OBSERVATIONS

ON THE NATURE OF

DEMONSTRATIVE EVIDENCE;

WITH AN

EXPLANATION OF CERTAIN DIFFICULTIES

OCCURRING IN THE

ELEMENTS OF GEOMETRY:

AND

REFLECTIONS ON LANGUAGE.

BY THOMAS BEDDOES,

En general, dans les mathematiques on ne s'arrête pas à reſoudre les difficultés metaphyſiques, qui ſe preſentent. . . . . M. TURGOT eût voulu qu'on diſſipât juſqu'aux plus petites obſcurités. *Vie de Turgot.*

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TO

DAVIES GIDDY, Esq.

Dear Giddy,

FOR the principal opinion, ſtated and exemplified in the following pages, it ſeems to me that I have the full evidence of intuition; and this evidence, you know, muſt always carry conviction to the mind of the individual. All he can deſire further is to learn, whether objects appear to the ſenſes of others as they appear to his ſenſes.

What additional confirmation it is poſſible in ſuch circumſtances to receive, was afforded me by your aſſent, when I formerly mentioned to you my ideas concerning demonſtrative evidence. Your uncommon proficiency in mathematical ſcience, and your no leſs uncommon diſcernment, I was well aſſured, perfectly qualified you to decide on ſuch a queſtion.

To thoſe who catch an idea the inſtant it is preſented, and who have facts in abundance at command, by which they can determine the validity of a principle, I ſhall frequently appear tedious. That ungrateful feeling, I will own to you, oftener than once while I was writing, came acroſs my mind. But you are at no loſs to conceive the manner in which I would defend my prolixity. What I have written, if it ſhould obtain regard, will be viewed by moſt readers with an eye of ſuſpicion; and

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by many, if they follow their firſt movement, it will be rejected as paradoxical. Will not ſuch conſiderations as theſe juſtify a variety of illuſtrations, and even repetitions?—They have alſo induced me to run the riſque of appearing ridiculouſly minute, in tracing the origin of terms.

Fortunately for the diffuſion of juſt ſentiments, Mr. Harris has loſt that authority which even among the learned he maintained too long. Our young men, however, I believe, ſtill frequently reſort to HERMES for that inſtruction, which he has not to ſupply. For obſerve, I beſeech you, what they will learn from this once redoubted doctor of *univerſals*, concerning mathematical reaſoning.—"It is ſome-"what remarkable," ſays he, ſarcaſtically glancing at the attention paid to the phyſical ſciences, "amid the prevalence of "ſuch notions, that there ſhould ſtill re-

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" main two ſciences in faſhion, and theſe " having their certainty of all the leaſt " controverted, *which are not in the mi-* " *nuteſt article depending upon experiment.* " By theſe I mean ARITHMETIC and GEO- " METRY." In a curious note, but which is too long to be inſerted entire, he has the inſolence to ſubjoin, " I would not be " underſtood, in what I have here ſaid, " or may have ſaid elſewhere, to under- " value EXPERIMENT; whoſe importance " and utility I freely acknowledge, in the " many curious noſtrums and choice re- " ceipts, with which it has enriched the " neceſſary arts of life. Nay, I go far- " ther—I hold *all juſtifiable practice in every* " *kind of ſubject* to be founded in EXPE- " RIENCE, which is no more than the re- " ſult of *many repeated* EXPERIMENTS. . . . . " In the mean time, while EXPERIMENT " is thus neceſſary to all PRACTICAL WIS-

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"DOM, with reſpect to all PURE and SPE-
"CULATIVE SCIENCE, it has not the leaſt
"to do. For who ever heard of *Logic*, or
"*Geometry*, or *Arithmetic* being proved *ex-*
"*perimentally* * *?*" That the affirmations of Mr. Harris may loſe nothing of their effect, they are here introduced in all their native pomp of CAPITALS and archneſs of *italics*.

The more I conſider the ſubject, the more I am inclined, in ſpite of Mr. Harris, to believe not only in the poſſibility, but the utility of rendering the elements of geometry palpable. If they be taught at an early age — a plan in which I think I ſee many advantages—models would make the ſtudy infinitely more engaging: From the mere ſlate and pencil moſt beginners experience a repulſive ſenſation. But if a child had ſomething to handle and to place in

* Hermes, p. 351-3.

various

various poſtures, he might learn many properties of geometrical figures without any conſtraint upon his inclinations. He would have no difficulty in transferring the properties of palpable to merely viſible figures, nor in generalizing the inferences. *You* will not object, that one cannot proceed far by this road: you will perceive, that much more would be gained in reality than appearance. We ſhould have laid a good foundation for the invaluable habit of accurate obſervation in general; and towards future progreſs in mathematics, we ſhould have warded off the firſt diſagreeable impreſſion of the aſpect of the ſcience, which is ſo very apt to ſtrike a damp to the heart of the beginner.

