A

LETTER

FROM

Sir *R*obert—— *S*ibbald——,

TO

Dr. *Archibald Pitcairn.*

EDINBURGH,

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A LETTER FROM Sir *R*—— *S*——, TO Dr. *Archibald Pitcairn*.

SIR,

I Should be thought very ungrateful, if I did not acknowledge the great Obligations I have to you, for the Trouble you have given your ſelf in collecting all the Miſtakes of my *Prodromus*, and inſerting them in that learned and elaborate Piece of yours, *De Legibus Naturalis Hiſtoriæ*; you have done me a very kind and friendly Office, which I do aſſure you I will not forget; and ſince my Obligations to you on that account are now become publick, it is but reasonable my return of Thanks should be so too. I can't follow a better Example than that you have set me; and therefore I have bestow'd some time in considering your Writings, and have sent you here some Remarks I made upon them, as a Token of my Gratitude for the Pains you were pleas'd to take with mine.

You were very kind in advising me to read the Mathematicians; for I must own I have had more assistance from them than I expected, in making the following Observations. Tho' upon a closer Inquiry I find your Acquaintance is not very intimate with them, yet you every where talk of them as your particular Friends; and profess your self so great an Admirer of their way of speaking, that you chuse to make use of their Language, even where your Sense wou'd be much clearer without it. For example in the 7th Page of your Book, *De Legibus*, you say, *That the most learned* Robert Sibbald, *proposed to himself this Problem to be solved in these words*; and then you quote from my Book, what I designed to do in my Natural History. An ordinary Man would have said, Sir *Robert Sibbald*, undertook to write a Natural History, which you call *Proposing to himself a Problem to be solved.*

In the foregoing Page you fall foul on the Anatomists and me, for talking of the Triangular Figure of the Heart, for say you with a demonstrative Air, in a very Universal Proposition. *Every Body must have more than three Angles.* Pray how many Angles has a Sphere? Or is a Sphere not a Body? I think in a Cone you'll have a difficulty of finding above one Angle; and that too will ſcarcely be allowed to be an Angle in the ſtrict ſenſe of the Word, as it is defined by *Euclid*, and yet both he and *Archimedes* ſpeak of Rectangular Acute angular and Obtuſe-angular Cones. But I ſuppoſe you reſtrain your Univerſal Propoſition to ſuch Bodies only as are terminated by Planes; and then your meaning is, that all Bodies except Spheres, Spheroids, Cones, Cylinders, and innumerable others muſt have more than three Angles. Since then your Rule admits of Infinite Exceptions, May not the Heart be excluded from it likewiſe? I am ſure it is not contained under plane Surfaces. Now becauſe the Figure that repreſents the Heart when projected, or delineated on a Plane, was a Triangle, the Anatomiſts thought from thence they might take the liberty of calling the Heart a Triangular Body. Juſt as *Archimedes* denominates a Rectangular Cone from the Species of the Triangle that is formed by its Section with a Plane through its *Axis*.

In Page 8th you ſay, *You undertake this Examination of my Book, to let your Countrymen ſee the way (if any of them in the times to come ſhou'd attempt ſuch a Work) of writing a Natural Hiſtory, and likewiſe at the ſame time to ſhew, that they are the beſt Friends to their Country, who are the greateſt Mathematicians.* How theſe two things can be ſhewn together, or what connection there is between them, I proteſt I don't underſtand; I think it would be as eaſy to ſhew from the true Method of Writing a Natural Hiſtory, that he who is the beſt Shoemaker or Weaver is the greateſt Friend to his Country, as from thence to prove, that the beſt Mathematician muſt needs be ſuch.

In Page 40, you gave me a briſk Attack, for affirming the Earth to be the *dryer*, groſſer, and ſolider Globe of the Univerſe, which you ſay is very falſe; and to prove your Aſſertion, you quote a Theorem from *Newton*'s Principles, which you ſay, in a very obliging manner, you'l tranſcribe on my account.

Thus you tranſcribe it with ſome Variations, *The Denſities (that is as you ex-plain it, the dryneſs, groſſneſs and ſolidity) of the Planets are as their diſtances multiplied into the Roots of their Apparent Diameters.* I never obſerved that *Denſitas* ſignified *Dryneſs*, or that *Denſus* was *dry* before. In *Virgil* I have read,

——— *Denſiſſimus imber.*

But I did not apprehend it ſignify'd a *dry* Shower of Rain. By this new way of Interpreting, *wet* and green Wood muſt needs be *dryer* than old and ſeaſoned Timber, becauſe it is much *denſer*. But you took the opportunity, I ſuppoſe, of letting us know you had got ſo far as the 8th Propoſition, and its Corollaries of the 3d Book of that Great Man's *Principia Mathematica*. But had you gone ſo far, as to underſtand the firſt definition of the firſt Book of the *Principia*, you could never have taken *Denſitas* in the ſenſe you do here. It happened a little unluckily, that in the Theorem, as it is printed, there is an error of the Preſs, the word *reciproce* being left out, tho' inſerted afterwards in the Demonſtration, which makes the ſenſe quite contrary to what it is, as you quote it. One would not have thought, that you who have alter'd some of the Expressions, would have left out the most material word of all, on which the Sense and Truth of the Theorem depends, had you that deep penetration in the Mathematicks, which you always take pains to make the Reader believe you have.

But my Prodromus provokes you so much, that you are resolved to give it no Quarter; sometimes you quarrel with it for a Trifle, and sometimes you let your Fury loose upon it, when 'tis in no fault at all. I happen'd to say, that *it had been observed at* Edinburgh, *whilst the Wind was North, that the Mercury had risen almost to the top of the Brass-plate.* This seems to you to be a strange piece of negligence, or want of exactness in a Writer of a Natural History; for say you, I ought to have told how high the Brass Plates were, and how much *Edinburgh* was higher than *Leith*, or the Surface of the Sea. I wonder you did not add, whether the Observation was made in a Cellar or in the 14th Story, which is as material a point as either of the other two. Who is there that has seen a Weather-glass, so ignorant as not to know that the Divisions on the Brass Plates, begin generally at the 28th Inch and reach to the 31st from the surface of the Mercury. And when I said that the Mercury rose almost to the top of the Brass Plate, it might have been from thence concluded, without the help of much Geometry, that the Mercury was near 31 inches high. Perhaps you went out of the way to do a good natur'd Office to a Friend, and tell the World, That one *George Sinclair, no ill Man*, makes very good Weather Glasses at *Leeth*: But sure 'twas an odd way to get your Friend Cuſtomers, by inſerting an Advertiſemement in the middle of a Book, which was never to appear in a publick manner.

I have a far better opinion of my Peformance in Natural Hiſtory, that I find you thus furiouſly fall foul on Dr. *Cockburn*, a Perſon whoſe diſtant Abode might have been ſome ſecurity from your Malice; if his good eſteem for *Bellini*, and his too good Services for that way of Phyſick had not prov'd a juſt ground of Quarrel to one of your Temper. His diſcreet Method of recommending * *Bellini*, a quality no Body could ever accuſe you of, has prov'd very effe-
7. ctual, and the Doctor is not aſham'd to praiſe him more than † himſelf, even when he is a mend-
2. ing him; nay, he ſays he deſpairs of ever attaining his elegance of Expreſſion, while he is reducing his uſeful Speculations about Bleeding into Practice, a method you wanted to learn of *Bellini*, and he attempted to teach, but in vain. How much like a Gentleman does he deal by *Bellini*, while he corrects his Doctrine of depreſs'd Pulſes: This he does in many other Particulars.

