Remarks on Mr John Bell's anatomy of the heart and arteries / [by Jonathan Dawplucker].

REMARKS

ON

MR JOHN BELL'S

ANATOMY

OF THE

HEART AND ARTERIES.

TO

JOHN BELL, Esq.

SURGEON IN EDINBURGH.

SIR,

THE ſecond volume of your very valuable work on Anatomy happening, ſoon after its publication, to fall into my hands, I wrote, by way of amuſement, a few remarks upon it, without any intention of giving them to the public. But when I was ſome time ago told, that you had declared publicly in your claſs that you valued and eſteemed truth above every other conſideration; that, in conſequence of this declaration, you had attacked the moſt reſpectable characters; and that you had not even ſpared your brethren of the ſame profeſſion—I no longer doubted that you would conſider the publication of my remarks, how trifling ſoever they may be, as a very particular favour. And as I have always been one of your moſt ardent and moſt devoted admirers ever ſince I had the felicity of being acquainted with your great name, I immediately reſolved to gratify you in this particular; conſidering that I would at the ſame time be promoting the cauſe of truth, for whoſe intereſts I am no leſs

zealous (pardon the compariſon) than your illuſtrious ſelf. I have, Sir, accordingly publiſhed them out of pure good nature and zeal to oblige you; and I thought I could not poſſibly do better than dedicate them to the celebrated man whom they moſt concern, and to whom, I am perſuaded, they will give the greateſt ſatisfaction and delight.

I HAVE no doubt, Sir, that you, who have taken upon yourſelf the vindication of truth, and who have exerted yourſelf already ſo ably and ſo diſintereſtedly in her cauſe, will embrace the earlieſt opportunity of reading theſe my remarks publicly in your claſs, and of recommending them to the careful peruſal of all your pupils. You may perhaps cauſe them to be bound up with your ſecond volume, that none of your readers may find the leaſt difficulty in coming at the truth. But I would take the liberty to hint, that this would not be altogether proper; for it would be beſtowing an honour upon my poor performance to which it is by no means entitled.

I INTENDED at firſt to have publiſhed my remarks on the ſecond part of your volume; but when I recollected that almoſt the whole of that part, even your diſcoveries, have been taken from Haller and Sabatier; and that the obſervations of your own, which now and

then

then occur, are of very little importance, I changed my mind. But if I find you ſufficiently grateful for this preſent favour of mine, I may perhaps, at ſome future period, gratify you with my remarks, not only on the remainder of your laſt volume, but on all your other publications.

As I am one of your moſt devoted and zealous admirers, nothing can give me greater pleaſure than to hear of your ſucceſs in life. Allow me therefore, before I conclude, to give you a hint or two, which you may perhaps find uſeful.

One thing you ſhould particularly aim at, I mean, to be attacked publicly by ſome eminent man; becauſe you might then, with great propriety, cry out *perſecution*. You would raiſe a party in your favour, and your ſucceſs would be infallible. You ſhould therefore attack the characters of the moſt reſpectable men of the ſame profeſſion with yourſelf; you ſhould treat them on every occaſion as a parcel of fools and knaves, and declare that their writings contain nothing but lies and abſurdities. The farther your aſſertions are from the truth, the more apt will theſe reſpectable characters be to attack you; and in that caſe you would gain your point. You would have only to repreſent their attack as proceed-

ing from envy at your ſuperior abilities and ſkill, and from a deſire of concealing from the world your merit, which if ſufficiently known would deſtroy themſelves. This plan you have indeed followed; but I do not think you have gone far enough. Theſe gentlemen are too prudent and too good-natured to retort, or perhaps they are too proud to ſpend even a thought upon you.

What do you think, therefore, of affirming, that my remarks have been written by ſome of theſe medical gentlemen out of pure ſpite and ill-nature, in order to tarniſh thoſe laurels which they could not hinder you from obtaining, and to diminiſh that glory which they could not rival? Upon very mature deliberation, I conſider this as the beſt plan which you can follow.

You can eaſily make a very *pathetic* ſpeech on the ſubject: You may ſay alſo, that this *unfortunate book* of mine fell into your hands by accident; that it is a *filthy pamphlet*; that the remarks which it contains are exceedingly *ſilly* and *trifling*; that the perſon who wrote them *knew nothing* of the matter; that he had diſplayed the daring and unpardonable ambition of being tranſmitted to poſterity as the antagoniſt of the illuſtrious philoſopher, whoſe name ſhall ſhine with

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an eternal luſtre, and who ſhall be known, and admired, and adored, in thoſe ages when Newton ſhall be forgotten, and the ſun, and the moon, and the ſtars, to uſe your own ſublime language, are gone to the vault of all the Capulets.—I am,

Illuſtrious Sir,

Your ſincere Friend,

And devoted Admirer, till death,

JONATHAN DAWPLUCKER.

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CHAP. IV.

It is requeſted of the Reader to alter with his pen the following

ERRATA.

In page 10. line 11. from the top, (P. 53.) ſhould be (P. 43.)
In page 15. laſt line, (P. 109.) ſhould be (P. 9.)

REMARKS

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ON

Mr JOHN BELL's ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. I.

THE following remarks were made during the perusal of Mr John Bell's Anatomy of the Heart and Arteries. As they are rather unconnected, the writer has not been very ſolicitous about their arrangement.

To begin with the firſt chapter, which is entitled,

OF THE MECHANISM OF THE HEART.

The deſcription of the heart is in general accurate and lively; at the ſame time, it muſt be acknowledged, that the author has been more ſolicitous to amuſe than to inſtruct his readers. This ſolicitude has probably occaſioned the extreme diffuſeneſs of the ſtyle ſo conſpicuous in every part of this volume, and has induced him to introduce ſo great a number of foreign topics, that the digreſſions occupy no inconſiderable part of the book. Thus in the preſent chapter we have the diſputes about the water which iſſued from our Saviour's ſide, the hiſtory of the bone of the heart, and a long account about big hearts and little hearts.

Perhaps alſo the author has been too eager to raiſe himſelf in the opinion of his readers at the expence of others; at leaſt, it is not eaſy to ſee any other reaſon for the harſh language which he conſtantly uſes when he ſpeaks of preceding writers. In his account of the irritability of the heart, he ſays, "Philoſophers have been "ſo bewitched with the deſire of explaining the phe-"nomena of the human body, but without diligence "enough to ſtudy its ſtructure, that from Ariſtotle to "Buffon it is all the ſame, *great ignorance* and *great* "*preſumption.*" (P. 53.)—Such an affirmation would have been improper in any writer, becauſe it is contrary to truth; but it is doubly improper in the preſent writer, becauſe every thing which he ſays about the irritability of the heart is contained in the writings of thoſe very philoſophers whom he thus vilifies. Every thing which he ſays on that ſubject may be ſummed up in theſe two propoſitions:

1. The heart is ſtimulated to contract by the blood.
2. The heart contracts by a *vis inſita.*

The firſt of theſe opinions was maintained by Lanciſi, Senac, Whytt, &c. Haller not only maintained it, but proved its truth by a ſeries of experiments; and it has been long almoſt univerſally received by phyſiologiſts.

The ſecond propoſition is merely the opinion of Gliſſon, &c. as new modelled by Haller. As our author has not attempted to refute the direct arguments brought againſt it by Whytt, Monro, and other celebrated philoſophers, and has not brought a ſingle new proof in order to ſupport it, he will not be ſur-

priſed to hear that many of his readers are diſpoſed to call it in queſtion.

When treating of the valve of Euſtachius, our author ſays that no good plate had ever been given of it except his own; yet ſome pages after he mentions a beautiful plate of it by Cowper, and he has forgotten altogether the plate of Haller. He tells us, that the uſe of that valve is ſtill imperfectly underſtood; and then he proceeds to inform us, that it ſerves merely to complete the auricle. Now *this very uſe* has been aſſigned by Haller in a book which our author quotes frequently, and which conſequently one would naturally ſuppoſe that he *muſt* have read.

When treating of the coronary veſſels, he ſays: "Thebeſius believed that there were ſome ſhorter "veins, by which the blood was returned, not by a "long circle into the right auricle, but directly into "the ventricles of the heart. Veuſſens, Thebeſius, "and others who belonged to their party, pretended "to prove this fact by injections: But what doctrine "is there which ſuch clumſy anatomy and aukward "injections may not be made to prove? They uſed "mercury, tepid water, and air; and they forced "theſe, the moſt penetrating of all injections, till they "exuded upon the inner ſurface of the heart; but "uſing any coarſe injection, as tallow or wax, the "injection does not exude this way, but, following "its natural courſe, keeps within the arteries and "veins, and ſometimes finds its way back to the au-"ricle of the heart." (P. 28.)—Is it not natural for the reader to ſuppoſe, that our author, when he ſpeaks in this ſtyle, has actually read the diſſertation of Thebe-

ſius? Yet Thebeſius informs us in page 15. of his diſſertation, that he made uſe of theſe *very courſe* injections of which our author ſpeaks.

Though the anatomical part of this chapter be in general accurate, there occur a few paſſages in it which it may be proper to mention. In page 49, the author tells us, that he ſuppoſes the pericardium ſurrounds the heart cloſely; becauſe when the heart is injected before the pericardium be opened, that covering is completely filled. Now anatomiſts know, that both the heart and arteries may be ſwelled out by injection much beyond their natural ſize; and that therefore the bulk of the heart, after being injected, is no proof that it filled the pericardium in the living body. In page 38 he informs us, that all the fibres of the heart are oblique; yet in the ſame page he affirms that ſome of them run nearly tranſverſely; and in the next page, that any attempt to extricate the fibres of the interior part of the heart, and conſequently to aſcertain their poſition, is abſurd and impoſſible. If ſo, how comes he to know that all its fibres are oblique? In page 54 he denies that water is ever found in the pericardium of thoſe perſons who have died ſuddenly, provided they be diſſected immediately after death. Haller, who certainly diſſected many more ſuch perſons, affirms the very reverſe.

As to the ſtyle, it is entitled to the praiſe of being lively and entertaining; but it is rather like the ſtyle of a female romancer than a man of ſcience. To elegance, or even neatneſs, it has no pretenſions: the ſentences are almoſt all ill conſtructed, and vulgar phraſes and improper expreſſions occur very often.

But

But moſt probably elegance was not our author's aim; and if he was only anxious to appear perſpicuous, he has in ſome meaſure ſucceeded: but by no means completely; for the language is too vague, and the ſentences too confuſed, to bear examination Many paſſages have ſcarcely any meaning at all. Some paſſages flatly contradict others; and even blunders in grammar, as the following paſſages will ſhow, are not uncommon.

"In both ventricles this is very remarkable, that "towards the opening of the auricle *it* is very rug- "ged." (P. 18.)—"The little horns or tags *becomes* "ſo tenſe." (P. 19.)—"They *prevent* the valve *be-* "*ing forced.*" (P. 24.)—"To *prevent it gravitating* "upon that which is riſing from the liver." (P. 31.) "How *terrible dangerous* it was to open an artery." (P. 61.)—"The Harveian doctrine had no ſooner "breathed life into the new philoſophy of the hu- "man body, *or* phyſicians *begun* to think of the "heart." (P. 67.)—"Nor can I believe that there "is any difference *among* all the three." (P. 98.) "If oil, mucilage, water, or any other fluid, be "*ſubſtituted to* the ſerum." (P. 99.)—"Many things "are to be *taken in* the calculation." (P. 125.)— "The function of the placenta actually is *equiva-* "*lent with* the function of the lungs." (P. 186.)— "Something *equivalent with* the function of the lungs." (P. 188.)

