Adhesions, or Accretions of the Lungs to the Pleura, and their Effects on Respiration considered, both with Respect to Theory and Practice, in a Letter to Dr. George Baker, Fellow of the Royal College of Physicians in London, and F. R. S.


To which is now added, A Vindication thereof From some Misrepresentations.

By Malcolm Flemyng, M. D.

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I have often wished, learned Sir, that a greater number, in our profession, of such as have been for a considerable time engaged in extensive practice, would leave behind them some marks of a commendable zeal to improve their art for the good of mankind, than at present do. I mean not that they should publish systems or large treatises. This would be requiring too much. And perhaps we have too many of those already. But methinks there are very few practitioners of large experience, who have not ob-

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served several things within their own sphere, worthy of being communicated to the public, which are not to be met with in books most commonly known, or not set forth so clearly and fully as they ought to be; and which might be drawn up for the press with no very great labour. Medicine, like other arts, will always be capable of further improvement. And this is at least one of the most certain and speedy ways of improving it.

Our late great physician, the celebrated MEAD, hath left us a shining example of this public-spirited, and truly charitable disposition. Very few physicians ever were engaged, for so many years together, in so general, and so laborious a practice. Yet he could find leisure from time to time to publish, and republish with improvements, elaborate and useful treatises, both theoretical and practical: And at length crowned the whole, by printing,
printing, for the good of posterity, his highly valuable *Monita & Præcepta Medica.*

Upon a principle of this kind, however crampt both in ability and opportunities, I ventured above twenty years ago to increase the number of authors in our profession; and have published, at different times, several small tracts on such parts thereof as I was best acquainted with; by the publication of which I apprehended it was most in my power to be useful to society. My Essays having been well received both at home and abroad, and, which I value myself not a little upon, in a particular manner approved by you, I go on with the greater cheerfulness in the same route; and send you my thoughts on a subject neither trite, nor un perplexed with controversy; a subject which in the course of my reading I have not found so closely canvassed as I think its
importance deserves. I wish my manner of handling it may please you as well as my other Tracts have done. I can assure you pains and attention have not been wanting on my side. Give me leave then, Sir, without any farther preamble or apology, to proceed to my purpose.

There are two opposite opinions amongst Physicians concerning the effects which Adhesions or Accretions of the Lungs to the Pleura, so often found on opening dead bodies, may have in Respiration. The one, at the head of which we shall place the late celebrated Boerhaave, maintains that under certain circumstances they create Dysexee or Asthma, to wit, if they are broad, and join together large areas of the surfaces of the lungs and pleura. The contrary doctrine is held by his illustrious scholar, the great physiologist of our times Baron Haller; whose authority in a point of this kind is not less to be regarded;
garded; as he has laboured so assiduously and successfully in Anatomy, and the animal oeconomy; and has made so many, and so important improvements on his master's system. He denies that any such adhesions can in the least contribute to impair respiration. See his *Elementa Physiologicæ humanæ*, Vol. 3. p. 132.

It were to be wished that this controversy could be clearly and fully decided, as it concerns practice more than one may at first view imagine. Suppose, for example, we are treating a person labouring under a difficulty of breathing, which obstinately resists the most powerful remedies in common practice, if such accretions, as we are considering, can be no-wise instrumental in creating, or aggravating this disease, in that case we are to pay no regard to them at all; nor trouble ourselves whether they exist or not; but direct our curative views quite another way. On the other hand, if they sometimes
times create, or even increase and heighten dyspnæa in co-operation with other causes, then we must go on with more circumspection; and not insist too obstinately and vehemently upon remedies directed against other supposed causes of the urgent symptoms, lest, while we are combating non-entities, we should harass our patient, not only unprofitably, but to his detriment: and we ought to join sometimes with other remedies some means calculated against the bad effects of such accretions, if means of this kind are to be found.

I am far from presuming to stand Umpire between two such great Names; but only propose in this paper to set forth the arguments on both sides, in the fairest and most impartial manner, leaving you, Sir, and the reader, to judge for yourselves, and shall conclude with some practical inferences, which may seem fairly deducible
Baron Haller in the above cited place of his justly admired Elements, after having fully shown from experiments and reason that in the sound living human body there is no elastic air, of the nature of our Atmosphere, within the Cavity of the Thorax, interposed between the lungs and internal surface of the Pleura, draws the following conclusion, "That by the converse of this proposition, as air, when it gets between the Lungs and Pleura, suppose from penetrating wounds of the Thorax, is hurtful to respiration, by a parity of reason the adhesion of that Viscus to the Pleura is not in the least detrimental to it, because it excludes all interposition of air; and does not permit the Lungs to recede from the Pleura. It hath been observed above (to wit, page 121 and 122 of the same volume) that such adhesions are extremely frequent"
quent in grown-up persons. In the Dorcas, (Antilope) a swift animal, the Lungs were found adhering to the Pleura by the Paris Academicians. And like instances have been met with, and are recorded by many authors of the best credit, (who are cited in the note) in the bodies of malefactors, that were executed, and others, who immediately before their death breathed freely and well: insomuch that it has been long the opinion of several celebrated practitioners, (whose names are likewise set down at the bottom of the page) that such adhesions are entirely harmless. Thus far that illustrious author.

