Pulmonary Medicine -
or
Remarks on Inhalation of Remedial Agents.
by
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A summary of the subject is given at
the end page 80.
Various reasons induced me to take up this subject of Inhalation for my Thesis. I am aware that Inhalation has been tried as a means of cure & declared to have failed, that it is said to be exploded. But as much has been said of other things which nevertheless have eventually succeeded, there is something a priori so plausible in the idea of employing inhalation that seems is probable that some diseases at any rate by means of gases taken into the lungs, if the subject has such apparent importance, that I hope I may be allowed to take a brief review of what has already been written on this subject. I also to consider some of the objections that have been made against Inhalation. I am the more encouraged to write on this subject here in Edinburgh, as it is here that Chloroform has been so boldly tried in practice, with how great an alleviation of suffering, it would be superfluous here to state; and in this latter case, I have myself heard most discouraging remarks I seen how timidly and therefore almost uselessly it has been employed in French Hospitals. Inhalation seems to have been frequently tried.
more especially at the end of the last century in England and in Spain; later Louis carefully reviewed the results of experiments on the inhalation of chlorine in Phthisis; again inhalation especially of oxygen was tried in various parts of the Continent via Russia, Poland, Berlin, and even France, during the Cholera that raged about 20 years ago. Indeed so plausible does it seem that certain diseases of the lungs, as well as diseases caused by breathing impure or pestilent air, should be benefited in like manner by breathing certain gases, that we are not so much surprised at inhalation having been frequently tried, as at its seeming to have failed so completely. I am not now going to make a synopsis of the cases collected by Beddoes, Thornton, & others, to try & prove that inhalation did succeed in some diseases but shall take it for granted that the desired result was never obtained, such I find being the general verdict of the Profession. Only I think I may safely say that the effects of inhalation were not always purely negative, in the individual cases I mean, whatever they may have been in the general results, some individual
cases do seem to have derived benefit (temporarily perhaps) others as they themselves fairly state more harm than good, yet sufficient, I think, to justify my saying the result was not always purely negative.

In Louis' researches on Phthisis speaking of chlorine inhalations under the head of treatment he reviews first all Atteron's cases, they are 13, I may perhaps be allowed to make extract from one or 2 of them, in the 3rd observation we read "une dame, née de parents phthisiques, fut prise le 16 juin 1828 d'une toux sèche - un mois et demi après le début l'amaigrissement était con-
"sidérable, la faiblesse prononcée, la maladie avait "les extrémités froides &. Expectorait une matière "abondante, puriforme, souvent striée de sang. "Il y avait un son mat accompagné de gargou-
"illement et de prêter l'oreille au sommet du poum. "Ganée des souffles très espacées dans le sommeil.

Le 3 août on commence le chloro. Six jours "plus tard les crachats de décolorant et de détachant "facilement" - " du 20 au 26 l'appétit est trés "la toux, les crachats, les souffles nocturnes, la fièvre "du soir sont moins de 

"le 20 septembre "la santé paraissant tout-à-fait retablir à cesse
"treatied.

This case M. Louis suffers
an accident, of diahesive 4, Thinks it was perhaps a
pericardia or aneurysm. In the 4th case,
and acute attack of some sort, there was obvious
hemoptysis. About 3 weeks after the commencement
the patient being despaired of, chlorine was begun.
He recovered in little more than 3 months having
had a relapse. However dying of excess at table.

Here again there was doubt of the diagnosis.

6th Observation. Un Etudiant en Medecine age de 26 ans,
fut pris au mois de Mai 1847, dans sa terre,
devient ces dianzant au mois d'août. Alors et a
les intervalles variables, il en plusieurs hemoptysin
le 5 janvier, coquel, vert Theodore, plaques, abundant,
douleur intercostal, ancaite. Se - visque le
point douloureux ressemble a la vessie et gargarisme
le 9 on commence les chlore. "le 25 retour de
l'apect, Se. Le 26 janvier, au 19 Mars, une
diarrhoe forte et une fièvre a forme intermittent.
Obligant par intervalles de suspender les aspirations
et reduisant le malade a une maladie etreme.
Le 20 au 31 Mars les aspirations sont regulieres de
l'apect et les forces renaisant Se. a great
improvement took place till the 20th April 9 then
perfect health (sante parfaite). In this case
Louis does not deny the Phthisis, but doubts if there was perfect restoration when M. Allaven thought so. Similarly of the rest of the 13 cases, Louis says they the details necessary to prove the character of the disease treated, if in summing up he says, I find no real proof of the efficacy of chlorine in Phthisis. The same conclusion results from the experiments of M. M. Bayle & Foulmouche, & finally Louis himself having tried chlorine on 50 consumption patients pronounces it a failure.