The following paſſages, and many more might have been ſelected, have either no meaning at all, or a very abſurd one.

"The diſcovery of the circulation of the blood has "been always regarded as one of the grandeſt in ſcience:

" ſcience: it has been ranked rather with the great " doctrines of philoſophy, than with the little diſco- " veries in our peculiar ſcience; and it has been " boaſted of by our countrymen, and much coveted, " and often claimed, by ſtrangers. Indeed its real " importance *falls little ſhort of the feelings* which all " theſe diſputes convey to the mind; *for* it is in itſelf " moſt INGENIOUS AND BEAUTIFUL; *and* it is the " foundation of all that phyſicians have thought or " practiſed, right or wrong, uſeful or deſtructive, ever " ſince that day." (Pref. p. 1.)

" We have trodden down *at once all their doctrines* " and principles. The chemiſtry of the preſent day " is no more like theirs than our reaſonings are. If " we ſpeak *now* of *mechanics*, *we mean* ſimply the " mechaniſm of the *human body*." (Pref. p. 8.).

" It is *peculiar* in this chiefly, that the *forms* of " the arteries and veins of the heart itſelf are *beauti-* " *ful*, and that the arteries riſe juſt under the valves " of the aorta." (P. 25.)

" Their form they preſerve only while in the blood, " and ſeem to be *ſupported* more by the *qualities* of " the *ſerum* than by their *own properties*; *for* if " mixed with water, they mix eaſily, and totally diſ- " ſolve; the water is red, but the globules are gone." (P. 90.)

" And this above all is a moſt ſingular property of " the ſerum, that it *admits freely* the air to paſs " through and impregnate the blood; for when the " coagulum of the blood is drowned deep in its ſe- " rum, if *turned up and expoſed to air it reddens*; " which, if oil, mucilage, water, or any other fluid,

" be

" be ſubſtituted to the ſerum, it will not do." (P. 99.)

" Modern chemiſtry proves to us, that it is not the " *loſs* of any principle that *endows* a metal, for ex- " ample, with *negative* powers; but the direct acqui- " ſition of a *new* principle, which endows it with *po-* " *ſitive* powers." (P. 106.)

" Water has all the appearance of a pure and ſimple " element, but it is in truth a compound body, con- " ſiſting of two parts; of *inflammable* AIR for its ba- " ſis, and of oxygene combined with it, in that *great* " *proportion which the great appetite of inflammable* " *air requires:* and as inflammable air, when ſatura- " ted with oxygene, forms not any acid air, but pure " water, it has changed its name, and is now called " hydrogene air." (P. 129.)

" This is the reaſon that when many ſmall fiſhes " are incloſed in a narrow glaſs, they all ſtruggle for " the uppermoſt place, *as in the Black-Hole;* and " that when in winter a fiſh-pond is entirely frozen " over, you muſt break holes for the fiſhes, not that " they may *come and feed*, but that *they may come* " *and breathe;* without this, if the pond be ſmall, " they muſt die." (P. 153.).

" Its ſtructure is ſtrong, muſcular, and *continually* " *active*, performing the office of a ſecond heart. " The aorta, when dilated, in nine of ten caſes is co- " vered with white ſpots; it is diſeaſed; *they are* " *aged people*, and almoſt always the dilatation be- " gins from the heart." (P. p. 246, 247.)

Of ſingle words, let the following inſtances ſuffice: *Extremeſt* veſſels (p. 109.) A *mechanical* and fixed

diſeaſe

diſeaſe (p. 223.) But the word which our author has treated with leaſt mercy is *tranſparent*. We have the heart of a fiſh as *tranſparent as a bubble of water* (p. 12.)—*tranſparent* veins (p. 14.)—the bones becoming *tranſparent* in old age (p. 32.)—the lungs of a crocodile very *delicate and tranſparent* (p. 150.)—the outſide membranes of the lungs of a frog as *tranſparent as a ſoap bubble* (p. 150.)—the lungs of the aſk *exquiſitely tranſparent* like the ſwimming bladder of a fiſh (p. 150.)—and the valve of the foramen ovale *perfectly tranſparent* (p. 181.)

What meaning the author has affixed to the word *tranſparent*, or whether he has affixed to it any meaning at all, we cannot pretend to decide.

While upon the ſubject of words, it may be proper to mention, that there are ſeveral names which our author conſtantly ſpells wrong. We have *Eriſtratus* in page 60 inſtead of *Eraſiſtratus; Vieuſſens* is always ſpelt *Veuſſens; Valſalva* is ſpelt *Vaſalva;* and *Drelincurtius* is ſpelt *Drellincartius.*

Bad grammar and bad ſpelling, it muſt be confeſſed, are rather ſingular phenomena in the writings of an author, who tells us that he is acquainted with *all the philoſophers from Ariſtotle to Buffon.*

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ON

Mr JOHN BELL's ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. II.

THE ſecond chapter treats OF THE APPEARANCE AND PROPERTIES OF THE BLOOD, OF THE CHEMISTRY OF OUR FLUIDS, AND OF THE INFLUENCE WHICH AIR HAS ON THEM. This chapter is very long, and will require a more particular examination than the laſt.

The blood is commonly conſidered as conſiſting of three parts; the red globules, the gluten, and the ſerum. This is the diviſion which our author adopts.

Our author's account of Leeuwenhoeck's theory of the red globules, with which the chapter begins, is by no means ſo accurate as it ought to have been. Leeuwenhoeck was one of the firſt diſcoverers of the exiſtence of red globules in the blood. Soon after this diſcovery he obſerved globules alſo in chyle and milk, and thought that their diameter was only one-ſixth of that of the red globules. Hence he was led to conjecture, that each of the red globules was compoſed of ſix of the globules which exiſt in the chyle. On adding volatile alkali to blood, he obſerved that the red globules were immediately broken down into ſmaller globules; and hence he conjectured, that the ſerum and

gluten conſiſted of globules. Theſe conjectures were confirmed by a great number of additional experiments which led him to form this theory. The red globules are compoſed of ſix ſmaller globules, each of theſe of ſix ſmaller, and each of theſe perhaps of ſix ſtill ſmaller; conſequently a red globule is compoſed of 36, or perhaps even of 216, ſmall globules.

Such was the theory of Leeuwenhoeck, as may be ſeen by conſulting his works. Both the theory which our author aſcribes to Leeuwenhoeck, and the improvement of it which he aſcribes to Martine, belong to Boerhaave. That illuſtrious philoſopher taught it long with applauſe, and publiſhed it in his *Inſtitutes*. And though his hypotheſis has proved erroneous, philoſophy lies under conſiderable obligations to him for it; as it gave riſe to a controverſy which was not decided till a great deal of new light was thrown upon ſome of the moſt difficult parts of phyſiology. Boerhaave's theory was adopted by Martine, Wintringham, Helvetius, Noguez, Lieutaud, &c. and oppoſed by Lanciſi, Brendel, Senac, and Haller, &c. by whoſe writings and experiments it was completely overturned. Our author has not mentioned one of theſe writers, nor has he produced a ſingle argument againſt the theory of Boerhaave, or of Leeuwenhoeck, as he has thought proper to call it: For the arguments mentioned in the 71ſt page are not to the purpoſe, becauſe they apply only to opinions which the philoſophers who maintained that theory never held. Yet he has thought proper to treat an opinion with ridicule and contempt which he evidently did not underſtand, and to accuſe (p. 69.) a man of attempt-

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ing to impoſe on his readers to whoſe writings he was a total ſtranger; a man who poſſeſſed a candour and zeal for truth which ought to entitle him to reſpect, and which muſt ſecure to him the eſteem of every friend to virtue and ſcience.

Our author, as it often happens with perſons who enter keenly into a ſubject which they do not underſtand, is ſo zealous againſt this theory of Boerhaave, that he has extended his reſentment againſt the red globules themſelves. They are not, he ſays, of that importance to the ſyſtem which phyſicians have ſuppoſed. Many animals want them altogether; and in thoſe which have them, *no likely uſe for them can* be aſſigned. Nay, what is worſe than all this, the "diſ-"ordered and miſerable ſtate of ſcience, which con-"tinued for nearly a century, *aroſe from having ob-"ſerved too much theſe red particles.*" (P. 68.)

It might be urged in defence of the red particles, that they cannot juſtly be charged with having led philoſophers into theſe blunders; and an inſtance will make the truth of this obſervation palpably evident. Let us ſuppoſe (ſince we are upon an anatomical ſubject), that an anatomiſt had been taught a little ſmattering of drawing; that he were very proud of that ſmattering; and that, in order to make his pupils admire his dexterity, he ſhould fall a painting ſkulls, and drawing the figures of veins and arteries upon living men. We appeal to our author, if it would be fair to charge the art of drawing with all thoſe abſurdities into which ſuch an anatomiſt happened to fall? We are not ſuppoſing that any anatomiſt was ever guilty of ſuch childiſh abſurdities; but allow-

ing, for illuſtration's ſake, that ſuch abſurdities had happened, and that they had happened in Edinburgh, would that juſtify us in uttering a philippic againſt the art of drawing?

The hypotheſis of Mr Hewſon about the formation of red globules may be paſſed over, as the writers from whom our author has taken his refutations of it are known to every one.

Next follows our author's refutation of Mr Hunter's theory concerning the life of the blood. We have always conſidered what that ingenious philoſopher has ſaid on this ſubject as too vague and confuſed to convey any preciſe idea. It did not ſurpriſe us, therefore, when we found that Mr B. had written at leaſt as confuſedly as his predeceſſor, and that he had no accurate notion of the opinion which he had undertaken to refute. He talks of it as ſomething in the higheſt degree abſurd and ridiculous, and at the ſame time as entirely ſubverſive of all our preſent phyſiological opinions. He tells us, that blood is in part a foreign body, and that it is contrary to all the laws of nature for the blood to be alive. Our readers will naturally aſk for the proofs of this opinion. Here they are: "A fluid is a body whoſe particles often "are not homogeneous, have no ſtable connection "with each other, change their place by motion, "change their nature by chemical attractions and "new arrangements; a body which can have no per-"fect character, no permanent nature, no living "powers connected with it. But the definition of "a ſolid is the reverſe of this: a ſolid among every "kind of metals, earths, or foſſils, is recognized by

"its

" its peculiar form and arrangement of parts; and in " the animal body, the arrangement of particles gives " the permanent unchanging character of each part; " and in the muſcles, for example, or in the nerves, " where feeling and irritability chiefly reſide, the " form and mechaniſm of the ſolid is in each moſt pe- " culiar, and is always the ſame.