The other opinion, besides still keeping in view that such adhesions are often found in asthmatic bodies, is supported chiefly by reasonings drawn from the nature of respiration, and from the structure and situation of the parts immediately concerned in that function. The great person, whom we have
have reckoned its principal patron, in his 
Institutions of Medicine, paragraph 835, 
specifies "broad accretions of the lungs 
" to the Pleura as one of the many causes 
" of Dyspnæa or Asthma." And in his 
prelections, published by Haller, explaining 
this passage, he briefly says, "in this case, 
" while the Thorax is enlarging (i. e. dur-
" ing inspiration) the lungs cannot de-
" scend, and follow the diaphragm; 
" whence an incurable difficulty of 
" breathing arises." And in his public 
lectures on the diseases of the nerves, very 
lately printed, there is the following pas-
fage. "The reason why the lungs do 
" not grow or adhere to the Pleura (in a 
" natural state) is because there is then 
" a moisture interposed betwixt them; 
" where that is wanting, accretions form; 
" and these patients, before their death, 
" labour under intolerable Asthma." And 
this is all I can find relating to this mat-
ter in his works.
In order to do what justice we can to this opinion in its turn, it will be necessary to lay before the reader some things concerning respiration in general, in which both sides, and I believe all intelligent physiologists are now agreed.

In inspiration the cavity of the Thorax is enlarged by the ribs being pulled up towards the first, and at the same time bent outwards, while the Sternum is pushed outwards and forwards, and somewhat upwards. This is effected chiefly by the intercostal muscles, the external, at least, if not likewise the internal. But it is also enlarged by the action of the diaphragm, which being rendered plainer by the constitution of its fleshly fibres, makes the Thorax longer and deeper. And in natural and healthy respiration, the diaphragm contributes much more to the enlargement of the Thorax, than the change made on the position of the ribs and Sternum.
Its action is greater in males than in females.

In expiration the capacity of the Thorax is diminished by the cartilages of the ribs restoring themselves by their elasticity, and reducing the ribs together with the Sternum to their former position; and by the cessation of the action of the diaphragm, while the muscles of the Abdomen, then acting in their turn, besides their contributing to the depression of the ribs, push its middle part into the Thorax, and render it convex, thus shortening and encroaching on its cavity.

By this mechanism principally the air is alternately drawn into, and expelled out of the lungs, in healthy respiration.

In all the different states of the Thorax, in all the intermediate degrees of its capacity from the greatest enlargement to the narrowest contraction, the Lungs equally
equally accommodate their volume to its cavity, by their distention or constriction, thus keeping it constantly and uniformly full.

There is no elastic air interposed between the surfaces of the lungs and Pleura, in a state of health. Tho' this hath been much controverted of late; yet it is proved beyond all doubt by the great physiologist above named, as hath been already mentioned. The lungs and Pleura constantly remain almost quite contiguous to each other; there being only an unelastic moist vapour in small quantity betwixt them.

These things being laid down, it may be urged in favour of the opinion which we are now considering, that supposing there are broad close adhesions, or rather accretions of the lungs to the Pleura, and particularly in the lower and under part of the Thorax, near where the action of the
the diaphragm is exercised, in that case the lungs, during inspiration, cannot descend so freely, and so far as if they were wholly detached. At the same time the diaphragm, it would appear, cannot have its full scope of play, because there will be a less quantity of air drawn into the Thorax, the lungs not being so much inflated, as they are when in a free natural state. And when the Thorax is as much enlarged as the descent of the lungs will permit, inspiration breaks off; the diaphragm ceases to act, and expiration succeeds. But while that goes on, the diaphragm cannot be pushed so freely and far into the Thorax as in a sound state, by the muscles of the Abdomen pressing the stomach and liver against it, because the lungs being fixed to the Pleura must in some measure resist its ascent: so that, it would appear, the motion of that important muscle must be cramp't in both states of breathing: Now as the diaphragm in a natural state contributes more to the change
change of the capacity of the Thorax than all the other causes put together, it would seem almost certain, that when its motion both upwards and downwards is thus confined, the due facility of respiration must be proportionably clogged and impaired.

The greatest difficulty attending this opinion arises from broad and close adhesions of the lungs to the Pleura having not been seldom met with in bodies where respiration had not been observed to be affected at all. And particularly the argument drawn from the Antilope appears to me so very cogent, that if there was reason to believe such adhesions are natural to that swift species of animals, I should give it up as altogether untenable. But looking into Pitfield's translation of the French Memoirs, (for the original is hard to come at in the country) I there find it expressly remarked that they were found only in one Antilope out of five that were opened. They therefore may be fairly deemed morbid, and for that reason the inference
inference drawn from them seems to lose the principal part of its strength.

