks near Bolton, J. Whitehead.

Dicranodontium aristatum, Sch. First discovered by Mr. A. M'Kinlay on rocks, Lennox woods, near Campsie; occurs on nearly all the Scotch mountains, always, according to M'Kinlay, in company with *Dicranum circinatum*, Wils., of which he considers it a form. *D. circinatum* occurs on wet rocks, as on Ben Voirloch, Loch Maree, &c., and has strongly falcate or circinate leaves, not deciduous; *D. aristatum* in dry places, and has the whole plant slender, and the leaves spreading, silky, delicate, and very deciduous. Under the microscope the two species are quite identical: fruit not known.

Dicranodontium sericeum, Sch., Soccoth Hill, Arrochar, Mr. A. M'Kinlay. A barren form of *Dicranum heteromallum* is very common on sandstone rocks, Cheshire, which may be confounded with this species. Fruit not known.

Campylopus Schwarzii, Sch. (*C. auriculatus*, Wils. MS.). Exceedingly abundant in Scotland, and frequent in the south of Ireland, on rocks—also on Snowdon, North Wales; liable to be overlooked as a state of *C. flexuosus* or *C. longipilus*. The structure of the leaf, however, more nearly resembles that of *C. fragilis*; but the leaves are auricled at the base, have a nerve with only a single layer of large hyaline cells, and are not interspersed with flagella.

Campylopus compactus, Sch. (*C. Schimperi*, Wils.). Frequent in the Scotch mountains, and also in the Hebrides and Shetland Isles. Allied to *C. Schwarzii*, but at once distinguished by its more slender habit and densely cespitose, compact habit of growth.

Campylopus alpinus, Sch. (*C. intermedius*, Wils. MS.). On the moors near Llanberis, North Wales, and on rocks at Stronachlacher, Loch Katrine, G. E. Hunt; near Arrochar, by Mr. M'Kinlay, with fruit very sparing. To be confounded with no species except *Dicranodontium longirostre*, from which it differs in its leaf, with longer, narrower cells in the lower part. The fruit is that of *Campylopus*.

Campylopus Shawii, Wils. A most beautiful species (I think, nearest to *C. setifolius*), having long, setaceous, almost bristle-like leaves. Discovered by Mr. J. Shaw in the

Outer Hebrides, July 1866. This and the preceding species, when found in dry situations, have the leaves falcate, and in wet ones frequently erect.

Campylopus polytrichoides, De Notaris (*C. longipilus*, Br. & Sch. Bry. Eur., but not the Suppl. nor Wils. Bry. Brit.). Next to *C. longipilus*, but with more rigid stems, like those of a *Polytrichum*, and much shorter, wider leaves. Male only known. Cornwall, Jersey, and west of Ireland.

Didymodon gemmescens, Mitt. MS. Leaves gemmiparous, nerve excurrent; allied to *D. flexifolius*. On old thatch, Amberley, Sussex, Mr. Mitten.

Trichostomum sinuosum, Lindb. (*Dicranella sinuosa*, Wils. MS.). Leaves reflexed below, cirrous, serrate at the apex. Barren. On old walls, Bangor, Nov. 1863; on beech trees, Hurstpierpoint, Sussex, Mr. G. Davies.

Trichostomum flavovirens, Br. Sands at Malahide, near Dublin, Dr. Moore; Brighton, Sussex, Mr. G. Davies.

Trichostomum cirrhifolium, Sch. Exceedingly abundant in crevices of rocks, Cromaglaun Mount, Killarney, with setæ, July 1865, G. E. Hunt. Distinguished from *Didymodon cylindricus* (*Trichostomum tenuirostre*, Hook.) by the more dilated bases of the longer leaves; nearest to that species. It is the *Anœctangium Hornschuchianum* of Hook. and Tayl. Musc. Brit., but not the true species.

Tortula cavifolia, Sch. (*Pottia cavifolia*, var. *gracilis*, Wils.) Mud-capped walls: Pontefract, Mr. Nowell; Oxford, Mr. Boswell.

Tortula Vahliana, Schultz. Near to *T. oblongifolia*, Wils., if not the same species. Angmering, Sussex, 1863, Mr. G. Davies.

Tortula intermedia, Brid. Intermediate between *T. ruralis* and *T. lævipila*. Occurs plentifully on rocks and old walls in North Wales and Scotland.

Tortula recurvifolia, Sch. Next to *T. fallax*, but distinguished from it by its shorter, wider, papillose leaves, trifariously arranged. Frequent on rocks and walls in mountainous districts. Fruit, which is very rare, occurred at Buxton, June 1865.

Tortula fragilis, Wils. Distinguished from all other species of *Tortula* and *Trichostomum* by its very fragile and brittle leaves, thick, opaque above, with very large diaphanous cells below, and subulate in the upper portion, usually broken. Ben Lawers, Mr. A. M'Kinlay, Sept. 1865.

Grimmia commutata, Brid. Moncrieff Hill, Perth, by Dr. Stirton, July 1864; and more lately at Stenton Rocks, near Dunkeld, with fruit, Dec. 1865, Dr. White of Perth.

Grimmia subsquarrosa, Wils. MS. Stenton Rocks, Dunkeld, Dr. White.

Grimmia Hartmanii, Sch. Rocks, Wales and Scotland, probably not unfrequent. First pointed out by Mr. Wilson.

Orthotrichum Sturmii, Hoppe. Distinguished from *O. rupestre* by its indistinctly 8-striated capsule, 16 equidistant teeth, and absence of inner peristome. Trap rocks, Scotland and Ireland. First pointed out by Dr. Wood.

Orthotrichum anomalum, Hedw. and of Bry. Europæa, but not of Bry. Brit., which is now named *Orth. saxatile*, Brid. Dr. Wood first pointed this out as a British species from specimens gathered at Aberdour, Fifeshire. Frequent in Scotland, occurs at Conway, always on trap rocks. Capsule 16-striated and peristome-teeth equidistant. *O. saxatile* always on calcareous rocks; capsule 8-striated and peristome-teeth in pairs.

Orthotrichum Shawii, Sch. (*O. orneum*, Wils. MS.). Distinguished from *O. rupestre*, which is abundant on trees in Scotland, by the beautiful white teeth of its peristome, reflexed so far as to lie back on the sides of the capsule, 16 in number, equidistant, no inner peristome, and by the glossy white, slightly hairy calyptra. On an ash-tree, Dailly, Ayrshire. Discovered by Mr. John Shaw.

Orthotrichum pumilum, Sw. On ash trees at Inverkip and Dailly, Ayrshire. The *O. pumilum* of Wils., Bry. Brit., is *O. fallax*, Sw., and has a shorter, wider capsule than the present species.

Orthotrichum obtusifolium, Schrad. Distinguished from all its allies by the plane, not recurved, margins of its ovate, obtuse, gemmiparous leaves. On trees near York and Bristol.

Ulota calvescens, Wils. Distinguished from *U. crispa* by its longer setæ, and shorter capsule, not contracted below the mouth when dry, also by its smooth glossy calyptra. Fruit ripens in June. Discovered at Killarney, on young oaks, by Dr. Moore; Dailly and Loch Doon, on trees, Mr. John Shaw.

Zygodon gracilis, Wils. MS. Leaves plain at the margins, denticulate near the apex; areolation close and punctate above, large and pellucid below; in habit much resembling *Tortula recurvifolia*, Sch. Old walls, Malham, Yorkshire. Discovered by Mr. J. Nowell, and fruit found by him, Sept. 1866.

Atrichum angustatum, Brid. Inflorescence dioicous, leaves narrower, capsule narrower than in *A. undulatum*. Braes of Doune, with fruit, Mr. A. M'Kinlay. Male plant in Sussex, Mr. Mitten.

Atrichum tenellum, Rohl. Dioicous; a smaller, more slender plant than *A. undulatum*. Near Loch Goil Head; near Killin, Perthshire.

Atrichum laxifolium, James (*A. crispum*, Wils. MS.). A most beautiful species, differing from *A. undulatum* in its dioicous inflorescence, shorter, wider leaves, and areolation twice as large. The male plant only found in Britain. Banks and rocks by the streams of the Saddleworth district. Borders of Oakmere, Cheshire, Mr. Wilson. The fertile plant is known in the United States.

Timmia megapolitana, Hedw. Ben Lawers, 1866. Dr. Stirton.

Polytrichum strictum, Menz. (*P. juniperinum,* var. *β. strictum,* Wils. Bry. Brit.). Mountain moors, common.

Webera gracilis, Scleich. (*W. Ludwigii, β. gracilis,* Sch. Syn. Musc.; *Bryum Schimperi,* Wils. MS.). Allied to *Webera Ludwigii.* Grows in very extended bright-green tufts on the grassy upper slopes of the mountains. Fruits on Goat Fell, Ben Lomond, and Ben Lawers; barren on Snowdon.

Bryum barbatum, Wils. MS. Very distinct in its exceedingly fragile, loosely reticulated leaves and delicate stems. Perhaps allied to *B. pallens.* Ben Ledi, Dr. Stirton.

Bryum neodamense, Itz. (*B. pseudotriquetrum,* var. *cavifolium,* Sch.). Distinguished from *B. pseudotriquetrum* by its thread-like stems, wide-spreading, obtuse, concave, cucullate leaves. In Southport, on places on the sands liable to inundation.

Bryum latifolium, Scleich. (*B. turbinatum,* var. *latifolium,* Sch. Syn.). At sight much resembling states of *B. pseudotriquetrum,* but distinguished by its plain-margined leaves. Boggy places on Ben More; Shetland Isles, Shaw and M'Kinlay.

Bryum Sauteri, Br. and Sch. Mr. G. Davies has specimens of this species said to be from Teesdale (Spruce), and Mr. Mitten from Scotland.

Bryum Muhlenbeckii, Br. Although entered in Wilson's Bry. Brit., this plant was not known as British until recently found on the Scotch mountains by Dr. C. Smith and Dr. Stirton.

Bryum Duvalii, Voigt. Boggy places on the Clova mountains, Glen Lyon, Ben Lawers, Hartfell, near Moffatt, Helvellyn.

Bryum murale, Wils. MS. (*B. erythrocarpum*, var. *murorum*, Sch. Syn.). Distinguished from *B. erythrocarpum* by its more inflated capsule, of a deep purple, almost black colour when ripe. Grows only on the mortar of old walls. Marple, Cheshire. Frequent in North Wales. Killarney. *B. erythrocarpum* grows on heath and sandy ground.

Bryum Funkii, Schwægr. Sandy shore, Southport, Mr. Wilson.

Mnium riparium, Mitt. MS. Allied to *M. orthorhynchum*, but the leaves with larger areolation, which much resembles that of *M. serratum*; that species, however, has narrower leaves, and synoicous inflorescence; the inflorescence of *M. riparium* is dioicous. Sussex, in watery places, Mr. Mitten.

Mnium spinosum, Voigt. Ben Lawers, Mr. A. M'Kinlay.

Funaria microstoma, Br. and Sch. Capsule, when dry, smoother than that of *F. hygrometrica*; inner peristome rudimentary. Maresfield, Sussex, Mr. Mitten, May 1864 (Seeman's 'Journal of Botany,' July 1864).

Philonotis cæspitosa, Wils. MS. Between *P. calcarea* and *fontana*; leaves more loosely areolated than in *P. fontana*; nerve about equally strong. Perigonial leaves acute, nerved to the apex. Plant slender. Male plant at Walton Swamp, Warrington, by Mr. Wilson. It has recently been found in Prussia.

Philonotis parvula? Lindb. A very minute species, somewhat resembling *P. marchica*. Barren plant only found. Shanklin, Isle of Wight, Mr. Wilson.

Bartramia stricta, Brid. Leaves green or brown at the base, not white. Nerve excurrent; peristome simple; inflorescence synoicous. Maresfield, Sussex, 1862. Discovered by Mr. G. Davies.

The following four species are entered as varieties of *Fissidens* in Bry. Brit. :—

Fissidens viridulus, L. Probably the plant so named in Bry. Brit. is a variety of *F. pusillus*. Inflorescence synoicous. Banks at Clitheroe, Dr. Wood. Near Bristol. Dr. Wood first pointed out the synoicous inflorescence as probably being a distinguishing feature in this species.

Fissidens incurvus, Schwægr. Inflorescence monoicous. Male flower gemmiform, at the base of the fertile stem; capsule curved, cernuous. The var. *Lylei* occurs in Cheshire, though very rarely. Varying in a single habitat from a minute plant, with 3 or 4 leaves, and setæ ¼ inch long, erect capsule, to a plant twice the size, with capsule slightly curved, and thence into true *incurvus*. The typical *incurvus* is very common on shady banks in Cheshire.

Fissidens pusillus, Wils. Sandstone rocks. Difficult to distinguish from the small state of *F. Lylei*.

Fissidens crassipes, Wils. Monoicous; male flower either gemmiform at the base of the fertile stem, or terminal on a long offshoot. A much stronger plant than the last, with larger leaves and thicker setæ. Capsules erect. Frequent in sluices. Mr. Boswell sends magnificent specimens from Oxford, fully an inch long.

Fissidens decipiens, De Not. (*Fissidens rupestris*, Wils. MS.). Allied to *F. adiantoides*, but distinguished by its pale-margined thickened leaves, more slender growth, and shorter setæ. Young plants, which, according to Mr. Wilson, are the males, are abundant, nestling between the wings of the leaves. Frequent on damp rocks and old walls throughout the more elevated parts of Britain.

Habrodon Notarisii, Sch. First discovered by Mr. J. Nowell at Windermere, but not then distinguished. Killin, Perthshire, Mr. A. M'Kinlay, July 1865. Devonshire, Mr. J. Nowell. On the trunks of elm and whitethorn. Previously only known in Sardinia and Italy.

Myurella apiculata, Hüb., distinguished from *M. julacea* (*Leskia moniliformis*, Wils.) by its less imbricated leaves, which terminate in a long apiculus. Ben Lawers, Perthshire.

Brachythecium campestre, Br. On the ground, Maresfield, Sussex, Mr. Mitten.

Brachythecium Mildeanum, Sch. This is the plant commonly known in Britain as *Hypnum salebrosum*, occurring on the sands at Southport, Fifeshire, Dublin, Cornwall. The true *H. salebrosum* probably occurs on trees near Kirkham Abbey, Yorkshire, Rd. Spruce; also in Sussex.

Brachythecium rutabulum, var. *plumulosum*, Sch. Has the aspect at first sight of a distinct species. Leaves usually narrower than in *Hypnum rutabulum*, somewhat striate, gradually tapering, acute, not acuminate; and the plant has a more glossy aspect. On the same stems, however, occur leaves like those of typical *rutabulum*. Sands at Southport.

Eurhynchium Stokesii, Turn. (*H. prælongum*, var. *Stokesii*, Wils. Bry. Brit.). Doubtless a common species, but liable to be overlooked as *H. prælongum*. On the continent, on the other hand, where it is universally acknowledged, *H. Swartzii* (a most distinct and beautiful species, common throughout Britain) and *prælongum* are confounded, *H. Swartzii* being apparently much the more common species there and usually distributed under the name of *H. prælongum*. *H. Stokesii* is known on rocks in England, Wales, and Ireland. *H. prælongum* has the stem-leaves widely cordate below, lengthened out into a long acumination, branch-leaves lanceolate; *H. Stokesii*, stem-leaves more shortly acuminated, and the branch-leaves ovate acuminate; *H. Swartzii*, both stem- and branch-leaves ovate, not acuminate. *H. prælongum* and *Stokesii* have the capsule olive, suddenly bent at its junction with the setæ; *H. Swartzii*, capsule reddish brown, subcernuous. The habit also of the three species is quite different.

Eurhynchium hians, Hedw. (*H. dispalatum,* Wils. MS.). Allied to *H. Swartzii,* areolæ larger. Sussex, Mr. Mitten.

Rhynchostegium megapolitanum, Bland (H. *confertum,* var. *megapolitanum,* Wils. Bry. Brit.). Sandy shores: Southport, Dublin, Hayle near Penzance, Sussex. I once saw the var. *meridionale,* Schr., at Southport.

Hypnum giganteum, Schr. Frequent in bogs, but very rare in fruit. In fine fruit at Wybunbury Bog, Cheshire. Discovered by Mr. Wilson.

Of the group *Adunca* we have four additional species, viz.:—

Hypnum intermedium, Lindb. = *H. Cossoni,* Schr. ? Frequent in bogs.

Hypnum vernicosum, Lindb. (*H. pellucidum,* Wils, MS.; *H. aduncum,* var. *tenue,* Wils. Bry. Brit.). Wybunbury Bog, Cheshire, Mr. Wilson.

Hypnum Sendtneri, Schr. Bogs, Scotland, A. McKinlay. Probably not unfrequent.

Hypnum Wilsoni, Schr. (*H. aduncum,* Sch. Syn., but not of Berkeley's Handbook, nor Wils. Bry. Brit., which are *H. exannulatum,* Gümb.). Abundant at Southport.

Although the above four species are usually distinguished at once in the field, it is almost impossible to lay down any certain characters by which to identify them; they are all dioicous, and may yet prove (distinct though they are in appearance) to form, along with *H. exannulatum* and *H. Kneiffii,* a single species.

Whilst at this section, it is curious to mark the many species to which the name *aduncum* has been applied (Dr. Schimper has conclusively proved that it is really applicable to the small form of *H. Kneiffii*) :—

1st. To *H. commutatum β condensatum,* together with *H. revolvens,* by Hooker and Taylor.

2nd. To *H. exannulatum,* by Wils. in Bry. Brit.

3rd. To *H. Wilsoni* of Schr. in Sch. Syn. Musc.

4th. To *H. vernicosum* of Lindb., by Wils. MS.; auct. specs., R. A. Hedwig.

5th. To *H. Kneiffii,* small form, which Dr. Schimper has recently shown to be the plant to which Hedwig originally gave the name of *H. aduncum.*

Limnobium engyrium, Schr. Next *L. palustre,* but distinguished by the very large fulvous alar cells of the leaves, and the shorter, wide, annulated capsule. Colour, a fine brown or red. Rocky streams: North Wales, Devonshire, Killarney. This, according to D. Schimper, may be distinct from the continental plant, and, as such, is named by him *L. Mackayanum,* Schr. It is more robust than European forms, but not more so than those from North America.

Hypnum sulcatum, Schr. Allied to *H. commutatum,* but a much smaller, less robust plant; stems slightly pinnate, leaves less strongly nerved and striated. Prostrate in deep crevices of rocks: Ben Lawers, July 1865, G. E. Hunt.

Hypnum falcatum, Brid. (*H. commutatum β condensatum,* Wils. Bry. Brit.); *H. controversum,* Wils. MS.; *H. aduncum,* Hook. and Tayl. olim). Stems irregularly branched, capsule short, cernuous, leaves like those of *H. commutatum.* Bogs, very frequent.

Hypnum imponens, Hedw. Much like *H. cupressiforme,* but distinguished by the large pellucid alar cells of the leaves and small phyllidia. Reigate Heath, Surrey, June 1864, female plant only, Mr. Mitten. See Seeman's Journal of Botany, July 1864.

Hypnum arcuatum, Lindb. (*H. pratense β,* Wils. Bry. Brit.). Common in clay soils.

Doubtful Species.

Sphagnum auriculatum, Mitt. Apparently a var. of *Sph.*

contortum, with stem-leaves very widely auricled at the base. Hayward Heath, Sussex, Mr. Mitten.

Anœctangium pellucidum, Wils. MS. Near Inverary, Mr. Wilson, but probably a form only of *A. compactum.*

Hypnum (*Stereodon*) *canariense,* Mitt. Like *H. cupressiforme,* var. *mamillatum,* but differing in the sharply serrulate leaves, with shorter and wider cells. Turk Mountain, Ireland, Mr. Wilson, 1859.

Orthotrichum patens, Br. Dailly, Ayrshire, Dr. Schimper.

Dicranum robustum, mentioned in Schimper's Syn. and Berkeley's Handbook as perhaps occurring near Warrington, is not this species, but *D. Schraderi.* To the kindness of Mr. Wilson I am indebted for the sight of the plant in this habitat. He also mentioned to me the error.