'Twas well for Dr. *Cockburn*, that this your Scandal was publiſh'd before his Problem, or he ſhould not have eſcap'd ſo well. What an indignity was it for him to put a queſtion to Dr. *Pitcairn*, or to undertake to ſolve a Problem, your peculiar Work: But was you not more highly affronted that he really ſolv'd it, after *you took it for a Banter, and an Undertaking too great for the Capacity of a Man*, and that becauſe it exceeded your own. I do not find your great in-

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duftry has yet found any fault in it; but this is fuch an unpardonable Crime in him, that he certainly ftole it from your accurate inaugural Oration, or your pious Letter to King *Gelo*; and for this you have, no doubt, a demonftration.

On the other hand, if your Inventious and Demonftrations, here adduc'd, are duly confider'd, it muft be granted your explanations of *Bellini* are fo fhort of that Author's Senfe, and your own pretended Difcoveries fo trifling, that any one would be ready to think you the very *Tonie* he complains loudly of, for defacing his Works by fuch like Labours as yours are.

'Tis very true, Dr. *Cockburn*, as many more young Phyficians, has had a favourable opinion of your underftanding fomething of *Bellini*, and fome other Writers of Phyfick; nay, has even call'd you the great † Improver of our Northern Phyfick, a Character too low for you, and therefore but juftly merits your Difpleafure at this time, as even *Bellini* has his times of favour with you. The Doctor therefore prefum'd to ufe you as he has *Hippocrates*, *Sanctorius* and *Bellini*; but now this caufe of Anger is remov'd; for he found his Error about the Scurvy, and is gone the quite contrary way on his own Strength, very much to the greater Satisfaction of the World, and I hope your Ingratitude will correct his Complements he has too freely beftow'd on you.

† *Prefac* Alvi P fluvia.

To make an end of this part of your Scandal; will it now be imagin'd that you can turn Pilferer from Dr. *Cockburn*, and that in the moft arrogant and open manner. Let your own Words gain credit to fo ftrange a thing, *Cantharides Chymice*

tractaræ

tractatæ exhibuerunt mihi Spiritus ipso Spiritu Cornucervi magis alcalici, &c. The whole of this Experiment was writ first by Dr. *Cockburn* in *Phil. Transf.* No 252. pag. 161. and that two Years at least before the World was blest with that other choice Piece of yours, *De Opera quam præstant corpora acida vel, &c.* Nay, I am sure too you read it the very Summer the Transaction was publish'd. This is the highest impudence to assume to your own very self what is already publish'd in so known a Paper. This is far worse than collecting the Writings of Optical Authors, in a Stile not barbarous, for equipping your dainty Inaugural Oration; and yet to be fobb'd upon Posterity for your own.

But to leave the Prodromus, let us take a view of your choice Performances: In Page 28 of your Dissertations you set about to find the Curve made by the Section of a distractile Canal with a plane Perpendicular to its Axis: For to discover the Property of this Curve, *you represent it by an infinite Number of small Right Lines, on all which there fall Perpendiculars of equal lengths, which you say will represent the lateral Pressure of the Fluid: But these Perpendiculars (since they are inclined to one another) will necessarily meet; wherefore the Question is brought to this, To find out the Curve whose Subtangents do all meet in one Point.* This Curve, you say, *the Geometers know to be a circle, and therefore the Fluids left to themselves, will necessarily form such a Canal whose Sections Perpendicular to its Axis are Circles.* I am not here to dispute with you, whether the Sections of the Blood Vessels be Circles; that they are

so,

ſo, I freely own, it being a common obſervation. But the Queſtion is whether you have proved that they muſt needs be ſo; and firſt, Sir, tho' I have read ſome Geometry, I muſt confeſs I don't know what is meaned by the Sub-tangents of a Curve meeting in a Point. The Sub-tangents in all Curves is the ſame by Poſition with the Axis, and it is not eaſy to underſtand how Lines that coincide, or are the ſame by Poſition, can be ſaid to meet in a Point: But I ſuppoſe you mean that the Perpendiculars to the Curve, and not the Subtangents, meet in a Point; in which caſe 'tis true the Curve is a Circle. But then you ought to be a little more careful in your Expreſſion than I find you are. However, your way of reaſoning is far from being good: For tho' you have proved that the Perpendiculars will meet, it does not from thence follow that they muſt all meet in one and the ſame Point, and unleſs this be proved, you cannot demonſtrate the Section to be a Circle. You ſeem indeed to aſſume, without a demonſtration, that Perpendiculars can fall from one Point on all the parts of the Curve, and that they are of equal lengths: But is not this to aſſume the very thing you would demonſtrate? And is it not juſt ſuch reaſoning as you have in your Propoſition about the encreaſe of Blood? Then next you aſſume, that equal parts of theſe Perpendiculars will repreſent the Preſſures on the ſides of the Canal; that is, you aſſume the lateral Preſſures to be every where equal: But how is that to be proved? I am ſure it is not true, if the Axis of the Canal lye in a Horizontal Poſition, and the Fluid be left to it ſelf; that is, if it have no Preſſure but what a-

riſes

rises from its Gravitation, as you suppose in your demonstration; for in that case the Pressures will be always proportional to the height of the incumbent Fluid, and the Curve then will not be a Circle, but another of a different kind, whose Nature is * determin'd by the Geometers. You see, Sir, how many faults you have committed in arguing on an easy Subject. However, it may be easily demonstrated, that in a distractile Canal, all whose parts are equally flexible, if the Fluid be not left to its self, but be pressed forward by an external force; or if they are pressed one upon another with a force vastly greater than that of their own Gravity, the Section will be a Circle, and this is the case of the Blood Vessels.

* *Vide* ...ta Eru...orum ...siæ.

In the 17th Paragraph of the Dissertation, you have another Demonstration which ought to be consider'd. It is to prove, that between the Evanescent Artery and the growing Vein, there can be no Space, Body or Interstice into which the Mouths of the Veins and Arteries open; but both these Canals must make one continued Duct: For, say you, *if there were any such space, the Blood within it pressing every way, would much easier compress the sides of the Membranes that form the Mouth of the Vein, and make them touch one another than enter through that Orifice into the Vein, and in that case the the Blood would not return to the heart by the Veins.*

I like a Demonstration that disproves Matter of Fact; but notwithstanding your Demonstration, that it is impossible, we are certain that there are Veins and Arteries that are not immediately conjoin'd, but there is a *large Space, Body or Interstice* between them. Perhaps you'll

wonder

wonder at this Aſſertion; but I hope to make it plainer and eaſier to be believed than you have made your Demonſtration, that it is impoſſible: I ſhall inſtance in the Spleen, to which there goes a Branch from the Cœliack Artery that divides it ſelf into ſmall Capillaries, or Evaneſcent Arteries. Theſe enter the Spleen, and are inoſculated into its Subſtance, without being immediately joyn'd to any Vein. And juſt ſo there ariſes from the Spleen the Splenick Branch of the *Vena Porta*, which is not joyn'd to any Artery, and this carries the Blood from the Spleen to the Liver. Now for all your Demonſtration, every Anatomiſt believes that the Blood paſſes from the Branch of the Cœliack Artery into the Splenick Vein through the Body of the Spleen, and that it ſtill moves on the preſſure of the Blood within, the Spleen no ways hindring it. This I think is a convincing Proof that your Argument is not good; let us now ſee where the Fallacy lies.