But let us try what may be further offered towards solving, or at least softening this difficulty. There is most certainly a considerable latitude even in good and laudable respiration; and many degrees intervene between the most perfect kind of breathing, such, for example, as is requisite in the swift runner of a long race, the tumbler and the posture-master; and that which may be tolerably commodious in a sedentary life, in corpulency, or in old age; so that there may subsist impediments to the former without remarkably incroaching upon, or hindering the latter; and accretions of the lungs to the Pleura may be one species of them. There are a great many conditions necessary to constitute faultless respiration; and if one only is wanting, while all the rest remain in high perfection, the inconvenience arising from that defect may be
be scarce, if at all perceptible. I have seen a sheep, while it was dragging to slaughter, exert great agility, as well as strength, in order to save itself, discovering no signs of confined or impaired respiration. When it has been killed there have appeared in its lungs, great numbers of hydatids, many of them of the size of small cherries. And butchers inform me that this appearance is frequent. Will it thence follow that such tumors are not to be numbered amongst the causes of Dyspnœa? See in the Sepulchretum of Bonetus an observation perfectly apposite here; in which such hydatids were the sole cause of an intolerable Dyspnœa in a great cardinal, which proved fatal *.

We took notice above that women use the diaphragm in respiration less than men. This is a most wise provision in nature, as it enables them during pregnancy, espe-

* Lib. 2. Sect. i. Observat. 33. Editionis Manget. cially
cially in its last months, to breathe com-
modiously, tho' the diaphragm then can
scarce act at all, being so strongly pressed
upwards by the distended uterus. It would
therefore appear highly probable that ac-
cretions of the lungs to the Pleura are
attended with much less inconvenience in
the female, than in the male sex.

Moreover it may be alleged that when
such accretions are formed very early in
life, while the ribs and their cartilages
are ductile and flexible, and in conse-
quence the Thorax is more easily dilatable
by the action of the intercostal muscles,
their bad effects will be less, than when
they first take place in advanced years,
when all the parts are more stiff and ri-
gid, and less obedient to the efforts of the
moving powers. In such a case a boy may
gradually be accustomed to breathe as girls
do, that is to supply the deficiency of the
motion of the diaphragm by a greater
rising of the ribs and Sternum in inspira-

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tion; and that habit in time may become natural.

Lastly, it may be added that they will be less hurtful when they are formed very slowly, than when brought on in a very short time, as by a pleurisy or a peripneumony. In the latter case the constitution will be as it were surprised, and greater disturbances ensue. In the former, it will gradually accommodate itself to the evil, and use the best means in order to mitigate its bad consequences, within the power of its own organism.

We shall next briefly run over such observations recorded in the abovementioned Sepulchretum, as seem not a little to favour Boerhaave's opinion. We shall take notice only of those, which appear highly pertinent; passing by many that might be adduced, were we disposed to make use of dubious and partial arguments.
The first we shall mention is the case of a boy, who was cut off at the age of five, by an acute pulmonary distemper, attended with great difficulty of breathing. He had been, when in his best health, constantly, and remarkably asthmatic upon running, or any other quick motion. His lungs were found adhering firmly to the diaphragm by the left lobe. The right auricle of the heart was larger than common; and stuffed with a gross, compact, white substance, like lard. The preternatural state of the auricle was, no doubt, the cause of his swooning, and his languid fluttering pulse, a little before his death. But the Dyspnæa, in my opinion, could scarce be occasioned by it, even in his last illness. A polypus of the heart is much apter to create a palpitation there, than a difficulty of breathing. And much less, I think, ought the pursiveness upon quick motion, when in his usual health, to be imputed to that cause. It remains then only, one would think, to ascribe his
bad respiration to the adhesion of his lungs to the diaphragm *.

Under one head are ranged nine instan-
ces, and briefly rehearsed; in all which it
would appear that adhesions of lungs to
the Pleura were the only preternatural ap-
ppearances, that could account for the Dy-
spnæa, which proved fatal in them; as
no other are noted †.

And to mention only one more, in the
body of a woman, who died in her preg-
nancy, and was asthmatic, the like strong
accretions were found; and both the lobes
of the lungs were of a preternatural col-
our. More is not said. It would appear
that in this case likewise the accretions
were (at least in a great measure) the cause
of the disease ‡.

† Ibid. Observat. 64.
‡ Ibid. Observat. 65.
This is the substance of what I can think of in favour of the doctrine of my ever honoured master: which however I have offered the more diffidently, because it clashes with the sentiments of the other illustrious writer, who is so much my superior; from whose elaborate works I have learned, and daily learn so much; who by his indefatigable and well conducted industry has contributed, and still continues to contribute, towards the advancement of true medicine, at least as much as any one person now living.

I proceed to offer some practical inferences from what hath been said; which was my principal intention in drawing up this short sketch. Supposing then, or taking it for granted, that accretions of the lungs to the Pleura sometimes create or increase Dyspnæa or Asthma, let us first enquire from what signs and appearances it may most probably be concluded that such accretions actually exist in any particular
cular case of *Dyspnoea* or *Asthma* which we may have occasion to treat. I say most probably, for I doubt whether any that are strictly demonstrative can be had. Such an enquiry however is necessary, that the indications of cure may be formed and prosecuted accordingly.