Dr. Walters (diseases of the lungs & heart) considers these experiments of Louis conclusive, and during my own stay in Paris I neither saw, or heard of chlorine, nor in fact anything else being inhaled as a cure for Phthisis. And therefore conclude that Louis' opinion is the general one. But the question at present is not of Phthisis in particular. And observe that Louis' book is a work of such rigid induction as to form almost an era in Medicine in France especially since his researches were published a more accurate manner of observing and stricter reasoning from facts seems to have sprung up.

Louis in the work above quoted confines himself entirely to Phthisis, the diagnosis of which seems to have been rather uncertain before his time.
* From Louis' observations I mean
evidently he himself distrusts all previous accounts. But he does not say in the cases referred to above that the effect of Chlorine was entirely negative on the contrary in his remarks on Mr. Cisternac's 3rd observation he seems to think that the action of the cure proved the case could not have been phthisis, and in the 6th he thinks the treatment could not have been curative but palliative an opinion in which he was evidently justified for in the 50 cases tried by himself (Louis does not give the particulars of these cases) where he was sure of the diagnosis he never obtained the cure of a phthisical patient. Therefore though it is proved that Chlorine does not cure consumption it is not proved that it is useless in all diseases nor, a fortiori, does the same follow of inhalation generally.

Following the chronological order, we now come to the inhalation of Oxygen as tried during the epidemic of 1830-32 in various countries on the continent. Periera says it was tried in Russia, Poland, Prussia, and France without success; giving as his authority Mérat and De Lëns Dictionnaire universel de Mat. Med. wishing to see the account of these experiments I turned to the work & passage quoted.
and found under the article Oxygen almost as summary a notice as Percival's own, but giving more references, chiefly Notices given in to the Académie Royale de Médecine & to be found in their Recueils des Séances. For these I now searched and succeeded in getting the volume before and the one after, and any volume but the one quoted, however feeling sure that any experiments of the least importance in all the Medical Journals of the time I carefully looked over each of these as I could get hold of, and they were not a few, works, pamphlets, the Chirurgie Générale &c. English and Foreign, and found one author who thought Oxygen might do good another had not tried the inhalation, but had injected Oxygen into the bowels which he thought did good. In a 3d case Oxygen had been tried but the patient could not bear it for a sufficient time to be benefited, evidently in this case it had been used undiluted, but I found nothing from which I could even extract a quotation. At last in the Archives Générales de Méd. I found reports of the Séances de l'Acad. Roy. de Méd. that I wanted I will give two that I believe are the chief Authorities on this subject. One is the report of Mr. Sandros, sent with some others by the French
Government into Poland to enquire into the Cholera in that country, one of those where oxygen was tried, the treatment is rapidly summed up in less than a page under about 10 heads. All unsuccessful under heading 6. in 18 lines, "de nombreuses excitants" external as frictions, sinapisms, Resicitorie, Mozes acupuncture y & y. & internal as Salp y Enum Mint, Cameomile, y, Oxygen, Sulphate of Quina, iover powder, Muck. y y.

The letter is a paper read by Mr. Jouyet before the Académie during the Session of 10 April 1832. "Mr. Jouyet cherche à démontrer qu'il ya entre "le Choléra et l'empoisonnement l'Oxygene. Sulfite, "la plus grande analogie. Il faut respirer "d'Oxygene au moyen d'un appareil particulier." Mr. Coster (? ) écrit pour reclamer la priorité "de l'emploi de ce gaz dans le choléra, il fait "respirer a grandes doses un melange de 3 parties "d'Oxygene et d'une partie d'air atmosphérique "contenant dans des ballons de baudruche, il cite "un cas de reussite &. il est vrai de dire qu'il "a employe en même temps des Resicatories sur "l'epigastre, sur la poitrine et a l'interieur "des cuisses." And this is all.