I need not explain to you the advantage of trying to engage Fancy on our ſide, by all the allurements we can offer to her. It is ſhe that ſmooths every path and ſtrews

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it with flowers. We all, men and boys, follow with alacrity wherever ſhe leads; neither the mind, nor the body, grudge any labour; and it is the enthuſiaſm ſhe inſpires, that has worked ſo many miracles in art and ſcience. By ſome ſtrange fatality however, ſhe is neglected, if not affronted in almoſt all the ſtages of education; and the firſt ſtep in almoſt every ſpecies of inſtruction is, to preſent knowledge to the ſtudent's imagination, in conjunction with ſome melancholy and hateful accompaniment; which ſort of management, I conceive to have much the ſame kindly influence upon this faculty, as an unſeaſonable froſt upon the tender petals of an expanding bloſſom.

The mode of initiation in geometry which I propoſe, could not, unleſs I very much deceive myſelf, fail to render the impreſſions of ſenſe more agreeable by rendering them more diſtinct. The *rigorouſly ſcien-*

tific method, as it is ſuppoſed to be, ſeems, on the contrary, to aim only at rendering them as obſcure as poſſible: an intention, I confeſs, perfectly in uniſon with the other parts of the eſtabliſhed proceſs of SCHOOL and COLLEGE ſtupefaction.

Whether you will allow that this important point is likely to be in this manner attained, I am not ſure. But you will agree with me in thinking, that it is high time to diſcard Euclid's Elements. The ſcience cannot be exhibited in a more diſguſting form; as we may be convinced by daily inſtances. Nor are theſe Elements any way neceſſary to lay a good foundation in mathematics, for there are few, I will venture to gueſs, of the eminent mathematicians of Europe, that have been initiated by the ſtudy of Euclid.

That by laying Euclid aſide, we ſhould be deprived of what Bacon calls the intervenient advantages of mathematics, is to me a vain

a vain apprehenſion. Thoſe who have dragged their underſtanding laborioufly along the tireſome circuit of ancient demonſtration, may be unwilling to grant, that they have taken all theſe pains to no purpoſe. Yet they can hardly flatter themſelves in ſecret, that they have acquired habits of attention or abſtraction ſuperior to thoſe, who having preſſed forward by the neareſt road, enjoy both the direct and indirect profits of their labour.

For want of time or opportunity or reſolution, I am not able to take ſo comprehenſive a view of the ſubject as I could wiſh. I cannot indeed ſuppoſe it poſſible, that either branch of mathematics ſhould ever ceaſe to be

Qualis ab incepto proceſſerit ;

but I am apprehenſive, that I have not preſented my obſervations in ſo advantageous a form as I might have done, if I had poſſeſſed more knowledge. I hope, however,

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to be intelligible to thoſe who may chooſe to examine them. In the main principle I cannot ſuppoſe myſelf miſtaken; where I have committed errors in the application, I ſhall receive correction with the chearfulneſs becoming a perſon, equally ready to hear and tell the truth.

I am,

With ſincere regard,

DEAR GIDDY,

Your's,

Oxford,
Sept. 6, 1792.

THOMAS BEDDOES.

OBSER-

OBSERVATIONS

ON THE NATURE OF

DEMONSTRATIVE

OR

MATHEMATICAL EVIDENCE.

IN proportion as the writings of Mr. Locke roſe in the public eſteem, Ontology and the old Logic appear to have declined: the memorable improvements in phyſical knowledge, which took place about his time, and thoſe which have ſince been made, by offering far different objects to curioſity, co-operated with Mr. Locke's endeavours

to bring that unprofitable jargon into disrepute. Such has been the happy effect of theſe conſpiring cauſes, that the world has been delivered from its long ſubjection to empty ſounds; the talent of wrangling is no longer conſidered as the grand object of education; and the means of acquiring this talent have been, by general conſent, caſt aſide into thoſe lumber-rooms of learning, THE SCHOOLS.

Mr. Locke's ſucceſs has, I imagine, already contributed, and will hereafter, in a greater degree ſtill, contribute to render his ESSAY leſs popular; for one may, without impropriety, call a book popular that has gone through twenty editions. The ſcholaſtic learning, which he occaſionally introduces on purpoſe to expoſe it, is now little familiar to the majority of his readers; and to thoſe readers who are not accuſtomed to its terms, many paſſages will be obſcure

and unintereſting. This great work contains, however, obſcurities, depending on a leſs honourable cauſe: the author ſometimes attempts to explain what he does not underſtand, and ſometimes, with wonderful unſteadineſs, half abandons his own principles. It has lately been ſhewn, that his capital miſtake was concerning language. Hence as he did not throw light enough upon this important ſubject to moleſt the grammarians in their operations, they ſtill worked on in the true ſpirit of their brethren, the ſchoolmen; and by their diſtinctions elucidated the nature of words, about as much as the ſchoolmen had advanced the knowledge of things by their ſyllogiſms.