Before I enter upon this disquisition, let me lay it down as a preliminary, that a full, regular, and every way seemingly commendable pulse furnishes no material argument against the existence of such accretions. This is clearly proved by the noted history of a very common disease so accurately described by Boerhaave; where tho' there were great, and close adhesions of the lungs to the *Pleura*, from the top of the *Thorax* to its bottom; tho' there was over and above a huge *fleatomatous* tumor within the cavity of the *Thorax*, weighing near seven pounds, yet all along the pulse appeared completely faultless.
less till a very little before the afflicted nobleman’s death *.

This admonition being premised, if in the case before us no other local cause of Dympnea is clearly pointed out; if that symptom is not sensibly diminished by any of the remedies found most generally effectual for that purpose in common practice; or by any means directed against other particular causes, that may most reasonably be suspected; in this precise situation a circumspect practitioner, who believes that accretions of the lungs to the Pleura can ever hurt respiration, will think of them, were it upon no other account than to form his prognostic. He will, with reason, be the more inclinable to entertain a belief of their existence, if he observes the patient’s respiration to be performed chiefly by the motion of the Thorax, with little or no assistance from the diaphragm, and muscles of the Abdomen:

* Atrocis rarissimique morbi historia altera.

To
To which let me add, if there is a remarkable freedom from cough. This thought was suggested to me by what I observed above thirty years ago in the body of a boy about six or seven years old; in which the lungs were found closely adhering to the Pleura all round. This child died atrophical; and appeared to have no remarkable difficulty of breathing. But he had been confined to his chamber, and stirred very little, long before I was called; and kept his bed mostly while I attended him. But, which appeared to me most extraordinary, he never was observed so much as once to have coughed during his illness. His voice was all along remarkably clear, and even shrill. These particulars I remember very distinctly, as they then made a strong impression upon me, tho' I cannot now so clearly recollect the other circumstances of his case. But that no more stress may be laid upon a story told so imperfectly
perfectly, than it deserves, let us consider the nature of the thing.

Coughing is occasioned by something irritating the lungs, their *Bronchia* and vesicles; the diaphragm, the *Trachea*, and its larger branches; the *Larynx*; and, upon the account of its vicinity, the *Pharynx* also: to which let me add the *Oesophagus*; and perhaps, in some cases, the stomach. It is executed in the following manner. There is first a more than ordinary effort of inspiration made; and particularly the diaphragm is strongly pulled down, pressing the liver and stomach before it, so as to make the *Abdomen* turgid. Upon which quickly succeeds a rapid contraction of the abdominal muscles, forcibly drawing up the *Diaphragm*, and creating a sudden and violent expiration, by which the air is expelled through the *Glottis* with a noise. See Haller’s *Elementa*, Vol. 3. p. 300. Now if the motion of the diaphragm is
remarkably confined by accretions of the lungs to the *Pleura*, as we have endeavoured to explain above, and at the same time there is no extraordinary *Stimulus* acting upon the parts which we have mentioned, there will be a remarkable absence of cough. The execution of that action is then uncommonly difficult. And as it is partly subjected to the will, an uncommon *Stimulus* is requisite to excite it; and that is supposed to be wanting. In the lamentable history above referred to*, there was indeed a cough from first to last, notwithstanding the broad and close adhesions of the lungs to the *Pleura*: but at the same time there was a strong perpetual *Stimulus* to excite it, to wit, the compression of the left lobe of the lungs into so narrow a compass, as to render it incapable either of transmitting blood from the heart, or of being inflated by air. But such a compression is the strongest spur to

* Atroci Samorbi Historia.
urge on a cough that can be conceived. For
the air vessels of that lobe must have been
so strongly squeezed, as to make the insides
of their walls come into mutual contact;
which creates the same sensation and irri-
tation, as if some foreign substance had
got into their cavities; and rouses the whole
muscular power of the Thorax and ab-
dominal system, in order to create a cough,
and shake off the irritating cause.

But supposing that our practitioner is
willing to use some means that may take
off, or at least mitigate the bad effects of
adhesions of the lungs to the Pleura, the
question remains, what means of this kind
can be found? Those authors (and Boer-
haave among the rest) who acknow-
ledge their extreme hurtfulness, declare
the evils which they bring on absolutely
incurable. If therefore I should set about
directing any measures with this view,
the passage in the poet might be applied
to me, only by changing one word,
Avia sanandi peragro loca nullius ante
Trita solo.

And therefore should I miss my road in an unknown country, I might hope for some grains of allowance, some excuse for my error from the equitable and good-natured. Let us then set out.

