A letter to Thomas Trotter : occasioned by his proposal for destroying the fire and choak damps of coal mines; containing chemical and general strictures on that work / by Henry Dewar.

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LETTER

TO

THOMAS TROTTER, M. D.,

OCCASIONED BY

HIS PROPOSAL FOR DESTROYING

THE

FIRE AND CHOAK DAMPS OF COAL MINES;

CONTAINING

CHEMICAL AND GENERAL

STRICTURES ON THAT WORK.

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A Letter, &c.

SIR,

THE openness with which you deliver your opinions, and the solicitude which you evince to have them subjected to public discussion, seem to obviate the necessity of any ceremonious apology for presenting you with the following observations. But, I confess, the occasions are very rare on which I have the slightest wish to appear in the character of a professed critic. Important as criticism is in its influence on the cause of literature and science, a person of a generous disposition will often be delicately sparing of it in public, when he would freely communicate it in private. But the boldness of your

pretensions, and the freedom with which you scrutinize the pretensions of others, lay your works open to the most unreserved criticism. Your errors, also, are of sufficient magnitude to justify an explicit avowal of disapprobation from any reader who has at heart the interests of humanity and of science, and the respectability of our profession.

The importance of the object of your late publication, entitled "A PROPOSAL FOR DESTROYING THE FIRE AND CHOAK DAMP OF COAL MINES," is beyond dispute. Notwithstanding the copious resources which chemical science has opened for increasing the blessings and alleviating the evils of human life, this is an object to which it has not hitherto been successfully applied. If it could be demonstrated that the application of a few simple principles, ascertained several years ago by Lavoisier, is adequate to the destruction of these noxious substances, it would not be the only instance in which the application of familiar truths to the highest objects has been neglected; and the man who called the

attention

attention of the world to the subject would be entitled to praise.

It might however be expected that any author who undertook the task should previously inform himself respecting the present state of coal mines, and collect the various facts relating to the formation and accumulation of noxious airs. It might also be expected that he should be fully acquainted with those parts of chemical and general science which are connected with his subject. The application of chemical principles in explaining the phenomena, ought to exhibit that precise degree of certainty which the present state of chemistry authorizes; and speculative opinions, if introduced at all, ought to be delivered with a diffidence which would teach the reader to distinguish them from established principles. If chemical agents are proposed for remedying the evil, they should be fully investigated, not only in their action on the fire and choak damp, but in every other property which can affect the safety of the pitmen and the success of their labours.

 I am

I am sorry to observe that, with all the advantages of your local situation, you betray a very deficient knowledge of the form of the mines, the manner in which they are worked, and the expedients already employed for securing them against the evils to which they are liable. No person who has visited these places with open eyes could speak, as you do, of their winding and tortuous form, and the frequency of the pillars which support the superincumbent strata; and lament over these circumstances as obstacles to their ventilation.* The mines of Newcastle have nothing tortuous in their form. They consist of straight and parallel passages. These are intersected by others at different distances, and generally at right angles, unless the boundaries of individual property render it necessary to give them either an oblique or a slightly irregular direction.

You insist particularly on the *depth* of the mines as highly disadvantageous to the success of ventilation. You tell us that " the effect

* Page 24.

effect of ventilation from the shaft cannot be perfect at so great a depth below ground as one hundred fathoms ;"* that "this appears to you of vast importance in a mining district ;" that " on considering the subject" you were " immediately struck with the great analogy it has to the ventilation and purification of ships;"† and again that " the deeper the mine, the more winding and tortuous its area, and the more frequent its pillars of support to the superincumbent strata, it is evident the air below will be more speedily adulterated, and less within the sphere of perfect perflation."‡ As the modern principles of ventilation seem to be wholly unknown to you, I shall give an account of the process, founded on actual observation. The air of a coal mine is put in motion by means of a large furnace near the edge of one of the shafts, inclosed in a covered building which surrounds the whole mouth of the shaft, and provided with a large chimney similar in appearance to a glass-house. The heated air, thus ascending through the

 chimney,

* Page 5. † p. 6. ‡ p. 24.

chimney, is succeeded by cold air from the shaft, which in its turn is succeeded by air from the lowest part of the mine. The whole is thus successively removed, and its place supplied by air which finds its way from above, through another communicating shaft open to the day. The certainty of this operation has evidently no dependence on the depth of the mine, its extent, or its form. The brisk current thus produced below naturally takes the most direct course betwixt the two shafts. The ventilation on each side is therefore accomplished by means of another contrivance. A continued communication is formed betwixt the two shafts in any required direction, by opening the proper avenues and closing all others. A continued current is sometimes made to pass in this manner for twelve or eighteen miles. If you had visited the mines of Walls End, or, I suppose, any others in your neighbourhood, you would have seen the truth of this statement; and if you had descended into the deepest of them, you would have found that they are as completely

pletely ventilated as any place above ground. The ventilation is more certain and complete than that of a ship's hold by means of a wind-sail, as practised in the navy. With justice you observe that your preliminary studies in the navy "prepared you" only "*in some measure* for the present investigation."*

Believing that the mines could not be ventilated, you justly "cast a sympathising look" to the poor pitmen perpetually exposed to the most dreadful dangers. But we know that the disasters arising from fire and choak damp are only occasional accidents, perhaps not much more frequent than in some other hazardous employments. At any rate, they are much less so than they would necessarily be on your hypothesis of a ventilation so very defective. This is not mentioned to detract from the merit of that Philanthropy which aims at a preventive against all such accidents in future; but to shew that, for want of information, you are not fully qualified for the task. Some other assertions which you make concerning

* p. 6.

concerning these mines, founded on mere conjecture or shallow theory, and contradicted by facts, will be afterwards noticed.

