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Edmund Davy Esq.

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**LXXII. Experiments and Observations upon the State of the Air in the Fever Hospitals of Cork, at a Time when they were crowded with Patients labouring under Febrile Contagion.—**  
*By EDMUND DAVY, Esq. Professor of Chemistry, and Secretary to the Cork Institution.*

FROM numerous experiments made on air collected in different countries by the most enlightened inquirers, it seems to be generally admitted that the chemical constitution of the atmosphere is nearly the same at all seasons of the year and in all parts of the globe. Nitrogen and oxygen gases form its principal component parts; and it also contains a minute portion of carbonic acid gas and a variable quantity of aqueous vapour. As oxygen gas is essential to animal and vegetable life, and to the processes of combustion, fermentation, &c.; and as it is constantly entering into new forms, by which its peculiar properties are modified or destroyed, it is considered the most important and most active part of the atmosphere. The most general and important change that the oxygenous portion of the air undergoes, is its conversion into carbonic acid gas, a substance which, though obnoxious to animals, is yet made subservient to vegetable life; and this change is invariably connected with the exertion of the vital functions of organic beings, and with the burning of coals, wood, candles, &c.

The salubrity and healthy state of the air depend in a great measure upon the quantity of oxygen gas it contains, and this quantity (about twenty-one per cent.) appears to exist in all places exposed to the free atmosphere and the influence of winds. But the same uniformity of composition does not prevail in the air of confined dwelling-houses, crowded theatres, and hospitals that are badly ventilated. At a time when typhus was very prevalent in Cork, and there were in the two Fever Hospitals about two hundred and eighty patients labouring for the most part under febrile infection, it occurred to my friend Doctor Daly, whose active exertions in the cause of humanity are well known, and likewise to myself, that it would be a desirable object to ascertain the state of the air in the fever wards; and I immediately undertook a series of experiments on the subject.

To give in detail all the minutiae of my experiments would far exceed the limits of this paper; I shall therefore briefly notice my methods and results, and close the communication with a few observations connected with the subject.

I procured air from five large and small wards in the House of Recovery, and from the two wards in Peacock Lane Hospital. I collected it from different parts of the rooms; as in the middle,

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at the sides, near the floor and at different heights from it, and close to the beds of the patients. In every instance, the air was obtained by emptying on the spot bottles that had been previously filled with distilled water, and immediately closing them. The bottles were perfectly air-tight, being all furnished with well ground glass stoppers. The air was examined soon after it had been collected.

The first and most important object of my inquiry was to ascertain the quantity of oxygen gas in the several bottles of air. For this purpose I employed hydrogen gas, and the electric spark, a method that seems to unite more simplicity and elegance than any other, and with due precaution is susceptible of great accuracy. As the purity of the hydrogen used in experiments of this kind is of consequence to the accuracy of the results, it may be proper to notice the mode by which it was obtained, especially as it has, I think, some little novelty, and seems to be quite unexceptionable. I put some small pieces of zinc into a glass, and nearly filled it with water that had been boiling for some time; then filled a tube with the boiling water, and inverted it in the glass; and after adding sulphuric acid, I shortly after collected the gas.

I made a great number of experiments, using in every instance an excess of hydrogen gas. In every trial I mixed 0.30 of a cubic inch of the air under examination, with 0.30 of pure hydrogen gas; and after agitating the mixture in a thick detonating tube furnished with wires, the charge of a Leyden phial was passed through the tube, and the residual air, on being transferred to the cubic inch measure, occupied about 0.40 of it. I venture to state this as a general result; for though in a few cases there was a difference of about 1 per cent. more or less, yet this difference was rather apparent than real, owing to the difficulty of measuring uniform quantities of air, and it was corrected by a careful repetition of the experiments. Now, as two volumes of hydrogen and one of oxygen gas enter into the composition of water; if the foregoing results are made the basis of calculation, the apparent quantity of the oxygen gas in the air from the different fever wards will amount to about 22.22 per cent.—but this is not the real quantity. A slight allowance must be made for a minute portion of air disengaged from the water, after the detonation of the mixed gases; and when this is taken into account, the oxygen may be fairly estimated at about 21 per cent. And according to the statements of Sir Humphry Davy, and other able chemists, 21 per cent. is the actual quantity of oxygen gas in the external atmosphere in different parts of the globe. It may be remarked that the variations in the temperature and pressure of the atmosphere, during the preceding experiments, were

so small as not to influence the accuracy of the general results stated. With a view to confirm the preceding statements, I made comparative trials upon air collected from the open atmosphere at the top of the observatory belonging to the Cork Institution ; a situation, perhaps, not less salubrious than any other in Cork. --The experiments were conducted in a manner precisely similar to those I have noticed ; part of the same hydrogen was employed, and every precaution used to insure accuracy. And in every case in which the electric spark was passed through a mixture of the air under examination and hydrogen gas, in the proportion of 0.30 of each, the residual air measured about 0.40. I collected air from Hughes's Lane, a place notorious for the number of cases it had furnished of typhus ; but it yielded, on examination, the same uniformity of result.