" What is this blood that it ſhould begin life and " ſupport it, and diſtribute it through all the ſyſtem? " Is it not a fluid which varies every hour, now rich- " er, now poorer, now loaded with ſalts, now drown- " ed in ſerum, now much, now ſparingly ſupplied " with air, now darker coloured, now red, now fully " ſupplied with chyle, and now ſtarved of its uſual " ſupply? Is it not loſt in aſtoniſhing quantities in " hæmorrhagies, and drawn very freely from our " veins upon the ſlighteſt diſeaſe? That ſuch quali- " ties are conſiſtent with life in the blood, is what I " cannot believe. But I can moſt eaſily imagine how " the ſyſtem, having by ſucceſſive operations convert- " ed the food into chyle, the chyle into blood, and " faſhioned the nutritious part of the blood into va- " rious ſolids; that theſe new ſolids may partake of " the vitality of all the parts to which they are ap- " plied, and to which they have been aſſimilated by " ſo peculiar and ſo ſlow a proceſs." P. p. 83, 84.

Thus our author has proved, incontrovertibly, that it is contrary to *all* the *laws* of nature for the blood to be alive. As we think his arguments excellent, we ſhall take the liberty of borrowing them for a little, in order to prove that it is *contrary to all the laws*

laws of nature for a *ſolid* to be alive. They will anſwer exceedingly well, as our readers may ſee.

" A *ſolid* is a body whoſe particles often are not " homogeneous, have no ſtable connection with each " other, change their place by motion, change their " nature by chemical attractions and new arrange- " ments; a body which can have no perfect charac- " ter, no permanent nature, no living powers con- " nected with it. But the definition of a *fluid* is the " reverſe of this: a fluid, among every kind of me- " tals, earths, or foſſils, is recogniſed by its peculiar " form and arrangement of parts; and in the animal " body, the arrangement of particles gives the per- " manent unchanging character of each part; and in " the muſcles, for example, or in the nerves, where " feeling and irritability chiefly reſide, the form and " mechaniſm of the fluid is in each moſt peculiar, and " is always the ſame."

" What is a *muſcle*, that it ſhould begin life and ſup- " port it, and diſtribute it through all the ſyſtem? Is " it not a ſolid which varies every hour; now richer, " now poorer, now loaded with ſalts, now drowned " in ſerum, now much, now ſparingly, ſupplied with " air, now darker coloured, now red, now fully ſup- " plied with chyle, and now ſtarved of its uſual ſup- " ply? Is it not loſt in aſtoniſhing quantities in *am-* " *putations*, and *cut* very freely from our *bodies* in " caſes of *gangrene* and *cancer?* That ſuch qualities " are conſiſtent with life in the muſcle is what I " cannot believe. But I *can moſt eaſily* imagine, " how the ſyſtem having, by ſucceſſive operations,

" con-

" converted the food into chyle, the chyle into " blood, and faſhioned the nutritious part of the " blood into various *fluids*, that theſe new *fluids may* " partake of the vitality of all the *ſolids* from which " they have been formed, and to which they have " been aſſimilated by ſo peculiar and ſo ſlow a pro- " ceſs."

This is an admirable argument, and does our author infinite honour. It ſettles the buſineſs completely. No man will talk again of the life of the blood!

Our author next proceeds to the analyſis of the blood. As his account is manifeſtly taken from the chemiſtry of Fourcroy and Chaptal, which are in the hands of every body, inſtead of following him minutely, it will be ſufficient to point out the miſtakes into which he is continually falling, partly from not underſtanding his guides, and partly from venturing ſometimes to wander from them.

In page 91 he ſays, " For the redneſs of the glo- " bules we know no *meaning* nor cauſe;" yet he allows that they contain iron. " But," ſays he, " the " cauſe which gives the oxyde of iron a red colour, " may give the blood a red colour." True; and in that caſe we may ſay with propriety, that we know not the cauſe of any thing whatever.

He tells us in page 93, that " the whole of the ani- " mal food which we eat is gluten, except the fat and " the earth of bones." This is a miſtake.—" That " flour contains *much* ſaccharine and extractive mat- " ter." This is another miſtake.—That the " mem- " branes, ligaments, tendons, perioſteums, and all the

" white

" white parts of the body, conſiſt entirely of gluten,
" and it is the buſineſs of cookery to boil them down
" into this jelly." Here are no leſs than ſeven miſtakes in one ſentence. He affirms, that " *no diſtinction* ſhould be made between the *gluten* and *albumen* OR *ſerum;* that *ſerum* exactly reſembles the *white* of an *egg;* that the *tendinous* and *fleſhy* parts of animals ought not to be diſtinguiſhed (p. 96.); that ſerum contains *foreign* bodies, ſuch as a *ſaccharine* or *extractive matter*, and ſome part of the oxalic, malic, or other vegetable acids (p. 97.); that there is *no difference* between the *red globules*, the *gluten*, and the *ſerum;* that ſuch diſtinctions are *ignorant* and *unmeaning;* and that the *halitus* of the blood is merely *water alone*, having a ſlightly *urinous* ſmell from its connection with the blood" (p. 98.) All theſe aſſertions are not only directly contrary to truth, but moſt of them are ſo completely ridiculous, that they could not have been maintained by any perſon who had the ſmalleſt knowledge of the ſubject.

He tells us farther, that all our ſolids and fluids can be reſolved into gluten; that, " bating the various pro-
" portions of the water which dilutes the ſerum and
" the red globules (whoſe proportion to the fluids
" cannot be named it is ſo ſmall), and ſome ſaccha-
" rine or extractive matter which is in the ſerum of
" the blood—what is there but gluten in all the ani-
" mal ſyſtem? Serum, coagulum, fleſh, tendons, liga-
" ments, bones, all are compoſed of it; and when
" gluten is thus united to the ſolids, forming with
" them one individual body, it acquires new powers,
" and is indeed alive." (P. 99.)

This

This is one of the moſt complete inſtances of abſurdity and nonſenſe that can well be conceived. How a man ſhould have thought of writing on a ſubject of which he was totally ignorant, and of attempting to reaſon on chemiſtry without knowing the very terms of the ſcience, is totally inconceivable.

He tells us farther, that the analyſis of the blood *contains almoſt* the analyſis of *all* the humours and ſecretions of the body; that *urine* very nearly reſembles *ſerum;* that *ſweat* is but a *ſerum* loaded with *ſalts*; that *ſaliva* differs but little from *ſerum;* that *milk* perfectly reſembles *ſerum*, ſince mixing *ſerum* with *water* produces a *milky fluid*, that is, a fluid which gathers *cream* on the top; that the *water of dropſies* is *pure ſerum;* and that the *mucus* of hollow paſſages is little elſe than inſpiſſated *ſerum* (p. 100.).—Our author might have added, with equal propriety and equal juſtice, the bones alſo are pure ſerum, the muſcles and nerves are pure ſerum, the whole body is compoſed of ſerum; fluid, ſolid, and bony ſerum.

By the bye, though our author began with telling us, that the red globules are of no great importance in the ſyſtem, he has here ſhewn that the whole ſyſtem is compoſed of red globules: for the whole ſolids and fluids may be reſolved into gluten or ſerum; and there is *no difference* between gluten, ſerum, and red globules.

The author now comes to an explanation of the function of reſpiration on chemical principles; and we have ſeen already how well qualified he is for the taſk. He begins with an account of the preſent ſtate of chemiſtry. "The *ſimplicity* of the facts in che-

" miſtry, the *correctneſs* of the reaſoning, the *gran-*
" *deur* which now the whole ſcience aſſumes, is *very*
" *pleaſing*, and makes us not without hope that in this
" ſcience *all* others, and OURS in an eſpecial manner,
" may be improved." (P. 101.)—His readers might be at a loſs to know whether it is the *ſimplicity*, *correctneſs*, or *grandeur* of chemiſtry, or altogether being *very pleaſing*, which leads our author to form theſe hopes; but he tells us himſelf, at the end of the ſentence, " *For* the action of veſſels *will do* much in " *forming* and *changing* our fluids; *all* the reſt is che-
" miſtry alone." This is a very ſatisfactory reaſon, and not the leſs ſo that it is totally deſtitute of meaning.

" The older chemiſts were coarſe in their methods,
" bold in their conjectures, in theory eaſily ſatisfied
" with any thing which others would receive. They
" condeſcended to repeat inceſſantly the ſame unva-
" rying proceſs over each article of the materia me-
" dica; and among hundreds of medicinal plants
" which they had thus analyſed, they could find no
" variety of principles, nor any other variety of parts
" and names than thoſe of phlegm, and oil, and al-
" kali, and acid, and ſulphur, and coal." (P. p. 101, 102.

Reader, theſe older chemiſts were Sir Iſaac Newton, Boyle, Boerhaave, Hales, Stahl, Homberg, Geoffroy, &c. the founders of the ſcience; entitled to the reſpect and veneration of every chemiſt, and many of them the authors of diſcoveries which have been the glory of their country and of their age.

" The

" The older chemiſts thought that they had arrived " at the pure elements, while they were working " groſsly among the groſſer parts of bodies. When " they had converted bodies into air, they thought " them annihilated. When they thus ſtopped at airs, " they ſtopped where only their analyſis became in- " tereſting or ſimple; ſtopping where they ſtopped, " among their oils and ſulphurs, made their ſcience a " mere rhapſody of words. Philoſophy they conſi- " dered ſo little, as not to know that the lighteſt air " is really a heavy body, and that with weight and " ſubſtance other properties muſt be preſumed." (P. 102.)

Who theſe older chemiſts were to whom the author alludes, it would be impoſſible for Oedipus himſelf to gueſs. Nobody that deſerves the name of chemiſt preceded Galileo and Torricelli; and ſince their time, who has been ignorant of the weight of air? Nay, farther, Mr Boyle, one of theſe older chemiſts, is the perſon to whom philoſophers are indebted for a great part of their knowledge of the properties of air; and Dr Hales, another of them, is the perſon who laid open the path of pneumatic chemiſtry.

" Modern chemiſtry begins by aſſuring us, that " *theſe airs* are often the *denſeſt* bodies in the *rareſt* " forms." (P. 102.) This chemiſtry muſt be *very modern* indeed which begins with ſuch aſſertions. What is a *denſe* body in a *rare* form? We might as well talk of a *light* body in a *heavy* form, or a *white* body in a *black* form, or a *cold* body in a *hot* form. The author has had no diſtinct conception of what he was writing.—" That airs are as material, *as ma-*

" *nifeſt to the ſenſes*, as the denſe bodies from whence " they are produced." (P. 102.) If this be true, *ſight* muſt be excluded from the ſenſes.—" That it is " *heat* ALONE that converts any ſubſtance into the ae- " rial form: That ſome bodies require for their flui- " dity merely the *heat of the atmoſphere*" (*This is an expreſſion which has no diſtinct meaning*): " That others " require ſome *new* principle to be added, in order to " give them the gaſeous or aerial form." (P. 103.) Do they indeed? Then it is not true that *heat alone* converts *any* ſubſtance into the aerial form.—" That " *all* aerial fluids ariſe, or muſt be preſumed to ariſe, " from ſome ſolid baſis, which ſolid baſis is dilated by " *heat* into an air." (P. 103.) So it *is true*, after all, that *heat alone* converts *any* body into an aerial form! Reader, whichſoever of the two opinions proves true, our author has adopted it. You ſee the unſpeakable advantage of this way of writing!

