Reply to Dr. James Carmichael Smyth : containing remarks on his letter to Mr. Wilberforce, and a further account of the discovery of the power of mineral acids in a state of gas to destroy contagion / by John Johnstone, M.D.

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REPLY

TO

DR. JAMES CARMICHAEL SMYTH,

CONTAINING

REMARKS

ON HIS LETTER TO MR. WILBERFORCE,

AND

A FURTHER ACCOUNT OF THE DISCOVERY OF THE POWER

OF

MINERAL ACIDS IN A STATE OF GAS

TO

DESTROY CONTAGION.

BY JOHN JOHNSTONE, M. D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, &c. &c.

Πονῆι μὲν ἡ μέλισσα κηρίον μέλι
Φιλοφρονοῦσα προσφυη συνεργάτιν,
Ἐπειπερ ἄλλω τῶ τρυγῶντι χηρία,
Τὸ κέντρον ἐξέπεμψε, καὶ πλήττει μέγα.

Sennarii prefixed to Soranus and Oribasius, from the collection of Nicetas, published at Florence 1754.

London:

PRINTED FOR J. MAWMAN, NO. 22, POULTRY.

1805.

By T. Gillet, Salisbury-Square.

1576

PREFACE.

THE nature of the diſcuſſion contained in the following Pages will account for, and perhaps excuſe, the length of this Publication. I have not ſhunned any one of the topics which Dr. Smyth has introduced into his Letter, nor have I entered upon any freſh matter unneceſſarily. The manner of Dr. Smyth's attack required, that the ſubject ſhould be worked agoniſtically.

That what I have written will produce conviction upon the minds of all men, I do ſcarcely flatter myſelf; for what proofs or what arguments can convince him who is intereſted in not believing, or determined not to believe? That it will eſtabliſh my father's claim to diſcovery in the minds of all diſpaſſionate and ſcientific enquirers I have no doubt.

I am

I am ſurpriſed at the ſlight mention of the power of mineral acid gaſes which has been lately made in certain publications, profeſſedly written for the purpoſe of preventing the communication or ſpreading of peſtilential contagion. Do the mineral acid gaſes deſtroy the matter of contagion or no? Surely if they do, they are agents capable of the moſt important medical application, and not to be overlooked or only ſlightly alluded to, in the prevention of contagious fevers. If they do not deſtroy it, the error ought inſtantly to be cleared up.

Nor do the ſtricteſt attention to cleanlineſs, and the moſt conſtant ventilation, altogether ſuperſede the mineral acid gaſes as antiloimics no more than theſe ſuperſede attention to cleanlineſs and ventilation. Cleanlineſs and pure air are neceſſary for the continuance of health, but they do not prevent the formation of contagious matter. They are only the

next

next aids after mineral acid gaſes for preventing its accumulation. By paſſing a current of air continually through the ſick room, the peſtilential air may be ſwept away. By removing the bed-clothes and the linen of the patient, the matter of contagion accumulated in them, may be removed too. But this cannot be done every moment; yet the matter of contagion is formed every moment. Here, then, is the eſſential agency of mineral acid gaſes. They do meet the matter of contagion every moment, and deſtroy it, as ſoon as it is formed.

I muſt, however, reſerve the diſcuſſion of theſe collateral topics for another occaſion. In importance, they yield to no ſubjects of merely human enquiry, and demand therefore to be diſcuſſed with candour and deliberation, without prejudice and without paſſion.

J. J.

Birmingham, Sept. 24, 1805.

ERRATA.

P. 46, line 5, for "quote" *read* "use."

P. 46, line 14, *dele* "father's."

P. 52, note, for "τροπου" *read* "τροπος."

P. 204, note, after "present" *dele* :.

REPLY

TO

DR. JAMES CARMICHAEL SMYTH'S

LETTER

TO

MR. WILBERFORCE.

IT is not denied that the firſt public mention of the reſpirable mineral acid gaſes, as correctors of putrefaction, was made by Dr. James Johnſtone, ſen. late phyſician in Worceſter, in 1758. The ſecond mention of them was made by M. Guyton de Morveau in 1773. In 1779 Dr. James Johnſtone, jun. recommended muriatic acid gas, as the ſureſt method to prevent the ſpread of contagion. In 1795 Dr.

James Carmichael Smyth recommended nitric acid gas, to deſtroy contagion. In 1802 the ſame Dr. James Carmichael Smyth petitioned the Houſe of Commons to remunerate him, as the diſcoverer of the power of mineral acids in their gaſeous ſtate to deſtroy contagion. In 1803 I publiſhed my account of the diſcovery, and proved, I hope ſatisfactorily, the claim of my father to priority of diſcovery. In 1805* Dr. Smyth, in anſwer to my pamphlet, publiſhed his letter to Mr. Wilberforce.

Had I only to reply to the *arguments* adduced by Dr. Smyth in this letter, againſt the facts, that prove my father to have diſcovered the power of mineral acids in a ſtate of gas to deſtroy contagion, there would be no occaſion

* These are the dates of the different publications; but it may be proper for me here to intimate, that in some of the following pages I shall have occasion to adduce my reasons for believing, that Dr. Smyth did not employ *the reſpirable mineral acid gasses*, to destroy contagion at Winchester in 1780.

for

for long and elaborate diſcuſſion. By ſtating my caſe again—by correcting Dr. Smyth's miſconception of that ſtatement—by availing myſelf of the various documents which, after lapſe of more than thirty years, I have been able to collect; during which period it was never foreſeen that any diſpute would ariſe on the ſubject of diſcovery of the principle, and conſequently during which no precautions were taken againſt ſpecious plagiariſm, or oſtentatious impoſture--by bringing forward theſe documents as they were left by my father and my brother, undecorated, unvarniſhed, and written without any view to competition for reward, or controverſy upon fact; the ſoundneſs of my father's claims, and the juſtneſs of my reaſoning in defence of them, would eaſily be eſtabliſhed.

But a more unpleaſant and a more ſerious taſk is impoſed upon me, by the *manner* in which Dr. Smyth has treated the ſubject, in

his letter to Mr. Wilberforce. Not content with the diſplay of the whole force of his own peculiar logic, he attacks me alſo with the artillery of his rhetoric. The eſtabliſhment of the plain matter of fact, whether my father did uſe the muriatic acid gas to correct or deſtroy contagion fifty years ago, is nearly overlooked by this renowned champion of diſcovery; who, reſolved to gain the glorious meed, buckles on his armour and girds himſelf for battle, ſhouting defiance. Be it ſo:

. . . . . σέθεν δ' ἐγὼ ἐκ ἀλεγίζω,
Οὐδ' ὄθομαι κολέονος·

I ſhall not ſwerve from the tenor of my former ſtatement,

Ἦ γάρ κεν δειλός τε καὶ ἀτιδανὸς καλεοίμην
Εἰ δή σοι πᾶν ἔργον ὑπείξομαι ὅτλι κεν ἔιπῃς.

I am ready to meet *him* on fair and honourable grounds. But I cannot deſcend to miſerable and unprofitable revilings; to cavilling about one ſolitary word in a quotation, when that word

is

is of little or no importance; to invectives, as giving aid to quibbles, and dignity to trifles; or to other petty and diſingenuous expedients, which are too often permitted to diſgrace the controverſies of ſcholars, and men of ſcience. It is, however, incumbent upon me to refute, as it is impoſſible for me not to reſent, the invidious charge of being a wilful miſquoter, to vindicate myſelf from the indirect imputation of falſehood, and to repel the direct and reiterated reproaches which Dr. Smyth has ventured to throw out upon my inaccuracy and my ignorance. Under ſuch unexpected and unmerited provocation, I may ſometimes feel myſelf warranted in accuſing Dr. Smyth, as well as in defending myſelf. When I aſſerted the claim of my father, I did not admit one harſh expreſſion into my ſtatement, nor employ one unhandſome inſinuation, that could in the ſlighteſt degree wound the delicacy of Dr. Smyth.

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The queſtion of diſcovery of the power of mineral acid gaſſes to deſtroy contagion, was firſt brought before the public by Dr. J. C. Smyth himſelf; and the manner and the terms of his petition evidently imply a diſcovery of the principle, not merely a variation of the agent. The report grounded upon that petition, ſubſtantially and expreſsly aſſerts the ſuperiority of the agent, employed by Dr. Smyth, to the agent employed by my father; I therefore turned my attention to the ſubjects which the petition itſelf, and the report grounded upon it, preſented to my mind. Looking to the petition, I maintained that the principle had been previouſly diſcovered by my father; looking to the report, I maintained, that the agent known to my father, and uſed by him, before either of the agents appears to have been known and uſed by Dr. Smyth, was of equal efficacy with that for which Dr. Smyth contended, and upon the diſcovery of which

he

he grounded his pretenſions to parliamentary reward. But in no one paſſage did I then deny, that Dr. Smyth himſelf had by the ſtrength of his own mind, or in the courſe of his own obſervations, ſtruck out a ſimilar diſcovery. I did not then diſpute his claim to priority in the uſe of the nitric acid gas. I contended ſolely and unequivocally for my father's prior uſe of the muriatic. I did not repreſent Dr. Smyth as intentionally depreciating, or encouraging others to depreciate, the merit of my father; I did not ſcoff at Dr. Smyth's profeſſional talents; I did not controvert the reality or the magnitude of his profeſſional ſervices; I am not conſcious of having ſpoken in any one inſtance either of Dr. Smyth or his patrons, with the rudeneſs of an opponent, or the malignity of a rival; I ſuppoſed him to be a man of ſenſe and experience, and I confided in him as a man of candour and honour, who, without undervaluing

the

the diſcovery of my father, would be ſatiſfied with the praiſe, which really, and under all circumſtances, was due to himſelf.

Now my father's health was rapidly declining, when Dr. J. C. Smyth's petition was preſented to the Houſe of Commons, in February, 1802. He died in April, before the committee, to whom the petition was referred, made their report. Believing the prayer of the petition (ſo far as Dr. Smyth's diſcovery of the power of mineral acids in the ſtate of gas to deſtroy contagion was concerned) to be erroneous, I wrote upon the ſpur of the occaſion to Sir William Pulteney, becauſe he was the old friend of my father, and to Mr. Addington, becauſe his approbation in parliamentary buſineſs was eſſentially neceſſary. I ſtated to them in general terms, my father's claim to prior diſcovery. The miniſter did me the honour to acknowledge the receipt of my letter, by his ſecretary, Mr. Barton, and it was the

advice

advice of Sir William, that I ſhould write to Mr. Wilberforce, who had preſented Dr. Smyth's petition to the Houſe of Commons. To this gentleman I accordingly wrote, informing him of my father's claim, and at the ſame time of my deſire not to interfere with any proper claim of Dr. Smyth's. The following is a copy of Mr. Wilberforce's obliging anſwer:

House of Commons, March 23, 1802.

SIR,

I am aſhamed and concerned to think that I have treated you with incivility, in ſo long delaying to anſwer your polite letter. Nothing, I can truly declare, has been further from my intention; but having been exceedingly occupied for ſome days, and having deferred for a little time the ſubject of Dr. J. C. Smyth's petition, it really eſcaped my recollection that I had not written to you.

I now beg leave to return you my thanks for your obliging communication, and am indebted to you for favouring me with any information to aſſiſt me in the diſcharge of my parliamentary duty. In preſenting Dr. C. Smyth's petition, I was actuated not by perſonal motives, but by a ſenſe of public duty. Though certainly I had a ſtrong preſumption, I would not term it a full conviction of the truth, or rather accuracy of the ſtatements in Dr. Smyth's petition, that the nitrous fumigation as adviſed by him extinguiſhes contagion, that it may be uſed without inconvenience to the ſick, in their chambers, and that it was Dr. Smyth's diſcovery. The committee will be deſirous of fairly inveſtigating the ſubject, and if you can ſtate any facts or circumſtances which you think material for their information, I ſhall be much obliged if you will make me acquainted with them, and I will lay them before the committee.

It is my wiſh to examine fully, and to form an impartial judgment in the reſult of it. Dr. Johnſtone's character, I aſſure you without a compliment, gives too much weight to whatever comes from him, not to make me deſirous, on every ground, of receiving any elucidation he can give to any ſubject. I am much preſſed for time at this moment, and am writing at a committee-table amid interruption; I will only therefore add, that the committee on Dr. C. Smyth's petition will ſit on Tueſday next, before which day, any communication with which you may favour me, will be in full time for being produce before them.

I am with reſpect, Sir,

Your moſt obedient Servant,

(Signed) W. WILBERFORCE

In compliance with the deſire expreſſed in this letter, I ſent the teſtimony of my brother

Dr. Edward Johnſtone, that he had propoſed the uſe of muriatic acid gas to deſtroy contagion, in the Royal Infirmary at Edinburgh; and that he defended its uſefulneſs before the profeſſors of that univerſity in 1779. I ſent alſo my own teſtimony, that I had known it uſed in the malignant fever of my brother in 1776. At the ſame time, my father's Diſſertation on Fever, and my brother's Treatiſe on Malignant Angina, were ſent to Sir W. Pulteney, in order to be preſented to the committee. From Dr. Smyth's letter, I find that they were preſented. Here the matter reſted. My father died in the courſe of the enquiry, and the hurry and anxiety occaſioned by his ſickneſs and death prevented me from taking any other ſteps at that time.

Entirely unacquainted with the proceedings of Parliament in ſuch caſes, I did not for ſome time know that any report on the merits of Dr. J. C. Smyth's petition had been printed.

printed. But having caſually learnt from Sir *John Wrotteſley*, that there was ſuch a report, I procured it from him in Auguſt 1802. The tenor of this report convinced me, that my father's claim had not been underſtood, and I therefore made the comparative experiments on the reſpective convenience of application of the nitric and muriatic acids in a ſtate of gas; I farther obtained the evidence of profeſſional men, and of other men of ſcience; and I wrote the pamphlet entitled, "Account of the Diſcovery of the Power of Mineral Acids in a State of Gas to deſtroy Contagion." Such is the hiſtory of my pamphlet.

It was not to wreſt from Dr. Smyth the reward which he ſo well deſerved for his ſucceſsful application of *one* of the mineral acid gaſes on board the Union hoſpital-ſhip, that I made theſe attempts. Had his reward been ten times greater I ſhould have rejoiced, provided

that

that reward had been aſſigned to him, not for having primarily diſcovered the general principle, but for having ſucceſsfully employed one particular ſpecies of mineral acid. For in truth he was not the diſcoverer. The real diſcoverer was my father. His claim therefore I aſſerted in my pamphlet. I endeavoured to aſſert it with firmneſs, but without violence; I meant to prove, and not to declaim. In doing juſtice to the merits of my father, I did wiſh, and I did ſtrive, not to interfere with the fair fame of other men. It was my earneſt deſire that the matter in diſpute ſhould be decided by reaſon and the force of facts, and ſhould not be diſturbed and obſcured by any of the angry paſſions. I flattered myſelf that I had ſucceeded, for my pamphlet was ſeen and approved, before its publication, by men venerable from their age, their ſituation, and their talents; the ſanctity of whoſe character, and whoſe profound knowledge of life

and

and of ſcience, will at leaſt afford me ſhelter from wanton reproach.

By Sir William Pulteney's advice, I had prepared my account as a paper for the Royal Society in the firſt inſtance, and had ſent it for his peruſal, that it might afterwards be preſented to that learned body by their very celebrated preſident, Sir Joſeph Banks. The following letters determined me to publiſh it as a ſeparate treatiſe.

London, 30th March, 1803.

SIR,

I received yours of the 22d two days ago, and have read with great ſatisfaction the paper you ſent me on the comparative merit of your father's diſcovery of the muriatic acid for deſtroying contagion, and of nitric acid for the ſame purpoſe. I have forwarded it with your letter to Sir Joſeph Banks for the Royal Society, and I ſhould think that even if it ſhould be publiſhed, as I truſt it will, by

the

the Society, yet that it ought alſo to be publiſhed as a ſeparate treatiſe, not only for the honor of your father's memory, but for giving very important information to mankind on a ſubject of very great importance.

I approve very much of the ſtyle and manner in which you have detailed the particulars concerning this matter.

I am, &c.

(Signed) WILLIAM PULTENEY.

(Directed)

Dr. John Johnſtone,
Birmingham.

Soho-ſquare, April 4, 1803.

SIR,

The paper you forwarded to me relative to your father's claims on the public, as the real inventor of mineral acid fumigation, has come ſafe to my hands; it is ably drawn up, and appears to me very proper for the public eye on many accounts.

It

It does not however appear to me from its nature a proper communication for the Royal Society, or one that the committee of publication would eaſily be induced to print in the Philoſophical Tranſactions; it ſtates little, if any thing, new, is principally controverſial, and would certainly draw forth from Dr. Smith an anſwer, which, if the original is publiſhed by the Royal Society, he would with much reaſon expect to be alſo inſerted in the Philoſophical Tranſactions, from the pages of which we wiſh as far as poſſible to baniſh that kind of controverſy which turns on claims to originality of invention; beſides, Sir, it may be ſaid to be purely medical, and therefore far more proper for the countenance of ſome of the reſpectable medical ſocieties that are now formed in London, than for the Royal Society, who have not for many years inſerted medical matters in their works.

I ſhall therefore, Sir, hold myſelf in rea-

dineſs to return your book in any manner you may chooſe to point out to me, or to deliver it to any perſon whom you may ſend to me to take charge of it; and I ſincerely hope you will not delay to publiſh it as a pamphlet, or communication to ſome medical ſociety.

I find by converſation with medical men here, that mineral acids have been long uſed as fumigations in apartments liable to contagion, eſpecially in the Lock Hoſpital; but I do not hear of any inſtances ſo early as your father's uſage of it at Kidderminſter. I meet alſo with a variety of opinions reſpecting the facility of breathing muriatic vapour; on the whole I think a preference ſeems to be given to the nitric, though by no means ſo deciſive a one as was aſſumed by the Committee of the Houſe of Commons in Dr. C. Smyth's caſe.

I am, &c. (Signed) Jos. Banks.

Dr. Johnſtone.

My

My late highly eſteemed and learned friend Dr. Percival of Mancheſter, to whom alſo I ſent my pamphlet in manuſcript, returned it with the following obſervations: "I am "aſhamed of having ſo long delayed to return "your valuable and intereſting manuſcript, "which I have read with attention and with "peculiar ſatisfaction. You have vindicated "the claim of your late excellent father to a "very important diſcovery, in a manner which "does honour to your abilities, candour, and "filial piety; and I am confident that the "public will approve your exertions on this "occaſion, and that even my friend, Dr. Car-"michael Smyth, will find in what you have "done, no cauſe of umbrage or complaint." He was miſtaken. In 1805 came out Dr. Smyth's letter to Mr. Wilberforce, in anſwer to my pamphlet publiſhed in 1803.

I ſhall now go over the ground again of my former ſtatement, with the addition of freſh

evidence, and ſuch elucidations as Dr. Smyth's letter ſeems to me to require, to juſtify my aſſertion, *that Dr. James Johnſtone, ſen. my father, had, in the year* 1756, *diſcovered a method of deſtroying contagion, that could be uſed with perfect convenience in the apartments of the ſick.* The main evidence of the date of the diſcovery is contained in An hiſtorical Diſſertation concerning the malignant epidemical Fever of 1756, &c. by James Johnſtone, M. D. London printed, 1758.

" I have ventured," (ſays my father in the preface) " to make this hiſtorical eſſay public, " upon a preſumption that the obſervations it " contains may be uſefully applied in treating " fevers of the malignant claſs. It has been " my cuſtom ſince I have engaged in the prac- " tice of medicine, to write down ſhort me- " morandums of the caſes of moſt of my pa- " tients labouring under fevers and other diſ- " eaſes, that I might have it in my power to

" conſider

" conſider thoſe which were moſt remarkable
" at my leiſure. From a great number of ſuch
" caſes the deſcriptions and obſervations in
" the following tract are deduced and com-
" piled. In treating of the cure of malignant
" fevers, *I lay down no ſanative precept which*
" *I have not, in my ſphere of practice, expe-*
" *rienced abundantly uſeful, and generally ſuc-*
" *ceſsful.* The cautions which I enter againſt
" known practices or remedies are alſo drawn
" from a repeated obſervation of their miſ-
" chievous effects. Some theoretical reaſon-
" ing and inferences, however unſuitable to an
" hiſtorical piece, I have added, with a view
" of rendering the nature of malignant diſor-
" ders more generally underſtood than, per-
" haps, they are by many who will be under
" the neceſſity of treating them; and alſo,
" further to recommend thoſe rules, of the
" utility of which *I was primarily convinced*
" *by obſervation,* to thoſe who demand evidence

" of

" of a different nature. Many practitioners " love to know *that a peculiarity in practice is* " *rational, as well as in fact ſucceſsful,* before " they make it their own. In a word, I have " thrown together in a narrow compaſs, and " with all poſſible plainneſs, ſeveral important " experimental truths, which I hope may be " of ſervice in preparing many, ſucceſsfully " to combat theſe terrible diſorders in ſitua- " tions and circumſtances ſimilar to thoſe I " have deſcribed."

Such are the declarations of my father concerning his own meaning, and his own work; a work which he avows contains *peculiarity in practice* founded on his own experience, and in which he lays down *no ſanative precept* unwarranted by its utility and its ſucceſs in his own practice. In the fifteen firſt pages of the diſſertation an account is given of the chief diſeaſes that prevailed in the borough of Kidderminſter from 1752 to

1756.

1756. At p. 17, ſpeaking of the malignant fever of 1756, "The fever which prevailed "during this remarkable year, was very evi- "dently contagious, for whole families were "either altogether, or one after another, ſeized "with it. Thoſe that viſited or nurſed the "ſick in one houſe often carried the diſtem- "per along with them to other places, and "were ſeized with it themſelves." From p. 20 to 26 the ſymptoms of this fever are detailed at length. "In every degree of this "diſorder lowneſs, debility, reſtleſsneſs, nau- "ſea, head-ach, general propenſity to coma "or delirium," were its ſymptoms, and in the worſt caſes "black parched tongue, flat livid "petechial ſpots, and a quick low ſoft pulſe." The prognoſis to p 32; from thence to p. 51, the method of cure. Among other methods, at p. 41 the dulcified acids and the anodyne mineral liquor of Hoffman are recommended. "At the beginning of the fever the ſpirits of

"nitre,

" nitre, frequently taken by tea-ſpoonfuls in " mint tea or the patient's common drink, " will anſwer every purpoſe, and may alone " be depended upon," p. 43. " At the " height of this fever then larger doſes of bark " in ſubſtance, with elixir of vitriol and red " wine, were highly uſeful and neceſſary," p. 45. Towards the decline, when there were white ſpecks or ſloughs about the glands or palate, reſembling thoſe of ulcerated ſore throats, " theſe were treated with antiſeptic " detergent gargles of tinct. myrrh. ſpir. of " vitriol, oxymel ſimp. and the bark in ſub- " ſtance was given internally, with vitriol, in " larger or more frequent doſes than in any " other condition of this diſeaſe." p. 48, 49. In the next page comes the conſideration of the air, in which is included the firſt notice of the uſe of the mineral acids in a ſtate of gas to correct contagion. " The tempera- " ture of the air in which the patient breathes

" muſt

" muſt be rendered as favourable to his re-
" covery as poſſible, throughout the whole
" courſe of the diſeaſe; for this reaſon it muſt
" be continually renewed, eſpecially when the
" patient or patients are crowded in ſmall
" confined places, by opening the doors and
" windows of the apartments, ſo as to admit
" the free paſſage of the air. This practice,
" ſo neceſſary in the ſmall pox, is no leſs ſo in
" every ſpecies of putrid fever; but it muſt
" be done ſo as not to repreſs perſpiration.
" The neceſſity of changing the air in a ſick
" room, by ſucceſſive ventilation, ariſes from
" the conſtant deſtruction of a certain pro-
" perty in that fluid by breathing, which ren-
" ders it afterwards uſeleſs; likewiſe from the
" atmoſphere being filled with the excremen-
" titious ſteams which fly off from the pa-
" tient's body continually, and which putrify
" in a ſtagnant unrenewed air, and render
" it truly poiſonous, a pabulum morbi, ra-

" ther than of life. The phyſician will order
" the room to be kept ſweet and clean, and
" ſtools and every thing offenſive to be removed
" as ſoon as poſſible. If the external air is
" immoderately cold and wet, the room muſt
" be kept warm and dry, and the fumes of
" amber, myrrh, benzoin and camphire, may
" be diffuſed in the room, if ſprinkled upon
" hot-iron. Vinegar may be ſprinkled about
" cold, if the weather is warm, and boiled
" with myrrh or camphire, an antiſeptic
" ſteam will ariſe into the air, which the pa-
" tient breathes greatly to his advantage.
" Theſe ſteams will preſerve the air free from
" putrefaction, and will inſinuate themſelves
" by the abſorbent veſſels of the lungs into the
" blood-veſſels, and will greatly aſſiſt in im-
" peding the progreſs of putrefaction in the
" fluids. Theſe are the moſt commodious,
" if not the moſt uſeful, methods of medicat-
" ing the air the patient breathes; however,

" thoſe

" thoſe who prefer the mineral acids may " order brimſtone to be burnt, or may raiſe " the marine acid very eaſily, by putting a " certain quantity of common ſalt into a veſ- " ſel kept heated, upon a chafing-diſh of " coals; if to this a ſmall quantity of oil of " vitriol is, from time to time, added, the air " will be filled with a thick white acid ſteam; " but both the marine and ſulphureous acids " muſt be diſengaged at a conſiderable diſ- " tance from the patient, otherwiſe their ex- " treme pungency will be offenſive to the " lungs." p. 50, 51. At p. 53, 54, are ſome perſonal directions for eſcaping contagion; the reſt of the diſſertation conſiſts of theoretical reaſoning on putrefaction, and ſome general remarks and deductions.

Such is my father's work. I have analyſed it as ſhortly as is conſiſtent with a clear enumeration of the contents; and from theſe contents I confidently appeal to all men of

ſcience whether it be not indiſputable that my father not only admitted and underſtood, but had diſtinctly obſerved and actually experienced the power of the mineral acids in a ſtate of gas, as correctors of putrefaction, in 1756. In his preface he ſays, "I lay down no "ſanative precept which I have not, in my "ſphere of practice, experienced abundantly "uſeful." At p. 51 he lays down the ſanative precept to raiſe the mineral acids, and gives preciſe directions, ſo far as relates to the muriatic acid gas; and yet, after quoting the whole paſſage at p. 51 of the Hiſtorical Diſſertation, Dr. Smyth ſays, at p. 7 of his Letter to Mr. Wilberforce, "The preceding paſſage re-"quires little comment, a fair and candid ex-"poſition of the text being all that is neceſſary "to do away the pretended claim. Dr. John-"ſtone recommends the fumes of amber, "benzoin, &c. with the fumes of vinegar, as "the effectual means of reſiſting putrefaction

"or

" or deſtroying contagion ; and only mentions " the mineral acids of ſulphur and ſea ſalt " upon a ſuppoſition that ſome perſons might " chooſe to employ them. For ſuch perſons " he gives the common directions how they " are to be procured, and a caution reſpecting " the inconvenience or danger that may follow " from their uſe; although his directions on " this ſubject are too general and inaccurate " to be of much ſervice."

In the firſt paragraph Dr. Smyth talks of "*pretended claim*;"—in the laſt he acknowledges that "*common directions*," how they are to be procured, and a caution reſpecting the inconvenience or dangers that may follow from the uſe of mineral acid vapours, are given. By what alchemy of reaſoning Dr. Smyth can reconcile his aſſertion of pretended claim with the ſubſequent conceſſion of common directions, is to me utterly obſcure. My father's own words may convince the meaneſt capacity that, in 1756,

he

he uſed the muriatic acid gas for the purpoſe of keeping the air free from putrefaction. Now, my poſition is, that he did uſe this gas at that time, for that purpoſe. Does Dr. Smyth mean, by pretended claim, to ſay that my father's words admit of no ſuch explanation? That marine acid does not mean marine acid, and that to keep the air free from putrefaction is deſtitute of any alluſion to the general ſignification of thoſe words? If ſo, he contradicts himſelf by conceding that my father has given common directions. Can he pretend to ſay that the printer has inſerted a ſentence in my father's book contrary to my father's intention, and which my father did not underſtand? If theſe be the grounds of his aſſertion, "pretended claim," let him deduce the corollary from the problem himſelf. Of pretended claims, indeed, the kinds are many and various, and Dr. Smyth may conceive the nature of ſome of them without any violent

lent ſtretch of the powers of his imagination. It is poſſible, for inſtance, that men may pretend they make a diſcovery in 1780, after deliberate reflection, when the ſame diſcovery is to be found in obvious places long before. They may pretend that they have diſcovered a principle in 1795, when the ſlighteſt inveſtigation may convince the reader that they have only varied the agent. In fine, they may claim reward in 1802 for diſcovery; which diſcovery, when they aſk reward in * 1781, they never ſpecify, although the deliberate reflection that led to ſuch diſcovery is ſaid to have taken place at the remote period of 1780.

At p. 8 of his Letter Dr. Smyth quotes a paſſage from my pamphlet, in which I have aſſerted, "that near fifty years before the "framing of the report of the committee of

* See Dr. Smyth's memorial to Lord North.

"the

" the Houſe of Commons, a country phyſician
" (my father) had acquired eminence by the
" diſcovery of a certain method of deſtroying
" contagion, which could be uſed with perfect
" convenience in the apartments of the ſick."
I adopted theſe terms, becauſe in the report of the committee a ſentiment of Dr. Lind had been quoted nearly in the ſame words, and becauſe they formed the baſis of the reſult of the report, as a document proving that no diſcovery of a power to deſtroy contagion had been made before Dr. Smyth's. For if any other perſon had diſcovered a certain method of deſtroying contagion *before* Dr. Smyth, then could not *he* be the diſcoverer. Now in the ſecond paragraph I before quoted from Dr. Smyth's Letter, he does acknowledge that my father gives "the common directions how
" they (mineral acid vapours) are to be pro-
" cured, and a caution reſpecting the inconve-
" nience

" nience or danger that may follow from their " uſe."

