With Dr Budd's regards

REPORT

OF

TWENTY CASES

OF

MALIGNANT CHOLERA

THAT OCCURRED IN THE SEAMEN'S HOSPITAL-SHIP,
DREADNOUGHT,
BETWEEN THE 8TH AND 28TH OF OCTOBER, 1837.

BY GEORGE BUDD, M.B. F.R.S., PHYSICIAN,

AND

GEORGE BUSK, ESQ., SURGEON,

TO THE DREADNOUGHT.

FROM THE TWENTY-FIRST VOLUME OF THE MEDICO-CHIRURGICAL TRANSACTIONS, PUBLISHED BY THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY OF LONDON.

London:

PRINTED BY G. WOODFALL, ANGEL COURT, SKINNER STREET.

1838.

REPORT

OF

TWENTY CASES

OF

MALIGNANT CHOLERA

THAT OCCURRED IN THE SEAMEN'S HOSPITAL-SHIP, DREADNOUGHT*, BETWEEN THE 8TH AND 28TH OF OCTOBER, 1837.

By GEORGE BUDD, M.B. F.R.S., Physician,

AND

GEORGE BUSK, Esq., Surgeon,

TO THE DREADNOUGHT.

READ DECEMBER 14TH, 1837.

The person in whom the disease first shewed itself was A. J. Bernet, aged 18, who was admitted on the 29th of September, the day of his arrival from Dantzic, on account of a lacerated wound of the scalp. On the third of October he became feverish, and on the following day, erysipelas appeared about the wound; it continued to spread until the 8th, when it occupied the whole of the hairy scalp. He was confined to his bed, and was on low diet.

* H. M. S. Dreadnought, three decker, is converted into a hospital for seamen in the port of London, and moored off Greenwich.

In the night of the 8th, was seized with cholera, of which he died, at eight P.M. on the 11th.

The second person attacked was Michael Wisdell, 25, a strong muscular man, who was admitted on the 5th of October, 14 days after his arrival from Quebec, on account of venereal sores and bubo. His treatment had been local, his general health was good on the 9th, and he was on ordinary diet. At 3 A.M. on the 10th, he was seized with vomiting and purging of bilious matter, with very severe cramps in the abdomen and limbs. The evacuations towards the evening were liquid and white, with flocculi. The urine was suppressed; there was deafness, loss of voice, vertigo; the surface became cold, and the features collapsed. The vomiting continued twenty-four hours. In the morning of the 11th, the evacuations were again bilious, and at 10 P.M. he passed some urine for the first time since his attack. Diarrhœa continued two or three days, during which his pulse was intermittent, and he was discharged on the 19th, well.

The 3d and 4th cases occurred in the nights of the 10th and 13th. The fifth case was that of James Thomas, 21, a strong, florid, healthy looking man, who was admitted on the 2d of October, ten days after his arrival from Dantzic, with chancres and bubo. His treatment was local, and he was on ordinary diet. General health unimpaired up to the 14th of October; in the evening of that day diarrhœa occurred, and occasional vomiting; he made no complaint until the next morning, when cramps supervened. The matters ejected were not kept.

October 15th. 9 A.M. Surface generally blue and cold; tongue and breath cold; pulse scarcely perceptible; complains of frequent vomiting and purging, and cramps; pulse more developed after a warm bath.

$10\frac{1}{2}$ A.M. Lying with eyes closed; lips very livid, skin of the fingers shrivelled; cramps in the hands and arms; constant jactitation; tongue less cold; voice not weak; pulse 120, just perceptible: complains of headache, and a sensation as if his ears were stopped. Matters vomited are yellowish and bitter.

$2\frac{1}{2}$ P.M. Surface generally very cold: lips, nose, and tongue blue; pulse imperceptible; occasional cramps in the abdomen; vomiting has ceased; diarrhœa continues; stools liquid, colourless, with white flocculi in suspension.

The vomiting did not recur; the stools became less frequent, and at $7\frac{1}{2}$ P.M. is the following note—four stools since 6 P.M., colourless with flocculi; pulse felt in the right arm, which is under the bed-clothes, but not in the left, which is exposed. Pain in the region of the liver and nowhere else. (Treatment, large doses of calomel and stimulants.)

He rapidly sunk; at 9 P.M. pulse imperceptible; at 11 P.M., violent cramps in the neck and throat; at $11\frac{1}{2}$ P.M. he died. No urine from the time of attack.

Case 6th. George Abbs, 18; in port of London from Richebucto, since the 22d of August. Admitted on the 16th of September, with periostitis and impetiginous eruption. Ordinary diet.

Was nearly well on the 14th of October, and his general health good. About 4 A.M. on the 15th, was seized with vomiting and purging. At 8 A.M., cold perspiration and dizziness, with partial deafness; about 9 A.M. violent cramps supervened, which were confined to the lower extremities. At this time, lips and countenance blue; tongue livid, whitish on the upper surface, moist, not cold; pulse 108, scarcely perceptible; matters vomited reported colourless, and insipid; has passed some urine in the morning.

2½ P.M. Cramps extended to the abdominal and thoracic muscles; surface much colder; pulse less distinct; no vomiting; a constant flow of liquid from the intestines, with brownish flocculi in suspension.

The stage of collapse was progressive; the vomiting and cramps remitted occasionally. At 9 A.M. on the 16th, is the following note.—Skin blue and cold; no pulse; vomits every thing he takes; diarrhœa profuse; stools characteristic of cholera. The calomel, of which he had taken large doses from the beginning, rejected from the stomach enveloped in viscid mucus, but otherwise unaltered.

In the course of the day, there was a slight return of pulse at the wrist, and the surface was not quite so cold; evacuations thicker and resembling gruel; a sensation of heat, though his surface was cold. Frequent and painful cramps.

At 9 A.M. on the 17th, he was cold, blue, and without pulse at the wrist; half comatose; eyes suffused; no vomiting or purging for some time past; no urine for 48 hours; refuses all food. At 1 P.M. he died.

It would be tedious to give the details of any more cases, as the preceding are sufficient to shew the identity of the disease with cholera, as it appeared in London, in 1832. We shall content ourselves, therefore, by presenting a tabular view of the remaining cases, and by mentioning the following circumstances, which, from their illustrating the history of the disease, seem to deserve notice.

In the 7th case, the evacuations, after having consisted, during several hours, of the whitish or gruelly fluid, said to be characteristic of the disease, assumed a peculiar character; although still profuse, they became brown or blackish, from the suspension in the colourless liquid of brown or black flocculi, sufficiently numerous to impart their colour to the whole mass. When poured on a filter, a colourless fluid transuded, and the brown or black flocculi remained on the paper.

In the 8th case, the dejections, continuing in other respects characteristic, were observed to become brownish at a later period of the disease; they consisted of a thick red fluid.

Evacuations of a similar character were noticed in cases 6 and 13. In all, they appeared after a prolonged continuance of the rice-water discharges, and seemed to owe their colour to the elimination, through the intestines, of the red particles of the blood*.

It will be seen from the tables, that these cases offer examples of cholera occurring in conjunction with each of the following diseases: typhoid fever, acute and chronic rheumatism, erysipelas, ague, dysentery, syphilis, and gonorrhœa.

(During former epidemics of cholera, the disease has occurred in the Dreadnought in persons affected with fever, cynanche tonsillaris, rheumatism, scurvy, and diabetes mellitus. In the subject of the last mentioned disease, the urine, which, before the attack of cholera, was highly saccharine and enormous in quantity, was as completely suppressed as in the others. This man remained in the ship a fortnight after his recovery from cholera; at the end of this time, the inordinate secretion of urine had not returned.)

The 4th and 16th cases were interesting, as shewing the influence of cholera on the rheumatic affection. The subject of the 4th case, was labouring under rheumatic fever of a severe form. On the 13th was totally unable to move his legs; both knees were much swelled, presenting an evident sense of fluctuation, and were protected by a cradle. At one P.M., on the 14th, 13 hours from the attack of cholera, it was noted that he was in a state of jactitation, and

* Evacuations presenting the same appearance have been noticed by Dr. Graves, of Dublin, who, in a lecture recently published in the Medical Gazette, ascribes their colour to the acetate of lead which he administered to his patients.

moved his legs freely; that the swelling of the right knee had quite subsided, and that of the left diminished. On the 16th, the swelling of both knees quite subsided, the pain in them ceased, and no traces of the rheumatic affection left. The knee joints were examined after death, and found to contain no fluid, but small shreds of white false membrane adherent to the synovial surface. The subject of the 16th case was admitted on the 19th of October, on account of a rheumatic affection of the joints, which had continued for ten weeks, during the last four of which he had been unable to walk, and had, in consequence, been confined to his hammock. At the time of his admission, was unable to walk, to extend his legs, or to clench his fist; complained of pain in the limbs, worse at night; there was tenderness, principally confined to the joints. In the night of the 21st, was attacked with cholera. On the 23d could move his legs freely, with scarcely any pain. On the 4th of November, was walking about the wards, presenting very slight traces of his rheumatic affection.

The following points of his former history are also interesting. He stated that he had twice previously been affected with cholera at Calcutta, that he was attacked, for the first time, while on board his ship, and subsequently conveyed to the hospital, on the 17th of May, 1831, two days after his arrival at Calcutta; for the second time in July, 1833, three weeks after his arrival at the same port; that each of these attacks was more violent than the present, (the vomiting, purging, and cramps more severe,) and that in each he was treated by violent rubbings, brandy, and opium. This man recovered, and during his convalescence was several times questioned respecting the circumstances of these attacks, of which he always gave us the same account.

The 17th case, the one most rapidly fatal, occurred in a negro, a native of Congo.

Mortality.—We have already said that the number of cases was 20; of these, 12 proved fatal; but there was a remarkable variation in the mortality. Thus, of the 10 persons first attacked, 8 died, while of the remaining 10 cases, 4 only proved fatal. The number of these cases is, perhaps, too small to justify us in drawing any conclusions from this circumstance. It may, however, be remarked, that it is confirmatory of an opinion sanctioned by previous experience, that epidemics are milder and less fatal towards their conclusion; especially, as this variation in the mortality cannot be ascribed to any difference in the treatment adopted, or in the previous condition of the patients.

Treatment.—The treatment, in most of the cases, consisted in a bleeding at the commencement, when practicable; in the administration of large and frequently repeated doses of calomel; and, during the cold stage, in attempts to restore the temperature of the surface and alleviate the cramps, by means of frictions, hot-air baths, &c. The general result is unsatisfactory, and is rendered still more so by an inspection of the table, which discloses the humiliating fact, that one half of the recoveries took place in cases comparatively mild.

Pathology.—The bodies were carefully examined after death in 11 of the 12 fatal cases; in all with the exception of case 9. The following is a summary of the appearances which they presented.

The only circumstances noticed in their external condition were rigidity and a violet colour of the back—the rigidity being generally greater, and the intensity of the violet colour less, as the bodies were examined nearer the epoch of death.

Organs of Digestion.—The appearance of the external or peritoneal surface of the stomach and small intestines varied, as the patients died at a period more or less remote from the attack. In the cases most rapidly fatal, or during the stage of collapse, they were observed to be viscid, and of a pale rose colour externally. The viscidity became less remarkable or disappeared, and the rose colour was replaced by their ordinary grey tint, in those cases that proved fatal after decided reaction. The large intestines were grey externally in every case.

The mucous membrane of the œsophagus was vascular in its lower portion in case 8 alone; in all the others, pale and healthy; studded with enlarged flat mucous follicles in its lower third in 17 cases; presenting a slight enlargement of the follicles in its upper part in 11.

The stomach was generally large; its mucous membrane pale in three cases, (7, 14, 16,) in the others, presenting a greater or less degree of redness, which could be seen to depend on the injection of very minute vessels on its free surface, either in the splenic or pyloric extremity, or both. Its consistence was carefully examined in all the cases, and in none presented any remarkable change ; it appeared, however, to be softer than natural in the splenic extremity in 1, 3, 8, 11.

In most cases it was more or less mammellated, this appearance being either general, or confined to the pyloric extremity. In 7, 14, by drawing the coats of the stomach between the finger and thumb, and using some pressure, a milky fluid was made to exude, and the mammellated appearance destroyed; the mucous membrane of the portion so treated afterwards appearing smooth, and of normal thickness and consistence. This was not the case in 17 ; the mammellated appearance could not be destroyed, nor any fluid expressed. In this case, the mucous membrane was coated with very viscid, firmly adherent mucus, but there was none adhering to it in 7, 14.

The general contents were similar to those evacuated during life, and require no particular notice ; there were some adherent patches of mucus, in which calomel was entangled, in 5 ; a few ounces of thick, grey mucus, not adherent, in 16 ; an universal coating of viscid, adherent mucus, in portions of which calomel was enveloped, in 17.

The condition of the duodenum was not noted in 1, 3, 4.

Its mucous membrane was vascular in 5, 8; pale in 6, 14; greyish in 7, 8, 17; this greyness resulting from minute black specks at the apices of the villi.

Nothing unusual was observed in its texture, excepting in some cases, perhaps, a greater friability than natural.

In every case in which it was examined, solitary glands were very conspicuous, giving the membrane more or less of a granular aspect: these were in all cases more numerous near the pylorus, and in none extending into the jejunum.

In 14, the villi of a white colour were remarkably distinct, and, on pressure between the fingers, yielded a milky fluid.

The orifice of the biliary duct was observed to be unusually prominent in 5, 6.

The mucous membrane of the small intestines was pale throughout in 3, 7, 14; but there was generally increased vascularity, giving rise, in some cases, to a purple colour, in portions here and there, especially near the termination of the ileum. It was observed to be grey from minute black specks in 7, 8, 17; this greyness was general in 7, 8, confined to the jejunum and upper part of the ileum in 17. It will be remembered that, in 7, 8, the discharges during life contained brownish or black flocculi.

In case 11, in the lower portion of the ileum, the mucous membrane was generally softened, and presented seven or eight small ulcerations, which probably resulted from the typhoid affection of which this man was the subject. In the other cases, the texture of the mucous membrane had undergone no appreciable change.

The glands of Peyer were remarkably developed in all the cases, and the most so generally in those that proved fatal rapidly, or during the stage of collapse. They were of the same colour as the surrounding membrane, but in two cases, (6, 8,) in which this colour was red, the tint of the patches was observed to be deeper. When pale, they were in all cases dotted with black points. The glands of Brunner were observed, in every case, in the lower portion of the ileum, as small elevated beads, of the same colour as the membrane. These, as well as the glands of Peyer, were the most developed in the cases that proved rapidly fatal, and in all these sufficiently so to give a sensation of roughness to the finger passed over the membrane.

The contents of the small intestines were found tinged with bile in none of the cases that proved fatal within thirty-six hours, but more or less so, with one exception, (case 8,) in all those that were more protracted. This tint was confined to the jejunum in 3, 6; to the ileum in 7. They were brownish in the jejunum in 7, 8; of a plum colour, evidently from the admixture of blood in the ileum, in 8.

In most cases, the mucous membrane was more or less coated by a pasty substance.

The mucous membrane of the large intestine was, in most cases, pale throughout; observed to be reddened from vascularity in some of the protracted cases only (1, 8, 11). This redness was confined to the first portion of the intestine in 1, 11; general in 8, but more intense in patches, and on the surface of these patches were streaks of effused blood.

Conspicuous follicles were observed in the large intestine in all the cases, with one exception, case 16. They occurred as flat, slightly elevated circles, about a line in diameter, with a central black speck, and, in every case, diminished in number, and were less conspicuous as we receded from the cæcum.

One small ulceration was found in the cæcum, in 11; and, in 14, in the transverse and descending colons, there were many, none of them larger than the surface of a split pea, most of them much smaller, with smooth edges, and apparently in process of cicatrization. The ulceration in the former case was probably the result of the typhoid affection; those in the latter of the dysentery, of which these patients were respectively the subjects.

The cæcum and ascending colon were generally distended; this was not the case with the descending colon, which, in several of the examinations, was observed to be contracted.

The mesenteric glands were enlarged in almost every case; in some, they were purplish, in others, pale.

The liver presented nothing remarkable. The gall-bladder was distended with dark bile, in every case, with the exception of 3, in which it contained pus, and presented numerous ulcerations of its mucous membrane.

The spleen was of natural size or smaller than usual, and firm in 1, 3, 5, 6, 7, 8, 16; of a light red colour in 5, 7, 8, 16; its colour not noticed in 1, 3, 6. It was unusually large in three cases only, 11, 14, 17; soft and dark-coloured in 11; soft, and contained a purple creamy fluid, which could be expressed, leaving a white spongy substance in 14; firm and dark-coloured in 17.

The appearances noticed in these three cases seem to depend on causes foreign to cholera: the subject of the 11th case was affected with typhus, of the 14th with ague, and that of the 17th was a native of the western coast of Africa.

Organs of respiration.—The condition of the lungs varied as the patients died more or less remotely from the attack. They were found healthy, or simply congested, in four of five cases that proved fatal within thirty-six hours; while of six cases, in which the patients lived at least forty-five hours after the attack, four presented pneumonia. In one of these (8), fatal at the end of forty-five hours, the pneumonia was very partial, interlobular, and confined to the lower lobe of the right lung; in two, (4, 7,) fatal at the end of 96 and 138 hours respectively, the lower lobes of *both* lungs were found in a state of red hepatisation. In all these cases, the pneumonia was latent; there were no symptoms indicating its presence, and, while the patients lived, we had no suspicion of its existence. We were not aware at the time these patients were treated, that the same observation had been made by Mr. Jackson, in his Report of Cholera in Paris, in 1832. By him, pneumonia was found to exist in one half of the cases that proved fatal after reaction, and in all these, it was latent. This is unquestionably, as Mr. Jackson remarks, the most important fact, with reference to practice, that dissection has as yet disclosed, and shows us the necessity of investigating by auscultation the condition of the lungs in all cases in which reaction has been established.

With the exception of old adhesions, there was no affection of the pleura in any case.

Heart. There was a small quantity of serum in the pericardium in two cases only, those the most rapidly fatal (14, 17).

The muscular substance of the heart was generally flabby and purplish, and in two protracted cases (4, 8,) it presented ecchymosed spots on its surface.

The contents of the ventricles were noted in ten cases. There were fibrinous clots in the left ventricle in two cases only, and those protracted (4, 8); in the right ventricle, in 3, 4, 5, 8, 14, 17.

In 4, the clot was firmer, and in 8, larger in the right ventricle than in the left. In all the other cases, the cavities contained a greater or less quantity of dark fluid, or grumous blood, which had communicated no stain to the lining membrane.

The kidneys were observed in every case, and in none presented any thing unusual in size or texture.