Whether or not all the details into which you enter on the principles of chemistry were neccessary, is a question of little comparative importance; but they ought undoubtedly to have been accurate, if they were intended either to inform the reader, or to impress him with a favourable idea of the philosophical information of the author.

But, in investigating the operation of chemical principles in the formation of fire and choak damps, you do not shew the smallest care to confirm your opinions by actual observation. The first principles of the modern chemical philosophy, with which you commence, are indeed founded on incontrovertable facts. But it does not follow that a conjectural application of them to the explanation of any of the phenomena of Nature must, independently of all further experiment, be equally correct with the conclusions of Lavoisier. It was proved by that celebrated philosopher,

philosopher, that water is a compound body, consisting of hydrogen united to oxygen, and that carbonic acid consists of carbon in union with oxygen; and coal has been found by other experimental enquirers to consist principally of hydrogen and carbon. But you draw from these premises a very gratuitous conclusion, when you infer that the hydrogenous gas, or fire-damp of mines, consists of the hydrogen evolved from water in consequence of the union of the oxygen of the water with the carbon of the coal, and that this same union is the true and only origin of the carbonic acid or choak damp. The only fact by which you support it is that of the well known evolution of hydrogenous gas from the water of a ship's hold, or from water long retained in casks. But that evolution evidently depends on the putrefaction of wood, and has no analogy with the present case. Mineral coal is not described in our systems of chemistry as subject to putrefaction. You might, however, have satisfied yourself in some measure of the truth, by the easy experiment

riment of confining a quantitity of coal in a vessel of water under an inverted jar, and observing whether or not this was universally followed by the formation of hydrogenous and carbonic acid gas. The formation of these gases from the steam of water transmitted through a red hot tube containing powdered charcoal, in the justly admired experiment of Lavoisier, does not authorize any man to infer that the same process goes forward in the ordinary temperature of coal mines. Such rash deductions were never made by Lavoisier: they are confined to persons who know the modern chemistry by superficial reading alone, and are unacquainted with the agency of the numerous delicate circumstances by which chemical operations are influenced. If any fact in support of your doctrine was already ascertained by the experiments of others, it would have been more satisfactory to have stated these, as not being generally known, than to refer the reader to the decisive experiments of Lavoisier on the composition of water, which are perfectly familiar

to

to the youngest tyro in chemistry. You lay down, indeed, a presumptive argument in the following words : * " Charcoal is one of those " *bodies* that *has* the strongest attraction for " oxygen, which accounts for the very fre- " quent composition and large production of " carbonic acid gas or choak damp." This is certainly not the case with the substance which we usually denominate charcoal, which consists of carbon already united with a certain proportion of oxygen. You mention elsewhere a fact inconsistent with it, that the formation of noxious airs in water casks is prevented by reducing the internal surface of the casks to the state of charcoal by fire, an improvement which you yourself introduced into the navy.† By charcoal then you must mean in this place, the carbonaceous principle, either in a simple state, or under some form of combination different from charcoal, as in uncharred wood. That the carbon of pit-coal has this strong attraction for oxygen at a moderate temperature, remains to be proved.

You

* p. 22. † p. 19.

You give no proof whatever by telling us that "this is the simplest method of accounting "for the formation of these airs." Many theories sufficiently simple in their structure have been found irreconcileable with fact.

It is a necessary consequence of your theory, and indeed one which you advance for the edification of your readers, that fire and choak damp are always formed at the same moment,* and that, unless by some means the one or the other is removed, they must exist together in determinate proportions. But this does not actually take place. A quantity of fire damp is frequently found where no symptom of choak damp appears, and choak damp is equally common without fire damp.

You infer in another place, that "where-"ever ventilation is imperfect, it is plain that "these airs must accumulate in great quan-"tities."† But the accumulation of these airs is well known to be irregular, and frequently unexpected, and places which for a long time have received no ventilation are sometimes

* p. 23. † p. 9.

sometimes found free from both. If their presence regularly depended on deficient ventilation, such disasters as that which lately happened at Hebburn * would be very rare, as the cause which produced them would always be foreseen.

The most common explanation of fire damp, and one much more probable than yours, is that which accounts for it by the decomposition of water by means of the native sulphuret of iron, commonly called pyrites. It is somewhat analogous to the process by which we obtain hydrogenous gas for the purposes of experiment. The sulphuret attracts oxygen from the water, which acidifies the sulphur, oxydates the iron, and converts the whole into sulphate of iron, or green vitriol. In consequence of this, the hydrogen of the water rises in the form of gas. The existence of pyrites in great abundance in coal mines is well known, and the fire damp frequently indicates the presence of sulphur by its peculiar smell. You ought to have taken this explanation

* See Dr. Trotter's Proposal, p. 5.

nation of the phenomena under mature consideration, and disproved it by experiment before advancing an opinion totally unsupported by fact.