If there really can be found no practicable method of removing in whole or in part the adhesions or accretions in question, yet perhaps some shift may be made to render their effects not so grievous and intolerable, as they would otherwise be. To give some relief in deplorable circumstances, to gain time, and even to render death easier, is doing no small service to our fellow creatures. And accordingly the great Verulam, in the work he valued himself most upon, warmly exhorts physicians to make it a part of their study how to bring about the Euthanasia exterior.*

* De Augment. scientiar.
In the memorable case before mentioned, Boerhaave directed an incessant application of flannels moistened with warm decoctions of the most emollient vegetables to the ribs, their cartilages and the Sternum; likewise steams of the same to be frequently drawn in by the mouth into the air vessels of the lungs; with an humid lubricating diet. This method alone was attended with such extraordinary success, that all about the noble patient for some time entertained hopes of his recovery. If therefore it was so effectual in relieving oppressed respiration, where there was, besides extensive accretions of the lungs to the Pleura, a tumor weighing near seven pounds within the cavity of the Thorax, surely it cannot miss of being serviceable where such accretions are the sole, or the principal cause of Dyspnæa. By the constant application of warm relaxing moisture, the ribs and Sternum must be rendered more pliable and obedient to the action of the intercostal muscles;
muscles; and the defect of the motion of the diaphragm be thereby, in some measure, supplied. As this method is perfectly safe, it ought always to be tried where other remedies fail. Nor is it improper, whatever the causes of Dyfnæa may be, which is no small recommendation of it.

As this is but a temporary remedy, and probably, unless constantly used, will produce no very lasting good effects, I have been at some pains to think of ways and means that may affect the principal cause, to wit, the accretions themselves; which if they should not be able totally to disengage them, yet may render them looser and more flexible; and so productive of lesser inconveniencies: If this could be effected, it would be more than palliating, it would be substituting a less disease in the room of a greater; and perhaps with a proper regimen enable the patient to hold out years. A remarkable diminution of the Dyfnæa, tho'
tho' falling considerably short of perfect freedom of breathing, may be tolerably comfortable, and perhaps grow better in time. I shall lay before you what occurred to me in this research.

Perusing Dr. Störk's treatises on the virtues of Cicuta, we find that chirruses and cancers, tho' firmly adhering to the ribs and Sternum, so as to seem growing to them, and unmoveable every way, have been often cured by its extract. As the cure goes on, the tumors become more and more moveable; and at length are found perfectly loose and free. Now while they are fixt firmly to the adjacent bones, it would appear there is some kind of adhesion of the membranes at their bottoms to those, which cover the bones, pretty much of the nature of the accretions of lungs to the Pleura: and therefore I should think it highly reasonable to give that extract a fair trial, either by itself, or joined to other resolvent and penetrating
netrating medicines, where it is apprehended that such accretions make a considerable part of the disease.

I am the more inclined to expect relief from this remedy, because I find it observed that there is sometimes a viscid humour about the Pleura and membrane of the lungs, which, it would appear, lays the foundation of the accretions. In a case recorded by Bonetius from Salmuth, to wit, the fourth of the nine already referred to, it is said, "That in the Emperour Ferdinand the lungs adhered to the Pleura by a viscid Pituita, which afterwards became gypsum. And Valcarengus, a celebrated physician of Cremona, in his elaborate observations on epidemical distempers, relates, that upon opening the bodies of many, who died of malignant pleurises then raging, he found the external membrane of the lungs as it were plastered over with a viscid substance, as with a

* Lib. 2. Sect. 1. Observ. 64.
new coat, which was sometimes more than an inch thick. The patients expectorated very little, and breathed with difficulty. Their inspiration was more uneasy than their expiration*. But, as the extract of Cicuta, according to Störk's observations, is found to be the most powerful resolvent of all medicines yet known, may it not be tried in such cases? If the glutinous humour, cementing the accretions, be attenuated and dispersed, may they not be rendered more flexible and yielding, and therefore at least more tolerable, if not completely cured? May not the cohering membranes, made thicker through obstruction, when they are become more permeable, become likewise thinner, and therefore more obsequious to the motion of the diaphragm? But this I only throw out as a hint at present; submitting it to the candid and intelligent.

If I have any thing hereafter worthy of being offered to the public concerning this matter, I shall not fail to impart it.

Give me leave upon this occasion, before I conclude, to offer some thoughts concerning the manner of making this extract. I am thoroughly convinced that the principal reason why it hath often proved ineffectual in our island is because it has been made when the plant has been too young. There can be, I should think, no material difference, in point of medicinal virtue, between the *Cicuta* growing spontaneously in *Germany*, and that found in the fields of *Great Britain*. The latitude of *Vienna* differs but three degrees and some minutes from that of *London*. Great cures have been performed with that extract in *Louvain* and *Brussels*. But these two cities are nearly in the same latitude with *London*. In garden plants, a peculiarity of soil, and manner of cultivation may produce a considerable diversity of virtues in the same species. But, in such as grow wild, the variation
variation of two or three degrees can, in my judgement, have a very inconsiderable effect, if any at all, in a general way.

The plant flowers in July; and its extract is directed by Dr. Störk to be made before the flowers open. But that is no reason why it should be made two or three months sooner. There is a wide difference between stalks and leaves new sprung, and those full grown. The one may be, and in some instances are, a mild agreeable salad, the other a nauseous medicine. I therefore take it for granted that it should not be made (without necessity) before the latter end of May at soonest, as it doth not till then acquire its full virose odour; and therefore is not possessed of its full medical powers.