Did any one ever give ſimilar directions before? No. Was the muriatic acid gas ever directed for the curative purpoſes of medicine before? No. Were theſe directions *common* when my father gave them? No. When, where, and for what purpoſe, were theſe directions, which I grant to be no common, given by my father? The time was 1756, the place was Kidderminſter, the purpoſe was to cure a malignant fever, and the book in which that purpoſe is announced appeared in 1758. Yet, with a ſtrange inattention to theſe plain facts, Dr. Smyth goes on, triumphantly declaring, that from my father's book, " no inference can be drawn that he " *ever* employed the mineral acids of ſulphur " and ſea ſalt; and that ſo far is he from re- " commending them to others, he cautions " us againſt the miſchief they may occaſion."

Theſe are Dr. Smyth's words, and the obvious import of them is, that my father *only* cautions, and by caution meant tacitly to diſcourage, rather than to recommend. Let us particularly attend to this ſpecimen of Dr. Smyth's logical talents. Perhaps the Doctor may have heard of certain wiſe ſayings very generally adopted in the language and the commerce of life, all of which indicate the neceſſity of caution. Perhaps, too, his own perſonal experience may have convinced him of the utility of ſome of them, for the conduct of life. If ſo, he muſt have diſcerned that there is a caution in ſelecting as well as rejecting, and that too much or too little, are as frequently the ſubjects of it, as decided and abſolute choice. My father in his practice as a phyſician, had doubtleſs learnt the neceſſity of caution. He had found it, doubtleſs, neceſſary to limit the doſes of medicines, and to regulate their application. In the appli-

cation

cation of a *new* and powerful medicament he was eſpecially called upon to exerciſe this caution, and he therefore adviſes the acid vapours "to be diſengaged at a conſiderable "diſtance from the patient, otherwiſe their "extreme pungency will be offenſive to the "lungs." And from theſe terms does Dr. Smyth infer, "ſo far is he from recommend-"ing them to others, that he cautions us againſt "the miſchief they may occaſion;" thus miſconſtruing a ſalutary caution into rejection, confounding the uſe of a medicine with its abuſe, and ſtrangely inferring ignorance, from that very circumſtance of diſcrimination, which is uſually conſidered as the criterion of attentive obſervation and accurate knowledge. How *could* my father caution againſt their extreme pungency had he not uſed them, and attended to their effects when uſed? Dr. Smyth cannot be ignorant that there are ſalutary doſes of muriatic acid gas, as of all other

 medicaments,

medicaments, and that it may be raiſed in a quantity ſo ſmall, as to be inefficacious, or ſo great as to be ſuffocating. The medium is the ſalutary doſe, that which my father's experience had taught him could be adminiſtered with perfect convenience, and which he virtually recommends, in guarding againſt a too pungent and irritating quantity. The precept which he gives therefore, for raiſing it at a diſtance from the bed of the patient, ſo far from affording any colour of probability to Dr. Smyth's language, "no inference can be "made," is particularly wiſe and uſeful, and ſhews that my father gave this caution, as I contend, experimentally, and, as Dr. Smyth muſt admit, juſtly; for the gas is thereby diluted before it reaches the patient, and has none of thoſe irritating effects on the organs of reſpiration which it would have if inſpired unmixed with a portion of atmoſpherical air. But how could my father know this, unleſs he

had

had ſeen marine acid uſed, ſometimes well, and ſometimes ill? Let me remind Dr. Smyth that Mr. Menzies, in his firſt trials of nitric acid gas on board the Union hoſpital ſhip, raiſed it too near the faces of the patients and thereby excited coughing*. Had Dr. Smyth's experience inſtructed him in the ſame cautious uſe of the nitric acid gas which my father's experience had taught him of the muriatic, he ought to have mentioned the caution, and then Mr. Menzies' practice would at firſt, have been ſafer. But this eſſential knowledge Mr. Menzies was left to acquire from his own experience, as he probably had never ſeen the caution of my father, and as he was left uninformed by Dr. Smyth. Knowing that the caution of my father was important as well as juſt, and that it was equally applicable to the nitric and muriatic acids, I expected to find

* Smyth's Account of the Experiment, &c. p. 12.

ſome

ſome mention of it in the full and elaborate account which Dr. Smyth has given of his treatment of the ſick at Wincheſter. If the nitric acid gas was really employed for them, Dr. Smyth could not fail to obſerve the ſame effect which my father had obſerved with the muriatic; and it is ſomewhat ſingular, that if obſerved by Dr. Smyth, it is not recorded in a ſtatement which no medical man can read without inſtruction, and which does the higheſt credit to the diligence, ſagacity, courage, and benevolence, of Dr. Smyth. I am content with noticing the omiſſion, and ſhall leave Dr. Smyth at his leiſure, and according to his diſcretion, to account for it. But to proceed.—It is nugatory, and almoſt captious to aſſert, that my father never uſed the muriatic acid gas, and that he makes no diſtinction between the ſulphureous and muriatic acids, when in the paſſage at p. 51 of the Hiſtorical Diſſertation, he gives preciſe directions

tions only for raiſing the muriatic. Without experience, he could not know that oil of vitriol poured on common ſalt would fill the air with a thick white acid ſteam *, and that this vapour was an uſeful method of medicating the air, and of preſerving it free from putrefaction. It is true that my father recommends vinegar imbued with many odorous gum-reſins, and his Diſſertation would have been

* This was not the chemical knowledge of 1756: even in Dr. Prieſtley's firſt experiments on air he had not hit upon this method of procuring the muriatic acid gas. In his "Experiments and Obſervations on different Kinds of Air," publiſhed 1774, he ſays, p. 229, § iv. "of Acid Air:"

"In my former experiments on this ſpecies of air, I procured "it from ſpirit of ſalt; I have ſince hit upon a much leſs ex- "penſive method of getting it, by having recourſe to the pro- "ceſs by which the ſpirit of ſalt itſelf is originally made. For "this purpoſe I fill a ſmall phial with common ſalt, pour upon "it a ſmall quantity of concentrated oil of vitriol, &c." which was in fact my father's method, and uſed long before Dr. Prieſtley's.

imperfect

imperfect without the mention of them, for medical knowledge was not then ſo matured as to warrant the rejecting them altogether; but his words obviouſly imply a clear knowledge and a ſtrong approbation of the mineral acids as uſeful agents, if employed with caution, although in this, as in many other parts of the Diſſertation, the phraſeology might have been choſen to greater advantage.

As to vinegar* he certainly ought not to have given it up as an uſeful agent, well knowing that the practice of phyſic requires a variety of agents, and that thoſe of weaker powers are ſometimes more commodious in application, and therefore, in certain caſes and ſituations, preferable to the moſt active. His experience muſt ſoon have convinced him of the

* The experiments of Guyton de Morveau prove that vinegar is not deſtitute of the power of deſtroying putrefaction, although leſs efficacious than the mineral acids. Traité des Moyens de Deſinfecter l'Air, p. 145, et Exp. 18, 19, 20, 21.

diſagreeable-

diſagreeableneſs of the ſulphureous acid gas, and of the impoſſibility of burning brimſtone in any conſiderable quantity in inhabited rooms. But it is equally efficacious in diſinfecting thoſe that are uninhabited; and therefore, on account of the principle, my father mentions the ſulphureous and marine acids together. Let it be remembered too, that brimſtone was one of the fumigations uſed by the old phyſicians, but not recommended by them as affording a mineral acid when burnt, whereas my father recommends it, becauſe when burnt, it *does* afford a mineral acid, which mineral acid in a ſtate of gas deſtroys contagion.

Before Dr. Smyth infers that my father conſidered the marine and ſulphureous acids as equally ſafe and equally uſeful, in the firſt place he muſt admit that my father had tried both; and ſecondly, he muſt contend that he was too ſtupid to ſee that difference

in the reſult of the trial which every patient muſt have experienced, and every nurſe muſt have obſerved, if the trial had been made without any regard to proportion in the quantity. He ſays that brimſtone may be burnt, and Dr. Smyth knows as well as I, that it often had been burnt by the old phyſicians, for the purpoſe of driving out contagion. But my father does more. He ſays it may be burnt for the purpoſe of medicating the air, that is, correcting contagion, and this power he aſcribes to brimſtone, *only when converted into a mineral acid.* I will grant to Dr. Smyth that my father would have written more ſatisfactorily, and more uſefully, if he had written more copiouſly and more explicitly, upon the comparative qualities both of the ſulphureous and muriatic acids. But I contend that he was acquainted in 1756 with their proper and ſpecific power of deſtroying contagion; with

that

that power which they contained as mineral acids, the very term be it obſerved, by which my father deſcribes them.

At p. 9 of his letter, Dr. Smyth proceeds thus: "Having laid before you the paſſage "in the father's book, and pointed out the "only concluſion that can be drawn from it, "I ſhall in the next place examine the ac- "count, or rather miſrepreſentation, of his ſon, "Dr. John Johnſtone. After mentioning the "title of the father's Diſſertation, publiſhed in "1758, he proceeds: 'In that Diſſertation, "adopting the theory of the day, he propoſes "to keep the air free from putrefaction by the "ſteams of vinegar, or, as a more effectual "method, the marine acid may be raiſed very "eaſily, &c.'" Let it be obſerved, that in the firſt part of the ſentence above quoted from my pamphlet, I merely declare the meaning of my father; I do not quote his words till I come to the phraſe "the marine acid, &c."

 I believe

I believe I did not err in ſtating that my father, by recommending vinegar, adopted the theory of the day—the theory of Fothergill and of Huxham, which Dr. Smyth has ſo archly marked in Italics. And that he propoſed in the mineral acid gaſes a method which is in fact more effectual, can any man have the effrontery to deny ?

Enough has already been ſaid on my father's caution reſpecting the uſe of mineral acid vapours, and I truſt it is clear, that ſo far from "flatly contradicting my aſſer-" tion," (that the method of deſtroying contagion, diſcovered by my father, could be uſed with perfect convenience in the apartments of the ſick,) as Dr. S. chooſes to expreſs himſelf, this caution proves my aſſertion. I truſt it is alſo proved that my father's terms *do* convey his conviction of the efficacy of the mineral acids in a ſtate of gas, to deſtroy contagion; and whether, according to the ſtrong language

of

of Dr. Smyth, my account is "ſuch a com-
"plete perverſion of an author's meaning,
"profeſſing merely to be a declaration of it,
"(as) has never before been preſented to the
"public," muſt be determined on a fair and accurate compariſon, by an unimpeached and impartial judge.

It will be immediately ſeen, by thoſe who may take the trouble to compare the two paſſages in the two pamphlets, that in Dr. Smyth's letter to Mr. Wilberforce the whole of the paſſage beginning at the words, "In that Diſſertation," is printed with inverted commas, without any diſtinction of parts, and yet upon the whole as a quotation, interpolated by me, in order to pervert my author's meaning, Dr. Smyth criticiſes arrogantly and contumelioufly. Violence is ſeldom the criterion of a juſt cauſe, and at leaſt it behoves a man to be correct before he become violent. By inſpecting page 5 of my pamphlet,

pamphlet, it may ſeen that the words " more " effectual method," are not quoted by me as the words of my father's book, and are not, therefore, an interpolation. They are my own words, and I quote them as ſuch, and as I conceive, at the ſame time, declaratory of my father's meaning. I do not quote my father's words till inverted commas are placed at " the " marine acid, &c." Inſtead then of my father's meaning being miſrepreſented by me, Dr. Smyth has miſrepreſented mine. He firſt makes me quote that which I do not, and then derides me for the blunder which he himſelf has made me commit. My father's words, thus miſquoted by Dr. Smyth, were intended by me to be declaratory of my father's meaning, and I ſtill think myſelf juſtified in thus interpreting the paſſage " theſe are the moſt " commodious, if not the moſt uſeful, methods " of medicating the air the patient breathes; " however, thoſe who prefer the mineral acids, " &c."

"&c."* The vinegar is the moſt commodious method doubtleſs, but if it be not the moſt uſeful, what is? There can be but one opinion, the mineral acids. The language is imperfect I grant, and in an interleaved copy of his Diſſertation my father has himſelf inſerted the words "ſome of," before "the

* I have ſhewn theſe words to ſeveral perſons of the learned profeſſions, and they thought as I did, that in my father's opinion they implied a preference of the marine acids to medicated vinegar. I have ſhewn them alſo to another friend, who is not very likely to be cajoled by ſophiſtry, or diſmayed by dogmatiſm; but that friend, with all his habitual admiration of my father, and all his ſtrong indignation againſt the general ſpirit and general reaſoning of Dr. Smyth's pamphlet, declares to me, that in his opinion the words under conſideration do not decidedly imply preference. I have not concealed this opinion becauſe it was contrary to my own, and becauſe it did not ſuit my purpoſe; nor will I conceal from Dr. Smyth, that my conviction of the ſagacity, wiſdom, and uprightneſs of the learned friend from whom I differ in this one inſtance, generally induces me to repoſe under his authority, and to ſubmit to his guidance.

"moſt

"moſt commodious." Were I, however, *ex hypotheſi*, to concede to Dr. Smyth, that from my father's words in this place, a reader might be juſtified in ſuppoſing, that the uſe of vinegar in my father's opinion, was more commodious if not more uſeful than the mineral acids, the queſtion would then turn upon my father's opinion of their *comparative* uſefulneſs, but his knowledge of their *poſitive* uſe would ſtill be indiſputable. He muſt yet have known, that the mineral acids had the power of medicating the air, that they who preferred them might uſe them commodiouſly and uſefully in ſome degree, be it great or little, and that by being prepared in a certain way, and diſengaged at a conſiderable diſtance from the patient, both acids would deſtroy contagion. Thus much is proved by my father's words, if underſtood in the moſt unfavourable ſenſe which an intelligent and impartial critic can aſſign to them. They juſtify my aſſertion

that

that my father had diſcovered a certain method of deſtroying contagion, which could be uſed with perfect convenience in the apartments of the ſick, by obſerving his caution as applied to them reſpecting it. They confute Dr. Smyth's poſition, that no inference can be made from them, that my father *ever* employed the mineral acids of ſulphur and ſea ſalt; that he is far from recommending them, and is content only with cautioning us againſt the miſchief they may occaſion. If my father had found them of no uſe, he had the faireſt opportunity of ſaying ſo when he was contraſting their efficacy with that of vinegar; and he ought to have ſaid ſo, when he was ſpeaking of the miſchief which they might occaſion by irritating the lungs. A writer of the moſt ordinary talents, and the moſt ordinary humanity, would have ſaid that there was much miſchief, but no utility in employing them, and, inſtead of reſtraining the miſchief by cau-

tions, he would have averted it by peremptory and direct prohibition. Whatſoever then be the interpretation of my father's meaning, Dr. Smyth's expoſition of the text of my father's words is neither fair nor candid, nor ſufficient to ſet aſide the claim of diſcovery, that the mineral acids did poſſeſs ſome power of deſtroying contagion, and that they could be uſed with ſome degree of ſafety as well as efficacy.

My father then muſt be confeſſed to have diſcovered the principle common to all the mineral acids in a ſtate of gas, that of deſtroying contagion. He aſſigns to the ſulphureous acid gas a ſpecific power which had never been aſſigned to it before. He gives directions for raiſing the muriatic acid gas which had never been given before. He gives cautions reſpecting the muriatic, which, of courſe, could not have been given before. He repeats cautions about the ſulphureous, which probably had been given before, becauſe the

effect

effect againſt which that caution is directed muſt have been obſerved when brimſtone had been burnt for a different purpoſe than that of deſtroying contagion. My father's experience, like that of every other intelligent obſerver, doubtleſs, enabled him to improve and regulate the uſe of the muriatic acid gas, after the publication of 1756; and that he continued to uſe it after that publication, and antecedently to the publication of Dr. Smyth's book in 1795, and even his experiments made, or I ſhould rather ſay, intended to be made, at Wincheſter in 1780, will be ſhewn by me abundantly, before the concluſion of my reply to Dr. Smyth.

I am ſorry to have been thus long engaged in a warfare about words—But words are the vehicles of facts, and, as it is my wiſh to have facts underſtood, I ſhall never be aſhamed of confeſſing any real or ſeeming error about the import of words by which facts are conveyed.

In my endeavours to be correct and ingenuous, I may have been prolix. I have not forgotten that Dr. Smyth has talked in his letter about a ſhort expoſition of my father's text. But there is a ſort of conciſeneſs which produces obſcurity, and there is a ſort of obſcurity very convenient for writers, who, profeſſing not to weary their readers, are intent upon miſguiding them.

I now come to Dr. Smyth's negative propoſition, founded upon certain other quotations from my father's diſſertation, in the ſelection of which he has not forgotten the precept of the antient poet and philoſopher,

Νᾶφε καὶ μεμνᾶς ἀπιϛεῖν, Ἄρθρα τᾶυτα τῶν φρενῶν * :

and which he has put together like a maſter in the ſchool of the Ephectics and Aporetics †.

* Cic. Ep. ad Attic. I. 19.

† With the common places of ſuſpenſion, and the ὀκτω τροπου of Æneſidemus, I will not weary the reader. The ſcholar finds more of this ſort of learning than he can underſtand in Sextus

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The propoſitions are, "that Dr. Johnſtone, the father, neither knew the ſuperior efficacy of the mineral acids in deſtroying contagion, nor *ever* had the moſt diſtant idea of employing them with this intention." When an author is quoted, his opinions ought to be cited in his own order and connection; for by inverting ſentences, and detaching the terms from thoſe links which bind them together in a common meaning, it is in the power of any bungler to overturn all ſenſe and verity, and to make even the holy ſcriptures themſelves utter blaſphemy and falſehood. Let us now attend to the two extracts from my father's Diſſertation, which Dr. Smyth introduces to ſupport his negative propoſition. The firſt is from p. 53 of the Hiſt. Diſſ. in which my fa-

Empiricus; perhaps the learned doctor has ſharpened the edge of his ſubtlety in Pyrrhonic: Hypotypos; lib. 1. cap. 17.

ther

ther gives perſonal directions for guarding againſt contagion. At p. 51 he has directed the means which he thinks moſt efficacious for medicating the air, that is, deſtroying the contagion in the apartments of the ſick. At p. 53, 54, he recommends thoſe rules which are beſt calculated to maintain health, and conſequently to reſiſt the poiſon of infection. "The "great buſineſs of preſerving the body free "from contagion conſiſts in preſerving the "motion of the blood free and regular, and in "ſupporting inſenſible perſpiration in a regu-"lar equable tenor. Fatigue of body and "diſſipation of ſpirits, and an intemperate uſe "of the non-naturals, muſt be avoided. Wines "are neither to be uſed immoderately nor "yet too ſparingly. I have ſeldom known "habitual drinkers of red wine affected with "putrid diſeaſes; this and the acids of le-"mons, oranges, vinegar, tartar, ſpirit of

"nitre,

" nitre, and elixir of vitriol, and the bark,
" are the beſt preſervative, as well as excellent
" curative remedies. The mind muſt be pre-
" ſerved in a fearleſs exhilarated ſtate. Laſtly,
" perſons who frequent the beds of the ſick,
" either in viſiting or attending upon them,
" ought to avoid breathing in the atmoſphere
" which is contiguous to the patients' body as
" much as poſſible; the ſaliva ſhould never
" be ſwallowed in the room; myrrh may be
" held or maſticated in the mouth; tobacco
" ſmoaked; and the mouth may be waſhed
" with vinegar or Tinct. myrrh. in water, after
" finiſhing thoſe offices which duty and hu-
" manity require to be performed to the af-
" flicted." In the firſt part of this paſſage the directions are purely anticipative and precautionary; they inſtruct the man who is expoſed to contagion, or who dreads it, how he beſt may maintain his conſtitution in a ſound condition; they relate merely to the management

ment of his exerciſe, his diet, his phyſic, and his paſſions. In the laſt part he gives directions to thoſe who frequent the beds of the ſick. He has already told you how the ſick man is to be treated; he has already ordered the ſick chamber to be fumigated by vinegar or by mineral acids, to deſtroy contagion; avoid, however, breathing too near the patients' body, avoid ſwallowing your ſaliva in the room, and *after finiſhing* thoſe offices which duty and humanity require, waſh &c. with vinegar. When Dr. Smyth cites the paſſage from p. 54, he entirely forgets or neglects the concluding ſentence, "after finiſh-" ing;" he chooſes not to notice that the directions are in the firſt place, perſonal and precautionary, and that to recommend the uſe of muriatic acid fumigation *before* a man enters the ſick chamber is not of the higheſt and moſt cogent neceſſity. While he is *in* the chamber he has all the benefit of the acid

fumigations,

fumigations, for *there* my father ſhews how they may be raiſed, and *after* finiſhing thoſe offices which duty and humanity require, that is, after the friend, the nurſe, or the phyſician, is got home, it would be ſuperfluous to raiſe them. The mention of the ſulphureous or marine acids would have been ſtill more ſuperfluous at p. 17, for there my father merely gives a hiſtory of the diſorder, and of its contagious nature. To have mentioned them at this place would have been utterly irreconcilable with the method of his compoſition; they would have been out of place. Methods of cure and precaution are inſerted at p. 51, 53, 54, of the Hiſt. Diſſ., and yet, after quoting p. 17, which is purely hiſtorical detail, comes out Dr. Smyth's fine apologue, in which the negative propoſition is ſummed up with all becoming ſentimentality. According to Dr. Smyth's mode of reaſoning, unleſs my father mentioned mineral acid gas,

at all times, and under all circumſtances, in connection with contagion, it muſt be inferred that he knew nothing of the matter. It is not ſufficient evidence that he gives preciſe directions for preſerving the air free from putrefaction, by diffuſing the gas of mineral acids in thoſe places where contagion actually prevails, unleſs he orders them, too, where it does not prevail. He can never have had the moſt diſtant idea of employing them with the intention of deſtroying contagion unleſs the words are always in his mouth, or under his pen. Let him give "common directions" for raiſing them before any other phyſician thought of it, let him uſe them all his life whenſoever occaſion call for their aſſiſtance, it is all a pretended claim, unleſs their uſe be urged in every ſentence of his writings and his converſation. Like the Indian Fakir, let him intermit his devotion or his penance for a moment, and there is an end of his former merits

rits and his prior claims. Has then Dr. S. himſelf been this medical Fakir? Have his converſations and his writings been perpetually engaged by the virtues of nitric acid gas? At leaſt his own declarations concerning his own practice entitle us to entertain ſome doubt, as we ſhall ſee hereafter. If thoſe doubts have any foundation, I muſt leave it to Dr. Smyth to *reconcile the ſuppoſition* of his being furniſhed by his own deliberate reflection, in 1780, with a method of preventing the ſpread of infection, *with the fact* of his not employing it till 1795. "Humanity revolts at the idea," is the ſentiment he applies to my father. Let me recommend him in future, before he weaves ſuch ſentiments on ſuch grounds, to look to himſelf, and before he applies them, to conſult the exclamation or the apoſtrophe of Sir Peter Teazle in the play.

But of all the perverſe conſtructions, and all the haſty aſſumptions, in which Dr. Smyth

has indulged himſelf, the moſt curious are thoſe in which he makes my father reſponſible for the theſis of my brother James. This inaugural *Theſis de Angina Maligna* was printed at Edinburgh in September, 1773, when my brother took his degree. In the acknowledgments to friends, my brother mentions three names: his father, and his maſters Gregory and Cullen. "*Primum omnium, patrem mihi* "*ſemper quam plurimum colendum, ut comme-* "*morem, ſuadent pietas, amor, officium, animuſ-* "*que beneficiorum non immemor, cujus attentio* "*vere paterna et prudens in me conſtanter in-* "*ſumpta, multo plus quam primordium vitæ ab* "*illo derivatum, me illi obligavit, cujuſque* "*conſilio amiciſſimo hocce opuſculum, meritum* "*ſuum, ſi quod habeat, præcipue debet.*" *De Angina mal. p.* 72. Such is the acknowledgment upon which Dr. S. builds his aſſertion, "that Dr. James Johnſtone's inaugural "Diſſertation is the joint work of himſelf and

"his

" his father, that its opinions are a declaration " under Dr. Johnſtone's own hand," and " we " can have no doubt that Dr. Johnſtone did " beſtow conſiderable pains on this firſt eſſay " of his ſon, which was to be equally honour- " able or diſcreditable to both." Behold another glaring inſtance, in which Dr. Smyth aſſumes a poſition unauthoriſed by the fact, another vigorous ſcyon, from the ſturdy ſtock of Dr. Smyth's fertile imagination. Here we have no leſs than three diſtinct aſſumptions branching out from two plain words, the meaning of which in plain Engliſh, can only be conſtrued, friendly counſel. If an honeſt though unlettered man were aſked, what friendly advice meant, he could be at no loſs in anſwering the queſtion for a moment. He would tell you that he gave his advice to the beſt of his judgment, under all the circumſtances of every particular caſe, and that he did not conſider himſelf as reſponſible for all

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the conſequences that might follow from all the actions of the individuals that aſked his advice, and acted upon it; this would be conſtituting him an actor, not a counſellor. In the caſe before us there is not the ſhadow of evidence, from the acknowledgment above cited, that my father was a joint party in the compoſition of the theſis. That he gave friendly advice is all that is aſſerted, the reſt is compliment; and ſurely, no unjuſt compliment for a ſon to pay to his father to ſay, if there were any merit in what he had done, it was derived from him. He was indebted to him for the firſt rudiments of education. He owed, and he paid to him, a tribute of gratitude for that paternal attention which, to every enlightened and well-regulated mind, has more endearments than the recollection of exiſtence itſelf. He had received from him many encouragements to induſtry, many hints in phyſic, many leſſons in philoſophy, which

formed

formed the baſis of a character of no common eminence. In his firſt public performance, an acknowledgment of theſe invaluable benefits was due to his father, and he made it. He does nothing more; and it required the full exerciſe even of Dr. Smyth's talent for diſcovery to diſcern that any thing more was intended. It will require more than all his learning to prove that *conſilium amiciſſimum* has the acceptation he has given to it. Will he venture to affirm that the meaning of *conſilium* can ever be diſtorted into a declaration of the ſame opinion being adopted in the ſame words, under the ſame hand? Where is the authority? Counſel muſt always be a mixed operation of the mind, the reſult of deliberation on the whole circumſtances of the caſe under conſideration. In giving counſel, a wiſe man will not only conſider that which is right, but alſo that which is expedient. Time, place, motives, age, muſt all modify his opi-

nions,

nions, to render his counſel profitable and fit for uſe. In the preſent inſtance, a father, who has made an intereſting diſcovery, gives friendly counſel to his ſon, then in the prime of youth, who is writing a public exerciſe for his degree. In his public exerciſe, the ſon does not mention this intereſting diſcovery; and therefore, thirty years afterwards, when both the parties are dead, it is denied by an intereſted controverſialiſt, that the father made this diſcovery. Such is Dr. Smyth's poſition. I know not whether this gentleman has ſons who have publiſhed academical exerciſes, or if they have, whether in ſuch exerciſes they have mentioned their father's very laudable uſe of nitric acid gas. Whatſoever be the fact, I ſhould eſteem it as feeble in reaſon and argument, as in reſource and deſign, to aſſert, that becauſe they do not mention it, he never uſed it. Nor will I impute to Dr. Smyth either the honour or the diſhonour of being a party in the compoſition

ſition of theſe exerciſes, although his ſons ſhall extol him according to his merits, or beyond them.

An inaugural theſis can ſeldom be elaborate in ſtile, or profound in ſcience. It generally conſiſts of a ſketch or hiſtory of a diſeaſe, or is an eſſay upon ſome moral or phyſical ſubject. It is never intended for the diſcuſſion of deep and diſputed points in doctrine, and when ſuch diſcuſſion is attempted, it is not always thought a proof of good judgment. As the work of a young and inexperienced, man it is rather expected to be a clear deſcription and a correct compoſition, than a didactic or practical work. I can readily conceive, therefore, why my brother ſhould omit the mention of my father's practice; that he knew it ſoon after this time I ſhall immediately bring proof. The hiſtory of the compoſition of my brother's theſis is, that it was compoſed whilſt he was abſent from my father, at Edinburgh, in the ſummer of 1773.

The Latin compoſition was firſt prepared by my brother, and then inſpected, not by my father*, but by one of my brother's friends at Edinburgh, probably by Dr. Henry the hiſtorian, my father's old inſtructor. My father did not ſee it till after its publication. When he did ſee it, he did not ſuffer his vanity to get the better of his judgment; nor did he reproach his ſon for not introducing a circumſtance which, however creditable to his own reputation, was unneceſſary, and might have been conſidered as impertinent and oſtentatious, from the very circumſtance of ſingularity in novelty, in the theſis of a young

* If Dr. Smyth had exerciſed his talent for conjecture upon my brother's book rather than his theſis he would have been more fortunate, and in my eſtimation, as well as his own, more ſagacious. My father had no concern at all with the theſis; but in the book he certainly did aſſiſt my brother, vid. Green's Hiſtory of Worceſter, Vol. 2, p. 69. It is not very probable that an antiquary ſhould explore a medical book, and the plain fact is that my father furniſhed Mr. Green with the foregoing paſſage, and with other *particulars* relating to my brother.

man.

man. But what my brother omitted when the propriety of introducing it was doubtful, he did not fail to introduce properly, and to ſtate unequivocally, when his knowledge was more enlarged, when his judgment was more matured, and when, for his opinions and attainments as a phyſician, he was more directly and more extenſively reſponſible to all profeſſional men.

In the note at p. 25 of his Letter, Dr. Smyth condeſcends to notice a volume of Medical Eſſays, containing one Eſſay of mine "on Mineral Poiſons," in which, ſays Dr. S. "although he mentions particularly the three "mineral acids and their gaſes, there is not the "moſt diſtant hint of their application to de-"ſtroy contagion." When the heat engendered by the intemperance of controverſy has abated, Dr. Smyth will perhaps *diſcover*, that in a work profeſſedly on poiſons, I was not immediately called upon to detail the curative application of medicaments. In regard to the

utility of the Eſſays, in their preſent form, I am neither daſtardly enough to be terrified by the deciſion of Dr. Smyth, nor vain enough to offer any opinion of my own. But I can readily ſpare the increaſe of credit which might have accrued to my brother or to myſelf, by the introduction of matter which had no immediate connection with my ſubject; and I beg leave to aſſure Dr. Smyth that neither his advice nor his example will induce me to mention my father's diſcovery, or his own, in any future publication, unleſs the introduction of them ſhall in my judgment, be uſeful, as proofs or illuſtrations of the facts which I relate, and the principles which I defend.