" Theſe airs can be alternately combined with a " body, and abſtracted again, *adding* or ſubtracting " from its *weight* and *chemical* properties, not only in " a *perceptible* but in a wonderful degree; ſo that theſe " *abſtractions* and *combinations* conſtitute ſome of the " moſt *general* and *important* facts." (P. 103.) This is wonderful indeed! and he that can decypher the meaning muſt have more ingenuity than we can pre- tend to. It is very beautiful for all that.—" When " the old chemiſts then neglected to examine theſe " airs, they refrained from examining the *laſt elements* " of bodies at the very moment in which they came " within their power." (P. 103.) *The laſt elements of bodies!* This is a very important diſcovery; and

we

we take the liberty to thank our author for it, in the name of the whole body of chemiſts: for we can aſſure our readers that it belongs entirely to our author; no *modern* chemiſt, as far as we know, having ever dreamed of it before.

" The older chemiſts obſerved, that when they " burnt an inflammable body, the ſurrounding air was " contaminated, the *ſubſtance itſelf was* ANNIHILA- " TED, nothing remained of its former exiſtence but " foul air." (P. 104.) What older chemiſts made the obſervation, that inflammable bodies, by being burnt, were *annihilated*, we pretend not to divine.— " They ſuppoſed that this inflammable body conſiſt- " ed of a pure inflammable principle, which was the " ſubſtance which ſpoiled the air, leſſening its bulk, " and making it unfit for ſupporting any longer ei- " ther combuſtion or animal life." (P. 104.) The *older* chemiſts who formed this theory, which was a very important improvement of the theory of Stahl, were Dr Rutherford and Dr Prieſtley. Why our author claſſed theſe ingenious philoſophers among the *older* chemiſts, let our readers determine; he evidently claſſes himſelf among the *younger* chemiſts, a place to which he is *eminently* entitled, if he does not actually ſtand at the very bottom of the liſt.

" Modern chemiſtry has explained how *all* theſe " phlogiſtic proceſſes (*combuſtion, calcination of metals,* " *reſpiration*) depend, not on the abſtraction of phlo- " giſton, but on the addition of a new principle; " that they *all* ariſe from *one* poſitive power; that " the *ſame principle* gives *life* to fuel, *heavineſs* (and " other effects of calcination) to metals, *acidity* to " acids,

" acids, and *redneſs* to the blood. Theſe are *all* per-
" formed by *one* power; they are *all eſſentially* ONE
" proceſs; they are *all* effected by the communica-
" tion of *one ſole* principle, viz. the baſis of pure air."
(P. 107.) Happening to turn over to page 123, we obſerved this paſſage: " *Burning* and *ruſting* are ve-
" ry *different*, and ſo *combuſtion* and *reſpiration* are."
We make no doubt that this paſſage, which is a flat and unqualified contradiction of the paragraph juſt quoted, will ſeem ſtrange to moſt of our readers; but they will pleaſe to obſerve, that our author's argument would have been *abſurd* in page 123, if he had ſuppoſed theſe proceſſes *the ſame*, and his explanation would have been *abſurd* in page 107 if he had ſuppoſed them *different*.

This is a ſpecies of argumentation which we would recommend to the attention of our readers. It is but very little known. We do not recollect to have ſeen it taken notice of in any ſyſtem of logic, though we have examined a great number on purpoſe. Nay, what is ſtill more, the profeſſor of logic in our univerſity does not exhibit a ſingle ſpecimen of it in the whole of his lectures. Mr Bell is a perfect maſter of it, and the rules for uſing it might eaſily be deduced from his writings. We humbly propoſe, therefore, in honour of our author, who may in ſome meaſure be conſidered as its inventor, to give it the name of JOHNBELLATION.

Every body knows the importance of the ſigns plus and minus in algebra, and how by their aſſiſtance mathematicians are enabled to extricate themſelves out of the greateſt difficulties. Johnbellation will be equal-

ly

ly uſeful in the other ſciences. Indeed it is founded upon the ſame principles with the uſe of the ſigns plus and minus in algebra, and is merely an extenſion of them. Suppoſe we have any propoſition, for inſtance this, *combuſtion and reſpiration are the ſame:* A writer, ignorant of johnbellation, if he wanted to prove that heat is not evolved during reſpiration, would be at a loſs how to proceed, or how the propoſition could help him out; but the knowledge of johnbellation would remove all his difficulties at once: for it proceeds upon this poſtulate, that every propoſition may be taken either *negatively* or *poſitively*. Conſequently, if it be true that combuſtion and reſpiration *are the ſame*, it is true alſo, according to the principles of johnbellation, that combuſtion and reſpiration *are not the ſame*. Here then we have our choice of two arguments; one or other of which muſt always be to our purpoſe. Conſequently, johnbellation gives us this unſpeakable advantage, that it enables us to prove any thing we pleaſe by arguments perfectly irrefragable and invulnerable.

The principles of johnbellation might eaſily be deduced from the writings of our author; and we may perhaps at ſome future period favour the world with a treatiſe on the ſubject; unleſs indeed, which is a thing rather to be wiſhed, our author anticipate us, by publiſhing himſelf the principles of his art. In the mean time, for the ſatisfaction of our readers, we ſhall inform them, that johnbellation is divided into ſeveral branches, each of which has its peculiar rules and its peculiar advantages.

I. The

I. The firſt ſpecies of johnbellation is *negative* and *poſitive* johnbellation, or johnbellation properly ſo called. This is the ſpecies which we have deſcribed above. As example is in all caſes better than precept, inſtead of laying down rules for uſing it, we ſhall produce a beautiful example or two from that volume of our author's valuable writings which we are at preſent conſidering.

1. "Not upon *any animal*, but in the *human* body." (P. 123.)

2. "Nature has appointed in every breathing crea-
"ture *two hearts*." (P. 4.)—"The frog, the newt,
"the toad, have *one ſingle and beautiful* heart."
(P. 5.)

3. "Of an hundred meaſures of atmoſpheric air,
"we find twenty-ſeven only to conſiſt of vital or pure
"air; ſeventy-two conſiſt of azotic air as it is called,
"fatal to animal life; and one meaſure only is fixed
"air, which is alſo an unreſpirable air. But of theſe
"twenty-ſeven parts of pure air, *ſeventeen* parts only
"are affected by reſpiration; ſo that in reſpiration we
"uſe much leſs than a fifth part even of the ſmall
"quantity of air which we take in at each breath."
(P. 127.)

"Our atmoſphere is ſo conſtituted as to hold but a
"fourth part of vital air, and of that ſmall proportion
"one *half* only is uſed in the lungs." (P. 128.)

4. "We may fairly begin our next general fact un-
"der the title of the oxydation or *oxygenation* of the
"blood." (P. 113.)—"We call this proceſs not the
"*oxygenation*, but the *oxydation* of the blood." (P.
117.)

117.)—" It is *not* a fair nor permanent oxydation." (P. 121.)

5. " The ductus venoſus enters the largeſt of the " *hepatic veins*." (P. 173.)—" The ductus venoſus " enters the *heart*." (P. 174.).

6. " The ſtimulant power of oxygene is moſt of all " apparent when we force a living creature to breathe " nothing but the pureſt air; for oxygenated or vital " air makes this proceſs too rapid; the pulſe riſes, the " eyes become red and prominent, the creature ſeems " drunk with the new ſtimulus, too great for its ſy- " ſtem. The *univerſal heat of its body is greatly in-* " *creaſed*, the eyes are turgid and red, and at laſt a " ſweat breaks forth all over it; and when dead, the " lungs (it is ſaid) are mortified or inflamed." (P. 116.)

" The next effect of oxygene is ſaid to be the com- " municating of heat to the lungs. But I ſuſpect, that " if the ſmall quantity of oxygen which can enter by " the lungs *do communicate heat*, it muſt be not to " the *lungs*, nor to the *blood*, but to the whole body, " through the medium of the blood. There are ſome " who pretend to ſay, that when they draw in vital " air, *they feel a genial warmth* in the breaſt, diffu- " ſing itſelf over all the body; but it is eaſy to feel " in this way, or any way, when a favourite doctrine " is at ſtake, while thoſe who know nothing about " doctrines breathe the vital air *without any peculiar* " *feeling* which they can explain." (P. 117.)

II. The next ſpecies of johnbellation belongs excluſively to our author: he uſes it upon many occaſions, and with great addreſs. It may be called *double*

johnbellation. It conſiſts in making uſe of arguments which would apply equally well to *both* ſides of the queſtion, and which of courſe ſerve at one and the ſame time to *prove* any thing and to *diſprove it.* The beſt inſtance of the double johnbellation that occurs anywhere is a paſſage formerly quoted in p. 20, 21. It is an argument againſt the poſſibility of life exiſting in fluids, and conſequently a proof that it exiſts only in ſolids. If we ſubſtitute *ſolids* for *fluids*, this *admirable* argument will prove equally well that *ſolids* cannot poſſibly be *alive*, and conſequently that all living bodies *muſt* be *fluid.*

III. The third ſpecies of johnbellation may be called *univerſal johnbellation.* It is founded on this axiom: Whatever miſtake has been committed by any *one* philoſopher who has written upon any particular ſcience, has been committed by *all thoſe* who have written or ſtudied that ſcience. Our readers will eaſily ſee, that this is not the leaſt important branch of johnbellation; and they can eaſily conceive how immenſely uſeful it muſt be to thoſe who wiſh to be very ſagacious, and very deeply verſed in ſcience. Our author is fully ſenſible of its great importance, and has therefore very often called it to his aſſiſtance.

By means of it, he has made John Hunter's miſtake about the diaphragm of birds the miſtake of the whole *mob* (to uſe our author's elegant phraſe) of anatomiſts and phyſiologiſts. Acccordingly he has very obligingly put the *world right* in this particular.

By means of univerſal johnbellation, the miſtake of ſome of the older phyſiologiſts about the uſe of reſpiration

ration is made the opinion of all the phyſiologiſts of the preſent day; and our author is obliging enough to put the *world to rights* alſo in that particular.

By it alſo the hypotheſis of ſome phyſiologiſts about the red globules is aſcribed to the whole of phyſiologiſts, even to thoſe who refuted them; and our author is obliging enough to put the world to rights in this particular again.

IV. The fourth and laſt ſpecies of johnbellation may be called *individual johnbellation.* It is the reverſe of the former, and is founded on this axiom: Every diſcovery which has been made, and every idea which has been ſtarted, by *any perſon* who has written on any particular ſcience, *may* be *claimed* and *appropriated* by *any other individual* who is engaged in the ſame ſcience. This ſpecies of johnbellation has been of infinite importance in the hands of our author. The individual of whom he has made choice, and in whom he has concentrated all the diſcoveries and thoughts of others in phyſiology and anatomy, is, as was moſt fit and proper, *his* illuſtrious *ſelf.*

By this ſpecies of johnbellation, the deſcription of the valve of Euſtachius, given by our author, is *his own;* the account of the irritability of the heart is *his own;* the refutation of Hewſon and Hunter is *his own;* the analyſis of the blood is *his own;* the experiments made concerning reſpiration are *his own;* the account of the reſpiration of birds is *his own;* the account of the reſpiration of amphibia is *his own;* the account of the reſpiration of fiſhes is *his own;* the anatomy of inſects is *his own;* and the account of

the circulation of the fœtus, and the uſe of the placenta, are all *his*, and *his alone*.