If it should be then and afterwards too dry to yield juice enough, there can be no harm, (it will even be proper, and necessary) in adding water enough to the stalks and leaves while you are bruising them,
them, in order to get all the good out of the mash. The water will exhale while the extract is making. By finishing the evaporation in the sun, which is then strong, (if the weather be dry) an *Empyreuma* in the preparation will be effectually avoided.

In this manner, about the twentieth of last May, I made, for my own satisfaction, an ounce of it, which perfectly answered the characteristics required by *Störk*; to wit, it was not black and tough, but of a dark, greenish hue, somewhat crumby if dry enough, without *Empyreuma*, but of a very particularly disagreeable smell, nearly resembling that of house-mice.

I remain,

Worthy Sir,

Your most respectful,

Obedient Servant,

Lincoln,
June 15th, 1762.

MALCOLM FLEMYNG.
VINDICATION, &c.

Little expected I should ever be obliged to make any Apology for, or any defence of the foregoing Letter. I thought I had therein expressed my meaning so clearly that it could not be misunderstood, and so cautiously, I might say modestly, that no umbrage could well be taken. And I apprehended some degree of utility might accrue to practitioners in Physic, and through them to society, from my making the essay public. If the utility should be but small, the pamphlet is but short, and can take up very little of the reader's time. And I thought I had some reason for apprehending so, because I found, or fancied I found myself somewhat improved by the pains I had taken in drawing it up. For I am not ashamed, after a more than five and thirty years practice, never unattended with study, to own that I learn daily something in my profession; and hope to do so as long as I exercise it. *Ars longa, vita brevis.*

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But
But quite the reverse hath happened. The Critical Rewiewer for August last, the month in which my pamphlet was first published, after acknowledging "the subject to be of some importance, because it is still matter of debate among the learned," adds, "this is more than we can say of the Doctor's "Pamphlet," which is, in plain English, condemning it as entirely insignificant. And this his sentence he supports solely by confident assertions, without the least detail of reasoning to back them. Whether this be a right critical way of reviewing a literary production; let the unbiaffed judge: I am sure it is not a logical one.

My duty to myself, as well as to the public, which should have no insignificant stuff obtruded on it, compels me, however unwilling, to defend my essay against this redoubted critic in masquerade, this self-elected dictator in medicine, in the best manner I can. In the course of my defence, I shall have opportunities of examining into the truth and consistency of his assertions; and, if he meets with any thing therein harsher than he could wish, let him blame himself for being the aggressor.
Si diēum in Se inclementius

Exiṣtimavit esse, sic exiṣsimet

Responsum, non diēum esse, quia læsit prior.

Who my antagonist is, I neither know, nor desire to know; and therefore I hope nothing in this paper will be constructed as personal: I have only to do with an ideal character.

The Reviewer acknowledges the subject "to be of some importance, because "it is still matter of debate among the "learned." I can help him to another reason, which is a much better one, to wit, because it hath a necessary connexion with the rational practice of physic; as I have clearly shown in page 5th and 6th of the Letter; to which I refer my reader. Learned men have very often had long-winded disputes upon points of little or no consequence: but every thing relating to medical practice, however small in proportion to the whole body of the art, is important, because it concerns the life and health of a rational creature, and in the course of years, may affect an indefinite number of individuals. This consideration, however material, however seriously urged in the Letter, the Reviewer hath thought proper entirely to omit.

But my pamphlet, says he, is altogether unimportant, or insignificant, "be-
cause I have advanced nothing decisive in it." The reader is here earnestly intreated attentively to consider the scope thereof, as declared in my own words: "I only propose in this paper to set forth the arguments on both sides in the fairest and most impartial manner; leaving the reader to judge for himself; and shall conclude with some practical inferences, which may seem fairly deducible from what is previously laid down." It is the duty of a rational physician, who undertakes every case that occurs, to settle this point within his own mind, to wit, whether Adhesions of the Lungs to the Pleura affect Respiration, so as to create or encrease Dyspnoea, or not; because his conduct in the treatment of that symptom must be regulated accordingly; that is, it must be affected differently, as he favours this or that side of the question. There is no neutrality to be observed here; one of the sides he must choose. And therefore, to lay before the reader the arguments pro and con. impartially, must be of some use and importance: and no writer, that I know of, had ever attempted to do this before. Perhaps the dispute cannot be decided with absolute clearness and certainty. But even in that case, the prudent
dent physician will be directed by the higher degree of probability: and not seldom, in difficult and intricate cases, there is no other way but this left to proceed in; which the Reviewer, if he is a practitioner, no doubt knows; so that, even if nothing decisive, (properly and strictly speaking,) had been advanced in my pamphlet, it deserved a better reception at his hands.