I do not recall during the last visitation of Cholera
Any particular experiments having been made with oxygen, nor in referring to the Lancet did I find anything on the subject worth quoting. Taking it for granted therefore that oxygen has been demonstrated to be useless in cholera, I think I may say it is not yet demonstrated at least as far as the above experiments are concerned that its inhalation would be absolutely useless in all possible diseases. Chlorine and one or two other gases have also employed for cholera, but of these the notices are still shorter than of oxygen.

From this time till 1846 few experiments of this nature seem to have been tried. An ingenious Frenchman having discovered that consumptive patients, deriving benefit at sea-port towns or from sea voyages, derived such benefit from the smell of Far there abounding, prepared in 1836 to turn this discovery to account, and a few other equally valuable ideas were perhaps broached during the same period.

But we now come to one of the happiest discoveries in medical science, viz. That of anaesthetic agent, ether in 1846 by Drs. Jackson & Morton in America; followed about a year after by that of chloroform by T. Simpson.
Having once given a rapid sketch of the history of the medicinal use of inhaled agents, let us see how far it may be considered satisfactory or otherwise. Undoubtedly from time to time and even from antiquity we find notices of agents, especially anaesthetic, having been administered by the lungs, but these I shall pass over & come at once to those at the end of the last century, as being the first systematically tried and first attracting public attention on the discoveries of Priestly and others, many naturally asked some benefit might be derived medicinally from the newly found gases. But can the experiments then tried be considered decisive? Were the properties of these gases understood then as now? Was Physiology understood as well? Have no advances been made in our knowledge of diseases? How in their diagnosis? And could not correct diagnosis be indispensable in conducting such experiments? Besides as to the experiments tried they were soon interrupted on the continent by the breaking out of the French Revolution. Lavoisier, who paid special attention to this subject in France was put to death by Robespierre, who wanted patriots, not men of science!

At Beddoes pneumatic Institution in this country
the new remedies were not very boldly tried. He seems always to have used the common remedies in addition and in fact soon discontinued the practice, which appears to have been unpopular. The opinion of many in the profession, especially among the older fashionable practitioners, was perhaps not very inaccurately represented by a satirical letter he wrote as if from one of them. Besides doubtless 50 years ago the profession deserved more than they now do (I mean the mass of it) the low popular estimation in which it has been held compared with the other professions. The minute accuracy and the severe logical induction of the present day in conducting philosophical experiments if not wanting at least were less in degree. I have already said I do not think the experiments tried produced purely negative results. I think I may add that the very recently acquired knowledge even of the existence of the gases the imperfect ideas on many diseases, and the confused diagnosis prevented any experiments tried from being conclusive. But have any conclusive experiments been made since? and where are they to be found? Perhaps no gas has given rise to greater expectations than Oxygen and for no disease does it seem to have been
thought of more than for Cholera. Let us see what has been done with this one gas in this one disease.

Armstrong in his work on the disease of India seems to have originated the idea, in the treatise on Cholera, he notices the thick, black, avascular state of the blood. At page 140 he says: "In almost every case where I had to perform necrosection whether in Cholera, Syphilitic Fever, Diphtheria, or Rheumatism"—"my attention has been particularly attracted to the thick & black appearance of the blood," and in a postscript, to the same page he says: "would not the inhalation of dilute Oxygen gas, or of Atmospheric Oxygen be attended with advantage in the treatment of these diseases?"

In another place he says: he should have tried this treatment in Cholera had he had any apparatus at hand at the time for so doing.

On the advent of the Cholera for the first time into Europe, Physicians naturally turned their attention to the writings of our Indian Medical Men and took the idea directly from Armstrong, Oxygen was tried, but how wisely I have already shown to so little indeed does the Profession seem aware that the question has been decided that in looking over the remarks in the Lancet during the last invasion of Cholera I find just the same kind of conjecture.
about the use of Oxygen as were made in 1830-32.

The writer at the later period thinks Oxygen would be useless saying "We have plenty of Oxygen in the air but want Electricity." Can it be said this question is decided? How much less is it decided that the inhalation of any gas would be useless in any disease whatever, always excepting Chlorine & perhaps one or 2 other gases settled as regards Potterie by Louis & others.

Again did not Ether and the anesthetic agents spring into existence at a time when inhalation had been almost forgotten. Has not anesthetizing met with its opponents? I myself have heard much the kind of objections against Chloroform that I fancy would be urged against any Inhalation. I even much the kind of torpidity in its employment I should expect if inhalation was advanced for any disease. Yet in this instance benefit has been derived—might not benefits of different kinds be derived from different gases differently inhaled? Would it be possible to produce permanent effects by different modes of administration?