Becauſe in this one Eſſay there is no hint of mine on the application of mineral acids and their gaſes to deſtroy contagion, Dr. Smyth concludes, with his uſual fairneſs, that the very idea of their power to deſtroy it did not exiſt in my mind. But the concluſion is at a wide

diſtance

diſtance from the premiſes. The idea muſt have exiſted in my mind unleſs Dr. Smyth will extend his reproaches from my want of judgment to my want of memory. The Eſſays were publiſhed in 1795. Now, it ſo happens that from my earlieſt youth, and in the vacations during my academical ſtudies at Edinburgh and at Oxford, I frequently viſited my father; that for one year, from 1793 to 1794, I aſſiſted him in his medical practice at Worceſter; that during this long period I had opportunities not only of hearing my father's opinions on the power of mineral acids, but of obſerving his ſucceſs in uſing them; that I had read my father's book publiſhed in 1758, and my brother's book publiſhed in 1779. With means therefore ſo abundant, and ſo conſtantly at hand, I did not need the aid of Dr. Smyth's book to form ſome idea of the power of the mineral acids to deſtroy contagion. The idea had exiſted in my mind long before the publication of the Eſſays, and the only addition

to

to my knowledge, for which I am indebted to Dr. Smyth, is, that the nitric acid gas may be uſed with nearly as much convenience as the muriatic, and with ſimilar efficacy. But even Dr. Smyth's uſe of nitric acid was not, at that time, known to me. If known I ſhould not have noticed it in my Eſſay on Poiſons, how much ſoever I might have been convinced of its utility. Let me here correct a ſmall error into which Dr. Smyth has, ſomehow or other, been precipitated. The Doctor ſays in his note, that the Eſſays are chiefly the production of my brother; but if Dr. Smyth's reflections upon his own diſcoveries will afford him leiſure to look once more into this volume, he will find that not *one* of them is written by my brother James. They are all the production of my father, except the caſes furniſhed by Mr. Gomery and by my brother, Dr. Edward Johnſtone, and one Eſſay of mine.

That Dr. Smyth may not imagine the power of muriatic acid gas unknown to my

brother

brother James about the time of his taking his degree, I ſhall now inſert part of a paper written by himſelf, on the medical virtues of airs, which paper was ſent by my father to Dr. Prieſtley, to be preſented to the Royal Society. Dr. Prieſtley's anſwer to this communication is preſerved; and as it will correct ſome of Dr. Smyth's viſions and theories at p. 21, 22, 23, of his Letter, clear up his doubts, and do away the effect of his cavils, I ſhall inſert it. The paper, written in my brother James's own hand, is entitled,

"Remarks upon the Medical Virtues of "different Kinds of Air," by James Johnſtone, jun. M. D. dated Worceſter, May 24, 1775.

After mentioning, in five pages, different airs which are antiſeptic, he goes on as follows:

"Another grand reſtorative of vitiated air "of Dr. Prieſtley is acid air, procured from "ſea ſalt. This is capable of a very important

"medical

" medical application, not mentioned by the " doctor and his medical correſpondents; " though his experiments and deductions ſhew " it likely to produce the beſt effects. *The* " *uſe I mean is that of correcting the air of* " *jails, hoſpitals, &c. and medicating the air* " *within the chambers of perſons labouring un-* " *der any putrid diſeaſe,* or ſuch as are troubled " with wounds or ulcers tending to putref- " cency. *This uſe of acid air was recommended* " *by my father, Dr. Johnſtone of Kidderminſter,* " *in his Diſſertation on the Malignant Epidemi-* " *cal Fever of* 1756."—He then quotes the paſ- ſage at p. 51 of my father's Diſſertation. " The " ſpirit of ſea ſalt has been a *common* ingre- " dient in the ſteams made uſe of by my fa- " ther for the ulcerated ſore throat; this is " now become the *uſual practice in this country,* " and every day produces freſh inſtances of its " ſalutary effects. The following is the moſt " convenient method: Let a quantity of com- " mon ſalt be put into a ſtone or earthen jar,

" and

" and let the jar be placed in a veſſel contain-
" ing warm water or heated ſand; by theſe
" means a conſtant gentle heat may be kept
" up, and a very gentle heat is ſufficient to
" raiſe the marine acid in its volatile aerial
" ſtate, by pouring oil of vitriol on the ſalt;
" after this more of the vitriolic acid may be
" added as occaſion requires. If the veſſel be
" placed at a conſiderable diſtance from the
" bed ſide, the patient's lungs will not be in
" the leaſt irritated; nay, it is poſſible it may
" be raiſed in ſo gentle a degree as even to be
" ſerviceable in caſes of ulceration of the lungs;
" but this ſtill remains to be tried. It is cer-
" tain that where it has been tried no incon-
" venience has been obſerved to ariſe either to
" the patients or attendants. *Experience has*
" *already ſhewn* the ſignal utility of this acid
" air in putrid fevers, and malignant ſore
" throats, and joins with theory in recom-
" mending it, not only as a purifier and reſtorer

" of infected air, but as a ſtrong reſiſter of pu-
" trefaction, and a moſt *eligible form* of admi-
" niſtering a powerful and efficacious remedy
" againſt all putrid diſorders." The paper from which the above ſentences are extracted was ſent by my father to Dr. Prieſtley. The following is the doctor's anſwer:

Calne, 10th July, 1775.

SIR,

I ſhould have acknowledged the receipt of your favour, incloſing your ſon's ingenious remarks on the medical virtues of the different kinds of air; but having lately removed from one houſe to another, I have been obliged to neglect moſt of my correſpondents.

As your ſon's paper contains only opinions and propoſals, the Royal Society would not receive it, as they make it a rule to admit nothing that is not ſupported by new facts. They attend to nothing elſe, be it ever ſo ingenious

and

and probable. Now I could wiſh, as your ſon ſeems to have a genius turned that way, he would make experiments in order to aſcertain the favourable or unfavourable effects of different kinds of air on the human body. It is a very extenſive field of inquiry, and could not fail to reward his application. In one reſpect your ſon has miſtaken my meaning, for *I did not find that acid air, corrected air injured by putrefaction, but the contrary.* It may, however, have a ſalutary effect on the air, in various ſtates. If he can aſcertain that, it will be a curious and valuable diſcovery.

I have been pretty fortunate in the proſecution of my experiments ſince the publication of my laſt volume, and I have now another in the preſs, containing a great variety of new facts, and ſome of them I think of conſiderable importance. Among other things I have found that by means of nitrous acid and earth I can make air either of the ſame degree of

purity with the air of the atmoſphere, or air five times as good. This, I ſhould think, is capable of very important medical uſes, but thoſe things I leave to phyſicians.

I ſhould be much obliged to you for a ſight of the Diſſertation you mention as having been preſented to the Royal Society.

With my reſpectful compliments to your ſon,

I am, Sir,

Your very humble ſervant,

(Signed) J. PRIESTLEY.

(Directed)

Dr. Johnſtone, Kidderminſter.

My brother, it ſhould be obſerved, writes in the foregoing paper as a phyſician, and Dr. Prieſtley as a chemiſt. My brother mentions not only opinions but facts; he does not deſcribe them circumſtantially and in detail, but he refers to them explicitly and generally; and thoſe facts include caſes in which my father had

had uſed the marine acid effectually for deſtroying contagion. I cannot account for my brother's miſconception of Dr. Prieſtley's opinion, and I need not apologiſe for it, becauſe my ſubject requires me to ſtate what my father practiſed, and what my brother knew in the cure of putrid diſorders, by the marine acid.

Surely I have produced documents that prove my brother James to have written a Treatiſe on the medical virtues of different kinds of air, in 1775; in which he particularly notices muriatic acid gas as the method recommended by his father in 1756 to correct vitiated air, as a purifier and reſtorer of vitiated air, and a ſtrong reſiſter of putrefaction; an agent capable of a very important medical application, *not mentioned by Dr. Prieſtley,* and his medical correſpondents; an efficacious and powerful remedy againſt all putrid diſorders, and for the application of which he gives preciſe directions; a remedy which, if adminiſtered

ſtered with caution, will not prove inconvenient either to the patients or attendants. Will Dr. Smyth ſay now that *no idea* of the power of mineral acid gaſes exiſted in the mind of my brother before 1779? Will he ſay now that my brother owed his knowledge of acid air to Dr. Prieſtley? Will he venture to aſſert now that Dr. Prieſtley owed his knowledge of it to Guyton-Morveau? Dr. Prieſtley declares, that *he did not find acid air, corrected air injured by putrefaction.* My father, in 1756, had found that it *did*, and M. Guyton-Morveau in 1773 had found the ſame. What then becomes of Dr. Smyth's poſition? "And if his ſon, Dr. James Johnſtone, ſhall "be thought to have made any approach to-"wards it, *he owed this entirely to Prieſtley*, and "he probably to Morveau." According to Dr. Smyth my brother borrows from Prieſtley an opinion which the ſuppoſed lender diſclaims, and Prieſtley filches from Morveau, an opi-

nion

nion which Morveau never held. Thus ſtands the order of facts. Acid air *has* a certain effect in the judgment of my brother and Morveau. Acid air has *not* that effect according to Prieſtley. But, according to Dr. Smyth's account, my brother borrows from Prieſtley an opinion which Prieſtley, the ſuppoſed lender, never held; and Prieſtley filches from Morveau an opinion which he never held. It does *not* appear that Prieſtley had ever ſeen Morveau's account, but it *does* appear that my brother had ſeen Prieſtley's account of acid air, and had drawn from it an inference which the doctor does not admit. And it alſo appears that my brother derived his firſt knowledge of the acid air from my father. All, therefore, that my brother owed to Dr. Prieſtley was the term acid air; a term certainly not more correct than the term marine acid, which he had previouſly learned from his father's Diſſertation on Fevers. The whole tranſaction proves

that

that my father was anxious to inform Dr. Prieſtley of his own diſcovery. My brother's paper is chiefly occupied with this part of the ſubject, and it appears to have been the chief aim to have made it public through Dr. Prieſtley. The Doctor, however, in his letter takes no notice of my father's having ſo long before uſed muriatic acid gas, but deſires to ſee the Diſſertation. Dr. Prieſtley's letter is precious to all men of ſcience, as giving a very early intimation of the diſcovery of oxygen gas. It is important to medical men, as containing a proof that information had been communicated by my brother to Dr. Prieſtley in 1775 of the application of muriatic acid gas to correct air injured by putrefaction. It is important as a corrector of Dr. Smyth's aſſumptions, even in the very inſtance where my brother miſunderſtands Dr. Prieſtley, and Dr. Prieſtley diſſents from my brother; for the Doctor plainly ſays that, according to his experience,

" acid

" acid air did *not* correct air injured by putre-
" faction ;" and my brother ſays, according to his experience and his father's, the acid air *did correct* air ſo injured.

Thus it is clear that my brother did not receive the hint from Prieſtley, and that Morveau did not give the hint to Prieſtley, (for Prieſtley never received it till he got it from my brother). Whether Dr. Smyth, or any other perſon, received it, without acknowledging it, from my father, or my brother, or Morveau, I know not.

My mention of the manuſcripts of my father, as not affording any preciſe evidence, Dr. C. Smyth has converted into circumſtances ſupporting the claim the firſt in order as in importance. His own expreſſions are, " the firſt of which, in order as in impor-
" tance, is his father's manuſcripts ;" alluding to what Dr. Smyth calls the other circumſtances, which I have ſtated, to ſupport my

 claims.

claims. So far from ſtating *theſe* manuſcripts as the firſt in importance, I declared that it would be uſeleſs to enter upon them, becauſe they conſiſted merely of names and preſcriptions. That I may be completely underſtood, however, I ſhall inſert one or two caſes, in the manner in which my father poſts them down, as an illuſtration of my meaning:

1. *Mrs. Bennet—Angina die quarto—Eruptiones die quarto—Gargariſma. Vapor Acid: Spir: Sal: Marin.—Sp. Nitri dulc: ℥i g^tt inſtilland: potu, ad gratam aciditatem.*

2. *Mr. Winton, æt:* 18. *febris putrida cum Delirio, Petechiis, Exanthemat: die* 10.

3. *Mrs. P. æt:* 45—*Tuſſis, febris nervoſa putrida, filio mortuo infecta, die* 8.

Pro Winton, Vinum liberrime, ſucceſſio Veſicantium, cortex, et antiſeptica mineralia liberrime.

The notes of phyſicians are ſeldom more full than theſe; and it is only in *connection with other*

other proofs, that ſuch notes can at all be conſidered as ſatisfactory documents. Alone they would at beſt afford only preſumptive evidence; and, reſting my father's cauſe upon the more direct evidence employed in my pamphlet, I thought it unneceſſary to adduce them in my ſtatement. They are not circumſtances the firſt in importance, according to my ſtatement. But they are the next in order after the teſtimony ſupplied by my father's book, and the next in order before the mention of the teſtimony of Mr. Crane, an accompliſhed profeſſional man, who knew my father's practice, and was daily concerned in it, thirty years ago, and who is prepared to give the fulleſt teſtimony, " that the muriatic acid vapour was ſo " commonly uſed when he firſt ſettled in buſi- " neſs, that the manufacturers placed it ſpon- " taneouſly in their ſhops, when fever was ap- " prehended, and that in malignant caſes it " was always ordered by my father." But

about that teſtimony which is next in order to my father's caſes, as they are ſtated in my father's manuſcripts ſhortly and imperfectly, and far ſuperior in importance, Dr. Smyth preſerves a deliberate and myſterious ſilence; whilſt he reſts with double attention upon that very part where he ought to have ſhewn that it was futile, or admitted that it was pertinent and concluſive, which I do not adduce as evidence, and declare I think it would be unneceſſary to adduce. He does indeed, in another part of his letter (p. 23) mention that teſtimony; and the mention of it here is accompanied by circumſtances equally remarkable with the omiſſion of it elſewhere. No opinion is given upon its value, no comment is made upon its contents, no reſiſtance is attempted againſt its force. From ſecret contempt, or ſecret fear, from blameable negligence or even blameable artifice, Dr. Smyth is content with ſaying, that the only proof which

I have

I have brought of my father's uſing the marine acid, excepting my own aſſertion, is the teſtimony of a ſurgeon at Kidderminſter. This poſition is in two reſpects inaccurate.* I have brought many *other* proofs, or at leaſt what ſeemed to me ſuch, *beſides* the teſtimony of Mr. Crane and my own aſſertion. I ſpoke of Mr. Crane not merely as a *ſurgeon*, but as an eminent and reſpectable ſurgeon in Kidderminſter; and his claim to ſuch appellations is not weakened, becauſe Dr. Smyth may have never heard of his name till he ſaw my pamphlet; juſt as he profeſſes, p. 31, never to have heard of my father's name, till my anonymous para-

* Quid iniquius quam ubi ex altera parte, longe probabiliores et firmiſſimæ rationes exiſtant, ex alterâ infirmæ admodum; tamen ideo rem ipſam venditare pro dubiâ incertâque, et inter rationes illas paritatem et ἰσοσθένειαν admittere, quamlibet ἀδοξαςως, quod nihil eſt, quam ludere in re maxime ſeriâ, et ἀδοξαςτως insanire.

FABRICIUS, in Præfat. Sext. Emp.

graph

graph in the Morning Chronicle led him to enquire for my father's book.

But to return to my father's manuſcripts. Though my father's manuſcripts, ſo far as they were known to me, had conſiſted merely of names and preſcriptions, without any regular ſtatement of ſymptoms, or of the particular operation of medicines, I contend that they contain ſuch a ſtatement as proves muriatic acid gas to have been uſed by my father ſubſequently to the year 1756, and, I now add, previouſly to the appearance of Dr. Smyth's book in 1795, and to his poſſible practice at Wincheſter in 1780. The ſame ſtatement, though not regular, is clear enough, and full enough to confute Dr. Smyth's inference from my pamphlet, "that, upon the au-
"thority of my own repreſentation, my father's
"manuſcripts do not furniſh a ſingle example
"of the ſucceſsful employment of the marine
"acid, *any* opinion of its ſafety, *any* evidence

"or

" or even *ſuſpicion* of its uſe." I have a volume of my father's manuſcripts now before me, and from that volume I will produce paſſages which demonſtrate, in ſeveral examples, the ſucceſsful employment of muriatic acid gas; which ſhew my father's juſt and fixed opinion of its ſafety, which contain evidence not merely for his ſuſpicion, but for his diſtinct and thorough knowledge of its uſe.

The following is an extract from my father's journal, dated Kidderminſter, 10th March, 1776. " I have attended ſeveral patients ill " of a low putrid fever, and even in ſome a " petechial fever." It was at this time and with this fever that my brother was attacked; and in whoſe cure I remember muriatic acid gas to have been employed, as appears by the following:

" My dear ſon Edward had a very copious hæmorrhagy from his noſtrils on the 7th day

of

" of this fever. He had been infected by " too closely observing some patients ill of it." At the close of his directions for the cure of this fever, my father says, " I always enjoin " washing the patient with vinegar and cold " water, to have his linen often changed; if in " a state of perspiration, his feet are wrapped " in flannel, wet in warm vinegar; and to *fill* " *the air with an antiseptic vapour, I order* " *oleum vitrioli, to be poured on sea salt, by which* " *its acid fume is diffused* in the chamber. I " also order vinegar to be sprinkled or boiled " in it," (*i. e.* the chamber).

10 Dec. 1800. " These observations, sent " in a letter to Dr. Lettsom for the Medical " Society, are thus introduced;

" DEAR DOCTOR,

" I send you the following notes as a friend- " ly endeavour to divert your grief and cares,*

* The son of this excellent man was recently dead.

" and

" and as a ſmall contribution to the under-
" taking of the Medical Society, and mark of
" attention to them. Theſe obſervations re-
" late to a fever which prevailed in ſome parts
" of this country and in Kidderminſter in
" 1775, 1776. In treating this and ſome
" other fevers which had *before* occurred, I
" adopted practical meaſures, which appear
" to have been ſucceſsfully tried by other *later*
" practitioners. It is not ſo much my view
" to claim priority in practice, as to confirm
" thereby attempts to cure by ſimilar means,
" and to obtain additional credit to my ob-
" ſervations, from the experience of other
" able phyſicians, Carmichael Smyth, Currie,
" &c."

In another manuſcript on a general view of fevers and of their cures, in which my father gives the reſult of his long experience, or rather has left it a legacy to his family and poſte-

 rity,

rity, the ſubject of fumigation is thus treated. The manuſcript was probably finiſhed the 6th Dec. 1799, as this date is inſerted at the cloſe.

'The vapours which ariſe from common 'ſalt and from nitre are powerful correctors 'of air; the vapour of the laſt reſtores the 'conſumed oxygen and vital air, the former 'adds to its antiſeptic power, and reſiſts pu-'trefaction. *With this view it was recom-'mended by me ever ſince the year* 1756, and 'is mentioned in the Diſſertation on Fevers 'which have prevailed at Kidderminſter; I 'have often had occaſion to apply it from that 'time. It is eaſily procured by pouring a 'little ol. vitrioli on dried ſea ſalt. It riſes 'by a very moderate degree of heat, and dif-'fuſes itſelf very widely in every part of the 'habitation of the ſick, and if not too near 'them, without the leaſt inconvenience to

'them

'them or their attendants. It refreſhes thoſe 'that are well along with the ſick, and pre-'vents the danger of infection."

'It appears by a memoir preſented, I believe, 'about the year —— by M. Lavoiſier, and pub-'liſhed in Memoirs of the Royal Academy of 'Sciences, that the celebrated M. de Morveau 'made uſe of this method to purify the air in 'infected priſons. The method, as deſcribed 'by him, is perfectly ſimilar to that recom-'mended by me *long before*, "and conſiſts in "diſengaging and diffuſing a great quantity of "marine acid, in a ſtate of vapour, in the in-"fected places. For this purpoſe a half of a "quarter meaſure of marine ſalt, more or leſs "according to the ſize of the chamber, muſt "be heated in a large iron ſpoon, or a ſmall "pan; when the ſalt is well heated a quan-"tity of oil of vitriol, amounting to one-third "or half the weight of the ſalt, is to be poured

" on it in the ſame veſſel; after which every
" one retires, and leaves the door of the room
" ſhut. The vitriolic acid by its action on the
" marine ſalt diſengages its acid; and the lat-
" ter riſes in the form of a white vapour, which
" diffuſes itſelf through the whole chamber,
" and neutraliſes the putrid particles, by which
" it was infected.' Dr. James Carmichael
' Smyth, in an ingenious and important work,
' deſcribing his treatment of the gaol diſtemper,
' has improved the method of deſtroying con-
' tagion, which he believes to be of a putrid na-
' ture, by employing the acid vapour of nitre,
' raiſed alſo by means of ol. vitrioli, for that pur-
' poſe. He has proved, in unexceptionable ex-
' periments, that it may be ſafely breathed by
' the ſick, and by thoſe that are well. And in
' private and in hoſpital practice, ſays he, p. 194,
" I can declare with truth, that when the ni-
" trous acid has been uſed conſtantly as a fu-
" migation,

" migation, I have not known an inſtance of a
" contagious fever having been communicated
" even to a nurſe or an attendant.'

' The Doctor, p. 193, with a due zeal, claim-
' ing priority of invention, ſays: ' From all the
" information I can procure, I do not find that
" any perſon has ever made uſe of the nitrous
" acid to deſtroy contagion but myſelf.'
' When nitre cannot be had, the Doctor has no
' heſitation in employing the marine acid, ' as
" in a moderate quantity it may be uſed ſafely,
" where people are preſent.' p. 191.

' I hope I alſo may be excuſed in preſenting
' to poſterity my claim to an early uſe of marine
' acid to deſtroy contagion in putrid fevers.
' This method of correcting contagion is ſug-
' geſted in my Hiſtorical Diſſertation on the
' Putrid and Malignant Diſeaſes which reigned
' at Kidderminſter; London printed, 1758.
' Alſo, in an Eſſay on the Ulcerous Sore
' Throat, by my ſon deceaſed.

' I add

'I add here, that ever ſince that time it has 'been conſtantly directed by me to be uſed in 'work-houſes, gaols, and in private practice, 'with a ſucceſs which has recommended it in 'this country. I do not mean to deprive M. de 'Morveau, and the French chymiſts and phy-'ſicians, nor Dr. C. Smyth, of the honour due 'to them, for applying and extending this in-'vention, eſpecially for more public and exten-'ſive uſe. But I alſo have a claim to aſſert in-'vention; and perhaps the ſpark of light ſtruck 'out by me may have kindled a more ſplendid 'blaze. What I recommended in the obſcurity 'of my retired ſituation may gradually have 'made its way to the attention and experience 'of theſe learned men, aided by a more exten-'ſive and correct knowledge of chemiſtry. 'NEVERTHELESS, I ALSO AM AN INVEN-'TOR.'

Such is the candid and reſpectful language concerning Dr. Smyth, deliberately and ſe-

rioufly

riouſly employed by my father, employed by that very man, whom that very Dr. Smyth repreſents as 'unacquainted with any diſtinction 'in the application of the ſulphureous and ma-'rine acids; as conſidering them as equally ſafe 'and equally uſeful; as giving common direc-'tions (and he muſt mean only common) 'how they are procured, and cautions reſpect-'ing their uſe, too inaccurate to be of much 'ſervice; as making under his own hand a de-'claration in my brother's theſis, which ſets 'aſide all pretenſions to the diſcovery claimed 'in his name; as having put to paper writing 'which furniſhed not a ſingle example of the 'ſucceſsful employment of the marine acid, no 'opinion of its ſafety, no evidence or even 'ſuſpicion of its uſe; as unknown to him (*i. e.* 'Dr. Smyth,) even by name, until his ſon's 'anonymous paragraph in the Morning Chro-'nicle had led Dr. Smyth to enquire for his 'book of which, with ſome difficulty, he

'procured

'procured a copy, and from which, if he had 'been poſſeſſed of it at an earlier period, *he* '*could have derived no uſeful information.*'

My indignation kindles while I tranſcribe theſe contemptuous paſſages, but I ought to produce them, becauſe they will aſſiſt the reader in contraſting Dr. Smyth's rudeneſs and injuſtice to my father, with my father's prompt juſtice and unfeigned reſpect to Dr. Smyth.

After reading the foregoing paſſages from my father's manuſcripts, is Dr. Smyth ſo ſtubborn as to retain, or ſo hardy as to avow his doubts, whether thoſe manuſcripts afford proof of muriatic acid having been uſed by him ſubſequent to the year 1756, and I will add before 1780? Will he inſult the candour or the ſagacity of profeſſional men by maintaining, that the report which I now make of my father's manuſcripts will not much add to his character in their opinion? Will he venture to tell me again that "my zeal is intemperate,

"and

" and injures the perſon whom it meant to " ſerve?" Will he perſiſt in maintaining that my brother's inaugural theſis " is to be " looked upon as a declaration under Dr. John- " ſtone's own hand, which at once ſets aſide " all pretenſions to the diſcovery claimed in his " name?" If my brother's ſilence in the theſis publiſhed in 1773 were an hypotheſis to be admitted as a proof, that neither he nor my father had at that time the leaſt knowledge of the mineral acids, nor had ever employed them in practice; ſtill my father's explicit declaration, *really* made in his own hand-writing, muſt be allowed to prove that in 1776 he had ſomewhat more than a *little* knowledge of the powers of the mineral acids, and had in *more* than one inſtance employed them in practice. Thus, what my father actually did record in his own hand-writing, ſupports that very pretenſion to diſcovery, which the declaration he is *falſely* ſuppoſed to have made in his

own hand-writing, is pronounced by Dr. Smyth to have at once ſet aſide. When Dr. Smyth, from conjecture, confidently imputes to my father what he did *not* do in my brother's theſis, and, by *inference*, as confidently denies what my father *had* done in his own manuſcripts, I muſt take the liberty of telling him, that *his* zeal in refuting me, is not very creditable to his caution or his candour, and that it may ultimately injure the perſon whom it meant to ſerve.

Well—Dr. Smyth may ſay, that whatſoever may have been my father's practice in 1776 there is no evidence for the uniformity of his employment of muriatic acid gas after 1758; and indeed my alert and ſagacious antagoniſt has explicitly ſaid, from my brother's theſis in 1773, "that neither father "nor ſon had in that year the leaſt knowledge "of the power of the mineral acids, or had "ever employed them in practice," p. 17. The acuteneſs of Dr. Smyth's logic, when he

is

is criticiſing my brother's theſis, is accompanied by a lamentable want of memory as to the contents of my pamphlet; and, while he with exceſs of eagerneſs purſues his chance of victory in one quarter, he inadvertently expoſes himſelf to attack from other quarters. Now unleſs Mr. Crane, a *diſintereſted* perſon, lied to me, or Mr. Cooper and Mr. Symonds, two perſons equally diſintereſted, told lies to Mr. Crane, my father before 1773 *did well know* the power of the mineral acids, and *had often* employed them in practice. Nay more, unleſs my brother told a lie in 1779, my father had long ago *ordered* the marine acid for the correction of vitiated air; and if a phyſician orders a medicine for his patient, even though he ſhould not recommend it to other profeſſional men, the medicine ſo ordered, involves a proof of his knowledge, and conſtitutes a part of his practice. The knowledge may be ſmall, and the practice may be wrong. But I ſeriouſly

hope not to incur the diſpleaſure of Dr. Smyth as a grammarian, or as a dialectician, when I ſay, that *long-ago* is not explained in our Engliſh dictionaries by the word *lately*; that *ſometimes* is not the uſual ſynonime for *never*; that *any* degree of knowledge differs from *not the leaſt* knowledge; and that *any* degree of practice is not quite the ſame thing with *no* practice at all.

Perhaps the experiments with mineral acid gaſes made by Dr. Smyth at Wincheſter induced him to believe, that in point of practical utility, a very little knowledge and a very little ſucceſs approximate to total ignorance and total failure. In ſuch a caſe the good ſenſe of Dr. Smyth might be permitted to check the impetuoſity of his logic, and ſuggeſt to him, that things ſo near to each other in reality, hardly deſerved to be diſtinguiſhed in terms.

Dr. Smyth next proceeds to a Treatiſe on Malignant Angina, which *was* the work of deliberate

deliberate reflection of my brother James; whereas his theſis was merely the neceſſary exerciſe for his degree; a publication, ſays Dr. Smyth, with which I have taken the ſame liberty as with my father's, "altering the lan-"guage, making particular quotations, and ſup-"preſſing entirely what does not ſuit his pur-"poſe." How utterly unjuſt and unfounded this aſſertion was, when applied to the quotation I made from my father's book, is already ſhewn. The alteration in language which eſcaped me in the quotation from my brother's book is the word "recommended," inſtead of the word "ordered," and this is all. To bluſter about ſuch an alteration, under any circumſtances, is ſurely very unworthy of the diſcuſſions of gentlemen and men of ſcience. In the preſent inſtance it is clearly a miſtake; the term "recommended" is of weaker import than "ordered;" and to cavil about it ſhews the

extreme

extreme diſtreſs, the laſt agony of the advocate for a weak and expiring cauſe.

In the quotation from my brother's book * I brought forward the paſſage which proved that both father and ſon had a knowledge of muriatic acid gas, and that both did employ it to correct contagion. I did not think it neceſſary to cram my pamphlet full of quotations. I did not even cite all the paſſages that proved the fact. It is, however, neither my wiſh to alter the language, nor to make partial quotations, nor to ſuppreſs that which does not ſuit my purpoſe. I ſhall rejoice if men of ſcience will take the trouble of reading both the books from which I have quoted, and I will conſent to be called an ideot if, after an impartial peruſal, they do not decide in favour of my father's claim.

* Treatiſe on Malignant Angina, &c. by James Johnſtone, M. D. Phyſician at Worceſter; Worceſter printed, 1779.

That

That I did not quote all the paſſages that ſuited my purpoſe ſhall now appear—I did not quote the following:

"The ulcerated ſore throat had for ſome "years paſt very rarely appeared; in thoſe few "inſtances which did occur its malignity was "much abated, and the management of it be-"came ſo familiar in many places that the "good women themſelves cured it by means "of the bark and antiſeptic ſteams." Treatiſe, p. 4.

"Antiſeptic ſteams compoſed of vinegar, "myrrh, and honey, or the acid air raiſed by "pouring oil of vitriol upon ſea ſalt; gargles of "vinegar, or tincture of roſes with ſpirit of "ſalt; bliſters to the throat or other parts of "the body; and mixtures, with a large quan-"tity of the bark rendered gently diaphoretic, "a generous antiſeptic diet, and vinous ſub-"acid drinks, conſtitute the only ſucceſsful "method

" method of cure. This method, applied early " and continued with due perſeverance has " very ſeldom failed," p. 6.