'Tis ſomething ſurprizing to obſerve the different caſts of Mens Heads, I ſhould have been apt to have reaſon'd from the Preſſure of the Blood juſt the contrary way, and ſhould have thought that the Orifices of the Veins would become larger and not quite ſhut up by the Preſſure of the Veins; for the Blood by its Preſſure would endeavour to extend the whole Surface of the Body that contains it, by which means the parts would be further removed from one another, or which is the ſame thing, the Pores would become larger. And then I would have conſider'd the Orifice of the Veins but as large Pores, which

would

would therefore be inlarged in proportion to the reſt of the Surface: Thus I would have reaſoned.

But you may think this too obvious a way of arguing, and therefore by a peculiar Art of reaſoning, have turned the Argument the quite contrary way, and have made the Blood to ſhut up the Orifice of the Vein, which is not to be done without ſuppoſing the Vein to enter for ſome way within the intermediate Body, and to hang looſe within its ſubſtance. But this is a Suppoſition, that I believe no Body will make beſides your ſelf, and you would do it only to make way for your demonſtration againſt it. But after all, I don't here diſpute the truth of your Concluſion with you; for I believe as you do, that the Capillary Veins and Arteries, except in this caſe of the Spleen, are immediately joyned together, and that they form one continued Duct. For I ſee neither any neceſſity, obſervation or uſe, for admitting an interſtice between them; yet this is more than you have prov'd.

In the ſame Diſſertation you give us an Hypotheſis for explaining the Phenomena of Secretions, which you think muſt needs be true, becauſe of its ſimplicity; I am of your Opinion, the more ſimple the better, provided it will ſuit the purpoſe; but otherwiſe, if it is not found ſufficient to explain the Phenomena of Secretions, it may be thought ſimple in another ſenſe than you mean. You divide the Glands into ſuch as are Conglomerate, as the Liver and Kidneys, and into Conglobat Glands, as the Miliary Glands of the Skin; the firſt carry out the thick Fluids from the Blood, and the other the finer parts of it; and the Orifices of the Secerning Ducts of the firſt,

muſt

muſt be larger than the Orifices of the Secerning Ducts of the ſecond kind; ſo that according to you, all the difference of the Glands ariſes from the different bigneſs of the Secerning Ducts, which go from the ſmall Arteries; and therefore all Fluids will paſs through theſe Ducts, if they be large enough. But they differing in Magnitude, ſome of them will admit only thin Liquors, and deny a Paſſage to theſe that are of a thicker kind, whereas other Ducts that have larger Orifices, will admit thicker Fluids; *that is, the finer mixed with other that are not ſo fine:* But if this were all that is needful to explain Secretion, there could be no Secretion of any ſimple Fluid from the Blood, beſides that which conſiſts of the fineſt Parts, and this you ſeem to allow to be true; I ſay likewiſe, that there can be no Secretion of the groſſeſt Fluid, whoſe Parts compoſe the Blood. For its Particles having the largeſt Diameters, whatever Orifice admits them, will admit all the reſt of the Particles of the Blood, whoſe Diameters are leſs, conſequently they entering with the largeſt, there can be no ſeparation; and the Fluid Secerned, will be of the ſame Nature with the reſt of the Blood. Then next for the Fluids that are of a mean thickneſs between the greateſt and the leaſt, they muſt conſiſt of all the Particles that are in the Blood, whoſe Diameters are leſs than that of the Orifice of the Secerning Duct, and they will be mixed in the ſame Proportion that they were in the Blood before Secretion. For the Blood being every where uniformly mixed, all the different ſorts of Particles will arive at the Mouths of the Secerning Ducts, in the ſame Proportion that they have to one another in the Blood before

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Secretion, and all enter, whose Diameters are less than the Diameter of the Secreting Ducts; therefore the different Particles that compose the Secreted Fluids, must have the same proportion to one another as they had in the Blood before Secretion. So that if the thin and serous parts of the Blood be greater in Quantity than any of the rest, the Quantity likewise of the Secreted Liquor must be greater than of any of the rest of the Particles; and because in these Secretions, only the thicker parts are excluded, and all the thinner admitted, the Liquor composed of such Particles must always be thinner than the Blood. Whence it necessarily follows, that there can be no Liquor secreted from the Blood this way, but what must be thinner than the Blood it self. But there are several Liquors secreted that are thicker than the Blood. Therefore it is plain that your Hypothesis is not sufficient to explain Secretions.

If I were as confident on this occasion, as you are on all occasions, I would call this a Demonstration; but I will say nothing more of it, but leave it to you to consider it; and if you find that your Hypothesis is not satisfying, I desire you to think of another that will answer all the Phenomena of Secretions. I assure you the thing may be done.

Before I leave this Dissertation, I cannot but take notice of a slip you have made. It is the 20th §. where before a Proposition you premise a Supposition, that two Secerning Ducts are of equal Orifices, and in the Proposition it self you suppose them to be unequal, *the quantity of Secretions being, as you say, proportional to the Orifices, there being nothing else to make the difference.*

Difference. You have ſtrange ways with you, ſometimes you give us a Suppoſition in your Propoſition, which is the very ſame with your Propoſition: At another time you premiſe a Suppoſition, and immediately ſuppreſs the contradiction of it in your Propoſition.

Your next Diſſertation is taken up in explaining the effect of the Air on the Blood in the Lungs, where you are poſitive, that the Air, which enters the Lungs in Reſpiration, does not mix with the Blood. But notwithſtanding your Confidence, I believe there may be ſuch Arguments alledged for its entering the Blood Veſſels, as you will not eaſily anſwer. I will here take your method, and lay down ſome Phenomena, or Experiments, which ought to be conſidered before this Matter can be determined.

And Firſt, It is known that Air will paſs thro' much thicker and cloſer Bodies than the Blood-Veſſels in the Lungs. To prove which, I will give you the following Experiment. There was a long Tube made of the very thickeſt part of an Oxes Hide, with a deſign that a Diver under Water might, by the help of this Tube, have a Communication with the open Air; the Tube was well pitched all over, and cloſely twiſted round with Packthread, which was likewiſe Pitched, and over that there came ſeveral Folds of the Inteſtines of a Sheep, each of which were covered with Packthread and pitched, the lower end of this Tube was immerſed about ten Fathom under Water, the upper reached above the Surface of the Water, and then the Air in it being preſs'd by the weight of the incumbent Water, made its way through all these thick Substances and came up, raising a large Foam on the Surface of the Water; since therefore the Air being compressed, forced its way thro' so strong a resistance as this Tube must needs have, it may easily be allowed, that a pressure, tho' much smaller, yet may have some effect in making the finer parts of the Air pass through the thin Coats of the small Blood-Vessels.