In the preceding history of the use of inhalation I have tried to show on what grounds I incline to the opinion that it has never had a fair trial.
And I must confine myself to this point, imagining that the belief that inhalation has been already fairly tried is the chief reason that prevents further experiments being made.

Other objections (some of them at least) would probably partake much of the nature of those urged against anaesthetic agents, against which the best I could do would be to repeat Dr. Simpson's able consideration that here at any rate they must be too well known to need repetition.

 Allow me however to notice a remark of the late Dr. Pereira in his Med. Oned. viz. It is remarkable that Electricity and Oxygen, two agents of vast influence in Nature, should possess but slight remedial power.

I am not now dealing with Electricity. Nor though this remark very naturally suggests itself, yet I think, the observed result is what we might a priori have expected, as far as Oxygen is concerned. Oxygen in a manner resembles food, we do not give bread, for instance, medicinally; for the benefit of the patient we may regulate the quantity of bread and pay attention to its quality, but similarly of Oxygen, do we not recommend some localities for the purity of the atmosphere? And in regulating the amount of exercise, do we not also regulate somewhat the
amount of Oxygen taken into the lungs?

Let us now consider the subject more on its own merits. And pass to the reasons which would lead us to expect benefit from, or induce us to try administering various gaseous or aeriform substances by the lungs.

If our bodies were nourished solely by food taken into the stomach, or if all necessary substances required to be assimilated before producing their poisonous effects, we must be justified in never attempting to act on the system otherwise than through the stomach or intestines; but this certainly is not the case, our nourishment is intimately connected with the nature of the atmosphere, from which a considerable part is directly derived, not only is Oxygen taken in, and Carbonic and water given off by the lungs, but it seems from experiments not impossible that the balance of Nitrogen in the system is affected by the air.

And in fact the purity of the air breathed generally enters into the consideration of the Physician, as well as the quality of the food, though he does not give remedies by the lungs, he gives directions for ventilation &c. and it is not part of the object of this ventilation to remove impurities from the atmosphere surrounding the patient, thus suffering certain substances are deleterious; for the object
is not always simple to procure fresh Oxygen.

Need I prove that certain substances are deleterious and not only negatively by taking the place of Oxygen but absolutely poisonous. Replace part of the Oxygen of the air by equal quantities successively of Nitrogen, Carbonic Acid, Sulfuric Acid, Hydrogen, or some of the Metallic Poisons (e.g. Arsenic) will the effects be alike in each case? Simply those of deprivation of part of the Oxygen? Or leaving the normal quantity of Oxygen, replace part of the Nitrogen by any of the preceding, shall we get merely negative effects? May we not cause almost instant death, or temporary or even permanent injury to the system according to the quantity given, or the time it is administered? And not only do we thus get results similar to that of poisons taken into the stomach, but can we doubt that diseases themselves are thus originated? Do not the Chloro, Dysentery, & Malaria poisons probably act through the lungs? To me it seems likely that many more diseases are infectious than Contagions, for Contagion (except in Viable N.) appears a most truly Homoeopathic belief, that difficulty we often experience in affecting the system through the skin as long as it & its unlike remain perfect.
Thus we observe that besides nourishment, many poisonous substances may be received by the lungs greater or less, and more or less permanent damage may be done to the system; and finally divers diseases may be inhale with the breath, yet can the invalid not be benefited, nor any disease be relieved by similar inhalation. But is this according to the usual beneficent working of Nature? Is it a philosophical conclusion to acknowledge freely that there are many deleterious substances, many causes of disease we can inhale by the lungs, yet to deny without the strictest proof that any remedial or palliative of a similar class are to be found? But here possibly it will be objected, that, the air is not meant to harm like our food, Nature seems to take every precaution to keep it at one unvarying standard, therefore deviations from this standard would naturally cause diseases, and we cannot expect any tampering with the atmosphere would be beneficial. Doubtless this may be urged, yet it does not seem a full answer to what has been said above, viz. that as by substances taken into the stomach, so may we be variously injured and yet divers diseases by substances taken into the lungs, and that I do not see why
I mean of course the time itself, not its effect, the
themselves which in practice have no end.
there should not be some that would not remedially
in the latter as in the former, perhaps the same
thing, differently administered.