" The patient's chamber filled with theſe " vapours (acid air) is the ſureſt method of " preventing contagion," p. 111.

Theſe ſentences I did not quote, though they ſuited my purpoſe; but if Dr. Smyth read them, he probably found that to produce them could not ſuit *his* purpoſe. I ſhall now produce the whole of another paſſage, part of which may be found in my pamphlet, and has been the ſubject of animadverſion from Dr. Smyth.

" Antiſeptic vapours (ſays my brother*) " are alſo to be conſidered as remedies of the " firſt importance. The ſteams of myrrh and " camphor boiled in vinegar and honey are of " great uſe in caſes of angina maligna, by fa-

* Page 109.

" cilitating

" cilitating reſpiration and increaſing the flow
" of ſaliva. It has already appeared (ch. 3. § 4,)
" that the air thrown out from the lungs
" is highly charged with putrid effluvia, and
" that theſe effluvia occaſion the parts about
" the throat, fauces, and noſtrils, to be parti-
" cularly affected with malignant ulcers. The
" antiſeptic vapours above mentioned are moſt
" immediately and eſſentially ſerviceable in
" correcting any putrid quality of the air,
" when it is either drawn into or breathed out
" of their lungs; by this means the *fomes* of
" the diſeaſe is altered and counteracted, at the
" ſame time that the parts already ulcerated
" are cleanſed and healed. The humid vapour
" of vinegar, with honey, or myrrh, or cam-
" phor, ſhould be drawn into the mouths of
" the ſick, as often as they can conveniently
" do it. For this purpoſe there are *ſteaming*
" *pots* fitted with proper pipes to convey the

" vapour with greater force into the lungs,
" which, being generally known, need not be
" described here. It is no inconsiderable ad-
" vantage of such vapours that they encourage
" a discharge of putrid lymph from the glands,
" while they act as topical correctors of putre-
" faction throughout the whole extent of the
" fauces, trachea, and ramifications of the
" bronchia. As it is impossible too cautiously
" to guard against the effects of so putrid a
" contagion, the acid air or spirit of salt should
" be kept rising continually in the room, by
" pouring oil of vitriol once or twice a day on
" sea salt, placed in a convenient vessel. This
" spirit will rise in the moderate degrees of heat
" from 60° to 70° of Fahrenheit, so as to be
" perceived in every part of the room by its
" penetrating acid smell. *This method of cor-*
" *recting vitiated air, which is useful in this*
" *and every other putrid disease, was long ago*

" *ordered*

" *ordered by my father,** *and is now recom-*
" *mended by Dr. Prieſtley. To have the pa-*
" *tient's chamber filled with theſe vapours is the*
" *ſureſt method of preventing the ſpread of con-*
" *tagion.*"

So wrote my brother in 1779, about the power of muriatic acid gas; and if language can convey in preciſe terms an obvious meaning it is done here. At firſt he recommends vinegar, &c. as topical correctors, " by facili-
" tating reſpiration and increaſing the flow of
" ſaliva," and for cleanſing and healing the parts already ulcerated, by correcting any putrid quality of the air. For theſe purpoſes there are ſteaming pots; and, to ſum up their whole power as topical applications, he adds,
" it is no inconſiderable advantage of ſuch va-
" pours that they encourage a diſcharge of
" putrid lymph from the glands, while they

* Johnſtone's Hiſtorical Diſſertation concerning the Malignant Fever of 1756, p. 51.

" act as topical correctors of putrefaction " throughout the whole extent of the fauces, " trachea, and ramifications of the bronchia." But " as it is impoſſible too cautiouſly to " guard againſt the effects of ſo putrid a con- " tagion, the acid air ſhould be kept riſing " continually in the room," &c. The next paragraph after this I did not quote in my pamphlet, and it is not noticed by Dr. Smyth, " to have the patient's chamber filled with " theſe vapours is the ſureſt method of pre- " venting the ſpread of contagion." Did Dr. Smyth read *theſe* words? Underſtand them he muſt; notice them he did not. But, with a contemptuous diſregard of their real and plain import, he maintains that my brother was evidently without experience; though upon that ſubject he gives the moſt exact and ſalutary directions that could flow from the pen of the moſt experienced practitioner: that my brother was ignorant of the powers of the

marine

marine acid gas, though that very brother ſpeaks of that very acid gas as the ſureſt method of preventing the ſpread of contagion. My brother did know that power; he communicated it to Dr. Prieſtley in 1775, he communicated it to the world in 1779. And yet it ſuits the logic of Dr. Smyth to believe, or his morality to affirm, either that he publiſhed what he did not know at all, or that his knowledge in 1779 was not prior to Dr. Smyth's knowledge, gained by Dr. Smyth's experience, in 1780. He either then publiſhed what he did not know, or his knowledge poſſeſſed and declared in the paper of 1775, publiſhed in the book of 1779, was not prior to Dr. Smyth's knowledge, gained perhaps in part at Wincheſter in 1780, and publiſhed to the world in 1795. Thus it appears, ſurely, that my brother does recommend the uſe of muriatic acid gas to prevent the ſpread of contagion, and that he knew his father to have uſed it;

and

and that he himſelf had experienced its uſe is clear, from the mention he makes of its ſenſible effects, "that its penetrating acid ſmell "may be perceived in every part of the room." As to the inconvenience which an imprudent uſe of it might occaſion, we may preſume that he had read the very book of his father, which he quotes; that he underſtood it, that he remembered it, and therefore would not ſlight the caution which his father had ſuggeſted for avoiding that inconvenience. He ſays expreſsly, that his father had ordered it *long ago* for correcting vitiated air; and when he himſelf calls it a method of correcting vitiated air, which is uſeful in every putrid diſeaſe, ſurely he recommends it. Nay, he again recommends it, when in the very next paragraph he again tells us, "to have the patient's chamber "filled with theſe vapours is the ſureſt method "of preventing the ſpread of contagion." Will Dr. Smyth then perſevere in contending

for

for the diſtinction which he ſets up between what my father ordered and recommended? Will he ſuppoſe that my father, in frequent and confidential converſations with my brother, did not *mention* the ſucceſs of that which he ordered; and of this ſucceſs can any expreſſions be more declaratory than thoſe uſed by my brother?

I hope Dr. Smyth will be convinced, before the termination of this reply, that *my* knowledge of my own language does juſt enable me to make ſome diſtinction in the meaning of the words *ordered* and *recommended*; and I truſt that I ſhall have juſt ſenſe enough to impreſs my knowledge of the meaning and of the value of words in ſuch a way, as ſhall deſerve to be remembered by Dr. Smyth, though, perhaps, the Doctor may be diſinclined to mention it.

Whether in his treatiſe my brother does not recommend muriatic acid gas as a remedy of more efficacy than vinegar, in my opinion,

opinion, cannot be a matter of doubt. The antiſeptic ſteams of vinegar, &c. are his topical correctors; they are a ſpecies of gargle to be drawn into the mouth from ſteaming pots, and which will correct the putrid matters within the fauces. But by muriatic acid gas he guards againſt effects; *they* are uſeful in that and all other putrid diſorders; *they* are the *ſureſt method* of preventing the ſpread of contagion. Dr. Smyth ſays, notwithſtanding, "marine acid gas is recommended by him, "not as a remedy of more efficacy than the "ſteams of vinegar, not as the ſole preſervative "againſt contagion, but as an additional ſe- "curity;* whereas Dr. Johnſtone, by omit-

* Dr. Smyth here acknowledges that in 1779, as "an addi- "tional ſecurity" againſt contagion, the acid air is recommended by my brother. If my brother knew in 1779 that mineral acid gas afforded any ſecurity againſt contagion, then could not Dr. Smyth *diſcover* the ſame principle of ſecurity in 1780. In Dr. Smyth's memorial preſented to Lord North, his ſervices at Wincheſter are enumerated at full length as the grounds of his

" ting what his brother ſays of medicated vi-
" negar, wiſhes to have it believed that the

his claim to remuneration; but not a ſyllable is ſaid about diſcovery. On the contrary, he alludes to purification as his chief agent, " and that theſe meaſures, if fully carried into execution, " would have entirely deſtroyed the contagion is extremely " probable; as none of thoſe Spaniards who were waſhed in the " river, new clothed, and lodged in the purified priſon wards, " were ſeized with the diſtemper;" Smyth, p. 229. If he had then known, or ſuſpected he had known, a method of purifying them in the hoſpitals, would he not have employed it? In another part of the memorial he ſays, " nor was it poſſible, how- " ever neceſſary, without compulſion, to take from the Spaniards " all their old clothes and bedding, which are known ſo power- " fully to retain the ſeeds of contagion." But the deliberate reflection of Dr. Smyth had before inſtructed him " that nothing " could ſo certainly or efficaciouſly deſtroy it (putrid contagion) " as mineral acids in a ſtate of vapour; but how to employ theſe " with ſafety was the difficulty: *hoc opus, hic labor.*"

Now ſurely there could be neither danger nor difficulty in expoſing *the old clothes* to this certain and efficacious power in any of its ſhapes!

Periculoſæ plenum Opus aleæ
Tractas, et incedis per ignes
Suppoſitos cineri doloſo.

"acid air was only employed." I had no ſuch wiſh; I muſt have been a fool to have had ſuch a wiſh. Vinegar in private practice is an uſeful antiſeptic, and ſo uſeful as to juſtify my brother in ſaying that it was immediately and eſſentially ſerviceable for the purpoſes ſubjoined. It could not be omitted in a treatiſe on malignant ſore throat: my brother, therefore, properly notices it as a topical corrector, and then enforces the uſe of muriatic acid gas as an uſeful agent for correcting vitiated air in putrid diſeaſes, and the ſureſt method of preventing the ſpread of contagion.

In my pamphlet I wanted to eſtabliſh the fact that my father knew and applied the powers of muriatic acid gas in deſtroying contagion; one of the proofs was the aſſertion of my brother that this acid gas had been ordered for this purpoſe long ago by my father. The quotation I made was quite ſufficient for my purpoſe, and therefore I did not quote the

preceding

preceding nor the ſucceeding paragraphs. I was guilty then of deliberate omiſſion, and I have aſſigned my reaſons for it. But the ſame omiſſion in Dr. Smyth deſerves the name of ſuppreſſion. The Doctor maintains here that in the opinion of my brother the muriatic acid gas was *not* a remedy of more efficacy than the ſteams of vinegar; *not* the ſole preſervative againſt contagion, but "an additional ſecu-"rity." Yet Dr. Smyth, when he read the next paragraph, muſt have perceived that in my brother's opinion it *was* of more efficacy than the ſteams of vinegar in preventing the ſpread of contagion; that it was a *better* preſervative againſt contagion; that it was the *ſtrongeſt* ſecurity; that it was even a method the *moſt ſure*.

The main point in diſpute between Dr. Smyth and myſelf, as I muſt again and again repeat, is priority in the diſcovery, and in the application of the mineral acid gaſes to deſtroy

contagion. I omit in my brother's evidence what is unneceſſary to my purpoſe of ſhewing that the principle and application could be claimed for my father long before they could be claimed for Dr. Smyth. Dr. Smyth ſuppreſſes the ſame evidence, for the purpoſe of ſhewing that my brother was not acquainted with the comparative efficacy of muriatic acid in caſes of contagion. But further, if what my brother ſays in the firſt paragraph had implied no more than Dr. Smyth erroneouſly aſſigns to it; if it had repreſented the marine acid as a ſecurity againſt contagion inferior to vinegar; ſtill it proves that the marine acid was known to my brother and my father before 1780; that it was uſed by both of them before that time, and that longer experience was then neceſſary to unfold to them the comparative value of the principle. But that experience in point of fact they really had, and having it, my brother ſays, "it is the ſureſt

"method,"

" method," &c. If then in the year 1779 my brother knew, as Dr. Smyth concedes him to have known, the muriatic acid gas to be an additional ſecurity againſt contagion; if he further pronounced that additional ſecurity, as Dr. Smyth knew, and ought to have remembered that he *did* pronounce it, to be the ſureſt method of preventing the ſpread of contagion; it contains two circumſtances moſt pertinent to my argument, and moſt favourable to my cauſe. It contains, firſt, a proof that the principle was poſitively known by experience; and, ſecondly, a proof that the extent of the principle alſo was known. It eſtabliſhes what Dr. Smyth would diſprove, the real priority of the diſcovery. It confutes all the reaſoning adduced by Dr. Smyth againſt that reality, from the limited advances of the diſcoverer in theory, and the limited uſe of the preventive itſelf in practice.

I ſhall truſt to the candour of all who can read,

read, and who reading can underſtand, whether it be poſſible to infer now, "that Dr. "James Johnſtone was led ſolely to the men-"tion of the marine acid from the recom-"mendation of Prieſtley." The reaſon why he mentions Dr. Prieſtley's name (and it is a name venerable and memorable in the annals of ſcience) is clearly, becauſe in the paper before quoted, he had communicated with him in the year 1775 on this ſubject; in which year Dr. Prieſtley certainly had not conſidered, if he at all knew, Guyton de Morveau's experiments.

At p. 22 of the Letter to Mr. Wilberforce Dr. J. C. Smyth enters upon the topic of my ignorance. He introduces it in the following words: "He (Dr. Prieſtley) therefore muſt "have ſeen Morveau's Memoir, publiſhed in "the year 1773, firſt in the Memoirs of the "Academy of Dijon, afterwards in the Journal "de Phyſique of the ſame year, although Dr.

"John

" John Johnſtone, with his uſual accuracy, af-
" firms that no account of Morveau's experi-
" ments was publiſhed until ten or twelve
" years after the year 1773, and then in the
" Memoirs of the Academy of Sciences. This
" gentleman's ignorance on a ſubject where
" he conceives his father's honour ſo imme-
" diately implicated is ſcarcely to be credited,
" but I leave the explanation to himſelf."

In the foregoing paſſage I am confidently accuſed of general inaccuracy; I am ſarcaſtically deſcribed as " this gentleman ;" I am pointedly charged with ignorance in the vindication of my father's character ſcarcely to be credited; I am contemptuouſly left to explain what, in the judgment of my accuſer, admitted no explanation conſiſtent with my credit as a ſon, a phyſician, and an obſerver of truth.

Upon this occaſion I muſt ſay,

Μωροῖς μωρίαν ὀφλισκανω.

SOPHOCL. in Antig.

When

When I alluded to the period of the publication of Morveau's experiments at Dijon, I explicitly ſtated in a note that I quoted from memory. I had not ſeen Morveau's Traité des Moyens de deſinfecter l'Air, when I wrote my pamphlet; and I did not then know that his method of fumigation had ever been noticed in the Journal de Phyſique. By M. de Morveau's own account, the Memoir on the Fumigation of the Cathedral was not firſt publiſhed in the Memoirs of the Academy at Dijon; it was the account of a ſecond attempt to purify the jail at Dijon, of which "*M. Maret,* "*ſecrétaire perpetuel de la ci-devant Academie* "*de Dijon, fit inſerer la notice dans le Journal* "*de Phyſique de Janvier* 1774."* The account of Morveau's experiments was afterwards publiſhed in Mem. de l'Acad. R. des Sc. 1780. Such was the ſource of what Dr. Smyth calls *this gentleman's ignorance.* I had

* Traité des Moyens, p. 12.

ſeen

ſeen the account publiſhed in theſe Memoirs; I did not know that it had ever been publiſhed in the Journal de Phyſique; I had not the book at hand to quote, and therefore openly declared that I quoted from memory leſt there ſhould be miſtake. Was it worthy of Dr. Smyth, was it generous in him, was it juſt, under ſuch circumſtances, to uſe ſuch expreſſions, or, indeed, to notice the matter at all? If my ignorance on a ſubject of this kind, and under theſe circumſtances, be ſcarcely to be credited, is it at all credible that Dr. James Carmichael Smyth ſhould be ignorant of the very ſame experiments at the time of *his* employment at Wincheſter, when his honour and his character were ſo immediately involved in a faithful diſcharge of his duty? If he was not ignorant of theſe experiments at the time when he was preparing to act under the conviction that nothing could ſo certainly or effi-

caciouſly deſtroy contagion as mineral acids in a ſtate of vapour, then muſt he be accounted a plagiary! Nay, more: if, knowing Morveau's experiments to have ſucceeded in deſtroying infection, and having himſelf been guided by the ſame experiments to the uſe of the ſame principle, for the ſame purpoſe, he afterwards aſcribes his uſe of that principle not to thoſe experiments, but directly and ſolely to his own deliberate reflections and his own unaided talents for diſcovery, then would Dr. Smyth ſtand convicted as an impoſtor! He would be a plagiary for borrowing that which he did not acknowledge. He would be an impoſtor for profeſſing to have himſelf diſcovered that which in reality he had learnt from another man, whom he knew to have anticipated the diſcovery. I leave him, therefore, to take his choice between two diſtreſſing alternatives—between

ignorance

ignorance equal to my own, or imputations of ſomething worſe than ignorance, from which he cannot eſcape, if he were not ignorant.

That my brother James did not owe his knowledge of the powers of muriatic acid gas to Dr. Prieſtley, I have proved; that he did owe it to my father, he has acknowledged at a time when there could be no room for controverſy, and even before Dr. J. C. Smyth's "attention had been called to this ſubject." Dr. Smyth alſo reluctantly allows, that my brother's Treatiſe does evince that he "had "made ſome approach" towards the diſcovery of a power that could certainly and efficaciouſly deſtroy contagion. It is remarkable that Dr. S. who takes infinite pains to inform his readers that he never heard of the name of my father, till an anonymous paragraph in the Morning Chronicle led him to enquire for his book, does *not* ſay that he never heard of the name of my brother James; who, the year

before Dr. Smyth was ſent to Wincheſter, publiſhed a work on contagious fever, in which he recommends muriatic acid gas to correct vitiated air, and as the ſureſt method of preventing the ſpread of contagion. In this book my brother, referring to the exact page of my father's Diſſertation, in which the precept for raiſing muriatic acid gas is contained, ſays (not very accurately indeed, according to Dr. Smyth's expreſſion), "that "this method of correcting vitiated air was "ordered long ago by my father." This is one of the boldeſt aſſumptions that ever was impoſed on the patience of the public. What, *not accurately*, when he refers to the very page of the Diſſertation of his father? Before any queſtion about priority of diſcovery had ariſen—Before Dr. S. had made his deliberate reflections at Wincheſter, my brother writes that his father had long ago ordered muriatic acid gas, and quotes a book

publiſhed

publiſhed in 1758, to prove what he writes; and yet it is daringly aſſerted by Dr. Smyth that it is not ſaid accurately, although the marine acid gas will be found to be ordered at the page cited. This is another of Dr. Smyth's diſcoveries, and when he has eſtabliſhed the grounds upon which he claims it, he may ſet up a canon of criticiſm well ſuited to maintain his grand diſcovery, and all future diſcoveries which he may *chooſe* to make, or which ſhall be found to proceed from a *like* deliberate reflection.

At p. 23 of his Letter to Mr. Wilberforce, Dr. S. introduces the mention of calamities which befel my family, in a way that renders it neceſſary for me to lay before the reader, at the ſame time my ſtatement, and his remarks:

My Statement, p. 9, 12.

In 1776 Dr. Ed. Johnſtone, now phyſician in Birmingham, was ſeized with putrid fever from attending ſome poor families

Dr. Smyth's Remarks, p. 23.

He informs us of two occaſions where it (muriatic acid gas) was employed in his own family; upon which I muſt needs

ies in Kidderminſter. The muriatic vapour was kept riſing continually in the room, and myſelf and the younger branches of the family were often officiouſly employed in ſtirring up the ſalt. My brother recovered almoſt miraculouſly from the laſt and worſt ſtate of this dreadful fever, and not one of the family, at that time conſiſting of fifteen or ſixteen perſons, was infected.

In 1783 a malignant fever broke out in the county gaol of Worceſter. The keeper of the priſon, his wife, and the ſurgeon who attended the priſoners, died; and ſo great and univerſal was the alarm, that attendance could not eaſily be procured. In this emergency Dr. James Johnſtone jun. was called upon, and he died too, but not till he had ſtopped the progreſs of infection in the gaol. My brother was infected by too ſcrupulouſly needs remark, that to ſtate the caſes of his two brothers, and a couſin, ſeized with the gaol diſtemper, as a proof of his father's knowledge of a certain method of preventing it, is a ſingular and new mode of induction.—He might with equal propriety and ſound logic have attempted to prove that his father's antidote to contagion might be uſed with perfect convenience to the ſick, from his declaration, that unleſs diſcharged at a diſtance from the patient, their extreme pungency would be offenſive to the lungs. We are further aſſured by Dr. John Johnſtone, that, owing to the marine acid vapour employed on thoſe occaſions, the fever was not communicated to the reſt of the family. But the gaol diſtemper ſeldom ſpreads, or is communicated by an individual, when removed from the fomes of

ſcrupulouſly diſcharging his duty; and, well aware of his danger, came to Kidderminſter, into the boſom of his family, to have the conſtant attendance of my father. Human art was exhauſted to ſave him, but in vain. All contagion was prevented from ſpreading in the family, which conſiſted of 12 perſons, by the conſtant uſe of muriatic vapour. We had all acceſs to my brother's chamber, and no one was materially affected except Major Johnſtone; who probably caught the contagion by attending my brother from Worceſter, ſhut up in a poſt-chaiſe, &c.

of contagion. Such inſtances afford at beſt but equivocal proofs of the efficacy of any means employed to prevent it.

I have put my ſtatement directly oppoſite to Dr. S.'s criticiſm, that the readers may ſee at once the accuracy of his repreſentation, the candour of his expoſition, and the acuteneſs of his induction. He does not diſplay his hardihood

dihood in contradicting it; he does not try his ſkill for candid expoſition, upon the particulars of it. He juſt ſays that I had informed my readers of two occaſions when it was employed in my family. The incautious or the inattentive part of Dr. Smyth's readers may ſuppoſe, that in his comment upon this information, he confutes the ſubſtance of what I had ſaid about both caſes. But his attempts to confute me are confined only to one of them, and upon that one he attacks me for a new and ſingular mode of induction, which in reality I did not employ. From the warineſs, however, of Dr. Smyth in not dropping one ſyllable of obſervation or conceſſion upon the other caſe, which was equally favourable to my ſide of the queſtion, and equally unfavourable to Dr. Smyth's, I infer that the Doctor has moſt exactly conformed to a very uſeful rule, which a great writer of antiquity lays down in his *Precepta Altercationis*—"*Ab iis quæ*

"*non*

" *non adjuvant, quam molliſſime pedem oportet* " *referre.*". Quintil. lib. vi. c. 4. If Dr. Smyth ever read the foregoing paſſage he has profited by it very notably; and if he has not read it, his conduct exemplifies another obſervation made by the ſame author, " *ſimilis eſt* " *arti, plerumque Natura,*" lib. viii., c. 4.

If I had ſtated that my brother and my couſin were ſeized with the gaol diſtemper, as a proof that my father knew how to prevent it, *then* ſhould I have been guilty of the abſurdity imputed to me. But the ſtatement ſo confidently aſſerted, and ſo triumphantly refuted by Dr. Smyth, occurs in *his* letter, *not* in *my* pamphlet. Dr. Smyth will hardly deny that the firſt of the foregoing caſes is accompanied with all proper notations of time and place, phyſician and patient, cauſe and effect. The time was 1776, or four years prior to Dr. Smyth's journey to Wincheſter—the place was Kidderminſter, where my father re-

 ſided—

ſided—the phyſician was that father, the patient was his ſon—the cauſe of the diſeaſe was contagion, communicated to my brother Edward when he attended ſome poor families—the effects of the application of muriatic acid gas were recovery from contagious diſeaſe to my brother Edward, and exemption from contagious diſeaſe to the reſt of my father's family.

My father had now been a practiſing phyſician for more than twenty years, and under the alarm which a domeſtic calamity excited in his mind, he would hardly have ventured upon the uſe of a medicine of which he had *no* diſtinct knowledge, no previous experience, no ſoothing ſuſpicion, that it could be employed with ſafety as well as advantage. If Dr. Smyth, in a ſtate of ſuſpenſe or conſcious ignorance, ſpared his patients at Wincheſter, Dr. Johnſtone, under the ſame circumſtances, may be ſuppoſed with the ſame ſolicitude to

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have ſpared a ſon. But my father was happily qualified by his "deliberate reflections" for immediate and correſpondent action. He diſcovered *more than an intention* to uſe the muriatic acid gas. He uſed it actually and ſucceſsfully.

Again I ſtated upon another occaſion, that my brother James was ſeized with the gaol diſtemper, and that a brother who was ſhut up with him in a poſt-chaiſe, had afterwards the local ſymptoms of contagion; that they came into a family conſiſting of twelve perſons; that muriatic acid gas was then employed by my father; that James died; that Henry recovered; and that the reſt of the family were not infected. James had been ſent into a gaol full of contagion at Worceſter, before the muriatic acid gas was employed, and being infected was removed to Kidderminſter, where it was uſed; but from the progreſs of diſeaſe, without effect, ſo far as related to him.

Now, Dr. J. C. Smyth himſelf caught the gaol fever at Wincheſter, of which he has given a long, and, no doubt, faithful account, under the head, "Deſcription of my own "Caſe," (from p. 15 to 27 of his Deſcription of the Gaol Diſtemper, &c.); in the whole of which Deſcription *not a word is ſaid of mineral acids in any ſhape.* Even in the experiments on board the Union hoſpital ſhip, notwithſtanding the ſhip ſtill continued to be fumigated once every day, contagion was not entirely deſtroyed, for one marine died, and one nurſe had a ſlight attack of fever.* Thus, then, Dr. Smyth did not prevent contagion from reaching himſelf, notwithſtanding all his previous deliberation and reflection; nor did the partial uſe of fumigation prevent the death of one man, even during the courſe of experiments. But let us apply Dr. Smyth's reaſon-

* Account of an Experiment, &c. p. 55.

ings

ings more immediately to the caſe he has put. Did it therefore become impoſſible, becauſe my father had diſcovered a method of deſtroying contagion in particular circumſtances, that contagion ſhould exiſt, and that my brother ſhould be expoſed to it? Does it not exiſt, notwithſtanding all Dr. Smyth's deliberate reflections on the power of mineral acid gaſes? Did it ceaſe to exiſt when freſh ſubjects were brought into the hoſpital ſhip, where his experiments were carrying on? If Dr. Smyth's deliberate reflections had taught him an effectual remedy and preventative of contagion, why was he infected at Wincheſter? I ſhould have diſdained to put theſe queſtions, had not Dr. Smyth inſulted the memory of my father by an unfeeling and wanton ſneer. For the phyſician cannot carry his cenſer in his hand whereſoever he goes, and he muſt always be expoſed to the riſque of contagion before he has had the power of taking thoſe ſteps of

cure

cure or prevention which his ſkill may enable him to direct. Such was the caſe of my brother. He was called upon to perform a great public duty at a moment of emergency and alarm. The gaol fever was ſpreading in the caſtle of Worceſter fatally. He caught the infection probably at his firſt viſit, before it was poſſible to employ the mineral acid gaſes, and whilſt he was giving thoſe directions for employing them, with other remedies, which were to reſtore health and ſafety to others; and his death is now attempted to be made more bitter to us by the cruel reflection, either that my father did not know a certain power of preventing contagion, or that in the caſe of my brother it was not employed. That it was employed too late, and therefore without effect, we have all to lament. But I ſhall leave it to the ſoundneſs of Dr. Smyth's logic to prove, whether it be a ſingular and new mode of induction, to make an unqualified deduction

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of failure, where there was in the firſt inſtance no application. I ſhall leave it to his ſagacity as a phyſician to determine, whether it be not poſſible for one who knows the power and the application of mineral acid gaſes, to be expoſed to contagion and to be infected by it. And I will now leave it to any man of humanity, to aſſign the proper epithet to that critic and to that phyſician, who ſhall ſo far wreſt the powers of reaſoning and of ſcience to an unworthy purpoſe, as to libel the fame of a father, by inſinuating that the life of his firſt-born child, at leaſt, was not ſaved by thoſe means of cure which he pretended to poſſeſs; when the critic muſt know, from the ſtatement, that the father did employ thoſe means as ſoon as it was in his power to employ them; and the phyſician muſt know, if he has any experience, that theſe means when not employed early, may be tried in vain.

I quite agree with Dr. Smyth's remark, that

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the *fomes* of contagion muſt be much more dangerous than an individual caſe; and yet one man infected with malignant fever may communicate it to thouſands. It is not poſſible to gain a more unequivocal proof of the benefit of a remedy, than that the effects which it was intended to prevent, were prevented.

It ſhould be obſerved that my father approved and recommended the uſe of muriatic acid gas only in putrid diſeaſes of the more virulent kind, when the danger from contagion was greateſt. In the common ſcarlatina my father ſeldom employed it, unleſs the ulceration of the throat became the moſt threatening ſymptom. His great truſt in the treatment of the general diſorder was in acids and cool open air, which he conſidered as ſpecific antidotes to the miſchief of that poiſon.

In his remarks on the angina and ſcarlet fever of 1778, publiſhed in Vol. III. of the

Memoirs

Memoirs of the Medical Society of London, there is no mention of mineral acid gaſes; but there is an expreſs reference to the ſucceſsful uſe of bark, wine, acids, antiſeptic diet, as a practice recently and fully deſcribed by his ſon James, and in his own Diſſertation on Fever. It is impoſſible that he ſhould forget the uſe of muriatic acid gas which occurs in both of them; and when the diſeaſe puts on its moſt malignant appearance he refers to his ſon's Treatiſe for ample directions how to cure it.

The leading object of my father's paper was to oppoſe Dr. Withering's diſtinction between ſcarlatina and angina maligna, and the method of cure by alkalis; which my father's theory led him to conſider as favourers rather than correctors of putrefaction.

My father's knowledge of the muriatic acid, his ſkill in applying it commodiouſly as well

as ſafely, and his confidence in its power to check contagion, increaſed, I am ready to allow, with his increaſing experience; and for ſuch experience there was ample room between 1758, when he publiſhed his book, and 1776 when he preſerved my brother Edward and the reſt of his family. His claims, ſo far as I am intereſted in ſupporting them, during this interval are endowed with the very beſt properties which an advocate for his cauſe could wiſh. *In loco opportunitas, in tempore longinquitas, in occaſione commoditas ad faciendum idonea, in facultate copia et poteſtas earum rerum propter quas, aliquid facilius fit, aut ſine quibus, omnino confici non poteſt conſideranda eſt. Cic. de Invent. lib.* 2. His final and determined preference of muriatic acid to all the other expedients which his own practice, as well as that of his predeceſſors and contemporaries, had ſanctioned, and his diſcontinuance of heat

when

when the apartments were ſmall, are intelligible and important inſtances of practical improvement upon his own diſcovery.