The cortical substance was purplish throughout, or pale, and presenting dark congested vessels in most of the cases. In all, with one exception, (case 6,) a white, puriform fluid could be expressed from the mammillary point. The urinary bladder was empty and contracted in all those who died during the stage of collapse; it contained a small quantity of urine in some of the others.

The head was examined in six cases (1, 5, 6, 7, 8, 14). In 1, 6, nothing remarkable was observed; in 5, 8, the vessels of the dura mater and of the hemispheres were congested, the cortical substance of the brain unusually dark coloured. In 8, the surface of the brain was viscid; in 14, there was a considerable serous effusion under the arachnoid, and in the sheath of the spinal chord. In 7, no vascularity of the dura mater; the surface of the brain exsanguineous, the cortical substance not darker than natural; on the inferior surface of the left anterior lobe, were two slight depressions on the surface of the brain, the largest having the area of a sixpence, both coated by a yellowish transparent substance, and apparently the result of old sanguineous effusions.

Besides these cases of decided cholera, three cases occurred in the medical ward of violent vomiting and purging, which continued some hours and then ceased, without having assumed any characteristic appearance, or without being attended by any considerable collapse. A great number of other patients became affected with diarrhœa, during the same period. Mention is made of this in the notes of the medical ward in seven cases, but it occurred in a much greater number, and from our attention being concentrated on severer cases, was not noted. Similar circumstances were observed in the surgical wards. They are probably attributable to the influence that produced cholera, and serve, perhaps, to shew that this influence was more general than would be imagined, from the number of cases in which unequivocal symptoms of this disease were manifested.

This view is confirmed by pathological considerations. The intestinal canal was carefully examined in every person who died in the ship during the prevalence of cholera, and in several who owed their death to various diseases, and who, during life, had manifested no symptoms of cholera, an unusual development of the intestinal glands was noticed. It would occupy too much space to enter into the details of these cases; we proceed at once, therefore, to the consideration of the causes of the disease.

Causes. The disease cannot be attributed *solely* to any general atmospheric condition, for the following reasons.

1st. All the cases occurred in persons previously in the ship, in a population of about 200, while not a single case was admitted from the ships in the river, in a population fifty times as great, sending nearly all their sick to the Dreadnought.

2d. Not a case occurred on board the Marine Society's ship, the Iphigenia, moored at the stern of the Dreadnought, and consequently subject to the same general atmospheric influences. The population of the Iphigenia is 107, of whom 100 are boys, whose average age is about 15, all healthy subjects. When sufficiently ill to require constant attendance, they are sent to the Dreadnought. This immunity may be partly owing to age, especially as it was observed, when the disease prevailed in 1832, that children were attacked less frequently than adults. This cannot, however, be the sole cause, as the subject of the ninth case was only 18, little above the average age of the boys of the Iphigenia. It is worthy of notice that during the epidemic in 1832, when cases of cholera occurred in almost all the vessels in the river, the Iphigenia enjoyed the same immunity. We can only account for this from the favourable hygienic conditions of youth, good food, and clothing, regular exercise, free ventilation, and complete separation of the sick.

3d. No case, that we can learn, has occurred in Greenwich Hospital, subject to nearly the same general atmospheric influences, in a population of 2,700 pensioners, between the ages of 50 and 80, besides officers and servants. The same cause of exemption does not hold here, as the disease was observed, in former epidemics, to be both more frequent and more fatal in old persons.

We must, then, seek for the immediate cause among the circumstances peculiar to the Dreadnought.

1st. *Infection from foreign ports.* If the disease was brought by any of those who experienced it, it was most probably by Bernet, in whom it first shewed itself. He left Dantzic on the 8th of

September, and no case of cholera occurred in the vessel in which he sailed; so that, on this supposition, the disease, in him, must have had a period of incubation of thirty days, a circumstance very improbable if we consider that the second and third cases occurred in the two following nights; that the five persons seized on the 21st and 22d had come from the four quarters of the globe, and consequently could not have brought the disease, and at the time of their attack had been in the ship from two to seven days only; and that the whole duration of the visitation was only nineteen days.

The only other person of those affected by the disease, who can be supposed to have introduced it, is Thomas, the fifth attacked. He also came from Dantzic, but he arrived in port seven days before Bernet, and had been in the hospital twelve days at the time of his attack.

It may be supposed possible for the disease to have been brought by a person who did not himself experience it. This is rendered less probable by the measures that are always taken with respect to the clothes of the patients. These, as soon as the patients have reached the beds allotted to them, are taken by the nurses and delivered to the boatswain, who keeps them in large lockers appropriated to this purpose under the forecastle, and returns them to the patients on their discharge.

The following is, however, the evidence on this head. From the 1st of September to the 12th of October, were admitted 334 patients. Of these, 158 came from English and Irish ports, 98 from ports beyond Europe, and 78 from European, including Mediterranean ports. We have carefully examined all the latter, and have included in the following table the names of the most suspicious persons,—the port from which they last sailed,—the number of days since their arrival in this country, and their diseases.

Date of Admission.	Name.	Port sailed from.	How long in port before admission.	Disease.
			Days.	
Sept. 26	J. Stokes ..	Hamburgh .	1	Venereal.
—— 27	J. Reis	Memel	3	Orchitis.
Oct. 4	J. Aiken ..	Hamburgh .	1	Fever.
—— 7	J. Archer ..	Rotterdam .	3	Bubo.
—— —	J. Grigg ..	Dantzic....	8	Fever.
—— 9	W. Bryan..	Hamburgh .	3	Gonorrhœa.

In the latter end of August and the early part of September, cholera was prevalent at Berlin; and, by enquiries made through the mercantile house to which the ship that brought Bernet was consigned, we have learned that some cases occurred about the same time at Dantzic; but they were too few in number to excite attention, and the Prussian consul in London received no information respecting them.

2d. *Infection from one person to another in the ship.*

The two following circumstances afford, perhaps, the strongest argument against this mode of propagation.

1. None of the nurses or medical men were at-

tacked; the latter lived on board, and were constantly employed in attending these patients, and in making the examinations after death, about each of which a considerable time was spent, and which were conducted in the lowest part of the ship, in a small cabin in which all the bodies were deposited.

2. The disease was not propagated from the Dreadnought. During its prevalence there, patients were almost daily discharged, who immediately entered other ships in the river, but did not in a single instance communicate the disease to their crews.

The remaining evidence on this point is best expressed by the accompanying tables *, one of which represents the three decks, with the position of the bed occupied by each patient at the time of his attack; the other, the day and hour in which this attack took place.

It appears from these tables,

1°. That 1 case occurred in the convalescent or spar deck.
3 in the upper deck.
8 in the lower or middle deck.
7 in the orlop or lowest.

The remaining case was that of the boatswain, whose cabin is under the forecastle.

Now, on the 12th of October, there were 60 patients in the upper deck, the one appropriated to surgical cases; 54 in the middle, or medical deck; and 58 in the orlop deck, in which are placed patients that require the least attention.

* See the end of the Paper.

The disease then, although the first case occurred on the upper deck, prevailed most in the middle and orlop decks.

2°. That the first case occurred in the upper, the second in the orlop, or lowest deck, and the third in the middle deck; and that only three cases occurred in the upper deck, in which the disease first shewed itself, and these in the nights of the 8th, 15th, and 16th.

3°. That all the patients, with the exception of one, the date of whose attack was not well marked, were seized in the night, reckoning as such the time from 5 P.M. to 7 A.M. In this interval the attacks seemed pretty evenly distributed, equal numbers (6, 6) happening before and after midnight; the date of the others some time in the night not specified.

Giving the above definition of the night, the cases were distributed as follows:

1	in the night of the 8th.	
1	.... 9th.	
1	.... 10th.	
1	.... 13th.	
2	.... 14th.	
5	.... 15th.	
1	.... 16th.	
1	.... 17th.	
3	.... 21st.	
2	.... 22d.	The precise time of attack of one of these not specified.
1	.... 23d.	
1	.... 27th.	

Since the night of the 27th, no fresh cases have occurred.

Thus it appears that there were two active periods of the disease, the nights of the 14th and 15th, and those of the 21st and 22d. In these nights nearly two-thirds of the cases happened. In the first of these periods, five out of seven cases occurred in the orlop deck; in the second, all occurred in the middle deck.

The preceding facts are not very favourable to the supposition that the disease was brought from abroad, or propagated by contagion in the ship. The influence on which it depended seemed to act with an intensity variable, but generally greater in the lower decks; and, in almost all cases, its effects were first manifested during the night.

We proceed to examine some other circumstances which may be supposed to have conduced to the greater prevalence of the disease in the lower decks.

Ventilation.—As this is effected principally through the port-holes, it is most efficient in summer, when these can be kept open and a current of fresh air allowed to pass through the ship. Now, during the fortnight which preceded the appearance of cholera, the weather had been remarkably fine and mild, and the ventilation of the ship in consequence as good or better than it usually is at that season. With respect to the relative ventilation of the different decks, it is unquestionably, from its position and the smallness of the port-holes, less perfect in the lowest or orlop deck. The only advantage which the upper has over the middle deck consists in its greater elevation.

Crowding.—The patients are always, from the number requiring admission, more crowded than it is perhaps desirable they should be, but, at the time when the disease broke out they were not unusually so. The number of patients on the 13th of October was 181, greater by 27 than at the corresponding time last year, but considerably less than the number frequently on board, and, as we have already seen, they were pretty evenly distributed in the three decks, which do not materially differ in size.

Condition of the Hold.—On the 21st of October, two extra labourers, two or three men from the ship, and the quarter-masters in rotation, in all seven or eight persons, were employed to pump out the bilge water and to clean the hold, a process which occupied them nearly a fortnight. Fires were lighted there, and the hold, as well as the decks, has since been whitewashed. The bilge water was in the condition in which it is usually found; a candle lowered into the well burnt clearly, and nothing was discovered in the condition of the hold that could in any way account for the disease. The boatswain, however, who superintended this process and slept under the forecastle, was attacked in the night of the 23d. All the labourers escaped; their hours of work were from six in the morning to six in the evening.

Predisposing Causes.—The food of the patients had evidently no share in the production of the disease. Of those attacked,

1 was on full diet.

11 were on ordinary or half diet.
2 .... milk diet.
2 .... milk diet with arrow root.
3 .... low diet.

The scale of diet is more liberal than those of the London hospitals, in consideration of the habits of sailors, whose daily consumption of food is much greater than that of the lower classes in the metropolis.

Previous Disease.—Five out of eight cases in the medical ward occurred in persons previously affected with diarrhœa. This was not the case in the other wards. Six of those attacked were in the hospital or slight venereal affections.

Previous condition as to force.—The disease did not attack the debilitated principally. This will be seen by reference to the cases; but it appears also from the circumstance that it was prevalent in the orlop deck, which is tenanted by the most robust patients. But, although the previous condition as to force seems to have had no influence on the frequency of the disease, it had a marked one on the mortality. This was greatest and the most rapidly fatal cases happened in the medical ward, where cholera generally occurred as a complication of some visceral disease, and in patients previously reduced in strength.

Previous Medical Constitution of the Ship.—From a review of all the cases admitted into the medical ward throughout the summer, it appears—

1. That the number of admissions into that ward has been nearly uniform, except in the month of July,

in which it was considerably less than in the other months.

2. That typhoid fever has been almost uniformly prevalent. Most of these patients were remarkably spotted, and the affection was of the type described by Huxham as jail fever. They were all admitted labouring under the disease. In no case did the disease shew itself in persons previously in the ward for other complaints, although this happened to two patients in the surgical wards.

3. That acute affections of the intestines, which were rare in the months of May, June, and July, became frequent in those of August and September. Under this head are comprehended cases of enteritis, or simple fever with diarrhœa, and common diarrhœa; and, for the month of August, four cases of cholera, two of which proved fatal.

The first of the fatal cases occurred in Edward Bowen, 50, negro, who was admitted, in a state of collapse, on the 5th of August, 14 days after his arrival from the Isle of France. He died at 7 in the morning following his admission. No account is preserved of the condition of the intestines.

The second of the fatal cases was that of Peter Johnstone, 61, who was admitted on the 12th of August, 14 days after his arrival from Leith. He was seized in the night of the 10th, and died in the night after his admission. We should be occupying too much space by entering into the details of this case, and shall merely remark that it seems impossible to distinguish it from those that have recently occurred in the Dreadnought, or to consider it essentially different. The symptoms during life—the nature of the evacuations, which were without bile—the appearances after death; but especially the unusual development of the agminated and isolated glands in the ileum, occurring, too, in a person of his age, are sufficient to establish their identity.

The two remaining cases were slight, and the patients did not present the symptoms of *malignant* cholera. The 1st of these was J. Austin, 22, admitted on the 8th of August, 14 days after his arrival from Sierra Leone; the 2nd, J. Aplin, 21, admitted on the 29th of August, 7 days after his arrival from Weymouth.

All these patients were admitted labouring under the disease.

In the month of September no case of cholera occurred, but a case, which, from its resemblance to some noticed by Dr. Roupell, as having preceded the cholera in 1832, is perhaps worthy of being mentioned. It is that of

Simon Sturrock, 46, a remarkably robust man, from Arbroath, and in the port of London since the 7th of September. He was quite well up to the 13th, when, at 1 P. M., he was seized with purging, which was succeeded by vomiting, cramps, urgent thirst. The purging, at first excessive, ceased at 4 A. M. on the 14th, but vomiting, with occasional hiccough, continued to the 20th, the day of his death, when he gradually sank, after passing a considerable quantity of blood by stool; no purging immediately preceded this occurrence. We have omitted, for the sake of brevity, the details of this case, which resembled one of malignant cholera in the violence of some of the abdominal symptoms, and in its fatal issue; but the aspect of the patient, the character of the evacuations, the dark crimson velvety appearance of the mucous membrane of the last foot of the ileum, and of the ascending and transverse colon, noticed after death, and the absence of conspicuous intestinal glands were, we imagine, sufficient to shew that it was of a nature essentially different.

In all former visitations, cholera prevailed in London as well as in the river; in the one that forms the subject of this paper, it existed in an almost isolated manner, in the Dreadnought. About the same time, there are said to have been five or six cases at Limehouse, and we have been informed by Dr. Sims that a few cases occurred in the St. Mary-le-bone Infirmary; but we cannot learn that the disease existed in any other part of the metropolis. It is singular that when cholera first shewed itself in London in 1832, within a few days of its appearance in the river and at Limehouse, and before other parts of London were infected, some cases occurred in Mary-lebone, the part of the metropolis most removed from, and maintaining the least intercourse with, the former places.

It is worthy of remark, that during the summer of 1837, cholera prevailed extensively in Italy; and that, following the law of its first diffusion, it had been marching towards us for several months, appearing in succession at Naples, Rome, and Berlin, before it shewed itself in the port of London.

RECAPITULATION.

1st. The disease, which recently prevailed in the Dreadnought, was identical with cholera, as it appeared in London in 1832; and, although it occurred in an almost isolated manner, and had no power of spreading, the individual cases were as severe as in former and more extensive epidemics.

2d. Cholera may affect persons labouring under various diseases, acute and chronic, and may attack the same person twice or more; circumstances in which it seems to differ from typhus, which rarely occurs in complication, or more than once in the same individual.

3d. The evacuations in cholera, which are ordinarily composed chiefly of the serous portion of the blood, occasionally contain, when the cold stage is protracted, the red particles.

4th. During the prevalence of cholera, diarrhœas, attended in some cases with vomiting, were frequent in the Dreadnought, and probably resulted from the cause that produced cholera.

5th. The recoveries took place chiefly in robust subjects, and in cases comparatively mild; but debility, although rendering the disease more fatal, did not seem to predispose to its attacks.

6th. The dissections in these cases confirm the important observation of Mr. Jackson, with respect to the frequency of pneumonia, and the latent form in which it exists, in cases that prove fatal after reaction.

7th. As no case occurred on board the Iphigenia, moored at the stern of the Dreadnought, or in Greenwich Hospital, or in other vessels in the river, the disease cannot be attributed *solely* to any general atmospheric influences; but no obvious local cause could be assigned for it either in the crowding or ventilation of the Dreadnought, the diet of the patients, the condition of the hold, or the previous medical constitution of the ship.

8th. The shortness of the interval between the occurrence of the 1st case and that of the 2nd and 3rd; the circumstance that persons, recently arrived from various and distant countries, were attacked within a few days after their admission into the Dreadnought; but, especially, the short duration of the epidemics, are unfavourable to the supposition that the disease admits of a long period of incubation. The last circumstance, the short duration of the epidemic, is, we conceive, of great force, as it shews that of all persons attacked during the epidemic, after the first, not one presented a long period of incubation.

9th. The circumstance that none of the nurses or medical attendants of the Dreadnought were attacked; that the disease was not propagated from the ship, although patients were almost daily discharged, who immediately entered other vessels in the river; the order in which the cases occurred, the 1st, in the upper deck, in the night of the 8th, the 2d, in the lowest deck, in the night of the 9th, the 3rd, in the middle deck, in the night of the 10th, and the disappearance of the disease at the end of three weeks, although no measures of seclusion, with respect to these patients, were taken, militate against the idea of its contagious nature.

10th. We can scarcely infer that the disease was brought from abroad, without admitting it to be contagious, and that it had, in the person who brought it, a period of incubation of, at least, 30 days, which is extremely improbable if we consider that this, at any rate, is a circumstance of very rare occurrence, and that only a few cases happened at Dantzic, the port from which this person last sailed.

11th. The history of the disease in Europe, in 1837, presents an epitome of that in 1832; its gradual advance towards this country probably depended on the same or similar influences in both cases.

TABULAR VIEW OF THE CIRCUMSTANCES OF ATTACK.