It would not be difficult to conceive several theories of choak damp equally plausible with yours; but as I have never heard of one distinguished by peculiar features of probability, it is unnecessary to mention any.

Some of the collateral opinions on chemical subjects intermingled with your explanation of these airs discover a gross inattention to fact; others are delivered with a culpable vagueness of expression.

You state, for example, that "when a cask "of spirits happens to leak into the hold "of a ship, the effluvium is extremely fetid "from the large proportion of hydrogen gas "that is evolved."* We all know that alkohol, or ardent spirit, contains much hydrogen; but we do not find this substance in common cases evolved from it in the state of gas, however long it is retained in contact with

*p. 13.

with wood. Unless this novel doctrine therefore is confirmed by experiment, we may still believe that the additional fetor, if it has existence, arises merely from the incongruous mixture of two smells, that of evaporated spirit and that of bilge vapour, on the same principle as the taste of two substances in mixture is often infinitely more nauseous than that of either in a separate state.

Perhaps I ought to be more cautious in disputing your theory of the fetid breath of the dram drinker, which you consider as originating from hydrogenous gas.* The experiments on which it is founded are perhaps detailed in your essay on drunkenness; but to a person unacquainted with that performance, this doctrine, if a judgement may be formed from its analogy with the other opinions contained in the present publication, bears a suspicious appearance.

To illustrate the properties of hydrogenous gas, you tell your readers that the noise of thunder, and the precipitation of water in

* p. 13.

thunder showers, are the effects of the explosion of hydrogenous gas in the atmosphere. * A similar opinion was, I believe, advanced by Lavoisier, but it it has long been abandoned by later philosophers.

You determine, in a very easy manner, the precise nature of the fire damp at Newcastle. "Hydrogenous gas has the property of dissolving and suspending in it carbon or coal, sulphur, phosphorus, and the substance called azote." "These" you say "are the substances most apt to *alter* its purity: and *in* proportion to the quantity of them, its perfect combustibility *depends*." And immediately you add that "in the coal mines of your district, it is probably generated in great purity, as is *proved* by its complete *inflammability* and *sudden explosion*." You ought to have known that its "inflammability" is not impaired by the substances which you enumerate as "impurities," azote excepted, and is very much increased by phosphorus. The "suddenness of its explosion" never

* p. 20.

never proceeds from its " purity," but always from an admixture of oxygenous gas, and is equally great when this last gas is mixed with that hydrogen which you denominate impure as with simple inflammable air. There are indeed various substances, the presence of which might diminish or destroy its inflammability. But these you keep entirely out of view. The substance called azote is foreign to your purpose, being evolved in union with hydrogen, (that is, in the state of ammonia,) only by the putrefaction of *animal* substances.

You state in one place that " different " substances of a vegetable nature greatly " promote the decomposition of moisture in " a ship's hold," and immediately add " Salt " water *thus* becomes sooner putrid than river " water. " * The fact is no illustration of your remark, unless salt is a substance of a *vegetable nature*.

Your account of the relative situation of different gases in a coal pit discovers a deficient knowledge of the statical laws of elastic

* p. 12.

fluids as ascertained by experiment. You inform us that "fire damp, being sixteen "times lighter than atmospheric air, must rise "to the roof of the mine, while choak damp, "being more ponderous, will sink to the "ground;" and that "if these airs do not "fill the space between the floor and the top "of the mine, a stratum of atmospheric air "will be between them, as being neither so "heavy as the choak damp, nor so light as "the fire damp."* This, however, is not universally allowed to be a law of elastic fluids. Different airs, indeed, separately and rapidly formed, arrange themselves at first according to their relative specific gravities; but they afterwards gradually penetrate one another, and when once intimately mixed, (as they must be in their first formation, according to your theory,) they continue in a state of mutual penetration, without shewing any tendency to resume the order of their specific gravities. Were not this the case, our atmosphere, consisting of three gases, all differing in

*p. 26. 27.

in specific gravity, would be divided into three separate strata, distinguished from one another by their chemical constitution. The carbonic acid contained in it would rest on the surface of the sea and the vallies, and every animated being that attempted to descend into these situations would be suffocated with the choak damp. Over this would lie the oxygenous gas, forming a vital region, and rendering the summits of the mountains exclusively habitable: and the higher regions of the air, consisting solely of azotic gas, or the gas deleterious to life, would destroy every bird, and every aeronautic adventurer, who should by any chance rise one inch above the limit of the oxygen. That nothing similar to this takes place is as well known to you as to any chemist. The uniform mixture of the three gases in our atmosphere is an undisputed fact: but, while Berthollet and others ascribe it to a slight chemical affinity, Mr. Dalton, finding it equally applicable to all mixtures of elastic fluids, ascribes it to some circumstance in the mechanical constitution of this class of

 substances.

substances. Do not suppose me ignorant that the fire damp of mines often ascends to the roof of the pit. This fact I believe, and I consider it as subversive of your doctrine of the slow, constant, and general formation of that gas. Besides, you do not recollect that even when it ascends to the roof it is not only in contact, but thoroughly mingled with atmospheric air, otherwise it could never burn with a sudden explosion.