XV. *On Polymorphina tubulosa.*

By Thomas Alcock, M.D.

Read January 7th, 1867.

In the course of examinations of the Dogs Bay sand, I have collected great numbers of detached branches of *Polymorphina tubulosa,* a form of foraminifer which is not likely ever to be found perfect in shore-sand. I have, however, met with several fine specimens of it with only the tips of the branches broken away; but the most interesting examples are some which are more damaged, and show several structural features difficult, if not impossible, to be seen in any perfect specimens. The main body of the shell of *Polymorphina tubulosa* has the form of Prof.

Williamson's *P. communis*, and appears to be identical with it, this form, so far as I have seen, only taking on the peculiar final development characteristic of *P. tubulosa.* It consists, in the mature state, of the rounded shell of *P. communis*, more or less concealed by several covered passages, commencing at the mouth and taking a direction towards the base of the shell; these passages have their arched walls developed into tubular prolongations, extending in all directions, and soon dividing irregularly into small branches, which, in one or two instances in the specimens shown, will be found to anastomose; they are either closed at their tips, as a small glass tube might be closed in the flame of a blowpipe, or they expand into little cauliflower-like excrescences, which are also apparently closed. The shell composing the parts just described is very delicate and thin compared with that forming the rounded nucleus; and its outer surface is frosted with small glassy projections of an irregularly squared figure, like imperfectly formed crystals. It is evident that this is a hastily deposited shell-covering on the sarcode, developed since the last regular chamber of the shell was formed, and which, instead of collecting itself into a definite shape, to produce a chamber similar to the others, had been surprised, as it were, while fully expanded, by the calcifying process, which consequently gives us a petrified representation of the ordinary appearance of this external sarcode with its pseudopodia protruded, the probable suddenness of the process being illustrated by the cauliflower excrescences which terminate many of the branches, and which have resulted from the contraction of the extremely fine terminal filaments of sarcode. It would appear that this is the final act in the life of the *Polymorphina*, its enfeebled vital power having been insufficient to gather together the sarcode for the formation of another regular chamber; and therefore, properly speaking, the shell is fully formed and perfect before this last addition is made to it. There is evidence, however, in the specimens I have now, to show that the animal must have lived for a considerable time in a full-grown state before it thus terminated its existence by producing a permanent likeness of its living self. These specimens have their arched coverings, with the branches proceeding from them, more or less broken away, so as to expose the floor beneath them, which consists of parts of the strong outer wall of the rounded nucleus, and which in all the cases examined presents the same peculiar appearance. It is riddled through with many large holes, sometimes nearly circular, but oftener oval or kidney-shaped, and so numerous as to open a very free communication between the external sarcode and that in the interior of the shell. It is not unusual to find *Polymorphinæ* (of a different type from these) with a few small round holes in their outer walls; but they are scattered irregularly, are few in number, and have no evident relation either to one another, or to any structural peculiarity of the animal; whereas in the present case they are invariably contained within the area of the floor of the covered passages, and are so numerous, and encroach so much on each other, that in some parts they leave only narrow isthmuses of the original shell-wall between them, and the larger holes have every appearance of having been formed by the union of several smaller ones. It is evident from a consideration of their character, that they have been produced by the removal of shell-material previously deposited; and this gives them a physiological interest; for though it is natural to suppose that a creature which has the power of precipitating carbonate of lime on its surface would also have the power of removing portions of it by solution or absorption, if required, the foraminifera are so structureless that we should hesitate to attribute to them this function without clear and positive proof.

In order to follow the successive changes in the latter part of the life of this *Polymorphina*, as they are illustrated in the specimens before you, the large rounded shells of *P. communis* should be first noticed, in which no opening is perceptible excepting the mouth, showing that at this stage the numerous large holes which are afterwards formed have no existence. The great thickness of the outer walls, compared with that of the internal parts of the shell, shows that the animal must have existed for a considerable time in this condition, during which the surface has been strengthened by repeated deposits of calcareous matter from its coating of external sarcode; and the smoothness and evenness of this surface shows that the coating was at that time spread uniformly over the whole of it. But broken specimens of *P. tubulosa* show that a change in the disposition of the external sarcode has been afterwards made; for in these it is found to have collected itself into two or three irregular bands, always commencing by one end at the mouth and extending towards the base of the shell—an arrangement clearly mapped out by the remains of its ultimately formed shell covering, fragments of which are seen still attached to the surface of the smooth rounded nucleus.

The next event in the life of this *Polymorphina* is the formation of those numerous openings through the thick shell-walls, the observation of which in the specimens before you has chiefly led me to introduce them to your notice. These show, by their definite position and the evidence they give of their progressive formation, that, when the external sarcode has once taken the form of bands, it remains permanently in that state, and that these bands hold a fixed position on the parts of the shell where they were at first placed. Among the specimens shown are some which only differ from ordinary shells of *P. communis* in being remarkably smooth on the surface, and in having numerous large holes, arranged in several rows radiating from the mouth towards the base of the shell, exactly as in undoubted specimens of *P. tubulosa*; but they are without the slightest trace of the external arched coverings and tubular branches. These might at first sight be set down as very much rolled and worn specimens of the ordinary *P. tubulosa*; but there is no evidence in the Dogs Bay sand of other kinds of foraminifera being worn to the extent which would be necessary to produce such a result; and the suggestion is uncalled for in this particular case, since it is evident that, at one part of the life of the animal, its shell must have presented the appearance of these specimens—unless it could be admitted that the holes are formed after the production of the shell covering on the expanded pseudopodia. But this last is clearly a single act, and its plan is evidently not such as would be adopted if the protection of sarcode were the object in view—the subdivision into many projecting branches most delicate and fragile at their points exposing it as much as possible to every injury, and therefore presenting a form and arrangement not at all likely to promote the comfort and convenience of the animal if it were to exist long in that state; and when to this we add that the pseudopodia, which are the means by which the foraminifera communicate with the external world, are sheathed by their shell covering so as to be incapable of action, and, moreover, that every part of the animal becomes completely enclosed, the conclusion seems inevitable that this is not a condition in which it passes any considerable portion of its life, but that it is, as already suggested, merely the closing and final act. The holes through the thick shell, however, present a different history; they show, by the quantity of shell-material removed, and by the way in which separate holes have run together, that time has been spent in their formation; and they have also a clear and intelligible use in the economy of the animal, this being to open free communications between the internal and external sarcode. As to the process by which the shell-matter is removed, it seems impossible under the circumstances to suppose it done in any other way than by absorption by the sarcode in contact with it. Among the specimens shown is one of *P. tubulosa* which has been completely broken open; and this shows that the process of absorption is not confined to the outer walls, but that the inner partitions, which at first formed parts of the walls of the separate chambers, are also in great part removed, throwing the whole of the interior into one large irregular cavity.

The quantity of carbonate of lime deposited, at once in the covering of the external sarcode and its pseudopodia, is so considerable that some unusual source might naturally be looked for to supply it; and this is apparently found in the shell-material redissolved by the process just described, which must eventually lead to the sarcode being excessively charged with mineral matter, and may be considered a sufficient reason for the final catastrophe; and if the view here given of the later stages of the life of *Polymorphina tubulosa* be correet, it adds another point of interest, by showing that the deposit of shell-material, in this one case at least, is more of a chemical than a vital act.

XVI. *On the Mean Weekly Temperature at Old Trafford, Manchester, for the Seventeen Years* 1850 *to* 1866. By G. V. Vernon, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, January 3rd, 1867.

As I am not aware that there have been any carefully deduced values of the mean temperature of this neighbourhood, perhaps the data accompanying this paper may serve until such time as a more extended series can be obtained. I may state that the thermometers used have all been standard ones compared at Greenwich, and all the observations have been reduced to that standard. The thermometers are placed upon a stand 4 feet from the ground, and carefully protected from radiation and other disturbing influences.

Unavoidable omissions in my register I have been enabled to supply by the kindness of my friend Mr. John Curtis, F.M.S., whose thermometers are placed similarly to my own, and within a very short distance from my station. The mean values have generally been determined from the readings of the maximum and minimum thermometers in the shade, combined with readings of a standard thermometer, read once a day, these observations being all made at 8 A.M.: in reducing them, Mr. Glaisher's corrections for diurnal range have invariably been applied. Whilst upon this subject I would like to suggest the great desirability of having these corrections deduced from a much longer series of observations; for although on the whole I find them to agree pretty closely, yet at times great differences exist, especially in comparing the mean temperature deduced from maximum and minimum readings with those of a standard thermometer read at certain fixed hours.

The coldest weeks in the 17 years' average appear to be those ending 7th and 14th January, and the warmest that ending July 22nd.

The week ending December 29th appears to be the one in which the greatest variation of mean weekly temperature is likely to occur, and the one ending August 19th that in which the least variation occurs.

Taking the mean differences for each month, we obtain the following figures:—

	Mean difference.
	°
January	16·40
February	18·62
March	13·40
April	12·05
May	14·37
June	12.42
July	13·22
August	8·20
September	10·26
October	11·34
November	13·62
December	20·22

From this table we see that the greatest amount of variation occurs in December, and the least amount in August.

October does not appear to exhibit any abnormal variation, although, from the amount of barometrical oscillation doing so, it might have been expected it would do.

I should like to see a much longer period observed for determining the values of the mean weekly temperature here, but hope, until such is the case, the values I have given may be deemed worthy of the confidence so short a period can deserve; and I can safely say, as the great mass of the observations were made by myself, that every care has been taken in order that they might be made correctly.

Mean Weekly Temperature at Old Trafford, Manchester, 106 feet above sea-level.

Week ending	Mean temperature 17 years, 1850–1866.	Difference between warmest and coldest week.	Week ending	Mean temperature 17 years, 1850–1866.	Difference between warmest and coldest week.
	°	°		°	°
January 7	37·5	21·7	July 1	60·0	13·4
" 14	37·5	15·0	" 8	58·3	10·7
" 21	38·3	13·9	" 15	60·6	16·9
" 28	38·7	15·0	" 22	61·0	12·1
Feb. 4	38·0	14·2	" 29	59·8	13·2
" 11	39·0	14·3	August 5	58·6	7·7
" 18	38·5	22·8	" 12	59·6	10·6
" 25	37·7	23·2	" 19	57·8	7·2
March 4	39·8	13·8	" 26	58·0	9·1
" 11	39·5	14·1	Sept. 2	57·8	6·4
" 18	40·8	10·8	" 9	57·5	7·6
" 25	41·0	12·2	" 16	56·1	13·4
April 1	42·4	16·3	" 23	54·5	13·8
" 8	45·4	14·1	" 30	53·3	10·1
" 15	46·5	8·2	October 6	53·2	7·5
" 22	48·0	15·1	" 13	49·8	11·2
" 29	48·0	10·8	" 20	49·0	12·7
May 6	47·3	12·8	" 27	47·1	16·6
" 13	49·3	12·9	Nov. 3	44·7	8·7
" 20	52·8	18·4	" 10	43·1	14·0
" 27	54·4	13·4	" 17	40·4	12·6
June 3	54·7	12·6	" 24	40·6	16·8
" 10	56·8	11·1	Dec. 1	39·5	16·0
" 17	57·2	14·1	" 8	40·1	14·3
" 24	58·1	10·9	" 15	40·7	20·9
			" 22	39·0	18·6
			" 29	39·0	27·1

XVII. *Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes.*—Part III. By J. C. Dyer, Esq., V.P.

Read February 6th, 1866.

On Nail-making by Machinery.

Until the early part of the present century, the use of *wood* in the construction of dwelling-houses and other buildings was very general in America. This caused a great consumption of nails, which were mostly imported from England, for the high price of labour among iron-workers prevented domestic nail-making, unless a more summary method could be devised for making them than by the hammer and anvil, which was then the general practice. In this state of the trade many attempts had been made to substitute machinery for the hand-working, to supply the home market for nails.

The kind of nails without heads, called "brads," had long been made by cutting angular slips from the ends of hoop-iron plates, so that the new process to be discovered was that of forming the nail-heads by uniting the process of cutting the slips with one for pressing, in forming the heads of nails, and to effect these two operations by continuous movements from a driving-shaft. A machine was constructed for this purpose and patented in America by Mr. J. Odiorne about the year 1806. Shortly after, a patent was also obtained by Mr. Jacob Perkins for his nail-making machine, which differed widely in its construction from the former, and effected the like purpose by completing the nails in one course of rotative action. At the time of obtaining those two patents, it was held doubtful as to which of the parties had first succeeded in putting his machine into practical operation; and since the forms and principles of action were quite distinct, each of the machines was held to be so far a new invention as to render both patents good in law.

A third patent for a nail-making machine was obtained by a Mr. Reed; but as this machine consisted of a mere combination of those of Odiorne and Perkins, it could at most be considered as containing some improvements on the former inventions. I understood it was so decided shortly after by the tribunals in some legal contests between the respective patentees. In the year 1810 a company in Boston*, having arranged with the patentees above mentioned, sent out to me in London models and specifications of each of the said nail-machines, with directions for patenting them in England, as "a communication from abroad," and for the joint account of the company and myself.

It will suffice here to explain the main features of the mechanism in each of the above-named inventions, as the details of them may be seen at the Patent Office. The processes are as follows:—

(1) In feeding the machine, plates of the proper width and thickness to form the nails are pushed endways over the fixed cutter, against a stop-gauge under the traversing cutter, and they are advanced at such an angle with the line of the cutters as to give to the severed pieces the proper taper to form the point and head ends of the nails, and the plates are turned over after each cut, to reverse the angle at the end for the next nail. The plate-iron is first rolled into sheets, some thirty inches wide, and then sheared transversely into slips to form the nails. By this means the fibres of the iron are lengthwise with the nails, which renders them flexible.

(2) To cut off the slips to form the nails (the width of

* "The Iron Works," on the Charles River, of Messrs. J. and S. Wells & Co., Boston.

the plate being the length of the nails), fixed and moving cutters are used, the one placed on a solid bed, the other passing up and down their edges in contact, in the common way of shearing iron.

(3) One of the "gripping" or holding dies is placed just under the fixed cutter, the face of it and the cutter lying in the same plane, and the counter die moves forward to bring the grooves in both together, so as to hold the nails firmly, allowing a portion of the large ends to stand out beyond the dies, to form the heads of the nails.

(4) The slips when severed are pressed down by the cutter, and a sliding piece advances (under the face of the cutter) to hold the nail against the face and prevent its falling, until it reaches the groove in the gripping dies.

(5) The heading die then advances (the cutter having risen out of the way) and presses the projecting end of the nail into such kind of heads as are sunk in the end of the heading-die, say into the "rose," "clasp," or "clout" heads of the trade.

It often happens that success or failure, with power-driven machines, turns upon slight points in their construction; and this appeared to apply to the patent nail-machines when they were first brought into use in this country. The making of cut nails was not a new manufacture at that time: the method in practice was to cut and head the nails by two separate processes, the passing from one to the other, being done by hand; and this labour was saved by uniting the cutting and heading in one course of operations by the patent machines.

The practical advantages obtained by these machines were found to be nearly inversely as the size of the nails made by them; hence the motive for using them chiefly to make the smaller sort of nails required in the market; but here an unlooked for obstacle arose—the new machine never having been used for, or adapted to, the making of any nails of less than about one inch in length, so that they could not be employed for making tacks, or very small nails, although this branch of the trade offered the greater chance of saving by self-acting machinery. It thus became an object of importance to make such changes in the patent machines as would fit them for making the tacks and small nails as well as the larger ones.

To ascertain whether this could be effected, I began by tracing the successive movements to find where the defective action took place, and *its cause*. After the nails were cut, they were carried down to the heading-dies below the cutter so that the head ends would stand out from the gripping dies when the cutter rose out of the way of the heading die; but before the gripping dies closed upon the nail, a small presser advanced to hold the nail near the point, and prevent its falling out of the line of the dies; but in the case of tacks or small nails, the greater weight of the other end caused it to fall and spoil the work. It therefore became necessary to have the nail held at the head end, in lieu of the point, when thus brought between the gripping dies, and for removing the holder out of the way to admit the advance of the heading die. To effect this purpose I made the bed-cutter in two pieces to act together in one line for cutting, and the portion cutting the head end, after serving to support the nail as above, to slide back out of the way of the heading, and by this simple contrivance, of dividing the bed-cutter into two parts, one fixed, the other moveable, the machine was quite as well adapted for making tacks and minute nails as it was before for making large ones; and this simple change rendered the patent nail-making a complete success; and by far the larger profits accruing from their use came from the machines to which this slight change was applied.

The course of movements of the several machines and their rate of working were exhibited by means of wooden models, constructed and adapted to make the nails and tacks from *lead plates,* merely by turning a winch. These working models were shown and explained to many of the large manufacturers from the districts where the nail-making was mostly carried on.

The average rate of working was about 100 per minute for nails, and 120 per minute for tacks. In after practice the tack-machines were found to average about 80,000 tacks per day of 10 working hours, whilst the best hand workers could only produce from 1200 to 1400 per day. Each machine being tended by one youth (similar to those employed in hand making), it followed that one hand with the machine turned out as many tacks per day as would require over 60 working on the old plan with the hammer and anvil; wherefore this labour-saving machine, of 60 for 1, was sure to supersede the hand makers in all kinds of tacks, except the few sorts required for special purposes; and the average cost of the nails was also so far reduced as to ensure a very extensive demand for them; but the Dudley and other nail-makers could not be induced to change their system of working by adopting the patent machines.

Shortly after, I succeeded in forming a company in London for establishing a patent nail manufactory on a large scale, to which company I transferred the patents, and undertook to superintend the building and starting of the machines for a period of six months; after which the concern was left wholly in charge of the company, the principal party in which was then an eminent London banker, who supplied the capital, and, as head of the concern, selected the parties to be charged with the management of the business. Besides a small sum received in money, I was to receive in compensation for the patent rights a certain share of the profits to arise from their exercise; but, from the lack of mechanical knowledge and business talents that appeared in the management, as also from some differences about the capital in the concern, I was induced to consent to an outright sale to the Company of my contingent share of its profits at a price which did not exceed what my share ought to have produced *per annum*, if the affairs had been conducted with judgment and prudence. I refrain from naming any of the parties, and have merely stated the above facts in justice to myself and to my friends in America before mentioned.

The principal movements in the nail-machine were given by the crank, lever, and wedge actions in common use, and therefore require no special notice; but in that invented by Mr. Perkins the heading dies were worked by what he called a "Toggle-joint," this being the finger-joint. It was suggested to him by observing the process of laying down floor-boards by carpenters:—that of nailing down two boards at some distance apart and placing several loose boards between them; then, bringing the edges of the latter together, they are pressed down between the fixed boards with *a force* that pinches them into the smallest practicable space without crushing the wood. This joint acts upon the principle of the wedge, with two circular faces meeting at the tangent to the circle, and thus acting like a pair of rollers, to pinch or press any body brought between them with a force limited only by the rigidity of the meeting faces. Now this force was found very efficient in pressing the ends of large nails into the several forms of heads required, and is here referred to because it has been since adopted and found very efficient in riveting-machines for making steam-boilers, bridge-girders, and in some others of recent invention.

The new system of nail-making, originating as above stated, has been so widely extended as to give profitable employment to many thousands of workmen, and to supply an important article of very extensive use, both for home consumption and for exportation; wherefore it seemed proper briefly to record the names of the parties from whose joint labours have sprung this important and successful branch of manufacturing industry and trade.

XVIII. *Further Remarks on the Plumules or Battledore Scales of some of the* Lepidoptera, *with Illustrations by* Mr. J. SIDEBOTHAM. By JOHN WATSON, Esq.

[Read before the Microscopical Section, March 25th, 1867.]

HAVING on two former occasions drawn attention to certain peculiar scales belonging to the Rhapalocera division of the Lepidoptera, as serving in some degree for generic or specific classification, and having then limited my remarks to the Pieridæ and Lycænidæ, I now beg to state the result of observations made in other families.