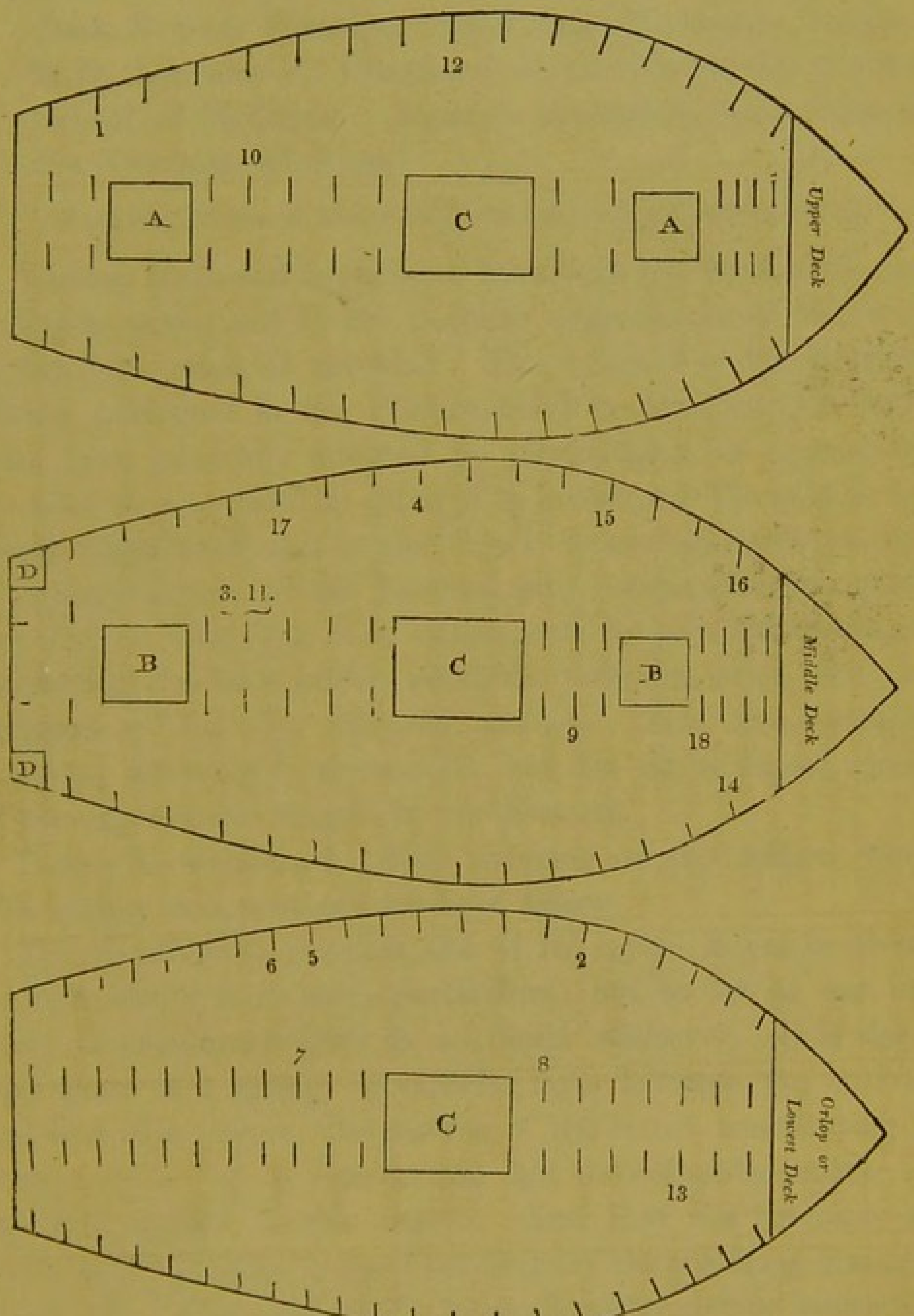
No.	Port last sailed from.	How long in port before admission.	How long in ship bef. attacked.	Date of attack.	Hour of attack.	Deck.	Bed.
1	Dantzic.	Same day.	10 days.	Nov. 8	In the night	Upper.	2
2	Quebec.	14 days.	4—5	10	3 A.M.	Orlop.	134
3	Newcastle.	1 day.	19	10	Evening.	Lower.	87
4	....	Many months	4—5	13	12 P.M.	Lower.	69
5	Dantzic.	10 days.	12	14	Night.	Orlop.	127
6	Richebuctoo.	25 days.	30	15	4 A.M.	Orlop.	126
7	Black Sea.	....	15	15	Evening.	Orlop.	149
8	Honduras.	16 days.	12	15	Evening.	Orlop.	146
9	Bangor.	Same day.	9	15	Night.	Lower.	97
10	Bristol.	3 weeks.	16	15	Night.	Upper.	26
11	Petersburg.	1 day.	17	16	Morning.	Convalescent; transf. to lower	87
12	Malta.	....	54	17	5 A.M.	Upper.	9
13	Petersburg.	6 days.	20	17	Night.	Orlop.	173
14	N. Orleans.	Same day.	7	21	6 A.M.	Lower.	103
15	Calcutta.	———	2—3	21	8 P.M.	Lower.	73
16	Seaham.	7 days.	4—5	22	3 A.M.	Lower.	76
17	Rio Pongo.	14 days.	3	22	5 P.M.	Lower.	66
18	N. Orleans.	Same day.	7	22	......	Lower.	99
19	....	....	12 years.	23	Evening.	Private Cabin.	..
20	Hamburg.	5 days.	5 days.	27	Night.	Orlop.	..

TABULAR VIEW OF THE CASES.

No.	Age.	Disease when attacked.	Condition as to force when attacked.	Diet when attacked	Circumstances of the Cases.	Result.
1	18	Erysipelas from injury.	Confined to his bed.	Low.	Soon fell into a state of collapse, in which he lingered till the time of his death.	Death, 68 hours after attack.
2	25	Venereal sores.	Good.	Ordinary.	Attack sudden; evacuations bilious for many hours; then choleric; cramps very severe; no urine for 43 hours after attack.	Recovery; convalescence rapid.
3	18	Admitted on account of hemetemesis.	Walking about in seeming convalescence.	Ordinary.	Attack sudden; evacuations bilious for many hours; then choleric; no cramps; did not rally from the stage of collapse. Gall bladder found extensively ulcerated, containing pus, but no calculi.	Death, 33 hours after attack.
4	22	Acute rheumatism.	In a state of fever.	Low.	Attack sudden; symptoms severe; early collapse; at the end of 48 hours, reaction established; evacuations again bilious; afterwards, in a semicomatose state, with occasional muttering; pulse 72—80; skin warm; eruption of minute papulæ on the forehead.	Death, 96 hours after attack.
5	21	Venereal sores.	Robust.	Ordinary.	Did not rally from the stage of collapse; symptoms severe; no urine from the time of attack.	Death, 24 hours after attack.
6	18	Trifling.	Good.	Ordinary.	Attack sudden; symptoms severe; at the end of 10 hours, stools contained brownish flocculi; at the end of 34 hours, slight and transient reaction; no urine for 48 hours.	Death, 57 hours after attack.
7	20	Gonorrhœa.	Good.	Ordinary.	Extreme collapse; at the end of 50 hours, purging ceased; vomiting excessive; matters ejected enormous in quantity, and containing blackish flocculi. This continued for 40 hours. At the end of 60 hours, reaction established; afterwards in a semicomatose state; skin warm; pulse 90, full; pupils contracted. Had two fits, one 12, the other 36 hours before death. No urine for 4 days after attack, when ℥vj were drawn off.	Death, 138 hours after attack.
8	31	Venereal sores.	Robust.	Ordinary.	Symptoms severe; great collapse; at the end of 18 hours, slight and transient reaction; flocculi in the stools brownish; at the end of 34 hours, stools scanty, consisting of a thick red fluid.	Death, 45 hours after attack.

No.	Age.	Disease when attacked.	Condition as to force when attacked.	Diet when attacked	Circumstances of the Cases.	Result.
9	15	Diarrhœa.	Very thin & delicate.	Milk.	Soon fell into a state of collapse, in which he lingered till the time of his death.	Death, 45 hours after attack.
10	48	Lacer[d] wound of the hand.	Good.	Ordinary.	Dejections choleric; no vomiting or cramps; collapse slight; reaction early.	Recovery; convalescence rapid.
11	18	Gonorrhœa; rigors, followed by diarr. and fever 3 days bef. attack.	Good before accession of fever.	....	Vomiting and purging, without cramps; evacuations choleric; collapse slight, and of short duration; afterwards, aspect typhoid; tongue, dry and brown; teeth, sordid; skin, hot and dry; occasional delirium; pulse 120.	Death, 8 days after attack.
12	17	Burn.	Good.	Ordinary.	Great collapse; reaction at the end of 14 hours.	Recov.; conv. rapid.
13	19	Venereal sores.	Good.	Ordinary.	Great collapse; cramps severe; reaction after 22 hours; after 24 hours, evacuations brownish from dark flocculi, scanty at the end of 36 hours; evacuations bilious.	Recovery.
14	20	Ague, dysentery	Very feeble.	Milk, arr. rt.	Fell very rapidly into collapse.	Death, 8 hrs. aft. att.
15	32	Chronic rheumatism.	No visceral disease.	Milk.	Previous constipation—attack sudden; urine not entirely suppressed; other symptoms severe; reaction at the end of 36 hours.	Recovery; convalescence uninterrupted.
16	58	Convalesc from jaund. with diarr.	Feeble.	Ordinary.	Urine not quite suppressed; did not rally from the stage of collapse.	Death, 24 hours after attack.
17	20	Diarrhœa.	Good.	Milk.	Symptoms extremely violent; cramps excessively severe; great jactitation; rapid collapse.	Death, 7 hours after attack.
18	19	Ague.	Feeble.	Milk and arrowroot.	Previous diarrhœa for two or three days; no vomiting or cramps; dejections very frequent, and choleric for some hours; slight collapse; afterwards diarrhœa for some days.	Recovery.
19	45	None.	Robust.	....	Previous diarrhœa for 36 hours; cramps severe; complete suppression of urine; other symptoms mild; slight collapse; occasional bilious vomiting, and diarrhœa, with loss of appetite for several months.	Recovery.
20	18	Swelled testicle.	Good.	Ordinary.	Attack sudden; collapse moderate; reaction at end of 20 hours; afterwards occasional diarrhœa and bilious vomiting, for a fortnight.	Recovery.

TABULAR VIEW OF THE DECKS.



A A, B B, the gangways: C C C, the main hatchway, the only entrance into the hold from the orlop deck: D D, two cabins for female nurses.

The short lines represent the fourfold row of beds in the several decks; those to which numerals are affixed, the beds in which the cases respectively occurred.

The beds on the starboard side of the orlop deck were unoccupied during the prevalence of cholera.

G. Woodfall, Printer, Angel Court, Skinner Street, London.

Dr. Percy from his friend the Author.

On the Functions of the Colouring Matter of the Skin in the Dark Races of Mankind.

By Robert Mortimer Glover, M.D., Lecturer on Chemistry in the Newcastle-on-Tyne School of Medicine. (Read to the British Association at the Newcastle Meeting.)

(From the Edinburgh New Philosophical Journal for October 1840.*)*

Various hints and hypotheses have been put forth as to the functions performed by the peculiar organization of the skin in the dark races of mankind. The opinions of Sir Everard Home, published in the Philosophical Transactions for 1821, have been generally adopted by physiologists as apparently founded on a methodical attempt to investigate the subject by direct experiment, and to elucidate it by analogical reasoning. The experiments of Sir Everard give results certainly quite opposed to what has been determined by physical observers respecting the laws which affect the radiation from, and absorption of heat by coloured surfaces. This circumstance drew my attention to the subject, and led me to repeat some of the experiments related by Sir Everard.

It may be mentioned, before entering on the subject, that this inquiry was proposed by Lord Bacon.

The structure of the skin and of its layers is yet involved in some doubt as to many particulars; but so far as our inquiry is concerned there is no doubt whatever. It is clear that there is a spongy or vascular layer between the cuticle and true skin; or on the surface of the latter, constituting a portion of it. It is also certain that the colouring matter of the skin resides in this region. And that the intensity of shade is the greater or less abundance of the colouring matter. Hence the European and the Negro furnish extreme instances in this inquiry; since in the one the colouring matter is in small quantity or of light shade, whereas the other has it so abundantly that in him we speak of the *pigmentum nigrum*. Between these extremes exist many curious varieties, in whom the functions of the colouring matter are well worthy of consideration, but we have data to reason only with regard to the European or White, and the Negro. Indeed, in many of the coloured races, the existence of something analogous to the dark pigment is only inferred, although the occurrence of Albinoes in all races should induce us to believe the presence of a pigment universal. So that what is said of the colouring matter in the Negro may be extended to all varieties of colour, reasoning by analogy.

It is scarcely possible to regard the dark colouring matter otherwise than as a provision for, in some way, enabling those who possess it in abundance to withstand the heat of the climate they inhabit. Accordingly, there are facts which prove such individuals to be more capable of withstanding the heat of torrid regions than acclimatized Europeans, or other whites born there. There are also facts to connect this power of withstanding excessive heat with the development of the dark colouring matter. Thus, Albinoes of Guinea, differing from both Europeans and their countrymen in this, that they totally want the colouring matter, according to many authors, are even less capable of resisting the heat of their native country than European strangers; indeed their skins are said to crack and blister on exposure to the sun's rays. And I am informed by Mr Granidge of Barbadoes, that he has observed the same fact in that island.

Now, when we reflect that the European cannot be without some colouring matter between the true skin and cuticle, since he must differ in this respect from the Albino, it seems as if a relation were established between the development of the pigment, and probably of the rete mucosum along with it, and the power of resisting the sun's heat in torrid regions.

It is clear that in this inquiry we should regard, not merely the physical properties of the organization we consider, nor its vital properties only, but the action and reaction of the whole, and their effect on the system of the individual. For want of a consideration of all circumstances, before the publication of Sir Everard Home's views, it was not conceived how the tint, which, on analogy, should absorb more heat than any other, could, in the hottest regions of the earth, confer any exemption on its possessor. And perhaps before this paper is concluded, it may be apparent that, since Sir Everard published, the matter has been misunderstood.

The notions entertained at present by physiologists, with regard to the operation of this pigment, are implicitly those of Sir Everard. And what they are, will appear from the following brief quotation from an elementary work:—"The secretion on the cutis vera, which gives the black colour to the skin, appears to assist in fitting men for residence in hot climates, because although such skin, by absorbing more caloric, rises to a higher temperature under the sun's rays than white skin does, yet it does not inflame so readily from a rise of temperature." Dr Alison's meaning is, that although the skin of a negro may rise to a higher temperature under the sun's rays than a white skin in the same circumstances, yet the dark skin is less likely to inflame at *that higher temperature* than the white skin at *that lower one*. This, then, is the conclusion of Sir Everard Home, whose paper I now proceed to examine.

The paper of Sir Everard Home contains alleged facts and experiments, tending to prove the Negro more capable of withstanding excessive heat of the sun's rays than the white man, and attributing this to a supposed property in dark surfaces of destroying the scorching and blistering effect of the sun's rays. The former conclusion has already been admitted. The facts by which Sir Everard supports his second position are to be considered.

Sir Everard having fallen asleep on the deck of a vessel exposed to a tropical sun, found, on awaking, his thigh scorched through a pair of *thin* white linen trousers. From this simple observation, the extravagant conclusion is drawn of black being a better protection against the sun's rays than white.

An experiment is next related, in which Sir Everard found, on exposing his hand to the sun's rays for 45 minutes, while a thermometer attached to it stood at 90°, that blisters rose and coagulated lymph was exuded. I have attempted to produce the same effect by the concentrated rays of the sun at the same temperature indicated in a similar way, and kept up to within one or two minutes of the time, when my patience was exhausted, without any result except slight reddening. Six years ago, while off the coast of Algiers, I sat for half an hour immoveable in the sun, having the greater part of my face exposed, the thermometer in the sun's rays being considerably above 100°, and though my face was scorched, nothing like the effect described by Sir Everard took place.

Sir Everard next attempted to compare the inflaming and blistering power of the sun's rays with that of hot water. He says, that water at the temperature of 120° was painful to the body, and became unbearable when still further heated. From this experiment and the preceding, he wishes us to infer a power of vesicating in the sun's rays not in proportion to their temperature.

In a third experiment, he exposed the backs of his hands to the sun with a thermometer on each, the one hand being uncovered, while the other had a covering of black cloth under which the thermometer was placed. After ten minutes, the degree of heat on each was marked, and the state of the surface examined, and this was repeated three times. During the last trial, the thermometer which had its ball covered by the cloth stood at 106°, while the other was at 98°. The exposed hand was scorched, that covered was unaffected in all the trials. I have not repeated this experiment because it is subject to an obvious fallacy, for the ball of the thermometer being between the cloth and the part, a space intervened, and across this space the heat from the cloth could only pass by radiation or by transmission through the thermometer, but not directly from the cloth to the hand, so that the heat might not accumulate on the skin.

In a fourth experiment, a Negro bore the sun's rays on his hand when a thermometer on the part indicated 100° without any scorching being the result. As the scorching of which Sir Everard speaks could be only a slight blush, it might not be observed on a sable skin. However, I do not question the result of this experiment.

Sir Everard observed in his next experiment, during the course of an eclipse, as the darkness on the sun's disk diminished, the scorching power of the rays, concentrated by a lens, increased in a ratio which is assumed to be greater than could be accounted for by the mere rise of temperature during the time of the experiment. Whence it is to be inferred that the excess of effect is due to the increased quantity of light present with the heating rays at each advance of time. A reference to the original paper will convince the reader that this assumption is established without sufficient data.

Most stress has been laid by Sir Everard, and those who have adopted his views, on the seventh experiment. We are told that, on the 9th of September, at 11 A. M., thermometer 90° in the sun, the concentrated rays applied to a piece of black kerseymere wrapped round the arm, gave no real pain, as it is expressed, during 15 minutes; and at the end of that time left no appearance on the arm; whereas, when white kerseymere was substituted, during the same time, and the concentration we are led to suppose being the same, the heat of a thermometer in the sun only 86°, yet blisters were formed. From this experiment, taken along with those preceding, it is supposed to be fully proved that although black surfaces rise to a higher temperature than white under the sun's rays, yet they scorch the surface of the body less; the scorching effect depending on a union of the rays of heat with those of light, the latter being supposed, by way of explanation, to be excluded by the black surface. First, I shall state my repetition of the experiment, and then attend to Sir Everard's explanation of his supposed fact.

I have attempted to ascertain the rise which the absorption of heat by black and white cloths respectively gives to the thermometer; to compare this observation with the effects of the same cloths under the sun's rays upon the body, and with the effect of the sun's rays on the naked skin. When the thermometer stands at about 80° in the sun, the solar rays concentrated on white cloth over the ball of a thermometer, to a space of an inch and a half in diameter by a burning-glass, caused a rise of the thermometer to 125° in a quarter of an hour. When black cloth was substituted the rise during the same period was to 172°. In five minutes, with the white, the rise was to 108°, with the black to 140°; and in some experiments in a proportion nearer that given by the longer period.

When the black and white cloths were applied to the skin at the same temperature, and with the same degree of concentration, as already mentioned, the black cloth generally caused intense pain in the course of a few minutes, and on being allowed to remain for five or at most seven minutes, produced blisters. During the same period very little apparent effect followed the application of the white cloth, though considerable pain was sometimes produced. The experiment was at different times performed on several individuals, all of whom found the black cloth give the sensation of pain sooner than the white. On the whole, I found nothing like the difference described by Sir Everard, though certainly the vesicating effect of the black surface appeared to be much greater than that of the white. From many experiments I conclude, that the rays of the sun will scorch when they are applied to the surface so as to cause a heat of about 130° and upwards. And from the experiment related by Sir Everard, it appears that hot water is capable of producing a similar effect at that temperature. From all this, I am inclined to deny the existence of a scorching power in the sun's rays, independent of the heat they contain, or at least of the effect they produce on the thermometer. Moreover, if such a power do exist, black cloth should yet scorch more than white, since it will absorb all the rays of light, whereas the other surface will reflect them.