At laſt the Επεα Πτεροεντα of Mr. Horne Tooke diſſipated the clouds left by the Eſſay on the Human Underſtanding; and the foundations of knowledge were laid fairly open to thoſe, who ſhould have the curio-

ſity to inſpect them. It was by deciſively ſhewing, that we have no general or complex ideas, and that every word in language, (interjections excepted, which are hardly entitled to the appellation of words,) ſignifies ſome object or perception of ſenſe, that Mr. Tooke compleated what Mr. Locke had begun. Mr. Locke indeed did every thing but make the diſcovery himſelf. According to his negative definition of the general idea of a triangle, "it muſt be "neither oblique, nor rectangle, neither "equilateral, equicrural, nor ſcalenon, but "all and none of theſe at once." It was eaſy, one might at firſt ſight ſuppoſe, to infer that the human mind is incapable of conceiving ſuch an idea; and hence that general terms are not ſigns of general ideas, but a contrivance to avoid a multitude of uſeleſs names, and that complex terms denote no fixed ideas like thoſe ariſing from external

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objects or impreſſions of ſenſe, but that each ſhortly denotes a number of ſimple perceptions or ſenſations, for which reaſon their import is apt to vary extremely, as they are uſed by different perſons.

In whatever ſtudy you are engaged, to leave difficulties behind, is diſtreſſing; and when theſe difficulties occur at your very entrance upon a ſcience, profeſſing to be ſo clear and certain as geometry, your feelings become ſtill more uncomfortable: and you are diſſatisfied with your own powers of comprehenſion. I, therefore, think it due to the author of ΕΠΕΑ ΠΤΕΡΟΕΝΤΑ to acknowledge my obligations to him for relieving me from this ſort of diſtreſs. For although I had often enough made the attempt, I could never ſolve certain difficulties in Euclid, till my reflections were revived and aſſiſted by Mr. Tooke's diſcoveries.—I have indeed lately learned, that a doctrine concerning language, partly the ſame as that of Mr. Tooke, had been for ſome time taught in the celebrated ſchool of Greek literature

at Leyden. Mr. Hemſterhuis, and his diſciples, had ſucceſsfully traced many words of that language to their primary *ſenſible* ſignification; and they expreſs themſelves with preciſion on the manner * of

* For inſtance, Lennep (Etymol. p. 7.) ſays: Notiones verborum *propriæ* omnes ſunt *corporeæ*, ſive ad *res* pertinentes, quæ ſenſus noſtros feriunt.—Quæ, præter verba et nomina, numerantur *partes orationes*, ea, vel ad verba, vel ad nomina, proprie referenda ſunt; niſi ſint quædam *interjectiones*. (id. ibid. p. 8.)—From Scaliger, Mr. Scheid adopts this principle: unius vocis *una* tantum eſt ſignificatio propria et princeps. (Animad, ad Analog. p. 351.)——Again, Valde veriſimile fit, vocabula ea in linguis antiquiſſima eſſe, quibus res deſignantur, ad vitam degendam neceſſariæ——

Porro non alienum erit, hic obſervaſſe non tantum ejuſmodi vocabula antiquiſſima exiſtimari debere, ſed etiam *ipſas* ſignificationes, verbis ſubjectas, tanto antiquioris uſus eſſe, tantoque magis proprias eſſe habendas, quanto ſunt propriores iis rebus, quas, corporis ſenſibus, percipimus. Ab iis enim ſemper, ſervatâ quâdam ſimilitudine, ad reliquas quaſcumque verborum ſignificationes, progrediendum eſt: ut adeo appareat pauciſſimas revera eſſe *proprias* verborum ſignificationes;

of ſignification of words. The grammatical reſearches however of the eminent Greek

ficationes; nec alias eſſe niſi *corporeas* ſive eas, quibus res ſenſibus exterius expoſitæ, deſignantur. E contrario autem, tranſlatarum ſignificationum copiam immenſam, quæ ex *propriâ* notione, tanquam ex trunco arboris rami, quaquaverſum pateant. *Lennep. Anal.* p. 41.

As Mr. Tooke has not yet ſatisfied the curioſity of the public by illuſtrating the manner of ſignification of the pronouns, and thoſe who have followed him as far as he has hitherto gone, muſt be extremely deſirous to underſtand the conſtruction of this part of language, I will ſubjoin an inſtance which perhaps may give the reader an idea how the pronouns ariſe, and what is their primary ſenſible ſignification.

TOT in Coptic ſignifies *hand*. See Woide Lexicon Ægypt. who goes on to ſay, *uſurpatur inſtar nominis poſſeſſivi*, and then quotes ſeveral Coptic phraſes, in which he tranſlates Tot by the poſſeſſive pronoun. Then by a prefix of the letter N, of which the primary ſenſe is not known, it ſignifies *to have*, *to poſſeſs*: as,

Greek ſcholars, who have flouriſhed in Holland, are, if we except thoſe of Schultens, but

"NTOT, habere, εχειν.—as Matt. xxi. 26. Ιωαννης γαρ NTOTου ὡς ουπροφηης, habent enim Johannem ſicuti prophetam."