In conjunction with my friend Mr. Sidebotham a complete treatise is in preparation, embracing the whole subject of these plumules; it is to be illustrated with several hundreds of figures; but the completion of the large number of plates necessary will occupy considerable time. The figures will be arranged in generic groups of all the species (or so-called species) which can be obtained, so that observers may judge whether or not the plumules of some differently named species are identical.

In the first place, referring to the genus *Pieris*, already treated of, I desire to draw attention to a small group of species placed at the beginning of the genus, which display no plumules. There are four species, viz. *Thestylis*, an unnamed neighbour, *Clemanthe*, and *Autothisbe*: we have before seen that the plumules are the possession of the males only; now, while deficient in this peculiarity, these species have another of their own, viz. a strongly marked serrated costal margin of the upper wings, easily felt by running the finger along the edge. A short time ago I drew Mr. Hewitson's attention to them, expressing a wish that they might be more correctly placed in a new genus. Mr. Hewitson had some time ago separated this group in his cabinet, and Mr. A. R. Wallace, who is at work on the *Pieridæ*, has done the same; and I was much pleased to receive from him lately an inquiry respecting the absence of plumules, showing that he attaches value to the subject. He proposes to call the new genus *Prioneris*, from the saw-like structure of the costal margin*. The only other species of *Pieris* which I have examined without finding plumules are *Agathon*, *Protodice*, and *Callidice*; the absence is very remarkable in the two latter, as their allies *Daplidice* and *Hellica* are abundantly supplied there-with.

There is a group of this genus to which I did not allude in my first papers, being then doubtful whether its scale could be considered a plumule—that is, of a character serving for distinction; such scales are very abundant on *P. Lycimnia*, *Flippantha*, *Isandra*, and some congeners, showing that these are perhaps all varieties of one insect. You will see a figure of it on Pl. I. fig. 6 *a*; and great has been my surprise to find a somewhat similar form in some members of the Danaidæ family, to which further reference will be made; these have not the bulb-and-socket apparatus.

The interlinking of affinities, and the manner in which Nature loves to repeat her works with variations, are strikingly shown in the plumules generally; and throughout the different families there may be observed assimilations of form existing in widely separated groups, just as is the case in the insects themselves.

* It is interesting to note that a similar serrated costa occurs in some species of *Papilio*, of *Charaxes*, and of *Gonepteryx*; and these are all without plumules.

Plumules is not an appropriate name for some of the forms of the scales which serve for distinctive classification; nor is battledores, which has been applied to those of the *Lycæna* genus; a more universal name would be better, proclaiming their private and peculiar property; and I would suggest Idiolepides (from ἴδιος, private and peculiar, and λεπὶς, a scale); but we will at present continue the former term, plumules.

Before entering into a relation of the families and genera in which these objects are found, let me say something about them specially for microscopists. I have before described them as rotund or cylindrical; but a term suggested by Mr. Sidebotham, viz. bellows-shaped, is more characteristic and correct. It is manifest that, if the form of plumule of *P. Rapæ* were actually rotund or cylindrical, the peduncle and bulb would often, on a slide, be covered with the membrane; but, when mounted, the scale always shows the lobes on each side of the bulb, proving its, in some degree, appressed form.

Then, as to the parts of the insect where they are to be found: generally on the upper surfaces of the wings, sometimes most abundant on the primary, sometimes on the secondary; usually in or near the discoidal cells of both wings; but occasionally very strangely placed, as we shall presently see when referring to the genus *Euplœa.*

The best way of collecting and mounting is by gently pressing the wing of the insect against a glass slide, by which means a sufficient quantity of the scales will adhere: to get a clean mounting, it is necessary to brush off the dirt which may be on the wing with a camel-hair pencil; but then care must be taken that the pencil does not convey scales to slides of other species; and, in fact, suspicious care must be used when mounting a number of slides, as the light scales will often be floating in the air and alighting unexpectedly on the slide which is under operation. Then cover with a thin glass, and fix with paper. In some small insects it is more convenient to take off the scales in the first instance on the thin cover, and then to affix it to the slide. The plumules are mostly of so delicate a membranous structure, and so deficient in pigment, as to become too transparent (and sometimes almost invisible) in Canada balsam; but it may be used with good effect where they carry some amount of pigment; and the structure of those of the *Lycænidæ* is thereby beautifully shown, although these are among the most hyaline. In some genera and species they are so small and so finely striated as to make a $\frac{1}{8}$-inch object-glass desirable to resolve them satisfactorily, or at least a $\frac{1}{4}$, with a B or C eye-piece; while a $\frac{1}{2}$-inch is sufficient for others.

The striæ particularly should be observed with high powers. Occasionally scales of different species appear under a low power identical, but a higher one reveals a complete difference of structure.

Taking for our text-book the 'Genera of Diurnal Lepidoptera' of Doubleday, Westwood, and Hewitson, we proceed to state the additional families and genera where the plumules have been found. Throughout this work there is evidenced an inkling of the writer's appreciation of the value of the scales, or of some of them, for aid in classification, but more in the direction of genera than of species; and the distinct character of the plumules is not recognized, nor the probability remarked that the insects are furnished with two classes of scales, as was suggested in a former article. In the consolidated treatise we are undertaking, we shall notice, *seriatim*, all the families and genera, with remarks on peculiarities of some scales, even when they do not assume the form of plumules.

In the work above named, the Diurnal Lepidoptera are divided into 15 families.

Family I. PAPILIONIDÆ.—No plumules found.

Family II. PIERIDÆ.—Found on many species already mentioned.

Family III. AGERONIIDÆ.—None.

Family IV. DANAIDÆ.—It is in the genus *Euplœa* only of this family that plumules have been found; and they bear a very different form from that of those of other genera, with the exception of an approach in *Pieris Lycimnia,* Pl. V. fig. 6 *a**. The typical form of *Euplœa* is shown in Pl. V. figs. 1 and 2; and I have found them on 13 species, whether or not all distinct may be questioned, but there can be no doubt about the two in the plate. When the plates containing all the figures are ready, their similarities and differences will be apparent. It is to my friend Mr. Labrey's industry and information that I owe a knowledge of these plumules. I had often examined the insects unsuccessfully: and it well might be so; for these scales are not found in the ordinary places, but, as I believe, only in the upper part of the secondary wings, where overlapped by the primary and fringing the light-coloured patch on the inferior wings; here they exist in *Euplœa Midamus* in large and compact masses, presenting an appearance similar to a bed of bulrushes at the edge of a marshy lake. I cannot doubt that further search in this genus will be rewarded with valuable evidence as to the identity or difference of many species.

Family V. HELICONIIDÆ.—Here I have been able to find plumules in the genus *Heliconia* only, but in 26 species. They are of singular interest in our view of their use for classification and for the determination of species. (In illustration of the following remarks I produce specimens of the insects to which reference is made.)

* The plates have been drawn by Mr. Sidebotham, specially for the illustration of this paper.

Mr. Bates, in his 'Naturalist on the River Amazons,' vi. pp. 251 &c. (1863), devotes some pages to show that many species of this genus have had a common origin, proving the "manufacture of new species in nature." He takes "*Melpomene*, abundant in Guiana, Venezuela, and some parts of New Granada," as the original species, and argues that *Thelxiope*, "ranging 2000 miles from east to west, from the mouth of the Amazons to the eastern slopes of the Andes," is merely a local modification; and yet he says that "if local conditions, acting directly on individuals, had originally produced this race or species, they certainly would have caused much modification of it in different parts of this region; for the Upper Amazons country differs greatly from the district near the Atlantic in climate, sequence of seasons, soil, forest-clothing, periodical inundations, and so forth." He then proceeds to contend "that there is some more subtle agency at work in the segregation of a race than the direct operation of external conditions," and that the principle of natural selection, as lately propounded by Darwin, "seems to offer an intelligible explanation of the facts."

The plumules, however, enable an observer to detect without doubt the species: if those taken from any number of specimens of the species *Melpomene*, *Thelxiope*, *Aœde*, and *Vesta* are examined, each can be named; but mere varieties of each species will exhibit the same plumule, as in the case of *Thelxiope* and *Aglaope*. Surely, if the Darwinian theory were true, that a change is constantly in progress, we ought to find plumules of an undecided form in some specimens, partaking of and hovering between the characteristics of their supposed ancestors. With all deference to Mr. Bates, whose opportunities of observation have been great, I cannot but regard his theory as improbable and far-fetched. Why should *Thelxiope* have descended from *Melpomene* rather than the latter from the former? and why suppose any necessity for derivation at all? Butterflies are often confined to narrow localities; and when species are widely spread in various geographical habitats, varieties occur; but the species continue recognizable, and the more specimens can be obtained the more certain is their determination. It is much more probable and philosophical to suppose that an intelligent Creator placed His creatures in such localities and conditions as suited their various requirements, and maintained them there; and, as Mr. Bates says, "a proof of this perfect adaptation is shown by the swarming abundance of the species."

This swarming abundance and teeming variety of life in the Amazons region is not confined to the insect tribe; for "Prof. Agassiz, who has lately been engaged in examining the fish of that river, states that he has not found one fish in common with those of any other freshwater basin, that different parts of the Amazons have fishes peculiar to themselves, that a pool of only a few hundred square yards showed 200 kinds of fish (which is as many as the entire Mississippi can boast), and that in the Amazons itself 2000 different kinds exist." (Athenæum, Mar. 23, 1867.)

We must look in vain for specific distinction, if such different insects as *Heliconia Melpomene*, and *Thelxiope* are to be regarded as of one common origin. Mr. Bates admits that "both are good and true species, in all the essential characters of species; for they do not pair together when existing side by side, nor is there any appearance of reversion to an original common form under the same circumstances."

Family VI. ACRÆIDÆ.—No plumules found.

Family VII. NYMPHALIDÆ.—Found in the following genera:—

Eueides.—Here on 5 species they have been detected, and they bear a very strong similarity to those of the *Heliconidæ*, the insects themselves being also alike. A comparison of *Heliconia Vesta* and *Eueides Thales* would induce a casual observer to regard them as almost identical; but Mr. Hewitson* has pointed out "a difference in the position of the discoidal nervures of the posterior wing, as well as in the orange rays which proceed from the base of the posterior wing;" and he well says, "If a butterfly or a genus resemble another (though placed systematically at a distance from it), let it be in colour or in form, it may be expected to resemble it in other characteristics." The plumule of *Eueides Thales* you will see on Pl. V. fig. 5.

Colænis.—Found in 5 species, two of the forms being shown on Pl. V. fig. 6.

Agraulis.—Found in 3 species, introducing a very distinct type, which we shall see is, as it were, played upon and repeated with variations in other genera. Pl. VI. fig. 7.

Terinos.—On the two species of this genus which I possess there is a very peculiar pear-shaped scale, not, however, I think, a plumule. I notice this genus here in its place because it possesses hairs of a bifid form at the apex, of a character similar to some which will presently be noticed under the genus *Argynnis.*

Lachnoptera.—This genus consists of a single species, "*Iole*;" and its very peculiar scale is shown on Pl. VI. fig. 8. It was noticed by Doubleday, who regarded it as probably of a sexual character, although he had never seen a female; nor have I. He describes it as "a hair-like scale, terminating in a vane like the feathers of the raquet-tailed humming-birds."

Argynnis.—Plumules found on 15 species. They have often been noticed by microscopists; and two were figured

* Journ. of Ent. vol. i. p. 156.

in the article by Deschamps, to which reference was made in my first paper. The type is shown on Pl. VI. fig. 9, and Pl. VII. fig. 17. Besides these plumules, however, there are found on some species some plumule-like hairs, as shown on Pl. VII. fig. 16. Many of the Lepidoptera possess fringes of long hairs, but with a simple pointed termination, while these have a large brush at the end. I doubt whether they should be regarded as serviceable for specific distinction; but further examination is desirable. It is strange that I have not succeeded in finding plumules on any individuals of the second section of the diurnal species of this genus, nor on any of the very closely allied genus "*Melitæa*." These two genera have been much mixed together by entomological classifiers. Will the presence or absence of plumules serve for a permanent separation?

Athyma.—Plumules have been found on 11 species, a type being shown on Pl. VI. fig. 10. I have searched in vain for them on the closely allied genus *Neptis.* There has been great difficulty in the generic separation of this group; but perhaps hereafter the existence of plumules may aid classifiers with regard to the allied genera *Athyma, Neptis,* and *Limenitis.*

Eteona Tisiphone.—This insect, although placed among the Nymphalidæ in our text-book, belongs no doubt to the family Satyridæ, as is now generally admitted, and as its plumule would serve to prove.

Thus we see that in the large family Nymphalidæ plumules have been discovered in but few genera, and those principally of the subfamily Argynnidæ of some authors.

Families VIII. *and* IX. MORPHIDÆ *and* BRASSOLIDÆ.—No plumules.

Family X. SATYRIDÆ.—Here we have generally a well-marked type, subject, however, to many aberrations.

Corades.—Found in 3 species.

Taygetis.—Found in 4 species. Pl. VI. fig. 11, exhibits the form of the plumule of *T. Rebecca,* reminding us in its outline strongly of *Pieris Belladonna;* the striæ, however, are very different; and this group does not possess the bulb-and-socket apparatus.

Zophoessa.—Found in 1 species.

Euptychia.—Found in 5 species. See the singular form of that of *Canthe,* Pl. VII. fig. 13, reminding us again, by its large lobes, of some of the *Pieridæ.*

Erebia.—Found in 13 species. A type shown in Pl. VII. fig. 14.

Chionobas.—Found in 7 species. A most interesting northern group, principally inhabiting Lapland and Norway.

Lasiommata.—Found in 10 species, the forms of *Mæra* and *Megæra* having been figured by Deschamps.

Satyrus.—Found in 32 species. A type, *Beroë,* is shown on Pl. VII. fig. 15. The plumule of *Janira* has long been known.

Families XI., XII., *and* XIII. EURYTELIDÆ, LIBYTHEIDÆ, *and* ERYCINIDÆ.—No plumules found.

Family XIV. LYCÆNIDÆ.—To these battledore scales I have before called your attention.

Family XV. HESPERIDÆ.—None found.

Having thus completed an account of observations already made, I annex a Table showing an approximate estimate of the number of species where plumules have been found to exist, and of the genera possessing them. Doubtless there is room for further research; and I would urge upon all a prosecution of this interesting study. I doubt not that among the Rhopalocera there will be further discoveries made; and the Heterocera afford an untravelled field to an observer. It will be very interesting to entomologists to learn whether any plumules are to be found among them, or any other class of scales serving for generic or specific classification.

Genera in which Plumules have been found.

Genus	Species.	Family	
Euterpe, about	19		
Pontia	1		
Pieris	132		
Zegris	2		
Anthocharis	29		
Thestias	4		
Hebomoia	3		
Eronia	11	—Pieridæ	201
Euplœa	15	—Danaidæ	15
Heliconia	26	—Heliconidæ	26
Eueides	6		
Colænis	5		
Agraulis	3		
Terinos	2		
Lachnoptera	1		
Argynnis	17		
Athyma	11	—Nymphalidæ	45
Corades	3		
Taygetis	4		
Pronophila	9		
Debis	5		
Zophoessa	1		
Euptychia	5		
Erebia	13		
Chionobas	7		
Lariommata	10		
Satyrus	32	—Satyridæ	89
Lycæna	121		
Danis	9		
Dipsas	1	—Lycænidæ	131
30 Genera.	507	Total	507

XIX. *On the Variable Star R Vulpeculæ.* $\alpha = 20^h\ 58^m\ 22{\cdot}9^s$. $\delta = +23°\ 17{\cdot}2'$. *Ep.* 1865·0. By GEORGE KNOTT, F.R.A.S. Communicated by JOSEPH BAXENDELL, F.R.A.S.

Read at a Meeting of the Physical and Mathematical Section, March 1st, 1866.

THIS star, which is No. 457, hour xx, in the Palermo catalogue, was first recognized as variable, so far as I am aware, at the Observatory of Bonn. It appears to have been observed with some care by Dr. Winnecke at the Pulkowa Observatory; and in a letter to the Rev. B. Main, printed in vol. xxii. of the Monthly notices of the Royal Astronomical Society, p. 285, that able astronomer assigns the following elements, "which represent seven maxima observed in the course of three years, with reference to Piazzi's estimations of magnitude in August, 1803," viz.:—

Period = 138·6 days.
Epoch = 1860, Nov. 6.

Having observed this star with more or less regularity during the past four years, it occurred to me that it would be interesting to compare the elements resulting from a discussion of my own observations with those which had been deduced by Dr. Winnecke. The results of this discussion I have now the honour of presenting to the Manchester Literary and Philosophical Society.

Projecting my observations in the usual way, I obtain the following dates of maxima and minima, with the corresponding magnitudes :—

Maxima.		Minima.	
1861. Dec.	30·0, 8·4 mag.	1861. Oct.	26·3, 13·6 mag.
1862. Oct.	5.0, 7·8 „	1863. Sept.	18·0, 13·2 „
*1863. Nov.	19·4, 7·6 „	1864. June	19·5, 13·2 „
1864. Aug.	16·3, 7·5 „	Nov.	4·0, 13·1 „
1865. Jan.	7·3, 7·7 „	1865. Aug.	6·3, 12·8 „
May	25·5, 7·8 „	Dec.	14·3, 13·7 „
Oct.	5·5, 7·5 „		

Treating the seven observed maxima according to Mr. Baxendell's method, we obtain the following elements :—

Period = 137·59 days.
Epoch = 1864, April 4·95.

Comparing the observed times of maximum with those calculated from these elements, and also from those of Dr. Winnecke, we obtain the following differences between calculation and observation :—

Knott's Elements. Calc. – Obs.	Winnecke's Elements. Calc. – Obs.
days.	days.
+1·41	−3·2
−2·41	−5·0
−0·01	+0·4
+4·24	+7·1
−2·23	+1·3
−2·78	+1·7
+1·81	+7·3

the sums of the squares of these numbers being 43·75 and 143·78 respectively. But while it thus appears that my own observations accord moderately well with Dr. Winnecke's elements, it must be confessed that these latter represent more satisfactorily than my own (as indeed might be expected) the magnitude-estimates of Piazzi in the year 1807 and 1810, as given by Dr. Winnecke in No. 1224 of the 'Astronomische Nachrichten.' At the same time it must be remembered that my own elements were deduced

* The projection of a series of his own observations of this maximum obligingly communicated to me by Mr. Baxendell, yields the following results, in gratifying accordance with my own :—Date of maximum, 1863, Nov. 18·9, mag. 7·5.

solely from my own observations, without reference to any of earlier date.

Treating the six observed minima in the same manner, we obtain the following elements, the period presenting a striking accordance with that deduced from the observed maxima—

Period = 137·55 days,
Epoch = 1864, June 17·50,

the differences between the calculated and observed times of minima being—

Calc. – Obs.
days.
+2·35
–1·60
–2·00
–1·95
–2·15
+5·40

An examination of the mean light-curve (a copy of which accompanies this communication), which was laid down from the coordinates resulting from a discussion of all the observations I have obtained, yields the following results:—

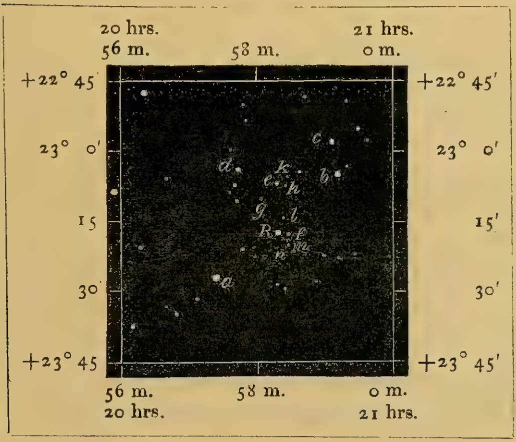
Mean magnitude at maximum	7·77
Mean magnitude at minimum	13·14
Mean range of variation	5·37 magnitudes
Mean magnitude	10·32
Interval from minimum to maximum	66·0 days
Interval from maximum to minimum	71·6 days
Interval from min. to mean mag.	25·8 days
Interval from mean mag. to max.	40·2 days
Interval from max. to mean mag.	37·3 days
Interval from mean mag. to min.	34·3 days
Interval from mean mag. before to mean mag. after maximum	77·5 days
Interval from mean mag. before to mean mag. after minimum	59·1 days

An examination of the various results of observation and calculation given in the former part of this paper suggests the following general remarks:—Like many other variable stars, R Vulpeculæ increases more rapidly than it decreases.