Perhaps you'll say, that the Air in the Lungs has an open and free Passage through the *Aspera Arteria*, and therefore none of it will go thro' the resisting Coats of the Blood-Vessels. But pray consider this a little better: Look over your Staticks, and reflect on the nature of Fluids. You know when they are pressed, they endeavour to recede by all ways from the pressure, and by that means will endeavour to get through the sides of the containing Vessel, as well as through any open Orifice in it. You own the pressure of the Air to be so great, as to comminute the Particles of the Blood, and dissolve their Cohesions. Now if the Sum of all these Pressures, on the sides of the Blood-Vessels, be any thing greater than the Sum of all the Resistances that the sides of the Blood-Vessels have to the entrance of the Air, in that case there will always some Air get into the Blood. To make this matter as clear as we can, let us bring it into Numbers. Suppose the pressure the Air suffers at an Expiration, be to the resistance of the Coats of the Blood-Vessels, as 1000 to 999; then in that case, if the Air that is forced out of the Lungs, at an Exspiration, be divided into 1000 parts, one of these will pass into the Blood, and the other 999 will go out by the *Aspera Arteria*.

You know that All Liquors have a Facility to admit the Air into their Interstices, there being none of them without a good Quantity of it, which easily discovers it self by the Air Pump; and if this Air, by long Pumping, be taken out of any Liquor, yet if the Liquor be afterwards exposed to the Common Air, it will be found in a little time to abound with Particles of Air as much as ever. By which 'tis plain, that the Air has a tendency or *Nisus* to insert it self into the Pores of Liquors; and since the Particles of the Blood are divided in the Lungs, and further removed from one another, why may it not be allowed, that the Air being pressed may get in between them. Next let me give you a Phenomenon, by which it may be proved, that the Air when pressed, does actually make its way through the Pores of the Membranes into all the Cavities of the Body, which have no communication with the external Air. You know, if a Diving Bell be sunk Thirty four Foot, the Air within it is compressed into half the space it had before, and its Pressure and Elasticity will be twice as great as that of the external Air. If the Diving Bell be sunk Sixty eight Foot under Water, the Air within it will take up only one third of the space of it before, and it will have three times a greater Pressure or Elasticity to expand it self. If it is One hundred and two Foot under Water, the Pressure of the Air within it will be four times greater.

Imagine now a Man to descend in the Diving Bell Sixty eight Foot under Water, which has frequently been done, I say the Air as it is comprest by degrees in descending, must make its way into all the Cavities of the Body which have no communication with the open Air: For if the Air in the Bell had no admiſſion into the Cavity of the Thorax, the Air that does there ſurround the Lungs could by no means reſiſt the Preſſure of the Air in the Bell, which is three times greater than that of the Air within the Body, which endeavours to expand this Cavity, and by ſuch a prodigious over-plus of Preſſure the whole Ribs and parts of the Body containing this Air, would be diſlocated, or the ſubſtance of the Lungs torn to pieces, to make way for the exteral Air to enter the Cavity. For to ſhow this by numbers, The Preſſure of the Air that ſurrounds the Lungs within the Body, having the ſame force with that of the external Air on the Surface of the Water, is equal to the weight of a Cylinder of Quickſilver Thirty Inches high, and whoſe baſe is equal to the ſurface of the Cavity. The Preſſure of the Air within the Bell on the ſame Surface, being three times greater, muſt be equal to the weight of a Cylinder of Mercury Ninety Inches. Since therefore the ſides of this Cavity are extended outwards with a force equal to the weight of a Cylinder of Mercury Thirty Inches high, and they are likewiſe compreſſed inwards by a force equal to the weight of a Cylinder of Mercury Ninety Inches high, the ſides will really be preſſed inwards by a force which is equal to the weight of a Cylinder of Quickſilver Sixty Inches high, whoſe Baſe is equal to the Surface of the Cavity. Let us ſuppoſe that Surface to be equal but to one Square Foot (tho' it is certainly much greater) that is, to 144 Square Inches, and this Cylinder will be

8640 Cubical Inches, whoſe weight is above 5000 Pound Troy. Since therefore a Man can deſcend eaſily in a Diving Bell, and feel no ſuch exceeding great Preſſure, it is plain that the Air muſt force its way into all the Cavities, and ſo make an *Equilibrium* with the Air that ſurrounds his Body.

It is neceſſary that they who deſcend in the Diving Bell to any great depth, do it ſlowly and by degrees, that the Air may have time to enter faſt enough into the Cavities and Blood-Veſſels; for otherwiſe it is obſerved that they feel a great preſſure and uneaſineſs, and the Blood being ſtrongly compreſt by ſo a great a weight, burſts out at Noſe and Ears: But when they go down ſlowly enough, they remain under Water as eaſy as in the open Air.

There is another Phenomenon, which inclines me to believe that the Air mixes with the Blood; and it is this, We find that Animals are frequently kill'd by infectious Steams and Effluviums which they draw in with their Breath; thus, there is a Pit near *Naples*, in which if a Dog be let down but for a little way, he dyes. This is not to be accounted for, unleſs the Air that carries theſe Steams be ſuppoſed to mix with the Blood. This Objection you your ſelf mention, and endeavour to anſwer it by ſaying, That it ought to be proved that Reſpiration cannot be ſtopped by theſe infectious Steams, unleſs they enter the Blood Veſſels, which you have never ſeen proved, nor ſeen any reaſon why when they are mixed with the Blood, they ſhould cauſe a ſudden Death. You know, you ſay, that theſe Steams are always accompanied with a greater or leſs Gravitation of the Air, from which alone ariſes the hindrance of Reſpiration.

Is it poſſible any Man can conceive that the ſmall Effluviums of Bodies ſhould have ſuch an effect on the Blood and Spirits, without being nearer them than the thickneſs of the Coats of the Veſſels. Your Notion about the greater or leſs Gravitation of the Air, is no ways a ſufficent Anſwer: For it is certain that an Animal can live where there is much greater changes of Air as to Gravity and Levity, than what is produced by theſe Steams; and therefore your Anſwer is nothing to the purpoſe. But you deſire to know the reaſon why theſe Steams when mix'd with the Blood ſhould bring an immediate Death, I muſt confeſs I am none of theſe Philoſophers that think themſelves obliged to give reaſons for every thing, I know there are infinite numbers of effects in Nature produced by Cauſes, the manner of whoſe Operations is yet undiſcover'd: However, if you require my Opinion, as to this Matter, I will freely give it you. Phyſicians know that there is a great variety of Bodies, which when mixed with the Blood, produce very ſtrange and wonderful Changes in it. Some ſort intirely diſſolve the texture of it, others only diminiſh and leſſen the degrees of Coheſion of the Particles, and make the Blood extreamly fluid; but there are other Bodies, and the many in number, that immediately coagulate the Blood, and make it, inſtead of a fluid, a very hard and cohering Subſtance. Now if you'l ſuppoſe the Infectious Effluvia which enter the Blood with the Air, ſhould be of ſuch a

nature

nature as to produce a ſtrong coagulation in the Blood, with which they mix, the Blood in the Capillaries of the Lungs becoming a hard and cohering Subſtance will obſtruct all the Paſſages by them into the Veins, and intirely ſtop the circulation of the Blood, for which reaſon the Animal muſt dye. And this I think to be a plain and eaſy account why ſuch Steams are the cauſe of ſudden Death.