In his Grammat. Egypt. p. 38, he ſays "TOT, mànus, NTOT, manus mea, i. e. ego, Pſalm cxxx. 8. ETOT, mihi, Syrac. xxiv. 8. DaTOTep, inter nos." With other ſuffixes it is uſed to expreſs the other perſons. Two or three other words, of which I do not find the primary ſenſe, are uſed alſo for the firſt pronoun poſſeſſive. Theſe other Coptic words might ſignify *hand* among ſome of the tribes, of which the Egyptian nation, like every other, was probably formed: or other actions, beſides taking a thing into the hand, might denote poſſeſſion, and hence a derivation of pronouns from words of other ſignification.

How the familiar action of ſeizing any thing for food or other uſe might ſuggeſt an emblematic action, as in the ceremony of marriage; how this act might alſo ſuggeſt an hieroglyphic for poſſeſſion; and how eaſily the word *hand* might ſupply ſpoken and written language

but beginning to be known abroad; and they appear to have adulterated the truth by the admixture of ſeveral highly improbable hypotheſes *.

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language with poſſeſſive pronouns, every perſon will at once perceive. Suppoſing the Coptic likely to ſupply primary ſignifications, I ſearched for this purpoſe what Dr. Woide has publiſhed of this language: he muſt be reſponſible himſelf for the accuracy of his interpretation.—OUOH ſignifies as well *and*, as *addi*, *augeri*. The coincidence of the primary ſenſe of this conjunction in Engliſh and Egyptian is ſatisfactory, but by no means ſurpriſing.

* Readers, attached to theſe ſpeculations, will find abundant entertainment in *Valkenarii Obſervationes*, &c. *Lennepii Prælectiones Acad.* and particularly in his *Etymologicum*, all publiſhed by Prof. Scheid, in 1790. A German critic, in whoſe ſtrictures one can hardly fail to recognize Mr. Heyne, has pointed out two or three of theſe extraordinary hypotheſes. See Gotting. Anzeig. 1791, p. 578, &c. He obſerves that it is juſt as probable that the great bulk of the Greek language was derived

The pretenſions of the abſtract ſciences have, it muſt be confeſſed, ſomething wonderfully alluring. In thinking they ſeem to promiſe, to ſpeculative minds, a ſort of

derived from *monads* as from the *duads*, which theſe writers and Lord Monboddo aſſign as the primitives, viz. αω, εω, ιω, οω, and ύω; for, continues he, from δω we may have δαω, δεω, δοω; and we have ſtill in the language βω, λω, νω, and other ſuch monoſyllables. This regularity, too, he obſerves in the formation of the language muſt, according to the Dutch doctrine, neceſſarily have exiſted among the Savages of Greece, a ſuppoſition, contrary to every argument, ſupplied either by phyſiology or pſychology. He might have added, that the doctrine not only implies this ſuppoſition, but that the authors expreſs it: as Valckenaer, p. 34, *primigeniæ ſignificationes a* SAPIENTIBUS ISTIS LINGUÆ CONDITORIBUS *verbis impreſſæ——labentibus annis, deleri cæperunt*, &c. ſo that Horace, if he had but known this, when he ſaid, "that many bold warriors lived before Agamemnon," might have added, "and wiſe philoſophers too, who held councils to provide

of independance upon external things, ſimilar to what ſome moraliſts have ſought to acquire in acting. And who is there, ſo immerſed in matter as to feel no deſire of ſpiritualizing the groſs body of his experi-

vide ways and means for the regular conſtruction of language!" A fact, doubtleſs, far more extraordinary.

It is alſo, as Mr. H. further remarks, very little probable that nouns were at firſt derived from verbs, as, for inſtance, that φλεγω ſhould have preceded φλοξ, or φλερυσσω, φλερυξ; abſent objects muſt have a name (hence nouns would be firſt formed), whereas actions could be imitated by geſture. It is not however to be denied, that other nouns were afterwards formed from verbs. Mr. H. ſeems to be miſtaken, in ſuppoſing that monoſyllables and diſſyllables precede polyſyllables; for, 1. In the ſpecimens of barbarous languages, of which we have lately received ſo many, you will hardly find a monoſyllable and few ſhort words; and, 2. Many imitative words muſt be long, becauſe the ſounds themſelves are long; they would be gradually ſhortened by uſe.

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mental knowledge, by infuſing into it a diviner ſpark from Ontology? Whatever be his purſuit, the ambition of that ſcholar muſt be very low in its aims, who ſhould not aſpire to catch one glimpſe of the pure eſſences of things, as they are preſented by the mirror of metaphyſics. But if it ſhould appear, that the ontologiſts have miſtaken the humble *poſteriori* for the high *priori* road; that they are juſt as dependant upon ſenſe and matter, as the mereſt experimenter; that they have laboured to no better purpoſe than to cloak the ſimpleſt indications of ſenſe in a fantaſtic garb; and that even the claim of their ſubtleties to ſerve as a whetſtone of a finer grain to the mind is groundleſs, ſince the habit of diſcrimination, as well as that of fixed attention, is to be perfectly acquired by ſtudies, that are alſo capable of uſeful application; if for ſuch reaſons as theſe we may calmly behold

hold Ontology ſinking into that grave, in which Alchemy lies buried, ſtill there will remain an abſtract ſcience, which has ſtood the competition of the phyſical ſciences, and indeed has grown with their growth. In *mathematical* reaſoning, the mind graſps the concluſions with full aſſurance of their reality; we are ſatisfied, that our advances in this ſcience are actual acquiſitions, and we find them as we go on continually capable of application.