Such are the unſpeakable advantages which reſult from the judicious uſe of individual johnbellation. Our author without it might have paſſed for a compiler or a collector; but by his ſkill in individual johnbellation, together with a judicious uſe of univerſal johnbellation, and the other two ſpecies of johnbellation formerly deſcribed, he has raiſed himſelf to the rank of an original and profound writer; and has demonſtrated to the world, that he is poſſeſſed of more knowledge and more ſagacity than all the anatomiſts and phyſiologiſts who have preceded him. Reader, if you are poſſeſſed of a ſpark of ambition, ſpend your days and your nights in the ſtudy of johnbellation; and endeavour, by a judicious uſe of that noble art, to become one day as *great* a man as our celebrated and illuſtrious author.

But we return from this digreſſion, which we hope our readers will forgive, on account of the great importance of the information which it contains.

" Could we have ſuppoſed that it (the atmoſphere) " was the cauſe, not merely of life in all living crea- " tures, but almoſt the cauſe of all the properties that " reſide in the moſt ſolid forms?" (P. 107.) We at leaſt could never have conceived that it was the *cauſe* of *life*, becauſe we do not *believe* that it is the cauſe of life. And, by the bye, is it not ſingular that *no fluid* can have life, as we have ſeen our author formerly prove, and yet that a fluid ſhould be the cauſe, and the only cauſe, of life? That *our atmoſphere ſhould be almoſt the cauſe of all the properties that reſide in the*

moſt

moſt ſolid forms, we never could have conceived, nor can we conceive it at this moment, becauſe we do not underſtand the meaning of it.

" Combuſtion is a proceſs which conſiſts in the rapid aſſumption of the baſis of pure air, and the conſequent *converſion* of the burning body into an air endowed with peculiar qualities and powers." (P. 109.) Every tyro knows, that it is not true that all bodies are converted by burning into an air. Why then does our author affirm that they are?

" Muſt it not be preſumed, that the principle which " gives an increaſe of weight, and ſuch ſingular pro- " perties to metals, have very intereſting effects on " the blood?" (P. 110.) Surely; and muſt not acids, which give an *increaſe of weight*, and *ſuch ſingular properties* to metals, have very intereſting effects on the blood?

"From this principle (oxygen) *all* acids are formed." (P. 110.) This has never yet been proved, but we ſhall not diſpute about it.—" And as *oxyd* is the Greek " name for acid." Oxyd is not a Greek word at all, and if it were, the Greeks had no word to ſignify *acids*; for the beſt reaſon in the world, they were not acquainted with them. They had indeed a name for vinegar, and an adjective ſignifying *ſour*, from which *oxyd* has by a ſlight change been obtained. We would not have mentioned this blunder at all, had it not been for the eagerneſs which our author diſplays on all occaſions to ſhow his learning, by explaining the meaning of words borrowed from the learned languages. He is even more unfortunate on other occaſions than we have found him at preſent, ſometimes miſtaking

ſtaking the meaning of the word altogether, and even the language from which it is taken.

In page 111, we are told that the ancients miſtook *azotic air* for *their phlogiſton.* We are utterly at a loſs what meaning to affix to the term *ancient* in this paſſage. We know of no ancient or modern who made this miſtake. The phlogiſtic theory was introduced into chemiſtry ſince the beginning of the preſent century; conſequently *the ancient* to whom our author alludes muſt have lived ſince the year 1700. Nay, farther, phlogiſtic air was unknown before 1770, and conſequently this ancient opinion cannot be 30 years old.

"In burning arſenic we have combuſtion, calcina-
" tion, and generation of acid, all in one proceſs; the
" product being named indifferently *oxyd of arſenic,*
" or *white calx of arſenic.*" (P. 111.) If this be the product, where is the generation of an acid? The *oxyd* of *arſenic* is not an *acid.*

"This principle (oxygen), which beſtows weight
" and cauſticity on metals, acidity on acid baſes, and
" new properties on all it touches, *muſt* make ſimilar,
" or at leaſt important, changes on the blood, con-
" verting it into an oxyd or ſubacid; and we may
" fairly begin our next general fact under the title of
" the oxydation or oxygenation of the blood." P. 113.

Our author has now come to the effect of reſpiration on the blood; and the paſſage juſt quoted contains the proofs of that effect. It is therefore of importance. Let us reduce it to the form of a ſyllogiſm. Oxygen beſtows *weight* and *cauſticity* on metals (*does it?*), acidity on acid baſes, and new proper-

ties

ties on *all* (what ALL?) it touches; therefore it MUST make *ſimilar*, or at leaſt *important*, *changes* on the blood, converting it into an *oxyd or ſubacid;* therefore we may fairly begin our next general fact under the title of the OXYDATION or OXYGENATION of the blood. Such is our author's argument for the *oxydation or oxygenation* of the blood. It is an admirable ſpecimen of double johnbellation: indeed it is as perfect a double johnbellation as can well be conceived; for it would apply with equal facility, and with equal effect, *mutatis mutandis*, to prove or to diſprove any thing whatever.

Our readers, all of them at leaſt who are not total ſtangers to chemiſtry and phyſiology, know, that concerning the changes produced upon the blood by reſpiration there are two opinions: One, that no oxygen enters into the blood, but that the change of venous into arterial blood is owing to the extrication of a quantity of hydrogen and carbon from it in the lungs: that theſe bodies combine with part of the oxygen of the air inſpired, and form with it water and carbonic acid—The other, that oxygen actually enters into the blood in the lungs, combines with it during the circulation, and is again extricated when it returns to the lungs.

The firſt of theſe opinions has been adopted by Crawford, Lavoiſier, Gren, Seguin, &c. and ſupported by the moſt accurate and expenſive experiments which have been made on the ſubject. The latter opinion has been adopted by La Grange, La Place, Haſſenfratz, Girtanner, &c. and has alſo been ſupported by very ingenious experiments. The difficulty of examining

mining this ſubject with accuracy is exceedingly great; and we do not think that all the experiments which have been made, numerous as they are, entitle any perſon to conſider the queſtion as decided. Accordingly, thoſe phyſiologiſts who have paid the greateſt attention to the ſubject, and who of courſe are beſt qualified to judge of it, conſider it as ſtill *ſub judice.*

Our author, however, decides upon it very peremptorily; and we have ſeen already the force of the proofs, by which the reaſonings and experiments of Crawford, Lavoiſier, and Gren, have been refuted and laid aſide.

Perhaps what he ſays in page 115 may be conſidered as additional proofs, and indeed they are very pretty ſpecimens of *double johnbellation;* but we do not think them equal to the paſſage we laſt quoted.

After being thus completely convinced by our author of the *oxydation or oxygenation* of the blood, we happened to turn over to pages 117 and 121, and the following paſſages ſtruck us: "We call this proceſs, "NOT the *oxygenation*, but the *oxydation* of the blood, "becauſe we are conſcious that it is an imperfect "proceſs—it is ſo imperfect, that we put it into the "*loweſt* point of *ſaturation*, and *call* it (what *it?*) an "*oxyd* or *imperfect acid*; and how far it may be *be*-"*low* the denomination even of an *oxyd* we do not "know." (P. 117.) "It is *not* a fair nor permanent "*oxydation*—the oxygen ſeems but *ſlightly* attached "to the blood; it is not ſo much *united* with the blood "as *conveyed* by it." (P. 121.)

We

We look upon theſe paſſages as exceedingly beautiful ſpecimens of *negative* and *poſitive* johnbellation. We regretted only that our author had left us at a loſs to know whether by reſpiration the blood be *oxygenated* or *oxydated*, or *converted* into an *oxyd* or *ſubacid*, or whether *any change* be produced upon it at all in the lungs. But on turning back to the 113th page, we have been fortunate enough to find our doubts completely removed. "Nature," he ſays, "diſregarding all "occaſional ſupplies, has appointed one great organ for "the OXYGENATION of the blood, viz. the lungs." This was certainly very kind in Nature; and the more ſo, as we conſider ourſelves as in ſome meaſure beholden to her for the removal of our doubts.

"When we expoſe blood to oxygen gas, the *pureſt* "of all airs (*is not azotic, or carbonic acid, or hydro-*"*gen gas, equally pure?*), it grows extremely florid; "and whenever it changes its colour, it is by abſorb-"ing oxygene or pure air; for it reduces in the ſame "proportion (*with what?*) the quantity of air." (P. 114.)—Theſe aſſertions are directly contrary to the experiments of Seguin and Gren, and we may add, too, of Lavoiſier, Prieſtley, and Crawford. We would wiſh therefore to know upon what authority they are founded.

The next paragraph contains ſeveral experiments of Prieſtley and Menzies, a good deal diſfigured: and by the rules of *individual johnbellation*, to which our author on all occaſions adheres very cloſely, the names of the authors are omitted, and our author ſpeaks in the *firſt* perſon.

Our author now comes to the conſideration of the *heat* of the blood. He had affirmed in page 108, that heat is produced during *reſpiration*; and he had told us in page 116, that when an animal breathes oxygen gas, *the univerſal heat* of its body is *greatly increaſed*. But as his object in this place is to refute the theory of Dr Crawford, he has very properly called in negative and poſitive johnbellation to his aid; and accordingly he begins the ſubject with this obſervation: "There are *ſome* who *pretend* to ſay, that when they "*draw in vital air*, they feel a *genial warmth* in the "breaſt, diffuſing itſelf over all the body. But it is "eaſy to *feel* in *this* way, or *any* way, when a favou-"rite doctrine is at ſtake, while thoſe who *know no-"thing about doctrines* breathe vital air *without any* "peculiar feeling which they can explain."

Having thus happily begun the ſubject in due form, he proceeds to refute Dr Crawford's theory by the following arguments, which we ſhall take the liberty of examining.

1. The oxydation of the blood *out* of the body produces *no heat*, conſequently it ought to produce *no* heat *in* the body. (See p. 117.)

We are ſomewhat at a loſs to diſcover the meaning of this argument. Does it ſuppoſe that the *ſame change* takes place in the blood when expoſed to the air out of the body as when in the lungs? If ſo, we ſhould take it kind if our author would *produce* the *proofs* which led him to form ſuch a concluſion. They would remove all the difficulties which have hitherto perplexed the ſubject of reſpiration. If the author has no ſuch proofs, as we ſuſpect ſtrongly from his not having

having produced them, his argument is of *no* weight; becauſe there are a thouſand chances to one that the blood does not undergo the very ſame changes when out of the body as when in the body. Nay, farther, we would adviſe our author, before he affirms ſo confidently that *no heat* is evolved by expoſing the blood to oxygen gas, to re-examine all the circumſtances. If he does ſo fairly and ſkilfully, we ſhall venture to predict that he will not again make ſuch confident aſſertions.

2. Our author's ſecond argument is, that "to ſup-"poſe but for a moment that all the heat which warms "the whole body emanates from the lungs, were a "groſs error in philoſophy. It were to ſuppoſe an "accumulation of heat in the lungs equal to this vaſt "effect of heating the whole body." (P. 118.)