In those experiments which I performed, care was taken to have the white and black cloth nearly of the same density. Sir Everard does not appear satisfied with his explanation of the extraordinary *fact* he relates, for he gives another furnished by Davy, who, indeed, is made to ascribe the alleged difference in vesicating power between black and white surfaces, to the former rendering the heat *sensible*. Were I not quoting from the Philosophical Transactions, a misprint might be suspected. I conclude that a black skin will absorb more heat than a white skin, and were it not for other accompanying circumstances, would produce inconvenience precisely in the ratio of the amount of heat absorbed. It must not be overlooked, however, that in the Negro the pigment is not superficial, but covered by a layer of translucent cuticle. The experiments of Dr Stark prove that colours absorb heat in proportion to their depth of shade through transparent media. It only remains to shew the cuticle to be a medium in the condition of those. For this purpose, I covered the balls of a differential thermometer, one with cuticle, the other with cuticle of the same thickness, having ivory black rubbed on its inner surface; on bringing the thermometer into the sun's rays, the column of liquid descended rapidly in the stem, the ball of which was covered with the blackened cuticle.

It is evident, from the result of experiments which I have related, that a much less degree of heat can be borne when the heat is applied locally, or so that the perspiratory process is not excited over the whole system, than Sir Joseph Banks and others were able to bear in heated apartments where perspiration was fully excited.

This circumstance leads me to offer an explanation of the functions, or, not to speak mincingly, of the *uses* served by the peculiar colouring matter in the dark races. Blumenbach and Dr Winterbottom concur in stating the Negro to perspire more readily than the European or White, and Dr John Davy, in the 3d vol. of the Medico-Chirurgical Transactions, gives its due influence to this property. After noticing that the excessive perspiration in dark people must keep down the temperature, he proceeds, "In the inhabitants of the tropics, the exhalant arteries of the skin seem unusually expanded, and the whole apparatus peculiar to this secretion unusually developed; and I believe that the blood itself is less viscid, more fluid, and flows more readily through the vessels, so as to promote perspiration, and by that means contributing to the cooling of the surface, and being cooled itself, it contributes again when it flows back upon the heart, to the reduction of the temperature of the internal parts."

Were the inhabitant of the tropic not possessed of this organization, his system could not respond to the stimulus of heat, by a determination of fluid to the surface of the body. And the heat absorbed by the skin being prevented from entering the system by the perspiratory process, the greater radiating power of a dark skin must be beneficial in cooling.

Again, the dark skin places the Negro in the conditions of his climate by causing him to radiate heat at night, and become at that time cooler than a White in the same circumstances. This is a fact which has been observed of the Negroes. Their propensity for exercise in the open air at night has been remarked. Thus we read that when the fleet of Hanno approached the shores of Negroland, the country which, during the day, presented only silent woods without the least trace of man, at night was lighted up with immense fires, while the woods resounded with the sounds of festivity. In a climate where, during the day, vegetation appears burnt up, the earth is cracked by the heat, and all living creatures languish; but where at night breezes refresh the air, and cheer exhausted nature, plants run with dew, and animals leave their haunts, man also, fitted by the structure of his skin to throw off heat, issues forth animated by the irresistible propensity to exercise which is always given by the bracing air of colder climates.

ON THE

DIFFERENCES OF THE LAWS

REGULATING

VITAL AND PHYSICAL PHENOMENA.

BY WILLIAM B. CARPENTER, M. R. C. S.

Late President of the Royal Medical and Royal Physical Societies, Edinburgh.*

From the Edinburgh New Philosophical Journal for April 1838.

1. All the knowledge which man possesses of the external world is derived from the impressions made by matter, under some of its forms, upon his organs of sense, which excite sensations, perceptions, and reasoning processes in his mind.

2. Of *matter*, in the abstract, man has no definite idea; he judges of it only by its properties; and as his notion of these properties results simply from the changes produced in his organs of sensation, the term "properties of matter" expresses nothing else than the relations between matter and the percipient mind. And just as the occasional varieties in the conformation of the organs of the senses lead to a different appreciation of certain of these properties (*e. g.* as regards colour or sound) in certain minds, so is it possible to conceive that beings might be created whose organs should take cognisance of a set

* The following paper constitutes a portion of the essay to which was recently adjudged the prize annually raised by the contributions of the students, and awarded by the Professors, of the University of Edinburgh. Circumstances, which it is unnecessary to specify, have rendered it desirable not to publish, in its present form, the division of the essay comprehended in § 9–20; an abstract of which has been introduced, however, for the purpose of shewing the course of the argument.

of properties appertaining to matter, of which man, as at present formed, has no conception.

3. The *properties of matter* may be classed in various ways;* some are *universally* possessed by all matter which comes under our cognisance; and our most definite notion of matter is therefore compounded of the ideas which they excite: even these, however, do not lead us to the knowledge of the entity or substance to which they belong; and it is generally allowed that "we are entirely ignorant of the real constitution of matter, the cause from which the most universal and essential of its properties arise." Besides these universal properties, however, there are others *partially* distributed among material bodies, of whose cause we are equally ignorant, but whose varieties (mostly recognisable by the organs of special sense) lead to all our knowledge of the particular forms and qualities of matter.†

4. Our ideas of the external or evident characters of material bodies, are simply derived from the changes produced by them (whilst themselves inactive) upon the organs of sense. It is probable that these primary impressions are of a material nature; and that the human mind, in the present stage of its existence, cannot take cognisance of any of the properties of matter, except by such material changes propagated from the extremities of the nervous system to the central sensorium.‡ If this be

* Of the ancient metaphysical distribution of the properties of matter into the *primary* or *essential*, and the *secondary* or *accidental*, it is scarcely necessary here to treat, since this distribution was not founded on the respective *universality* or *partiality* of these properties, but on their supposed connection with what was regarded as the fundamental or primary character of matter, viz. its occupation of space. (See Adam Smith on the External Senses, and Prichard on the Vital Principle, p. 43.)

† The sense of Touch, considered in its simple and passive state, leads only to the idea of *resistance*; and hence to the knowledge of the universal properties of matter. When actively employed in combination with other more special sensations, it leads to the appreciation of those external forms and characters of matter which are liable to constant variation.

‡ "The impressions excited in us by external objects are the results of certain actions and processes, in which sensible objects and the material parts of ourselves are directly concerned." Herschel's Preliminary Discourse, p. 118.

the case, what is true of a great number of instances may be stated as a general proposition,—that none of the properties of a material body can be recognised by the mind, except when that body is placed in relation with some other, and an action of some kind occurs between them. That this is true of many properties, as that of gravitation, is self-evident. If but one mass of matter existed in the universe, it might be endowed with all the properties which we are accustomed to regard as essential to matter; and yet, from the property of gravitation never being brought into action, the mind would remain ignorant of it.

The same may be said of electricity, which is probably to be regarded not as a distinct entity, but as a universal property of matter.

In these latter cases, the body itself must undergo a change in state or situation, in order that its occult properties may be manifested, and until it has been placed in circumstances by which the existence of such properties can be tested, we have no means of judging of their presence or absence.

Thus, supposing a new metal to be discovered, we should not be able to say *a priori* whether or not it had magnetic properties; we could only determine this by experiment. Further, until our experiments have been varied in every conceivable manner, we have no right to judge of the absence of such a property; thus Faraday has shewn that it is by no means impossible that all metals are possessed of magnetic properties, within a limited range of temperature.

5. Still, therefore, keeping in view the statement with which we set out, that the term "property of matter" expresses no more than a certain relation between matter and mind, we may regard it as implying a capability of producing or of undergoing a change when the body is placed in conditions adapted to manifest it.* If this change be immediately produced on any of the organs of sense, when the body is brought into relation with it, the property may be regarded as of an *evident* nature; if an action with some other body be required to manifest the property of the senses, the property may be designated as *occult*. This distinction though not perhaps metaphysically correct,

* "If all the objects in nature remained constantly at rest, it is very evident that we could have no notion of any property of matter whatever." Brown's Lectures, p. 26.

will be found useful for practical purposes, and will be frequently referred to in subsequent observations.

6. The manifestation of the properties of matter, in the actions which bodies, placed in certain relations to one another, present to our observation, gives rise to our idea of *power*. Thus, when two bodies possessed of the property of gravitation are placed within the sphere of each other's attraction, the *power of gravity* is developed, and manifested by their approach to one another. Or when a piece of iron is brought near a magnet, the electric equilibrium of the former is disturbed, and the attraction caused by the opposite electric state of the neighbouring parts of the two bodies gives rise to their tendency to approximate ; or in common language, the *magnetic power* causes their motion towards each other. Of the abstract nature of any *power*, as of matter itself, we know nothing ; it is only recognisable by its effects ; but it is, as we have seen, to be ultimately referred to the *properties* of matter, which may be regarded as axioms or postulates in any course of reasoning founded upon them. Of power as a *cause* of *effects*, it is generally acknowledged that we judge of it only by its invariable connection with subsequent phenomena.

7. To the term *principle* no very definite meaning can be attached. It has been remarked that " this word, characteristic of a less advanced state of science, has been generally employed (as the final letters of the alphabet are used by algebraists) to denote an unknown element, which, when thus expressed, is more conveniently analysed."* Thus we speak of the principle of electricity, or of the principle of magnetism, as the unknown causes of certain phenomena that are as yet imperfectly understood. When the laws of these phenomena, however, are perfectly ascertained, they terminate in referring all of them to simple properties of matter, from which they may be deduced by demonstrative reasoning, just as all geometrical theorems are founded on the axioms first assumed. It has been in the science of life that the term Principle has been most used, and most abused ; the conditions of vital phenomena are not yet determined with sufficient precision to enable us to refer all ob-

* Mayo's Physiology, p. 2.

served facts through the medium of general laws to simple vital properties; and the term *vital principle* has been used as a convenient expression for the sum of the unknown *powers*, which are developed by these properties in action. This at least is its true signification among those who have not regarded it as a distinct *intelligent agent.* But when these vital properties shall have been so far unveiled, and their laws so fully ascertained that we can reason deductively from them, then the term Vital Principle will become wholly useless, except as a comprehensive expression of the *sum* of the properties of living organized matter not possessed by bodies in general. But perhaps it may be speculated without improbability that the *vital* and *physical* properties of matter may ultimately be shewn to result from some higher, more general quality; an advance in the path of philosophy, should it ever be proved, far beyond any which has been already attained, or which we have in immediate prospect.

8. The term *law*, which has been already more than once employed, expresses the *conditions of action* of the properties of matter. The Divine Creator of the universe "has, by creating his materials, endued with certain fixed qualities and powers, impressed them in their origin with the *spirit*, not the *letter*, of his law, and made all their subsequent combinations and relations inevitable consequences of this first impression."* In our study of the phenomena of nature, it is our object to ascertain their laws by the inquiry into the conditions under which the occurrences present themselves; and a *law* deduced from this source is nothing more than a general expression of the conditions common to a certain class of phenomena, leading us to the belief, that, under the same conditions, the same phenomena will constantly occur. When this is found to be the case by the experimental application of the law to unknown cases, the law is said to be verified, and it may then fairly rank as a general fact to be included with others of like standing in a still higher expression of the conditions common to all these, and therefore to all the particular instances included in them. By successive generalizations of this nature, we aim to ascend

* Herschel's Preliminary Discourse, p. 37.

from the most complicated and restricted to the most simple and universal statement of the phenomena of the universe; and in so far as this is attained in every science, giving us the means not only of explaining new phenomena as they arise, but of predicting otherwise unexpected occurrences, *that* science may be regarded as perfect.

Summary of Sections 9–20.

The object of these was to shew that there is nothing essentially different in the *character* of the laws regulating vital and physical phenomena, either as to their comprehensiveness, their uniformity of action, or the mode in which they are to be established by the generalization of particular facts. Matter, in that form usually denominated *inorganic*, is possessed of *properties* which render it capable of performing a great variety of *actions*, and of developing many *powers;* and the nature of the properties determines the conditions of action, or, in other words, the *laws of physical phenomena.* According to our means of observing the actions dependent upon the different properties in an insulated form (whether that insulation be natural or artificially contrived) are we able to arrive at the fullest knowledge of these properties; and where the combinations, under which natural phenomena so frequently occur, are placed (as in meteorological science) beyond our power of analysing, although we may, through the assistance afforded by collateral sciences, determine most or all of the laws in operation, we are unable to apply these laws to the complete explanation of individual phenomena, or to the prediction of unexpected occurrences, owing to our ignorance of the mode in which they are combined, and the relative parts they play in the production of the effects in question.

On the other hand, *living organized matter* is possessed of properties (apparently superadded to those of inorganic matter) which render it capable of performing a great variety of *actions*, and of giving rise to many *powers;* and the nature of the properties determines the conditions of action, or, in other words, the *laws of vital phenomena.* The peculiar difficulties which beset the investigation of these laws have greatly retarded our acquaintance with them, and, until a comparatively recent period, led to the belief that the inductive process was not applicable to their elucidation. It is obvious that these difficulties are in many respects similar to those which present themselves in the science of meteorology; but we have not in the science of life the same assistance which meteorology has derived from the collateral branches of philosophy, since to whatever degree the physical properties of matter are concerned in the actions of life, it would be absurd in the present state of our knowledge to deny the existence of a class of *vital* properties essentially distinct from these, and never manifested but by matter in that form which is denominated *organized*, and which the skill of man can never hope to produce or imitate.

21. When we compare the constant changes which we encounter in living organized beings, with the inert state of inorganic matter, we are compelled to conclude, that to whatever

extent the forces which control the latter contribute to the action's going on in the former, there must be additional forces resulting from the operation of properties, to which we know nothing analogous elsewhere. The degree, however, in which these superadded properties harmonize or interfere with those common to other forms of matter, constitutes a fair and highly interesting branch of inquiry; and it is a question of a still higher nature, whether the existence of *vital* properties is to be regarded as the *effect* or the *cause* of the peculiar material constitution of the tissues which exhibit them? Each of these questions we shall now examine in some detail.

It would be useless to recapitulate the various opinions which have been at different times advanced regarding either of these subjects; different writers have espoused the opposite extremes, some maintaining that the actions performed by living beings are purely of a physical nature; others, that physical powers are not concerned in them; and others have even presumed upon the existence of a distinct intelligence, "Divinæ particula auræ," presiding over the affairs of each organism. The truth appears, with regard to this, in common with so many disputed questions, to be in the mean between the opposing extremes.

22. The actions performed by living beings, the sum of which constitutes their life, may be divided into three classes. In the *first* division may be included all those which are of a purely *physical* character, depending solely upon the common properties of matter, and performed by dead as well as by living organized substances, as long as no obvious change has taken place in their composition. The *third* division will comprehend all those changes which result strictly from the *vital* properties of the living tissues, and which, therefore, entirely cease at the moment of death. The *second* or intermediate division includes all the changes in which *physical* laws may be regarded as the immediate agents, but in which the conditions of their operation are determined by the action of vital properties. It will, we think, be found that all the changes by which the organism is immediately connected with the external world (whether those changes are a part of the *organic* or *animal* functions) belong to the *first* class; that the actions concerned in the conversion of the crude aliment, introduced from without into *organizable* materials, which may serve as the pabulum of the various tissues, are to be referred to the *second* class; whilst the act of *organization*, and the endowment of the newly

formed tissues with vital properties, evidently belong to the *third* class; as do also most of the changes in which the functions of *relation* consist. Setting out with these general views, we shall endeavour to apply them in detail to the analysis of some of the functions; and as it is obviously advantageous to select for illustration the instances in which these functions present themselves in their simplest form, we shall commence with the study of vegetable life.

23. The properties of the elementary tissues of plants may be separated into *vital* and *physical*, with some degree of definiteness; since these tissues are frequently but little prone to spontaneous decomposition after the loss of their vitality. Hence we are easily able to satisfy ourselves that the properties of *elasticity*, *hygroscopicity*, and that peculiar modification of the latter which produces *endosmose*, are evidently the result of the mechanical arrangement of the particles of the tissues which exhibit them; since they are not dependent upon vitality for their existence, and may be imitated by artificial combinations of inorganic matter.* On the other hand, these tissues are also possessed, in their living state, of properties which, when called into action by stimuli fitted to excite them, maintain those changes which essentially constitute the *life* of the plant, and without which the mere physical properties would be inert.

24. The first and most general of the actions of a vegetable organism consists in the absorption of nutrient materials from without; this absorption takes place during every period of the life of the plant, from the first appearance of the embryo within the ovule, to the final decay of the structure. Now, there would seem strong reason to believe that the mere act of absorption is due to the physical property already referred to, as the capability of producing endosmose. The succulent extremities of the spongioles in the perfect plant, or the tegumentary membrane of the rootless cryptogamia, serve as the medium required for the process; but it is important to keep in view,

* Thin laminæ of baked pipe-clay are found to be excellent media for the production of endosmose; and porous limestones possesss a similar property in an inferior degree.—Dutrochet's Memoires, Anatomiques et Physiologiques, vol. i. p. 23, &c.

that, for its commencement and continuance, a constant difference is necessary in the density of the fluids on the two sides of the septum. This difference appears to be produced in the living plant by the admixture of *elaborated* or descending, with the *crude* or ascending sap; and in consequence, a portion of the former is excreted from the roots by the action of exosmose. The cessation of this action of admixture (a change obviously dependent upon vital actions) at the death of the plant, fully accounts for the non-continuance of endosmose, which might also be occasioned by the facility with which the delicate tissue of the spongioles undergoes disorganizing changes. It is also evident that the continuance of the vital processes going on in the leaves is necessary for the removal of the imbibed fluid as fast as it is absorbed by the spongioles, just as the combustion of the oil at the apex of the wick is necessary for its continued rise by capillary action beneath;* and it is now acknowledged that the motion of the sap in spring commences at the extremities of the branches, owing to the vital actions which result from the stimulus of heat and light, the endosmose not occurring at the roots until the superincumbent column of fluid has been drawn off. If these views be correct (and they are not negatived by the few instances of *selective* absorption which occur in plants, as these are readily explicable on this theory (*Edin. Med. and Surg. Journ.* xlviii. p. 24), it follows that we are to regard *absorption* as a change resulting primarily from the *physical* properties of the tissue, but dependent for its continuance upon actions of a *vital* nature in other parts of the organism.