But allow me yet a few remarks on this perfect
standard of our atmosphere. Are the effects of
climate all imaginary? Is it demonstrated even that
they depend merely on temperature and moisture?
Take an atmosphere acknowledged to be poisoned, e.g.
by cholera febris, or Typhus poisoning, has any analysis
detected this poison, does it not escape our senses and
all their aids. Would the analysis of a jar of air
from one of the streets of Edinburgh, foreign to this
standard, yet stand on one of the neighbouring heights
and what a cloud may be seen born far to leeward
obscuring the prospect for leagues whilst elsewhere
the skies may be beautifully clear. In this case
the contamination of the atmosphere is well seen.

The following analogous case is extracted from an
essay on Cholera by C. Scarle, Surgeon A.E.C.S.
"A frigate cruising along the Coast of India where
Cholera was raging, retained her health as was
believed by rigidly preventing any communication
with the shore. The Captain's conduct in this respect
having arisen from his experience of African fevers—
being on one occasion at anchor about 3 miles from
"Shore, the land wind came off to the vessel, and with such effect that the usual smell attending melancholy, well known to the officers and men from their African experience was immediately made sensible. There was not a sick man on board at the time, there had been no communication with land for weeks, the alarm being immediately taken the vessel was ordered to weigh for sea, and every man not wanted on deck was ordered below... An accident having happened to the iron cable the armouress was the first man employed, and though in perfect health when he came up, was immediately seized with giddiness was quickly incapable of proceeding in his work became insensible in three, and died of cholera, which also seized 4 others before the vessel could be got under weigh, who also died. The author cites this case in support of his own peculiar view with which at present we have nothing to do, but it well illustrates how a poisonous contamination in the air might be detected by the sense, which would probably completely escape the other, done in its remote effects. The substance might be detected by sight or smell, yet completely escape all analysis or attempt at collection, and if they escape sight & smell, they may elude our observation entirely unless by the effects which however need not always be definite diseases, but sometimes only.
Some modification of health, but most all foreign impregnation of the air necessarily produce diseases. May not some produce no cognizable effect, and therefore remain unknown; and may not some under certain circumstances even produce beneficial effects? May not the saline matter in the air derived from the breakers round a coast in some cases affect the system? Who has not derived pleasure from the perfume of the air during summer, and as decaying organic matter causes disease, may not on the contrary the odours with which summer breezes are loaded have some part in producing the healthy influence of that season? The pleasure that such breezes bring is certainly no argument against such a supposition. And are these eminent best adapted to move where no vegetation blooms? May not all these influences have their share in forming climate? So we are perhaps not entirely without warrant in supposing that some of the substances with which the air is at times charged may act beneficially.

Our taking nature for our guide should we perhaps be entirely without precedent in trying artificially impregnated air in medicine.

And even if we find no example in nature, does this exclude us from trying if any derivative bodies
have remedial qualities?

Can we expect all in the vast category of disease to be curable by stomach remedies? Is this indication derived from the fact that all diseases enter with our food? Surely not. Is not administration by the lungs as plausible a priori as per annum?

But the present practice is the result of the experience of mankind from the earliest ages. The medicines now employed are what we find to have been successfully used amongst various people, from whom we have borrowed them. Many of them, such perhaps as could only have got into practice during ages truly heroic. We have not now to combat general prejudice when we administer medicines by the stomach. The world is educated to the idea, and ready to take the most trust compounds.

And the prevalence of stomachic medicine is what would naturally be expected. In the infancy of knowledge men naturally confined themselves to such medicines as could be eaten. Naturally tested almost every substance they found that could by any chance be edible & so acquired experience; took various hints from animals as to the medicinal properties for, but how could they do this with the gaseous substances, whose very existence would perhaps at first escape them? The combination of the
Atmosphere itself was not discovered till very recently and it would require ages of observation to give us the same practical knowledge of the remedial powers of gases that we possess of the present medicines. It would be some time before prejudices, not merely on the part of the professers, but of their patients, could be sufficiently got over to enable us even to put confidence in our experiments. For how much more liable the patients are to imagine results in the case of a novel medicine than in an old established one—suffering even they use it exactly as ordered.