After all the trials which Dr. Smyth had made, and all the knowledge which he had accumulated in 1795, Dr. S. allows that future experience may improve upon what has been already done; and ſurely upon looking back to his own paſt experience he will find it to have been progreſſive.

If Dr. Smyth, when he was at Wincheſter, had really employed both the muriatic and nitric acid gas, if he had employed them directly and ſpecifically for the purpoſe of deſtroying contagion, if he had employed them with thoſe precautions which my father ſuggeſts, and the propriety of which Dr. S. cannot diſpute, he muſt have found both of them ſafe and both of them uſeful; and he might then have alſo formed ſome opinion of their comparative ſafety and uſefulneſs. But in what-

ſoever manner, and with whatſoever effect, either of them were uſed by him at Wincheſter, he, by his own account, had much to learn; much too he did actually learn from the experiments made with Mr. Hume of Long-Acre; and well does it deſerve to be remarked again and again that theſe experiments were prudently made upon irrational animals, after the numerous opportunities which a priſon and a hoſpital muſt have afforded in 1780, for aſcertaining the effects of mineral acid gaſes on human beings. Yet if I do not miſunderſtand Dr. Smyth he then diſcovered, for the firſt time, that the muriatic acid gas was leſs reſpirable than the nitric.

Dr. Smyth infers that becauſe my father mentions the ſulphureous and marine acids together, that he was evidently unacquainted with any diſtinction in their application, and conſidered them as equally ſafe and equally uſeful. Were I to admit ſuch a concluſion

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from ſuch premiſes, it would ſtill be apparent that my father had uſed the marine acid, or ſeen it uſed; that he knew the method of raiſing it eaſily, and that he diſcerned the neceſſity of raiſing it at a diſtance from the patient, and that the errors imputed to him about the ſulphureous were incompatible with the judicious application of the marine. But I have already reſiſted the concluſion, becauſe it is impoſſible for my father to have employed both the ſulphureous and marine acids without perceiving a difference in them, without obſerving that in equal quantities they are not equally ſafe and equally uſeful, without knowing that in different quantities they could not be equally commodious, even if he thought them equally uſeful.

My father, in common with phyſicians, apothecaries, and nurſes, was aware that burning ſulphur had been uſed to drive out contagion; but if the uſe of it as a corrector of con-

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tagion ever formed a part of his practice, there is ſufficient proof that he diſcontinued the ſulphureous while he continued the marine. The ſulphureous was not uſed in 1776 when my brother Edward was ill. It was not uſed in 1783, when my brother James died and my brother Henry recovered. It was not mentioned in 1779 when James publiſhed his Treatiſe. It was not mentioned in that year by my brother Edward, in his academical exerciſe at Edinburgh. It is not mentioned in my father's manuſcripts. It is not mentioned in Mr. Crane's letter.

Some further evidence which my brother Edward ſupplied, and which I endeavoured to convey in the moſt unadorned and inoffenſive terms, has been treated by Dr. Smyth with the diſingenuouſneſs of a ſophiſt, and the ſupercilioufneſs of a ſcoffer. I ſhall therefore produce that evidence again, preciſely in the ſame terms which I uſed before. I ſhall vindicate it from the objections which are raiſed againſt

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it by Dr. Smyth, and I ſhall ſtrengthen it with ſome additional matter which may convince my verſatile and doughty antagoniſt, *Scuto ſibi magis, quam gladio opus eſſe. Liv. lib. iii.*

" In 1779 Dr. Edward Johnſtone propoſed " this practice in the clinical ward of the " royal infirmary, Edinburgh. It had not a " fair trial, being uſed only one day, and then " laid aſide on ſlight grounds. He defended " it afterwards in one of the diſſertations for " his degree before the profeſſors of that uni- " verſity."

Such were my words in p. 10 of my pamphlet. I ſhall now produce the anſwer made to them by Dr. Smyth: " As for what he ſays " of his brother, Dr. Edward, having men- " tioned the marine acid at Edinburgh in the " year 1779, and that it was tried for one day " in the royal infirmary of that city, it only " ſhews that Dr. Edward as well as his bro- " ther, Dr. James, had, at the time, heard

" ſomething

" ſomething concerning the marine acid from
" Dr. Prieſtley. But ſuppoſing the fact to be
" exactly as he has ſtated it, it would not im-
" preſs us ſtrongly in favour of the marine
" acid vapour to know that it had been tried
" and rejected at Edinburgh, whilſt Dr. Cul-
" len, the celebrated profeſſor of chemiſtry,
" was at the head of the medical college and
" chemical wards; whom no one who ever
" knew him will accuſe of having been an
" enemy to experiment or even to novelty.
" The inconvenience and danger, therefore,
" attending its uſe was probably the true
" cauſe of its being diſcontinued; and it was
" never afterwards, ſo far as I know, either
" tried or mentioned at Edinburgh, or any
" where elſe." Letter, p. 24.

My obſervations on the preceding paſſage in Dr. Smyth's Letter will be employed; 1ſt, upon what it does contain, and 2dly upon what it ought to have contained and does not.

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How far my brother James was indebted for his knowledge of the marine acid to Dr. Prieſtley has been already conſidered, and much of the reaſoning which I then employed is applicable to my brother Edward. There is no evidence that Dr. Edward ever had any correſpondence or converſation with Dr. Prieſtley on the powers of the marine acid, or that he had ever read the work which my brother James had read, and, in ſome reſpects, miſunderſtood. But unleſs error, like putrid fevers, be contagious, my brother could not learn from Dr. Prieſtley that the marine acid contains powers which Dr. Prieſtley neither expreſsly nor virtually aſſigns to it. He could not believe on the authority of Dr. Prieſtley that the marine acid was uſeful in deſtroying contagion, when Dr. Prieſtley in his letter to my father had complained of being miſunderſtood by my brother James, and had poſitively maintained that in his own opinion,

and according to the reſult of his own trials ſo far as they had then gone, it had no ſuch uſe. Dr. Edward had, indeed, in the year 1779 and before, learned ſomething upon the ſubject from another quarter;—ſomething true in theory and beneficial in practice—ſomething reſulting from medical, and therefore more important to him than deductions from chemical experiment—ſomething which inſtructed him in the real efficacy of the marine acid to deſtroy contagion, and which pointed out to him the method of uſing it ſafely as well as advantageouſly. *That ſomething* he had learned from his father. He could not but remember the uſe of the marine acid in 1776, for in that year his own life and the health of fifteen or ſixteen perſons in his father's family had been preſerved by it. He could not but underſtand it, for he was then training up under the eye of his father for the medical profeſſion, and in medical knowledge he had *then* made ſufficient

proficiency

proficiency to attend ſome poor families in Kidderminſter, who were under his father's care.

In 1758 my father had publiſhed a Treatiſe upon Fevers, in which he mentions the uſe of the marine acid, deſcribes the method of raiſing it, and gives proper precautions for employing it. Without any undue partiality as a ſon, or any extraordinary curioſity as one who intended to be a phyſician, Dr. Edward might have been induced to peruſe that book. But if, upon the peruſal of it, the ſame ſcruples had occurred to him which occurred to Dr. Smyth, about my father's opinion upon the *comparative* utility of the marine acid in 1758, thoſe ſcruples muſt have been removed by events ſubſequent to that time: they muſt have been removed by his own experience, and by the experience of his father's family, when the marine acid was uſed with ſuch favourable and unequivocal effects in 1776; they muſt have

been removed by the perusal of a book published by his brother Dr. James in 1779. I shall presently bring forward evidence, to shew that Dr. Edward *had* perused that book with attention and conviction; and I shall now quote for a second time some passages from the book itself, to prove that he could not read it without knowing that such a thing as the marine acid existed; that it possessed the power of destroying contagion; and that his father having perceived in it that power, had employed it for that purpose. My brother James says, in p. 4 of his Treatise on the Malignant Angina:

That "the ulcerated sore throat had for "some years past very rarely appeared; that "in those few instances which did occur its "malignity was much abated; and that the "management of it was become so familiar "in many places that the good women them-"selves cured it by means of the bark and an-"tiseptic steams." Again,

Again, in p. 6, he ſays, "antiſeptic ſteams "compoſed of vinegar, myrrh, and honey, or "the acid air raiſed by pouring oil of vitriol "upon ſea ſalt, conſtitute," together with other particulars which he enumerates in the ſame page, "the only ſucceſsful method of "cure. This method applied early and with "due perſeverance, has ſeldom failed."

Yet farther, in pages 110 and 111, he ſays, "As it is impoſſible too cautiouſly to guard "againſt the effects of ſo putrid a contagion, "the acid air, or ſpirit of ſalt, ſhould be kept "riſing continually in the room, by pouring "oil of vitriol once or twice a day on ſea ſalt "placed in a convenient veſſel. This ſpirit "will riſe in the moderate degrees of heat "from 60 to 70 degrees of Fahrenheit's ther-"mometer, ſo as to be perceived in every part "of the room by its penetrating acid ſmell. "This method of correcting vitiated air, "which is uſeful in this and every other pu-

"trid

" trid diſeaſe, *was long ago ordered by my fa-*
" *ther*, and is now recommended by Dr.
" Prieſtley.

" To have the patient's chamber filled
" with theſe vapours is the *ſureſt* method of
" preventing the ſpread of contagion; for
" which reaſon, as well as the patient's parti-
" cular advantage, care ſhould be taken to
" preſerve the air as pure as poſſible."

So wrote my brother James in 1779; ſo read my brother Edward, as will be hereafter ſhewn, in that very year; and if he underſtood what he read, and believed what he underſtood, he muſt have known that the marine acid poſſeſſed the powers which are imputed to it, and that his father had long before 1779, called thoſe powers into action, by ordering it for the benefit of his patients.

I produced the three preceding paſſages from the Treatiſe on the Malignant Angina before, in order to ſhew that my brother James

derived

derived his knowledge of the marine acid from my father. I have produced them *a ſecond time* in order to ſhew that my brother Edward derived part of his knowledge from the book of his brother James, and that he ſtood in no need of learning any thing from Dr. Prieſtley; to whom *alone* Dr. Smyth moſt contemptuouſly and moſt raſhly aſcribes "*the ſome-*" "*thing,*" and by implication, *the whole,* which my brother Edward knew "when he pro-" "poſed the practice of my father in the royal" "infirmary at Edinburgh."

My brother, Dr. Edward, might or might not have known that Dr. Prieſtley in writing to my father, had expreſſed very ſtrong doubts on the power of the muriatic acid gas to deſtroy contagion; but he muſt know that Dr. Prieſtley, even if he admitted that power, was a chemiſt not a phyſician, and therefore would have recommended rather than ordered. He muſt know that my father being a phyſician

was

was required to order; and that if he were an intelligent and an honeſt phyſician, he would not have ordered any powerful medicine, which upon *proper* occaſions, he was not prepared to *recommend*. He muſt know, that my father in 1758 had *mentioned* at leaſt the marine acid, had deſcribed the method of raiſing it, and had expreſsly permitted the uſe of it, whether he at that time, did or did not *decidedly* prefer it to the antiſeptic ſteam of vinegar. He muſt know that ſubſequently to 1758 and long before 1779, my father *had given it that preference*, that he had ordered it in his practice, that he had recommended it to his ſons James and Edward, that he had availed himſelf of it for the protection of his own family, and had even familiariſed the uſe of it among his poor patients in the neighbourhood of Kidderminſter. Knowing theſe things at this time, he knew more about the powers of the acid gaſes than Dr. Smyth appears to have

known

known at the ſame time, or even at a later period. He knew *that*, which no well-informed phyſician, nor well-bred diſputant would condeſcend to deſcribe by the word *ſomething*. He knew it *not* from Dr. Prieſtley but from his own experience when he was cured by his father, from the teſtimony of his own family when they were preſerved by his father, and from his own reading when he looked into two books, one of which contains deſcriptions and directions of his father, about the marine acids, and the other of which ſhews that his father had long ordered it.

Dr. Edward *mentioned*, as Dr. Smyth ſays, the marine acid, or, as I ſay, "propoſed "a practice," which included the uſe of it in the royal infirmary of Edinburgh. It may be preſumed then, that the conviction of a young man muſt have been very ſtrong, before he would *venture* to propoſe any new and active medicine, to inſtructors venerable from their

age, their experience, and their ſtation. It can hardly be imagined that an opinion founded upon ſomething he had heard about the marine acid from Dr. Prieſtley, would have emboldened Dr. Edward at all to *ſuggeſt*, or induced any of the Edinburgh profeſſors at all to *permit* a ſingle experiment, involving the uſe of a very powerful agent, and amounting to an alteration in the eſtabliſhed treatment of putrid diſeaſes in a royal infirmary. Whatever may have been the fondneſs of Dr. Cullen or his aſſociates for novelty, and whatever their indulgence to experiments, they would not upon grounds ſo ſlight, have ſanctioned a practice ſo dubious, in caſes ſo dreadful. But my brother had weightier reaſons than the ſomething he is ſaid to have learned from Dr. Prieſtley, for making the propoſal, and weightier too than that ſomething, were the reaſons which led an Edinburgh profeſſor even for once to accede to it.

The

The marine acid then *was tried*, and it was alſo rejected at Edinburgh, "*whilſt* Dr. Cullen, "the celebrated profeſſor of chemiſtry, was at "the head of the medical college and chemical "wards; whom no one who ever knew him will "accuſe of having been an enemy to experi-"ment, or even to novelty." Be it ſo: but I am not ſure that the authority of Dr. Cullen will peculiarly bear upon this queſtion, for Dr. Cullen was not at the head of the ward, nor indeed had any *practical* concern in the infirmary, when the experiment was propoſed. Dr. Hope, not Dr. Cullen, was at the head of the ward. The trial was made with the permiſſion not of Dr. Cullen but of Dr. Hope. The marine acid was laid aſide with the knowledge and approbation not of Dr. Cullen but of Dr. Hope. My brother did mention it to Dr. Hope, but to Dr. Cullen he mentioned it *not*, nor is there any evidence that Dr. Cullen, though he was in the room when my brother talked with Dr.

Hope, attended to their converſation, or had ever heard of the propoſal, the trial, or the rejection, from either of them, or any other perſon whatſoever.

The marine acid, as I ſaid, was uſed only for one day, and therefore I ſtated that it had not had a fair trial, and had been laid aſide on ſlight grounds. Surely Dr. Smyth, who now knows the power of the muriatic as well as the nitric gas to deſtroy contagion, though he holds the nitric to be more reſpirable than the muriatic—Dr. Smyth, who at Wincheſter had only ſufficient confidence in the fuming nitrous acid vapour, to apply it there in ſmall gallipots to the ſick—Dr. Smyth who by his own confeſſion, had erred in the means of procuring the nitric at the very time when he profeſſes to have formed ſome intention of applying it, and who for many years after had met with no favourable opportunity of procuring it—Dr. Smyth who, in order to remove his

own

own doubts, or to ſupply the imperfections of his own knowledge, was at ſome period ſubſequent to his Wincheſter journey content to make experiments upon two little animals, and by thoſe experiments was enabled to diſcern the ſuperior excellence of the nitric to the muriatic acid gas in point of reſpirability; and who, in recommending it afterwards for the cure or preſervation of human beings, found it neceſſary to avert inconvenience or danger, by laying down the moſt explicit cautions for the uſe of it on board the Union hoſpital ſhip. Surely, after ſuch gradual acquiſitions of knowledge, with ſuch a deep ſenſe of the difficulty with which it is gained, and with ſuch direct experience of the conſequences which ariſe both from want of caution and want of perſeverance, ſuch a phyſician, or if he pleaſes, ſuch a diſcoverer as Dr. Smyth, will readily allow a trial of one day not to have been *quite fair*. He will allow the inconveniences and

dangers

dangers reſulting from one experiment, not to be ſufficient grounds for immediate and abſolute rejection. He will allow that even if the nitric acid gas had been tried *without due precaution*, the *ſame* inconvenience and danger would have ariſen, and that under the ſame circumſtances, it in all probability, would have been attended with the ſame effect of diſcontinuance. In either caſe, however, the conduct of perſons who, after one trial, and that perhaps reluctantly begun and incautiouſly conducted, had rejected the uſe of either gas, can have little weight againſt the more numerous, the more judicious, and the more ſucceſsful experiments made upon the muriatic gas by my father, and upon the nitric gas by Dr. Smyth.

No man feels more veneration for the profeſſional abilities, or more affection for the ſocial virtues of Dr. Cullen than my father, and my brothers James and Edward, were

accuſtomed

accuſtomed to feel and to expreſs. During my reſidence at Edinburgh it was ſoon my ill-fortune to be deprived of his moſt valuable inſtructions. I paid, in common with my fellow-ſtudents, a juſt tribute to his memory by putting on mourning; but I had not the honour of living with him upon terms of perſonal intimacy; and from his writings I am not warranted to decide upon the *extent* in which he was likely to be influenced by that ſort of fondneſs, or at leaſt indulgence towards experiment and novelty, which is aſcribed to him by Dr. Smyth. In my general obſervations, however, upon the characters of men eminently diſtinguiſhed by their talents and attainments, I have ſometimes found that the love of novelty is moſt active, when the indulgence of it, is not adverſe to ſome peculiar or favourite ſyſtem. If, therefore, confiding in the ſufficiency of his own theoretical opinions, and the ſoundneſs of his own practical rules

for

for the treatment of putrid diſorders, Dr. Cullen *was* inclined to undervalue the uſefulneſs, or to exclude the conſideration of the experiments made upon the marine acid by my father, I have only to lament that a very great and a very good man, was in one inſtance ſubject to an infirmity, from which Dr. Smyth himſelf is not entirely exempt. I think the diſcoveries of Dr. Smyth more remarkable for novelty in the means than originality in the principle. But I know that Dr. Cullen was not wanting in candour to acknowledge, or ingenuity to improve upon every hint which he caught from Hoffman; and were he now living to contraſt the claims of Dr. Smyth with thoſe of my father, I ſhould repoſe the utmoſt confidence in the accuracy of his judgment and the fairneſs of his verdict.

The name of Dr. Cullen has been introduced *not by me* but by Dr. Smyth; but perhaps after all, the marine acid was neither

tried

tried, nor laid aſide, by his direction, or even with his knowledge. Be this as it may, I ſhall not retract my opinion that it was laid aſide without a fair trial, and I leave my readers to decide upon the validity of the reaſons which I have aſſigned for adopting, avowing, and retaining that opinion.

Dr. Smyth tells us, that "after being diſ-
"continued on account of the inconvenience
"and danger attending its uſe, the marine
"acid gas was never afterwards, ſo far as he
"knew, either tried or mentioned at Edin-
"burgh, or any where elſe."

Surely the limitation of Dr. Smyth's knowledge as ſtated by himſelf, is no proof that the knowledge of my brother Edward was equally limited; and if the marine acid gas, though eventually diſcontinued, had been mentioned and tried at Edinburgh in 1779, an impartial reader will now be diſpoſed to admit that the trial and the mention of it at *that* time, were

the conſequences of an intimation from my brother Edward, and that ſuch intimation was the conſequence of *ſomething* which he had before heard and ſeen concerning the marine acid, not as recommended by Dr. Prieſtley, but as known to his father and his brother, and publiſhed by both.

What Dr. Smyth knew, or knowing might find it convenient to ſuppreſs, about the trial or the mention of the marine acid at Edinburgh or any where elſe, after the diſcontinuance of it in that city in the year 1779, I leave Dr. Smyth to determine. His ignorance may, or may not, be real; it may be even venial, if real. But it is no meaſure for the knowledge of my brother Edward, and therefore has no weight in counteracting his teſtimony. It is poſſible, and I add certain, that previouſly to the diſcontinuance of the marine acid at Edinburgh, and ſubſequently to it, he knew *facts* which Dr. Smyth did not know—

that

that he had ſeen trials which, in 1780, and, ſo far as appears, before 1795, Dr. Smyth had not ſeen—that in caſes of diſeaſe unknown to Dr. Smyth, at times unknown to him, in places unknown to him, and before perſons unknown to him, my brother might *himſelf* have mentioned the uſe of the marine acid, and himſelf have uſed it, though a total ſilence about it, and a total diſuſe of it, prevailed at Edinburgh. It is probable, too, that in converſing with profeſſional men about his father's practice and his own, he might have heard the muriatic acid mentioned by *other* perſons, for other perſons have long known the practice of both; though the evidence of my brother is rudely ſlighted by Dr. Smyth, and the name of my father is ſaid not to have been even heard of by him till 1802.

I ſhall now proceed to the conſideration of that part of my brother's evidence which Dr. Smyth ought to have mentioned, but does not;

and in ſo doing I ſhall have occaſion to introduce the additional matter which has been already noticed.

In the paſſage ſome time ago quoted from p. 10 of my pamphlet, it is ſaid that "my "brother, who had propoſed the uſe of the "marine acid in the clinical ward* of the "royal infirmary, Edinburgh, where it was "uſed only one day and laid aſide upon ſlight "grounds, defended it afterwards in one of "the diſputations for his degree before the "profeſſors of that univerſity."

Dr. Smyth, who, in his remarks on the theſis of my brother James, expatiates on the omiſſion of the marine acid in that theſis, ſhews a want of attention or a want of candour in paſſing over a Latin Diſſertation for a degree, in which the uſe of the marine acid *was* mentioned and defended by *another* brother.

* I muſt here acknowledge a miſtake: it was not ſpecially the clinical ward, as will be ſeen hereafter.

The

The defence itſelf might be unſatisfactory to the profeſſors. But if my brother entered upon that defence when the muriatic gas had been tried and laid aſide, his conviction muſt have been firm, though his arguments had unfortunately been inconcluſive; and at all events he cannot be ſuppoſed, without ſuch conviction, to have undertaken the defence of a principle which he was not impelled by vanity to hold forth as a diſcovery of his own, and which, in the very moment of defending it, he believed to have been the diſcovery of his father. Such puerile levity, ſuch unwarrantable audacity, ſuch unprofitable wantonneſs muſt not be imputed to a young man, who was ſpeaking in the preſence of his ſeniors and his inſtructors, and whoſe credit depended not upon their aſſent to his opinions, but their approbation of his exertions. Upon this ſubject, however, little room is left for cavil to my opponent. I have aſſerted that my brother defended the practice

of

of *his father*: I ſhall now endeavour to ſupport and illuſtrate the aſſertion; and whatſoever impreſſion I may make upon the underſtanding of Dr. Smyth, yet contending for a plain matter of fact, I ſhall aſſume, without reſerve and without apology, that the delicacy becoming Dr. Smyth's profeſſion, the prudence becoming his age, and the diffidence becoming his cauſe, will alike prevent him from trifling with the veracity of my brother Edward, and my own.

The queſtion upon which my brother drew up his academical exerciſe now lies before me in the hand-writing of Dr. Hope, and runs thus:

"*Juvenis* 18 *annos natus, die Menſis* 20 *ſub dio dum plueret verſatus, poſt reditum rigore et laſſitudine correptus fuit.*

21. *Laſſitudo cum capitis dolore.*

22. *Calor pulſus frequens, dolor capitis magnus; ſanguis parvâ quantitate e naribus ſtillavit.*

vit. Emeticum ſumpſit ſine ſymptomatum levamine vel ullius in ſtomacho cruditatis indicio.

23. *Caput dolet multum, adſunt calor ſitis cum pulſu frequenti debili: in vultu hebetudo & tumefactio, adeſt delirium cum tendinum ad carpum ſubſultu, os ſaliva ſcatet quam copioſe expuit. Alvus eſt ſtricta. Sudor perpetuus urget. Quær. morbus: prognoſis: ratio ſymptomatum: Indicationes curæ et remedia:*

"Do. Jonston,
31 May 79, prop. J. H."

In diſcuſſing the foregoing ſubject my brother took occaſion to mention and to juſtify his father's uſe of the muriatic acid.

Dr. Edward lays no ſtreſs upon the merits of his academical exerciſe as a compoſition. But, in a diſcuſſion upon the claims of his father, he conſidered it as a teſtimony which, in

ſome

ſome degree, ſupported them, and in conſequence of Dr. Smyth's letter to Mr. Wilberforce, he endeavoured to obtain it, and wrote to Dr. Gregory in the following terms:

DEAR SIR,

Preſuming on the kindneſs I received from you during my reſidence at the univerſity, I take the liberty of troubling you on a ſubject peculiarly intereſting to myſelf and my family. Among many other marks of your attention, you did me the honor to appoint me your clinical clerk during the latter period of the ſeſſion 1779. You may perhaps recollect, that immediately after the courſe of clinical lectures was cloſed, the ward appropriated for the men was filled with ſoldiers, belonging to the duke of Buccleugh's fencibles, affected by a contagious fever. Having experienced the good effects of muriatic acid vapour in obviating contagion, under the direction of my father at Kidderminſter, I recommended it to

the

the late Dr. Hope, who immediately ordered it to be uſed, but in a ſhort time, I think the next day, diſcontinued it.

Among the exerciſes for a degree Dr. Hope gave me the caſe of one of theſe ſoldiers to comment upon. In my commentary I ſtated that my father had long and ſucceſsfully uſed the muriatic acid vapour to prevent contagion. Dr. Hope very candidly explained that his reaſons for diſcontinuing it were—aggravated cough, and ſome pneumonic ſymptoms which attended that fever; and, upon conſulting Dr. Bell's theſis, I find theſe are regarded as conſtant ſymptoms. I then ſtated my own experience of its uſe during a violent typhus caught by attending the poor at Kidderminſter in the ſpring 1776, and that not one of a very numerous family was infected by me, which was attributed to the vapour being conſtantly raiſed in the room, and that I did not perceive the leaſt inconvenience from it. You and the late

Dr. Cullen were in the library during my examination; but I can ſcarcely expect that after a lapſe of near 26 years you can remember what then paſſed; and indeed my dear Sir, I have only to requeſt that you will be pleaſed to get a proper perſon to look over the infirmary-book for May or the beginning of June 1779, and if any report is made reſpecting the uſe of the muriatic acid vapour, to procure me a certified extract of it, and likewiſe to get me a certified copy of my commentary on the caſe given me by Dr. Hope, which I believe, agreeably to the rules of the univerſity, is depoſited in the library, and I ſhall, with gratitude, diſcharge any expence you may incur. If you have ſeen my brother's eſſay aſſerting my late father's claims to the diſcovery of the effects of mineral acid vapour in deſtroying contagion, and Dr. C. Smyth's anſwer, you will perceive the neceſſity of my procuring theſe documents. My brother, who originally took

up

up the buſineſs, is preparing an anſwer to Dr. C. Smyth, which I will ſend you when publiſhed.

I am, dear Sir, with the greateſt eſteem,

Your obliged and faithful ſervant,

E. JOHNSTONE.

Lady-Wood, near Birmingham,

February 24th, 1805.

I ſhall ſubjoin the anſwer which my brother had the honor of receiving from the very learned Dr. Gregory:

Edinburgh, Saturday evening,

13*th April,* 1805.

DEAR SIR,

You muſt, no doubt, have wondered much at my delay, and thought me very negligent in not ſooner anſwering your letter of Feb. 25th—but do not condemn me unheard. I loſt no time in making the proper inquiries at the college library by means of the librarians,

and at the royal infirmary by means of Dr. James Hamilton, the ſenior phyſician of it, whom probably you will remember. He has been very obliging on this occaſion, and has taken a great deal of trouble about it, but hitherto in vain. He can find no veſtige of the uſe of the muriatic acid vapour about the time and in the manner that you mention. But again and again he begged of me not to write to you till he ſhould have made further reſearches. Nay, even this day, when he told me of his hitherto bad ſucceſs, he wiſhed me not to write to you, as he is reſolved, as ſoon as he can command two or three hours for the purpoſe, to make one trial more. The truth is, the regiſter of the infirmary has been very irregularly kept; and many of the original report books have *been ſtolen* before they were quite finiſhed, or *could* be tranſcribed into the regiſter.

As to the *written exerciſes* of our *candidates*, I could

could have told you, from diſtinct memory, that *about* or *before* the year 1779 the practice of *binding* them up, and *keeping* them in the college library, was given up for very ſubſtantial reaſons. If I remember right, ſome of thoſe volumes, formerly bound up and kept in the college library, were *cut up*, and the ſeveral profeſſors took away the *caſes* which they had given out, and the commentaries written on them. Theſe kinds of exerciſes have never ſince been kept. But not being ſure of the *preciſe time* of that change I inquired at the under librarian's, who aſſured me there were no ſuch MS. volumes in the library, unleſs they were in ſome of the locked cheſts, the keys of which were kept by Mr. Profr. Dalzel (who ſucceeded Rabbi Robertſon as principal librarian). Mr. Dalzel was at that time, and indeed is ſtill, ſo much indiſpoſed as to be unable to go to the library; but he aſſures me (and he is one of the moſt accurate men I

ever

ever knew) that there are no ſuch MS. in the locked cheſts, the contents of which he knows perfectly; and that there have been no ſuch MS. volumes in the library in his time. All this I could have told you near a month ago, but I waited day after day for Dr. Hamilton's final anſwer. If he ſhall even yet find the record which you inquire after, I ſhall without delay, ſend you an authentic copy of it. But the chance of this ſeems to me very ſmall.

Your's moſt truly,
J. GREGORY.

According to his own views of propriety and convenience Dr. Smyth will account for his ſilence about my brother's academical exerciſe, which had been mentioned in my ſtatement as *one* of the evidences tending to ſupport the claims of my father; and on the peruſal of Dr. Gregory's letter he will find that my brother Edward is not reſponſible for the probable loſs

of

of a document which I ſhould have gladly produced, if he had been fortunate enough to obtain it. In the mean time he will give Dr. Edward credit for memory enough, not to be *groſsly* miſtaken upon the plain and ſubſtantial facts of having propoſed my father's practice in the infirmary, and defended it in his exerciſe; for veracity enough, not to have invented a ſtory about circumſtances which never exiſted; and for prudence enough, not to appeal to a document which muſt have been injurious to his honour, if, upon being found, it had contained *no* veſtiges of the muriatic acid, as uſed by my father.