I will give you one Phenomenon more, that brings along with it a very good Argument to prove, that the Air conduces ſome other way than by its Gravity towards the conſervation of Life, and 'tis this; An Animal when ſhut up in a cloſe Place with an Air of the ſame Denſity and Gravity with the external Air, it commonly breathes, will dye in a very little time: But if there be forced into the ſame Place, when the Animal is almoſt expiring, more Air without letting out any of what was there before, the Animal will become thereby much exhilerated. I know you ſay the Death of an Animal proceeds from the greater Preſſure and Elaſticity of the Air, ocaſioned by the Heat of its Body: But I believe this increaſe of Elaſticity can never be ſo conſiderable, as to produce ſuch an effect, ſince the heat of the Veſſel is not diſcover'd to be very great; and I think I have ſhowed that an Animal may eaſily live where the Preſſure of the Air is triple, whereas the Preſſure that ariſes from the Air being heated, cannot be above one Tenth of the whole, and therefore it is plain that ſuch a ſmall Change as this can never be the cauſe of the Animal's Death. Beſides, when the Preſſure becomes greater by the intruſion of freſh Air, the Animal is ſo far from dying the ſooner for it, that it is much relieved thereby.

It ſeems to me that this proceeds from ſomething in the Air that is abſorbed by the Blood, and when that Matter is all ſpent, as it muſt needs be by frequent Reſpiration of the ſame Air, that Air will be unfit for the further conſervation of Life, whereas when new Air is admitted, there will be more of this Matter, which ſome way contributes to the Performance of the Vital Functions. What this Matter is, or how it operates, I will not take upon me to determine, I muſt confeſs I don't know, nor did I ever ſee any thing ſatisfactory written on that Subject. Perhaps if Philoſophy be advanced as much in after Ages, as it has been in this preſent, this will come to be found out; but at preſent I ſee no way of diſcovering it.

But ſtill you ſay it is very evident that the Air does not enter the Blood Veſſels in the Lungs. But becauſe the reaſon you give for it cannot be expreſt in *Latin* or *Engliſh*, I muſt ſet it down in your own Words as they are in §. 12. Page 48. of your Diſſertations, *Ex hiſce Perſpicuum eſt aerem vaſa Sanguifera Pulmonis nou ſubire cum eandem poſt mortem Auimalis os non obturati mole inveniatur in vaſe cui incluſum eſt.* It ſeems there is ſomething very evident here, but what it is I know not. I can ſee nothing very conſpicuous but the Intricacy of the Sentence. When I read the Sentence, I concluded that there was an error in the Printing, and turning to the *Errata*, I found it ſo: But now it is ſet right, it is ſtill as unintelligible to me as ever, and cannot I gueſs what you would have.

What

What you have here ſaid, you call a Phenomenon, and you take upon you to explain the reaſon of it. In other Authors, that write more clearly, we can find out what they would prove, from the Reaſons they give: But it is my unhappineſs here, as neither to find the Reaſons nor the thing to be explain'd by them.

Tho' in this Diſſertation you have propoſed the Solution of *Harvey's* Problem; yet I cannot ſee you have ſatisfy'd all the Difficulties that occur in this Matter; for I deſire to know how it comes, that an Animal breathes when it firſt comes into the World. You ſay the Air ruſhes into the Lungs, being preſſed by its Gravity and Elaſtick Force, as into a Place that does not reſiſt its entrance, and that this is done before any dilatation of the Thorax. But pray conſider this a little better, I think from what I have already ſaid, that all the Cavities of an Animal's Body muſt be conſtantly filled with Air, and even the Cavities that are in the Body of the Fœtus, whilſt in the Womb, muſt have Air in them of the ſame Denſity with the Ambient Air, and therefore when a Fœtus comes into the World, and is taken out of the Teguments that involv'd it in the Womb, there muſt be Air in the Cavity of its Thorax, which being endowed with the ſame Gravity and Elaſticity, as the external Air, will reſiſt the admiſſion of more Air; and 'tis the ſame caſe in Animals that live in the open Air; for it will not enter the Lungs unleſs the Cavity of the Breaſt be firſt expanded. I think therefore it is plain, that when a Fœtus comes into the World, that the Air will not ruſh into the Lungs, unleſs the Thorax be firſt dilated, which is contrary to your Aſſertion; ſince then there can be no admiſſion of the Air into the Lungs, unleſs there be a precedent Dilatation of the Breaſt; that is, unleſs the Muſcles of the Breaſt act and enlarge its Cavity. I would know why theſe Muſcles ſhould juſt act then and never before. There are ſeveral other Difficulties that occur in this Matter; but I will not trouble you with them at preſent.

In Page 57. you undertake to anſwer ſome Difficulties, as how ſome ſort of Animals that ſeem to be dead all the Winter, have yet Life and Reſpiration; and how it comes that in ſome Diſeaſes a Man ſeems to have loſt both Pulſe and Reſpiration, and yet is alive; and you ſay both theſe Phenomena's may be eaſily accounted for from this Obſervation.

The Breaſt of a Man you take to be a Spheroid, whoſe leaſt Diameter in thoſe that are come to their full growth, is about Fifteen inches. Whilſt the Breaſt is dilated, the leſſer Diameter is encreaſed, and the bigger is not diminiſhed, and by that means the Cavity of the Breaſt becomes bigger. Suppoſe the encreaſe then from the Back Bone to the Sternum to be only the $\frac{1}{100}$ *of an inch, and then the encreaſe of the Cavity will be* 31 *inches, and ſo much Air will enter the Lungs when its Diameter is only encreaſed* $\frac{1}{100}$ *of an inch: But if the encreaſe of the Diameter be* $\frac{1}{50}$ *of an inch, the quantity of Air drawn into the Lungs will be Sixty two inches.*

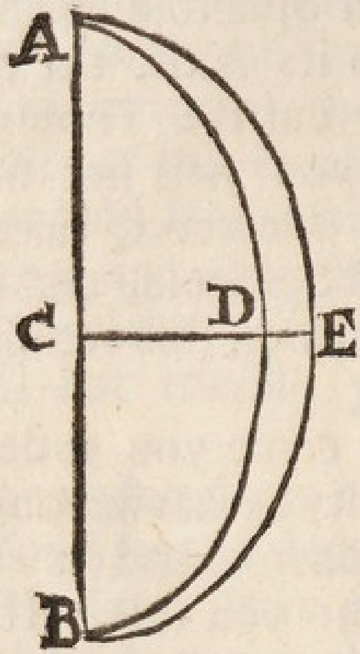
How the Breaſt comes to be a Spheroid, I can't conceive. I have ſome reaſon to believe that it is not; becauſe *Archimedes* has demon-

ſtrated

ſtrated, that if a Spheroid be cut with a Plane Perpendicular to its Axis, the Section muſt be a Circle. Now cut the Thorax with a Plane as you pleaſe, you will not find the Section to be a Circle. However, ſince you will have the Breaſt to be a Spheroid, and you are to ſhow your Mathematicks on this occaſion, I will allow that it is one.