It may therefore be intereſting to enquire into thoſe circumſtances which conſtitute the irreſiſtable force of mathematical evidence. We ſhall at the ſame time, if we are ſucceſsful in this enquiry, diſcover upon what depends the difference in the cogency of proof between demonſtrative evidence, and ſuch evidence as leſs powerfully commands our aſſent. Without this, I do not ſee how we can ever take

take a clear ſurvey of evidence in general, or enjoy the ſatisfaction of accounting to ourſelves fully for our own conviction or belief.

It ſeems to me, in the preſent ſtate of our knowledge, ſo eaſy to point out the nature of this and the other ſorts of evidence, that I wonder how it can be miſtaken. Yet frequently as the topic is expatiated upon, I know no book in which the true principles have been fully explained and applied; and in general, I have reaſon to believe that very erroneous ideas prevail upon a ſubject, of unqueſtionable importance to the theory of the human underſtanding. I might recite the opinions of a conſiderable number of writers, and offer arguments againſt them. But if I ſucceed in eſtabliſhing my own, I ſhall at the ſame time ſufficiently refute what I imagine to be the miſtakes of others; and the reader will

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at once perceive how far each is wide of the truth, for all are not equally wide.

On examining a train of mathematical reaſoning, we ſhall find, that at every ſtep we proceed upon the evidence of the ſenſes; or, to expreſs myſelf in different terms, I hope to be able to ſhew *that the mathematical ſciences are ſciences of experiment and obſervation, founded ſolely upon the induction of particular facts, as much ſo as mechanics, aſtronomy, optics or chemiſtry.* In the kind of evidence there is no difference; for it originates from perception in all theſe caſes alike, but mathematical experiments are more ſimple and more perfectly within the graſp of our ſenſes, and our perceptions of mathematical objects are clearer. So great indeed is the ſimplicity of mathematical experiments, that at whatever moment we are called upon to reaſon from them, we have the reſult of many of them diſtinctly

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in our memory; the obſervations caſually made in the courſe of life, leave a ſufficient conviction of their truth upon the mind; and we are beforehand ſo fully ſatisfied as ſeldom to take the trouble of repeating them. The apparatus is ſimple: no motion or change admoniſhes us, that we are engaged in an experimental enquiry; and this is, I ſuppoſe, the reaſon why we are ſo little aware of the nature of the intellectual proceſs we are going through. Sometimes, however, notwithſtanding we are ſo well prepared, we do repeat ſome of theſe experiments; and there have probably been few teachers of geometry, who have not, at the beginning of their lectures, deſired their pupils to repeat certain fundamental experiments, till they ſhould have ſatisfied themſelves as to the reſult.

No ſooner do we look into an elementary treatiſe for the proofs of this opinion, than

than we meet with them at every ſtep in every demonſtration; and I ſhall, I hope, be allowed to have eſtabliſhed it firmly, if I ſhew that Euclid ſets out from experiments, and proceeds onwards by their aid, appealing conſtantly to what we have already learned from the exerciſe of our ſenſes, or may immediately learn. The ſame thing muſt needs be equally true of every other elementary author. After having exemplified the nature of demonſtrative reaſoning, I ſhall leave the reader to extend this mode of conſidering it to other caſes, in a full perſuaſion that he will find the ſame proceſs repeated, in every demonſtration upon which he may chooſe to make the trial. In order therefore to avoid needleſs repetition, I ſhall paſs on, and, *ſecondly*, apply the principle to the ſolution of certain difficulties in the elements of geometry, about which a great deal has

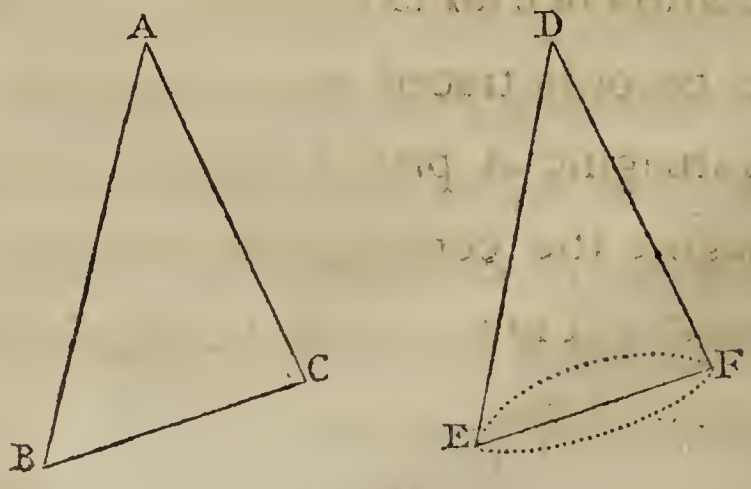
been written. It will appear, that neither the doctrine of parallel lines, nor the fifth definition of the fifth book, on which Euclid founds his reaſoning concerning proportion, have any thing obſcure or doubtful, when properly conſidered. *Laſtly*, to take off that gloſs of novelty, which ſo much ſcandalizes mankind, when truth appears before their eyes for the firſt time, I ſhall ſhew that Mr. Locke has diſtinctly announced the ſame opinion. The reader may therefore, if he chooſes, conſider what follows as a commentary, upon a paſſage of Mr. Locke, which was totally loſt upon me, and, as it appears, upon others. A good commentary would prevent or relieve much perplexity.