I know not what reliance the candour or the condeſcenſion of Dr. Smyth will induce him to place on the mere accuracy of Dr. Edward's recollection. But if he will concede to Dr. Edward even a ſmall portion of common ſenſe, he will find in the letter written to Dr. Gregory very clear evidence that my father knew

and

and employed the marine acid gas at leaſt in the year 1776; that Dr. Edward makes no mention of Dr. Prieſtley; that he does not controvert, at leaſt, my late father's claim to the diſcovery of the effects of the marine acid in deſtroying contagion; that he had himſelf experienced the good effects of it; that he had mentioned thoſe effects to the late Dr. Hope, recommended the uſe of it in the infirmary, and in an exerciſe for his degree had ſtated that his father had ſucceſsfully uſed the muriatic acid vapours to prevent contagion. If the exerciſe had been found, his evidence probably would have been fuller and more diſtinct. It ſo happens, however, that having been induced by Dr. Smyth's letter to look over his papers, he lately found one which may in part ſupply, what the exerciſe contained. That paper is now before me in the hand-writing of my brother. It is written in Engliſh, and was prepared for the purpoſe of

being

that when the diſorder was aſcertained I could eaſily foretell what plan he would direct in caſe of putrid fever, which uſed to be very prevalent in this town. He always directed the fumigation of oil of vitriol and ſalt, the patient to be waſhed in vinegar and water, with bark and mineral acids according to circumſtances, and plenty of freſh air.

In 1776 a fever broke out in the workhouſe here; he ordered the muriatic vapour, and his uſual plan.

In 1784 a fever broke out in the workhouſe here; he ordered it again, and was thanked by the town. Indeed I believe there are few caſes on record in which practitioners have been more ſucceſsful in curing fevers than Dr. Johnſtone, of which this town and neighbourhood are ſufficiently grateful in their remembrance.

I know not what Dr. Smyth means by *ex-parte* evidence; it is impoſſible that he can

preſume to deny facts which he has not inveſtigated, and which I am ready to ſtand forward to ſubſtantiate.

I remain, dear Sir,

Your obedient ſervant,

(Signed) JOSEPH CRANE.

(Directed)

Dr. John Johnſtone, Birmingham.

In my pamphlet I endeavoured to relate facts nearly in their chronological order. But the laws of controverſy are more complex than thoſe of ſtatement, and therefore that order has been diſturbed, by the neceſſity impoſed upon me ſometimes to adopt the arrangement which Dr. Smyth found moſt convenient in his Letter to Mr. Wilberforce, and ſometimes to ſtep aſide into collateral diſcuſſions, which the cavils or the revilings of Dr. Smyth compelled me to purſue. It will, therefore, be proper for me, in this place, to recapitulate the

various grounds upon which I have endeavoured to vindicate my father's claim to the prior knowledge and prior uſe of mineral acid gaſes to deſtroy contagion. Theſe claims reſt upon my father's Diſſertation on Fevers, which, if controverſy had been foreſeen, would doubtleſs have been more perfect in ſome reſpects, and which Dr. Smyth depreciates in many others, in which it is appoſite and important. Upon the paper of my brother James on different kinds of air, which was ſent by my father to Dr. Prieſtley in 1775, and is now in part publiſhed for the firſt time. Upon the medical treatment of my brother Edward's fever in 1776, which Dr. Smyth ſlightly notices, without an attempt to controvert the relation which it bears to our diſpute. Upon the Treatiſe on Malignant Angina, which was publiſhed in 1779, by my brother James, and in which he expreſsly ſtates what had been long the practice of my father.

Upon my father's manuſcripts, in which, after paying a juſt tribute of praiſe to Morveau for his ſubſequent diſcovery of the ſame principle in the ſame agent, and to Dr. J. C. Smyth for his ſucceſsful application of that principle to another agent, he deliberately and unequivocally declares himſelf to be an inventor. Upon my brother Edward's recommendation of my father's practice to the royal infirmary of Edinburgh, in 1779, his mention of it in an Engliſh paper, and his defence of it in a Latin academical exerciſe, during the ſame year, in the ſame univerſity. And, finally, upon the teſtimony of Mr. Crane, which is too pertinent to be ſlighted, too explicit to be evaded, and too deciſive to be refuted.

In conformity to the intention which I intimated to the reader, by a note at p. 2 of this Reply, I ſhall now proceed to examine Dr. Smyth's own claim ſomewhat more minutely.

How

How did Dr. Smyth employ the mineral acids in a ſtate of gas at Wincheſter in 1780? Upon that uſe is founded his earlieſt claim. "In the "year 1780, (ſays Dr. Smyth,) the three mine- "ral acids were employed by me in the priſon "and hoſpital at Wincheſter, and my opinion "of their ſuperior efficacy for deſtroying con- "tagion was communicated by letter to the "Board of Sick and Hurt, and mentioned "publicly by me on many occaſions," p. 25. When Dr. Smyth brings teſtimony of the employment of mineral acids in a ſtate of gas in 1780, before the committee of the Houſe of Commons, the only evidence tending to prove that fact is that of Robert Lulman, Eſq. "He "ſaw the nitrous acid uſed. The veſſels in "which it was contained were placed on the "floor, between the patients' beds, and in "other parts of the wards, and the good ef- "fects were ſoon apparent, as deſcribed in Dr. "Smyth's publication. The nitrous acid was

"uſed,

" uſed, in a fuming ſtate, while the patients " were in their beds, and was kept there night " and day; nor did he ſee or hear any com- " plaint of its producing ſuffocation or diffi- " culty of breathing to the patients or attend- " ants. Dr. Smyth's ſtatement of theſe pro- " ceedings, as given in his work, is perfectly " correct." Report from the committee on Dr. C. Smyth's petition, Ap. No. 2.

" Dr. H. R. Reynolds, phyſician to his " Majeſty, ſtated that he never heard of " the nitrous fumigation till it was ſuggeſt- " ed by Dr. Smyth, previous to his going " to Wincheſter, when they were colleagues " together at the Middleſex hoſpital; and " he never heard of its being put in prac- " tice till his return from Wincheſter," &c. Appendix, No. 25.

This is the whole of the evidence brought before the committee, even for Dr. Smyth's uſe of *fuming nitrous acid* in 1780. Dr. Smyth's letter

letter to the Board of Sick and Hurt, to which he alludes in his Letter to Mr. Wilberforce, is not mentioned in the report. There is no evidence of his having publicly mentioned it, except to Dr. Reynolds, and no uſe of it in hoſpital or private practice is even alluded to; on the contrary Dr. Reynolds, his colleague at the Middleſex hoſpital, ſays, expreſsly, that he never heard of its being put in practice till his (Dr. Smyth's) return from Wincheſter.

His own book, had it been publiſhed in 1780, and the letter he wrote in 1780, would have been ſatisfactory teſtimony; but the letter is not forthcoming, and the book, we may ſuppoſe, received ſome little bias from the corrections and improvements which would probably take place between 1780 and 1795, in which laſt year it was publiſhed. The interval between my father's uſe of muriatic acid gas at Kidderminſter, accompanied by all proper precautions, and the publication of his Hiſtori-

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cal Diſſertation, was two years, from 1756 to 1758. The interval between Dr. Smyth's uſe of *fuming nitrous acid* and the publication of his account of the Wincheſter fever, was about 15 years, from 1780 to 1795.

In his letter Dr. Smyth ſays, "the three mi-"neral acids were employed by me at Win-"cheſter." Now, the queſtion is not, whether the acids in their uſual ſtate were employed, but whether they were employed by Dr. Smyth in a ſtate of GAS. In his work on Wincheſter fever, the uſe of them is to waſh the bed-poſts, &c. with diluted muriatic acid, to burn brimſtone, and to burn nitre in the wards, and to expoſe gallipots three parts full of fuming nitrous acid in them. This is all the uſe of the mineral acids mentioned by Dr. Smyth, as uſed by him at Wincheſter, though he propoſes a better plan before he cloſes his book. Let it be remembered that this book is not publiſhed till the autumn of 1795, a

year

being delivered before the medical ſociety, where for ſome reaſon or other Dr. Edward had no opportunity of producing it; and it is repreſented by him to me, as containing much of the matter which he introduced in his Latin exerciſe.

I ſhall make the following quotations, becauſe they will ſhew the purpoſe for which the paper was drawn up, the ſubjects to which it relates, the time at which it was compoſed, and the methods of cure which Dr. Edward then knew to be employed by his father and recommended by his brother James:

"MR. PRESIDENT,

"As we have lately had an opportunity of "obſerving the progreſs of a very virulent con- "tagious putrid fever, I ſhall beg leave to offer "a few hints on that ſubject to the ſociety." P. 1.

"Acids are excellent remedies as antiſeptics, "whether vegetable or mineral. The vege-

" table have been recommended as much pre-
" ferable to the mineral, from a ſuppoſition
" that the latter only paſs through the inteſti-
" nal canal without entering the ſyſtem. But
" I muſt confeſs that I am very doubtful of
" the fact, and from the tonic power of the
" ſp. vit. I am much inclined to give it the
" preference to any." P. 19.

" The patient ſhould be kept in a large
" room, where the air is free, and in which the
" putrid effluvia ſhould be corrected either by
" the ſteams of vinegar kept boiling in it; or,
" what has been found *to anſwer much better*,
" the acid air of ſpirit of ſalt kept riſing in the
" room by pouring oil of vitriol once or twice
" a day on ſea ſalt placed in a convenient veſ-
" ſel in the room. An objection to this has
" been made, that it is too acrid; but though I
" have *frequently* had an opportunity of ſeeing
" it tried, yet I have never found the patients
" complain, except juſt at firſt, when they were

" not

" not uſed to the ſmell; *and my father has long* " *uſed it* in common practice, and aſſured me " of the ſame thing." P. 21 and 22.

" For many hints on this ſubject, *i. e.* the " uſe of the antiſeptic plan, I would recom- " mend a treatiſe lately publiſhed on the An- " gina maligna, by Dr. Johnſtone of Worceſter, " from whoſe work I have taken many hints, " and whoſe method of cure I frequently had " an opportunity of ſeeing the ſucceſs of." P. ult.

In the 3d quotation from my brother's paper we ſee not only the poſitive but the comparative utility of the muriatic gas, as long and ſafely uſed by my father in his common practice. In p. 21 of the Letter to Mr. Wilberforce, Dr. Smyth makes a diſtinction between my brother James's aſſertion that the acid air, as Prieſtley calls it, or the muriatic acid gas, was ordered by my father, and his not ſaying that he had known him uſe it, or that he him-

ſelf had employed, or ever ſeen it employed by others; and to this frivolous and captious diſtinction he immediately ſubjoins an unqualified declaration that Dr. James "was evi-"dently without experience on the ſubject, "equally ignorant of the powers of the marine "acid, and of the inconvenience which an "imprudent uſe of it might occaſion." If Dr. James learnt nothing more from his father's book, he might, at leaſt, have learned the inconvenience which an imprudent uſe of the marine acid might occaſion. He might alſo have learned the power which, together with other mineral acids and the ſteams of vinegar, it poſſeſſes of medicating the air which the patient breathes. He might have learned a more ſure way of raiſing it than Dr. Smyth, by his own confeſſion, knew for raiſing the nitric acid gas, when he was at Wincheſter. Even with ſuch knowledge we may preſume that he was not wholly without experience: but

but his book ſufficiently refutes the rude imputation both of inexperience and of ignorance. What my brother's practice was, and from what ſources he derived the knowledge which led to it, may be ſeen in his treatiſe: but the paper written by my brother Edward is of uſe, as containing additional teſtimony to the reality and the efficacy of it; and teſtimony I will add, which is not defective in one very material condition preſcribed by Dr. Smyth. Dr. Edward frequently had opportunities of *ſeeing* the ſucceſs of his father's practice.

In p. 26 Dr. Smyth tells us that his treatiſe on the Wincheſter fever was ſent to Birmingham, where Dr. Edward Johnſtone is, or was, phyſician: that public trials were made with the nitrous vapour at that place, by order of Dr. Withering, and that the reſult of them was publiſhed in Dr. Duncan's Annals of Medicine. Dr. Edward now is, and for more than twenty years has been, a phyſician at Birmingham;

mingham; where, without the indulgences of courteſy or the bounties of panegyric from Dr. Smyth, he is likely to retain that eſteem which his profeſſional abilities have long procured for him among his employers, his medical brethren, his neighbours, and his friends.

In regard to the paſſage where Dr. Smyth ſpeaks of the trials made by Dr. Withering, my brother has favoured me, in writing, with the following communication: "I did not "know of Dr. Withering's experiments on the "nitric vapour, nor had I ever heard of his "trying it. I never have tried the nitric va-"pour in my practice, becauſe I had expe-"rienced the good effects of the muriatic acid "gas both in my own caſe, and that of others; "and when I firſt read Dr. Smyth's book it "appeared to me that my father's method of "raiſing the muriatic vapour was far more "ſimple and convenient than Dr. Smyth's "mode of raiſing the nitric acid vapour; in-

"dependent

" dependent of my conceiving that the taſte
" and ſenſations produced by the former were
" far more agreeable and leſs inconvenient than
" the effects of the nitric acid gas."

I am thankful too that I can have the living teſtimony of a man whoſe good ſenſe, profeſſional ſkill, worth and reſpectability, well entitle him to public confidence. The following letter I received from Mr. Crane, ſurgeon in Kidderminſter, on preſenting to him Dr. C. Smyth's Letter to Mr. Wilberforce:

Kidderminſter, April 2, 1805.

Dear Sir,

I thank you for a ſight of Dr. Carmichael Smyth's letter, and ſo far as it relates to me, truly I do rejoice that I am enabled to bear further teſtimony to the diſcovery of your father, my old and confidential friend. I ſettled in this town in the year 1775: at that time I well remember that Dr. Johnſtone conſtantly uſed (by pouring oil of vitriol on common ſalt) the muriatic

muriatic vapour in all caſes of putrid fever; and I was informed by Mr. Cooper and Mr. Symonds, who had known Dr. Johnſtone's earlier practice, that he had uſed it ſoon after he ſettled in this town, and that it always continued to be his practice. It was indeed ſo well known to be uſeful in deſtroying putrefaction, that thirty years ago, the manufacturers were accuſtomed to place it in their ſhops, of their own accord, as I believe I ſtated to you before.

From my firſt profeſſional acquaintance with your father I have never known any ſerious putrid diſorder in which he did not uſe the muriatic fumigation, and in which we had not been taught by him to rely on it as the chief corrector of putrefaction: indeed from my firſt knowledge of Dr. Johnſtone to his laſt month, and I was employed almoſt conſtantly with him from the year 1775 to four weeks previouſly to his death in April 1802, his practice in fevers continued ſo invariably the ſame

that

year after Guyton de Morveau had publiſhed his "*Inſtructions ſur les Moyens, &c.*" when the muriatic acid gas was ordered to be uſed in the hoſpitals of the French armies. From 1780 to 1794 the reſult of Dr. J. C. Smyth's reflections, ſo important to mankind, ſo important to his own honour as well as his emolument, lies aſleep; nor is it awakened to life till Dr. Morris, in 1794, applies to him for advice. Then he ſtarts up, and his recollection being probably refreſhed by Morveau's wide and notorious uſe of the muriatic acid gas, he beſtirs himſelf, either to diſcover ſome means upon the ſame principle but not the ſame agent, or to ruminate upon his old *intention* to diſcover ſome ſuch agent. With this view he fixes upon the nitric acid gas; perhaps having diſcovered at Wincheſter that fuming nitrous acid does not furniſh a reſpirable gas, and knowing that in the uſe of the muriatic he had been anticipated by Morveau, and poſſibly by

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others. The nitric acid gas he did *not* uſe, and according to his own account, he *could* not have uſed at Wincheſter in 1780, for gallipots of fuming nitrous acid would give out ſuffocating *yellow or red fumes only*. Nay, Dr. Smyth confeſſes himſelf miſtaken in the principle on which he burnt the nitre, and as to the fumigations of brimſtone, as well as the deflagration of nitre and gunpowder, they were in common uſe long before, as drivers out of contagion, *not* chemical correctors of it. There is no evidence, then, from his book, or from the teſtimonies of Mr. Lulman and Dr. Reynolds, that Dr. Smyth was acquainted with the principle in 1780, ſince, if we may believe his own ſtatement, he did not employ *either* of the reſpirable mineral acid gaſes. He employed fuming nitrous acid I admit, and he employed it from a previous experience, "that the fumes "of nitrous acid did not affect the breathing "in the ſame manner (as what?) as the fumes

"of

" of ſulphur, which are ſo ſucceſsfully made " uſe of to fumigate clothes or furniture," p. 55.

Now, in regard to Dr. Smyth's comparative feeling of inconvenience from the fumes of ſulphur, or of fuming nitrous acid, it is entirely perſonal, and if he can at all bear fuming nitrous acid, I may ſay peculiar. To my perception fuming nitrous acid is nearly as intolerable and ſuffocating as burning ſulphur, *(cæteris paribus)* and I cannot but conceive, whatſoever may be the peculiarity of Dr. Smyth's own ſenſe of its impreſſion, that his patients " expoſed to the nitrous fumigation in hoſpital " wards, and in the apartments of the ſick, " without perceiving any unpleaſant effect " from it," muſt have been expoſed to it at a very conſiderable diſtance, and in ſuch a diluted ſtate, as for it to be nearly imperceptible, and therefore, in all probability, not very efficacious. From all that appears in his book, Dr.

Smyth made his firſt experiments with fuming nitrous acid on his patients; but when he determines "to render the experiment ſtill more "concluſive," after expoſing a mouſe and a greenfinch, he then expoſes himſelf, and his friend Mr. Hume, (ourſelves) to the fumes of nitric acid, "obtained by mixing nitre with "heated vitriolic acid." Here is the beginning of the true method, but we are not informed when it was employed; the conſummation in medical practice, certainly took place not till November 1795 on board the Union hoſpital ſhip. It will naturally occur to the reader, did Dr. Smyth know of Morveau's experiments at this time? He confeſſes he did, and the progreſs of his mind was from doubt to conviction, from very imperfect to perfect knowledge, from intention which he firſt formed, then diſtruſted, and then laid aſide, to direct and plenary execution.

In his memorial to Lord North, in which

Dr.

Dr. Smyth enumerates his ſervices, as furniſhing a claim to remuneration, he ſays not a word of his uſe of mineral acids, or of his diſcovery, or of any principle for deſtroying contagion, even though he details his plan of cure at ſome length. Had he thought himſelf a diſcoverer in 1780, as he profeſſes to have thought in 1802, would he not have mentioned it immediately after? Is it poſſible that he could forget ſuch a plea, whilſt his mind was intent on reward, and in the act of accumulating all his merits and all his ſufferings, for the purpoſe of preſenting them to the eye of his patrons and the public? If Dr. Smyth did verily believe himſelf to be the diſcoverer of the power of mineral acid gaſes to deſtroy contagion, he is the firſt ſupplicant for reward I ever heard of, who forgets in his petition, his *moſt* meritorious ſervice; the firſt memorialiſt who neglects, in his memorial, his *greateſt* act of benefit; a benefit not confined

to

to the particular office in which he had been employed, but extending to all mankind. For I appeal to Dr. Smyth's memorial itſelf, which is printed in his volume on the Wincheſter Fever, and which does not hint at the diſcovery of any chemical power to deſtroy contagion, but deſcribes the uſe and well-known effects of *purification* only.

In p. 24 of his Letter to Mr. Wilberforce, Dr. Smyth tells us "this ſuppoſed diſcovery (*i. e.* my father's) "remained* a dead letter

* That Doctor Smyth's critical powers may not be rouſed into action by this various reading, I muſt acknowledge that the word printed in the Doctor's book is "remaining," but the ſentence would not then be grammatical; therefore *meo periculo*, I have reſtored the true and original reading, on the preſumption that the blunder was not made by my author but his printer. After this explanation, I hope that my "talents for quotation" will not be again arraigned.

As Dr. Smyth has taken upon him to ſchool me upon my uſe of the Engliſh language, I ſhall beg leave to pay the ὑρεπλήρια due to my inſtructor, by recommending to his conſideration a few paſſages which are not ſtrictly conformable to general grammar,

" for fifty years, and Dr. John Johnſtone,
" who now comes forward as the claimant for
" his father, and champion of his family, was
" for twenty-three years, at leaſt, ſilent on

grammar, or to the idiom of what Engliſhmen call their vernacular tongue:

" I embrace the firſt moment," p. 1—we ſay, embrace opportunities, not moments. " You was chairman," p. 1—ſince the publication of Lowth's grammar, men, women, and children, have been taught to write " you were," p. 10. " This " caution flatly contradicting the ſon's aſſertion." If Dr. Smyth recollects that full ſtops are placed at his own word paragraph, and at the quotation ending at the word lungs, he will find that contradicting ought to be contradicts. P. 13 " ſet it at reſt for ever"—if the word " it" refers to pretended diſcovery, Dr. Smyth would have written more correctly, by ſaying, " ſet the queſtion about it at reſt." P. 31, " the preceding au- " thorities *is* the only anſwer." I cannot gueſs for what particular purpoſe of rhetorical emphaſis, the monoſyllable *is*, has been decorated with italics. But I believe the generality of men of letters would have written, are. After all, theſe petty verbal imperfections have little to do with the merits of our controverſy, and I have pointed them out with the deſign not to annoy Dr. Smyth, but to check the eagerneſs he diſplays, to inſult me.

" the

" the ſubject." Now, during theſe fifty years, my father publiſhed one book, my brother James publiſhed another, my brother Edward propoſed the practice in the univerſity of Edinburgh, and defended it in one of the public diſputations for his degree. All three of them ordered it in their practice. My father had given it ſo much notoriety in the neighbourhood of Kidderminſter, that the poor people were able to apply it for their own protection; and in order to beſtow upon it yet more uſeful and more extenſive notoriety, he had communicated it to Dr. Prieſtley, with the hope that it would be preſented to the Royal Society, and printed in their tranſactions, which are circulated throughout Europe. The diſcovery of the muriatic acid gas, therefore, was *not* " a " dead letter," unleſs the abſence of an application to Parliament for reward be the ſpecific property, which in Dr. Smyth's apprehenſion, ſubjects any diſcovery to that name, theoretical

or

or practical, medical or chemical. My ſilence, it ſeems, continued only twenty-three years, in which the *terminus a quo*, is Dr. Smyth's journey to Wincheſter in 1780, and the *terminus ad quem*, is the publication of my pamphlet in 1803. With all due ſubmiſſion to Dr. Smyth's accuracy in arithmetic, I was not quite ſilent for quite twenty-three years. According to Dr. Smyth's own account, I had ſent a paragraph to the Morning Chronicle of March 12, 1802, and finding (as the Doctor *conceives*) that this paragraph did not attract any notice from phyſicians or from Parliament, "I came for-" ward *propria perſona*, by a letter addreſſed to "Mr. Wilberforce, as chairman of the com-" mittee, and I employed the interpoſition "of my friend, Sir William Pulteney, who "brought for (the committee's) inſpection "the father and brother's publications," alias *dead letter*. To be ſure I was thus far guilty of ſilence, as not to proclaim, by any publication of my own, the diſcovery which, in my

judgment, had been proclaimed before by my father and my brother James to that public; and I broke my ſilence to Parliament, becauſe Parliament had been recently appealed to by Dr. Smyth, and the object of my appeal, was to ſecure my father's claim to the prior diſcovery of a principle, which then engaged the attention of Parliament.

Let me now, in my turn, be permitted to aſk, why Dr. Smyth was ſilent for a very long time about his diſcovery? For about the diſcovery he certainly was ſilent in his memorial to Lord North; he was ſilent upon the ſame ſubject, though communicative enough upon another, and very diſtinct ſubject,* in his converſations with Dr. Reynolds; he was ſilent to the public till 1795; he was ſilent to Parliament till 1802; and yet from 1795 to 1802 he moſt aſſuredly was, from his own perſonal

* I muſt again and again entreat the reader to obſerve the diſtinction between the uſe of fuming nitrous acid and of nitric acid gas.

experience,

experience, acquainted with the principle and the uſe of the reſpirable mineral acid gaſes. I ſuppoſe that he broke his ſilence to Parliament when he found a favourable opportunity for breaking it in ſupport of his own claim; and I did the ſame when a proper, and indeed the firſt, occaſion aroſe for my endeavours to preſerve my father's claim to priority from encroachment. I did not mean to interfere with Dr. Smyth's great and merited reward for the application of the nitric acid gas, but I did mean to give Parliament an opportunity of beſtowing that reward in ſuch a manner, as ſhould do no injuſtice to my father's claim to the prior diſcovery and application of the principle, in muriatic acid gas.

In what manner, and to what extent, were the deliberate reflections of Dr. Smyth employed from the time of his returning from Wincheſter to the time of his making experiments on birds and men, jointly with Mr. Hume of Long-Acre? The Doctor was during this

time, a practiſing phyſician in the Capital. He was alſo phyſician to an hoſpital. Let me aſk, then, whether among his patients, there did not occur ſome putrid diſeaſes in this interval? Let me aſk, whether he repeated among them, the experiments which he made at Wincheſter with fuming nitrous acid, and what was the reſult of thoſe experiments? That they prevented the ſpreading or further communication of contagion*, for that effect is expreſsly aſcribed in p. 193 to the vapour ariſing from the yellow or ſmoking nitrous acid, as applied by Dr. Smyth in hoſpitals and private practice for ſixteen or ſeventeen years before 1795. Whether the facts be true or not, I muſt beg leave to remark, that, according to his own ſenſe of the phraſe, this uſe of nitrous vapour remained *a dead letter*, for thoſe ſixteen or ſeventeen years.

In the paſſage now under conſideration, there

* The fuming nitrous acid abſorbs the reſpirable portion of the air, when perſons are preſent: it muſt be therefore impracticable to apply it, in an efficacious quantity to deſtroy contagion.

ſeems

ſeems to be a remarkable want of preciſion and clearneſs in Dr. S.'s ſtatement. His words are theſe: "I formerly mentioned, that "I had employed the nitrous acid in two "different forms; either the vapour ariſing "from the yellow or ſmoaking nitrous acid, "*which is a mixture of the acid with nitrous gas*, "or the more pure nitrous acid detached from "nitre, decompoſed by the vitriolic acid. In "one or other of thoſe forms I have uſed it, "both in hoſpitals and in private practice, for "ſixteen or ſeventeen years paſt; and have "had the ſatisfaction to obtain the moſt de-"ciſive evidence of its happy effect, in pre-"venting the ſpreading or further communi-"cation of contagion." Smyth on Gaol Diſtemper, &c. p. 193.

If Dr. Smyth had ſaid, I had uſed it in both theſe forms, there would have been no room for the imputation of ambiguity, but when he ſays, I have uſed it "in one or the other," I may, without impropriety, aſk, in which of the two?

In

In what year did he begin to uſe the nitric acid gas? If he had uſed it in hoſpitals and private practice for ſixteen or ſeventeen years, how ſhall we account for his not uſing it at Wincheſter? For ſurely, if he had uſed it there, he would have told us ſo, and he had an opportunity of telling us ſo pertinently at p. 194, when he was talking both of hoſpitals and his private practice.

Again, if his experiments of the uſe of nitric acid gas among his patients had been very frequent, and had continued for a courſe of years, where was the neceſſity for transferring his experiments from his own ſpecies to a bird and a mouſe? Surely the reſpirability of nitric acid gas would be more clearly aſcertained by the feelings of his patients, than of his mouſe or his greenfinch. I obſerve that no date is aſſigned to the experiments made with Mr. Hume, and I am aware that many *convenient purpoſes* might be anſwered by the omiſſion of that date. It muſt however be preſumed,

that

that the mouſe and the greenfinch were not expoſed to experiments, till Dr. Smyth had *met* with opportunities of trying nitric acid gas upon his patients. My queſtion is, did he avail himſelf of thoſe opportunities? Did he then know enough of the nitric acid gas to try it upon his patients? Did the reſult of the trial furniſh deciſive evidence of its happy effect in preventing the ſpreading or further communication of contagion? If both analogy and experience had proved this power of the nitric acid gas, before the experiments made with Mr. Hume, there ſeems to have been little occaſion for calling in the aid of two poor little animals.

I am endeavouring to trace Dr. Smyth's intentions and proceedings by the guidance of his printed words. He does not ſay that he and Mr. Hume made their experiments with any view to aſcertain the reſpirability of nitric acid gas. No, but he does ſay, that he made theſe experiments to aſcertain with more preciſion, what were the effects of nitrous acid in fumigation;

fumigation; that very acid which he had frequently uſed in hoſpital wards, and in the private apartments of the ſick. True it is, the ſuffocating properties of the fuming nitrous acid may juſtify the caution of Dr. Smyth; but, if he had uſed the nitric acid gas as my father uſed the muriatic acid gas, a very little experience, joined with a very little ſagacity, would have pointed out the ſafe uſe of it.

During the progreſs of theſe experiments the nitric acid gas was perhaps diſcovered, or at leaſt it was remembered. For now we find it clearly and intelligibly ſtated, firſt that the mouſe, and then the greenfinch, and then that Mr. Hume and Dr. Smyth themſelves were expoſed to it. The poor bird was alſo expoſed to the marine acid, and though now and then ſomewhat uneaſy, upon the removal of the jar, it hopped about as lively as before. I wiſh Dr. Smyth would explain perſpicuouſly and ingenuouſly, what ſuggeſted to his mind trials either of the nitric or muriatic acid gas, during

a proceſs

a proceſs which is profeſſedly inſtituted for the purpoſe of ſome experiments on the *fuming* nitrous acid. How did the powers of the muriatic or the nitric fall into the train of Dr. Smyth's deliberate reflections? The principle of the muriatic had been publiſhed by my father in 1758, and by my brother in 1779, and by Morveau in 1773 and 1780. That principle, if known from either of theſe publications, might ſuggeſt to any inquiſitive or intelligent man experiments on the nitric, and the idea of this transfer would have occurred the more readily if Dr. Smyth had chanced to read Dr. Prieſtley's Obſervations on Air, publiſhed 1774. "It will be, perhaps, thought "that the moſt uſeful if not the moſt remark-"able of all the properties of this extraordi-"nary kind of air (nitrous air) is its power "of preſerving animal ſubſtances from putre-"faction, and of reſtoring thoſe that are al-"ready putrid, which it poſſeſſes in a far "greater degree than fixed air. My firſt ob-

" ſervation of this, was altogether caſual," &c. Prieſtley's Experiments and Obſervations on different Kinds of Air, Vol. I. p. 123.