But then how come you to determine the encreaſe of the Cavity by having only one of the Diameters of the Spheroid and its encreaſe. What *Archimedes* taught you this? If it was any, it muſt be he that wrote the Latin Letter about the Trinity to King *Gelo*. I am ſure you might have learn'd from the true *Archimedes*, that wrote about Spheriods, that this Problem was undetermin'd, and that having one of the Diameters, and its encreaſe, the encreaſe of the Spheriod might be what you pleaſe, if the length of its Axis is not given.

What Method you took to find out the encreaſe of the Cavity of the Spheroid from the encreaſe of its Diameter, I will not take upon me to diſcover; for that is as undeterminable as your Problem. However, allowing that you have rightly aſſign'd the encreaſe of the Spheroid from the encreaſe of its Diameter, I will undertake from thence to find out the Solid Content of the Spheroid, and the length of its Axis; for that becomes a determined Problem from the Data you give. You know that it may be eaſily demonſtrated from what *Archimedes* has ſhow'd in his Book about Spheroids, that all Spheroids are two Thirds of the Cylinder circumſcribed, which has the ſame Axis with the



the Spheroid: And therefore if there be two Spheroids generated by the rotation of two Semi-Ellipſes ADB AEB about the ſame Axis AB, they will have the ſame Proportion to one another, as the Cylinder circumſcribed. Or which comes to the ſame thing, if the Spheroid ADB comes to be dilated ſo much as to fill the ſpace of the Spheroid AEB the encreaſed Spheroid will be to the Spheroid before it was encreaſed, as a Cylinder deſcribed about AEB is to a Cylinder deſcribed about ADB, and theſe Cylinders being of the ſame height, muſt have the ſame Proportion as their Baſes have, or as the Squares of the Diameters of their Baſes. Call the Diameter of the leaſt Cylinder a, and that of the greateſt $a + e$ then is $a^2 + 2ae + e^2$ to a^2 as the Spheroid AEB is to the Spheroid ADB, and conſequently by Diviſion of Ratio $2ae + e^2$ is to a^2 as the encreaſe AEBDA is to the Spheroid ADB; that is, e multiply'd into $2a + e$ is to a^2 as AEBDA to the Spheroid

ADB,

A D B; and *a* being in the preſent Caſe Fifteen inches e = $\frac{1}{100}$ of an inch, and A E B D A, according to you 31 Cubical Inches; $e \times 2a + e$ will be $\frac{30}{100} + \frac{1}{10000}$ and a^2 is 125 : wherefore as $\frac{30}{100} + \frac{1}{10000}$ is to 125, ſo is 31 to the number of inches in the Spheroid A D B, or to the Capacity of the Thorax, which therefore by the rule of Proportion muſt contain 23242 ſolid Inches,or muſt hold above a hundred Gallons of Wine Meaſure, that is a Hogſhead, and very near two Thirds of another, and ſo much according to your numbers muſt the Breaſt contain. I muſt confeſs this Thorax of yours, is of the largeſt ſize I ever yet read of.

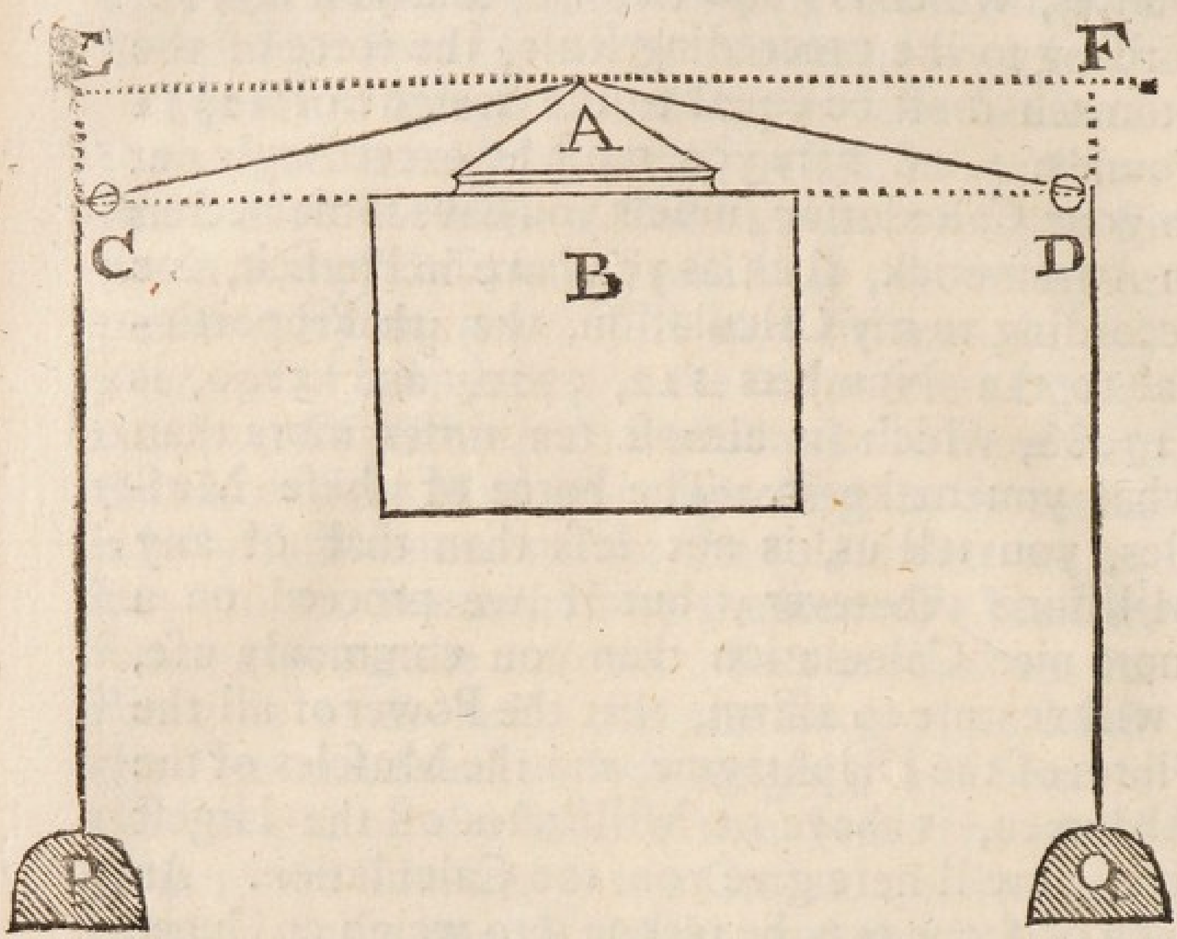
Let us next ſee what muſt be the length of the Axis of the Spheroid. The Cylinder circumſcribed is to the Spheroid as Three to Two, and therefore the Cylinder muſt contain 34863 ſolid Inches, and the Diameter of the Baſe being 15, the Baſe muſt be 177 Square Inches, by which if we divide the number 34863, we ſhall have 196 Inches for the height of the Cylinder, which muſt be the ſame with the height of the Thorax or Breaſt; and becauſe a Man's length is commonly more than Sextuple of the Cavity of the Thorax, the heighth of a Man at this Rate of reckoning muſt be above 1176 Inches ; that is, muſt be allowed to be 100 Foot high. A Man of this height may ſtand upon the Ground, and eaſily reach the top of the higheſt Houſe in *Edinburgh*, tho' the Houſes here be as high as in any Town in *Christendom*. You ſee, Sir, what Concluſion your Geometry is able to bring forth.