25. In the early stage of vegetable existence, before the evolution of the organs, whose object is to convert the crude aliment into organizable materials, the embryo is supplied with nourishment already prepared by the parent. When, however, the plant has arrived at its full growth, its aliment is derived from the simplest materials, viz. water, carbon, and, in a few instances, azote; and it is in the conversion of these into the organic compounds which are found in the nutritious juices of plants, that we may recognise the action of physical laws,

* Henslow's Botany, p. 179.

under those peculiar conditions which a living organism alone can supply. In so far as the skill of the chemist can imitate those conditions, he may hope to produce similar combinations, and this has to a certain extent been already effected; but even those who believe that the existence of vital properties is the result of the organization of the tissue which manifests them, must despair of themselves effecting that process, which seems to be entirely dependent on the previous existence of some other organizing body. The three great steps in the function of assimilation (*first*, the absorption of aliment; *second*, its conversion into organizable materials or proximate principles; *third*, its organization and vitalization) present us with illustrations of the three classes of actions which we assumed as expressive of the mode in which the changes constituting life may be most simply and philosophically distributed. But the opinion here given with regard to the formation of organic compounds, which we have referred to the action of chemical affinity (believing the peculiarity of the results to be due to the mode in which the elements are presented to each other, rather than to any thing essentially different in the bond of union), is opposed to that of many eminent physiologists of the present day; and as it may be regarded as one of the most important questions in physiology, and one which the weight of the facts and arguments on either side renders peculiarly open to discussion, we shall devote some space to the consideration of it.

26. The question may be advantageously stated in its broadest form,—"Are the affinities which hold together the elements of living bodies, and govern the elaboration of *organic products*, similar to, or distinct from, those which preside over inorganic compounds?" On this question we find the greatest number of modern physiologists opposed in belief to the chemists of the present day; the former maintaining the existence of a distinct set of *vital affinities*, sometimes, and indeed generally, opposing the usual action of chemical laws; the latter believing, that, however at first sight the actions of vital chemistry may appear inexplicable by the known laws of affinity, they are to be ultimately explained on the same general principles. The physiologist points to the evident truth, that the tissues and fluids which maintain a certain composition whilst

possessed of vitality, speedily resolve themselves into new combinations, a fact so obvious that it has given rise to the well known definition of *life*, that is, the power by which decomposition is resisted. Hence it is inferred, that the affinities which hold together the elements during life are of a different nature from those which operate in producing their subsequent separation. Now, it may be replied to this argument, that no solid or fluid compounds which have a disposition to spontaneous decay after death, can continue to exist without change during life; that the activity of the processes of interstitial absorption and reposition seems to bear a pretty constant ratio in every case with the natural tendency to decomposition; and that the maintenance of the original combination is not so much owing to any thing peculiar in its *vital affinities*, as to the constant provision for the removal of particles in a state of incipient decay, and their replacement by others freshly united,—processes which are obviously dependent on vital actions for their continuance. Thus we find that all the most permanent parts of the animal frame, such as the massive skeletons of the polypifera, the calcareous tegument of the mollusca, the bony scales of fishes, all of which are believed by geologists to have remained almost unchanged for thousands of centuries, are completely extravascular in the living animal, undergoing no interstitial change when once formed; next to these in order of durability are the osseous structures of animals and the woody fibre of vegetables, whose connection with the circulating system seems rather adapted to meet the exigencies of growth, injury, or disease, than to maintain a constant change required by the tendency to decomposition. When we examine the softer tissues, on the other hand, we find that the rapidity of interstitial change fully compensates for the increased tendency to decay; and that if, from any cause, this change be prevented, decomposition and *consequent* loss of vital properties ensue, as in the case of spontaneous gangrene from obstructed circulation. It is interesting to remark also, that the liberation of carbonic acid, which, when it begins to take place after death, is one of the first signs of putrefaction, is the most constant and necessary excretion of the body during life. It might also be ragued, that, independently of the changes already adverted to, by which the normal composition of the organic constituents is maintained, the circumstances under which these compounds exist in the living body differ in so many particulars from those of dead matter, that no conclusion could fairly be raised upon the fact of their spontaneous decomposition after death; since inorganic chemistry affords us numberless examples of such changes occurring under the influence of very slight variations in temperature, electricity, light, &c. Moreover, many proximate principles, when reduced by chemical means to their simplest form, are as permanent as the greater part of the inorganic compounds; and it is difficult to suppose that they retain their vital affinities after being subjected to chemical action.

Thus starch and gum may be converted into sugar by the operation of various acids; and sugar at moderate temperatures is as little liable to change as any saline compound.

27. It has been said by the physiologist, that the processes of vital chemistry are "not only inexplicable by, but manifestly opposed to, the known action of chemical and physical laws;" and it is therefore presumed that a distinct set of forces must be in operation, by which these changes are effected; which forces have received the name of vital affinities. This presumption carries with it a specious probability, and is not easy to be refuted by positive statements; we shall examine, however, whether the real probability does not support a contrary view. t may be well to repeat that the question we are now arguing relates only to the formation of *organic products*, such as gum, sugar, albumen, gelatine, &c., which are destined to be further organized—or such as urea, cholesterine, &c. which are to be thrown off from the system, and not to those which, like starch or fat, already present traces of structure, and have therefore partaken of those influences which we consider essentially vital.

In the first place, then, we may remark that it is rather premature to assert that any such changes in constitution are contrary to the known laws of chemical affinity; for no one who has watched the progress of science during the last few years, can hesitate in the belief, that we as yet know but little of those laws, compared with what future discoveries will reveal to us. The same assertion might have been made within a very recent period, with regard to many of those phenomena which we can now readily explain. Would it have been thought possible, for instance, by a chemist thirty years ago, that the same substance should act the part of an acid in one case, and a base in another?—that *water* should be possessed of such properties?—or still more that hydrochloric *acid* in combination with chloride of platinum should act as the *base* or electro-positive ingredient? Yet such are the facts. These would have appeared to a chemist at the commencement of the present century, totally inconsistent with what he knew of chemical action; but they are now readily comprehensible under laws which include all the facts hitherto ascertained. Or, to take a different illustration, would any electrician, twenty years ago, have supposed it consistent with physical laws, that a mechanical force, 50,000 times greater than that of gravity, may be instantaneously generated by the action of galvanism on a metallic alloy (as shewn by Sir J. Herschel), or that a feeble current of electricity, issuing from a single pair of plates, may generate (if properly applied) a magnetic force capable of sustaining many hundred pounds. The higher and more general are the laws we attain, the more do we find that they include facts, which at first sight appeared inconsistent with them; and unless a *distinct* set of *laws* could be established, regulating vital affinities, which has not yet been accomplished or even attempted, we are scarcely justified in assuming that these laws may not be accordant with those which we recognise elsewhere.

And this leads us to remark, in the second place, that whilst recent discoveries in inorganic chemistry have tended to widen very much our notion of the play of *chemical* affinities, the application of accurate and minute analysis to what have hitherto been considered the proximate principles derived from the organic world, has shewn that many, if not all of them, may be considered as binary combinations of other substances much simpler in their composition, and every fresh discovery is therefore tending to break down the barrier between the two classes of organic and inorganic bodies, as far as regards their method of chemical combination.*

* Turner's Chemistry, p. 782, Prout's Bridgewater Treatise, p. 418, &c.

The chief ground for the assumption of a distinct set of vital affinities, appears to be, that whilst man has the power of effecting or controlling those changes which are produced by physical laws (so far as the materials concerned in them are within his reach), and can therefore imitate, to a great extent, the immense variety of combinations which the mineral kingdom affords,* he is at present unable to effect or control the action of similar materials, so as to produce any of the class of substances usually termed organic. Every fresh discovery, however, tends to prove that the powers *immediately* concerned, are, like the elements on which they act, the same in all cases; the difference in the effects produced being owing, not to any alteration in the physical properties of the constituents, but solely to the manner in which they are brought to bear upon one another. We cannot yet succeed in producing artificially any organic compound, even of the simplest kind, by directly combining its elements, because we cannot bring them together precisely in the requisite states and proportions; we do not see why it should be doubted; that if the elements could be so brought together by the hand of man, the result would be the same as the natural compound. For, as Dr Prout justly remarks, "the organic agent does not change the *properties* of the elements, but simply combines them in *modes* which we cannot imitate." If we once admit the doctrine of the substitution of vital for chemical affinity as the *immediate* agent in the production of organic compounds; we do not see how the conclusion can be avoided, that *all* the changes which take place in the living system, whether animal or vegetable, are to be referred to the same source, and that, in fact, chemical affinity has nothing to do with them—a conclusion to which we imagine that few would assent. Those who are acquainted with the influence of temperature, electricity, light, the *form* of the body operated on, and the *state* in which it is presented for combination—on chemical actions, must be well aware how greatly the effects

* In this point of view the recent experiments of Mr Crosse and others on artificial minerals, are highly interesting; no chemical laws previously known were adequate to account for the formation of many common mineral combinations.

are modified by slight differences in the conditions; and it scarcely seems too much to assume that, ignorant as we are of the nature of these conditions in the living organism, the acknowledged differences in the results may be dependent upon their variations; perhaps, also, other conditions whose nature is yet unsuspected may also have a share in their production.

Having now endeavoured to shew, by negative arguments, that we are not justified in assuming that the union of elements in organic compounds is effected by a novel kind of affinity, unknown in the inorganic world, we shall next briefly inquire what *positive* reason we have for believing that chemical affinity is the agent concerned in the formation of organic products. It would be endless to enumerate *all* the actions which we might, without passing beyond the limits of our absolute knowledge, adduce in support of our argument; but we may notice a few of the most remarkable. Many of the proximate principles of plants are mutually convertible; thus gum may be changed to sugar; and starch, which is a semi-organized form of gum, may be made to undergo a similar transformation, by the action of heat and sulphuric acid.

This is one of the remarkable instances of the catalytic action described by Berzelius,* which is common to organic and inorganic chemistry, and which is not yet comprehensible in the known laws of chemical affinity; the peculiarity of the action consists in the change produced by one compound upon another, without itself undergoing any alteration. Thus, in the present instance, the quantity of sulphuric acid undergoes no diminution, nor does it form any new combination.

Now, there are two distinct actions in this change, the rupture of the vesicles causing the liberation of the contained gum, and the conversion of the gum into sugar; the former is effected by the heat, the latter by the heat and acid combined. The process is one of the most common in the vegetable economy, being performed whenever an embryo begins to be developed, and makes use of the store of nourishment previously prepared by the parent, as in germination; but in the plant it is accomplished in a manner somewhat different. A secretion which has been termed *diastase* is brought to act on the fecula, and

* Edinburgh Philosophical Journal, vol. xxi.

effects its conversion into sugar, just as the chemist may do by heat and sulphuric acid: this diastase is previously stored up in contact with the embryo, and when the vitality of the latter is excited by its appropriate stimuli, it is carried into the substance of the albumen. Diastase may be easily obtained from the tuber of the potato, where it surrounds the buds; and when the chemist employs it as a reagent, its effect on starch corresponds with that which it produces in the living plant, and with that of the purely chemical operation of sulphuric acid. Another example is afforded by the function of respiration,* which, although not yet perfectly elucidated, seems almost entirely under the control of physical and chemical laws. The object of this function is the excretion of superfluous carbon from the system, in a manner calculated to give rise to the development of heat. We shall not enter into the question at present most keenly debated, with regard to the form in which carbon exists in venous blood. This appears to have been recently decided by the experiments of Magnus; but there can be no question that, when that fluid is exposed to the air through the medium of a thin membrane, an interchange of ingredients takes place, carbonic acid being removed and oxygen absorbed.

The removal of carbonic acid is proved by the experiments of Edwards on respiration in hydrogen; the absorption of oxygen (which cannot be detected *free* in arterial blood) by the predominance which often occurs of the quantity of oxygen which has disappeared over that of the carbonic acid which replaces it.

A constant absorption and exhalation of nitrogen also appears to take place. Now, all these facts are explicable upon the known laws of the mutual diffusion of gases. The most evident of these changes take place after the removal of blood from the animal, if it be exposed either directly or through a membranous septum to the contact of oxygen; and we can scarcely, therefore, view them in any other light than as chemical. But these changes, like others which result immediately from the relations between organized beings and the external world, although themselves resulting from physical laws, require for their continuance the assistance of actions dependent upon vital

* We use the term in its most general sense, denoting the interchange of ingredients between the air and circulating fluid.

properties, and hence cease soon after general or somatic death, although they may continue so long as the necessary conditions can be artificially maintained. It has been considered by many writers that the excretion of carbon in respiration is due to a secreting action in the lungs; this we do not mean to deny, particularly as the elementary structure of the lungs corresponds precisely with that which is now known to be common to all secreting organs; but we think that from the function of respiration, which is essentially a chemical process, an argument may be drawn respecting the operation of chemical principles in the formation of other secretions, although their nature is at present so obscure. There can be no doubt, however, that they are modified by the electric state of the organs by which they are respectively formed; and this fact leads us to another very interesting series of details, from which it would appear that electricity is a most important, though little recognised agent, in the operations of organic chemistry.

The late researches of Dr Faraday have fully proved the identity of electrical with chemical affinity, and that all chemical changes are attended with a disturbance of electric equilibrium. If, therefore, the changes occasioned by the growth of organized systems are immediately governed by laws similar to those of inorganic matter, we should expect to find that electricity is constantly being developed by them, in the same manner as we artificially obtain it by chemical decomposition or recomposition.*

There is no deficiency of evidence that this is the case. During the germination of seeds, the two principal changes are the rejection of carbonic and acetic acids; and it has been recently ascertained that there is at the same time a manifesta-

* M. Becquerel has described a most simple apparatus for the development of electricity, consisting merely of a syphon filled with fine sand, and having one leg filled with an acid, the other with an alkaline solution. These fluids meet at the most depending part of the tube, where there is an orifice plugged by a bit of asbestos, which conveys away the compound solution as fast as formed. Wires placed in the two legs indicate strongly opposed electrical states, and the voltaic current thus produced, continues until all the fluid elements have been united. It is impossible to consider this result without acknowledging the remarkable influence which *capillarity* must have over chemical action, a condition so evident and constant in organized beings.

tion of electric action. The seed may indeed be considered an electro-negative system, retaining the bases and rejecting the acids; and it has been accordingly found that grains applied to the negative extremity of a voltaic pile, germinate much more rapidly than those uninfluenced; and that positive electrical influence retards the process. In like manner slightly alkaline solutions accelerate, and acids delay or altogether check it. In the later periods of vegetable growth, the contemporaneous chemical changes are by no means uniform in character; and it is probably from this cause that artificial currents of electricity do not seem to assist the growth of plants, although atmospheric electricity, which is undoubtedly much connected with the processes of vegetation, appears to accelerate it. That there is constant electric disturbances during the growth of plants, has been fully proved by the experiments of Pouillet; and by many writers, the changes produced by the exhalation of fluid, and the gaseous alterations effected by the leaves, are believed to be the main source of the constant variations in the electric state of the atmosphere.

The connection of *capillarity* with electric action has been already noticed; but some other facts may be briefly stated. Various substances having minute porous structure, possess the power of occasioning the union of oxygen and hydrogen at comparatively low degrees of heat; thus spongy platinum will produce this effect at common temperatures, and charcoal or porcelain biscuit at about 300°. It does not seem very clear to what this power is to be attributed; and we are almost equally in the dark regarding the phenomena of endosmose, in which electricity would appear to have some share, the known laws of capillary action not being adequate to explain some of the recently observed facts.*

Many facts corresponding with those to which we just now alluded as having been obtained with regard to the electrical state of different organs of animals, have been remarked in vegetables also. Thus it has been ascertained that wires passed into the pith and applied to the bark indicated opposite electrical states; and the same is true of the two extremities of

* Cyclopedia of Anatomy, p. 110.

most fruits. Some of the most interesting proofs of the occurrence of electric actions in plants are derived from the experiments of M. Becquerel and Mr Crosse, on the effect of currents of voltaic electricity of very feeble intensity, in producing the crystallization of many substances, which, from their insolubility, the chemist has been hitherto unable to procure in that form, but which occur abundantly in vegetables, such as silex, and the carbonate and oxalate of lime. Now, unless we suppose that vital affinity or action possesses this remarkable property in common with electricity, a supposition which appears entirely gratuitous, we cannot hesitate to set down the deposit of these salts in a crystalline state in the vegetable tissues, to the electricity developed by other chemical actions going on in the plant.

It is hoped that the foregoing statements will have established the probability (which is all that the present state of our knowledge on these subjects will allow us to assert) that the forces *immediately* concerned in the production of the changes of decomposition and recomposition in the living body, are the same as are in constant operation in the world around ; and that it is at any rate premature to assert that the operations of vital chemistry are directed by distinct laws, and due to new forces. Indeed, one at least of the writers who has employed the term vital affinity, would seem not to differ essentially from the opinions here given, since he considers,* that this property *modifies* (not supersedes) the chemical relations of the substances subjected to it. This modification we believe with Dr Prout to be due to the *mode* in which other vital properties (the *ensemble* of which seems to constitute the *organic agent* of Dr Prout) bring together the elements ; and if this be true, there can be no definite conception attached to the term *vital affinity* as distinct from chemical affinity.

We may also express our concurrence with the opinion of Dr Prout, regarding the use of the small quantities of elements found in organized tissues which are usually considered as accidental ; namely, that, by their electrical influence, (which may be most powerful, although they are present in

* Alison's Physiology, p. 58.

extremely minute quantity) they may contribute to the production of those striking differences observed among bodies having essentially the same chemical combination, and whose diversity appears at first sight so mysterious. And were we disposed to speculate still further, we might imagine without much difficulty, that the chemical changes effected by the process of nutrition in the fluid contents of the capillaries, might give rise to a new set of electric attractions and repulsions, by which the independent circulation in these vessels might be maintained. If this speculation should prove true (and Sir J. Herschel has expressed his belief, that the circulation of the fluid in chara is analogous in its immediate cause to the motion of globules of mercury or an amalgam submitted to the poles of a voltaic battery) there will be no necessity to have recourse to the theory of a distinct set of *vital attractions and repulsions*, which otherwise is necessary to account for the phenomena. It can scarcely be regarded as a valid objection to it, that it puts out of view the influence of *mind* upon the changes in the capillaries; since there is no more reason for supposing that mind cannot influence (through the nervous system) *electrical* attractions and repulsions, than those of a *vital* nature. In both cases the mind acts upon the properties of matter; and the great obscurity lies in the transition from the immaterial to the material form of existence.

28. We now come to consider those essentially *vital* properties to which nothing analogous is presented in the inorganic world, which are only manifested by organized structures, and whose existence necessarily involves the idea of their continued communication from parent to offspring.