The intervals between successive maxima and minima are subject to some little irregularity. And the observed magnitudes at maximum and minimum vary to the extent of some nine-tenths of a magnitude. Still, as compared with some other stars, the movements of this variable must be regarded as tolerably regular. Although by no means so highly coloured as some variables, I have frequently noted the star in my observation-book as "ruddy," or "decidedly ruddy." The maxima observable during the present year will fall, according to my own elements, on the following days :—July 9·5 and November 24·1. The observable minima will occur on May 6·3 and September 20·8.

The stars which I have used for comparison with R Vulpeculæ are shown in the small chart which accompanies this paper; and their magnitudes are as follows, the numbers in the cases of *a, b, c, d, g, l, m, n* being the means between my own values and those assigned by Mr. Baxendell :—

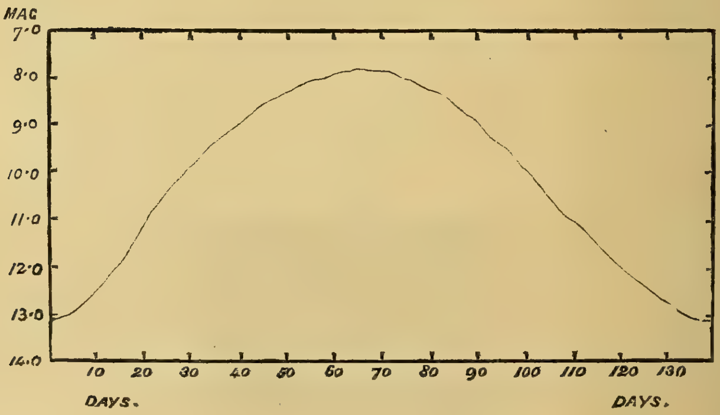
$a = 7·1$	$g = 10·0$
$b = 7·3\pm$	$h = 10·5$
$c = 8·3$	$k = 10·9$
$d = 9·1$	$l = 11·4$
$e = 9·5$	$m = 11·9$
$f = 9·7$	$n = 12·6$



The star *b* is variable to the extent of some few tenths of a magnitude, and may therefore with advantage be rejected as a comparison star; *g* is a double star, the magnitude assigned above being that of the two components seen as *one star.*

I cannot close this communication without gratefully acknowledging the courtesy and kindness of Mr. Baxendell in freely communicating to me his own methods, and in affording me all necessary explanations in cases of doubt or difficulty. It is to be hoped that the time is not far distant when the best methods of procedure in this branch of the science will find a place in our textbooks of practical astronomy.

Mean Light-curve of R Vulpeculæ, as derived from the observations at the Woodcroft Observatory in the years 1861–1865.



XX. *Observations of the Meteoric Shower of Nov.* 13–14, 1866. By JOSEPH BAXENDELL, F.R.A.S.

Read November 27th, 1866.

THE early part of the night of November 13th was very squally and cloudy, with showers of rain and hail, and occasional flashes of lightning. At about 12h. 15m. a break occurred near the zenith, and in a few minutes the clouds had almost entirely disappeared. My observations of the meteors commenced at 12h. 16m. Greenwich mean time, and were directed principally to the determination of the time of maximum frequency, and the position of the radiant-point. The observations of frequency were as follows:—

		Number of Meteors observed.
From $12^h\ 16^m$	to $12^h\ 32^m$	60
32	48	153
48	13 4	287
13 4	20	378
20	26	122
26	42	316
14 19	14 42	54
15 20	15 35	6

From 13h. 42m. to 14h. 19m., and again from 14h. 42m. to 15h. 20m., the observations were interrupted by clouds and rain, and only 73 meteors were counted during the two intervals. At 15h. 35m. clouds came on again very suddenly, and the sky remained obscured at 16h. 5m., when I ceased to watch.

During the whole time of observation the sky was rarely entirely free from clouds for more than two or three minutes; but the errors arising from this cause are probably pretty evenly distributed through the intervals above given, and cannot, therefore, materially affect the final determination of the time of maximum frequency. The results of the observations are as follows:—

		Average number of Meteors per minute.
At 12^h	24^m	3·7
	40	9·5
	56	17·9
13	12	23·6
	23	20·3
	34	19·7
14	$30\frac{1}{2}$	2·3
15	$27\frac{1}{2}$	0·4

The curve formed by a projection of these numbers gives 13h. 12m. as the time of maximum frequency. The probable error of this result can hardly exceed one minute.

In order to determine the position of the radiant-point, the positions of the intersecting points of the paths, continued backwards, of a great number of pairs of meteors were noted. By far the greater number of these points fell on a space bounded by lines joining the stars γ, ζ, μ, ϵ, and η Leonis; and, allowing equal weights to all the observations, the mean position was found to be R. A. 9h. 58m. 12s. $=149° \, 33'$; Dec. $22° \, 57{\cdot}5'$ north. Calculating the position referred to the ecliptic, we have lon. $=143° 41{\cdot}0'$; lat. $=9° \, 54{\cdot}5'$ north.

At the time of maximum frequency the earth was advancing in the direction of a point on the ecliptic the longitude of which was $141° \, 28{\cdot}3'$, or $2° \, 12{\cdot}7'$ less than that of the radiant. It appears, therefore, that the meteors were crossing the earth's orbit from within outwards, and that their aphelion distance is very sensibly greater than the earth's radius vector on the 13th of November.

The velocity of the earth in its orbit on the 13th of November is 18·38 miles per second, and the velocity of the November meteors when they enter the earth's atmosphere has been found to be 40 miles per second. With these data and the latitude of the radiant-point as given above, 9° 54·5′ N., we find that the inclination of the orbit of the mass of meteors to the plane of the ecliptic is 17° 59′, and that their orbital velocity at the time they encounter the earth is 22·31 miles per second. The excess of this velocity over that due to their distance from the sun arises, in part at least, from the accelerating effect of the earth's attraction.

An attempt was made to estimate roughly the relative numbers of meteors of different magnitudes, and it was found that they occurred in about the following proportions:—

Out of every 100 meteors, 10 were above the 1st mag.; the brightest of these were 2 to 3 times as bright as Sirius;

15	were between the	1st and 2nd mag.
25	„	2nd and 3rd mag.
30	„	3rd and 4th mag.
15	„	4th and 5th mag.
5	were below the	5th mag.

The average magnitude was 3·0.

The trains left by many of the larger meteors had a beautiful emerald green colour; others were of an ashy grey, and the remainder white. The meteors themselves were mostly white or bluish white; but many were of a fine golden colour.

In order to give some idea of the great velocity with which the meteors enter the earth's atmosphere, it may be remarked that it would be sufficient to carry a body through the entire circuit of the earth in an interval of less than ten and a half minutes.

As I had the good fortune to witness the great meteoric shower which occurred on the morning of the 13th Nov. 1833, I may state that the late display was far inferior to it, both in the number of meteors seen and in the brilliancy of the larger ones, and I am therefore inclined to think that a much finer display may be expected to occur in November next. At the time of the 1833 great shower I was at sea, off the west coast of central America; and although I then knew little about meteors, and the idea of a radiant-point had not, so far as I am aware, ever occurred to any astronomer or meteorologist, the tendency of the great majority of the meteors to diverge from a particular region of the heavens was so strongly marked that it at once engaged my attention; and I find, on referring to my notes, that I fixed the central point of this region in the constellation Cancer, a few degrees east of the stars δ and γ, and not in Leo, as observed by Professor Olmsted and others in the north-eastern portion of the North American continent. A great number of the meteors, however, had other radiant-points; and some of the finest moved in long horizontal arcs, or in directions nearly perpendicular to that of the main stream. This fact seems to me to be strongly opposed to the cosmical theory of meteorites, except on the rather improbable supposition that the earth, on that occasion, encountered two or more groups, all, at the same time, crossing each other's orbit as well as the orbit of the earth. It may, however, be urged that such a supposition is hardly more unlikely than that which ascribes the November meteors to a ring of small bodies moving round the sun in an orbit differing little in magnitude from the earth's orbit, but the motion being retrograde, or contrary to that of the earth, and therefore inconsistent with the general analogies of the solar system, and opposed to Laplace's almost universally received nebular hypothesis.

XXI. *Observations of the New Variable Star, T Coronæ.* By Joseph Baxendell, F.R.A,S.

Read November 27th, 1866.

While engaged on the night of the 15th of May last in observing some of the naked-eye variables, my attention was suddenly arrested by a strange and rather conspicuous star about a degree distant from ϵ Coronæ, in the south following quadrant. It was at once carefully compared with some of the neighbouring stars, in order to ascertain its magnitude exactly; and a rough determination of its position was made, which led me to conclude that it was identical with Argelander's No. 2765 of Zone +26°, 9·5 magnitude, of the 'Bonner Sternverzeichniss,'—a conclusion which was fully confirmed the following night by more exact observations. On the night of the 7th of May I observed all the naked-eye variables then visible, and also several telescopic ones, and among the latter R and S Coronæ; but this star, if at that time really visible, entirely escaped my notice; and as the nights between the 7th and 15th were cloudy at Manchester, I am unable to say at what date it attained its maximum brilliancy. My observations of its magnitude, colour, &c. from the date of discovery to the present time are as follows, and I may add that all determinations of magnitude after the star became invisible to the naked eye were, with only three exceptions, made at Mr. Worthington's observatory with his equatorially mounted achromatic of five inches aperture, care being taken to use always the same eyepiece, a positive, having a magnifying-power of 68 times.

Observations of T Coronæ.

Date.	Mag.	Colour, &c.
1866.		
May 15	3·7	White, with a very slight yellow tinge, whiter than ε.
16	4·2	Cream-coloured, but the light very bright, and star well defined, without any hazy appearance.
17	4·9	With naked eye. Cream-coloured, exactly similar to ε.
	5·1	With 5-in. Ach. power 68.
18	5·3	Cream-coloured or buff. At times I have an impression of a blue tinge, as if the yellow of the star were seen through a film of a blue tint.
19	5·6	Deep cream, buff, Bath-brick- or wash-leather-colour, with a tinge of blue over it. ε the same colour, but perhaps slightly lighter and without the blue, or at least with excessively little of it. Repeatedly examined also by Mr. Dancer and Mr. Williamson, with different powers, and their estimations of colour precisely the same as my own.
20	6·2	Buff-coloured, with a tinge of blue, and deeper than ε Coronæ, which is yellow or light buff.
21	7·1	Leaden or slaty blue; the yellow colour has almost entirely disappeared.
22	7·4	The light of the star is dull, and is of a slaty blue or dark French white colour, or nearly like Smyth's No. 4 blue. No traces of yellow or red can be clearly made out.
23	7·5	Dull grey or French white. Sometimes there seems to be a trace of yellow.
24	7·7	Dull white, with a slight tinge of yellow or orange.
25	7·8	Dull and slightly orange-white. A shade of blue sometimes suspected.
26	8·0	Dull orange-white.
29	8·4	Dull orange-yellow.
June 8	9·3	Dull orange-yellow.
10	9·2	Dull orange-yellow.
16	9·4	
17	9·5	
19	9·5	
25	9·6	Orange-yellow.
26	9·7	Orange.
July 11	9·7	Dull yellow.
16	9·7	
19	9·6	
20	9·7	
21	9·6	
22	9·5	Dull pale orange.
30	9·7	Orange-yellow.
Aug. 20	9·8	
27	9·5	
31	9·3	Dull yellow.
Sept. 14	7·9	Dull yellow, almost exactly Smyth's yellow No. 3.
15	7·8	Yellow.
17	7·9	Pretty bright yellow.
22	7·9	
24	7·8	Yellow.
30	7·8	
Oct. 1	7·7	

TABLE (*continued*).

Date.	Mag.	Colour, &c.
1866.		
Oct. 6	7·6	Greyish yellow.
8	7·6	
10	7·5	Yellow.
14	7·5	Light yellow.
19	7·7	Light yellow.
28	7·8	Yellow.
Nov. 6	7·9	Smyth's orange No. 4.
19	8·3	Dull ruddy orange.

It will be seen that the brightness of the star diminished with great rapidity for several days after my first observation, and afterwards more gradually, and that on the 26th of June it had sunk to the 9·7 magnitude. It then remained with little change till about the 20th of August, when another rise commenced, and on the 15th of September it had attained the 7·8 magnitude. On the 10th and 14th of October it was of the 7·5 magnitude, and since the latter date its brightness has again slightly diminished.

It will also be noticed that in the recent observations no mention is made of the blue tinge which formed so striking a feature in the colour of the star for some time after its first appearance. In connexion with this the following extract of a letter from Mr. Huggins, F.R.S., dated October 13th, will be interesting to the Members of the Society. Referring to T Coronæ the writer says, "I observed its spectrum on Sept. 16th, 27th, 28th, and October 8th. The bright lines are not now to be distinguished. If they exist they cannot be much, if any, brighter than the parts of the spectrum where they occur. The observation of its spectrum is now very difficult."

In the scale of magnitudes I have employed, the light-ratio is 2·512; but the expression in magnitudes of the brightness of a variable star gives a very imperfect idea of the nature and extent of its changes; and as any speculations respecting the causes of variability must be based upon a consideration of the variations in the intensity of the light, and not in the magnitude of the star, I have calculated the relative intensities of the light of T Coronæ for every night of observation, taking the intensity of the light of a 10th magnitude star as unity. The results are given in the following Table:—

Intensities of the Light of T Coronæ.

Date.	Mag.	Intensity, 10 mag. being = 1·0.	Date.	Mag.	Intensity.	Date.	Mag.	Intensity.
1866.								
May 15	3·7	331·2	June 17	9·5	1·6	Sept. 17	7·9	6·9
16	4·2	209·0	19	9·5	1·6	22	7·9	6·9
17	4·9	160·0	25	9·6	1·4	24	7·8	7·6
18	5·3	75·8	26	9·7	1·3	30	7·8	7·6
19	5·6	57·5	July 11	9·7	1·3	Oct. 1	7·7	8·3
20	6·2	33·1	16	9·7	1·3	6	7·6	9·1
21	7·1	14·4	19	9·6	1·4	8	7·6	9·1
22	7·4	11·0	20	9·7	1·3	10	7·5	10·0
23	7·5	10·0	21	9·6	1·4	14	7·5	10·0
24	7·7	8·3	22	9·5	1·6	19	7·7	8·3
25	7·8	7·6	30	9·7	1·3	28	7·8	7·6
26	8·0	6·3	Aug. 20	9·8	1·2	Nov. 6	7·9	6·9
29	8·4	4·4	27	9·5	1·6	19	8·3	4·8
June 8	9·3	1·9	31	9·3	1·9			
10	9·2	2·1	Sept. 14	7·9	6·9			
16	9·4	1·7	15	7·8	7·6			

The outburst of T Coronæ appears to have been first observed by Mr. J. Birmingham of Tuam, on the night of May 12, when its brightness was equal to, if not greater than, that of *a* Coronæ, the magnitude of which is 2·6. The relative intensity of the light of the variable on that night was therefore not less than 912·1. Comparing this with the intensities in the above Table, it will be seen that during the first ten days of the star's appearance its light diminished with extraordinary rapidity, the intensity on the 22nd May being only 11·0 against 912·1 on the 12th when first seen by Mr. Birmingham, and 331·2 on the 15th when first seen by myself. Since the 22nd of May its changes have been comparatively slight. On the 20th of August the intensity was at a minimum, being only 1·2, or $\frac{1}{760}$ of its amount on the 12th of May.

An inspection of the curve of intensities suggests strongly the idea that a force of an explosive character, such as could result only from the action of highly elastic gaseous matter, had been in operation to produce the sudden increase and subsequent rapid diminution of brightness which had taken place; but as some of the well-known periodical variables occasionally exhibit an equal, and even a greater rapidity of change, this view cannot at present be received with much confidence; and notwithstanding the remarkable and highly interesting conclusions which Mr. Huggins and Dr. W. A. Miller have drawn from the results of their spectroscopic observations of this new variable, we are constrained to admit that the cause of variability is still involved in the deepest mystery.

The following Table contains the positions and magnitudes of the stars with which I have compared T Coronæ in the course of its changes. The positions are from the 'Bonner Sternverzeichniss,' and the magnitudes are from my own observations. The magnitudes of some of these stars have been determined independently by Mr. Knott, F.R.A.S., and it is satisfactory to me to find that our results are almost identical.

No.	Star.	R. A. 1855·0.			Dec. N. 1855·0.		Magnitude.
		h	m	s	°	′	
1	β Serpentis ...	15	39	29·3	15	53·1	3·7
2	γ Herculis......	16	15	31·1	19	30·3	3·8
3	γ Coronæ	15	36	40·0	26	45·6	4·2
4	ε Coronæ		51	35·7	27	18·1	4·3
5	δ Coronæ		43	31·0	26	31·5	4·8
6	π Serpentis ...		56	3·5	23	12·3	4·8
7	ι Coronæ		55	38·2	30	15·1	5·1
8	25 Herculis ...	16	20	14·8	37	42·7	6·0
9	Arg.2575+27°	15	56	38·2	27	1·1	7·5
10	3009 25		53	23·8	25	51·1	7·6
11	2767 26		55	2·8	26	34·6	7·7
12	2762 26		52	32·8		57·2	7·9
13	2754 26		49	16·8		26·3	8·0
14	3003 25		52	39·3	25	59·5	8·1
15	2563 27		52	30·0	27	16·8	8·1
16	2769 26		55	28·9	26	48·0	8·4
17	2763 26		52	54·1		33·6	9·0
18	2758 26		51	49·5		9·9	9·2
19	2760 26		52	7·3		40·0	9·4
20	2761 26		52	25·4		21·2	9·6
21	2764+26		53	22·3		33·9	10·8

I may state that two of these stars, Nos. 10 and 18, have shown decided indications of slight variability, the range of variation, so far as I have yet observed them, being about four-tenths of a magnitude.

XXII. Notes on Varieties of *Sarothamnus scoparius*, Koch, and *Stachys Betonica*, Benth., from the Lizard, Cornwall. By Charles Bailey, Esq.

Read December 11th, 1866.

The Lizard district has long been known to be singularly prolific in critical and rare British plants; and the purpose of this communication is to draw the attention of botanists to what appear to be two undescribed but well-marked forms of the plants whose names are placed at the head of this notice, and which are found in that district.

I. *Sarothamnus scoparius*, Koch, var.

It is only in recent years that this plant has been admitted a Cornish species, Mr. H. C. Watson, in vol. i. of his 'Cybele Britannica,' p. 274, giving Devon, Isle of Wight, and Kent as its most southern limit; but in the additions included in vol. iii. of the same work, Mr. Watson states (p. 404) that "the south limit extends to Cornwall, according to Mr. Gibson and Mr. Pascoe"—no details, however, being given as to the precise part of the county in which it occurs. The specimen exhibited was found growing in small patches on the cliffs of serpentine rock about Vellan Head, situate about four miles north-west of the Lizard Lights, and it differs from the normal form, here named var. *a*, in the following characters:—

Var. *a. erecta*.—Stems erect, bushy; leaves stalked, the petioles as long as, or longer than, the leaflets; leaflets elliptical-obovate, bluntish.

Var. *β. prostrata*.—Stems prostrate, spreading; leaves shortly stalked or sessile; leaflets ovate-acute, acuminate.

The Cornish form, here named *β. prostrata*, differs from the normal plant chiefly in its habit of growth, which, instead of being erect and bushy, is remarkably prostrate, the branches spreading out in fan-shaped patches and growing flat upon the ground; the branches, particularly in the upper half, are densely clothed with short spreading hairs; the leaves have shorter stalks, with a greater tendency to suppress the two lateral leaflets, the majority of the leaves, in fact, being unifoliate; the pods are less numerous, have their dorsal and ventral sutures covered with long silky hairs, and are black rather than brown, shorter, and have fewer seeds.