I am firſt to review the outſet of Euclid's reaſoning, in order to ſhew that he begins from experiments, and proceeds by experiments. Theſe experiments may, indeed,

indeed, be called *mental* experiments, ſince the appeal is made to recollection, and they are commonly repeated in thought. But they are not at all more independant of experience than my acquired knowledge of the reſult of an experiment with a balance of which one arm ſhould be ten inches, and the other one inch long, and each arm ſhould be loaded in an inverſe proportion to its length: or, to take another example, ſuppoſe a train of reaſoning were to ſet out on the ſuppoſition of iron yielding hydrogene air, while it is diſſolving in vitriolic acid of a certain ſtrength, I ſhould moſt aſſuredly not think it neceſſary to repeat the experiment. Now juſt as familiar as a ſuch reſult is to a perſon at all converſant in chemiſtry, juſt ſo familiar to every perſon, at leaſt every one arrived at the years of diſcretion, and having the uſe of his hands and eyes, are many of the experiments of geometry.

The fourth propoſition of the firſt book is the firſt theorem occurring in the elements of Euclid; this propoſition, and the axiom as it is called, upon which the demonſtration is founded, viz. the 10th in Simſon's edition, may be regarded as the corner-ſtone of geometrical reaſoning. The propoſition is this:

If two triangles have two ſides of the one equal to two ſides of the other each to each; and have likewiſe the angles contained by thoſe ſides equal to one another, they ſhall have their baſes, or third ſides, *equal; and the two triangles ſhall be equal; and their other angles ſhall be equal, each to each, viz. thoſe to which the equal ſides are oppoſite.*



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It will be granted, that the ideas of a triangle, of equal or unequal expanſions between lines meeting at a point, and of the equal or unequal length of different ſtraight lines, are all acquired by the exerciſe of the ſenſes. It is, indeed, evident, that we cannot in any other way acquire them.——

Now the ſide AB being made equal to DE, and AC to DF, and the angles BAC and EDF being equal, let us obſerve the experimental proceſs of the demonſtration.

If you have a model of each triangle cut out in paſteboard, or any other material, you are to place the point A upon D, which is to commence an experiment; if the triangles be only traced upon a ſurface, you are to imagine A placed upon D, which is to imagine the commencement of an experiment. Then you are to lay AB along DE; now B will fall upon E. "Why?"

becauſe from experiments or obſervations already made, you know that if you have two ſtraight ſtrips of wood, for inſtance, and put the end of one exactly over the end of the other, and turn the upper ſtrip in the direction of the lower, the other end will neither over-reach nor fall ſhort of the other end of the lower, but lie exactly over it: and this is what we mean by equality of length, a term ſolely derived from ſuch experiments of menſuration, made in early life.

To ſhew by experiment, likewiſe, which alone can ſhew it, what an angle is, and moreover that, if the line AC ſtretches as much from AB as DE does from DF, AC muſt lie along DF, whenever AB lies along DE, a carpenter's ruler may be opened and ſhut to various angles, and one carpenter's ruler may be placed over another.

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This experiment having been performed till the reſult is allowed and underſtood, it will be ſeen that the point C muſt fall upon F, from the equality of the lines AC and DF.

Then BC muſt fall upon EF; "and why?"—make the experiment; you cannot place two ſtraight ſticks, or trace two ſtraight lines, ſo that encompaſs a ſpace, try as long as you pleaſe and ſatisfy yourſelf: if you were to try to incloſe a piece of land by two ſtraight hedges, you would find all your attempts vain; there would be one or two open ends, place the hedges how you will; and then your field would not be encloſed, but open. Therefore BC muſt needs conſent to fall along EF; otherwiſe B being upon E, and C upon F, as I have already ſhewn you, BC muſt go on one ſide EF like one of the dotted lines; but then BC and EF, two ſtraight lines,

would encloſe a ſpace, which the whole courſe of your experiments has ſhewn you, they cannot poſſibly do.

Then the triangles muſt exactly fall upon one another, &c.