The writer of this essay considers that we are fully warranted by evidence in rejecting the supposition of spontaneous generation as commonly understood, that is, the origination of organized beings by fortuitous combinations of inorganic matter. He thinks, however, that there is also evidence to render it a question worthy of consideration, whether plants or animals of a low degree of organization may not be developed by degenerations of the tissue of those more elevated in the scale. Our belief that such should *not* be the case is founded only on a limited experience, including only the highest classes in each kingdom. There is no anatomical difference, as far as our means of observation extend, between such a simple cellular plant as the *protococcus*, and a single vesicle from the parenchyma of a vascular plant; and recent observations on the development of the lower cryptogamia have shewn that we as yet know very little of the variety of forms which the same germ may assume, according to the circumstances under which it is developed.

How far the act of organization in itself communicates to the assimilated matter the properties in question, will be a subject for future consideration ; at present we shall consider (with Tiedemann) the property of assimilating, organizing, and communicating vital properties to nutritious matter, as that which most prominently characterizes living tissues. And we may go farther and say, that vital properties may exist in fluids also; for the blood must be regarded as something very different from a mechanical admixture of albumen, fibrin, saline matter, water, &c., since it manifests properties which can scarcely be considered otherwise than vital, and presents a structure which must be regarded as approaching that of organized matter. The same may be said of the descending or elaborated sap of vegetables from which the cambium is formed; since, according to the observation of Amici,* the glutinous sap of the vine, when removed from the stem, assumes, during the species of coagulation which it undergoes, regular forms closely analogous on the one hand to those of the lower confervæ, and on the other to the elementary tissues of which the fluid is the pabulum. Hence we may fully unite in the view expressed by Tiedemann, " that as the solid parts obtain from the general nutritive fluid matters which they receive into their composition and organic structure, and to which they communicate their vital properties, so also do the organs which prepare the assimilative liquids from the general nutritive fluid, appear to communicate to them, by the same act, qualities that give them the faculty of acting on the aliment so as to effect their assimilation."

Every tissue possesses vital properties peculiarly its own, besides that which we have spoken of as common to all. Thus muscular fibre is essentially characterized by its contractility, nervous matter by its capability of conveying impressions, &c. These vital properties all require the application of certain stimuli, in order that they may be excited to activity ; and each property of each organ has stimuli appropriate to itself. It is

* Annales des Sciences Nat. t. xxi.

usual to class under the denomination of *vital stimuli* not only those which themselves originate in vital actions taking place in the system (as the stimulus communicated by the motor nerves to muscular fibre), but also those which result from the relation of the organism to the external world. Now, if it be true (as we have already attempted to prove, § 22–27), that the actions *immediately* resulting from this relation are of a physical nature (the *conditions* only being furnished by vital processes within), we can hardly regard the physical agents light, heat, &c. as *vital* stimuli, unless we employ the term *vital actions* in its most comprehensive sense, to indicate *all* the actions going on in a living organism, which conduce to its preservation. On the other hand, the stimulus of the circulating fluid, itself possessed of vital properties (we restrict this term in plants to the descending or elaborated sap), and giving rise to actions of the most strictly vital character, cannot be regarded as itself otherwise than purely vital.

29. Life, therefore, the sum of all the actions performed by an organized being, is, in the first place, the result of the operation of external agents upon the physical properties of the organs by which the relation between the organism and the external world is maintained. The changes which are the consequences of these actions are not identical in their effects with those which we witness in the inorganic universe, because the actions are performed under conditions furnished by the living system with an express tendency to its preservation; and the products of these actions are of a nature to supply the purely vital properties, which then come into play, with the means of their operation.

Thus the quantities of carbon, hydrogen, and oxygen, which combined, under ordinary circumstances, would form carbon and water, are united into gum and sugar (the two simplest of proximate principles) in the ascent of the crude sap up the stem of plants; these principles afford the conditions requisite for the purely vital actions, since out of them are formed, by the process of assimilation, all the complicated variety of vegetable tissues and products.

Now the manifest tendency of all these processes to a common end, has given rise to the supposition of a presiding power by which the whole are regulated—a "vital principle"— an "organic agent endowed with a faculty little short of intelligence,"

—in fact a distinct ουσια or entity, of the nature of whose existence, whether material or immaterial, modern philosophers hesitate in giving a distinct opinion. It will scarcely be worth while to examine the arguments upon which this doctrine has been maintained, since we find the highest authorities of the present day joining in condemning it as altogether unphilosophical.

"This idea is both unsupported by evidence, and useless in the explanation of facts."—Alison's Physiology, p. 3. "The hypothesis of a vital principle is a weapon ready to cut any knot, but capable of untying none."—Prichard on the Vital Principle, p. 128. "It has been proved by a careful examination to be wanting in every characteristic of a legitimate theory."—Ibid. p. 132. "Ainsi, les forces premières, que l'on dit animer les divers corps naturels, ne sont pas des êtres réels, existants par eux-memes, comme on le croyait dans l'ancienne philosophie."—Adelon, Physiologie, tom. iv.

But it may not be amiss to add a few observations suggested by the foregoing reasonings, tending to shew that the supposition is not in the least degree requisite, that it is inconsistent with known facts, and that it receives not the shadow of a confirmation from the analogies of matter.

30. We have endeavoured to shew that an analysis of the functions of living beings terminates in referring them all to certain properties possessed by their component structures, which properties stand in the same relation to organized tissues as the properties of gravitation, electricity, &c., do to matter in general. Their existence must, for the present at least, be regarded as an ultimate fact in physiology. These properties are called into action by stimuli of various kinds fitted to excite them. Whatever adaptation, therefore, we discover in the various functions, both to each other and to the general end of all, implies no more than an adaptation of the structure and properties of the organs by which they are performed. Now, every tissue has its own laws of development; but all these laws are subservient to one general principle, that all organized structures shall take their origin from previously existing organisms. Our inquiry, therefore, leads us back to the first creation of each species; and if we conceive that at that period the Parent of all impressed upon the elements of which each was composed, the spirit of the laws which should in future govern its growth and reproduction (just as he impressed upon the

bodies composing the solar system the spirit of the laws of gravitation and of motion, § 8), we require nothing but the continued action of these laws to account for the continued existence of the race (just as the continued action of the laws of gravitation and of motion retains the planets in their orbits). To suppose that the adaptation of these laws to each other, and to those of the external world, *could* be otherwise than perfect, would be to cast a stigma upon Infinite Wisdom. What they are, it is the object of the physiologist to ascertain by induction; and he certainly will not derive any assistance by setting out with the notion of a secondary presiding existence, whether ψυχη, *anima*, *divinæ particula auræ*, *vital principle*, or *organic agent.*

31. The doctrine of a single presiding existence controlling and directing the actions of each organism, is contrary to known facts. If such a principle exist in connection with the living body, we cannot but suppose that its departure would be coincident with what has been termed *somatic death*, that is, the general disunion of the vital functions and the cessation of the functions of the nervous system. But we well know that the vital *properties* of each one of the tissues concerned in *organic* life may be retained for a considerable period, and that *vital actions* may be performed by individual organs as long as the conditions which those actions require in the living body are supplied to them. The time at which the vital properties finally disappear varies according to circumstances; it is different in every tissue, and the extinction is not sudden but gradual. If, then, we hold the doctrine of a vital principle, we must allow that it may be split into as many individual existences as there are organs in the body; and this is, in fact, approaching to the view of the vital properties which it has been our object to establish.

32. Further, the doctrine of a vital principle is wholly unsupported by the analogies of Nature. No reflecting mind has any doubt that this earth and its inhabitants form a system (which we might almost call an *organised* one, if the idea of a particular *structure* were not involved in the term), of which every part is perfectly adapted to the rest, and of which all the actions and changes, however in appearance contrary, have one

common tendency, the ultimate happiness of the creatures of Infinite Benevolence. It cannot be regarded as an improbability, that the other spheres and systems, whose countless multitudes, revealed by the aid of science, impress our minds with the nearest approach to the conception of infinity of which our finite comprehension is capable, are peopled with beings, if not similar in structure to ourselves, at least equally worthy of the Creator's care. In the government of our own planet, itself but a point in the vast universe, we are able to recognise, to some small extent, the laws by which its physical changes are guided; and we discern faint glimmerings of those by which the moral condition of sentient beings is controlled. So far as we can understand the mutual adaptation of these laws, we every where see them working towards the same end; and we entertain the highest anticipations of the beauty and harmony which will be revealed to us when our imperfect knowledge shall be extended and corrected by the light of Eternal Truth. Should we not consider it degrading to the dignity of Infinite Wisdom to suppose, that, at the creation of each world, he had found it necessary to delegate to a subordinate the control over its working, instead of at once impressing upon its elements those simple properties, from whose mutual actions, foreseen and provided for in the laws according to which they operate, all the varieties of change which it was His intention to produce, should necessarily result. The application of this argument to the present subject is too obvious to require further development.

33. We have now to consider in what relation the vital properties of the various tissues of which a living body is composed stand towards the peculiar form in which the molecules of those tissues are arranged, and the peculiar character of the chemical combinations of which they are composed; in a single word, their organization. It appears to us that much discussion on this subject might have been spared had more attention been paid to the true meaning of the terms employed. It has been said by some that vital properties are *superadded* to organized matter. This supposes the distinct existence of the property, and is but another form of the doctrine of the vital principle. If the property be nothing but the matter itself in

a certain relation to the mind, the expression is manifestly inconsistent with itself. But there is nothing inconsistent with our knowledge of the physical properties of matter in the belief that all matter, or each at least of those forms of it capable of being assimilated, is also endowed with vital properties which remain undisplayed or occult (see § 5) until brought into action by being subjected to those conditions which a living organized system alone can afford.

Experience and observation lead to conclusions not dissimilar. Organization and vital properties are simultaneously communicated to the germ by the structures of its parent; those vital properties confer upon it the means of itself assimilating, and thereby organizing and endowing with vitality, the materials supplied by the inorganic world; and, as long as each tissue retains its normal constitution, renovated by the actions of absorption and deposition by which that constitution is preserved, and surrounded by those concurrent conditions which a living system affords, so long, have we every reason to believe will it retain its vital properties and no longer; and, just as we have no evidence of the existence of vital properties in any other form of matter than that denominated organized, so have we no reason to believe that organized matter can exist in those conditions which a living body supplies, without manifesting vital properties.

There is no difficulty in accounting, on this view, for the general loss of vitality which results from the cessation of any one function; and as the advance of pathological science renders it every day more probable that derangement in function always implies some structural alteration (although it may be imperceptible to our senses) or change in the character of the necessary stimuli by which the properties are called into action, the usual phenomena of death can readily be explained upon the principles here advocated. There are some cases of sudden destruction of vitality, however, in which no perceptible change in solids or fluids can be detected; but this is probably owing to the imperfection of our means of research. For instance, we can scarcely doubt, that, when the vitality of an egg is destroyed by an electric shock, or moderate exposure to heat, the agent produces, in obedience to chemical laws, some alteration in the material structure inconsistent with the continued existence of the vital properties. A seed may be deprived of its vitality by immersion in water of the temperature of 160°, and nothing but a very minute examination would discover any structural alteration in its tissues; but collateral experiments prove that this is just the temperature at which the vesicles of fecula are ruptured, so that the physical change produced by a physical agent is evidently the cause of the loss of vital properties.

34. Our general conclusions, then, from the foregoing arguments, are,—

1. That the *properties* of any aggregation of matter depend upon the method in which its ultimate molecules are combined and arranged.

2. That the simplicity of our notion of the properties of inorganic matter depends upon the facility of our becoming acquainted with them through the command which we possess over the agencies by whose operation they are manifested.

3. That the vital properties of organized tissues are not less the result of their material constitution; but that, whilst the *materials* of an organized tissue may be prepared by the operation of the ordinary laws of affinity acting under peculiar conditions, the tissue cannot be constructed without the agency of a previously existing vitality; and that hence man is debarred from the most advantageous means of becoming acquainted with the laws of physiology.

4. That *vital properties* are not *added* to matter in the process of organization; but those previously existing, and hitherto inactive, are called out or developed.

LETTER,

&c. &c.

LETTER

TO THE RIGHT HONOURABLE

GEORGE, EARL OF ABERDEEN, K.T.,

&c. &c.,

PRINCIPAL SECRETARY OF STATE FOR FOREIGN AFFAIRS,

CHANCELLOR OF THE UNIVERSITY AND KING'S COLLEGE, ABERDEEN,

ON THE STATE OF

THE SCHOOLS OF CHEMISTRY

IN THE UNITED KINGDOM.

BY WILLIAM GREGORY, M.D.,

F.R.S.E., M.R.I.A.

PROFESSOR OF MEDICINE AND CHEMISTRY IN THE UNIVERSITY AND KING'S COLLEGE, ABERDEEN.

LONDON:

PRINTED FOR TAYLOR & WALTON,

28, UPPER GOWER STREET.

1842.

Printed by J. L. Cox & Sons, 75, Great Queen Street,
Lincoln's-Inn Fields.

LETTER,

&c.

My Lord,

I have been induced to take the liberty of addressing your Lordship on a very important subject, namely, the state of the Schools of Chemistry in the United Kingdom, by the circumstance, that your Lordship is the head of the venerable Institution in which I have the honour to occupy a chair. But, independently of this, the high reputation acquired by your Lordship as an accomplished scholar and a munificent encourager of the arts, added to the powerful influence which your Lordship's high official station and personal character must give you in her Majesty's councils, would justify the call I am about to make on your Lordship's attention and indulgence.

Convinced that no one has the interests of science and the true welfare of his country more sincerely at heart than your Lordship, I shall make no apology for beseeching your Lordship's attention to a subject which has the most direct and powerful influence on both, but proceed at once to lay before your Lordship certain statements of fact, in reference to the great importance of chemistry to the British nation, and to the means of instruction in the practice of chemistry which are accessible to her Majesty's subjects.

In no country will the vast importance of chemistry to mankind be more readily acknowledged than in Britain. We have constantly before our eyes, in a greater degree than any other people, the innumerable applications of this science to useful purposes in our manufactures; and every one knows, that the steam engine, the safety lamp, gas light, and a hundred other inventions, are so many benefits conferred by science on mankind within the last half century.

But, even among us, few are aware of the extent to which chemical manufactures are essential to our national prosperity, and inextricably interwoven in the whole fabric of those useful arts on which our physical comfort and happiness, as well as our trade and commerce, are entirely dependent.

Your Lordship will, therefore, I trust, forgive me, if I venture here briefly to trace the history of one branch of chemical manufacture, with its applications to other departments of industry. The example I shall select is the manufacture of sulphuric acid, which has in this country reached so high a state of perfection. I shall endeavour to explain the cause of that increased demand which led to the great improvements in the manufacture of this acid, the very remarkable results which gradually flowed from its increased supply at a low price, and the important bearings of this branch of industry on the national interests.

It is particularly worthy of remark, that these results have followed in very rapid succession, and that, without exception, they have arisen from the application of purely scientific principles to the solution of practical problems;

a striking proof of the truth that the cultivation of natural science for its own sake is the only true source of useful discoveries. The problems I have alluded to could only have been solved by men who had devoted their lives to purely scientific research, and had thus established those principles of universal application which enabled them to attain the objects at which they aimed. The past success ought also to teach us, that the diligent research after new truths, however they may at first appear remote from any practical application, will yield, in course of time, practical results of equal or greater importance; that no well ascertained fact in natural science is ever barren: and that the best method of promoting practical improvements is to encourage scientific research.

In describing briefly the multiplied relations of this important manufacture, I shall avail myself of a very able anonymous article on the subject, which lately appeared in the "Allgemeine Zeitung," and has obviously been written with a view to direct the attention of the German public to the importance of the subject. Without professing to give a mere translation, I will give the substance of this paper, requesting your Lordship to bear in mind, that the whole account is given as a specimen of the effects produced by one chemical manufacture only, although that is probably the most important of all, if we except the iron manufacture.

The first great stimulus to the improvement of the manufacture of sulphuric acid was given by the announcement of a prize of 1,000,000 francs (£40,000), offered by the Emperor Napoleon for the discovery of a simple and cheap process for extracting soda from sea salt. Soda, as is well known, has been used, from time immemorial, for the manufacture of soap and glass, two products of the highest value to mankind. Indeed, the use of soap is so essential to comfort, that the quantity of soap consumed by any people may be viewed as a direct measure of the degree of civilization and happiness they enjoy. Its use depends on the feelings of comfort, nay, on the sense of the beautiful, which are inseparable from cleanliness. Where these feelings prevail, there, we may be sure, civilization and happiness are to be found. The princes, counts, and barons, the rich and powerful in the middle ages, who concealed with costly spices and odours the offensive exhalations of their skin and of their clothes, which rarely came into contact with soap, indulged, it is true, in greater luxury in their sumptuous feasts and splendid dresses than their descendants in modern times. But how vast is the difference between their days and ours, in which personal filth has come to be synonymous with absolute misery!

It is to glass, again, that the poor man owes the inestimable blessing of the free admission of light to his dwelling, even in the coldest climate. It is not easy to exaggerate the value of these two products, soap and glass, to mankind. During the war, France was deprived of her accustomed supply of barilla (the usual source of soda) and of soap from Spain, the ports of both countries being watched by the British fleet. The high price of soda, soap, and glass, consequent on this state of matters, led to the offer of the prize above mentioned; and the problem was solved by the French chemist, Leblanc, who furnished a cheap and simple process for extracting soda from sea salt. France soon supplied herself at a cheaper rate than before; manufactories of soda, soap, and glass, arose and flourished; and the bitter feelings excited among the Spaniards by the permanent loss of a lucrative trade were not without their influence in bringing the Peninsular war to a fortunate conclusion, and in hurling Napoleon from the imperial throne.

Such were the immediate results of Leblanc's discovery; but it is painful to add, that he never received the reward he had so well deserved. The restoration occurred in the interval; the new government had more pressing debts to discharge; and it is understood that the claim has now been shut out by prescription. Let us now consider the nature of Leblanc's process.

To convert salt into soda, the first step, according to this process, which is now, with some modifications, uniformly followed, is to convert the salt into sulphate of soda. This can only be done by means of sulphuric acid, of which 80lbs. are required for 100lbs. of salt. Hence, one of the first effects of Leblanc's discovery was, to create a very large demand for sulphuric acid. It is obvious, that as soon as the government, by reducing the duty on salt, reduced its price to a minimum, the price of soda became dependent on that of sulphuric acid. This circumstance, together with the extensive demand, and the large profits realized by the makers of sulphuric acid, turned the attention of men of science to the improvement of this latter manufacture; and every year produced some new amelioration, while the price of the acid steadily fell, and the demand for it as steadily increased. Its formation was studied by the most accomplished chemists, and brought by degrees to its present nearly perfect state.