The season was too far advanced for any flowers to be met with, either on Vellan Head or in the small valley running down from Jollytown, the only other locality in Cornwall where the plant was observed.

II. *Stachys Betonica*, Bentham, var.

Of this plant three well-marked forms have been described: *a, Betonica hirta*, Reich.; *b, B. serstina*, Host.; and *c, B. stricta*, Ait.; and in many respects the form about to be described agrees with the first of these forms. In Mr. Babington's Manual (ed. v. p. 261) it is stated that "the English plant has the round crenate, not emarginate, lower lip of *B. hirta* (R.);" but Professor Boreau is of opinion that, while the three forms just named preserve their remarkable differences of aspect when cultivated together, the distinctive characters furnished by the divisions of the corolla are but slightly constant. (Flore du Centre de la France, &c., ed. iii. vol. ii. p. 530.)

Stems decumbent, numerous, radiating from the rootstock, square above, rounded below, clothed with many short hairs, which are closely appressed in the upper part and pointing downwards, those in the lower part more spreading, but still much reflexed; spikes slightly inclined, just raised above the ground, compressed-globose, the verticils many-flowered, never distant; calyx covered with straight hairs, the sepals ending in stiff points; corolla three times longer than the calyx, the exterior covered with scattered shaggy hairs, which are long and silky at the base of the tube, but becoming shorter and more scattered as they approach the lip; opening of the mouth very wide, lower lip crenate, wavy; lower leaves on long stalks, cordate at the base, oblong, regularly crenate, glandular on the under surface, with short scattered hairs; upper leaves lanceolate, on short stalks.

Specimens of *B. hirta*, Reich., have not come under my notice, nor have I been able to meet with Reichenbach's diagnosis; but the form described above seems to agree very nearly with Professor Boreau's description of that plant, which is here appended for the sake of comparison: "Stem clothed with many short stiff hairs; leaves with soft long hairs, very distinctly crenate; spike short, interrupted; calyx softly hairy at the summit; lower lip of the corolla rounded crenate" (Flore &c., *loc. cit.*). Mr. Bentham, in his 'Labiatarum genera et species,' p. 532, gives, amongst the synonyms of his *Stachys Betonica*, "*Betonica hirta*, Leyss., Reichb. Icon., Bot. Eur. 8. 4. t. 711," which may be identical with *B. hirta*, Reich.; but the only reference to it which I have met with is in Dr. Garke's 'Flora von Nord-und-Mittel Deutschland,' where it is shortly described as "Var. *a, hirta*, Leyss.—Stem with short hairs, calyx rough-haired."—(Ed. vi. p. 318.)

The Cornish form is very plentiful on the cliffs of "Killas" rock, lying between Caerthilian and the Lizard Lights, growing with *Genista tinctoria*, L., var. *humifusa*, Dicks., which it much resembles in habit. The same form is also met with in several other parts of South-Western Cornwall, as at Cuddan Point and the Mount's Bay district generally.

The above communication was preceded by a few remarks on the following plants of South-Western Cornwall, specimens of which were exhibited at the Meeting:—

Raphanus maritimus, *Sm.*	Cliffs under the Lizard Lights.
Brassica alba, *L.*	" " "
Arenaria verna, *L.*, var. *β*. Gerardi, *Willd.*	Rocks at Rill Head.
Spergularia rupestris, *Lebél non Camb.*	Nanjissal Bay, Land's End; plentiful.
Tamarix Anglica, *Webb*	Mount's Bay.
Lavatera arborea, *L.*	Cliffs, Newlyn.
Trifolium subterraneum, *L.*	Penzance.
" scabrum, *L.*	"
Anthyllis vulneraria, *L.* (a very robust form)	Porthgwarra, Land's End.

Anthyllis vulneraria, *L.*, var. β. Dillenii, *Schult.*	Forming the herbage on the sandy downs above Whitsand Bay, and common elsewhere.
Genista pilosa, *L.*	Gue Graze.
„ tinctoria, *L.*, var. β. humifusa, *Dicks.*	Plentiful between Caerthilian and the Lizard Lights.
Illecebrum verticillatum, *L.*	Madron Parish.
Herniaria glabra, *L.*	Common at the Lizard.
Valerianella olitoria, *Mönch*	Fields, Sennen Cove.
„ dentata, *Koch*	„ „ „
Wahlenbergia hederacea	St. Paul, and generally distributed.
Erica vagans, *L.*	Gomhilly, Pradannack, and Lizard Downs.
„ ciliaris, *L.*	Edgecombe Downs, Carclew.
Erythræa pulchella, *Fries*	Mount's Bay.
„ centaurium, *Pers.*	A stunted broad-leaved form, from Porth Curnnow (non E. latifolia, *Sm.*).
„ littoralis, *Fries*	Mount's Bay.
Sibthorpia Europæa, *L.*	St. Madron's Well.
Linaria Elatine, *Mill*	Marazion.
Allium sibiricum, *L.*	Rill Head.
Asparagus officinalis, *L.* (?)	„
Asplenium lanceolatum, *Huds.*	Whitsand Bay.

XXIII. *Notes on Wood-eating Coleoptera.* By Joseph Sidebotham, Esq.

Read December 11th, 1866.

The number of species of Coleoptera that feed upon wood in this country is considerable, some attacking growing trees, others when cut down or partially decayed, others attack solid timber when cut up and used for buildings or furniture. The various species are not confined to one or two of the great divisions, but are to be found scattered through most of them, being found in the sections *Necro-*

phaga, Lamellicornes, Sternoxi, Malacodermata, Rhyncophora, and *Longicornes* &c.

As might be expected, their modes of attack on trees are as varied as their organization; and their study is one of great interest to naturalists, besides being of the greatest importance to the owners of plantations and forests. Some species mine in the bark, others between the bark and the solid wood, others in roots and branches, and there are few, if any, of our native trees that are not liable to attacks from one or more species.

The amount of damage done to a forest when a few species get fairly established in it is very great; and too often the woodpeckers, which would assist in checking their ravages, are destroyed, because they bore holes in the trees to get at the insects.

At the present time the fine spruces in Dunham Park are being rapidly destroyed by one of the large Weevils, *Hylobius abietis,* and many ash trees by a small species mining in the bark, *Hylesinus fraxini.* In the valleys of the Spean in Perthshire the alder trees are being destroyed by the larvæ of *Stenocarus bifasciatum.* They begin the work, which is joined in by some smaller species, to such effect that you may see there trees, 30 inches in diameter, through which you can thrust a walking-stick.

A short time ago I accompanied a friend to his fishing-cottage in the north of Lancashire. It had been closed up for a little time, and the chairs and tables had been attacked by *Anobium striatum,* and although in appearance quite perfect, when touched almost crumbled to dust.

I bring for exhibition a piece of bark mined by two species of beetle, of great interest to entomologists, *Hylesinus vittatus* and *Nemosoma elongata.* The latter species was taken in 1833 by Mr. Ingall, at Sydenham, and since then it has not been met with, except about three specimens, until the spring of this year, when my friend Dr.

Power found it in Warwickshire and investigated its habits, publishing an account in the 'Entomologist.' From his observations he ascertained that this species feeds on the larvæ of *Hylesinus vittatus,* carrying its galleries across so as to intercept and devour them. Having myself once found the *Hylesinus* in some old railings at Beeston, near Nottingham, when I was at the British Association Meeting in August I again visited the place, and after careful search found some specimens of the rare *Nemosoma* in its mines across the tracks of *Hylesinus.* The portion of bark exhibited will show the mines, and the carded specimens will show the two species.

Stevens mentions Colwich near Nottingham, on the authority of Dr. Howitt, as a locality for *Nemosoma*; so no doubt by careful search it may be found in many other places around Nottingham. I also exhibit some bits of oak branches from Dunham mined by *Scolytus intricatus,* and a few other species of wood-boring Coleoptera found in this neighbourhood, with portions of wood attacked by them.

Scolytus destructor, the species which has been so destructive to elm trees near London and Paris, is not common here, probably in some measure because *Ulmus campestris* is not one of our common trees.

To the entomologist the investigation of the specific differences, the habits and instincts, and the peculiar conformation of these creatures to adapt them to their mode of life are sources of great pleasure; but he is at the same time more impressed with the enormous amount of damage they inflict than an ordinary observer, and also with the want of knowledge of those who are interested in the preservation of our woods and forests. It may happen that the removal of a tree, or even a branch, when attacked by a particular species, may save a forest; but it must be done at the proper time, so as to destroy the insects with it before they can escape to propagate their species; and it certainly ought to be part of a qualification for a forester or park-keeper that he should know the life-history of these wood-boring beetles, and the plans that have been adopted for their destruction.

XXIV. *On a New Form of the Dynamic Method for Measuring the Magnetic Dip.* By Sir William Thomson, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

Read April 16th, 1867.

Seven years ago an apparatus was constructed for the natural philosophy class of the University of Glasgow for illustrating the induction of electric currents by the motion of a conductor across the lines of terrestrial magnetic force. This instrument consisted of a large circular coil of many turns of fine copper wire, made to rotate by wheelwork about an axis, which can be set to positions inclined at all angles to the vertical. A fixed circle, parallel to the plane containing these positions, measured the angles between them. The ends of the coil were connected with fixed electrodes, so adjusted as to reverse the connexions every time the plane of the coil passes through the position perpendicular to that plane. When in use, the instrument should be set as nearly as may be in the magnetic meridian. The fixed electrodes being joined to the two ends of a coil of a delicate galvanometer, a large deflection is observed when the axis of rotation forms any considerable angle with the line of magnetic dip. On first trying the instrument I perceived that its sensibility was such as to promise an extremely sensitive means for measuring the dip. Accordingly, soon after I had a small and more portable instrument constructed for this special purpose; but up to this time I had not given it any sufficient trial. On the occasion of a recent visit, Dr. Joule assisted at some experiments with this instrument. The results have convinced us both that it will be quite practicable to improve it so that it may serve for a determination of the dip within a minute of angle. I hope, accordingly, before long to be able to communicate some decisive results to the Society, and to describe a convenient instrument which may be practically useful for the observation of this element.

XXV. *Observations on the Alteration of the Freezing-point in Thermometers.* By Dr. J. P. Joule, F.R.S., V.P.

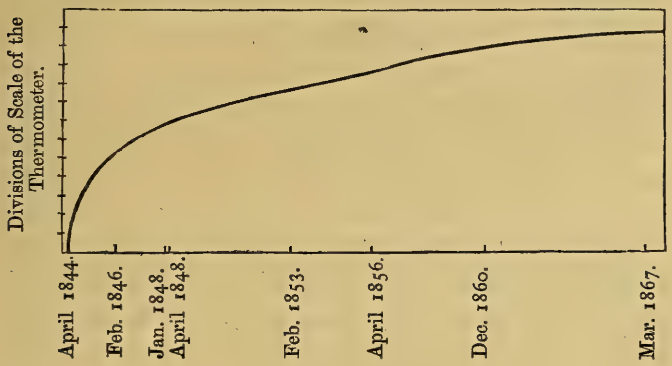
Read April 16th, 1867.

Having had in my possession, and in frequent use, for nearly a quarter of a century, two thermometers, of which I have from time to time taken the freezing-points, I think the results may offer some interest to the Society. Both thermometers are graduated on the stem, and are, I believe, the first in this country which were accurately calibrated. Thirteen divisions of one of them correspond to one degree Fahrenheit. It was made by Mr. Dancer, in the winter of 1843–44. My first observation of its freezing-point was made in April 1844. Calling this zero, my successive observations have given

0	April 1844.	8·8	February 1853.
5·5	February 1846.	9·5	April 1856.
6·6	January 1848.	11·1	December 1860.
6·9	April 1848.	11·8	March 1867.

The total rise has been, therefore, ·91 of a degree Fahrenheit. The other thermometer is not so sensitive, having less than four divisions to the degree. The total rise of its freezing-point has been only ·6 of a degree; but this is probably owing to the time which elapsed between its construction and the first observation being rather greater than in the case of the other thermometer. The rise of the two thermometers has been almost identical during the last nineteen years.

A projection of the observations given above is shown in the following diagram:—



XXVI. *On the Microscopical Examination of Coal-Ash or Dust from the Flue of a Furnace, illustrated by the Microscope.* By J. B. Dancer, F.R.A.S.

Read April 2nd, 1867.

When coal is burnt in a furnace to which atmospheric air has free access, a portion is converted into gaseous and volatile matter, and the incombustible substance which remains is the ash. The amount of ash in coals from different localities is very variable; it is said to range from

1 to 35 per cent. The ash or dust which is the subject of this paper was collected from the flue of my steam-boiler furnace, in which common engine coal is used as fuel. This coal leaves a considerable amount of incombustible matter. A specimen of the dust is now before you; it is of a reddish-brown colour, and free from soot or carbonaceous particles*. When this dust is examined under the microscope with a power of 40 or 50 diameters, it is found to consist of ferruginous matter and crystallized substances, some particles transparent, others white and red. It contains also a number of curious-looking objects, which vary considerably in size and colour; the majority of these bodies are spherical, and when separated from the irregularly shaped particles forming the bulk of the dust, they become interesting objects for the microscope. I shall confine my remarks more especially to these globular bodies. Some of these are as perfect in form as the most carefully turned billiard-balls, and have a brilliant polish. The various colours which these globules exhibit give additional interest to their examination. Some are transparent crystal spheres, others are opaque white, many are yellow and brown, and variegated like polished agates or carnelian of different shades. The most abundant of the highly polished balls are black: there are others which look like rusty cannon-balls; some of these have an aperture in them like a bomb-shell, and many are perforated in all directions. To obtain these objects the dust should be washed in a bowl and all the lightest particles allowed to float away; the remainder consists of fragmentary crystalline and ferruginous substances; mixed with these are the polished balls described, which, under the microscope, by a brilliant reflected light, look like little gems. To separate the spherical bodies from the irregular ones it

* My attention was drawn to this subject by Mr. Johnson, of Wigan, in November 1866.

is only necessary to sprinkle some of this material on an inclined glass plate, and by gentle vibration the balls roll down and can thus be collected. Having satisfied ourselves with the examination under the microscope, it is natural that we should desire to know more about these novel objects. What is their elementary constitution? Why are they spherical? How do they get into the flue? I have not attempted a chemical analysis of these minute bodies, many of which are less than the 100th part of an inch in diameter. I can only therefore offer an opinion as to their probable constitution, judging from what is known of the chemical analysis of coal-ash, and from the appearance they present under the microscope. Referring to the chemical analysis of coal-ash, we find that it sometimes contains silica, magnesia, alumina, sesquioxide of iron, lime, soda, potash, sulphate of calcium, anhydrous sulphuric acid, anhydrous phosphoric acid, sulphur, and sometimes traces of copper and lead. The vegetable origin of coal is now generally admitted; and doubtless some of the substances I have just named have been taken up by the coal-plants, whilst other portions may have collected in the locality where the coal was formed. As this is not immediately connected with our present inquiry, I proceed to speculate as to the constitution of these globular bodies. The transparent spheres I imagine to be silicates of soda or potash; the opaque white are most likely silicate of soda or potash combined with lime and alumina; the yellow and brown are silicates coloured by iron in different proportions. The black globes are not all alike in composition; some of these are silicates coloured by carbon, others are iron balls coated externally with a silicate. Many of these rusty cannon-balls are probably ferrous oxide formed by the action of heat on the iron-pyrites in the coal. There are also balls of black magnetic oxide: the perforated shells are probably ferrous sulphides. The globular form of these bodies suggests that they have been thrown off in scintillations, such as are seen during the combustion of iron in oxygen gas, and whilst in a fluid state they assume a spheroidal form. They are carried by the draught into the flue, and being of greater specific gravity than the carbonaceous matter forming the smoke, they fall before the current of air has reached the chimney. Some of the dust has been a considerable time in the flue, exposed to the intensely heated circulating flame; the reducing action of this would probably convert some of the oxide into metallic iron. Many of these balls have the appearance of reduced oxides. The flue-dust contains a larger amount of ferruginous matter than can be accounted for by the analysis of coal-ash. I think the surplus may be regarded as representing the wear and tear of the ironwork about the furnace, such as fire-bars, boiler-plates, &c. The brickwork and cement about the boiler and flues may also supply some of the silica, alumina, and iron for these balls, numbers of which are merely thin shells. The movements of these objects, caused by the approach of a magnet under the stage of the microscope, are somewhat amusing; and it is at times startling to see the crystalline objects, both spherical and irregular, exhibit magnetic attraction: probably they contain particles of iron imbedded in them; if they do not, may we not imagine that there is some magnetic compound in which the crystalline matter predominates? When we consider the accidental condition under which this matter has combined, it is just possible that some new molecular arrangement or combination of elements may have taken place. It is very probable that many of these polished balls are much more complex in their elementary constitution than I have stated. They are, in fact, a kind of glass, and many of them merely bulbs. Pelouze states that glass is probably an indefinite mixture of definite silicates. Glass, containing small quantities of ferrous oxides and sodic sulphates, when exposed to sunlight becomes yellow; and possibly some of these balls may have changed in colour since they came from the flue. Hydrochloric and nitric acids exert very little action on the ferruginous globes: this may be due, in some measure, to the high temperature at which the oxides have been formed; in other cases they are no doubt protected by an external coating of some silicate. It would require much time and patience to collect a sufficient number of each kind of these minute objects for a chemical analysis; but the spectroscope might probably assist in revealing their constitution. When time permits, I hope to resume the subject.

XXVII. *Notes on Cotton-Spinning Machinery. Roving-Frames.* By J. C. Dyer, V.P.

Read March 20th, 1866.

In the preparation of cotton for spinning, several distinct classes of machines are employed, such as the clearing, scutching, and blowing machines, and the carding, doubling, and drawing engines, by which cotton is brought into the state of continuous slivers, or "rovings," before it comes to the "roving-frames." These operations for clearing, carding, and arranging the fibres into loose ropes or rovings, are of a simple nature, and are sufficiently indicated by the names of the several machines used for that purpose; and as no special description of them seems called for, I proceed to consider the more intricate and scientific properties of the machines known as roving-frames, which come next to the mule-jenny in the order of scientific interest, on the ground of their having also engaged the labours of many ingenious and able mechanicians through a long course of years to bring them from a rude state into their present very accurate form of working. Hence it seems desirable to place on record an account of the successive changes and inventions that have been applied to this class of machines, as being a useful contribution to the "History of Inventions" connected with cotton-spinning during this century.

A retrospect of this period will show our advanced and advancing knowledge of the "mechanical sciences reduced to practice," to be the main source of our wealth and power as a nation; wherefore it will be held useful to have as full and fair notices as can be obtained of the progress in the several inventions that have led to the final success of the most intricate and scientific kinds of machinery now in general use.

To make these roving-frames work safely, and with the requisite accuracy in their several complex movements, was no easy task; and many even of its constructors were unable to comprehend the *principle of action* to be brought under such exact control to make it work safely. To render this more evident, it will be necessary to trace the successive operations required to reduce the slivers from the drawing-frames to the proper state for spinning.