I have been purpoſely prolix in this demonſtration, to ſhew how it begins in experiment, goes on by experiment, and ends in an experimental concluſion. There may be another uſe in inſiſting ſo particularly upon the nature of the reaſoning proceſs here: among thoſe who teach mathematics, without underſtanding their practical application, and alſo without entertaining a juſt idea of the nature of demonſtration, there prevails a ſort of pedantry productive of infinite diſguſt to the learner. If by detached figures I could ſhew the truth of any propoſition in an inſtant, I am forbidden, becauſe this is an *unmathematical* mode of proceeding; that

is,

is, mathematical reaſoning is ſuppoſed to be ſomething independent of experience, and the ſcience to be more refined than the experimental ſciences. Hence, if a Greek writer happens to have written a demonſtration a mile long, which demonſtration can be nothing but a concatenation of the reſults of obſervation and experiment, I muſt take this tedious round, rather than be allowed to arrive at the point deſired by only traverſing half a dozen yards, provided this ſhorter road leads through the unhallowed region of the ſenſes.

The fifth propoſition is ſaid to have ſtopped many ſtudents of geometry in their career; this is owing partly to the length of the demonſtration, and partly to the complication of the diagram. The demonſtration is, however, nothing but the reſult of the experiments in that of the fourth, combined with the reſult of two

other very ſimple experiments; of which the one, that if you take equal parts from equal lines, the remainders ſhall be equal, will be eaſily granted from diſtinct recollection. The other, that if from equal angles you take equal parts, the remaining angels will be equal, ſhould be ſhewn by two pair of compaſſes, or two carpenter's rules opened equally, and then brought nearer together in both an equal and unequal degree. The reaſon why it becomes neceſſary to take pains to make beginners comprehend the nature of an angle, is becauſe in life we do not pay attention to the different expanſions of lines meeting at a point. On the contrary, there is not a child but what is accuſtomed to meaſure ſimple lengths.

I would rather chooſe to appeal to theſe two experiments, than to the third axiom placed before Euclid's elements, viz. that

if equals be taken from equals, the remainders will be equal. Mr. Locke has ſhewn the inſignificance of theſe axioms in the ſeventh chapter of his fourth book. In fact, they are only founded upon the induction of particular experiments and obſervations; and until that induction be compleat, we can never be convinced of their truth. They do not prove any thing themſelves, but require to be proved; and if a Newton were to devote his powers to the ſtudy of axioms for an hundred years, he would not be able to draw from them one ſingle concluſion worth notice.

In this manner does every demonſtration proceed upon the reſults of experiments, as the reader will find, in as many inſtances as he ſhall take the pains to examine. And ſince the appeal in demonſtrative reaſoning is always made to what is now exhibited to the ſenſes, or to what we have before learned by the exerciſe of the ſenſes, too much pains cannot be taken, at the commencement of the ſtudy of geo-

metry, to ſatisfy the mind of the learner by appealing to his ſenſes. The more diſtinct and deep the impreſſions of ſenſe are at the beginning, the greater will the power of *abſtraction* afterwards be, when the progreſs of his ſtudies ſhall have carried him into the higher mathematics. *Abſtraction* is not, in fact, a diſtinct *power*, as the metaphyſicians, who ſeem to imagine that they increaſe the importance of their ſcience, as they multiply diſtinctions, teach. We *abſtract*, when we narrow the ſphere of ſenſations and dwell upon impreſſions, or when we recollect the ideas thus acquired. So far is this talent from forming a diſtinction between man and beaſt, that the animals which do not take cognizance of more than two or three objects in this ſublunary world may, I think, be fairly reckoned the moſt abſtracted of all living creatures.—It is, at leaſt, evident that of any object, I ſhall recollect the whole or *any part* the better, as the original impreſſion was more lively. If I am to imagine, or *form an*

image,

image, by putting things together in my mind, in an arrangement different from that in which I have beheld them, and thus create a whole which I have not ſeen, out of parts which I have ſeen; the diſtinctneſs of the original conceptions will be equally ſubſervient to this proceſs. By appealing in this manner to his ſenſes, and making him feel the firmneſs of the ground on which he treads, one might probably inſtruct a boy, at an early age, in the elements of geometry, ſo as rarely to give him diſguſt, and frequently great ſatisfaction. He would by imperceptible degrees acquire the power of *abſtraction*, or learn to reconſider each ſeparate perception, as well as to combine them anew.

Suppoſing it unneceſſary to multiply inſtances of the experimental reaſoning of geometry, ſince the inſtance already quoted fairly repreſents all the reſt, I ſhall ſhortly con-

conſider the definitions of the firſt book of Euclid, except the merely nominal definitions, ſuch as thoſe of a rhomb, trapezium, &c.

There is, it ſeems, ſome uncertainty as to the author of the definitions. I ſhall take them, as they occur in Dr. Simſon's tranſlation, occaſionally, however, referring to Dr. Gregory's edition of the Greek text *.

DEFINITION I.

A point is that which hath no parts, or which hath no magnitude.

Σημειον ἐστι, ου μέρος ϑεν.