These loose details do not prepare the reader for receiving with sanguine expectation your practical proposal; and it is with some degree of distrust that he peruses your flourishing transition:

" With this preliminary knowledge, we " are now in some measure enabled to turn " our attention to such prophylactic measures " as may be necessary for securing the work- " men."

Still, however, the candid reader would listen with attention to any valuable hint for the attainment of an object so important, and a small degree of practical utility would expiate

piate many speculative errors. Passing over, for the present, the extraneous matter which forms the bulk of this department of your subject, I shall shew why I am obliged, after the most careful perusal, to consider the utility of your proposal as in no degre superior to the accuracy of your preliminary doctrines.

For preventing the *formation* of these airs in the mine, you lay down a direction unfounded in principle and pregnant with inconvenience. As the formation of fire damp is on all hands considered as depending on the presence of *water*, it is certainly a singular advice to propose the introduction of fresh water as an article of prevention. * The coal mines of Newcastle, at least such as I have seen, do not greatly abound in water, either in a fresh or putrid state. Where it exists, it is an object to remove it, and this requires expensive machinery: but to supply its place with fresh water, is entirely out of the question. It would not diminish the danger; a greater

 quantity

* p. 25.

quantity of fire damp, on the contrary, would be formed, and the engines and pumps must then be more than doubled for the removal of a quantity of water unnecessarily introduced. The owners and superintendants of coal mines are too enlightened to listen to such directions.

You inform us that fire damp when formed is *discovered* by its fetid smell.* The smell however does not depend on the presence of *hydrogen*, but on certain substances dissolved in it. If your arguments for the peculiar *purity* of the hydrogen gas at Newcastle were satisfactory, it could not be very readily discovered by that test.

The direction which you give for the *safety* of a person who has discovered the presence of fire damp, that is, that he should "lie "down with his face flat on the ground,"† is highly useful, and is already universally known and attended to. But it is hardly consistent with your ideas of the mode in which noxious airs exist in coal mines. At p. 26 you say that

* p. 27. † p. 28.

that " fire and choak damps are necessarily " formed at the same time, and that, while " the fire damp ascends to the roof, the choak " damp rests on the bottom;" and, only two pages after, you direct the pitman, when he is alarmed by the discovery of fire damp, " to " lay his face flat on the ground. You do not consider that, if your previous doctrine is true, the face in this situation must be quickly covered with the paleness of death. The poor miner, to secure himself from the dubious alternative of death or a severe burning from the fire damp, must plunge into the certain destruction which awaits him in choak damp, like a despairing lover who escapes from the anguish of his mind, by throwing his body from a precipice.

Impressed with the importance of the object of your proposal, the reader cannot fail to be surprised when he finds you making foul smells and fire damp identical, merely because they are occasionally conjoined, and immediately proposing, for the *destruction* of fire damp, agents which extend only to the destruction of foul smells.

" In

" In order to destroy these smells" you tell us " we have only to employ some of the stronger " acids in a state of vapour, such as the " acetic, nitrous, or oxygenated muriatic. " These acid vapours seize the hydrogen of " the fire damp, which suspends the others, " and the fetid effluvia immediately disap- " pear." This sentence is followed by a theory of the *modus operandi* by which the hydrogen disappears: and the paragraph concludes with an assurance that " this is the " whole secret of destroying hydrogenous gas " or fire damp." No simple uninformed reader could suppose that you had fallen into a mistake in imagining that the hydrogenous gas disappears at all. In so far, however, as the acetous and nitric acids are concerned, you have misrepresented the fundamental fact; your fine theory therefore falls to the ground, and the reader is completely deceived who believes this to be " the secret of destroying fire " damp." Acetous acid is not known to exert any action whatever on hydrogenous gas. Nitric acid, on the application of a red heat, combines

combines with it with a violent explosion, an experiment not altogether desirable in a mine. If it remains in contact with the hydrogenous gas at the usual temperature of the earth, it can only augment the volume of unrespirable gases, and, diffusing itself gradually through the mine, it must render it wholly unfit to be worked, by its noxious effects on the respiratory organs of the pitmen.

You insist most largely, indeed, on the oxygenated muriatic acid gas.

In Dr. Thomson's System of Chemistry we are told that "If one measure of hydrogen gas "be mixed with two of the oxygenated mu- "riatic acid, and kept in a close vial for "twenty-four hours, and the vial then opened "under water, the whole gaseous contents "disappear."* On the accuracy of this experiment I have some reliance: (though indeed a degree of doubt as to the precise nature of one of the gases employed is expressed in a note.) If you could make an exact estimate of the quantity of hydrogen gas existing in any

* p. 84, IId. Vol., 2d. Edition.