But I maintain that there is, at least with respect to our Reviewer, something decisive advanced in it; and shall prove it by his own words. "Haller, says he, "confirms his sentiments, not only by physiological reasonings, but by anatomical remarks, which seem to determine clearly in his favour." So that by something contained in my pamphlet, (the declared purport of which is to state impartially the arguments on both sides) the Reviewer is made a proselyte to Haller's opinion. This is indeed a manifest inconsistency in him, which let him get rid of if he can. However that be, it was ill judged, and unbecoming our vigilant Annalist of Literature, to give his readers so unfavourable an impression of my Letter, as might scare them from purchasing, or even looking into it; and so preclude them from reaping the same benefit
benefit from Haller's arguments he himself had done: especially, as they were published for the first time, only the year before, at Lausanne in Switzerland, and had never been put into English till my pamphlet appeared. And it seems to me highly probable, (I should be sorry however to wrong him) that this was the very first time he saw them himself; home published books being the principal game he flies at: for methinks he would scarcely have taken particular notice of the single paragraph, in which these arguments are all contained, in a quarto volume, of near 500 pages, and so full of various matter, even tho' he had had the curiosity to look into it.

He says, "I have offered nothing but physiological conjecture." What he calls by that name is a set of physiological positions, which both sides are agreed in; which I lay down as principles to reason from; and their immediate consequences. And surely this is a fair way of proceeding, to wit, to draw a conclusion against Haller's opinion, from premises which he himself admits. But these positions are all supported, not only by the closest reasonings, from the manifest structure of the parts concerned in respiration; bulikewise by a variety of observations, and
and experiments made upon living animals. Are such positions, and consequences fairly deduced from them, to be termed only conjectures? This is a specimen of the Reviewer's candour. After such an assertion, I should scarce be surprized if he called the circulation of the blood a conjecture; they being pretty nigh as fully proved as it is, and by arguments of the same kind.

My conjectures, to wit, the positions just now spoke of, and their consequences, he owns, "are supported by ingenious observations on cases deduced from the Sepulchretum of Bonetus." I thank him for his compliment. If they are fair, and not strained, it is enough for me. But there is one part of that work referred to in page 20th, in which nine instances are recorded of fatal Dyspnœa, in all which adhesions of the lungs to the Pleura are the only preternatural appearances, which that symptom can be imputed to, no other being noted. This is a cluster of stubborn facts, which tallying with positions so well founded as mine are, and their consequences, go a great way towards even demonstrating that such adhesions are sometimes the principal cause of Dyspnœa. There is no occasion for ingenuity
unity here; the observation is plain and obvious.

He says, "I (the Letter-writer) am sensible my reasonings from Bonetus do not amount to conviction." This is a misrepresentation: I have nowhere said so, nor any thing from which this may be fairly drawn as a consequence. I say, indeed, "that I have laid before the reader the substance of what occurred to me in defence of Boerhaave's opinion the more diffidently, because it clashed with the sentiments of the other illustrious author." But this is only a decent apology for differing in opinion from so great a man as Haller, and comes with sufficient propriety from one, who only proposes to state the arguments on both sides; whatever his own private opinion may be, and leaves the reader to judge for himself. However, as I find a scrupulous modesty is no recommendation to the Critical Reviewer's good graces, I take this opportunity of declaring that, according to the best of my own judgment, I have, in the Letter I am now defending, said enough to make it appear that Boerhaave's opinion is by far the more probable of the two.

He next adds, "Nor are the practical inferences any more precise and satis-

factory."
"factory." This is roundly asserted; but 'tis our medical dictator that speaks. Upon this occasion let me ask him, is not an assiduous application of fomentations made of the most emollient vegetables to the sternum, the ribs and their cartilages, precisely adapted to give relief in Dyspnœa, upon the supposition that adhesions of the Lungs to the Pleura are its cause, either in whole or in part? And even tho' such adhesions should never affect respiration, yet as this remedy is perfectly safe, may it not, yea ought it not to be tried in every obstinate Dyspnœa, where the means in common practice are ineffectual? (See page 29th.) This is perhaps the most interesting passage of the Letter, as it points out a safe and valuable remedy against Dyspnœa in general, which is seldom or never thought on; and yet our Reviewer passes it over, perhaps that it might not stand in the way of his unbounded and outrageous spirit of censure. However, give me leave now to add, by way of improvement on this most rational measure, that in the intervals between fomenting, a large mucilage plaister, sufficient to cover all the Thorax, may be kept loosely on it, in order to preserve, as much as possible, the flexibility, and easy motion of the parts.
Immediately after he says, "the frequent extract will give a sufficient idea of the whole tendency of the pamphlet, and the instruction which the practitioner may expect from this publication." And quotes from my Letter a long passage, (from page 30th to 34th) in which nothing is contained but a recommendation of the extract of Cicuta, where adhesions of the Lungs to the Pleura are apprehended to be the cause of Dyspnæa. This is an innuendo that the Letter was calculated solely for serving that purpose, which is a very great mistake. I should have published something on the same subject, tho' Dr. Stork had never written on Cicuta. The case of a worthy patient of mine here, was the occasion of my first taking it into close consideration. He laboured under a constant Dyspnæa, of a long standing, with a seemingly good pulse for the most part; and was all along remarkably free from cough. I apprehended, and declared, that adhesions of the Lungs to the Pleura were the principal cause of the Dyspnæa; but an eminent physician in the south, (who was consulted on that occasion) was of opinion that an Hydrops Pectoris caused it; and the principal reason he gave for thinking so was, that he looked upon
upon such adhesions, as I attributed the DySpncœa to, as entirely harmless. This set me to work for my own satisfaction, and the result of my enquiry I have offered to the public. Dr. Stork's observations naturally presented themselves in my way. To affirm, therefore, that the quotation above specified gives a sufficient idea of the whole tendency of the pamphlet is another misrepresentation of the Reviewer's, and groser than the former. What hath been said in the last paragraph proves that the practitioner may reap some instruction from it, independently on the virtues of Cicuta.