The rovings, after being equalized by repeated doubling and drawing, are brought (in tin cans) to the roving-frame, by which they are again reduced by the drawing-rollers, and then pass through the neck of the throttle-spindle and down one arm of the flyers, through a hook, from which they pass to, and are wound upon, the barrels of the bobbins. A slight twist is given to the rovings by the flyers, to prevent their being stretched in their passing to the bobbins. Supposing, in commencing, the diameter of the front rollers and that of the bobbin-barrels to be exactly the same, it is obvious that the rotative speed of the delivering rollers and the taking-up barrels must also be the same, in order to convey the roving to the bobbins without causing any strain or looseness of the rovings. But this applies only to the first few laps of the rovings; for the barrels are enlarged by the successive coils of roving, and their rotative motions must be reduced accordingly. As the bobbins are being filled with rovings, their surface-motion must be made to correspond with that of the delivering rollers, so as to take up equal lengths in equal times. The spindle and flyer that twist the rovings, and the bobbins that take them up, are driven by separate first motions, whereby their speeds are adapted to the required differences, to be noted further on. The bobbin and fly frame was not strictly a new invention when first used as a roving-frame; it was a modification of the throttle spinning-frame of Arkwright. The bobbins used for spinning are about 3 to 3½ inches long, and some 2 inches in diameter; those for the rovings are from 6 to 9 or 10 inches long, and 3 to 4 inches in diameter. It was thus necessary to adapt the roving-frame to these larger-sized bobbins. But the essential change from the throttle frame consisted in the addition of the new apparatus for driving the bobbins apart from the spindles, by trains of wheels and pulleys, so as to ensure the differential motions for taking up the roving. This separate driving of the flyers and the bobbins was the problem to be solved by the new construction. To explain this, it should be stated that, in spinning, the spindles and flyers are driven to twist and deliver the threads to the bobbins, and the bobbins are carried round with the flyers by the pull of the threads, against a slight friction, as they are taken up by the bobbins; and the bobbins thus make fewer turns than the flyers, by the number of coils taken up by them. The tightness of winding on is thus determined by the strength of the threads, and the friction as drag of the bobbins. But as the rovings are slightly twisted, they have no strength for guiding the taking-up motions of the bobbins; wherefore they must be driven apart from the flyers, with a variable speed, as aforesaid.

Had no more been required than to secure a regular diminishing speed of the bobbins directly as their diameters were enlarged by the rovings wound upon them, the relative motions of the flyers and bobbins could be easily obtained by the common traversing straps working over *reversed cone-pulleys*. But frequent readjustments of those motions were required in using the different sorts of cotton (as coarse and fine, long and short staple, and the like), in which more or less twist was necessary to fit the roving for spinning; and in the changes of twist the speed of the delivering rollers had also to be changed, and consequently that of the bobbins for taking up such varying lengths of rovings in equal times. We must keep in view that if the rovings have too little twist, they will stretch and become uneven, through lack of strength to turn the bobbins in drawing them off for spinning. Now these frequent readjustments of the differential motions above mentioned constitute the main difficulties in using the bobbins and fly frames. In carefully considering the nature of the operations performed by this machine, no surprise will be felt that so many years should have passed in trials to improve its working before the discovery of any effectual plan for securing the differential motion by a self-regulating apparatus.

From 1815 to 1825 I had witnessed the progress of the many improvements in the construction and the method of regulating the delicate movements of this roving-frame,—those of the greatest value in practice having been made or suggested by my late friend Mr. John Kennedy, of Ardwick Hall, whose well-earned fame as one of the most ingenious and talented mechanicians of his time is too well established to require any aid from my pen. I might also mention other able and distinguished mechanics who contributed to the improvements in building these frames; but it were no easy task to assign to each his true share in them. Besides, the "differential motions" were still so imperfect in the principle of action as to call for other and more essential changes to secure the correctness required for safe working.

This defective state of the bobbin and fly frame had led to the invention of a substitute for it, in which the bobbins were turned (in taking up the rovings) by surface friction instead of by central action. The operations of this new roving-frame are as follows, viz. :—

In place of using the throttle-spindle and flyers, the rovings are conducted to and placed upon the bobbins by means of revolving tubes (arranged in a line with the roller-beam), through which the rovings pass, and are compressed by being *twisted* and *untwisted* before they are placed upon the bobbins. A range of fluted cylinders, the length of the bobbins, are placed along a driving-shaft (the length of the roller-beam); and the bobbins, being mounted upon the cylinders (under suitable pressure), are carried round by the simple contact-friction of their surfaces. Thus the rovings are duly taken up and wound upon the bobbins as they pass from the roller-beam through the twisting-tubes to the surface of the bobbins. The cylinder-shaft is geared (by wheels or pulleys) to the axis of the front rollers, so as to make their speeds inversely as the diameters of the cylinders and delivering-rollers. Thus the equable surface-motion is secured for taking up the rovings, at whatever speed they may be given out from the roller-beam. A steel presser is placed just opposite to the nose-end of the tube; the rovings pass through a hole in the presser as they leave the tubes, and are firmly pressed upon the bobbins in being wound upon them.

In the above processes the rovings are made compact, so that the bobbins contain a larger weight of cotton, and are more portable than on the old plan; and though without any twist, the rovings have sufficient strength to turn the bobbins in drawing them off for spinning. It is obvious from the above that the differential motions in the bobbin and fly frame are superseded and wholly dispensed with, by turning the bobbins by the equable surface-motions employed in the patent tube frame, as above explained.

The new roving-frame was first brought out at the "Taunton Cotton Works" of Messrs. Crocker and Richmond, of Boston, who informed me that it had been invented by a Mr. Danforth, who was employed in their works. In the year 1825 Mr. Charles Richmond, of that firm, came over to England and brought a working model of the new roving-frame, and placed it in my charge for the purpose of having it patented for the joint account of his house in America and myself. Accordingly I obtained a patent for the invention in the form in which it had been thus communicated to me. It was, however, so imperfect in its construction, that many improvements were obviously necessary to make it work to advantage in this country, and which were subsequently made under my directions, and patented as such for the same joint interest. I then submitted the building of this frame to several of our *then* first-class machine-makers, with the exclusive right of introducing it for use among the spinners. Among the houses to which this offer was successively made were Messrs. Cocker and Higgins, Messrs. Hughes and Wren, and Mr. Henry Gore. But, after the experiments and investigations they had respectively made, each of them declined to take the machine in charge, having come to the opinion that the invention could not be made to work with that accuracy and economy which would justify its adoption by the spinners in England.

Having formed a favourable opinion of the principle of the invention, and believing it to be susceptible of great improvement in working by several changes in its construction, I could not allow the unfavourable conclusion come to by the above-mentioned parties to deter me from further efforts to bring the machine into practical operation in competing with the bobbin and fly frame.

With a view, therefore, to have the tube frame fairly and fully tested, I commenced the Machine-making Works in Manchester, where they were subsequently built upon the improved plans that rendered them of practical importance for spinning most of the lower numbers of twist. But it took a long time, and required much patient labour, to effect the changes and improvements necessary before the tube frame could be brought into a state sufficiently simple and safe-working to prove its real merits and induce the spinners to adopt it in place of the fly frame.

In the course of my experiments in building them, several frames were broken up before any certain success could be realized. But about the end of 1828 the demand for them began to extend; and soon after, a large trade was established in building the tube frames, then called the "Yankee Speeders" by some, and by others "Dyer's Frames" or speeders.

The extensive adoption of the tube frames for low numbers led to many attempts to render them applicable for fine spinning also; but this could not be done without putting twist in the rovings to strengthen them when reduced enough for high numbers; and it was quite impossible to have any twist in rovings as they passed through the twisting and untwisting tubes; wherefore the bobbin and fly frame was still to be looked to for this branch of the trade. In this instance (as we have seen in many other manufactures) we find how naturally one successful invention suggests and leads to others of equal importance, as applied to kindred objects. Thus the action of the tube machine, in pressing and condensing the roving hard upon the bobbins, was seen to be an object of great importance if it could be applied to the fly frame. With this view my friend Mr. Henry Houldsworth made a series of experiments, by attaching a presser to one arm of the flyer, to act against the bobbins in the fly frame as in the tube frame. By an arrangement with him, this new application of the presser was included in my second patent for "Improvements in Roving-frames." This improvement so far changed the working of the bobbin and fly frame, that the scale soon after began to turn in its favour with the spinners. To show the relation of these two roving-frames to the work to be done by them, we must keep in view the different degrees of tenuity of the roving required for fine and for coarse spinning. The rovings are in general drawn on the mule-beam some ten or twelvefold, say from one to ten or twelve yards in length. When they are for spinning the Nos. 30, 60, and 120 twist (taking the draft of one into ten), the rovings for those numbers must of course be reduced to the sizes of 3, 6, and 12 hanks to the pound weight.

The hank of 840 yards equals 2520, 5040, 10,080 yards to the pound weight. In practice it was found that rovings of the tenuity exceeding six hanks were too weak to draw off from the bobbins, unless they were slightly twisted, and the twist could only be given to the fly frame. In other respects the rovings from this frame were very defective, compared with those from the tube frame. The former being soft and loosely wound on, the bobbins were liable to injury in carrying from the roving- to the spinning-rooms. This made it necessary to use the old form of spools, or barrels with disk ends to support the rovings. But the bobbins on the tube frame were simple barrels without end disks, as the rovings are compressed and wound hard on them, and the traverse endwise of the bobbins, diminishing in length with their increase of diameter, gave conical ends to the rovings, making them safely portable. From these considerations it was highly important to transfer to the fly frame the like movements for winding the rovings hard upon the bobbins, by uniting the compression to that of twist, in rovings for the higher numbers in spinning.

In the tube frame the axes of the bobbins are horizontal, and the pressure of the rovings upon their surfaces is obtained by simple weights attached to the tube carriers. But in the fly frame the bobbins are arranged vertically; so that the pressure must be given by means of springs attached to the arms of the flyers, or by the centrifugal force of weights revolving with the flyers and acting upon the pressers. Now both of these plans of pressing the rovings upon the bobbins are very clearly described in the drawings and specifications of my said patent for these improvements on the fly frames, as before mentioned; notwithstanding which, we have since seen that some six or eight (so-called) inventions have been patented for various and very trivial changes in the forms and modes of applying the springs, and the centrifugal force of weights revolving with the arms of the flyers, to press the rovings on the bobbins. These patents have led to many legal contests, and afforded gainful work for the lawyers in seeking to award the honours and profits pertaining to such inventions.

I come now to speak of the really valuable and beautiful invention for controlling the differential motions of the fly frame, made and patented by Mr. Henry Houldsworth. Having already explained the complex nature of the continually varying rotations required to make the delivering, twisting, and winding-on motions agree (when changes in either are required), and also that those motions had before been regulated mostly by the "trial and error" method, rather than by any reliable self-adjusting movements, the discovery of a very simple train of movements for maintaining the differential rotations required in the bobbin and fly frame will at once be recognized as an invention of a high order in mechanical science. From the proper limits of this paper, these notices must be confined to the main features of the delicate movements and the general operations of the machinery in question; so that those wishing for the minute description of the different motions above-mentioned must consult the drawings and specifications of them given in the patents, as before stated*.

* The movements are as follows:—

a. Rollers, 300 turns per minute.
b. Rovings, 33 yards per minute.
c. Flyers, 3300 turns per minute.
d. Twist, 100 turns per foot $= 8\frac{1}{3}$ turns per inch.
e. Bobbins, 300 turns per minute.

Taking the front rollers and the bobbins to be of the same diameters at first, then by these proportions the rovings will be wound upon the bobbins without strain or slackness. But the speed of the rollers must be altered to suit the drawing to the different kinds of cotton, and then that of twist to the lengths delivered. Again, the speed of the bobbins must be changed as their size and shape alter by the rovings wound upon them.

Now the problem is to make all the motions relatively the same as above, when the changes take place to meet the conditions stated. The following are the visible operations of the roving-frame:—A strap descends from the driving-pulley, and works one fast and loose pulleys upon an axis fixed on one end of the frame. This axis is geared by changeable wheels and pinions to drive the rollers on the beams; and from the same axis, through proper wheels, pulleys, and bands, are driven the flyers and spindles for giving the twist, and by another train of gearing the bobbins for taking up the rovings. The length of the roving delivered depending on the speed of the front rollers, all changes of these, of course, require corresponding changes in the speeds of the flyers and bobbins. The continually varying speeds of the latter are obtained through the action of traversing straps, working over reversed cone-pulleys. These straps traverse endwise on the cones, and the distances of such traverse motions are regulated by the "notch-bars," viz. sliding plates so graduated that the division shall move the straps just enough to give the equable surface-motions to the bobbins through all their changes of size. These, then, comprise mostly what can be seen of these frames, or what can be set forth in words without the aid of drawings.

However, to make the nature of the differential motions more readily comprehended by persons wholly unacquainted with these compound motions, I may take a supposed case to illustrate them. Let a travelling carriage, drawn by horse or any other moving power, have a common windlass fixed in it, with a rope wound upon the barrel and extending thence some yards behind, let the outer end of the rope be fastened to another carriage to be drawn after the first by the rope; then it is evident that the two carriages will move with the same speed so long as the tow-rope continues without any change of length, at whatever speed the first carriage be driven. But let a man in the leading carriage have charge of the winch to lengthen or shorten the rope (by winding it on or unwinding it from the barrel of the windlass); then the rate of taking up or giving out of the rope will be the exact measure of the difference of speeds of the two carriages at each of the rates at which the leading one may be driven. Let the one move, say, ten miles the hour, the other must follow at the same rate whilst no change is made in the distance between them; but if the rope be wound up at the rate of one mile an hour, or unwound at the same rate, it follows that the speeds will respectively be as ten to eleven, and as ten to nine miles per hour,—and so on with every change made in the connecting link between the two moving bodies. In this case the will of the man at the winch regulates the differences of these principal moving bodies. But again, the wheels of the two carriages may be of different diameters, and then their rotative motions will differ accordingly; and these revolving speeds will have their differences made to accord with the varying motions of the carriages. Now such are the kind of differences to be provided for in the bobbin and fly frame. The invention of Mr. Houldsworth provides a correct substitute for such giving and taking of a rope to maintain the relative speeds of the prime motions in the fly frame. This is effected by the action of three wheels or pulleys* revolving together, and geared so that one of them was driven faster and the other slower by the middle wheel, as the rotations of the latter were governed by those of the delivering-rollers. This apparatus at the time was known by the significant title of "Houldsworth's Jack-in-the-Box;" and in fact it did solve the "differential problem" which had so long baffled so many clear heads to master. I must be content with thus pointing out the general properties of the different classes of the roving-frames above noticed, as it would be vain to attempt a description of their minuter parts without drawings to illustrate them.

The successful application of the presser and the Jack-in-the-Box again turned the scale in favour of the bobbin and fly frame as competing with the tube frame; and the former may be held as the most complete triumph of genius and scientific skill now exhibited in the cotton-mills, with the sole exception of the self-acting mule, as

* This important invention of Mr. Houldsworth was at first carried into effect by pulleys and straps; but, from their liability to slippage, it was found desirable to substitute toothed wheels in place of pulleys. The suggestive nature of new discoveries was then strikingly shown; for this want soon drew the attention of other eminent mechanicians to the subject, among whom both Mr. John Kennedy and Mr. Peter Ewart succeeded in giving the same motions by means of three wheels so acting together that the middle wheel controlled the relative speeds of the outer ones, just as Mr. Houldsworth had done by the pulleys. Mr. Ewart employed mitre wheels, and Mr. Kennedy adopted spur-gearing; and the latter, I believe, has been found the most simple and best in practice. It is also worthy of remark that, shortly after the application of the three mitre wheels to govern the varying motions in the roving-frame, it was found that the same action of three mitre wheels had for many years been in open use for giving a uniform surface speed in the side lathe for turning conical pulleys, just such as are used in the roving-frames: so here was a curious discovery by Mr. Ewart of a new use for an old action of wheels, which he had often seen in operation with the turning-lathe, but dreamed not of its being of value for other purposes, until led thereto by the new want, as above.

described in a former paper, of which this is a second part.

It is about sixty years since Mr. John Kennedy adopted the following method of putting twist into rovings, namely:—He placed a range of pulleys along the front of the drawing-frames, so as to revolve horizontally on fixed centre pins close to the floor. The top sides of the pulleys were dished, or turned hollow to receive the bottom ends of cans of rovings, making them revolve with the pulleys. These were driven by an endless cord passing round them and thence up to the common driving-shaft. Thus, as the rovings were delivered from the drawing-frames into the cans, they were twisted in proportion to the speed of the pulleys and that of the delivering-rollers. But the cumbrous nature of this apparatus was found to limit the rotation of the cans to so low a speed, compared with that of the drawing-rollers, that the requisite amount of twist could not be given to the rovings without great loss of time, by retarding the drawing-process; so that this defect (of twisting by the cans) led to the application of the throstle-spindle and flyers, as before mentioned.

The flyer-spindles, in the first place, were adapted for very large bobbins, in what was called the "slubber-frame," used in the first operation upon the rovings; and the next, or finishing-frame, was the bobbin and fly frame, the leading features and operations of which have been already explained, as also the successive changes effected for improving them. It will thus be seen that the vicissitudes of these roving-frames afford striking proofs of the gradations through which most of our great mechanical inventions have had to pass ere they could be made to realize the aspirations of their original inventors. Looking to the high degree of perfection now displayed, and the mighty powers daily exerted by the several classes of machines employed in the cotton-mills, it seems of real importance that we should have some faithful records of the successive steps taken among our most talented machinists to bring into their present state those gigantic aids to manufacturing industry which at once contribute so largely to our national wealth and power and afford so fair a ground of patriotic pride. This task can only be well performed by some of those who have witnessed or contributed to the gradual approaches to such triumphant issues of the labour, skill, and talent so long and patiently employed in working out these results*. A concise account should be given of the manipulations of the cotton before it comes to the roving-frame:—(1) By the "devilling-machine," for opening the cotton from its compressed state in the bags. (2) Blowing-engine, for clearing and separating it from dirt and extraneous bodies. (3) The carding-engines for arranging the fibres in even sheets,

* If we wish to know how any work is performed, we must consult those who have done the same kind of work. If we desire to understand the properties of any machine, we should have them explained by persons conversant with the construction of such machines.

In like manner, if we can hope to see plain and reliable accounts given of the progressive advances made in labour-saving machinery of late years, they must come (as said in the text) from "some of those who have witnessed and contributed to the gradual approaches to the state of perfection" now attained. I would therefore invoke the pens of the few eminent men still left who belong to the class thus named, to record (as I have humbly sought to do) their own knowledge and experiences respecting the many other able contributors to the rapid advances in the science of practical mechanics in our times.

I may here point to the example set by my highly gifted friend, William Fairbairn, LL.D. &c., whose long course of engineering practice and scientific labours have been of the highest order and importance in widening the fields and clearing the paths of the mechanical engineer; for Mr. Fairbairn has given to the public, through his valuable lectures and published works, the results of his experiments, and the principles disclosed by them, as well as of his engineering labours, all of which are so plainly set forth as to enable the working man to comprehend them.

I greatly wish that another much valued and able friend, Mr. Henry Houldsworth, would perform the same task respecting his own important inventions and labours for advancing the mechanical sciences, second only to those of Fairbairn.

and doffing these in the form of loose ropes or rovings. (4) The drawing-frames for doubling and elongating the rovings to equalize them for the roving-frames. Each of these should be traced from its former rude to its present efficient and beautiful state as practically realized. The subject also of a good paper might be to trace the several forms and methods of using the throstle and spindle and flyers which have been invented since the adoption of this form of spindle by Arkwright, but was taken by him from the very ancient "wheel-and-distaff machine," in which the single spindle and flyers with bobbins were used for spinning flax by hand,—also that the pointed or mule spindle was adopted from the equally ancient "wheel-and-band machine," with single spindle, used for spinning wool by hand.

Since Arkwright's patent throstle came into use, there have been some dozen or more patents taken out for substitutes for the throstle and flyers. Some of them at first appeared likely to supersede the original form of the throstle spindle; but we find in this a curious exception to the general results of improvements by successive artists, who devise new modes of action in mechanics; for the throstle-frame, as now constructed by our leading machinists, is substantially the same in principle and action as that of Arkwright. The greater speed and efficiency of modern frames is owing to the advanced skill and knowledge of the builders of them, compared with the former. By this judicious construction they are much stronger, more durable, and less subject to derangements than formerly; but nothing really new has been added by way of invention.

Besides the intricate and scientific character of the machinery described in these notes, there are many others in the cotton-mills which should be explained in the like simple way in which I have aimed to do in this case. After the process of spinning, in order to prepare the finer sorts of yarn for the lace-frame, and for the many delicate fabrics wrought by the loom, they are mostly doubled or quadrupled, and the finishing touch given by the "fiery ordeal" of passing each thread through the flame of gas, to singe, or clear them from all protruding fibre-ends, which leaves the filaments of cotton as smooth and bright as are those of glossy silk. When rightly comprehended, the cotton-mill, with its vast aggregations of statical and moving forces, cooperating in such perfect harmony, and thereby converting the matted and tangled masses of cotton into such even and delicately attenuated threads, must constitute a subject of admiration to the philosopher, the statesman, and the physicist. Wherefore, to have all of the separate operations employed for realizing these results so set forth as to render them of easy comprehension must be held worthy of the labours of those who may undertake to follow and complete the work still needful to the fulfilment of this task.