This argument is ſtill worſe, if poſſible, than the former. It goes upon the ſuppoſition that Dr Crawford taught, that all the heat neceſſary to continue the temperature of the body is evolved in the lungs during reſpiration, and is from thence diſtributed to the whole body, preciſely as if a fire or a candle were placed in the lungs. Such an opinion would indeed be unphiloſophical, but it is very far from being Dr Crawford's opinion; and therefore its being unphiloſophical, is no argument whatever againſt the theory of that very ingenious philoſopher.

Dr Crawford's theory is this: The capacity of arterial blood for heat is greater than that of venous blood, yet its temperature is the ſame; conſequently it *muſt* contain more heat. Venous blood is converted into arterial blood in the lungs; and ſince its tem-

perature is not diminiſhed, it *muſt* receive heat in the lungs. Arterial blood is converted into venous blood during its circulation; therefore it *muſt* gradually give out heat during its circulation. It is this gradual evolution of heat that maintains the temperature of the body.—Such is an abſtract of the theory of Dr Crawford: we do not ſay that it belongs to him originally, but we are indebted to his labours for the facts by which it is ſupported. Theſe facts, allowing their truth, render it invulnerable; and it is not by miſrepreſenting it and railing at it, but by examining the facts which form its baſis, that it muſt be either eſtabliſhed or refuted.

One of theſe facts our author has conſidered in the following terms: "Dr Crawford was extremely anxi-
"ous to prove, that in proportion as air was changed
"by reſpiration, it gave out its heat to the blood;
"he alſo wiſhed to put reſpiration and combuſtion on
"one level; and by this ſecond thought he forgot
"entirely what he firſt had in mind to prove. Ac-
"cordingly, having incloſed a Guinea-pig in pure
"air, and under water, he found that the air which
"it had reſpired communicated nearly the ſame heat
"to water that burning the ſame quantity of air ſhould
"have done: by which he proved much more than
"he intended; he proves plainly by this, that all the
"heat which reſpiration can poſſibly generate is by
"the fixed air carried from the lungs, and he forgot
"to reſerve any for going into the blood." (P. p. 120, 121.)

Now ſo different is our opinion concerning this experiment of Dr Crawford from that of our author, that

that we conſider it as a very ſtrong argument in favour of Dr Crawford's theory. The temperature of hot blooded animals is conſiderably above that of the ſurrounding atmoſphere; therefore they muſt be continually giving out heat to the ſurrounding bodies. But their temperature is conſtant; therefore they are continually receiving a quantity of heat juſt equal to what they are giving out. In Dr Crawford's experiment, the water muſt have been heated partly by the warm air which the animal expired, and partly by the heat which was continually paſſing out of all parts of its body. Now as the temperature of the animal would continue the ſame, it muſt have been conſtantly receiving a quantity of heat equal to that which it was loſing. If therefore the water was raiſed exactly to the degree of heat to which it would have been raiſed by the combuſtion of the ſame quantity of oxygen gas which was conſumed in reſpiration, it follows, that the oxygen actually parted with all the heat loſt, and that therefore a quantity of heat exactly equal to what the animal *loſt* during the experiment muſt have been furniſhed it by reſpiration; which is the very thing that Dr Crawford wanted to prove.

This is the only experiment of Dr Crawford which our author has thought proper to examine. But he tells us that theſe experiments were very ill made; that they proceeded upon very fantaſtical and abſurd laws; that they were much fitter for a magician than a philoſopher to undertake; that the intricacies of Dr Crawford's theory are its beauties; that it is a hypotheſis illuſtrated by experiments, which have no other tendency than to make it look well in the face,

and

and which are made with ſuch affectation of niceneſs as is completely ludicrous; that he begins his doctrine with a *petitio principii;* and that his main experiment is wrong*.

Such is the ungenerous and unmanly language in which this writer chuſes to ſpeak of the labours of Dr Crawford; one of the moſt amiable and ingenious men whom the preſent century has ſeen. His theory is ſo completely miſrepreſented, and the ſmall number of his experiments which Mr Bell has thought proper to mention are ſo wretchedly ill ſtated, that it is not poſſible for us to ſuppoſe that Mr Bell has ever read Dr Crawford's book.

It is not worth while to examine our author's explanation of the production of animal heat. His opinion, if he can be ſaid to have any opinion at all, coincides with the hypotheſis of La Grange and La Place, as illuſtrated by Haſſenfratz and Girtanner. In his attempts to eſtabliſh this opinion, he contradicts every thing which he had ſaid in his refutation of Dr Crawford, and adopts the very ſame firſt principle which he had a few pages before vilified as a *petitio principii.* For he lays it down as a law of nature, that all bodies, on paſſing from a fluid to a ſolid form, give out heat; yet in page 119 he ridicules Dr Crawford for ſuppoſing that fleſh, rye, barley, &c. contain leſs heat than blood.

In page 122 he affirms, that a part of the oxygen gas inſpired combines in the lungs with inflammable air.

* This is the experiment which we have juſt conſidered.

air. Where *this air* comes from *we* cannot conceive. In page 125 he tells us, that the *acidum pingue* is the ſame with the acid of fat. Every body knows that the *acidum pingue* was a hypothetic acid of Meyer, which has no connection whatever with the acid of fat.

The laſt ſection of this long chapter is entitled, *Of the Reſpiration of Plants.* It contains the following propoſitions: 1. Water is compounded of oxygen and hydrogen. 2. The ſtructure of plants is perfectly ſimple. 3. Plants abſorb and decompoſe water. What theſe facts have to do with the reſpiration of plants, we cannot conceive. By the bye, our author's proof of his ſecond propoſition, that the ſtructure of plants is perfectly ſimple, is an excellent double johnbellation, and might be employed with equal ſucceſs to prove *the perfect ſimplicity* of the ſtructure of animal bodies, or to prove that the ſtructure of vegetables is exceedingly *complex.*

Our remarks upon this chapter have been rather long, and probably our readers will be fatigued; but the following pretty little ſtory, which we have ſelected with great care and after infinite reſearch, will, we doubt not, recover them entirely.

The Looking Glass.

A bear of ſhag and manners rough,
At climbing trees expert enough;
For, dext'rouſly, and ſafe from harm,
Year after year he robb'd the ſwarm.
Thus thriving on induſtrious toil,
He glory'd in his pilfer'd ſpoil.

This

This trick ſo ſwell'd him with conceit,
He thought no enterpriſe too great.
Alike in ſciences and arts,
He boaſted univerſal parts;
Pragmatic, buſy, buſtling, bold,
His arrogance was uncontrol'd:
And thus he made his party good,
And grew dictator of the Wood.
The beaſts, with admiration, ſtare,
And think him a prodigious bear.

REMARKS

REMARKS

ON

Mr JOHN BELL's ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. III.

We come now to the third chapter, which is intitled OF RESPIRATION, and which may be conſidered as the moſt perfect ſpecimen of *univerſal* and *individual johnbellation* in any language. We therefore recommend it moſt earneſtly to the careful ſtudy of every ingenuous young man who is ambitious to excel in that noble and important art.

The diviſion which our author has adopted in this chapter belonged originally to a celebrated French writer whom he has not mentioned, as far as we recollect, in his whole book.

"It is now full time," ſays he, "to *correct* many "*miſtakes* into which *modern* as well as *ancient* authors "have wandered from want of general principles, "and from want of anatomical knowledge. I ſhall "endeavour to make this chapter *intereſting* and "*ſhort.*" (P. 134.)

After this excellent commencement, our author enters upon a refutation of thoſe anatomiſts who thought that the lungs are poſſeſſed of a muſcular power.

Malpighi, Thurſton, Swammerdam, &c. had trodden the ſame ground before him; and all anatomiſts have long known that the lungs poſſeſs no ſuch power. Our author has with great propriety omitted to mention all this, and has introduced no anatomiſt or phyſiologiſt, except thoſe individuals who entertain erroneous opinions concerning the nature of the lungs. He is therefore to be reckoned the *firſt* perſon who explained the *real ſtructure* of the lungs. This is one great miſtake which our author has corrected by virtue of individual and univerſal johnbellation.

The only unlucky circumſtance is, his obſerving that man, and all animals that *breathe by a diaphragm*, have heavy lungs of a ſtrong *fleſhy* texture (p. 137.) This would lead one to ſuppoſe our author an advocate for the opinion which he had juſt ſo *ably* and ſo *modeſtly* refuted; for if the lungs be of a *fleſhy texture*, they *muſt* be *muſcular*. But this ſmall ſlip of our author vaniſhes into nothing when contraſted with the admirable individual johnbellation which follows immediately after, by means of which he has made himſelf the *firſt* perſon who has given an accurate account of the diaphragm, and of the manner in which reſpiration is performed in man.

"Forſaking for a moment *authority* and minute "anatomy," ſays he, "let us explain it in the ſhort-"eſt and moſt intelligible way." (P. 137.)

The greateſt part of the ſecond ſection is employed in refuting a ſingular opinion of John Hunter, viz. that fowls breathe by the help of a diaphragm. This opinion our author has aſcribed to all anatomiſts and phyſiologiſts. "Until I ſet this point to rights," ſays he

he, " *my* arrangement" (that is, *my* arrangement by virtue of individual johnbellation) " is good for no-" thing." (P. 139.)

After our author has corrected this ſecond miſtake into which *all writers ancient as well as modern* have fallen, he proceeds to give an account of the reſpiration of birds; and, by virtue of individual johnbellation, to which our author, as uſual, has had recourſe, this account is entirely a *new account*, and was *never* before conceived by any writer either ancient or modern. The fact, however, is, that the ſame account had been given by Swammerdam and many other philoſophers; and that the reſpiration of birds was familiar to all anatomiſts and phyſiologiſts who were not (to uſe a johnbellation) totally ignorant of anatomy and phyſiology.

In the third ſection our author gives an account of the reſpiration of amphibia, or of thoſe animals *which are ſaid in the firſt chapter not to breathe at all.* This account he has alſo rendered his own by individual johnbellation. A hundred years ago it belonged to Swammerdam and Malpighi. We ſuſpect, however, that our author has only paid attention to theſe amphibia upon paper, and not very much even there. He has ſelected the frog as an inſtance of the reſpiration of theſe animals, and has placed at the beginning of his chapter of reſpiration a drawing which, he ſays, repreſents the frog's mouth. But of the frog of this country it certainly is no accurate repreſentation. " At (*a*)" he ſays, " is ſeen its " tongue of *prodigious length;* it is *hinged*, not like " the tongue of any other creature, far back in the

" mouth, but is fixed in the chin to increaſe its length;
" at the further end it is forked. We ſee it launch-
" ing out this *monſtrous tongue in catching flies; per-*
" *haps alſo with this it rakes mud.*" (P. 146.)