He concludes his strictures in the following manner: "What share of additional reputation to Dr. Flemyng may accrue from this seemingly premature epistle, we shall leave to time to determine." This speech at first astonished me, and, for a while, I could not tell what to make of it. At his setting out he had pronounced my Letter altogether unimportant or insignificant; and that absolutely and without reserve. But now, he softens the sentence, and intimates, that it is at least barely possible, that by some event within the womb of time, I might acquire additional reputation, upon the account of having published...
ed this epistle, tho' it be seemingly premature at present. If this be not his meaning, I own I am so dull as not to be able to make any sense at all of it. But before I proceed farther, let me observe, that the Reviewer's first sentence was not only seemingly, but certainly premature, because he changes his mind before he finishes his reflections on the Letter under review; which, setting aside verbal quotations from it, scarce make one page in octavo: but let us go on.

The grand question remaining was, What could that event be? I believe I have at length found it out. If I am mistaken, it is not wilfully, but through his dark and enigmatical, or, which is still more puzzling, his inconsistent manner of writing.

The Critical Reviewer, in his account of Dr. Stork's third and last treatise on the Virtues of the Cicuta, informs his readers, that a society of gentlemen, properly qualified for the purpose, had undertaken to enquire into its effects here at home, in order to determine whether it is as much to be depended upon for the cure of several obstinate and grievous diseases, as Dr. Stork, and other celebrated foreign physicians and surgeons have given out. And he justly waves passing any
any judgment upon that performance, till the result of the enquiry of the society be known. I therefore apprehend, that by calling my Letter seemingly premature, he means its publication might have been better put off till the society had finished their report, as there is mention made in it of the extraordinary resolving powers of Cicuta, and some stress laid thereupon, for the removing or lessening the bad effects of the adhesions I was treating of. Now, even supposing I had known early enough that such an enquiry was set on foot, (and to the best of my remembrance, I did not know of it till after my copy was sent to the printer) I had no reason to delay publishing my Letter upon that account. The testimony of Dr. Stork, and other eminent physicians, among whom is the celebrated Van Swieten, was surely foundation enough for every thing I have said in favour of Cicuta; especially, as I sum up all by declaring, that what I propose is only thrown out as a hint at present, and submitted to the candid and intelligent. To finish properly an enquiry of this kind, must require years; and not a very few, if a negative is to be proved. I might not live long enough to see its conclusion. And there are other material things in my pamphlet,
pamphlet, independent on that enquiry. It was not therefore in the least measure incumbent on me to put off its publication upon that score.

But the Reviewer is not aware, (I still proceed on the supposition I have not mistaken his meaning) that by pronouncing my pamphlet seemingly premature, upon the account just now mentioned, he ipso facto changes sides. He had said before, that Haller's arguments seem clearly to determine in his favour, that no Adhesions of the Lungs to the Pleura, can, in any measure or degree, hurt respiration. *Nibil nocet quidquam (respirationi) ejus visceris (pulmonis) ad pleuram adhesio,* are Haller's words. If this proposition is true, no result of the society's enquiry, be it favourable or unfavourable to the German observations, can ever add any utility to my pamphlet, or reputation to its author; because, should even the resolving virtue of Cicuta be confirmed by their experiments, it can have no object, as far as I am concerned. As the adhesions are entirely harmless, there subsists no curative indication of loosening them to be answered. But if my pamphlet can become in time of any use through the society's labours, that must be by their confirming the resolving virtue
virtue of Cicuta, whereby the adhesions may be removed or relieved, and this necessarily implies their being hurtful, so that the Reviewer, if he will make sense of what he concludes his remarks with, must give up the favourable judgment he had before passed upon Haller's arguments, and admit, that Adhesions of the Lungs to the Pleura are sometimes detrimental to respiration. So consistent is he with himself! so well founded are his magisterial assertions! But however fickle he is in his speculative opinions, he is staunch and true to one practical point, to wit, the running down of my pamphlet. He confutes, changes sides, and still confutes; like the moon, with respect to our earth, tho' he be still turning round, yet he always keeps the same face towards his author.

Having now taken into close consideration every thing the Critical Reviewer hath advanced, in order to disparage, and, if it were in his power, to crush my short pamphlet; and having, I flatter myself, made it incontrovertibly appear, that it is neither so insignificant, as he impotently would represent it, nor premature upon the whole, I shall take my leave of the reader, with certification that whatever may henceforth be published, either against
against the Letter itself, or this defence, shall pass unnoticed by me. I have already bestowed as much time and pains upon my subject, as, I apprehend, so small a particle of medicine may be fairly thought to require, and rather more than my other occupations could well permit.