any mine, and, without danger, trace its precise extent, you might, for any thing I know to the contrary, by introducing a double quantity of oxygenated muriatic acid, annihilate this portion of fire damp. But as it is evidently impossible to determine beforehand the quantity of hydrogen gas evolved, it is impossible to apply the remedy. If a quantity of the artificial gas remains uncombined, or if the gas is, in the first instance, set at liberty in the mines with the intention of destroying fire damp in its nascent state, the horses as well as the men below ground, if not completely suffocated, will be subjected to perpetual coughs of the most convulsive kind, whenever these animals are so unfortunate, or the men so imprudent, as to spend much of their time in such infernal regions. When this gas was first employed in bleaching, it was found highly detrimental to the workmen, in those trifling quantities which escaped by casual crevices, or in consequence of such occasional communications between the internal parts of the vessels and the external air as the nature

of

of the manufacture required. Hence the advantages resulting from the improvement made in bleaching by fixing the gas by means of alkaline solutions, and mixtures of water and quicklime. Fire damp cannot be destroyed unless mixed with a *double* quantity of this uncommonly suffocating substance in a volatile state; and it cannot be prevented, unless the mine is almost entirely filled with it. If the mines in your neighbourhood, which are "one hundred fathoms deep," and therefore "do not admit of perfect perflation," are to become the theatres of this experiment, it is high time to sing the funereal dirge of the trade of the river Tyne, and bid a long adieu to the opulence of the coal owners.

When your "proposal" was announced in the Medical and Physical Journal, an expression of regret was added at some "unworthy" opposition which had been made to the exertions of "the active philosopher." If permission was requested to make the singular experiment now mentioned, it is not difficult to account for the abrupt refusal it must have

encountered

encountered from the interested views of the parties concerned.

Whenever you resort to plain experiment, I feel all the reverence due to fact and truth, and all that respect to which the faithful interpreter of Nature has a just claim. On this principle, I do not doubt the efficacy of the oxygenated muriatic acid in destroying nauseous "smells" of various descriptions, and I believe that in foul kennels "any *person* who "has the curiosity to try may soon satisfy "*themselves*."* But if, in correcting such smells, they destroy *fire damp*, it would be kind to forewarn them that their experiments cannot be performed by *candle light* without the danger of an explosion.

The substance of your "proposal for the "destruction of CHOAK DAMP" has already fallen under our review. The owners and superintendants of coal mines must best know if any danger experienced from that evil renders it desirable to deluge their mines with water.† Where the choak damp is considerable,

* p. 39. † See the Proposal, p. 43, 44.

able, the water required for its absorption must be enormous; for in volume, you observe, it must exceed the choak damp by one half.† For this "proposal" however, they can only give you the credit of a good intention. Choak damp is easily dislodged, by subjecting the place in which it is found, or suspected to exist, to ventilation, which they are in the habit of managing with sufficient dexterity.

There is one consideration in the management of coal mines to which you might have directed your attention with great propriety. It may sometimes be desirable to open an extensive range of mines which has neither been ventilated nor visited for many years. If it were certain that they contained no foul air excepting choak damp, there would be no difficulty in clearing them. But the more probable apprehension, that they contain large quantities of hydrogen gas, renders it a matter of great risk to subject them to the usual process of ventilation. A mixture of this gas with common

†See the Proposal, p. 43, 44.

mon air, connected with an unknown volume of a similar mixture under ground, might, in passing over the furnace, expose the mine to an explosion little inferior in its consequences to an earthquake, and ruinous to all the buildings and machinery connected with it. This apprehension, I have heard, is seriously entertained. Perhaps oxygenated muriatic acid might diminish the quantity of hydrogenous gas, and, if a complete ventilation were then performed, the suspected mines might be made safe. But the practicability even of this plan ought to be well ascertained before experiments are made on so large a scale. I am informed that the expence of the process, if employed in such a manner as to insure the event, would be enormous. Every calculation on such subjects must be fallacious that does not take into account the prodigious area of coal mines in general, and particularly of those of Newcastle. This circumstance you have uniformly overlooked.* But a particular examination

* On this subject I beg leave to throw out a hint for the consideration of the owners and superintendants of coal mines. It appears to me that if the air at the mouth of the ventilating

nation of your calculations was not necessary to point out the futility of your doctrines.

Chemistry is not the only subject on which your reasonings are exceptionable.

A digression on *contagion* is introduced, for the purpose of informing the reader that you entertain a very indifferent opinion of the efficacy of acid fumes for destroying contagious effluvia and that no person must suspect you

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shaft were raised only to a temperature of 212° of Fahrenheit by steam, the ventilation of the most suspected old mines might be completed without the danger of explosion. Let the air from the shaft pass through a tall cylinder of tinned iron. Let this be contained within another cylinder sufficiently large to leave a considerable vacant space between the two; and let this space be accurately closed in all directions, with only two openings, one for admitting the steam from a boiler, and the other for allowing the condensed water to escape. It is hardly necessary to observe that the external cylinder must be surrounded with baked clay, brick, or some other slow-conducting substance, to prevent a superfluous expenditure of heat. This mode of ventilation would be slower than that in common use but it would evidently be much more safe in the cases here referred to. Would it not be equally certain in its effect? and, (where the price of fuel is no object) would it not be sufficiently economical? The progress of the ventilation might from time to time be ascertained by subjecting small quantities of the dislodged air to eudiometrical trials.