There are many more things in your Diſſertations that would be worth while to conſider. But I have not leiſure now to examine them ſo much as they deſerve; I will only take notice at preſent of your great skill in Arithmetick, and your Art of calculating at the end of your Diſſertation on Digeſtion. You undertake to ſhew that the force of the Muſcles compreſſing the Stomach, is fully ſufficient to reduce the Solids that are taken into the Stomach, into the form of a Fluid fit for nouriſhing the Animal. You proceed on the Principle of *Borrelli*, that the force of all Muſcles is proportional to their weight; and becauſe the Weight of the Muſcle that bends the third Joint of the Thumb, is 122 Grains, and its force equal to 3720 Pound weight, and the weight of all the Muſcles that preſs on the Stomach, is 8223 Grains: From whence you ſay, As 122 Grains is to 3720 Pounds, ſo is 8223 to 248235 Pounds. 'Tis ſomewhat ſtrange, that ſo great a Mathematician as you ſhould not be able to work the common Rule of Proportion, or the Golden Rule, which every School-boy knows exactly. Perhaps you have ſome new way with you, or ſome other Notion of Proportion than what is founded on the Elements of *Euclid*. I am ſure if we take the ordinary Method which I learned at School, the fourth proportional to the Numbers 122, 3720, 8223 is 250734, which Number does not agree in any Figure with yours, but in the firſt. And when you come to calculate the Muſcular Fibres of the Stomach it ſelf, you ſuppoſe the weight of the Stomach to be eight

Ounces,

Ounces, which is 3840 Grains; and you ſay, according to the preceeding Rule, the force of the Stomach muſt be equal to the weight of 12951 Pounds. And here you muſt be exceedingly out in your Calculation, unleſs you have ſome Secrets in Arithmetick, ſuch as you have in Phyſick, for according to my Calculation, the 4th Proportional to the Numbers 122, 3720, and 3840, is 117088, which is almoſt ten times more than what you make it. The Force of theſe Muſcles, you tell us, is not leſs than that of any Millſtone whetever; but if we proceed on a more nice Calculation than you commonly uſe, I will venture to affirm, that the Power of all the Fibres of the Diaphragme, and the Muſcles of the Abdomen, is above 90 Millſtones of the largeſt ſize. I will here give you the Calculation. An Inch of Stone may be reckon'd to weigh an Ounce and a half, and therefore a Cubick Foot of Stone will weigh 216 Pounds *Troy*. Now there are few Millſtones that are 4 Foot in Diameter, and they being Cylinders, the Area of the Baſe will be about 12 Foot and a half, and ſuppoſing each Millſtone a Foot thick, it will contain 12 and a half ſolid Feet, and weigh 2700 Pounds, which is about 91 times leſs than what you ſay is the Power of the Muſcles that compreſs the Stomach; ſo that 91 Millſtones are but equal in force to the Power of theſe Muſcles, and who would not be ſurprized to think that there is a force equal to that of ſo many Millſtones, employed for the Attrition of the Aliment. But after all, if this matter be examined more clearly, and things be fairly ſtated, it will be found that there is not the $\frac{1}{10000}$ part of this force

that



that preſſes the Stomach. For tho' the Abſolute force of all the Muſcular Fibres be ſo great as you have ſhowed. Yet all that is to be deduced from thence, is, that if theſe Fibres acted in any Body in Parallel Directions, the weight they could in that caſe ſuſtain, would be that of 91 Millſtones; or which comes to the ſame, if all the Fibres of theſe Muſcles acted directly againſt two Bodies to preſs them together, the Bodies will be as much preſſed, as if 91 Millſtones lay upon them. But this is not the caſe of the preſſure of the Muſcles on the Stomach; for their Action on it is very Oblique, and much the greateſt part of their force is ſpent in their pulling againſt one another, ſo that notwithſtanding the abſolute force of theſe Muſcles is ſo great, yet perhaps

perhaps their preſſure on the Stomach will not be ſo great, as if 10 Pounds weight lay upon it. To make this clearer, let us ſuppoſe two great weights P and Q tyed together by a Rope that paſſes over two Pullies C and D, and by that means they preſs the Body A upon B. Let us ſuppoſe the Horizontal Diſtances of the Ropes from A; that is, E A, F A equal, and the Weights alſo equal. From the Principles of Staticks, the Preſſure of A upon B, will be to the weights P and Q, as C E is to 2 C A; and therefore if each of the Weights P and Q were 10000 Pounds, and C E twenty thouſand times leſs than C A, then the force by which the Bodies will be preſſed together, will be but the ten thouſandth part of the Bodies P and Q; ſo that if one Pound weight lay upon A, the Bodies A and B will be as much preſſed as they are by the Weights P and Q. Let us ſuppoſe now C A D to be one of the Fibres of the Diaphragma, which touches the Stomach in A; this Fibre, contracting, endeavours to bring it ſelf into a right Line, and by that means preſſes the Stomach. Now if CE were but the twenty thouſandth part of the length of the Fibre C A D, the Preſſure on the Stomach will be but the twenty thouſandth part of the whole force of the Fibre. You ſee then, Sir, that to determine whether the action of the Muſcles is ſufficient for the Attrition of the Aliment, you muſt not only calculate their abſolute force, but their force by which they preſs the Stomach; and without this, all your other Calculation ſignifies nothing, for their abſolute force may be much greater than you make it, and yet not ſufficient for the Reduction of the Aliment to a fluid Form.

Let us next inquire, whether you be more happy in your reaſoning about the practical part of Phyſick, than we find you are in the Speculative. This ſhall be of a Diſeaſe that falls oftneſt under the Phyſician's Conſideration, and the only practical Diſſertation in all your Works; where we may hope you are likewiſe more accurate, ſince the Subject is of ſuch moment, and that we find all your pretences to Learning are ſummoned together to expoſe your Brethren.

At preſent, it will be ſufficient to attend how you make good this heavy Charge. We ſhall therefore lay aſide all your Digreſſions, and learn what Secretion or Evacuation ought to be made choice of for curing Fevers, when ſome Secretion is to be choſen. More particularly, that Fevers are oftner cur'd by Medicines that evacuate by the Skin, than by Purging Medicines; and that, in oppoſition to ſome ignorant Phyſicians in *Edinburgh*.