Sulphuric acid is made in vessels, or rather chambers, of lead, and so large is the scale of operations in some manufactories, that one of these chambers would contain with ease a middle-sized house of two stories. So nearly does practice in these great manufactories approach to theory, that 100 lbs. of sulphur, which, by theory, should yield 306 lbs. of sulphuric acid, do actually yield 300 lbs.

In this manufacture, the price of the product depends partly on the apparatus, partly on the price of the materials, sulphur and saltpetre; and in both, a great reduction has been effected. Till lately, the plates of lead, of which the chambers are formed, were soldered together with difficulty, by means of lead, no other solder being able to withstand the action of the acid. The operation of soldering cost nearly as much as the plates themselves; but now that the oxy-hydrogen blowpipe is used for the purpose, the expense is a mere trifle, while the operation is so easy, that a child may perform it. Again, the acid was formerly concentrated in enormous glass retorts; these were exposed to breakage, occasioning heavy loss, and destroying the furnaces; vessels of platinum are now used for concentrating the acid, and although these sometimes cost from £1,000 to £1,500 apiece, they are found, from their durability, to be a source of economy, and have materially contributed to bring about the very low price of the acid: moreover, it is the demand for platinum for such vessels that alone renders profitable the working of the Russian mines of that metal. We may see by this, how every discovery acts in many different ways, and always advantageously.

When economy had been pushed thus far in the apparatus, the price of the materials became a point of more importance than previously; that of nitre was so high, as to stimulate the manufacturer to search for some substitute, which was speedily found in the nitrate of soda, enormous beds of which cover whole plains in South America. This salt is much cheaper than saltpetre, and preferable to it for the manufacture both of nitric acid and of sulphuric acid; but besides the direct effect of cheapening these acids, the introduction of nitrate of soda, by limiting the use of saltpetre to the making of gunpowder, for which nitrate of soda does not answer, has produced the indirect effect of cheapening gunpowder, the price of saltpetre necessarily falling as the demand for it diminished. This must be, in time, a material source of saving to governments.

Finally, with regard to the chief material, sulphur, on which the price of sulphuric acid now principally depends, it is well known that our manufacturers derive nearly their whole supply from Sicily, so that Naples may be said to possess a monopoly of that article. That the trade in sulphur is highly important to both nations is obvious, when we reflect on the enormous quantities of sulphuric acid now manufactured in Britain alone. A small manufactory will produce from 250 to 300 tons annually; a large one 3,000 tons, or more: it is no wonder, then, that the late interruption to the trade in sulphur caused great uneasiness among our manufacturers; but it had another effect: the attention of chemists was keenly directed to other means of procuring sulphur, and, during the period of obstruction to the sulphur trade, it is said that no less than fifteen patents were taken out in England for recovering the sulphur from the sulphuric acid used in the soda manufacture. The restoration of the trade to its accustomed channel has postponed the accomplishment of this object; but the impulse has been given, and

Naples may ere long find good cause to regret that she ever allowed any obstruction to the trade in sulphur. We have whole mountains of gypsum and heavy spar, and abundance of pyrites and galena, all of them minerals containing sulphur, which we shall one day find the means of extracting economically; indeed, during the period above alluded to, many tons of sulphuric acid were actually made from iron pyrites. When we consider the resources of modern chemistry, it will not appear improbable, that if the sulphur trade had been obstructed for a year longer, it might by this time have been lost to Naples for ever.

These considerations are of themselves sufficient to shew that the manufacture of sulphuric acid has become a matter of national importance, were it only on account of its use in making soda; that alkali is now sold in a state of perfect purity, and at a wonderfully low price, so low, indeed, as almost to have put an end to the use of potash. The quality of glass and soap has been very much improved, and their price greatly diminished; the consumption of both articles has naturally increased in a corresponding ratio. Wood ashes, no longer in demand to nearly the same extent as formerly for manufactures, must also fall in price, and will soon be employed as one of the most powerful manures for our wheat fields.

Such are a few of the bearings of the manufacture of sulphuric acid, called into existence, or at least vitally improved, by the demand for cheap soda: but this is not all; and although it is impossible here to follow out all the ramifications of this remarkable branch of industry, I cannot refrain from pointing out one or two of its immediate results, which have not yet been adverted to.

It has already been mentioned, that sea salt, in order to yield soda, must first be converted into sulphate of soda; now, in acting on the salt for this purpose with sulphuric acid, an enormous quantity of muriatic acid is produced, which, in the earlier periods of the manufacture of soda from salt, was thrown away as worthless, so great were the profits realized on the soda; but muriatic acid contains chlorine, and no other compound of chlorine yields that body more easily or more cheaply than muriatic acid. The bleaching properties of chlorine were known, but had not yet been applied on the great scale. At first the chlorine was disengaged directly from the muriatic acid, and brought in contact with the cloth to be bleached, in the form of gas; but it was soon found that, by combining the chlorine with lime, it might be obtained in a solid form (bleaching powder), capable of transportation to any distance; hence arose a new and lucrative manufacture, of such importance, that it may safely be asserted, that, but for the discovery of the bleaching powder, the cotton manufactures of Britain would never have attained their present development: nay more, had the British manufacturers been tied down to the old method of bleaching, they could not long have competed, in the price of cottons, with France or Germany.

To bleach in the old style, the first requisite is land, and that good and well exposed meadow land. The cloth must be exposed for several weeks, and that only *during summer*, to sun and air, and must besides be constantly watered by hand. Now, a single manufactory, of moderate size, near Glasgow, bleaches, on the new system, on an average, 1,400 pieces of cloth *daily throughout the year*. Let us only consider what an amount of capital would be required merely to rent the land necessary for bleaching in the old manner this enormous quantity of cloth, in the vicinity of a large city. Let us reflect on the time and labour that would be indispensable, and we shall soon perceive that, with such burdens, the British manufacturer could not compete with his rivals on the continent, where vast tracts of fine meadow land might be had, distant from any great city, at a far cheaper rate, and in a more sunny climate. The superiority of our machinery would thus be in a great measure neutralised, were it not for the manufacture of bleaching powder, which in its turn depends on those of sulphuric acid and of soda. I need not do more than allude to the use of the bleaching powder in paper-making, which is one great cause of the superior quality and low price of paper in Britain.

Another important use to which the muriatic acid produced in the soda manufacture, and formerly thrown away, is now applied, is that of preparing cheap and superior glue from bones. Bones consist of bone earth and glue; the former is readily dissolved by diluted muriatic acid, while the latter is left, and has only to be dissolved in warm water to be ready for use. The acid solution of the bone earth, on the other hand, promises to be an admirable form of using that earth as manure. Professor Liebig, in his late valuable work on Agricultural Chemistry, has recommended this application. At present, the solution in question is thrown away as useless in the glue manufactories.

The last application of sulphuric acid which I shall here mention is a very recent one, and owes its origin to one of the most scientific chemists of the day, M. Gay-

Lussac. It consists in its employment in the refining or purification of silver.

Silver, as it comes from the mines, is alloyed with one-half, or rather more, of copper. It also contains a small quantity of gold. It must be refined—that is, purified; and the pure or fine silver is then alloyed with the due amount of copper to form the standard silver.

Raw silver was formerly refined by cupellation, a process which cost about 35*s.* for 50 lbs. of silver. The gold contained in the silver would not repay the expense of extracting it, and was therefore allowed to remain, and to circulate in the silver, absolutely worthless. But by means of sulphuric acid, cupellation is avoided; the silver is refined at a most trifling cost, and the gold is obtained by the same operation: nay, even the copper which was formerly lost, is now preserved; and although the gold only amounts to from $\frac{1}{2000}$th to $\frac{1}{1200}$th of the weight of the silver, yet as its value is about $1\frac{1}{2}$ per cent. of that of the silver, it not only repays the whole expense of refining, but leaves a clear profit to the refiners. This beautiful application of chemistry has given rise to the singular and apparently anomalous result, that the seller of raw silver receives from the mint the exact quantity of pure silver which his alloy, on being tested, is found to contain, and likewise the whole amount of the copper present in the alloy, thus apparently paying nothing for the process of refining. The refiner is paid by the gold which he retains, and which was formerly lost to every one. The saving effected by this improvement to the French mint is stated to have been enormous.

I must not dwell longer on details of this kind; but I would mention, in passing, that but for the wonderfully low price of sulphuric acid, it would have been impossible to produce a number of useful articles not yet mentioned; such as the beautiful stearine candles, phosphorus, phosphorus matches, lucifer matches, and many other products equally remarkable for their superior quality and for their astonishing cheapness. The present prices of sulphuric, nitric, and muriatic acids, of soda, soap, glass, phosphorus, &c. &c. would have been considered, a quarter of a century ago, as fabulous and impossible. Who can foresee what new discoveries, or what improvements on old ones, may arise during the next twenty-five years?

I trust that the foregoing considerations will render it obvious that our chemical manufactures possess a higher degree of national importance than people are generally aware of. Let us only reflect in how many ways a rise in the price of sulphuric acid would be disadvantageously felt; that it would affect the price of soda, soap, glass, bleaching powder, cotton, and all the products I have mentioned, besides many more; and it will then appear quite natural that the late obstruction to the sulphur trade should have threatened to excite war. It is clear that these manufactures are now inseparably connected with almost every branch of our trade and commerce, and that they cannot suffer without causing severe national loss.

It is obvious, from these facts, that if any nation is bound to encourage and promote the study of practical chemistry, it is the British nation, which has derived, and continues to derive, such vast advantages from the application of its principles to the useful arts. Yet, if we investigate the subject, we shall find that the opportunities afforded in this country for the study of practical chemistry are exceedingly limited; and that, in point of fact, we possess no institution where the student can acquire, at a reasonable rate, the art of scientific research, in the only way in which it can really be acquired, that is, by constant practice in the laboratory.

The elements of chemistry must always be taught by lectures, and for these there may be said to be sufficient provision in this country. But lectures are not sufficient; they cannot carry the student beyond the elements, and the idea of teaching the art of research in lectures is quite absurd. That art consists in the making of experiments, with a view to ascertain some new truth, or to correct some error.

Every experiment may be regarded as a question addressed to Nature: the phenomena observed are the words of the language in which Nature answers the question; and it is only when the inquirer is familiar with the language, and when the question is distinctly put, than an answer can be expected. If these conditions be fulfilled, the answer is obtained without fail, and the observer is capable of interpreting it.

Now in the lecture-room we can only learn the alphabet, as it were, of this language, the elements of the new speech; such are, for example, the properties of matter, the laws of combination, and the characters by which one body is to be distinguished from another. It is in the laboratory alone that we can learn the use of this alphabet; that we can become familiar with the words it yields; that we can learn to shape our questions aright and to read the answers. When we know thus much, we can enter on research with some prospect of success; and it is not too much to say, that no question has ever been distinctly addressed to Nature in the language of experiment, which has not received an answer; that no problem in natural science has ever been clearly given, which has not been experimentally resolved. Hence the vast, the paramount importance to the chemist of a proper education in the art of research, or, in other words, of practical chemistry.

But practical chemistry, or the art of research, is, from its very nature, an expensive study; probably much more expensive than any other branch of education. There are required, first, a convenient and spacious laboratory, expressly fitted up for the purpose; secondly, a complete apparatus; thirdly, a large supply of fuel; fourthly, the substances or materials without which chemistry cannot be practised; and lastly, a qualified assistant, capable of taking charge of the laboratory, and of superintending, under the professor, the working pupils; besides preparing the experiments for the lectures, which, for the benefit of beginners, should always be given in a practical school.

The laboratory must include several rooms, of which one, the largest, should be devoted to the practical operations of the students. This room must be well supplied with water, airy, well lighted and ventilated; it must contain several furnaces and hearths, so arranged as to allow the products of combustion to be carried off: one large sand-bath at least, with a hot air chamber below it; a still, for providing a constant supply of distilled water; and, finally, it must be well furnished with tables, drawers, presses, and shelving.

With regard to apparatus, besides the common and indispensable articles of glass, porcelain, and metal, including portable furnaces for charcoal and gas, and spirit lamps, every laboratory of research should possess one or more delicate but strong balances, with accurately divided weights; one or more air pumps, with numerous plates of ground metal, capable of being detached from the pump when exhausted, and kept in that state.

A store of glass and other ordinary apparatus should be kept in a distinct room, which should be sold at prime cost to the students, that each may have his own, as far at least as the smaller articles are concerned. A blow-pipe table for working glass should be attached to this store-room, or to another in which the stock of materials is kept. A small room, clear of the laboratory, is also required for the balances, air-pumps, and other delicate apparatus, which would be injured by corrosive vapours. In this room, a select library of chemistry, for the use of students while in the laboratory, ought to be kept; and either here or elsewhere, a cabinet of chemical specimens, carefully arranged and labelled, should be preserved, for the purpose of illustration. If possible, there ought to be an inferior laboratory or small kitchen, for operations on the large scale, such as the distillation of acids, and the preparation of other products consumed in a laboratory of research; and a sheltered table in the open air ought to be provided, for offensive and unwholesome processes, which often occur. Of course, there must be a cellar for fuel. Finally, the lecture-room ought to adjoin the laboratory, if possible, that all the apparatus, materials, and specimens, required in the lectures, may be close at hand.

Such is a brief and imperfect sketch of what is absolutely essential in a laboratory where the art of research is to be taught. I regret to say, that hardly any university in this country, and but few on the continent, can be said to possess one-half of this necessary accommodation.

In the laboratory of the university of Giessen, built and furnished under the superintendence of Professor Liebig, all the conveniences above mentioned, and a good many more, have been liberally supplied. I have carefully examined the whole of Professor Liebig's arrangements, and shall have occasion presently to allude to the good effects produced by them. At present, I would direct attention to the fact, that although such arrangements cannot be carried into effect without expense, yet the sum required, if the plans be judicious, is far smaller than could possibly have been expected. It is certain that, in many cases, more money has been expended on the most imperfect laboratory than would have sufficed to produce one even superior to that of Giessen.

The necessity of a qualified assistant will appear, when we consider the continual superintendence required by beginners in practical chemistry, and the vital necessity of order and method in a laboratory. It is true that the professor may often obtain the temporary assistance of some advanced pupil, who exchanges his labour for the additional knowledge to be thus acquired; but this can never supersede the necessity of a permanent assistant, who knows by experience the wants of the students, and learns to keep up a regular supply of those materials which must be made in the laboratory, because they either cannot be purchased at all, or cannot be purchased cheap. Indeed, with all the gratuitous aid that students can give, the place of assistant is one of severe labour. The assistant must therefore have a salary, the amount of which will vary in different localities and under different circumstances. Besides such an assistant, however, qualified by his chemical knowledge, there must be a servant for the coarser and menial work, who may soon learn to perform certain useful operations, and who may also be storekeeper. He must also be paid, in one form or another.

The absolutely necessary expenses of a laboratory may therefore be classed under three heads :

1. The original outlay in building and furnishing the laboratory.

2. The annual outlay for materials, for the wear and tear of apparatus, and for fuel.

3. The salaries of an assistant and a servant.

It is clear that this expense must be borne, either by the institution itself to which the laboratory belongs, by the public, by the professor, or, finally, by the students.

In this country, the existing laboratories have generally, indeed I may say always, been provided by the universities to which they belong ; while the burden of the annual expenditure is thrown on the professor or on the students. In most cases, the apparatus, or a great part of it, has been purchased by the professor, and consequently belongs to him, an arrangement fraught with inconvenience in the case of the death or removal of the professor, and the appointment of his successor.

It has already been stated that our laboratories are generally very far from being complete, and the same may be said of the apparatus. This is only the natural result of the system, by which so large a share of the expense is thrown on the professors or on the students. Even where a tolerable laboratory and apparatus have been provided, it cannot reasonably be expected that the teacher should bear the annual expense. There is but one way in which he can avoid a heavy loss, and that is, by charging a high fee for laboratory practice. In Paris, the usual fee for an eight months' course is 1,500 francs, or £60. In London, for a six months' course, it is generally about £50.

Nothing can be more certain than that these charges, although far from exorbitant, are quite beyond the means of students in general. Nor is it among those who can readily afford such fees that the most diligent students are to be found. The natural consequence is, that but few students study practical chemistry on these terms. In short, the attempt to throw the burden on the students effectually prevents the formation of an efficient school of research.

In those instances in which such a school has been formed, and has existed for a time, the burden has invariably been borne by the teachers, who have exacted only such fees as were within the power of the students to pay; but as this plan involved a serious loss to the teachers, it has also failed in creating an efficient school. Those teachers who, much to their credit, have made the attempt, have found it absolutely necessary to relinquish it, after sustaining considerable loss.

At present several laboratories are nominally open to advanced students, for the purposes of research, at the high fee above mentioned, or even at a charge considerably lower, but it may be stated as certain, that such a school as I am anxious to see established nowhere exists in the United Kingdom.

That such a school, however, may be formed, and may flourish without any peculiar advantages of situation, we have positive proof in the case of Giessen, already alluded to, where, at this moment, fifty practical students are employed in the laboratory; the number having steadily increased up to this point during the last fifteen years, and more especially since the completion of the laboratory in 1835.

In this country, for the reasons above stated, hardly any students have the advantage of a tolerably complete education in chemistry, except the few who act as assistants to our professors ; but even they have not the same advantages as the students at Giessen, because the languishing condition of our schools affords no inducement to multiply the facilities of practical study. As a proof of the truth of this statement, it may be mentioned that any one who wishes to become practically familiar with the processes employed in organic analysis cannot do so at home ; but must go either to Paris, to Berlin, to Göttingen, or to Giessen, where he will see all these important operations hourly practised.

Many of our rising chemists, indeed all such as have distinguished themselves by researches in organic chemistry, have studied this branch of science on the continent; and I could name several of our professors who have done the same. These facts can only be accounted for by the want of proper schools at home; and that this is no temporary deficiency is obvious from the fact that the number of young British chemists who study abroad is annually increasing; while a reference to the scientific journals will shew, that till our students adopted the system of studying in the continental schools, organic chemistry, which has made such amazing progress of late years, was cultivated almost exclusively on the continent, and lamentably neglected here.

It may safely be affirmed, that if proper schools of research had existed in the United Kingdom, British chemists would have taken a far more active part in the cultivation of organic chemistry. Our general neglect of this fertile and inexhaustible field of discovery, the importance of which cannot be overrated, is in itself a sufficient proof that our system of instruction must have been very deficient.

On the other hand, nothing can prove more clearly the superiority of the system established at Giessen than the acknowledged fact, that to this school alone we are indebted for a very large proportion of those researches which have advanced organic chemistry to its present very flourishing condition. It would hardly be exaggeration to say that all the other schools of Europe together have not done more for organic chemistry than the school of Giessen. The resort to it of students from all parts of the world is another proof of the very high character it has acquired as a school of research.