There are spots on the Earth where various gases issue from the surface. Suppose such to have been common that small gaseous jets were plentiful as springs that men had found remedial value in them, that pulmonary medicine had sprung up & been preferred the present medicines, if known, being rejected as poisons and nauseous; men had got used to these gases, to carry them about, to imitate them, & so accustomed to the modes of administering them, that any old woman could (or thought she could) give them efficaciously. What chance now would our medicines have? What would be the use of pointing out how many diseases were incurable by inhaled remedies? That the success of urging patients just to try a little black draught
of ascertaining the benefit of jalap or castor oil? Would it not be replied, that the very taste of these substances was nature's warning against their use? And difficulties must also be expected in the introduction of Pulmonary Medicine, but with the patients at least these would probably not be so great as in the previous supposition; there would not be the same repugnance to taste; the very manner of exhibiting, breathing, when the patient was once used to it, would perhaps inspire confidence rather than the contrary, the patient feeling it was in his power to discontinue the breathing the moment he chose. The difficulties would most probably be in the administration, in procuring the gases, the apparatus, &c. in the introduction of a system of which we have no experience, for one we are accustomed to, substituting doubtful for positive results, becoming for a time uncritical.

Let us then consider these objections; and first remember it is not the substitution of one system for another, Pulmonary is not to take the place of Stomachic Medicine, but to be an addition to our present means, and who will say that we do not want any addition? Certainly we can alleviate many diseases by our present treatment but in how many would it not be more shocking
to point to the success of our remedies? And who can say that more of the long catalogue of diseases which we are obliged to leave to nature, would be benefited by some (as yet) untried gas? Would it not be sometimes if one or 2 diseases should be added to the list of those amenable to treatment?

What medical man has aged in his profession, yet never known the deep mortification, the grief of that feeling of utter powerlessness caused by the sight of some disease that takes its victim as if none were present, and still he cannot help feeling they are not cases where any vital organ has been destroyed, that they are not cases that should be so helplessly allowed to pass away before our very eyes cannot help feeling Nature must have a remedy could it but be found.

And can we ever be justified in giving up the search after such remedies? I have in the beginning of this treatise (in the History) given the reasons why I fancy the experiments in Pathemary Medicine are as yet insufficient, and further on, why I think there are a priori grounds for hoping that continued experiments could discover perhaps a few diseases that might be thus benefited - Surely such a field of observation should not be left uncultivated.
When its importance to human life & happiness is considered—

It would be completely out of place for me, to make any, but the most general observations on the kind of experiments to be tried, on the diseases most likely to be benefited or the gases most likely to be useful &c. There is no would be likely to have charge of the experiments would have far less crude ideas on these subjects than I have. Sufice it to say that the diseases least amenable to present treatment would naturally suggest to be experimented on. As to the substances to be employed—something perhaps might be done by occasionally changing the proportions of the constituents of the air, but perhaps more from medicating it. And allow me here to remark (looking more to Nature's experiments) on the smallness of quantity of the contaminating principle that seems sometimes so powerfully to affect the system; doses to truly Homœopathic, that the symptômic theory of many diseases seems to owe its ready adoption to this cause. The agents themselves at our command would of course be the various known gases, & the volatile principles, vegetable &c. Especially such things as are already in use, and in impregnating the air with such matter, advantage might perhaps be taken of the high temperature of air, experiments have shewn. May
be breathed for a time at least, with impunity, though
don't then care should be taken that the substance would
not become solid in the lungs.

Some other questions will have interest, as whether the
steady, constant administration of a minute ingression
of the air breathed, differs in its effects from a
similar quantity given (otherwise or not) in one or 2
larger doses. & other questions readily suggesting
themselves. In a well conducted series of experi-
ments there might be some help (as sometimes
happens in other cases) of some unexpected discovery
repaying the time and trouble expended.

As to the mode of administering such agents, I
should little, I had thought of devising in some
drawings, but in searching authorities for my
subject I found I had discovered nothing new, or
in fact anyone might easily invent such apparatus
as best suited his own case, nothing complicated being
wanted. One difficulty may be noticed, viz.
the large quantity that might be required of some
gases, and the bulky apparatus therefore needed
and also that of keeping a supply or reservoir for
cases of emergency, in which especially, it seems
possible gaseous remedies might be useful, were
they at hand. For the present however, till
More experience has been obtained, hospitals &c. would I think be the best places for conducting the experiments and there at any rate these difficulties need not be insuperable. But I do not if very large quantities would generally be required.