I have made some trials on the other gaseous constituents of the air collected from the different fever wards, and compared them with similar experiments on air from the observatory of the Institution, and I have found a very near coincidence in both series of results. Thus, judging from the absorption that took place in the bottles of air from the fever wards, when placed for some time in water, and when agitated in this fluid, and especially from the effects of lime water on the air ; and comparing, by similar trials, air collected from the atmosphere in salubrious situations, I could scarcely, in either case, discover a perceptible difference in the quantity of carbonic acid gas. In one instance, I filled a two-quart ground-stoppered bottle with the air from a large ward at the House of Recovery, and, on the spot, I put into the bottle a small phial of lime water and well closed it. — After much occasional agitation and an interval of about two days, I examined the carbonate of lime formed, and compared it with the quantity produced under similar circumstances from the same bottle filled with air from the Observatory, and treated with lime water : and I was unable in this way to detect any appreciable difference. If this method may be relied on, I think I may venture to state, that the air from the ward did not contain nearly 1 per cent. more of carbonic acid gas than the air from the observatory.

After I had separated oxygen and carbonic acid gas from the different airs examined, I could not detect the presence of any other gas than nitrogen, which exhibited its characteristic negative properties. The want of leisure prevented me from varying and multiplying my experiments, so as to ascertain the exact proportion of the carbonic acid and nitrogen gases in the airs ; and it may be proper to observe, that during the time I was engaged in this inquiry, the variations of temperature, moisture, and pressure of the atmosphere were very small, and too often

connected with accidental circumstances to be accurately noticed.

*Observations.*

Though I did not indulge any sanguine expectations as to the benefit likely to result from a chemical examination of the air in the Fever Hospitals, I thought the inquiry might be useful. If the air in the wards had been found impure, means certainly ought to be adopted in order to improve it; but as this is not the case, the very knowledge of the fact may tend to lull suspicion where it is alive, and create some degree of confidence in the public mind. In this point of view, my investigation may perhaps have some little value, though I am far from attaching any undue importance to it. All my experiments seem to lead to this gratifying conclusion, that there is no material difference in the chemical constitution of the air in the crowded fever wards of this city, and the atmosphere in places that are very generally supposed to be more salubrious. I certainly was not prepared to expect this uniformity of result; but it seems to me to be intimately connected with the *situation*, and more particularly with the *ventilation*, of both Fever Houses. The site (as might be expected) is certainly very good in both cases, and the ventilation, especially in the House of Recovery, seems to be quite unexceptionable.

The necessity of a thorough ventilation in sick chambers, hospitals, &c. is universally felt and acknowledged, and the tendency of this inquiry is to prove its importance. It shows that the air of fever wards crowded with cases of infectious disease may, by a well regulated ventilation, still preserve its salubrity.

Respiration being in all cases a consumption of oxygen or vital air; this process, especially in crowded fever wards, is attended with great loss of oxygen; and a deficiency of this principle is equivalent to an excess of the other two noxious gases, carbonic acid and nitrogen. In circumstances where the uniformity in the composition of the air is every instant destroyed, it is difficult to conceive how it can be momentarily renewed, except by the quick and uninterrupted circulation of its parts. Perhaps, a *thorough ventilation* is, of all others, the most simple, and at the same time the most effectual means of preserving the salubrity of the air in crowded sick wards; and ventilators on the most approved construction, that allow a free ingress and egress of the air, and fires that quicken the circulation, would seem to be the most efficient methods for securing this desirable object.

In close moist weather, and in cases when, from different causes, the air of crowded sick chambers may be damp, or contain an excess of aqueous moisture, the use of quick lime in powder, I presume, will be found very beneficial; it will absorb the excess of moisture, and render the rooms comparatively dry.

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For this purpose, large surfaces of it may be exposed in shallow earthen vessels, in tubs or boxes. The lime will also exert the salubrious effect, of absorbing carbonic acid gas from the air.

Cork Institution, Dec. 6, 1817.

\* \* To these remarks of Mr. Davy I beg to add, that the results of his experiments tend to establish this truth : that *the matter of contagion* is imponderable, as those substances which, in their state of greatest dilution, merely affect the olfactory organs ; but I may also add, like these too, they may be capable of being taken up, neutralized, and precipitated, by chemical agents. Not only effluvia may be added to gases and liquids, but gases to gases, and liquids to liquids, without increasing their apparent volume. It is true that by the instruments of science the mixed gases and liquids may be presented separate ; but it would be unphilosophical to infer, because our means cannot yet separate and weigh or measure the matter of contagion, that therefore it has no existence.

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LXXIII. *Upon the Extent of the Expansion and Contraction of Timber in different Directions relative to the Position of the Medulla of the Tree.* By THOMAS ANDREW KNIGHT, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir JOSEPH BANKS, Bart. G.C.B.P.R.S.\*

MY DEAR SIR,—MANY attempts have been made by writers on vegetable physiology, to account for the force with which the sap of trees has been proved by Hale to ascend during the spring, without any hypothesis having been offered which has been thought satisfactory : and almost all which have been offered have been justly rejected as wholly inadequate. I have suggested in the Philosophical Transactions of 1801, second part, page 333, the expansion and contraction of these cellular processes which proceed from the bark to the medulla, which I have there called the true or silver grain of the wood ; and which have generally, though most improperly, been called medullary processes. I have there shown, that this substance expands and contracts very considerably under changes of temperature and moisture ; and I have stated that a board of oak, which has been formed by cutting across the supposed medullary processes, can scarcely be made, by any means, to retain the same form and position when subjected to various degrees of heat and moisture. I had not at that time ascertained, with accuracy, the comparative expansion and contraction of timber when divided in different

\* From the Transactions of the Philosophical Society for 1817, part ii.