Let us inquire, therefore, what there is, in the system adopted at Giessen, to account for results so different from those which have flowed from the system followed in this and some other countries. We shall find the cause of the difference to be very simple.

The principle on which the School of Practical Chemistry at Giessen is founded is that of enabling the professor to open his laboratory, provided as it is with every thing necessary for research, on terms which allow almost every student with ease to avail himself of the opportunity.

At an early period, I expect, through the kindness of Professor Liebig, to be enabled to lay before your Lordship the plans of the laboratory at Giessen, with a statement of its actual expense, and other important details of its management: at present I can do no more than give a very brief and general account of the expenses connected with it, and the terms on which it is opened to students. I beg leave to state, that in this matter I speak from intimate personal knowledge of the laboratory, and of the working of the system there adopted.

The professor has a handsome salary and a free house. The laboratory has been built, on plans approved by him, at the expense of his government; and the entire cost, including all furnaces, sand-baths, water-pipes, and the numerous indispensable fixtures of a laboratory, amounted to 13,000 florins, or about £1,120 sterling. Considering its extent and completeness, and the large number of students it can accommodate, this sum must be regarded as very small; and it is certain that more has often been expended in the construction of far inferior laboratories. In many of our universities, I have no doubt that a much smaller sum, judiciously expended in altering, enlarging, or improving the existing laboratories, would suffice to furnish ample accommodation for such a number of students as might be expected to enter them. On an average, £1,000 might be assumed as sufficient, making allowance for the higher price in this country, both of labour and of building materials. I do not know whether the above sum of £1,120 at Giessen includes any part of the moveable apparatus, or, if it does so, how much. But of this kind of apparatus our universities have generally a tolerable supply, although it is often, in great part, the property of the professor. At all events, a moderate sum would secure a sufficient apparatus, to belong to the laboratory, and to be used by all the students, under certain regulations; while each student, according to the plan followed at Giessen, which I will presently describe, should purchase certain cheap articles of apparatus, to belong to himself, and to be used by himself alone.

The next point in the system adopted at Giessen is the annual allowance for laboratory expenses, that is—for wear and tear of apparatus, for chemical materials, and for fuel. At Giessen, the annual allowance, under this head, is about £130, which is found sufficient to defray the cost of the lecturers and laboratory, when the working students do not exceed fifteen. It is paid by the government.

The government also pays the salaries of an assistant and a servant. Two or more assistants are necessary when the working pupils are numerous; but at present I cannot say whether the second assistant is paid by the government or by the professor. I know that a third, when required, is paid for by the professor.

The fees paid by working pupils are calculated according to the number of days per week, during the course, that they employ in the laboratory; it being found far more advantageous to devote one whole day to practical chemistry than two half ones, owing to the tedious nature of many processes, which, besides, cannot be interrupted. So much is this the case, that no student is received, except on condition of devoting the entire day to practical chemistry. Those who work one day during the week pay 13 florins for the course, or about £1. 2*s.* For every additional day, the charge is the same; and those, the great majority, who work six days per week, pay 78 florins, or about £6. 14*s.* for the course. The length of the course is from eight to nine months.

Each student provides himself, from a stock kept by the servant, and sold at prime cost, with a certain quantity of apparatus, such as a spirit lamp, test tubes, combustion tubes for analysis, precipitating jars, evaporating basins, funnels, &c. These articles belong to himself exclusively, and remain at that part of the table which is marked off for him. The tables are furnished, at frequent intervals, with complete sets of bottles, labelled in enamel, containing all the necessary re-agents: these belong to the laboratory. When a student's course is finished, he either takes with him the apparatus he has purchased, or disposes of it to a new-comer. Many of the students take home with them complete sets of portable apparatus, of which they have learned the use.

The fee above mentioned, and the small apparatus, constitute the whole of the necessary charge to the student, and the latter is a very trifling item in Germany. In this country, glass and porcelain, for chemical purposes, are considerably dearer, besides being of very inferior quality; but if the excise laws, in regard to glass, were so modified as to allow good glass, for chemical purposes, to be made at home, or if the duty on foreign glass and porcelain, *imported for scientific purposes*, were remitted, as it ought to be, such apparatus might be as cheap in this country as it has long, to the great benefit of science, been on the continent. The heavy duty (or rather duties, for there are two, amounting to about 50 or 60 per cent.) on foreign glass is a most serious obstacle to the cultivation of chemistry in this country.

The student, having previously attended lectures on chemistry, is first carried through a course of qualitative analysis, till he is thoroughly acquainted with the characters of chemical substances. He then proceeds to enter on some original investigation, either selected by himself, or suggested by the professor. He thus rapidly acquires a full command over the resources of chemistry, and is very generally enabled to obtain results worthy of publication. By this means he becomes known, and in a short time obtains some situation, either as professor in a university or gymnasium, assistant to a professor, or superintendent of some chemical manufacture.

It is worthy of notice, that whether the object of the student be to qualify himself as a teacher of chemistry, to learn the bearing of that science on medicine and physiology, or to become a manufacturer, the same purely scientific education in the art of research is recommended to all. It would be impossible, for example, to teach specially all the different chemical manufactures, so that the future iron-smelter should learn only iron-smelting, the soap-boiler only soap-boiling, &c.; and, if possible, it would be the reverse of beneficial. It is found by experience, that when all learn the general principles of chemistry, they acquire the special details of any manufacture in the manufactory in a far shorter time than they could have done in the laboratory. No attempt is made, therefore, to teach on the small scale processes that must be practised on the large scale. The student learns practically those principles by which all chemical manufactures must be regulated, and the result may be best stated in the words of Professor Liebig himself.

" It is generally in fear and trembling that they follow " my advice, which is, to devote their whole attention, not " to imitating manufacturing processes in the laboratory, " but to learning how purely scientific problems are to be

" resolved. Their intellect soon and easily learns how to " find the best means : it is they themselves who modify " and shape their means according to circumstances. " Every operation, every analysis, which serves to clear " up a given question, or which must be performed in " order to discover the conditions essential to the resolu- " tion of the problem, has a specific object. Each process " thus acquires a certain charm that effectually wards off " fatigue; and when the problem is once resolved, the " student has learned the means of solving all similar " problems. I am acquainted with many of my former " pupils who are now at the head of every variety of " chemical manufacture. Without having ever practised " these in the laboratory, they became in the first half- " hour perfectly familiar with the whole process; and the " next half-hour commonly produced a number of well- " devised improvements in the manufacture. They had " acquired in the laboratory the most exact knowledge of " the properties of the materials which they had to em- " ploy, and were accustomed, as the only way of avoiding " errors, to subject the products of chemical reactions to " a searching investigation in regard to their composition; " and they thus at once discovered the sources of error, " the means of avoiding loss, and the best methods of " improving the apparatus, or perfecting the process. All " this is not learned, when the student enters a laboratory " for the purpose of practising a given process by recipe."*

I have been thus minute on this point, because I wish it to be generally known, that, even for directly practical purposes, the most purely scientific education is really the

* Ueber den Zustand des Chemie in Preussen, by Dr. Justus Liebig, Professor of Chemistry at Giessen, 1840.

best, and is more certain to lead to improvements in practice than the most laborious experience in any one manufacture, gained, as it generally is, at the expense of general principles.

Such is a sketch of the system pursued at Giessen. For its results, we have only to look at the very numerous and valuable researches which have issued from that school; to the large number of its pupils now distinguishing themselves as teachers of chemistry, as writers and experimenters, and as chemical manufacturers; and finally to its flourishing condition at the present time, so different from that of any British school of practical chemistry.

As courses of practical chemistry are given in our medical schools, it may be necessary to state, that these have hardly any thing in common with such a course of instruction as I have described. They are confined to medical students, who are required to attend them; but as they are limited to one hour daily for three months, it is obvious that they afford no means of acquiring the art of research. As far as they go, they are doubtless useful; but they are in a great measure devoted to impressing more firmly on the minds of the students the elements of the science as taught in lectures, to pharmacy, and to the detection of poisons. The schools of practical chemistry which I have endeavoured to describe and recommend are not for medical students, but for training those who wish to devote themselves to chemistry as a profession; and such schools we have not.

It will now, I trust, appear clearly, that the cause of this deficiency is the circumstance, that the necessary expenditure, not being otherwise provided for, falls on the teacher or the pupil, neither of whom is able to bear it.

It is the liberality of the government of Hesse Darmstadt, in enabling Professor Liebig to open his laboratory on reasonable terms, that alone has made the establishment of a school of research possible at Giessen; and it may safely be stated, that till our professors are enabled to do the same, they will never succeed in forming such a school.

I have endeavoured to shew that the necessary expense is not very heavy, although beyond the means of our professors or of our students. I would fain hope, that where a university possesses funds applicable to such an object, the authorities, when convinced of its importance, will not hesitate to provide what may be required. But your Lordship is well aware, that in the Scottish universities there are no funds out of which this expense could be defrayed. I believe that every one of them, without exception, has made considerable sacrifices with a view of promoting the study of chemistry; but more is required, and I trust your Lordship will agree with me in thinking that the importance of the object justifies an application to government.

I would not be understood to object to the system, which has produced such good effects in the Scottish universities, of making the income of a professor dependent, in a great measure, on his diligence and success as a lecturer. But before this principle can be applied, the expenses necessary to carry on the course must, when they are considerable, be otherwise provided for. No professor, out of the income derived from students' fees alone, could support the expenses of a practical school, including laboratory, apparatus, materials and assistants; and if the fee be raised, the school will fail for want of students.

Even where lectures alone are given, the expense is such, that the necessity and justice of relieving the professor, of the whole or of the greater part of it, has always been admitted. Hence a laboratory and apparatus, however imperfect, is generally supplied; and there is, I believe, occasionally an allowance for an assistant, or for laboratory expenses. But in no case that I am acquainted with are the allowances such, that the expenses do not materially encroach on the income derived from fees, even in the case of the mere lecturer; while in most cases, not only is the accommodation quite insufficient, but the whole expenditure is borne by the professor; a burden from which, in almost every other branch of education, the teacher is necessarily free. The expense is naturally increased where practical chemistry is taught.

It may be said, that British chemists have hitherto, under the present system, borne their full share in the discoveries of modern science: and the remark is true, down to a certain period. When, towards the end of the last century, the researches of Lavoisier, Black, Cavendish, Priestley and Scheele, created the modern science of chemistry, it was for some time cultivated, with zeal and success, by a few distinguished men in all countries; and if the continent produced Berzelius and Gay-Lussac, we could boast of Davy and Wollaston. But a new era has begun, and while the continental chemists, just named, have formed flourishing schools, England cannot be said to possess a school at all. We can still boast of individual chemists standing on a level with the most illustrious names of the continent; but where are we to look for their successors? Not certainly among home educated chemists, for the great majority of our rising chemists have studied abroad, as already mentioned: moreover, the boast that we have borne our full share in modern discoveries has for a good many years ceased to be true. The history of the rapid development of organic chemistry, for example, is not one in which we have any reason to be proud of the place we hold, or the share we have taken up to the present time; and yet it is this very branch of chemistry that forms the distinctive character of the science during the last quarter of a century. The wonderful discoveries in electricity, galvanism, magnetism, light and heat, made during the same period, although in many cases the work of chemists, belong to physics rather than to chemistry. It is in pure chemistry that we have fallen behind, and no longer occupy the position of our fathers, and this, as I trust I have shewn, from the want of schools of research. Science is no longer confined, as it was fifty years ago, to a few individuals; and while our continental neighbours place it within the reach of all, we must do so likewise, or be content to fall still further behind, and to see our young men, such of them at least as can afford it, go abroad to learn what they cannot learn at home.

There never was a time at which the study of chemistry promised more splendid results than it now does. The scientific world is occupied, among other subjects, with the applications of the newly created science of organic chemistry to agriculture and physiology, as developed in a recent work by Professor Liebig. That most interesting volume shews that we are only at the commencement of an epoch, in which the labours of those who have established the principles of organic chemistry will be applied to practice, leading to results, the value of which to mankind cannot be estimated. Now, for the means which have enabled Professor Liebig to carry on the science of agriculture so far beyond the point at which the illustrious Davy left it—in a word, for the existing science of organic chemistry—he has been indebted, almost exclusively, to the labours of continental chemists; and if British chemists, as a body, have had so small a share in laying the foundation, we cannot hope that they shall take the lead in erecting the superstructure.

I have cited the school of Giessen as the model of a school of research, and I have done this for two reasons: first, because I could speak of it from accurate personal knowledge, and because its success is too obvious and universally known to be for a moment placed in doubt: secondly, because, even on the continent, it is generally admitted to be superior to any other; in fact, in most continental countries, the state of the schools of research approaches pretty nearly to that of our own. In Austria there are none. In Prussia, for want of a system like that of Giessen, the expense is thrown on the student or on the professor; and the consequence is, as with us, that the very distinguished chemists who fill the chairs of chemistry in the Prussian universities are unable to form schools of research, and rarely have more than one or two pupils, generally assistants, engaged in research. In Paris, although the ordinary students must bear the expense, as formerly stated, and consequently there are few on these terms, yet the very numerous professorships in the French metropolis, and the great demand for qualified assistants to these, give a stimulus sufficient to form an important school, although very far short of what a better system might produce.

I have great pleasure in being able to state that the example so liberally set by the government of Hesse Darmstadt, and the results which have followed the system adopted at Giessen, have not been lost on other states. The Austrian government has taken measures for placing its schools of chemistry on a better footing; the Prussian government has applied to the universities for advice and information on the subject. In Saxony much has already been done in the way of improvement; and there is every prospect of the establishment of an effective school in Leipsic under Professor Erdmann. Finally, in Hanover, a school has already arisen, at Göttingen, which began under the celebrated Stromeyer, and is now steadily improving under the auspices of Professor Wöhler. I trust that the British government will not be the last to see the importance of schools of research, or to adopt such measures as will render their permanent existence in this country possible.

What these measures are I have endeavoured generally to explain. Each professor ought to be supplied with a well-appointed laboratory and apparatus; with an assistant and a servant, or an allowance for them; and with an annual sum, for laboratory expenses, sufficient to protect him from loss, on the supposition of a certain limited number of working students.

I beg that your Lordship will particularly observe, that my only object is to enable the professor to teach the art of research without loss. This branch of the profession can never, from its nature, be a source of emolument to him, nor indeed of advantage, except indirectly by increasing his reputation; but he is, I humbly conceive, entitled to the small amount derived from fees such as I have mentioned, which form no adequate compensation for the labour and anxiety to which he is exposed.

In Giessen, when the working pupils amount to fifteen, the annual allowance is just sufficient; and the professor receives the fees paid by these pupils. When the number is greater, the excess of expenditure is paid by the professor, and, of course, with a large number, soon swallows up the fees of the working pupils—if it does not encroach on the fees paid by the students attending the lectures only, or on the professor's salary, his income being made up of the two last items. I have no doubt that the laboratory at Giessen is, with its present numbers, and has been for some years, a source of loss to the professor, whose income would have been larger had he confined himself to lecturing, with his present salary. It will be sufficient, however, for the purposes of science, if the allowance for expenses be such as, with a given number of working pupils, eight, ten, twelve, or fifteen, according to the place, would leave to the teacher the fees of students and his salary, if any. As any increase in the number of working students beyond the prescribed number is quite voluntary on his part, he might be allowed to pay the additional expense, as is done at Giessen. The general result would be, that if the prescribed number of working pupils were taught, the income of the professor would, at first, be increased by the amount of the laboratory fees; while his lectures would most probably, in a short time, be better attended than before, namely, by students, looking forward to the laboratory, and a small addition to his income would arise in this way also. His income, where no salary is attached to the chair, would depend, as at present, entirely on his success as a teacher; but we should possess schools of research, which we never can possess, as long as the attempt to establish them entails loss on the teacher. This, as I have attempted to explain, is the case at present, when the fee is made low enough to meet the means of the student. The plan of protecting the teacher by high fees has been tried, and has entirely failed.

Taking into account the usual rate of fees in this country, and the importance of making practical chemistry as accessible as may be, it appears desirable that the teacher should be enabled to receive working pupils at £1. 1*s*. per day weekly for a course of six-months; or, at all events, at not more than £1. 10*s*. per day weekly. A full course of six days weekly for six months would thus cost the student, in the first case £6. 6*s*.; in the second, £9. The latter is nearer to the standard of Giessen, if the difference in the value of money and in general expense be considered. But the former is certainly as much as is likely to be paid in many places; for example, in Aberdeen, whatever students in London might be disposed or enabled to give. In either case, no teacher can be expected to undertake the labour of a school of research, if even this pittance be not secure—that is, if the necessary expense be not otherwise provided for.

If, however, the necessary expense of laboratories of research be provided for, I have no doubt whatever that schools will soon arise in all our universities; that our young men will find the means of becoming accomplished chemists at home, and will no longer be compelled either to go abroad for their education, or to elinquish the study if unable to do so; and that in a very few years British chemists will again assume their proper place among the chemists of Europe. Every one who has had experience in teaching chemistry must have seen, as I have done, many young men, with first-rate abilities and a decided turn for chemical research, who were totally unable to bear the expense of a thorough chemical education at home, or to go abroad in search of it; and who, therefore, turned their attention to some other profession, the education for which was within their means.

I do not know what means the richer universities of Oxford, Cambridge, and Dublin, in none of which does a school of research at present exist, may have of establishing such a school; but the Scottish universities have no funds applicable to this purpose. All of them have some locality set apart as a laboratory, more or less complete in several cases; but in two, namely, King's College, Aberdeen, and St. Andrew's, very deficient. In King's College, the lecture-room, a very good one, is the only laboratory; and it is quite unfit for the accommodation of students engaged in research. A similar arrangement, I believe, exists in St. Andrew's. Notwithstanding this, however, the chief point to be attended to in Scotland would be the annual expense, as, on the average, a small sum would suffice to provide a laboratory. It is not easy to see from what source, save from the public purse, the annual expense could be defrayed. I beg leave, therefore, once more to express a hope that your Lordship may be inclined, in consideration of the very great importance of the object to be attained, to give your countenance to an application to government, for the means of establishing schools of chemical research in such of our universities as do not possess the necessary funds for that object.

In the event of an inquiry being made into the subject, I would beg leave respectfully to suggest that Professor Liebig should be applied to. His experience and success as a teacher of the art of research point him out as the individual whose testimony would be the most valuable.

I hope, before long, to be enabled to lay before your Lordship the plans of the laboratory at Giessen, with details; and in the mean time,

I have the honour to remain,

My Lord,

Your Lordship's most obedient and humble servant,

WILLIAM GREGORY.

Printed by J. L. Cox & Sons, 75, Great Queen Street,
Lincoln's-Inn Fields.