Having thus attempted to bring forward the principal reasons for directing attention to Pulmonary Medicine, and to answer as best I might the chief objections occurring to one against it, I will now conclude with a few general observations.

I was led to adopt this subject whilst reading Arrowsley's diseases of India, for the purposes of another thesis, being struck with some remarks in it (I have quoted some at page 12) he compared Cholera to Tobacco poison & Cobra bites, now in London last Summer I was present at the death of the man bitten in the Zoological Gardens, & of course shall not readily forget the case. Other things at the time had led me to reflect on the number of the cases where we are summoned to be spectators of some of the most harrowing of nature's scenes, or though summoned for our assistance yet compelled to be as helpless spectators as any of those around us.

Having the pleasure of attending Dr. Simpson's present course, I heard him remark, talking of Chloroform
on its speedy and transient effect when taken by the lungs as compared with that when taken into the stomach, and unless my memory deceives me, he added, we may some day see more of this when inhaled agents have been more tried.

In fact Pulmonary Medicine seems to have suggested itself, been half tried, been forgotten. As happens to so many other things before they finally get recognised and gain their rightful place.

Not having it in my power to put it to the test, I resolved to adopt it as the subject of my Thesis as being that which lay in my power.

I do not think there would be much difficulty in persuading patients to try inhalation, as I have already observed the power of stopping when they knew would give confidence, and they are already used to the idea by the introduction of Chloroform, besides other agents are not required to cause loss of conscious, one of the objectives to Chloroform.

Nor do I think the most timid Practitioners need fear it, as the quantities might be regulated to any degree of nicety from pure air upwards, though probably the most timid would not be those who obtained the most useful results. They experimented boldly, who discovered the anaesthetic agents.
Here allow me a passing remark. The ethers had been used and inhaled long before their anaesthetic properties were discovered or their importance appreciated. May not a similar some day apply to other gases that are now said to have had their trial?

Even if Pulmonary Medicine fails on trial, the profession will escape the reproach of not having tried all the means in their power to arrest the course of some of the most terrible scourges of mankind.

For he it remembered the medical man not only practices for his own advantage, but he is a member of a body that are the pioneers of mankind in their advance towards the knowledge of all things in nature relating to Hygiene in its widest sense.

Time was when a mysterious assumption of knowledge inspired awe, now most probably it would inspire contempt. The multitude nowadays are fast treading on the footsteps of the men of Science, and faster and faster still will follow. And before long they may demand of their pioneers, why every tract has not been examined.

Much of what is here urged for inhalation, might perhaps also be applied to other things, to Electricity & its kith & kin, Magnetism &c. But Electricity is not yet sufficiently understood, I fancy, to be
I regret much that the lateness of the period at which I adopted this subject for my thesis, has prevented me properly consulting authorities. Had I done so, I could not have been bent in this way. Possibly owing to this defect, I have even written on entirely false data.
efficiently applied in Medicine, but we can hardly now have the same excuse for not trying Inhalation. The day may come, when imitating Nature we treat through the Stomach & Intestines, through the Lung & with Electricity & its co-operers &c. Then we may be ready to detect and battle disease, in whatever way it may direct its insidious attack, nor yet be without all help, though it may have reached the futter. When Medicine may at last become a monument of science worthy the human intellect.

My argument may perhaps be made more intelligible by the following brief Summary.

The preceding treatise should I think have been divided into the following heads.

* I. A short of the employment of inhaled agents medicinally, from the time of the discovery of Oxygen &c. to the present day. This is followed at page 10 by

II. A brief review of its value in settling the question whether Pulmonary Medicine has been sufficiently tried or not.

III. (Page 15) A few general considerations, mostly derived from Natural Analogies.
defence of Pulmonary Medicine, and in favour of still continuing experiments in it.

IV (Page 21) Answers to such objections as most readily occurred to me.

V (Page 25) A few, but very general remarks, on the substances &c. to be used; the diseases to be benefited &c.

VI (Page 27) Concluding observations.

Any principal authorities were Beddoes & Watt on fractious airs.
Life of Beddoes by Dr. Stock.
Louis on Phthisis.
Medical periodicals of the time of the Cholera of 1830-32.
The Lancet (Vol. 1, 1847) on the introduction of anaesthetics, especially an article by Dr. Gardner on this subject.
Also the succeeding volumes of the Lancet.