If Dr. Smyth had been leſs thrifty in his expenditure of dates, we might have been able to aſcertain with more preciſion, the time at which, and the cauſes by which, his attention was turned either to the muriatic or the nitric acid gas, and his choice determined to the nitric in preference to the muriatic. Perhaps, however, ſome rays of light may be thrown on the ſubject by the manner in which Dr. Smyth announces certain other diſcoveries of his, and by contraſting his expreſſions with certain expreſſions of my father. They are contained in the two books ſo often quoted in the foregoing pages, viz. :—Dr. Johnſtone's Diſſertation concerning the Malignant Epidemical Fever of 1756, printed in 1758; and Dr. Smyth's Deſcription of the Jail Diſtemper as it appeared among the Spaniſh Priſoners at Wincheſter in 1780, printed in 1795.

Dr. Johnstone's *Dissertation concerning the Malignant Epidemical Fever of* 1756, *printed* 1758.

"Pursuing these views after the emetic had finished its operation, I always ordered the patient to use some antiseptic sedative: spiritus nitri dulcis taken to the quantity of a teaspoonful in mint tea or mint water every three or four hours, will answer this purpose better than any of the neutral saline and emetic mixtures which are generally ordered, and which I also tried, though with less success than followed from the dulcified acids. All those liquid sulphurs, for such in fact are the dulcified spirits of nitre, vitriol, and sea salt, and the anodyne mineral liquor of Hoffman, are certainly excellent remedies in malignant fevers of this class: they allay that

Dr. Smyth's *Description of the Jail Distemper as it appeared among the Spanish Prisoners at Winchester in* 1780, *printed* 1795.

"This, however, is greatly obviated by the spiritus ætheris vitriolici, the other medicine which, from a long experience of its efficacy, *I* have ventured to recommend in the cure of contagious fevers. This medicine has an advantage over most cordials, as it does not increase the heat of the body or the quickness of the pulse, but on the contrary renders the action of the heart more regular and slow." P. 113.

"This medicine, though long kept in the shops, has very seldom, I believe, been employed in practice; a preparation extremely analogous to it, well known by the pompous title of *liquor*

that vomiting, which is for the moſt part increaſed by all other medicines, and correct putrefaction both in the ſtomach and duodenum, and alſo counteract the influence of the malignant virus in the vaſcular ſyſtem, by gently incraſſating and preſerving the texture of the blood globules. Sweet ſpirit of nitre in particular promotes very effectually the ſecretion of urine, and that of perſpiration. In theſe reſpects they are greatly preferable to the ſingle uncompounded mineral acid ſpirits. In a word theſe dulcified acids poſſeſs the excellences of the moſt celebrated alexipharmics, without their pernicious effects and qualities, and that in a very compendious and agreeable form." P. 41, 42.

" In the beginning of the fever the ſpirits of nitre frequently taken by tea-ſpoonfuls in mint tea or the patient's common drinks, *liquor anodynus mineralis Hoffmanni* being commonly preferred as an antiſpaſmodic; but neither the one nor the other (ſo far as I have been able to learn) had, in this country, ever been uſed in the cure of fevers, previous to the year 1769, unleſs the dulcified ſpirit of vitriol may be ſaid to have been ſo, as forming part of the compoſition of a quack medicine ſold in Town by the name of Clutton's Febrifuge Tincture. It was in the ſummer of the year 1768 that I began to make trial of the dulcified ſpirit of vitriol in the cure of fevers. *From reflecting* on the ſenſible qualities and compoſition of this medicine I was led to imagine that I ſhould find it uſeful as a cordial and antiſeptic in caſes of the putrid and malignant kind. A little experience convinced me that my conjecture was well founded." Appendix, p. 139.

" The

drinks, will anſwer every purpoſe, and may alone be depended upon." P. 43.

"The neceſſity of changing the air in a ſick room, by ſucceſſive ventilation, ariſes from the conſtant deſtruction of a certain property in that fluid by breathing, which renders it afterwards uſeleſs; likewiſe from the atmoſphere being filled with the excrementitious ſteams which fly off from the patient's body continually, and which putrify in a ſtagnant unrenewed air, and render it truly poiſonous, a pabulum morbi, rather than of life. The phyſician will order the room to be kept ſweet and clean, and ſtools and every thing offenſive to be removed as ſoon as poſſible. If the external air is immoderately cold and wet, the room muſt be kept warm and dry, and the fumes of amber, myrrh, benzoin, and camphire, may

"The only merit I can claim, if it can be reckoned ſuch, is the having been the firſt to point out the efficacy of this medicine in contagious fevers, and I am ſtill the only one who has attended to its effect in reducing the frequency of the pulſe." P. 166, 167.

"My attention having been called to this ſubject, and my character involved in the ſucceſs, I was ſatisfied, after the moſt deliberate reflection on the nature of putrid contagion, that nothing could ſo certainly or efficaciouſly deſtroy it as the mineral acids in a ſtate of vapour, but how to employ theſe with ſafety was the difficulty, *hoc opus, hic labor*. The fumes of ſulphur which are ſo ſucceſsfully made uſe of to fumigate clothes or furniture could not be employed either in hoſpital or priſon wards, as theſe fumes are well known

may be diffuſed in the room, if ſprinkled upon hot iron. Vinegar may be ſprinkled about cold, if the weather is warm, and boiled with myrrh or camphire, an antiſeptic ſteam will ariſe into the air, which the patient breathes greatly to his advantage. Theſe ſteams will preſerve the air free from putrefaction, and will inſinuate themſelves by the abſorbent veſſels of the lungs into the blood-veſſels, and will greatly aſſiſt in impeding the progreſs of putrefaction in the fluids. Theſe are the moſt commodious, if not the moſt uſeful, methods of medicating the air the patient breathes; however, thoſe who prefer the mineral acids may order brimſtone to be burnt, or may raiſe the marine acid very eaſily, by putting a certain quantity of common ſalt into a veſſel kept heated, upon a chafing-diſh of coals; if to this a ſmall quantity known in ſmall quantity to prove immediately deſtructive to animal life. *I had frequently remarked* that the fumes of the nitrous acid did not affect the breathing in the ſame manner, notwithſtanding which I conceived they might prove of equal efficacy in deſtroying or diminiſhing the virulence of contagion; and *the ſucceſs attending the employment of theſe at Wincheſter*, as well as many trials which I have made both in private and in the Middleſex hoſpital, have convinced me of the power of the nitrous acid in deſtroying contagion, and likewiſe of the ſafety with which it may be uſed." P. 54, 55.

"After removing all the hammocks, bedding, &c. from the wards, they are firſt thoroughly cleaned out, then the hammock poſts were well waſhed with diluted marine acid, and the ſame thrown,

quantity of oil of vitriol is, from time to time, added, the air will be filled with a thick white acid ſteam; but both the marine and ſulphureous acids muſt be diſengaged at a conſiderable diſtance from the patient, otherwiſe their extreme pungency will be offenſive to the lungs." P. 50, 51. So writes my father in 1758 upon his own practice in 1756, an interval of two years.

thrown, by means of garden watering machines, to the upper parts of the poſts, as high as the ceiling. The wards when dry were cloſely ſhut up, and pots placed in them at different diſtances, containing from half a pound to a pound of nitre, which was deflagrated by an iron heater put into each pot. The wards were then ſhut up for ſome hours, and when opened were expoſed to a free ventilation. After this proceſs had been once or twice repeated the wards were again furniſhed with freſh hammocks, palliaſſes, and bedding, inſtead of the old bedding, &c. which was entirely taken away." P. 58, 59.

"The floors of the wards (after being ſwept) muſt be watered by means of a gardener's watering-pot, with diluted marine acid. The wards are then to be fumigated

fumigated with cenſers of burning ſaltpetre, and two or three gallipots about two-thirds filled with fuming ſpirit of nitre are to be placed in the middle of each ward, to remain conſtantly night and day, and to be renewed every morning. The beds themſelves are to be ſprinkled with vinegar."

There are other reſemblances of opinion which it is not neceſſary to adduce. For inſtance: Dr. Johnſtone theoriſes on putridity, as the cauſe of malignant fever. Dr. Smyth endeavours to prove that the contagion of jail-fever is "a vapour produced by putrefaction." But is it not ſingular that Dr. Smyth ſhould conceive himſelf a diſcoverer exactly in thoſe points in which Dr. Johnſtone has ſo obviouſly anticipated him? Is it not very ſingular that the only two peculiarities of Dr. Smyth's practice ſhould, in their principle, be the peculia-

rities

rities of my father's? It is clear that my father recommended the dulcified acids in 1758, and that he uſed them in 1756, and yet ſuch is Dr. Smyth's love of diſcovery, ſuch his ardour for originality, that he claims this practice as peculiarly his own. It is ſingular too that both Dr. Smyth's diſcoveries are uſhered in with the ſame phraſeology, "From reflecting "on the ſenſible qualities and compoſition of "this medicine *(ſpiritus ætheris vitriolici)* I "was led to imagine that I ſhould find it uſe-"ful as a cordial and antiſeptic in caſes of the "putrid and malignant kind."

When Dr. Smith diſcovers the powers of mineral acid vapours, *this* too is the work of deliberate reflection. "I was ſatisfied after "the moſt deliberate reflection on the nature "of putrid contagion that nothing could ſo "certainly or efficaciouſly deſtroy it as the mi-"neral acids in a ſtate of vapour." How unfortunate is it that ſo much deep reflection

ſhould have been waſted in diſcovering in 1780, that which had been promulgated in 1758, in 1773, and in 1779! For at the firſt of theſe periods my father, without any parade of novelty, any pomp of diction, any profeſſed felicity of conjecture, any boaſts of deep and deliberate reflection, had declared the uſe of the dulcified acids as medicines beſt ſuited to counteract the conſequences of contagion when operating upon the living body, and that the mineral acids in a ſtate of vapour were uſeful means of counteracting putrefaction and contagion out of the body. I muſt again lament this waſte of genius; for had the ſame powers of reflection been employed on ſome other ſubject there is a poſſibility that one or other of thoſe diſeaſes which now embitter the comforts of life and ſhorten its duration, would have been expunged from the mournful catalogue of human woes.—But to proceed: After reading with ſurpriſe and ſome diſtruſt, the

imaginations,

imaginations, conjectures, repeated trials, and deliberate reflections of Dr. Smyth, I was ſuddenly and agreeably refreſhed by the new diſplay of his talents for confeſſion. "It will "probably be aſked why I did not make a "complete trial of it (nitrous acid) there (at "Wincheſter). To this I anſwer, that with re-"ſpect to fumigating infected clothes, bed-"ding, &c. I did not think myſelf warranted, "eſpecially on an occaſion of ſo much im-"portance, to make trial *of an uncertain re-"medy when a certain one was in my power.* "As to fumigating the priſon and hoſpital "wards, it evidently was *my intention* to have "employed the nitrous acid, but I was miſ-"taken in the means I took to procure it, and "*have not ſince had a proper opportunity* of re-"peating the experiment." P. 195.

The reader will probably recollect that Dr. Smyth's book does mention that gallipots of fuming nitrous acid were employed in 1780

at Wincheſter. He will alſo find that Dr. Smyth's bad health, and the peace which followed in 1783, are aſſigned by him as reaſons for laying aſide the publication of what he had done and obſerved at Wincheſter till he was led to reviſe his notes, in conſequence of an application from Dr. Morris, in what he calls "laſt ſummer," by which I underſtand 1794, as he dates his dedication 14th Auguſt, 1795. During this interval the Doctor, I ſuppoſe, ſhared with other men of ſcience the great improvements which had been made in chemiſtry, and which would probably inſtruct him that the combuſtion of common nitre (nitrate of potaſh) did not exactly furniſh nitric acid gas, or air purer than that of the atmoſphere. Guided then by the diſcoveries of chemiſts in their own ſcience, and knowing, as he muſt, the inefficacy of ſmall and the dangerous nature of great quantities of *fuming* nitrous acid, he would probably learn that he had

had *not* employed "the nitrous acid in two different forms,"* that he was miſtaken in ſuppoſing that deflagrated nitre gave out nitric acid gas, and that had the patients at Wincheſter inſpired his *fuming* nitrous acid ſome of the effects of ſuffocation would have enſued. I am ſtill perplexed to aſcertain the time at which either the application or the diſcovery of the nitric acid gas is to be fixed, and the embarraſſment which I feel from the ambiguity of Dr. Smyth's language, is only partially relieved by his confeſſion, that he did not make a complete trial of the nitrous acid at Wincheſter. Yet he lays claim to the merit of *intending* to have employed the nitrous acid, but he confeſſes that he was miſtaken in the means he took to procure it, and adds that he has not ſince had a proper opportunity of repeating the experiment.

This error, and this failure in procuring the

* Smyth, p. 193.

nitrous

nitrous acid, conſtitutes a complete proof that he did not uſe the nitric acid gas at Wincheſter, that his deliberate reflections on the power of mineral acids had not been very frequently accompanied by previous trials upon his patients, and that the reſult of thoſe trials had produced ſuſpicion without conviction, deliberation without deciſion, and intention without action, whilſt he was performing the high functions of his miſſion to Wincheſter.

Thus then, at p. 55 of his book, Dr. Smyth talks of his ſucceſsful uſe of nitrous acid at Wincheſter.* At p. 25 of his Letter he ſays,

* My readers muſt have obſerved that in my quotations from Dr. Smyth's Letter I ſometimes have laid before them his meaning embodied in complete paragraphs, and then have broken them up into parts, where "*disjecti membra ſophiſtæ*" ſeem to me endowed with thoſe properties of artifice and annoyance, which characterize both the ſtatement and the reaſoning of my opponent. The neceſſity for doing ſo was impoſed upon me partly by the intenſe watchfulneſs and the fierce indignation of Dr. Smyth on the ſubject of quotation, and partly by the peculiar talent which he poſſeſſes for ſlurring over that which I wiſh

to

" the three mineral acids were employed by " him at Wincheſter, and his opinion of their " ſuperior efficacy for deſtroying contagion " was communicated by letter to the Board of " Sick and Hurt, and mentioned publicly by " me on many occaſions." At p. 195 of his book he declares, " that he was miſtaken in " the means of procuring it, but that he *diſco-* " *vered an intention* to uſe it." What inference muſt we not draw from all this palpable contradiction but that he did not uſe the reſpirable mineral acid gaſes as antiloimics at Wincheſter, that he did not uſe them till he wrote his book, not having had proper opportunities of repeating the experiment? What experiment?

to be diſtinctly examined, and for confounding what I have often been compelled to ſeparate. This apology will, I hope, juſtify the occaſional repetition of the ſame paſſages from Dr. Smyth's book, my father's, my brother's, and my own. *Accedere oportet ad ſingula, ſic univerſa frangentur.*

Cic. *Oratoriæ Partitiones.*

Why,

Why, that very experiment which he has the inſtant before confeſſed he had failed in! When he does make experiments, he expoſes a mouſe and a greenfinch to the mineral acid vapours, to aſcertain whether they are reſpirable, forgetting his former declaration, that in his private practice, and at the Middleſex hoſpital, he had frequently remarked that nitrous vapour did not much affect reſpiration. Such was the reſult of the *hoc opus, hic labor*, of Dr. Smyth; and truly upon few occaſions could the adage be better applied, *Parturiunt montes, naſcetur ridiculus mus.*

Before I finiſh this topic let me remark further, that although Dr. Johnſtone and Dr. Smyth uſe the ſame principle of practice in the two inſtances before adduced, yet the agents are different. In every thing Dr. Smyth is reſolved to be original. My father recommends all the dulcified acids upon a like principle, but adheres chiefly to the *ſpiritus ætheris nitroſi.*

nitroſi. Dr. Smyth recommends excluſively the *ſpiritus ætheris vitriolici,* not forgetting, however, "a preparation extremely analogous "to it, well known by the pompous title of "*liquor anodynus mineralis Hoffmanni.*" When Dr. Smyth, after deliberate reflection, uſes mineral acids in a ſtate of vapour, he does not ſelect that acid which has been tried before the muriatic, but his mind undergoes all the agonies of intellectual parturition to bring forward the nitric.* *Hoc opus, hic labor.* His firſt effort does not ſucceed: in this throe he only brings forth an INTENTION. In his ſecond, fourteen years afterwards, he is more fortunate, and all his paſt pangs may be ſuppoſed to be

* If the reader be a partiſan of the Darwinian Theory of Generation, it will perhaps ſtrike him as poſſible, that whilſt in the heat and agitation of that deliberate reflection which produced the fuming nitrous acid, Dr. Smyth might have had his mind's eye intently fixed on certain charms and qualities of muriatic acid gas.

forgotten in the education of a promiſing brat, who, in leſs than fourteen years more, is to be richly endowed. The muriatic he treats as a foundling, whom accident had thrown in his way, and though in ſome of its features it bore a pretty ſtrong reſemblance to his own favourite offspring, yet after a little inſpection, he rejects it, as an unneceſſary incumbrance to himſelf, and leaves to be picked up by its own parents, or by ſome generous protector, who might have more leiſure or more inclination to nouriſh and bring to light any good qualities with which it might be endowed.

In reſpect then to the muriatic acid gas, to the fuming nitrous acid, and to the dulcified acids, I make the following obſervations. The uſe of the one by my father was antecedent to the uſe of the other by Dr. Smyth. The publication of that practice by my father was previous to Dr. Smyth's practice. The reſemblance in the principles, in both caſes, is complete.

plete. The variation of the agents, as announced by Dr. Smyth, is very remarkable.

Now this double coincidence of identity in the principle, and diverſity in the inſtruments, though in the caſe of Dr. Smyth it might be the reſult of mere accident, ſtill muſt be allowed to carry with it the ordinary appearances of deep contrivance and dextrous execution—of a mind conſcious that ſomething had been previouſly diſcovered, and that ſomething elſe was to be added or altered to ſupport pretenſions to a firſt and pure diſcovery, which, in reality, was only a ſecond and qualified one. I do not, however, intend to preſs any invidious conſiderations of this kind. I am aware that the *juſt* claims of Dr. Smyth are compatible with thoſe of my father in every circumſtance except that of *priority*; and therefore I virtually maintained in my ſtatement, as I have repeatedly affirmed in this Reply, that Dr. Smyth is not the ſole, nor even the ori-

ginal diſcoverer, employer, and promulgator of the principle, for the diſcovery, employment, and promulgation of which he was ſome time ago a claimant to Parliament, and has ſince ſtood forth a competitor with my father.

I wrote my pamphlet after deliberate reflection. Dr. Smyth, doubtleſs, prepared his book on jail fever, and his Letter to Mr. Wilberforce, after deliberate reflection. For the ſake of preciſion in my ſtatements, clearneſs in my reaſoning, and juſtice to my father's cauſe, I have again and again endeavoured to ſeparate the points really in diſpute between Dr. Smyth and myſelf, from collateral conſiderations, and I reſpectfully entruſt the deciſion, to the deliberate reflections of impartial and intelligent readers.

In my pamphlet, "On the Diſcovery of the "Power of Mineral Acids in a State of Va-"pour to deſtroy Contagion," I have related

my

my father's method of raiſing muriatic acid gas, and Dr. Smyth's method of raiſing nitric acid gas. I have alſo related ſome ſtriking inſtances of the power of muriatic acid gas in deſtroying contagion, and ſome experiments on the relative convenience of application of the muriatic and nitric acid gaſes, by ſome learned friends. "On their obſervations (the obſervations of "my friends, ſays Dr. Smyth,) I ſhall only re-"mark that they come too late and prove too "much."* YES: they would have come too late, for a man who is intent only on emolument, and they do prove too much, for a man who would darken the truth.

On the aſſumed ſuperiority of the nitric acid gas over the muriatic, in regard to the eaſe and ſafety with which it may be breathed, I ſhall now ſay a few words. The committee of the Houſe of Commons, to whom Dr. J. C. Smyth's petition was referred, "pointed out

* Letter, p. 28.

"the

" the fundamental and important diſtinction
" between the nitrous and other two mineral
" acids, (to be) that the former can alone be
" reſpired without injury, and therefore can
" alone be uſed, except in places, from which
" the patients and all other perſons have been
" removed,"* and upon this diſtinction proclaimed Dr. Smyth the diſcoverer of the principle, in its only practicable ſhape, and recommended him therefore to be rewarded.

Now it is remarkable that Mr. Davy, who gave evidence before the committee, told them, " he believes the fumes of the marine acid " equally diluted are reſpirable with equal " ſafety." To Mr. Davy's chemical attainments I do pay unfeigned homage, but he will pardon me for imagining, that the difference of ſenſation which he mentioned afterwards, aroſe from the muriatic acid gas being breathed by him, in a leſs diluted ſtate than the nitric.

* Report, p. 7.

Dr.

Dr. Andrew Duncan, in the Annals of Medicine for 1803-4, cloſes a long account of the report on Dr. Smyth's petition with the following obſervations: "But during the "courſe of February, March, and April, 1804, "Dr. Duncan founding on the obſervations "of Guyton Morveau, employed in the clini- "cal wards of the Royal Infirmary of Edin- "burgh, common ſalt in place of nitre with "no leſs advantage, and he thinks with leſs "inconvenience." P. 463. That muriatic acid gas is, to a certain degree, reſpirable, Dr. Smyth admits,* thus contradicting the fundamental poſition of the committee on his own caſe. That it can be reſpired undiluted, I never have aſſerted: but that it can be reſpired with equal utility, ſafety, and convenience, in like quantities with the nitric, I ſhall now prove; and alſo that it could not be the inconvenience or danger attending its uſe

* Letter, p. 28.

that

that cauſed it to be diſcontinued at Edinburgh, and never uſed, as Dr. Smyth ſays, "ſo far as "I know *any where elſe." I preſume that this is an error of memory, or of the preſs, for in two pages afterwards Dr. Smyth, ſpeaking of his own book, ſays: "This Treatiſe con- "tained alſo a relation of the trials made with "the marine acid gas in the hoſpitals of "France."† Now the year 1794 was certainly ſubſequent to the year 1779, and trials made in the hoſpitals of France were certainly made ſomewhere. But if trials of muriatic acid gas were made *any where* with effect and ſafety, then are the committee of the Houſe of Commons miſtaken in their fundamental poſition. They were repeatedly made both in France and England before the committee were called upon to decide, and it cannot but be eſteemed unfortunate, both for the cauſe of ſcience as well as the cauſe of juſtice, either

* Letter, p. 24. † Letter, p. 26.

that

that theſe trials were not ſeen in their true light, or were not obſerved at all. By dedicating my pamphlet to the Houſe of Commons, I did mean to appeal to the judgment of the Houſe from the judgment of the committee, and with all due reſpect to the authority of both, I do now confeſs myſelf not ſatisfied with the deciſion of either. But I ſhall not again return to the charge.*

Inſtead of explaining what value is to be ſet upon the teſtimonies which I have produced from my friends, or upon the experiments and calculations, which I made for aſcertaining the equal if not the ſuperior convenience in application of the muriatic acid gas to the nitric, Dr. Smyth contents himſelf with declaring, that "the preceding authorities *is*† the only "anſwer he ſhall give to the experiments of

* Smyth's Letter, p. 28.

† The word "*is*," thus decorated with italics, is copied from Dr. Smyth's Letter exactly.

" my friends." " Forgetful that a relation of " experiments and obſervations, by perſons qua- " lified to make them, of undoubted veracity, " and free from all ſuſpicion of partiality or " prejudice, is the only evidence that a ſubject " of natural knowledge can admit." Smyth adverſus Trotter, Appendix to the Report, No. 35, p. 92.

I ſhall therefore again bring forward thoſe teſtimonies, ſhall again inſiſt upon the propriety and validity of my own ſtatement; I ſhall add to them the reſult of further experience, and by this proceſs I ſhall hope to prove, that the authorities employed by Dr. Smyth are not quite ſo deciſive as he may ſuppoſe them to be.

That muriatic acid gas is a power capable of deſtroying contagion, the committee of the Houſe of Commons did not deny. I ſhall beg the attention of the reader to the following ſtriking evidence of its efficacy, which is co-

pied

pied from my pamphlet: "Four years ago "Mr. Bloxham, ſurgeon and apothecary at "Haleſowen, aſked my advice concerning a "malignant fever which had proved very fatal "to many perſons, and had continued long in "the poor-houſe of that town. For as often "as freſh paupers were brought into the houſe "they received the infection, though it had "apparently ſpent its violence upon thoſe who "had reſided in it for ſome time. The houſe "had been white-waſhed, and the ordinary "precautions of waſhing the floors with vine-"gar, burning tar, &c. were uſed in vain. I "adviſed him to fumigate all the apartments "with the muriatic vapour, and to continue it "for many days. This was done thoroughly, "and afterwards there was no inſtance of the "recurrence of contagion."

"I could fill a volume with caſes in my own "practice in which muriatic vapour was effica-"cious in preventing and deſtroying contagion.

" The following will undeniably prove that " this vapour can be uſed in very large quan- " tities, in places from which perſons have *not* " been removed:

" In Mr. Wilſon's houſe, conſiſting of two " rooms, about fourteen by twelve, and a ſmall " ſtaircaſe between, on each floor, the father " and four children had ſcarlet fever and ulce- " rated ſore throat. Half a pound of ſalt was " placed in every apartment except the kit- " chen, and upon the two landings of the ſtair- " caſe, the whole moiſtened in the courſe of " two days with lb. iſs. of vitriolic acid; that is, " more than three ounces of acid to each pound " of ſalt, making in all ſeven veſſels yielding " vapour. In this ſmall ſpace the ſmell of " muriatic acid was very pungent during two " days, and yet after the firſt mixing, it was " reſpired not only without difficulty, but even " with pleaſure as well as advantage.

" Miſs Greaves had dreadful putrid fever:

" ſhe

" ſhe lay in an apartment at moſt nine feet
" ſquare, without any chimney. On firſt mix-
" ing the vitriolic acid and ſalt (as is very com-
" mon) the ingredients were mixed together ſo
" incautiouſly and in ſuch large quantity that
" the room was inſtantly overloaded with va-
" pour, and cough excited. But this inconve-
" nience was remedied by after-experience, and
" the room kept full of vapour for ten days.
" She recovered, and not one of a large family;
" and of the attenders in a public ſhop, was
" infected. Here then is a proof that reſpiration
" can continue in a room nine feet ſquare
" filled with muriatic vapour, not only with-
" out inconvenience but probably a public
" bleſſing, by preventing the diffuſion of a ma-
" lignant contagion in the centre of a popula-
" tion of ſeventy-thouſand ſouls.

" Mr. Harris, jun. had ulcerated ſore throat,
" with eruptions all over the ſkin, but eſpe-
" cially of the thighs, ſtrikingly like the meaſles,
" and indeed at firſt miſtaken for that diſorder

" till

" till ſome of them terminated in black and
" purple ſpots. In this alarming caſe the mu-
" riatic vapour was recommended and perſe-
" vered in for many days. The young man
" recovered, and none of a large family, who
" had free acceſs to the ſick apartment, were
" infected. In mixing up the ingredients for
" the firſt time, the nurſe held the veſſel con-
" taining ſalt in her hand, and poured the acid
" upon it, juſt under her noſe; of courſe it
" excited coughing. So ſoon as it was diffuſed
" over the room it had no ſuch effect, but
" was exceedingly grateful to the patient.

" In the uſe of muriatic vapour this caution
" muſt always be obſerved: On firſt mixing
" the acid with the ſalt, the vapour imme-
" diately ariſes in larger quantity than it does
" from nitre, and the undiluted riſing ſteam
" of vapour ſhould be avoided till diffuſed ge-
" nerally over the apartment. All coſtly furni-
" ture, poliſhed iron, gilt chairs, coloured li-
" nens, ſhould be removed, as the muriatic

" acid

" acid gas corrodes the metallic bodies, and " diſcharges the colours of the linens and " weakens their texture. The nitric vapour " has the ſame effect, but it riſes more ſlowly, " and in the proportions ordered by Dr. Smyth " there hardly appears any fume, and the ſmell " is not very perceptible. It ſhould be re- " marked likewiſe, that from equal weights " of nitre and common ſalt, decompoſed by vi- " triolic acid, nearly one-fourth more gas is " extricated from the common ſalt than from " the nitre ; as one hundred parts of common " ſalt contain nearly fifty parts of muriatic acid, " whereas one hundred parts of nitre only con- " tain thirty-four or thirty-five of nitric acid.

" On the comparative convenience or incon- " venience of application of the nitric and mu- " riatic vapours, I ſhall now give the deciſive " evidence of experiments made in the infir- " mary of Worceſter and the general hoſpital " near Birmingham ; and I ſhall ſubjoin the " teſtimony of an intelligent and amiable lady,

" to

" to whom the afflictions of a large family have
" unfortunately afforded too much ſcope for
" accurate obſervation on this ſubject. The fol-
" lowing is the reſult of the experiments made
" by my learned friend Doctor Skey, phyſician
" to the Worceſter infirmary.

" DEAR SIR,

" I regret that various circumſtances have
" combined to prevent my returning you an
" earlier anſwer to your letter. The time,
" however, which has elapſed has enabled me
" to give a fairer trial of the two acid vapours,
" as to their reſpective effects on the lungs.

" The general reſult of the experiments
" made in three of our wards, namely, two
" on the women's ſide and one on the
" men's, with as accurate an attention as I
" could pay to aſcertain the fact in ſimilar
" circumſtances, was, that on the whole
" more diſtreſs was felt by the patients when
" the ward was filled with nitrous acid va-
" pours than when with muriatic. The firſt

" gave

gave riſe to ſickneſs, vomiting, cough, and dizzineſs, the laſt did ſo too, but in a much inferior degree; ſuch at leaſt was the almoſt unanimous opinion of the patients themſelves. I ought to remark, that as the object was to try the relative effects of the acid vapours on breathing, in both caſes the patients were more expoſed to the vapours than is commonly neceſſary in fumigating. To this muſt be attributed the more than uſually diſagreeable effects of the nitrous acid vapour, which occurred particularly in one of the women's wards, where were ſeveral much debilitated patients.

I remain, dear ſir,

Yours faithfully,

(Copy.) Jos. Skey.

Worceſter, Sept. 22, 1802.

In the general hoſpital near Birmingham the following experiments were made under the inſpection and direction of the Rev. George

Hickes, A. M. rector of Burnsall, in Yorkshire, and of Mr. Partridge, house surgeon and apothecary to the hospital.