At firſt ſeting out we muſt have Patience till we hear of your acquaintance with *Steno*, and a touch at your own Praiſe. But let us begin to Buſineſs; you deſire it may be obſerv'd, that Fevers go off by augmenting Secretion at the Skin, by the Glands of the Kidneys, and by making a Diarrhæa. Secondly, that there are not any Veſſels, nor any Glands of our Body ſerving for Secretion (as if ſome Glands did not) that may not be ſo enlarg'd as to receive and ſeparate any Liquor commonly ſeparated in other Glands. From theſe Obſervations you conclude, that there is not any Fever, the like whereof has not been carried off more frequently by Secretion at the Glands of the Skin, than at any of the reſt; and

therefore

therefore there is not any kind of Fever-matter that may not be carried off by the Glands of the Skin. *Q. e. d.*

If we had follow'd you through all your Digression, we had never come to a clear sight of this Mathematical Conclusion, which will still be more plain, if we view it in a common way of speaking. Experience shews us, that Fevers are cured by Sweating, Urine, or Stool; and therefore Fevers may be thus cur'd, is your Conclusion and Discovery; but that from the same Observation, or your second, they are oftnest cur'd at the Glands of the Skin, is not made appear; tho' it is what your Adversaries wanted, and you undertook to demonstrate.

Before we proceed; I must tell you that your second Observation is very ill supported by the Particulars you alledge in its favour, tho' it serves you to no great purpose at present, if it were realy true. Moreover, your expression about Secretion shews how little you know of that Affair; but as this has been done already at greater length, I shall only observe how widely you differ from Dr. *Cockburn* on this Subject; yet he must needs be oblig'd to you for his Doctrine. Read him again, and you'll find your changing Hands of Secretions very absurd; as also that your circular Ducts of different Diameters help us very little in the case of different Secretions.

But, to leave this, let us advance with you in your further Disquisition in the Subject of Fevers. The next Enquiry then is, *How much, and in what manner the Blood is chang'd or alter'd in time of a Fever.*

In order to resolve this useful Question, some Experiments, you say, may be proper, that shew the Nature of the Fever-matter to be such, as may pass by any Vessel. But, once more, these Experiments are really so proper and material, that you think fit to put off the Disquisition at this time; especially that you seem to be of Opinion, that it may be fully satisfy'd, by shewing what proportion the Natural Secretions have to one another. Howsoever improbable a Medium this seems to be; yet we are willing to learn, how, by the given Proportion of Natural Secretion, the Disposition and Aptitude of the Fever-matter may have to pass indifferently by any of the Secreting-Vessels. But, instead of the Demonstration, all this account is turn'd into a pitiful Comparison of *Sanctorius*'s Experiences, by the help of a little Arithmetick; tho' *Huygens* must be brought into the Scene upon account of the *Chance* that this Fever-matter may have to pass rather by the Skin than at any other Passage of Evacuation. Had it not been more proper, first to have answer'd your Question, and to have told us the nature of the Fever-matter; then your Arithmetick might have found a place to shew us the odds of this Matter being voided at the Skin, and at other Parts? But your new method of Demonstration is to beg the Question, and to fall foul on your Adversaries. On the other hand, if you'll allow a little of your Liberty to Dr. *Brown*, one *Supposition* for him will conclude all you have said more powerfully against you, than your many *Suppositions* make for you. Suppose then, this Fever-matter or ferment, is in a very inconsiderable quantity in the Blood: It will

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thence follow, that it may be soon discharg'd by Stool, or otherwise; and far more certainly that way, than by the Skin; the means for producing that effect being more constant in their Operation. This one Consideration of a greater certainty, at once determines the Choice of any Prudent Person.

In effect, the only thing that is plain without a Supposition, is that Dr. *P———n* knows nothing of a Fever; since after all your specious Pretences to Demonstration, we know nothing of this Fever-matter, nor how it affects our Blood; nor whether Evacuation is at all necessary, nor which is the best. Only as the Inquiry seems to be about the Choice of Evacuations, we suppose there is an equal aptitude for this Matter going any way, and then there will be a greater quantity of it spent by the Skin. But as all this is right upon Supposition, we leave you to make it further out.

The Corollaries, you draw from these Demonstrations, standing and falling by their sufficiency may be justly neglected, please only to remember, that as Fevers having been cur'd in ways of Evacuation, is founded on Experience; so the like Experience evidently teaches us how little necessary a Quantity of any Evacuation is: Nay, how hurtful a great quantity has often been, is well known to every Physician.

Dr. *P.* himself seems abundantly apprized that a sufficient Evacuation may be had by Purging; but then, according to his Modern Method of Demonstration, *Purging* is the Evacuation by the Skin. If this is allow'd him against the Sense of *Sanctorius* and all Physicians,

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he may be in the right; but, in that caſe, his Adverſaries and he are agreed, and there's an end to the Diſſertation. This abſurdity is ſo groſs, that nothing leſs than his own Words can gain credit to the Obſervation. " For, ſays he, as " to what concerns an Evacuation made by the " means of Lenitive Medicines, or of ſuch as " clear the firſt Paſſages of Excrements that " ſtick to them; this is no more to be atribu- " buted to Purging or augmenting the Secreti- " on of the Belly, than the waſhing of the out- " ward Skin: For thoſe Lenitives only pro- " mote ſuch a Perſpiration of the Inteſtines, as " that of the outward Skin; for when the Pores " of the Inteſtines are open'd, a greater quanti- " ty of Tranſpiration ruſhes out there, than " out of a like Portion of the outward Surface " of the Body.

From theſe Words it is, at leaſt, plain, that theſe Lenitive Medicines are a more certain means of evacuating a quantity by the Bowels, than the proper means can do by the Pores of the Skin; a concluſion altogether againſt your purpoſe. But ſay you, this is not purging. Pray good Doctor, what do theſe Medicines paſs for among Phyſicians, *Purges* or *Sweats*? Purging Medicines they have all along been reputed; and yet they are effectual means for curing a Fever, as Dr. *Brown* aſſerted, and you prove.

I know not how poſſibly you can get clear of this Abſurdity, but by having recourſe to your common way of Demonſtration. For the purpoſe; ſuppoſe Fevers only to be cur'd by Tranſpiration; but they are cur'd by lenient, *Anglicè geutle*, Purging: Therefore gentle Purgatives

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promote Tranſpiration; by no means Purging. This is a clear Demonſtration, the Expreſſion new, contrary to the uſe of theſe Words among Phyſicians, and downright oppoſite to *Sanctorius*. Thus you ſtand by your ſelf in a new Languge to no manner of purpoſe, but to be reconcil'd to Dr. *Brown* when you leaſt intend it.

I deſign'd, Sir, to have ſhewn to how little purpoſe you have attempted *Bellini*'s Theorem, and that your Skill in Mathematicks is as deficient in its diſcovery, as in the mentioned inſtances; but we muſt keep within the due bounds of a Letter. I hope what is already ſaid, will conduce a little to make you know your ſelf; I am ſure it will make you better known to our Country-men, however great a Stranger you have hitherto been to both. By this, your Civility, in endeavouring to make me known abroad, is ſomewhat repair'd; and it had been much to the honour of our Country, that you were likewiſe a Stranger there.

F I N I S.