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of retracting sentiments elsewhere delivered, merely because you consider acid fumes as applicable to some useful purpose. Fire-damp and contagion are demonstrated by decisive arguments to be wholly different things. You "propose" acid fumes for the destruction of fire-damp, but steadily disapprove of them for the destruction of contagion. The merits of your "proposal" have been considered. Your arguments against acid fumes in contagion seem equally illogical. It is granted that these agents are not always necessary, and that free ventilation, with the use of soap and water, may generally supersede them. But it is not so easy to prove that they are ineffectual, and that the employment of them is in all cases improper. When we find you stigmatizing the practice with the charge of *quackery*, we naturally stop to enquire from what quarter such ideas, and such language can proceed.*

All

* Dr. Trotter's words are as follows:—"Morveau an adept "in Pneumatic Chemistry asks "*What then is the nature of those* "*invisible corpuscles, which, like organic beings, possess the* "*power of reproduction, and of assimilating to their own*

All the reason assigned for a sentence so extraordinary is, that we cannot explain the nature of the new combinations formed by the acids with the matter of contagion. No chemical agents must be recommended for destroying this substance, before we have determined the proportions of carbon, hydrogen, oxygen, and azote contained in contagious effluvia, the precise mode of combination on which the contagious quality depends, and the change of chemical constitution produced by the action of reagents.

" *essence every thing with which they come into contact, and* " *which seem to assume* life, *but for the purpose of propagating* " *death.*" " Such" he proceeds " is Morveau's creed on " contagion; and if any meaning can be drawn from such a " confession, he must have believed in an animalcular theory. " But we must at the same time remember he was not a " physician: yet he certainly ought, as a chemist, to have " explained the nature of the new combinations formed by " the muriatic acid vapours. This, *like the author of the* " *nitrous vapours* (Dr. Carmichael Smith) he withheld; pro- " bably *by* foreseeing that it would lead to inconsistency, and " an open avowal of *quackery.*" " Proposal" p. 35. — If Morveau believed in an animalcular theory, might he not believe that animalcules are deprived of activity and life by muriatic acid, without assigning any name to the resulting compound?

On the same principle we ought, in medicine, to begin with explaining the mode of combination in which the simplest principles of vegetables exist in *radix jalappæ* and every other article of the Materia Medica, and prove *a priori* that this chemical constitution ought to produce a particular ultimate effect. Every doctrine that rests solely on the basis of experience must be exploded. Some just animadversions on your opinions respecting the use of acids in contagion, as delivered in your Medicina Nautica, are contained in the 2d. Vol. of Mr. Aikin's Annual Review. When a sophism is advanced for the first time, it may be proper to take some pains to refute it. But a determined adherence to former errors, after their futility has been clearly pointed out, checks all the hopes which might otherwise be entertained of successfully inculcating the dictates of reason.

Dismissing now the substance of your "investigations," I beg leave to observe that your language, with all its pomp and glitter, is extremely deficient in the estimable

qualities

qualities of purity and precision. A few passages in which you have transgressed the common principles of grammar have been already quoted, and the words distinguished by italic characters. Several more might be mentioned, if it were necessary.

It is somewhat lamentable to find you so little sensible to the disadvantages under which you labour. The style of your prefatory address " to the owners and agents of coal mines" indicates the highest sense of your competence to the task you had undertaken. " The subject, I think, admits of demonstra-
" tion, at least as far as human knowledge
" can depend on the faith of experiment. I
" have not attempted to puzzle a plain under-
" standing by offering conjectures instead of
" truths, or to bewilder common sense by
" specious, but fanciful theories. Though
" a physician, my doctrine is intelligible,
" and my practice humble."* Here you seem to soar far above those feeble minds that are liable to be bewildered by fantastic

 speculation,

* See the whole address.

speculation, and incapable of discriminating the trivial from the important, or of tracing the real connections which subsist among the phenomena of nature. From this it might be supposed that, like Newton, you conducted the understandings of your readers, by a path plain and unerring, from the simplest facts to the most important conclusions. It is to be hoped that since that, address was written, some faithful friend has endeavoured to undeceive you.

In recommending chemical science as an elegant pursuit, you give the whole subject a fantastic air. Your exclamations* partake more of that transient ecstacy with which the juvenile mind receives the first dawnings of truth, than the calm approbation of an enlarged mind, and the steady ardour of an enquiring philosopher. By contenting ourselves with rambling, in an irregular enchanting revery, over a few ideas which we have acquired, we may deprive ourselves of all ability to co-operate with the philosophic minds

* p. 8. 13. 23.

minds by whom science is improved, in the straight path of experiment and induction.

The praise and censure which you distribute among other writers make a very ungraceful appearance in the midst of so many errors.

You consider the well-meaning zeal of the late Dr. Brownrigg of Whitehaven † as of little utility, because he only proposed to dissipate the noxious gases through the atmosphere, that they might cease to be injurious, instead of aspiring at the triumph of destroying them on the spot. But with you Dr. Brownrigg stands excused, because " Pneu-
" matic chemistry was at that time in its
" infancy." If the present state of that science is faithfully exemplified in your " proposal," it certainly has not yet passed its childhood. It speaks indeed; but its articulation is lisping; its ideas are incongruous; and whenever it attempts to walk, it totters and stumbles.