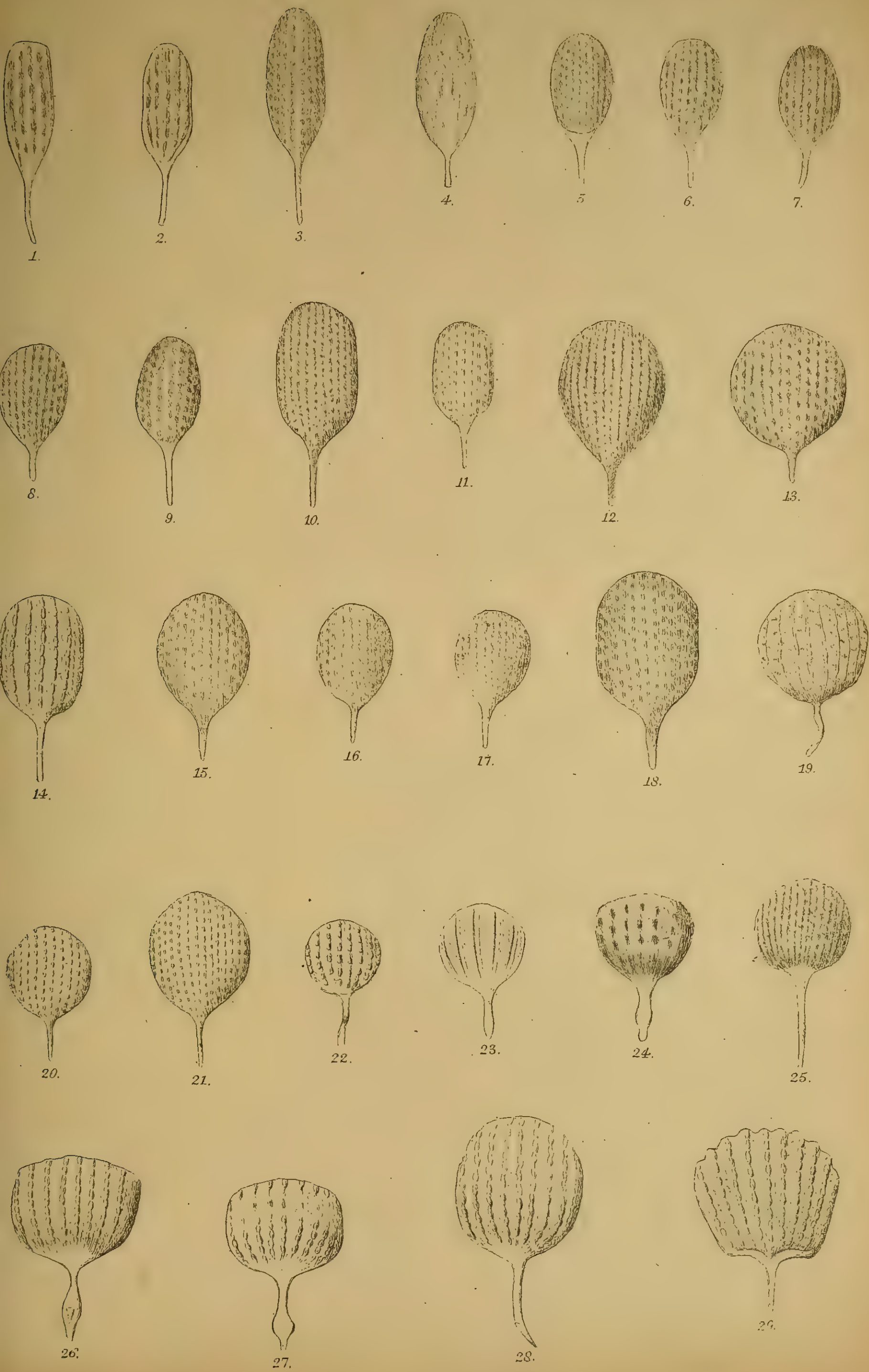
In conclusion, I may observe that many elaborate treatises, some of them very able ones, have appeared on the steam-engine, from its first rude and very wasteful principles of action, down through the diverse forms and clearer principles since applied to the uses of steam-power, before arriving at the varied constructions and correct modes of working now in general use. We have also had some valuable treatises on the different classes of mill-gearing and engines for transmitting moving forces, from their sources to their destined purposes. Again, we have seen many well-written memoirs on the lives and labours of several of the great engineers of our times, as well as of those of earlier date, whose well-earned fame has been thus duly awarded. On the other hand, with the exception of the meagre details and disputes concerning the first projects and after inventions of Paul, Whyatt, Arkwright, Kay, and Compton, hardly any public notices have been seen of the great body of leading machinists whose genius, scientific skill, and persevering labours have been exerted to convert the former rude workshops of these districts into the scientific and powerful machine- and tool-making works that now abound in our great hives of industry, which create and sustain our vast cotton-trade.

Upon these grounds I have endeavoured to do some slight though tardy justice to the names of some of the eminent contributors to the great advances achieved in the mechanical sciences, and especially in cotton-spinning machines by those with whom I have cooperated in some cases, and whose successful labours I have in many others witnessed, whereby the cotton-mill is placed among the most eminent of modern creations.

On former occasions I have brought before the Society some brief accounts of the inventions of several eminent mechanicians, to whom our age is indebted for substituting power-driven machines for hand-labour; and, as before said, it must be held important to obtain the like details of many other inventions, made within the last eventful period of 80 or 90 years, which would afford trustworthy materials for a methodical history of modern inventions, a work much called for, to follow Beckman's valuable 'History of Inventions' down to his time. If this labour be long delayed, it will greatly increase by the loss of time, and its value diminish for lack of authenticity, which depends so much on the evidence of living witnesses.

The papers above referred to are the following:—

1. On the Introduction of Steam Navigation.
2. The Mule-Jenny and Self-acting Mule.
3. Lace-making by Power-driven Machinery, and Wire-card-making by Power-driven Machinery.
4. Nail- and Tack-making by Power-driven Machinery.
5. The use of Steel Dies for Engraving.
6. The present paper, on Cotton-roving Frames.



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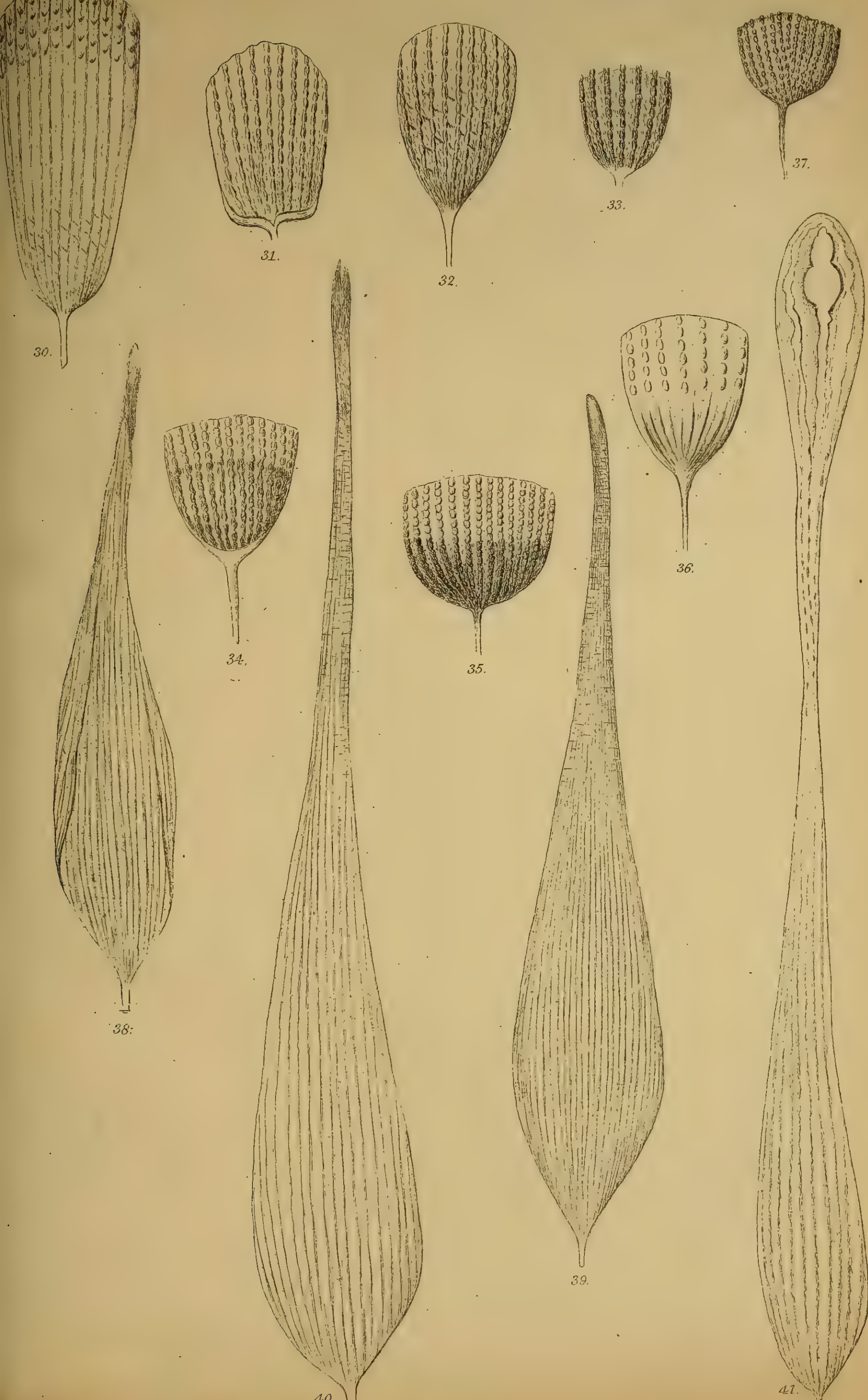

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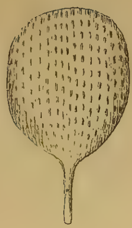

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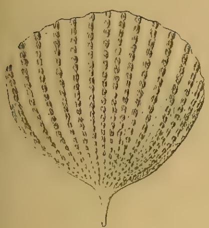

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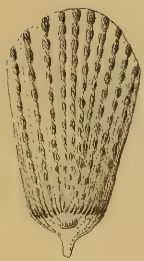

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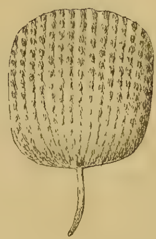

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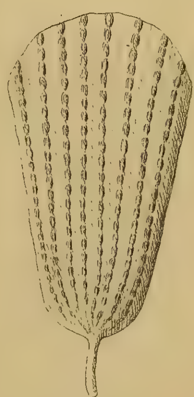

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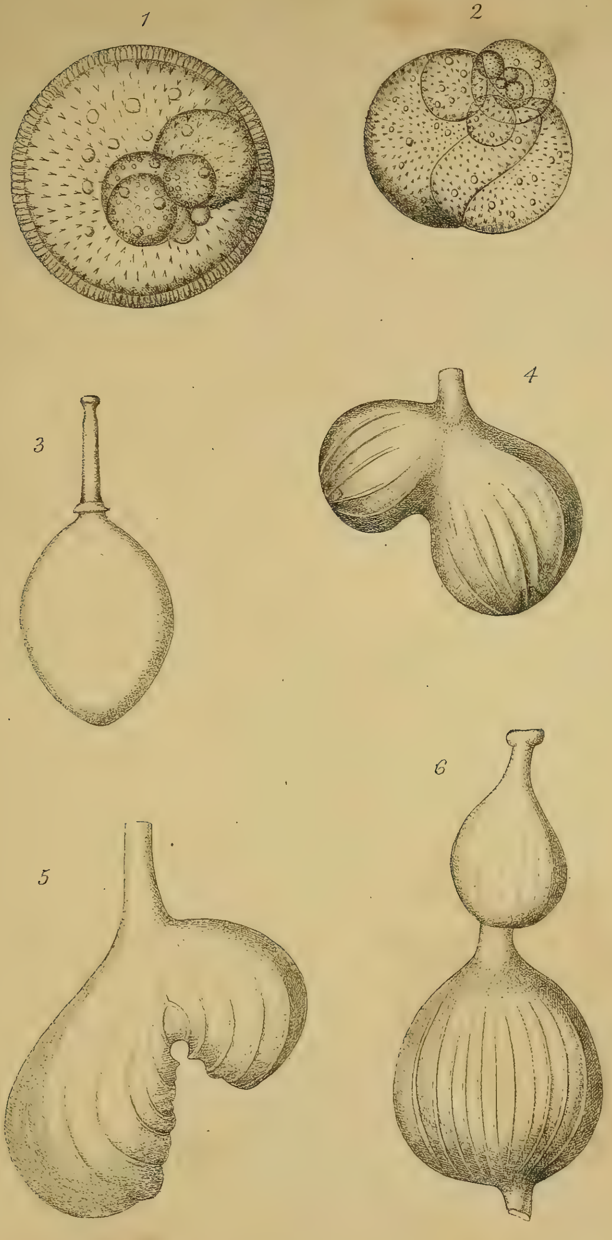

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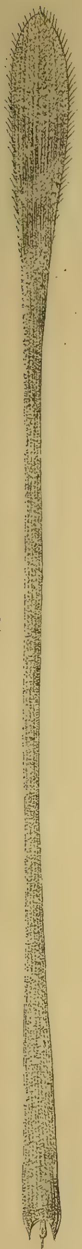

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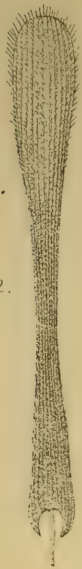

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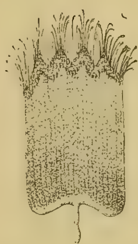

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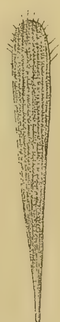

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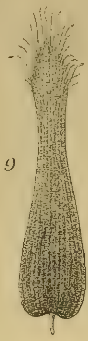

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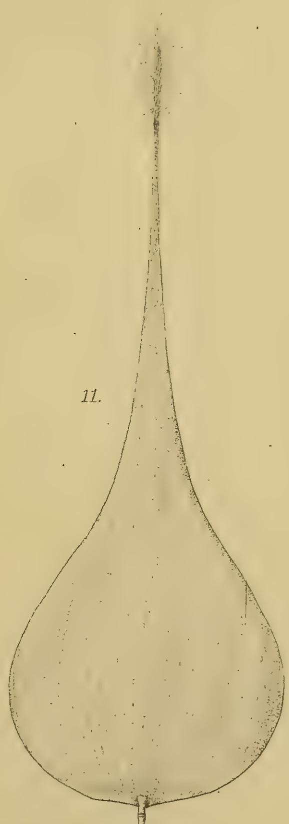

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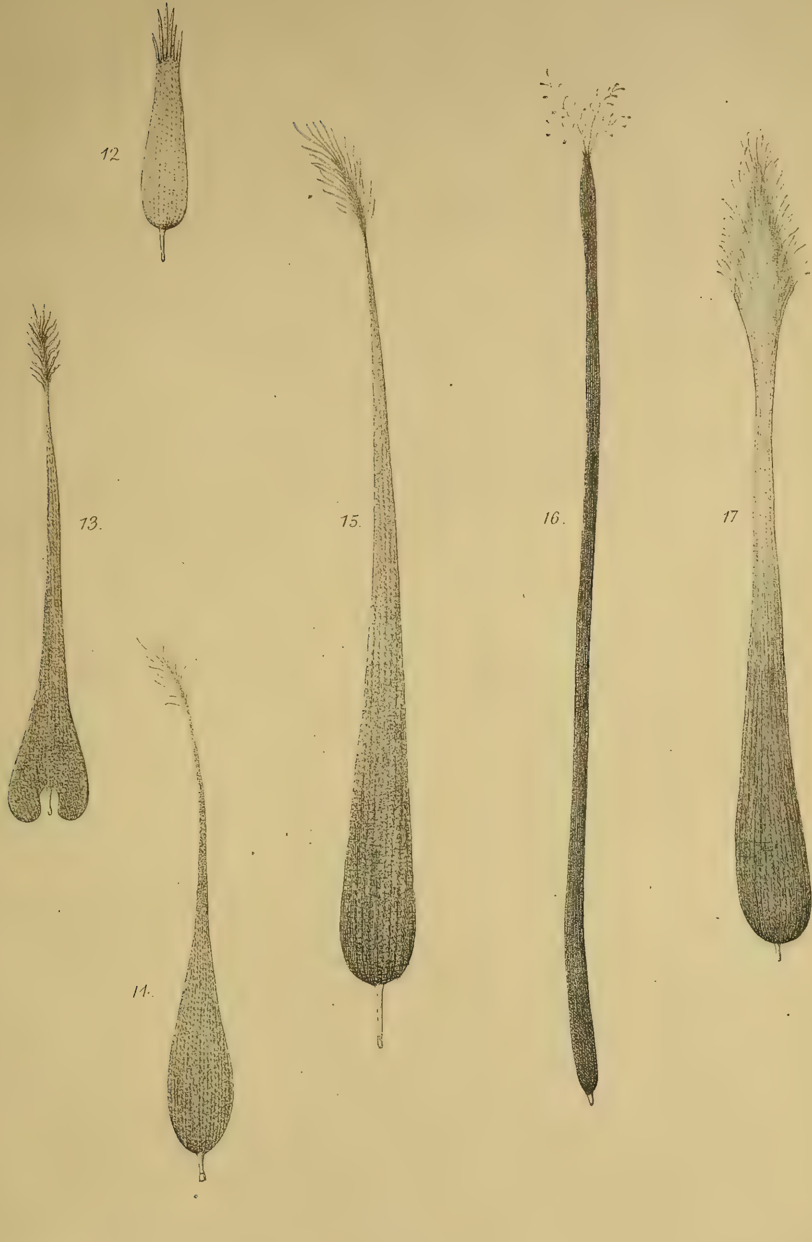

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DATE OF ELECTION.

Brussels, Rheims, Nantes, Antwerp, Linnean Socc. Caen, Angers, Marseilles, La Rochelle and Paris. 19 *Steinweg, Regensburg, Bavaria.*

1812, Jan. 24. Granville, Augustus Bozzi, M.D., F.R.S., V.P.O.S., M.R.C.P. Lond., M.R.C.S. Engl., Knt. of the Order of St. Michael of Bavaria, of the Crown of Würtemberg, of the Lion of Züringen of Baden, and of St. Maurice and St. Lazarus of Sardinia, For. Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Turin and Naples, Nat. Hist. Soc. Dresden, Philom. Soc., Soc. Méd. d'Emulat. and Cercle Méd. Paris, Socc. Georg. and Curéo, Florence, Med.-Chir. Socc. Petersburg and Berlin, Corr. Mem. Roy. Acad. Sc. Brussels, &c. 5 *Cornwall-terrace, Warwick-square, London.*

1850, Apr. 30. Harley, Rev. Robert, F.R.S., F.R.A.S. *Leicester.*

1816, Apr. 26. Kenrick, Rev. John, M.A. *York.*

1838, Apr. 17. Koechlin-Schouch, Daniel. *Mulhouse.*

1862, Jan. 7. Lancia di Brolo, Federico, Duc. Inspector of Studies, &c. *Palermo.*

1859, Jan. 25. Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, Mem. Imp. Leop.-Car. Acad. Nat. Sc., Imp. Soc. Naturalists Moscow, Acad. Nat. Sc. Philadelphia, Roy. Botan. Socc. Regensburg, Leiden, Edinburgh, Botan. Soc. Canada, Linnean Socc. Lyon, Bordeaux, and Caen, Physiogr. Soc. Lund, Imp. Roy. Geol. Instit. Vienna, Imp. Roy. Zool. and Botan. Soc. Vienna, Roy. Acad. Sc. Lucca and Prague, Imp. Acad. Sc. and Lit. Chambery, Toulouse, Rouen, Caen, Lille, &c., Acad. Socc. Cherbourg and Angers, Hortic. Soc. Cherbourg, Roy. Acad. Archeol. Brussels, Socc. Nat. Sc. Catania, Athens, Boston, Dorpat, Riga, &c. *Cherbourg.*

1857, Jan. 27. Lowe, Edward Joseph, F.R.S., F.R.A.S., F.G.S., Mem. Brit. Met. Soc., Hon. Mem. Dublin Nat. Hist. Soc., Mem. Geol. Soc. Edinburgh, &c. *Nottingham.*

1861, Oct. 29. Maury, Captain Mathew Fontaine, LL.D., &c.