This ſublime deſcription cannot ſurely apply to our Britiſh frog! He is at great pains to inform us, that the frog, which *launches out this monſtrous tongue*, never opens its mouth, and that it always keeps its mouth under water; facts which every ſchool-boy (to uſe a Scotticiſm) knows to be falſe; and thinks it a very peculiar property in that animal that it breathes through its noſtrils. Now we always thought that this had been the caſe with man and all other animals that have noſtrils. Though this ſection is intitled *Of the Reſpiration of* AMPHIBIÆ, our author inſiſts that theſe animals are *not amphibia*; that their being able to live for a conſiderable time under water is no proof that they are, becauſe they will live as long without their heart or their head. Now frogs may be made to live for weeks, and even for months, under water; but who ever heard of a frog living for weeks or for months without its head or its heart? He tells us farther, that it is the nature of the lungs of theſe animals to *oxygenate* but a ſmall quantity of blood, and that they have not the ſame occaſion for reſpiration. This may be the caſe; and if ſo, they ought to be amphibia. But our author's proof is not to the purpoſe. He argues from the ſmall quantity of blood which is ſent to the lungs at a time; but Dr Hales has ſhewn, that the blood circulates in the lungs of a frog 43 times faſter than in the muſcles; conſequently, ſuppoſing that only 1-43d part of the blood propelled by the

heart

heart went to the lungs, all the blood might paſs through the lungs for every time that it circulated through the body.

The whole of the fourth ſection, in which our author treats of the reſpiration of fiſhes, belonged originally to Boyle, Swammerdam, Willis, and Monro. Our author, however, has appropriated it to himſelf by individual johnbellation.

But the moſt ſingular ſection in the whole chapter is the fifth, on the reſpiration of inſects. The whole of it is taken very faithfully from Swammerdam, with the precaution of altering the language and the arrangement. It is illuſtrated with nine or ten figures, all of which except two are taken from Swammerdam. Yet our author has adhered ſo ſtrictly to the rules of individual johnbellation, that he not only never mentions Swammerdam's name, but expreſſes his aſtoniſhment that theſe ideas never occurred to any writer before himſelf. "I only mention difficulties," ſays he, "which it is ſurpriſing that others have not "declared and inveſtigated."

We only regret that our author was totally ignorant of the numerous diſcoveries which have been made in this branch of natural hiſtory ſince the days of Swammerdam; for then he would have been able to explain thoſe difficulties which *nobody has declared and inveſtigated;* and he might have elucidated the function of reſpiration by the application of ſeveral very important facts, which, if followed out, might lead to a method of deciding at leaſt a part of the difficult queſtion concerning the changes produced upon the blood in the lungs.

We

We are ſorry, too, that our author has not ſtudied Swammerdam with greater care; he would not then have talked of the *air veſſels* of *ſnails*. We are ſtill more ſorry to ſee him affirm, that inſects deſtroy proportionally more air than large animals, and that many inſects live beſt in the *fouleſt* air; becauſe theſe aſſertions entirely deſtroy the effect of the beautiful johnbellation contained in this ſection, by pointing out too clearly to the reader that the author is writing about a ſubject of which he is *totally ignorant*. Had it not been for theſe unlucky ſlips, and one or two more; ſuch as "bags reſembling the *alga marina* or *ſea-weed* in "ſhape," and "*rigid* tubes like a *flexible* catheter," we would have conſidered this ſection as the boldeſt and moſt excellent individual johnbellation in the book, and would accordingly have recommended it to the careful ſtudy and imitation of the aſpiring reader.

The author ought to have concluded this chapter with the following

PROCLAMATION.

We hereby prohibit all our readers and pupils from looking into the works of Mayow, Swammerdam, Haller, Monro, Sabatier, or any other anatomiſt or phyſiologiſt whatever. All THEIR *opinions* and *diſcoveries* are henceforth to be conſidered as OUR *opinions* and *diſcoveries*. Our will and pleaſure therefore is, that their names be eradicated from the catalogue of philoſophers, and that our name be ſubſtituted in their place. We alone are the only phyſician, and ſurgeon, and author: We are the ſtaff of Moſes converted into

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a ſerpent, which has ſwallowed up the ſerpents of the magicians: We are our own ſupra-ſcapular artery, formerly unknown, but now grown ſo large as to annihilate all its fellows *.

Given at our Anatomical Theatre, Surgeons Square, this — day of ——, —— years.

We ſhall finiſh our remarks on this chapter with the following little ſtory, which has been carefully tranſlated from the original Greek.

" A daw that would fain appear finer than her " companions, decked herſelf with peacocks feathers, " and all the other gay feathers that ſhe could find : " ſo ſhe would not ſtay any longer with birds of her " kind, but muſt needs go among the peacocks and " other fine birds ; but as ſoon as they diſcovered the " cheat, they fell a pulling of her : and when every " bird had taken his own feathers away, the ſilly daw " was ſtript to the ſkin, and nothing left to cover her " nakedneſs."

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* See pages 77 and 356.

REMARKS

REMARKS

ON

Mr JOHN BELL's ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. IV.

THE fourth chapter, which treats OF THE PECULIARITIES IN THE CIRCULATION OF THE FOETUS, is by no means deficient in very happy and not inelegant johnbellations.

The circulation in the fœtus was more than a century ago very accurately explained by Harvey; and we do not think that any addition of conſequence has been made, or rather remained to be made, by ſubſequent authors. We ſhall not therefore enter minutely into our author's account of it, but content ourſelves with a few remarks.

He begins with giving us reaſons why the whole blood of the fœtus is not ſent through the lungs. "Pehaps," ſays he, "it might rather be contamina-"ted there." (P. 170.) Why *there*, pray, rather than in any other part of the body?

"The ductus venoſus," he ſays, "is the part moſt "difficult to be underſtood, and never without the "help of a plan." (P. 172.) Paſſing by the *language*, which is *barely intelligible*, we ſincerely wiſh that

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Mr Bell had given us the plan by which he himſelf firſt underſtood it; for neither the *plan* nor the *deſcription* which he has given us convey any very preciſe ideas. Not to mention his having in his plan converted the right ſide of the liver into the left, and the left into the right ſide, we ſuſpect that he has been ſomewhat miſled by copying from a dried preparation; for certainly the angle at which he makes the umbilical vein and the vena portæ meet, is very different from the real ſtate of things; and it was probably this preparation which led him (p. 175.) to ſuppoſe, that after birth the blood goes through the ſame veſſels in a retrograde courſe. In his deſcription, we are told in one place, that "the umbilical vein enters the "liver at the top of the great *tranſverſe cleft*, which "divides the liver into two lobes;" and in another, that "it enters the liver at the great *longitudinal* cleft, "which divides the liver into two parts." How is it poſſible to know the *direction* of the *cleft* from this deſcription? In one place, we are told that the ductus venoſus joins the largeſt of the *hepatic veins;* and in another, that it "carries the blood directly to the "back of the liver, or that part which touches the dia-"phragm, and *there* the *ductus venoſus* ENTERS *the* "*heart.*" What are we to make of this?

Our author has diſplayed unuſual addreſs in his account of the circulation in the fœtus. He declines all diſputes, he tells us, about the nature of this circulation. At the ſame time he takes care to give ſuch an account of theſe very diſputes, as naturally leads his readers to ſuppoſe that the generality of phyſiologiſts have hitherto been miſtaken. The fact is, that he

hypotheſis of Mr Mery is nearly a century old; that it was oppoſed at the time by Duverney and Verheyen; that it was entirely refuted 40 years ago by Haller; and that the account which Mr B. gives as the true one, and which he wiſhes to paſs for new, has been, ever ſince the days of Harvey, the general opinion of anatomiſts and phyſiologiſts. Harvey has himſelf deſcribed this circulation with great accuracy; and has particularly mentioned, that the two ventricles in the fœtus act as one, and that both their forces are conjoined in propelling the blood through the body of the fœtus.

Our author, after thus ſettling the circulation in the fœtus, paſſes to the reſpiration in adults. "The mi-"ſtake which all phyſiologiſts have fallen into," he ſays, "is this, *they* have *not* obſerved that *no creature* "*can live* with a *ſingle heart* which has the oxydation "of its blood performed by *lungs*." (P. 186.) If this be a *miſtake*, it is the miſtake of nature, and not of phyſiologiſts; for the frog, the lizard, and many other amphibious animals, have only a *ſingle heart*, and yet the oxydation of their blood (as our author chuſes to call it) is performed in the *lungs*.

After this promiſing beginning, our author proceeds to *prove* that the placenta ſerves the fœtus for lungs. This proof exhibits a very pretty individual johnbellation. The hypotheſis belongs to Mayow; but our author ſeems to have got it at ſecond hand.

"One great miſtake," continues our author, "runs "through the whole of phyſiology. It has been uni-"verſally believed, that the free and eaſy tranſmiſſion "of the blood was the chief uſe of the lungs, as if "they

" they had acted like fanners to flap on the blood " from the right to the left ſide of the heart. They " affirmed, that either continued diſtention, or conti- " nued collapſe, hindered the progreſs of the blood; " and they alſo believed univerſally, that if but the " ductus arterioſus or foramen ovale, or any thing, in " ſhort, were left open to let through the blood, that " perſon might live in ſpite of hanging, drowning, or " ſuffocation of any kind." (P. 188.) This is a very bold *univerſal johnbellation.* The fact is, that ſince the diſcoveries of Prieſtley no phyſiologiſt has been ignorant that the lungs ſerve other purpoſes than thoſe juſt mentioned by our author. Nay, the real uſe of reſpiration was ſuſpected by ſome phyſiologiſts before the diſcoveries of Prieſtley, as any perſon may convince himſelf by reading the tract of Mayow on that ſubject, and by conſulting ſeveral parts of the writings of Dr Whytt. Thus our author has aſcribed an opinion to all phyſiologiſts which ſcarcely a ſingle phyſiologiſt has believed for at leaſt theſe twenty years.

Philoſophers have indeed believed, and continue to believe, that both the *collapſe* and *over-diſtention* of the lungs oppoſe the eaſy paſſage of the blood through them; becauſe a great number of experiments have demonſtrated that this is actually the caſe. Our author has thought proper to deny this fact, and to affirm in page 195, that the experiments ſo often repeated by Hooke, Croone, and others, in confirmation of it, are not to the purpoſe. This aſſertion is ſufficient to ſhew us what degree of attention our author has paid to ſubjects on which he decides with ſo much confidence, and to let us ſee how much credit his affirmations deſerve.

About the end of laſt century, the effect which compreſſing and dilating the lungs has upon the circulation of the blood through them, was keenly debated in the Royal Society: And the *very opinion* which Mr B. has *advanced as new*, and which he has ridiculed Dr Hooke for having oppoſed, was *ſupported by Dr Hooke* with great keenneſs: and the *very experiment* which our author has derided was made by Dr Hooke, in order to demonſtrate that the compreſſion or dilatation of the lungs has *no effect* whatever upon the motion of the blood through them, and that this motion is impeded by the abſence of pure air, and by that alone.

Had our author conſulted the 5th volume of the Edinburgh *Medical Eſſays*, page 806, which, as a member of the Royal College of Surgeons, he ought naturally to have read, he would have ſeen Dr Hooke's experiment diſtinctly ſtated, and ably refuted. And had he peruſed Dr Stevenſon's ſenſible and ingenious paper, he might have eſcaped a great number of very aukward literary blunders into which he has fallen.

The experiment of Hooke was ſhown by his opponents to be inaccurate, and the experiments of Haller have ſince rendered it an incontrovertible fact, that both the collapſe and over-diſtention of the lungs impede the circulation of the blood through them.