You begin a compliment to Guyton de

† p. 33.

Morveau, by intimating that his absence is compensated by your presence at Newcastle. "Had Morveau lived in a coal district, this "essay of mine might have been anticipated. "His active revolutionary genius could not "have with patience heard on the spot, of "hydrogenous gas bursting into flame, and "destroying thirty men at a blast, without "adverting to some measures that would "either prevent or alleviate such occurrences "in future. Though he is a passive slave "to the present tyrant of France, his other-"wise enlightened spirit DESERVES this ac-"knowledgment from a British Physician."*

If this acknowledgment should by any accident find its way to Morveau, methinks I see the sarcastic shrug with which he must pronounce the words *fort obligé*. A faithful imitator of his experiments, though of the humblest pretensions, might receive some borrowed lustre from his splendid reputation; but a writer who betrays deficient information in some of the first principles of chemistry seems

* p. 31. 32.

seems to push himself a little too far forward when he claims an alliance with him, and wishes to measure out his due portion of applause.

A very intelligible hint immediately follows, that Morveau and yourself belong to a description of men distinguished by their readiness to exert themselves in the cause of humanity without fee or reward. "Sordid "and selfish indeed must be the tenor of life "in that professional man, who can traverse "the acres of this neighbourhood for daily "bread, and not do one kind action *gratis*; "or sometimes cast a sympathizing look to "those industrious miners who toil amidst "darkness and dangers." No great positive merit is here claimed, as the reader is only reminded how sincerely you despise all professional men who are strangers to such generous sentiments. The contrast of your own character, however, with this prevailing selfishness reflects on you no small comparative honour. Nothing is lost in the ultimate effect. Your object is equally gained when you

you defame others and when you compliment yourself. But, I should hope that the medical men who " traverse the acres of your neighbourhood" are not all destitute of generous feeling. Some of them certainly devote a part of their time to a gratuitous attendance on the poor. Some there have been who have testified a liberal zeal for the erection and improvement of charitable institutions. If none of them have written or distributed such proposals as yours *gratis*, it should be recollected that it is not fair in any instance to dictate the particular mode in which a man ought to express his generosity.

The most remarkable instance in which you overleap the usual boundaries which diffident authors hold in reverence, occurs in the introduction to the practical part of your subject.* After the reader has followed you through a few of your rambling declamations, he finds you not only taking credit for brevity and propriety, but bringing into view other works published in your neighbourhood

hood that they may serve as a foil for illustrating the brilliant qualities of your own performance. "I might swell this essay to a great bulk, were I to digress into these subjects, or to contrast all that has been said and written. Even this neighbourhood has lately heard contagion discussed in a new style without much edification." The disputes on the laws of contagion, as affecting the establishment of fever-wards, are still fresh in the memory of the inhabitants of Newcastle. On this subject, you are probably surrounded with a greater number of formidable critics than any other part of the country can boast of. Those disputes, founded in difference of opinion, and connected on both sides with the most philanthropic wishes, were warm and interesting; and no pains were spared to collect arguments on both sides of the question. Contagion was indeed "discussed in a new style." The argument was purified from the dross of vague theory, and the parties were taught by each other to pay due respect to the results of experience. In affirming

that

that all this was followed by little "edification," you only satirize the prevailing indocility of your neighbourhood; but, I trust, without any real foundation on their part, as it is certainly without intention on yours.

The late Dr. Clark took a very conspicuous share in that discussion and his publications on the subject are more extensive and much better known than any others. As you thought proper, during his life, to embrace an opportunity of placing your own character in full contrast with that of this eminent physician, you must excuse me for considering the sentence on which I am commenting as an attack on his "Collection of papers on Fever-wards" and of course an attempt to detract from his posthumous reputation. It is not entitled to greater indulgence because it flows without effort, and appears only an easy effusion of sentiments familiarized to the mind of the author. I shall not stir up the embers of the dispute which you formerly attempted to exhibit to public notice; nor insist on the impropriety of stepping out of your way to attack after death a physician whom

whom you once affected to consider as your rival. I shall view your words simply as expressing your contemptible opinion of his "Collection of papers" compared with your "Proposal." In this point of view they furnish a well-marked instance of the predominant power of self-love in blinding mankind both to the excellencies of others and to their own deficiencies.

Although it may be hoped that your admiration of your unfortunate "Proposal" is now a little abated, it may not be so easy to convince you that Dr. Clark's writings ought to be treated with greater respect. But it may be proper to observe that your censure can have little influence on the opinions of others. Dr. Clark's publication betrays no proneness to throw out dashing uncertainties instead of sound reasoning. The evidence which he lays down, for proving the limited extent of the operation of febrile contagion, is clear and satisfactory. To preclude the possibility of erroneous statement, he delivered it in the words of those physicians who furnished him

him with the results of their experience. On this account it extends to a considerable length. It may perhaps seldom attract the idle reader, whose imagination must be amused with ingenious but crude speculations; but it will not fail to secure the interest of the industrious physician in quest of sound information. It presents us with a pleasing specimen of the sentiments and language of many of the first medical characters, and shows that they may universally agree in the belief of doctrines, confirmed by extensive reading and observation, which are not to be found, or are perhaps questioned and opposed, in the superficial publications of the day. It contains much useful intelligence concerning various medical charities, which must interest every philanthropic mind zealous for the welfare of such establishments. Your "Proposal for destroying fire and choak damp" has a very slender claim to the honour of a competition with a publication of this description.