"In two unoccupied lower wards, each "about thirty by twenty-one, and fifteen feet "high, were placed two earthen vessels, with "a pound of sulphuric acid in each. In the "vessel on one side a pound of common salt "was added to the acid, and in the other a "pound of common nitre, and both rooms were "shut up for some time. The ward with the "mixture of common salt and sulphuric acid "was full of vapour, which was pungent to the "nose but excited no cough, and was thought "pleasant. A water-closet adjoining, which "before was very offensive, in two or three "minutes was rendered perfectly sweet on the "door being opened and muriatic acid gas ad-"mitted into it. In the corresponding ward "the smell of nitrous vapour was not near so "strong, nor when we stood over the vessel

"did

" did it ſo ſenſibly affect the noſtrils as the
" muriatic; but it much more ſtrongly irri-
" tated the top of the windpipe, producing
" the irreſiſtible neceſſity of coughing; and the
" gentlemen obſerved that the chief differ-
" ence of their feelings in the two wards was,
" that in the one the muriatic acid gas was more
" ſenſibly felt in the noſe, affecting it with a
" ſort of tickling, which was far from unplea-
" ſant, and that in the other the nitric acid
" gas always affected the throat ſo ſtrongly
" as neceſſarily to induce coughing, (which
" was not the uſual effect of the muriatic) but
" did not irritate the noſe.

" In the two large middle wards three veſ-
" ſels, each containing half an ounce of ſul-
" phuric acid, were placed, according to Dr.
" Smyth's direction, at the diſtance of fifteen
" feet from one another, and an equal weight
" of nitric was added to the three veſſels in
" one ward, and of ſalt to the ſame number
" in the other. To the veſſels containing
" nitre heat was applied. The ſmell of the

" two acid vapours was very faintly percep-
" tible in both wards, and the patients felt
" no inconvenience from, and expreſſed no
" diſlike of, either.

" Mr. Partridge, who has repeatedly uſed
" both the nitric and muriatic vapours in the
" different wards of the hoſpital, upon the
" whole finds the muriatic more agreeable to
" the patients than the nitric, after it has been
" diffuſed over the apartment. On its firſt
" riſing, and till it was diffuſed, it of courſe
" could not be agreeable. The difference,
" however, he thinks inconſiderable, and he
" finds both equally uſeful for deſtroying the of-
" fenſive ſmell of crowded wards, and both
" nearly equal in convenience of application."

My friend Mrs. Whateley's letter is deciſive of the fact of convenience; and on this point I would prefer the experience of a tender mother, watching over her ſick child, to the opinions of all the chemiſts in Europe. It is a plain queſtion of fact, cogniſable by the ſenſes, and aſcertainable by the ſimpleſt and moſt eaſy experiments.

Birmingham, December

DEAR SIR, 14th, 1802.

Having unfortunately had occaſion, from the illneſs of my children, to make uſe of fumigation, and hearing of your intention to publiſh on the ſubject, I ſend you the following ſtatement: My youngeſt boy, a child eight years of age, was very dangerouſly ill in November laſt of a malignant fever and putrid ſore throat. By the advice of our ſurgeon, Mr. Freer, I fumigated the room with the muriatic acid vapour, and though a young ſervant girl and myſelf conſtantly attended him, and we had alſo continual intercourſe with the reſt of the family conſiſting of nine other perſons, not one had the ſlighteſt attack of the diſeaſe. I fumigated the room ſo ſtrongly that a white cloudy vapour was viſible conſtantly floating in it, yet I perceived not the ſmalleſt difficulty of breathing; and the child, though the veſſel which contained the fumigation ſtood very near his bed, appeared and expreſſed himſelf

refreſhed

refreſhed by it; and an old nurſe more than ſeventy, and aſthmatic, perceived not the leaſt inconvenience, on the contrary ſhe thought her cough allayed by it.

On a former melancholy occaſion I uſed the nitric acid vapour, which I found troubleſome from the heating of the ſand; and on approaching too near the veſſel which contained this fumigation and pouring in more vitriol I was nearly ſuffocated, and with difficulty got out of the room, and the unpleaſant effects did not leave my lungs of more than two hours. I approached the veſſel which contained the muriatic fumigation as near frequently, and renewed the proceſs (of pouring the vitriol on common ſalt) but was affected in an infinitely ſlighter degree.

I am, dear ſir,

Yours truly,

(Signed) MARY WHATELEY.

At this period, whilſt I am writing this part of my reply to Dr. Smyth, I daily attend a female

male ſervant of my eſtimable friend Mr. J. Crompton. She has malignant fever of the worſt kind, and the ſmell of the room when the muriatic acid gas roſe in diminiſhed quantity, has been once or twice intolerably offenſive. But the offenſive ſmell has been always removed by renewing the proceſs, and raiſing the gas. In a room ſeventeen feet ſquare two pounds of ſulphuric acid, with an adequate proportion of ſalt, have been uſed every twenty-four hours for five days paſt, without any inconvenient pungency. To-day (April 6th) a deep jug, filled with two or three pounds of ſalt, conſtantly ſtirred up and ſupplied with ſulphuric acid, is uſed. The muriatic acid gas is very perceptible, not at all diſagreeably pungent, and the room is perfectly ſweet, although the caſe is of the moſt deſperate and putrid kind. In this inſtance I uſed a quantity of muriatic acid gas, nearly five times greater than that of nitric acid gas, recommended by Dr.

Smyth

Smyth in the ſame ſpace, and entirely without inconvenience.*

The whole queſtion of convenience of application indeed appears to me to depend upon quantity. In equal quantity, muriatic acid gas is at leaſt as uſeable as nitric, as uſeful too, and probably more agreeable. Both muſt be diluted with atmoſpherical air before they can be breathed. The undiluted ſteam of riſing gas of both muſt be avoided. Mr. Menzies, in "Account of the Experiment made on board "the Union Hoſpital Ship," p. 12, ſays: "I "however proceeded in this firſt trial ſlowly "and cautiouſly, following with my eyes the "pipkins in every direction, to watch the effect "of the vapour on the ſick, and obſerved that "at firſt it excited a good deal of coughing,

* As to the quantity of muriatic acid gas which can be borne without inconvenience in a given ſpace, I find that in an apartment containing 360 cubic feet reſpiration is not diſturbed by the gas extricated from one ounce of ſalt by one ounce of ſulphuric acid.

"but

" but which gradually ceaſed in proportion as " it became more generally diffuſed through " the wards." This fact proves that the nitric acid gas may excite coughing as well as the muriatic, and that *it requires to be diſengaged at a diſtance from the patient, otherwiſe its extreme pungency will be offenſive to the lungs.* I will not, however, deduce from this experience of Mr. Menzies, as Dr. Smyth has done from the caution of my father, that it plainly ſhews " how little he thought them " (mineral acid gaſes) calculated for being em- " ployed in the apartments of the ſick."* I ſhall not deſcend to the mean competition of extolling the muriatic for the ſake of depreciating the nitric acid gas. They are both uſeful upon the ſame principle, and both may be uſed for the ſame purpoſes, with no great difference of convenience. What difference there is, plainly is in favour of the muriatic, from the ſimplicity of the proceſs by which it can be

* Smyth's Letter, p. 8.

raiſed. An earthen veſſel, filled with ſalt, and free from all metallic ſubſtances, and a bottle of ſulphuric acid, are the only inſtruments neceſſary in the diſengagement of it, to which may be added occaſionally water and heat. The only caution neceſſary, is, not to pour too much ſulphuric acid at once upon the ſalt. Pour it on by little and little, and frequently, and now and then ſtir up the mixture with a ſtick. When the atmoſphere is very dry, and a quantity of moiſture does not adhere to the ſalt, the application of the agents to each other is aided by the addition of a little water. In general the caloric let looſe by the mixture of ſulphuric acid and muriate of ſoda, is adequate to all the purpoſes of extricating muriatic acid gas. Whenſoever it is wiſhed that a large quantity of gas ſhould be diffuſed quickly, then the veſſel may be placed on hot coals, or in any other way expoſed to heat. For the purpoſe of correcting the vitiated air of the chamber in which the ſick man lies, and of deſtroying contagious

tagious matter, the ſimple pouring of ſulphuric acid in ſmall portions, and at intervals, on common ſalt is ſufficient. But whilſt there is any chance of contagious matter being formed the muriatic acid gas ſhould be raiſed without intermiſſion; the ſmell of it in the room ſhould be always perceptible. It will anſwer little good purpoſe to uſe it one hour, and intermit it the next. The poiſon of contagion, formed by peſtilential diſeaſes, is every inſtant exhaling from the lungs the fauces and the excretions of the body; every inſtant, therefore, ſhould it be met, and conſumed by the muriatic acid gas. Whereſoever it lurks it muſt be met, and therefore ſhould the bed-clothes be frequently turned down and a current of muriatic acid gas be paſſed through them. All infections are equally deſtroyed by the application of the mineral acid gaſes, if we may rely on the obſervations of Morveau and many of our own ingenious countrymen.* I direct the

* Crawford, Cruikſhank, &c.

muriatic acid gas frequently in caſes of infection unattended by fever, when rooms are rendered offenſive by venereal ulcers, and other putrid exhalations of animal matter. I do not mean to ſay that the conſtitutional effects of theſe poiſons, when acting on the living ſyſtem, are cured by it, although I believe that recovery may be facilitated and even haſtened by the patient being thus placed in circumſtances more conducive to his comfort, and more favourable to his general health. The great and important action of mineral acid gaſes is in decompoſing and conſuming the poiſon and contagion of dead animal matter. In no reſpect do they ſuperſede the neceſſity of ſtrict attention to cleanlineſs, and of conſtant ventilation.

The diſadvantages of the application of nitric acid gas in compariſon with the muriatic are, that it requires more attention and gives more trouble. Heat muſt be applied, and conſequently the effects of fire muſt be guarded againſt; which in ſhips is a conſideration of

great

great moment. The materials are more expenſive, and after all, the nitrate of potaſh is ſeldom bought ſufficiently pure; it is generally adulterated with ſalt. This adulteration may occaſion conſiderable inconvenience by letting looſe nitro-muriatic acid gas when ſulphuric acid is poured upon the mixed ſalt, and this acid gas is very ſuffocating. The nitric acid gas is leſs expanſible than the muriatic, and is ſooner condenſed. Nitric acid gas only gives out oxygen*, by being converted into nitrous gas,

* It was my father's opinion that in the extrication of nitric acid gas a larger portion of oxygen was communicated to the air. This opinion is not chemically correct; but my father has erred in adopting it, on a point in which that moſt ingenious and celebrated chemiſt, Mr. Keir, was content to doubt. "Whe-"ther theſe white fumes derive any advantage from the vital or "oxygenous air that is extricated from the mixture of oil of vi-"triol and nitre, I will not preſume to offer an opinion." Report, ap. 37, p. 116.

Guyton Morveau has fully diſcuſſed this point, and he aſſerts that no oxygen is communicated to the air, except by the converſion of nitric acid gas into nitrous gas.

I allow

and nitrous gas acts only on the reſpirable portion of the air. Finally, if metallic ſubſtances are in contact with the mixture, the ſuffocating irreſpirable nitrous gas is let looſe.

If then we compare the two proceſſes fairly it appears to me that the preference muſt be given to the muriatic, which, I again aſſert, in equal quantity to be equally reſpirable and agreeable as the nitric acid gas.

The opinions of profeſſor Odier, of M. Mojon, and of Dr. M'Gregor, if we had the power of exactly aſcertaining the grounds on which they were formed, I have no doubt would be found to depend upon quantity. Dr. Smyth

I allow with Mr. Keir that the application of nitric acid gas was the proper diſcovery of Dr. Smyth; I allow him the merit of the further diſcovery, "which diſtinguiſhes it from the red "vapour of nitrous acid, and from the fumes of ſulphur." But I muſt except the diluted fumes of ſalt, which Mr. Keir has not excepted. I am confident if this accurate and ſagacious obſerver will be pleaſed to make the comparative experiments, that he will find the muriatic acid gas, to be at leaſt equally reſpirable with the nitric, in like quantity.

recommends

recommends nitric acid gas in ſuch ſmall doſes that it can be never diſagreeable, often ſcarcely perceptible when his rule is ſtrictly followed.

The preſident of the criminal tribunal mentioned by Dr. Odier, muſt have given his orders ſolely from quantity. The muriatic acid gas was riſing in exceſs at the moment of his entering the priſon; perhaps, the attendants ſeeing his approach, added on the ſpur of the occaſion, too much ſulphuric acid to the ſalt, and thus expoſed him to the pungent effects of an undiluted cloud of gas. Under the ſame circumſtances had nitre been uſed inſtead of ſalt, the effects would have been the ſame, and he then probably would have ordered the muriatic inſtead of the nitric fumigation.

Dr. M'Gregor's patients had an over-doſe, otherwiſe there could have been no difficulty in applying, and continuing to apply, muriatic acid gas whilſt the patients were confined to their beds. I again repeat that in all theſe ſeeming

exceptions

exceptions the muriatic acid gas was firſt of all raiſed in exceſſive quantity, and probably diſcontinued without further trial. I cannot otherwiſe account for the different reſult of the experiences of theſe gentlemen from my own, and from the common experience of this country. I cannot otherwiſe account for the very different reſult from that of Dr. Duncan, ſen.* who in 1802 employed nitrous fumigation with good effect, and who in 1804 employed muriatic with no leſs advantage, and he thinks *with leſs inconvenience.*†

The experiments of Guyton Morveau have, however, determined this point; and I ſhall now adduce ſome of them, as they fix an everlaſting ſeal upon the diſcovery of my father.

Traité des Moyens de deſinfecter l'Air, de prevenir la Contagion, et d'en arreter les Progres. Par L. B. Guyton Morveau, à Paris 1801.

* Appendix to the Report, No. 12, p. 50.

† Annals of Medicine, 1803-4, p. 463.

"All

" *All the mineral acids poſſeſs the power " of deſtroying contagious miaſmata, and the " ſmells which announce their preſence, eſpe- " cially when in a ſtate of vapour or gas. But it " may be aſked are they all efficacious in the " ſame manner, are they all equally prompt and " complete in their actions?" To aſcertain the fact, Morveau made experiments on the gas extricated from putrefying meat, and the reſult, as drawn up by Berthollet, proved that

†1. The gas produced by putrefaction contains a good deal of carbon and no hydrogen.

2. Urine expoſed to light in cloſed veſſels remains acid. In the dark it forms ammonia.

3. Meat kept fifteen days in cloſed veſſels with a ſmall quantity of water, rendered the water acid with a little ammonia.

4. This meat, on ſtewing it, was ſtill capable of affording jelly.

* P. 65. † P. 79.

5. The gas from putrefaction gave him the colic twice. It is neceſſary to make experiments with it cautiouſly.

6. The putrid principle in the air is not abſorbed by lime water, but when diſſolved in water it is.

7. A ſubſtance not putrid can abſorb a good deal of this gas without putrefying, but at a certain point it is much diſpoſed to putrefy.

8. Water is compoſed in the greater number of putrefactions, but there is no developement of hydrogen.

9. The moſt antiſeptic ſubſtances are bark and nut-galls.

By expoſing the gas from putrefaction to various powers, Morveau deduces the following facts: "* *Water* does not deſtroy the odour, on "the contrary retains it. *Lime* is only uſeful for "decompoſing animal ſubſtances before putre-

* Moyens de Déſinfecter l'Air, p. 270, &c.

"faction

" faction commences, or for abſorbing the car-
" bonic acid. Freſh ſlacked lime in large quan-
" tities ſuſpends for a time the cadaverous ema-
" nations. Air full of putrid miaſmata is not
" purified by paſſing through lime water. *Re-*
" *ſinous ſubſtances* only maſk for a moment the
" putrid odour, without purifying the air. No
" advantage is to be expected from *fires*, and
" *throwing various ſubſtances on hot coals.* Vi-
" negars are burnt rather than converted into
" vapour; nitre only lets looſe unreſpirable gas,
" after having rendered the fire more intenſe
" by its oxygen; and firing gunpowder exer-
" ciſes only a mechanical effect on the air.
" *Sulphur* acts otherwiſe. Thrown on burning
" coals it gives out a ſulphureous vapour, which
" deſtroys the putrid miaſmata, but does not
" diffuſe itſelf to any diſtance, and is unſup-
" portable in inhabited places. It may be uſeful
" for purifying infected merchandize, and re-
" moving ſtagnant air. *Common vinegar* may
" be reckoned one of the beſt antiloimics for

" bodies that can be plunged in it, or well
" waſhed by it. Its vapours are not ſufficiently
" expanſible for the purpoſe of fumigation.
" *The acetic acid* is not more expanſible than
" common vinegar, but its action on infected
" ſubſtances is more rapid and intenſe; it is
" a perſonal preſervative not to be neglected
" by thoſe who are obliged to expoſe them-
" ſelves to putrid effluvia.

" It is generally known that the mineral
" acids are antiſeptic, that they put a ſtop to
" all vegetable and animal fermentation, and
" that they decompoſe the contagious poiſons.
" *The ſulphuric acid,* on account of its fixity,
" cannot be employed for purifying air. Even
" in its concentrated ſtate Dr. Crawford ob-
" ſerved that it was leſs prompt in deſtroying
" the fetid odour of animal hepatic gas, than
" the nitric and oxygenated muriatic acids.
" We have ſeen how far ſulphureous acid
" gas could be employed before.

" The nitrous acid only acts on the reſpirable

" part

" part of air; the vapours it exhales are ſuffo-
" cating. The nitric acid diſengaged after
" the manner of Dr. Smyth deſtroys entirely
" putrid miaſmata,* but it riſes little, it con-
" denſes ſoon, and only acts as an oxigenator
" in giving out nitrous gas; it is only by re-
" peating the operation frequently, even in a
" confined and narrow ſpace, that we can rely
" upon its efficacy. Finally, the operation
" requires precaution in the choice of mate-
" rials, and the manner of uſing them. I do
" not ſpeak of the conſumption of nitre, which
" ought to be perfectly pure, and will be con-
" ſequently dear, this would ſuppoſe us ca-
" pable of trading in the preſervation of hu-
" man beings. *The muriatic acid*† preſents
" here the greateſt advantages, on account of
" its prodigious expanſibility, which enables
" it to reach preſently the matters to be acted

* Exp. xxvi, xxxii, xxviii.

† Exp. xxix, xxxii, xxxiii, xxxiv.

" upon.

" upon. The manner of employing it too is " very ſimple and unexpenſive, and expoſes " ſhips and combuſtibles much leſs to acci- " dents from fire than the method of Dr. " Smyth, ſince all other heat may be diſpenſed " with except that produced by the mixture of " the ingredients. In adding to this operation " a ſmall quantity of the oxide of manganeſe " we procure the oxygenated muriatic acid " gas, which I have proved to be the moſt " ſure preſervative, the antiloimic by excel- " lence, and which further recommends itſelf " by the facility of appropriating it to all caſes."*

On the theory of action of the mineral acid gaſes, Morveau concludes thus: " When we " have burnt the clothes and furniture of an " infected perſon was it ever ſuſpected that any " portion of the virus remained in the aſhes? " Are we not forced to acknowledge that the " whole poiſon is deſtroyed by the combuſ-

* P. 251, &c.

" tion?

" tion? But if the poiſon be a combuſtible,
" obedient like all the others, to the affinities
" of the oxygen of the atmoſphere, upon what
" ground can it be imagined that it will reſiſt
" the condenſed oxygen of our anti-conta-
" gious power, which operates ſo rapidly ſuch
" aſtoniſhing combuſtions, which is the power
" *the moſt burning* that we know in nature?
" We need only then apply here, the defini-
" tions before eſtabliſhed, that is, to call things
" by their right names; to declare, without
" waiting for further vain trials, that the poi-
" ſon attacked by the ſuperabundant oxygen of
" the muriatic acid, will be as entirely *burnt* as
" by fire raiſed from any other combuſtible, and
" will be accompanied with heat, let looſe by the
" oxygen of the air, in departing from its gaſ-
" eous ſtate. I ſhall uſe another language to
" thoſe to whom this doctrine is not familiar.
" Is it not proved by unfortunate experience
" that a letter ſent from one infected by the

" plague

" plague can communicate this terrible diſeaſe?
" Is it not on this account that we never re-
" ceive a letter from ſuſpected countries which
" has not been dipped in vinegar? When it
" has undergone this operation you touch it
" without fear, becauſe you believe that any
" poiſon ſticking to it has been deſtroyed. It
" is not neceſſary then to ſeek further for
" proofs of the poſſibility of this deſtruction.
" Your cuſtoms, formed after the example of
" thoſe of all other men, are founded upon
" this principle, and declare conviction.

" Compare now, only in their ſenſible effects,
" the agent which has given you this ſecurity,
" and that powerful anti-loimic which is offer-
" ed to you in the oxy-muriatic acid. On
" the one hand is a liquor placed in the loweſt
" order of antiſeptics, which produces a very
" ſlightly ſtimulating effect on our ſenſes,
" which ſeaſons rather than alters ſubſtances
" of taſte, which *adds its own colour* to colours

" the

" the leaſt fixed without ſeparating the ele-
" ments of them, and whoſe action always ſlow
" is only efficacious upon bodies ſoaked in it.
" On the other hand is a ſubtle fluid which
" once diſengaged, darts on its own wings and
" ſuddenly occupies the whole ſpace of the
" largeſt habitations; leaves nothing untouch-
" ed, and touches nothing that it does not
" appropriate; which radically deſtroys colours,
" taſtes, and odours the moſt virulent, which
" ſpontaneouſly inflames oils, ſulphur, and
" metals; which breaks aſunder the texture of
" all organiſed matter, and of which no living
" ſoul can receive the ſlighteſt impreſſion
" without ſome extraordinary ſenſation giving
" notice of its preſence. Such is the great in-
" ſtrument of diſinfection with which modern
" chemiſtry has made us acquainted, and which
" it has taught us to employ without danger,
" and with the certainty of obtaining a ſalu-
" tary reſult. I do not believe that any doubt

" can remain that peſtilential poiſons, what-
" ſoever ſhape they may aſſume, and where-
" ſoever they may exiſt, will be reduced to the
" ſtate of inert matter by the energy of its
" action."

I muſt entreat my reader to pardon me, for the introduction of one or two more particulars, before I conclude this long and painful inveſtigation of the ſubjects forced upon mymind, by Dr. Smyth's Letter to Mr. Wilberforce.

With reſpect to Dr. Withering's uſe of nitric acid gas, I never knew that he did employ it, although I was in the habits of friendly intercourſe with him from Auguſt 1794 till his death; nor did I know that he had publiſhed any thing about it in Duncan's Annals of Medicine till the reſult of his trials was pointed out by Dr. Smyth.* Whatſoever were the fact, ſo far as nitric acid gas was concerned, ſtill Dr. Withering muſt have been previouſly

* Letter, p. 26.

acquainted

acquainted with my brother's recommendation of muriatic acid gas, for my brother James's Treatiſe on Malignant Angina, and Dr. Withering's book on ſcarlatina, were in ſome ſort controverſial; and in the latter work it appears to me, that alkalies are preferred to acids. With Dr. Smyth's publications I have long been acquainted, but they did not ſeem to me to require controverſy, for they did not provoke it. The ſtatement was inconſiſtent, the nitrous acid was extolled, the principle of diſcovery was not explicitly urged, and though a ſort of claim to originality was irregularly ſcattered and indiſtinctly hinted, yet I had "no wiſh to criticiſe any man's writings, "however liable they might be to cenſure, "unleſs when provoked by inſult or impelled "by duty." When Dr. Smyth's petition was preſented to Parliament, claiming reward for diſcovery of the principle, I *then* was impelled by duty to aſſert my father's claim, and even

then I did not criticiſe Dr. Smyth's writings. I plainly ſtated my father's caſe with as little reference as poſſible to Dr. Smyth. It is now only that I feel myſelf called upon to criticiſe his writings, when I am provoked by inſult.

Let me aſſure Dr. Smyth that it was no idea of the reward annexed to diſcovery that firſt ſtruck me, and that firſt impelled me to vindicate the claim of my father. It was Dr. Smyth's excluſive claim that ſtruck me for the firſt time,* THEN, when his petition was preſented to the Houſe of Commons. My father was THEN dying, and therefore did I become "the claimant for my father, and champion "of the family."† But it was not reward that impelled me in the firſt inſtance, except indeed the conſciouſneſs of having diſcharged my duty to my venerable old father, can be called ſuch. I believe I have at leaſt as clear a perception as Dr. Smyth, *quantum inter laudem*

* Letter, p. 27. † Letter, p. 25.

et

et lucrum interſit. The pecuniary reward ſhould deſpiſe, without the praiſe.

Falſus honor juvat, et mendax infamia terret,
Quem nifi mendoſum ?

The maxim which Dr. Smyth has choſen to apply to me, and which is next in order to my "firſt Eſſay," *

Quis enim virtutem amplectitur ipſam,
Præmia ſi tollas ?

* Referring to an article of the Morning Chronicle, March 12, 1802, and imputing it to me, Dr. Smyth quotes the following words: "It ſtill remains to be proved whether the diffuſed "vapour of muriatic be not equal in efficacy, and equally con- "venient in application with the nitrous acid vapour for correct- "ing vitiated air in ſhips, priſons, and hoſpitals." In a tone of contemptuous exultation the Doctor ſubjoins, "and thus in "1782 (1802 it ſhould be) it ſtill remained to be proved what "in his pamphlet he affirms to have been proved fifty years be- "fore." Upon this ſarcaſm I muſt remind the Doctor, *non ſat commode diviſa ſunt hæc temporibus.* Ter. Adelphi.

The abſurdity lies not in my calculation but in Dr. Smyth's miſcalculation—not in my affirmation but in his miſtatement—not in my logic but in his pleaſantry. I never affirmed, nor even hinted, that the efficacy or the convenience of the muriatic acid gas, as compared with the efficacy and convenience of nitric acid

he himſelf has not diſavowed practically. He acted upon it when he wrote his memorial to Lord North; he acted upon it when he preſented his petition to the Houſe of Commons. And in the interval between the application to Lord North and the publication of his book on Jail Fever, "his humanity did not revolt "at the idea" of forbearing to publiſh the reſult of his experiments at Wincheſter, and in hoſpitals and private practice.

The undiſguiſed and unceaſing ſuperciliouſ-

acid gas, had been eſtabliſhed by my father fifty years before, or that they had occurred to him at any later periods of his life as ſubjects of compariſon. I contended for his uſe and his knowledge only of the muriatic acid gas, long before the nitric appears to have been preferred or even uſed by Dr. Smyth. Whether the one be equal in convenience or efficacy with the other, was a doubt merely my own. I was not ſatisfied with the experiments which Dr. Smyth employed to ſolve it, and in order to obtain fuller ſatisfaction I made myſelf ſeveral experiments, and I collected from other men that information which is introduced into my pamphlet, and in which, my confidence is certainly not ſhaken by the paſſages which Dr. Smyth oppoſes to it.

neſs

neſs with which Dr. Smyth has been pleaſed to treat the character of my father compels me to protect his inſulted memory, and the reader I truſt will give me credit for intending not to laviſh upon him, ſuperfluous and oſtentatious panegyric, but to vindicate him, by well-merited and extorted praiſe againſt contumely. My father had never, like Dr. Smyth, been honoured with any public commiſſion from the authority of government. He never had acceſs to patrons who were powerful in their influence and munificent in their favours. He* was deſtined to be, and he was content with being, a country phyſician. But his powers and his attainments are not to be meaſured by the accident of his ſituation. On the contrary his knowledge was at once comprehenſive and

* I know not any phyſician who conformed more exactly to the rule laid down by Hippocrates in his Epidemics, lib. 1, 5, and thus luminouſly expreſſed by Celſus: "*Mederi oportere, et communia et propria intuentem. In Præfat.*

accurate

accurate—his application both to books and to profeſſional duties was intenſe—his apprehenſion was quick—his memory was retentive—his penetration was keen—his judgment was correct—his elocution was ready, copious, and energetic—his practice was long and extenſive—his employers were numerous and reſpectable—and in the catalogue of his private friends he had the honor to recount the names of men eminently and deſervedly diſtinguiſhed for elegant taſte, for claſſical erudition, and for deep reſearches into all the various branches of philoſophy, which are intimately or remotely connected with the ſtudy of medicine.

Nihil is vidit, neque in vita, nec gratia, nec rebus geſtis, neque ingenio, quod deſpicere deberet Smythius.—Cic. Philip. ii.

I do not mean to depreciate the abilities of Dr. Smyth, or to deny the importance of his ſervices, when he attended the Spaniſh priſoners at Wincheſter, and ſuperintended or directed

rected the experiments that were made on board the Union hoſpital ſhip. I think him entitled to the ſplendid reward which he received from Parliament, by various and ſolid merits both of perſonal exertion and perſonal ſuffering; by his diligence in examining, and his judgment in applying the powers of nitric acid gas, and by his copious though managed communication, of his various experiments to the public. But I contend that for the diſcovery of the principle my father has a ſubſtantial and invincible claim to priority; and in regard to the agents, I maintain that the muriatic, *to ſay the leaſt*, is equally uſeful in deſtroying contagion, and equally convenient in application with the nitric.

Upon queſtions of ſcience when the members of a Parliamentary committee are requeſted to decide not on the merits of theories but the evidence of facts, I had been accuſtomed to look upon them both as honor-

able patrons, and as competent judges. I therefore wiſhed to receive from Parliament ſome mark of favour, not for myſelf be it remembered, but for my father, to whoſe memory I ſhould gladly have erected a monument with any ſum which might have been granted for that purpoſe; or for ſome other branch of my family, upon whom Government in its wiſdom might have been pleaſed to confer ſuch, and only ſuch patronage, as would have implied aſſent to the proofs, which I had adduced of the claims of my father. That wiſh I ſtated in my private correſpondence with Sir William Pulteney, and I now abandon it with regret indeed, but without anger.

Whatſoever may have been the cauſes of my diſappointment, and whether or no they may have been more within the ken and under the controul of Dr. Smyth than they were of my own, I forbear to inquire. But my filial piety and my ſenſe of juſtice, will be ſatisfied

by

by the conſciouſneſs of my endeavours ſo to eſtabliſh the pretenſions of my father, as ſhall ſecure both reſpect to his talents from his medical brethren and from men of ſcience, and gratitude for his ſervices, not only from his contemporaries but from poſterity.

THE END.

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