Whether the contraction and diſtention of the lungs by ordinary reſpiration be ſufficient to produce ſuch effects, is a different queſtion. Our author decides it in the negative. "The lungs," he ſays, "do not "*collapſe* by expiration in any *ſenſible* degree." (P. 192.) Yet he allows himſelf in p. 170, that the *collapſe*

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of the lungs may prevent the circulation in the lungs of the fœtus; and from his own data it follows, that at every expiration the bulk of the lungs is diminiſhed one-fifth, and that in forced expirations their bulk is diminiſhed one-half. Now, whether the fifth part, and the half of 220, be ſenſible quantities or not, we leave our readers to judge. But we have another queſtion, which Mr Bell will be good enough to anſwer. When a perſon deſiſts from reſpiration, how is it that his face becomes livid and turgid? Certainly the venous blood accumulates in it, and conſequently the reſpiration is impeded. Now what occaſions this? He will ſay, perhaps, the blood cannot be oxydated, and therefore it cannot ſtimulate the heart. This does not remove the difficulty: For, in the firſt place, the heart ſtill continues to act, and therefore muſt be ſtimulated; and, in the ſecond place, there is a conſiderable quantity of air in the lungs, which, as we learn from the experiments of Fontana, contains enough of oxygen gas to produce the uſual changes on the blood for a longer time than a perſon *can* continue without reſpiring. Why then does not the uſual quantity of blood paſs through the lungs? We have no doubt that our author is both able and willing to give a ſatisfactory anſwer to this queſtion.

But our author has uſed another argument on this ſubject, which it is but fair to produce. "Is it not plain," ſays he, "to the meaneſt apprehenſion, that "if the blood moves *twice* through the lungs in expi- "ration, and *twice* during inſpiration; or, *in other* "*words*, if there be *four ſtrokes* of the artery for "each reſpiration, and if each of the four pulſes be "equally

" equally ſtrong, that the blood paſſes through the " lungs in all ſtates and conditions with equal eaſe ?" (P. 194.) Certainly, provided the *whole* blood paſſes twice through the lungs during every expiration and every inſpiration ; but if this be not the caſe (and it actually *is not* the caſe), the four pulſes furniſh no proof whatever.

Our author, near the end of this chapter, aſſerts, that Buffon, when he affirmed that puppies littered in warm milk, lived for about an hour without breathing, *impoſed* upon his readers. If he had peruſed Haller with more attention, from whom he *has taken all the facts* mentioned in this chapter ; or if he had underſtood better *his own new theory* of the placenta—he would have perhaps judged more favourably of Buffon. We would adviſe him, before he impeaches any perſon's integrity again, to *repeat* the experiment which he wiſhes to diſpute.

He had juſt before fallen a-laughing at Dr Beddoes for ſaying, that " by frequent immerſion in wa- " ter the aſſociation between the heart and lungs " might perhaps be diſſolved, and an animal inured " to live commodiouſly under water." The fact, however, is, that this *can actually be done* with regard to one animal, the frog ; ſo that it is not quite ſo abſurd as Mr Bell imagines.

As the fifth chapter contains nothing but quotations from various authors, it is not worth while to examine it ; and we do not mean to enter minutely, at preſent at leaſt, on the ſecond part. Our

readers,

readers, however, are not from this to conclude, that the ſecond part is not as rich in elegant johnbellations and profound diſcoveries as the firſt part. Almoſt the whole of it, indeed, is taken from Haller and Sabatier. We mention this circumſtance, becauſe we think that it is both for our author's honour and intereſt that it ſhould be known; as it will prevent his reader from ſuppoſing that in many inſtances he is quarrelling with his own deſcription, when he is really doing no more than quarrelling with the borrowed deſcriptions of others. For inſtance, as the full and particular deſcription which he has given of the *external circumflex artery* of the thigh is entirely borrowed, we need not be ſurpriſed to ſee him adding in his own perſon: "But to give a more ſimple notion "of this circumflex artery, it ſhould be deſcribed "thus," &c. (P. 468.)

After obſerving that the perforating arteries are extremely irregular in *place, ſize, and number*, our author is certainly not accountable for the particular deſcription of their place, ſize, and number, which follows; as it muſt be evident, after what he has ſaid, that ſuch a deſcription cannot poſſibly be his; and therefore no reader ought to be ſurpriſed when he adds: "This "minute deſcription of any *important* ſet of arteries "never conveys any clear ideas to the reader's mind," (p. 473.), and that "there is *no artery* from the pro- "funda downwards *worth naming, not even thoſe which* "*I have juſt deſcribed.*" (P. 477.)

If the accounts which our author has given of the ſupraſcapular and ſubſcapular arteries cannot be juſtified on the ſame grounds, it muſt be allowed at leaſt, that

that the bold individual johnbellations which he exhibits are, notwithſtanding ſome flaws in them, entitled to all our admiration. We ſhall preſent our readers with his account of the ſubſcapular artery.

" The ſubſcapular artery is of a *wonderful* ſize: It " is *hardly* deſcribed in books, I would ſay is *hardly* " *known* to anatomiſts. Douglas, and *moſt eſpecially* " Sabbatier, have *ſcarcely named* it, though it is in " fact *one* of the *largeſt* arteries in the body, being *as* " *large abſolutely* as the axillary artery from which it " takes its riſe." (P. 363.) What a pity that our author was not more guarded in his expreſſions. A little more caution would have made this johnbellation perfect.

But Douglas and Sabatier, it appears, from his own account, have both *named* this artery, and he *might* have added too, ſome of its branches; and the unfortunate note at the bottom of the page informs us, that it was known to other anatomiſts. " It is " named often," ſays that note, " the ſcapularis infe- " rior, or infra ſcapularis." Now anatomiſts could not ſurely name, and name often too, what they did not know. They have not indeed deſcribed it as of a *wonderful ſize*, nor as *in fact one of the largeſt arteries of the body*, nor as being *as large abſolutely as the axillary artery* from which it takes its riſe; becauſe it is neither *wonderful*, nor *in fact one of the largeſt arteries of the body*, nor *as large abſolutely as the axillary artery*. Every tyro knows, that the axillary artery muſt always contain as much blood as the ſubſcapularis and brachial artery together. But paſſing by theſe flaws, which we notice with regret, our author has

ſhewn

ſhewn great dexterity in never mentioning the name of the author from whom he has taken the whole of his deſcription, we mean Haller, who has given two moſt excellent plates of the ſubſcapular artery, ſhewing its great relative ſize, and its ſeveral branches, with their inoſculations both on the dorſum and concave ſide of the ſcapula, and who has mentioned a great number of different authors who had deſcribed this artery before his time.

The following univerſal johnbellation is *wonderfully* bold: "Though the profunda is plainly the artery "of the thigh; yet, from the ignorance of anatomiſts "and ſurgeons (*who never knew till about* 20 *years* "*ago* that there was *more* than *one* great artery), the "ſuperficial artery has been named the artery of the "thigh." (P. 474.) To be ſenſible of the merit of this johnbellation, the reader has only to conſult Haller; who informs us, that Euſtachius, and many other anatomiſts, had actually delineated it more than a century ago.

We allow that our author has been rather unfortunate in his attempts to make diſcoveries in *angeiology*; but we think that even his greateſt enemies muſt acknowledge that he has made very conſiderable ones in *myology*. Were Albinus alive, how would the old gentleman bluſh to find, that not only a number of *new muſcles*, but *new origins* and *new inſertions*, of which he never dreamed, had been diſcovered by our author. For inſtance, he tells us, that "the axillary "artery is covered by the pectoral MUSCLES, becauſe the "pectoral muſcles *ariſe* from the CLAVICLE." (P. 359.) All that Albinus knew was, that a *part* of the pecto-

ralis major aroſe from the clavicle. " The long tho-
" racic artery is more important, ſupplying *all* the great
" pectoral muſcles." (P. 360.) Albinus has mentioned only *one* great pectoral muſcle, having a fellow on the oppoſite ſide ſupplied by a long thoracic artery of its own.

We have given theſe few ſpecimens, merely to ſhew the reader what he may expect from a careful peruſal of the ſecond part. Important myological diſcoveries, and beautiful johnbellations, grow everywhere luxuriant in the greateſt abundance; and we would adviſe every intelligent and aſpiring young man of *modeſt aſſurance*, who wiſhes to be at once aſtoniſhed and improved, and who has any taſte for the *ſlender*, the *delicate*, the *very pleaſing;* the *great*, the *marvellous*, the *wonderful*, the *prodigious*, the *vaſt*, the *immenſe*, or the *abſolutely large*—to go thither, contemplate, admire them, and gather them, to adorn his brows with the moſt fragrant roſes of ſcience, and to ſatiate his appetite with its moſt delicious ſweets.

Nay, this ſecond part contains a *prodigious* number of inſtances of the *true ſublime*. The following remarks on the femoral artery, which we ſhall give by way of ſpecimen, are *moſt beautifully* ſublime and *immenſely pleaſing*.

" To enumerate all the variety of accidents which
" may affect this artery were *impoſſible;* but ſurely,
" from the little that *I dare venture to ſay* in this
" place, it muſt SEEM *one of the largeſt*, the *moſt ex-*
" *poſed*, the *moſt dangerous*, and by all this the *moſt*
" *important* artery IN THE BODY." (P. 461.)

Our reader, by conſulting the ſecond part, will ſee alſo

alſo the vaſt importance of uſing the ſuperlative degree as often as poſſible in all anatomical deſcriptions. That cold creeping ſort of language, which conveys only clear and accurate ideas, can never poſſibly affect the fancy, and it always leaves a feeble and vulgar impreſſion on the mind. Minute accuracy and nice diſcrimination ought to be laid aſide: Darkneſs, ſays Burke, heightens the ſublime.

With regard to our author's preface, we think it not inferior in beauties to any other part of the book. The 1ſt page ſhows how a man may write very fluently without any meaning whatever; and the 2d, 3d, 4th, 5th, 6th, and 7th, how he may talk very learnedly about philoſophers, and ſhow that all their writings contain nothing but abſurdities, without giving himſelf the trouble to peruſe theſe writings, or even to know the ſubjects of which theſe philoſophers have treated.

As to the attack upon the anatomical nomenclature, with which the preface concludes, we agree with our author perfectly, that anatomiſts have ſometimes from ignorance or pedantry talked in a manner not very intelligible. We are only ſurpriſed to find that he has ſervilely followed this nomenclature; although he has ſhewn in ſome inſtances, as when he ſpeaks of the *coronory* proceſs of the *lower maxillary bone*, that he has not always underſtood it.

We ſhall conclude with congratulating the Royal College of Surgeons in Edinburgh, upon the luſtre which this noble diſplay of our author's learning and wiſdom will reflect upon it. How highly will its name be reſpected abroad! How ſuperior will it appear

pear when compared with the great ſchools in other countries! Foreigners will doubtleſs conclude, that the other members of that body are not much inferior in anatomical, and more eſpecially in chemical, knowledge to our illuſtrious author. He alone has known properly how to preſerve and to keep up its dignity: He alone has known properly how to appropriate the diſcoveries of others. And ſhould any grumbling individual dare to complain, he knows how to reduce him to ſilence, by telling him, that the conſtitution of the Royal College of Surgeons gives to every one high privileges in ſpeaking.

FINIS.