1864, Apr. 19. Mitchell, Jesse, Captain, Superintendent of the Government Museum, Madras.

DATE OF ELECTION.

1862, Jan. 7. Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*

1851, Apr. 29. Pincoffs, Peter, M.D., Knt. of the Turkish Order of the "*Medjidié*" 4th Cl., Mem. Coll. Phys. London, Brussels, and Dresden, Hon. and Corr. Mem. Med. and Phil. Socc. Antwerp, Athens, Brussels, Constantinople, Dresden, Rotterdam, Vienna, &c. *Naples.*

1808, Nov. 18. Roget, Peter Mark, M.D., F.R.S., F.R.C.P. Lond., F.G.S., F.R.A.S., V.P.S.A., Corr. Mem. Roy. Acad. Sc. Turin. 18 *Upper Bedford-place, London,* W.C.

1867, Feb. 5. Schönfeld, Edward, Ph.D., Director of the Mannheim Observatory.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

1853, Apr. 19. Wilkinson, Thomas Turner, F.R.A.S. *Burnley.*

ORDINARY MEMBERS.

1861, Jan. 22. Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. *Bowdon.*

1861, Jan. 22. Anson, Rev. George Henry Greville, M.A. *Birch Rectory, Rusholme.*

1837, Aug. 11. Ashton, Thomas. 42 *Portland-street.*

1865, Nov. 15. Bailey, Charles. 17 *Kossuth-terrace, Moss Side.*

1824, Jan. 23. Barbour, Robert. 18 *Aytoun-street.*

1865, Nov. 15. Barker, Thomas, M.A., Prof. Math. Owens College. *Owens College.*

1867, Nov. 12. Barrow, John. 20 *Bellevue-street, Hyde-road.*

1840, Jan. 21. Bateman, John Frederick, F.R.S., F.G.S., M. Inst. C.E. 16 *Great George-street, Westminster.*

1858, Jan. 26. Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys. Econ. Soc. Konigsberg, and Ac. Sc. and Lit. Palermo. *Crescent-road, Cheetham Hill.*

1847, Jan. 26. Bazley, Thomas, M.P. *Eynsham Hall, Oxford.*

1867, Apr. 16. Beasley, Henry Charles. 3 *Rook-street.*

DATE OF ELECTION.

1847, Jan. 26.	Bell, William. 51 *King-street.*
1858, Jan. 26.	Benson, Davis. 4 *Chester-street.*
1854, Jan. 24.	Beyer, Charles. *Stanley-grove, Oxford-street.*
1842, Jan. 25.	Binney, Edward William, F.R.S., F.G.S. 40 *Cross-street.*
1821, Jan. 26.	Blackwall, John, F.L.S. *Hendre, Llanrwst.*
1861, Jan. 22.	Bottomley, James. 2 *Nelson-street, Lower Broughton.*
1855. Jan. 23.	Bowman, Eddowes, M.A. *Upper Park-road, Victoria Park.*
1839, Oct. 29.	Bowman, Henry. *Upper Park-road, Victoria Park.*
1855, Apr. 17.	Brockbank, William. 37 *Princess-street.*
1861, Apr. 2.	Brogden, Henry. *Brooklands, near Sale.*
1844, Jan. 23.	Brooks, William Cunliffe, M.A. *Bank,* 92 *King-street.*
1860, Jan. 24.	Brothers, Alfred, F.R.A.S. 14 *St. Ann's-square.*
1867, Dec. 10.	Broughton, Samuel. 253 *Cheetham-hill Road.*
1846, Jan. 27.	Browne, Henry, M.D., M.A., M.R.C.S. Engl. 206 *Oxford-street.*
1864, Nov. 29.	Buxton, Edmund Charles, jun. 81 *Peter-street.*
1859, Jan. 25.	Carrick, Thomas. 37 *Princess-street.*
1858, Jan. 26.	Casartelli, Joseph. 43 *Market-street.*
1852, Apr. 20.	Chadwick, David, F.S.S., Assoc. Inst. C.E. *Cross-street Chambers.*
1842, Jan. 25.	Charlewood, Henry. 5 *Clarence-street.*
1854, Apr. 18.	Christie, Richard Copley, M.A., Prof. Hist. Owens College. 7 *St. James's-square.*
1841, Apr. 20.	Clay, Charles, M.D., Extr. L.R.C.P. Lond., L.R.C.S. Edin. 101 *Piccadilly.*
1853, Jan. 25.	Cottam, Samuel. 2 *Essex-street.*
1859, Jan. 25.	Coward, Edward. *Heaton Mersey, near Manchester.*
1861, Nov. 12.	Coward, Thomas. *Bowdon.*
1847, Jan. 26.	Crace-Calvert, Frederick, Ph.D., F.R.S., F.C.S., Corr. Mem. Roy. Acad. Sc. Turin, Acad. Sc. Rouen, Pharmac. Soc. Paris, and Industr. Soc. Mulhouse. *Royal Institution, Bond-street.*
1851, Apr. 29.	Crompton, Samuel, M.R.C.S. Engl., L.S.A., F.R. Med.-Chir. Soc. 69 *Piccadilly.*
1848, Jan. 25.	Crowther, Joseph Stretch. 28 *Brazennose-street.*
1861, Apr. 2.	Cunningham, William Alexander. *Bank,* 37 *King-street.*
1854, Feb. 7.	Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*
1842, Apr. 19.	Dancer, John Benjamin, F.R.A.S. 43 *Cross-street.*
1863, Feb. 10.	Darbishire, George Stanley. 14 *John Dalton-street.*

DATE OF ELECTION.

1853, Apr. 19. Darbishire, Robert Dukinfield, B.A., F.G.S. 26 *George-street.*

1854, Jan. 24. Davies, David Reynold. 33 *Dickinson-street.*

1861, Dec. 10. Deane, William King. 25 *George-street.*

1855, Jan. 23. Dickinson, William Leeson. 1 *St. James's-street.*

1818, Apr. 24. Dyer, Joseph Chessborough. *Henbury, near Macclesfield.*

1859, Jan. 25. Eadson, Richard. 75 *Dale-Street.*

1824, Oct. 29. Fairbairn, William, C.E., LL.D., F.R.S., F.G.S., Corr. Mem. Imp. Inst. France, and Roy. Acad. Sc. Turin, Hon. Mem. Inst. Eng. Scot. and Yorksh. Phil. Soc. *Polygon, Ardwick.*

1861, Jan. 22. Fisher, William Henry. 16 *Tib-lane.*

1856, Apr. 29. Forrest, Henry Robert. 3 *Clarence-street.*

1857, Apr. 21. Foster, Thomas Barham. 23 *John Dalton-street.*

1860, Apr. 17. Francis, John. *Town Hall.*

1854, Jan. 24. Fryer, Alfred. 4 *Chester-street.*

1840, Jan. 21. Gaskell, Rev. William, M.A. 46 *Plymouth-grove.*

1861, Apr. 30. Gladstone, Murray, F.R.A.S. 24 *Cross-street.*

1817, Jan. 24. Greg, Robert Hyde, F.G.S. 2 *Chancery-place, Booth-street.*

1849, Oct. 30. Greg, Robert Philips, F.G.S. 2 *Chancery-place, Booth-street.*

1865, Nov. 28. Hampson, Francis. 63 *King-street.*

1862, Nov. 4. Hart, Peter. 45 *Back George-street.*

1839, Jan. 22. Hawkshaw, John, F.R.S., F.G.S., M. Inst. C.E. 33 *Great George-street, Westminster, London,* S.W.

1828, Oct. 31. Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*

1861, Apr. 30. Heys, William Henry. *Hazel Grove, near Stockport.*

1833, Apr. 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington Palace Gardens, London,* W.

1864, Mar. 22. Heywood, Oliver. *Bank, St. Ann's-street.*

1851, Apr. 29. Higgin, James. *Hulme Hall Chemical Works, Chester-road.*

1845, Apr. 29. Higgins, James. *King-street, Salford.*

1848, Oct. 31. Higson, Peter, F.G.S. 94 *Cross-street.*

1839, Jan. 22. Hobson, John. *Bakewell, Derbyshire.*

1861, Apr. 2. Hobson, John Thomas, Ph.D. *West Leigh Lodge, Leigh, Lancashire.*

1854, Jan. 24. Holcroft, George. *St. Mary's Gate.*

DATE OF ELECTION.

1855, Jan. 23. Holden, Isaac. 64 *Cross-street.*

1846, Jan. 27. Holden, James Platt. *St. James's Chambers,* 3 *South King-street.*

1824, Jan. 23. Houldsworth, Henry. *Newton-street Mills,* 34 *Little Lever-street.*

1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 42 *Quay-street, Salford.*

1859, Jan. 25. Hurst, Henry Alexander. 61 *George-street.*

1866, Nov. 13. Jack, William, M.A., Professor of Natural Philosophy, Owens College. *Owens College.*

1866, Nov. 13. Jevons, William Stanley, M.A., Professor of Logic, &c., Owens College. *Owens College.*

1850, Apr. 30. Johnson, Richard, F.C.S. *Oak Bank, Fallowfield.*

1865, Jan. 24. Johnson, William B. *Altrincham.*

1821, Oct. 19. Jordan, Joseph, F.R.C.S. Engl. 70 *Bridge-street.*

1848, Apr. 18. Joule, Benjamin St. John Baptist. *Thorncliff, Old Trafford.*

1842, Jan. 25. Joule, James Prescott, LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S., and Inst. Eng. Scot., Corr. Mem. Roy. Acad. Sc. Turin. *Cliff Point, Higher Broughton, Manchester.*

1843, Jan. 24. Kay, Samuel. 66 *Fountain-street.*

1852, Jan. 27. Kennedy, John Lawson. 47 *Mosley-street.*

1867, Nov. 26. Kipping, James Stanley. *Branch Bank of England.*

1862, Apr. 29. Knowles, Andrew. *High-bank, Pendlebury.*

1830, Apr. 30. Langton, William. *Manchester and Salford Bank, Mosley-street.*

1860, Jan. 24. Latham, Arthur George. 24 *Cross-street.*

1863, Dec. 15. Leake, Robert. 100 *Mosley-street.*

1850, Apr. 30. Leese, Joseph. *Altrincham.*

1860, Jan. 24. Leigh, John, M.R.C.S. Engl., L.S.A., F.C.S. *York Chambers, King-street.*

1839, Oct. 29. Lockett, Joseph. 100 *Mosley-street.*

1857, Jan. 27. Longridge, Robert Bentink. 1 *New Brown-street.*

1854, Jan. 24. Lowe, George Cliffe. 37 *Lever-street.*

1850, Apr. 30. Lund, Edward, M.R.C.S. Engl., L.S.A. 22 *St. John's-street.*

1859, Jan. 25. Lynde, James Gascoigne, M. Inst. C.E., F.G.S. *Town Hall.*

1855, Oct. 30. Mabley, William Tudor. 14 *St. Ann's-square.*

1829, Oct. 30. McConnel, James. *Bent-hill, Prestwich.*

1838, Apr. 17. McConnel, William. 90 *Henry-street, Oldham-road.*

1844, Apr. 30. McDougall, Alexander. *The Eaves, Chapel-en-le-Frith.*

DATE OF ELECTION.

1866, Nov. 13. McDougall, Arthur. 11 *Riga-street, Hanover-street.*

1823, Jan. 24. Macfarlane, John. *Edge-hill House, Coney-hill, Bridge of Allan, Scotland.*

1859, Jan. 25. Maclure, John William, F.R.G.S. 2 *Bond-street.*

1849, Apr. 17. Manchester, the Right Rev. the Lord Bishop of, D.D., F.R.S., F.G.S., F.C.P.S., Corr. Mem. Arch. Inst. Rome. *Diocesan Registry Office, 7 St. James's-square.*

1858, Apr. 20. Mather, Colin. *Iron Works, Deal-street, Brown-street, Salford.*

1864, Nov. 1. Mather, William. *Iron Works, Deal-street, Brown-street, Salford.*

1837, Jan. 27. Mellor, William. *Lime Works, Ardwick.*

1864, Mar. 8. Micholls, Horatio. 7 *Nicholas-street.*

1864, Mar. 22. Montefiore, Leslie J. 17 *Cannon-street.*

1861, Oct. 29. Morgan, John Edward, M.B., M.A., M.R.C.P. Lond., F.R. Med. and Chir. S. 1 *St. Peter's-square.*

1849, Jan. 23. Morris, David. 1 *Market-place.*

1864, Mar. 22. Mudd, James. *St. Ann's-square.*

1852, Jan. 27. Nelson, James Emanuel. 17 *Bridgewater-street, High-street.*

1854, Feb. 7. Nevill, Thomas Henry. 19 *George-street.*

1850, Jan. 24. Newall, Henry. *Hare-hill, Littleborough.*

1862, Dec. 30. Ogden, Samuel. 10 *Back Mosley-street.*

1861, Jan. 22. O'Neill, Charles, F.C.S., Corr. Mem. Industr. Soc. Mulhouse. 4 *Bank-place, St. Philip's Church, Salford.*

1844, Apr. 30. Ormerod, Henry Mere. 5 *Clarence-street.*

1861, Apr. 30. Parlane, James. 16 *Dickinson-street.*

1861, Jan. 22. Parr, George, jun. *Phœnix-works, Chapel-street, Ancoats.*

1866, Mar. 20. Patterson, John. *Oak-mount, Withington.*

1861, Jan. 22. Perring, John Shae, M.Inst.C.E. 104 *King-street.*

1857, Apr. 21. Platt, William Wilkinson. *Iron-works, Deal-street, Brown-street, Salford.*

1854, Jan. 24. Pochin, Henry Davis. 42 *Quay-street, Salford.*

1860, Apr. 17. Pocklington, Rev. Joseph Nelsey, B.A. *Rectory, St. Michael's, Hulme.*

1861, Jan. 22. Radford, William. 41 *John Dalton-street.*

1854, Feb. 7. Ramsbottom, John. *Railway-station, Crewe.*

1859, Apr. 19. Ransome, Arthur, B.A., M.B. Cantab., M.R.C.S. 1 *St. Peter's-square.*

DATE OF ELECTION.

1859, Jan. 25. Rideout, William Jackson. 11 *Church-street.*

1860, Jan. 24. Roberts, William, M.D., B.A., M.R.C.P. Lond. 89 *Mosley-street.*

1864, Dec. 27. Robinson, John. *Atlas-works, Great Bridgewater-street.*

1822, Jan. 25. Robinson, Samuel. *Black Brook Cottage, Wilmslow.*

1864, Jan. 12. Rogerson, John. *Gaythorn.*

1858, Jan. 26. Roscoe, Henry Enfield, B.A., Ph.D., F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*

1851, Apr. 29. Sandeman, Archibald, M.A. *Tulloch, near Perth.*

1842, Jan. 25. Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*

1863, Apr. 7. Schwabe, Edmund Salis, B.A., F. Anthrop. Soc. 41 *George-street.*

1855, Jan. 23. Sharp, Edmund Hamilton. *Seymour-grove, Old Trafford.*

1852, Apr. 20. Sidebotham, Joseph. 19 *George-street.*

1865, Dec. 26. Simpson, Henry, M.D. 335 *Oxford-street.*

1859, Jan. 25. Slagg, John, jun. 12 *Pall Mall.*

1838, Jan. 26. Smith, George Samuel Fereday, M.A., F.G.S. 2 *Essex-street, King-street.*

1845, Apr. 29. Smith, Robert Angus, Ph.D., F.R.S., F.C.S., Corr. Mem. I.R. Geol. Inst. Vienna. 20 *Devonshire-street, All Saints.*

1864, Dec. 13. Sonstadt, Edward. *Brunswick-terrace, Prestwich.*

1859, Jan. 25. Sowler, Thomas. *Red Lion-street, St. Ann's-square.*

1851, Apr. 29. Spence, Peter, F.C.S., M.S.A. *Alum-works, Newton-heath.*

1864, Dec. 27. Spencer, Joseph. *Brown-street.*

1852, Jan. 27. Standring, Thomas. 1 *Piccadilly.*

1847, Apr. 20. Stephens, James, F.R.C.S., L.S.A. 68 *Bridge-street.*

1858, Jan. 26. Stewart, Charles Patrick. *Atlas-works,* 88 *Great Bridgewater-street, and Oaklands, Victoria-park.*

1863, Oct. 6. Stretton, Bartholomew. *Bridgewater-place, High-street.*

1814, Jan. 21. Stuart, Robert. *Ardwick-hall.*

1859, Jan. 25. Tait, Mortimer Lavater. 7 *Church-street.*

1856, Jan. 22. Taylor, John Edward. 3 *Cross-street.*

1860, Apr. 17. Trapp, Samuel Clement. 18 *Cooper-street.*

1821, Apr. 19. Turner, Thomas, F.R.C.S. Engl., F.L.S., F.R. Med.-Chir. S., Hon. F. Harv. Soc. 77 *Mosley-street.*

DATE OF ELECTION.	
1861, Apr. 30.	Vernon, George Venables, F.R.A.S., F.M.S., F. Anthrop. Soc. Mem. Met. Soc. Scotl., and Met. Soc. France. *Auburn-street, Piccadilly.*
1859, Jan. 25.	Watson, John. *Rose-hill, Bowdon.*
1857, Jan. 27.	Webb, Thomas George. *Glass-works, Kirby-street, Ancoats.*
1858, Jan. 26.	Whitehead, James, M.D., M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., Corr. Mem. Soc. Nat. Phil. Dresden, Med. Chir. Soc. Zurich, and Obst. Soc. Edin., Mem. Obst. Soc. Lond. 87 *Mosley-street.*
1839, Jan. 22.	Whitworth, Joseph, F.R.S. *Chorlton-street, Portland-street.*
1859, Jan. 25.	Wilde, Henry. 37 *Lever-street.*
1859, Apr. 19.	Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*
1853, Apr. 19.	Williamson, Samuel Walker. *St. Mark'-place, Cheetham-hill.*
1851, Apr. 29.	Williamson, William Crawford, F.R.S., Professor of Natural History, Anat., and Physiol., Owens College, M.R.C.S. Engl., L.S.A. 172 *Egerton-road, Fallowfield.*
1851, Jan. 21.	Withington, George Bancroft. 24 *Brown-street.*
1836, Jan. 22.	Wood, William Rayner. *Singleton Lodge, near Manchester.*
1855, Oct. 30.	Woodcock, Alonzo Buonaparte. *Orchard Bank, Altrincham.*
1860, Apr. 17.	Woodcroft, Rufus Dewar. *Cornbrook Chemical-works, Chester-road.*
1860, Apr. 17.	Woolley, George Stephen. 69 *Market-street.*
1840, Apr. 28.	Worthington, Robert, F.R.A.S. 96 *King-street.*
1863, Nov. 17.	Worthington, Samuel Barton, C.E. *Crescent-road, Cheetham-hill.*
1865, Feb. 21.	Worthington, Thomas. *John Dalton-street.*
1864, Nov. 1.	Wright, William Cort, F.C.S. *Whalley-range.*

NOTE.—*It is requested that any mistakes or alterations in the designations or addresses of Members, as given in this list, be notified to the Librarian of the Society.*

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