It would hardly be consistent with decorum

rum to introduce a formal eulogium on the late Dr. Clark into the present letter. You have however evinced an ardent wish that a comparison should be stated betwixt him and yourself. Few other medical men would have hazarded your language respecting any eminent character, that you were in one respect "his equal," in another "his superior."* There is now no occasion for any ceremony in telling you that, in a talent for accurate observation and just reasoning, and also in classical purity of language, your "Proposal" declares you to be infinitely his inferior. Such is my opinion: though I shall employ no arguments with any hope of conveying the same impression to your mind.

Do not imagine that the freedom I have used with your writings proceeds from a wanton intention of detracting from your reputation. A physician's abilities may be respectable

* "His equal in professional rank: his superior in those "gradations of study and education that lead to the fair and "regular acquirement of medical honours." Medical and Physical Journal Vol. xiii. p. 134.

respectable, though not of the first order. He may be worthy of the esteem of his brethren, though justly accused of endeavouring, by ostentatious conduct, to procure a degree of admiration to which he is not entitled. My mind does not reproach me with having done you the smallest injustice.

The testimonies which you publish in your own favour are of the most respectable kind; and I believe your account of them, in the preface to your essay on drunkenness, to be as candid as the testimonies themselves are honourable. Dr. Webster was "delighted" with your inaugural dissertation: "Dr. Gregory perused it with great pleasure." "Dr. Cullen introduced your public ex-"amination with some elegant allusions to "your thesis;" and "shortly after" Dr. Hawes observed, when he "transmitted to "you the thanks of the Humane Society," that "such an investigation as yours was "*never* before thought of, and "that it was "a subject left, *happily* left, to be ingeniously "executed and amplified by Dr. Trotter."

A compliment from Dr. Cullen must have been highly gratifying to an aspiring mind, and was not to be forgotten. But I must observe that, if it had merely animated you to such exertions as would of themselves have recommended you to the esteem of the world, it would have proved more serviceable than when published by your own pen. The promising talents which Dr. Cullen complimented required good direction to bring them forward to maturity, and he could not foresee the exact degree of progress which, at a distance of so many years, the works of any pupil would indicate. As for the testimony of Dr. Hawes, which forms the summit of the climax, it would have been considered by many as too complimentary even for a private letter. You might have been excused for valuing it as an effusion of fond and partial friendship: but, by publishing it in a preface to one of your books, you discovered how little you were aware of the ridicule with which the world receives from an author such expressions of self com-

placency. A manly tyro warmly praised by his teacher, or by a visitor of the school, feels a glowing satisfaction which he has not power to conceal. But he does not publish among his companions the compliments which he receives. He knows that the uncorrupted good sense of boys would not treat with much respect any appearance of gross ostentation. As we advance in life, however, we frequently lose those delicate sensibilities.

While writing this letter, I have been told that, if it was my object to point out your improprieties, I might have found materials still more to my purpose in some other effusions of your pen. But I delight not to read the writings of any man with a view of exposing blemishes. When I shall read yours, I hope it will be with the purer motive of a desire of instruction, and not without some expectation of finding it. Those parts which previously came in my way appeared sufficiently characteristic to place in a clear point of view your imprudence

dence in wishing to contend in close combat with a physician of Dr. Clark's genuine abilities. If you had not indulged in that unnecessary and highly unmerited reflection on his public labours, it is probable that you would not have been favoured with the preceding observations; but, having sat down to express publicly my sense of your conduct, I conceived it would be useful to point out that series of mistakes which you have committed on important subjects, and to show from your writings that a man ought to habituate himself to think guardedly before he attempts to instruct the world. I am not conscious of having used any expression unworthy of member of a liberal profession. I have not, so far as I know, had recourse to sly insinuation, or low scurrility. You have been told a round unvarnished tale, which, if well received, may prove more beneficial than all the compliments of your partial friends.

I cannot conclude without applauding your zeal

zeal for the improvement of your profession, and your regard for the general interests of mankind. Among medical men, however, these virtues are far from being rare. They are cherished by numberless examples, and they are well known to open the readiest path to honour. But, to direct in an unexceptionable manner our researches into nature, to discover our abilities without ostentatious and empty display, and to wait with patience for the attainment of that rank in practice which the world will allow to our talents when known and appreciated, without departing from the observance of the strictest delicacy towards our rival brethren, requires an exquisite discriminating faculty, which in perfection is perhaps the endowment of few, but ought to be cultivated by all.

If the present attempt to suggest a few hints for your assistance in prosecuting this valuable object does not express that unqualified approbation which would be most grateful to your feelings, I hope you will do

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me the justice to allow that it is fully consistent with good wishes for your happiness.

I am Sir,

Your obedient humble servant,

HENRY DEWAR.