Here the beginner immediately finds himſelf tranſported into the land of won-

* ΕΥΚΛΕΙΔΟΥ ΤΑ ΣΩΖΟΜΕΝΑ. Oxon. 1703. Ex Recenſione Davidis Gregorii, M. D.

ders;

ders; and ſuppoſing it neceſſary to his progreſs to conceive a thing that has no parts, he is apt to ſurmiſe that mathematics is a ſtudy for which nature never deſigned him; and as he proceeds, he looks back from time to time with an eye of regret upon the firſt definition, earneſtly wiſhing he had but force of mind enough to comprehend it. Dr. Simſon's demonſtration will not afford him any aſſiſtance in his difficulty; and he will ſtill be unable to conceive what that can be, which has no parts or magnitude; if a variety of phraſes be, as uſual, repeated to him; he may reply, it is in vain to utter new ſounds; what I want is ſenſible evidence of the thing; and if he ſhould but have the good fortune to attend to the evidence of his ſenſes, and to underſtand the nature of language, the difficulty will inſtantly vaniſh: for a point is firſt the end of any thing ſharp; *omne*

quod

quod pungit: then, by an eaſy derivation, any mark made by that ſharp thing; and this is the meaning of point in geometry. Perhaps the difficulty had never occurred, if inſtead of *point*, the word *dot*, or even *mark*, σημειον, had been always uſed in its place, and there had been no attempt at definition. For a point is only to mark the place whence a line is to begin, or where it is to end: γραμμῆς δέ πέρατα σημεῖα. DEF. III. Thus in a circle it marks the ſpot within the figure, from which all ſtraight lines drawn to the circumference, are equal. Now one would make ſuch a mark as ſmall as poſſible, provided it be ſtill diſtinct, that the length of lines and their meetings and interſections may appear plainly, and from this effect of convenience has ariſen the phraſe that is ſuppoſed to deſcribe its eſſence; *that it is without parts.* This idea has nothing to do with the reaſoning;

ſoning; all that is neceſſary is, that the *dot* or *mark* ſhould take up no ſenſible part of the line, in order that the diagram may be diſtinct. *Points* then are only ſubſervient to the convenience of conſtruction.

The next definition, after this explanation of the firſt, will preſent no difficulty.

DEFINITION II.

A line is length without breadth.

Γραμμη δε, μηκος απλαϛες.

Draw your lines as narrow as you conveniently can, your diagrams will be the clearer; but you cannot, and you need not, conceive length without breadth.

DEFINITION III.

The extremity of a line are points.

DEFINITION IV.

A ſtraight line is that which lies evenly between its extreme points.

Ευθεια γραμμη εϛιν, ἥτις ἐξισϖ τοις ἐφ' ἑαυτῆς σημειοις κειται.

The impoſſibility of defining a word expreſſive of a ſimple perception is well known. The definition of a complex term conſiſts merely in the enumeration of the ſimple ideas, for which it ſtands. The only way of rendering the meaning of a ſimple term intelligible, is to exhibit the object of which it is the ſign; or, if you pleaſe, ſome ſenſible repreſentation of that object. A ſtraight line therefore muſt be ſhewn; and by drawing a crooked one at the ſame time, it will be perfectly underſtood, if any one require an explanation. All definitions muſt have ſome term, equal-

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ly requiring a definition with that defined, as ἐξισῳ, *evenly, upon an equality.*

The definitions of a ſurface, and a plane ſurface, muſt in like manner be made intelligible by an appeal to the ſenſes. By putting a ſtraight rule along different ſurfaces, it will appear whether they are plane or otherwiſe.

DEFINITION X.

When a ſtraight line ſtanding on another ſtraight line makes the adjacent angles equal to one another, each of the angles is called a right angle, and the line which ſtands on the other is called a perpendicular to it.

Here we have an appeal made to the ſenſes, which alone can inform us what is the expanſion between lines meeting at a point, or what is their inclination or bending towards each other. The eye can

pretty well determine, whether the meeting lines are more inclined to each other on one ſide than the other, i. e. the equality or inequality of the adjacent angles. This meaſure of the eye would not be ſufficiently exact to ſatisfy us that the angles are equal; we muſt obtain a meaſure by real or imagined ſuper-poſition, as we do in one particular caſe, by applying the reſult of the demonſtration of the eighth, to the eleventh propoſition of Book I. But the term *right* probably preceded the application of any ſuch exact meaſure; and I ſhould conjecture, that it might be deduced from the meeting of lines or of bodies, that ſeemed to ſtand perfectly erect and not to bend towards each other; in the caſe of lines, ſtraight and right are perfectly ſynonymous; and an angle formed by the meeting of ſtraight lines, ſtanding right or upright, with reſpect to each other, would

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be called a right angle: *linea recta, angulus rectus.* The application of the term *right*, would not, I ſuppoſe, be ſo obvious to certain angles formed by curved lines; yet it would be eaſily made, when men came to conſider ſuch angles, with a view to take their relative dimenſions accurately.

DEFINITION XV. and XVI.

A circle is a plane figure contained by one line, which is called the circumference, and is ſuch, that all ſtraight lines drawn from a certain point, within the figure to the circumference, are equal to one another.

And this point is called the centre of the circle.

In theſe definitions, there is doubtleſs nothing embarraſſing. Yet for the ſake of perſons unaccuſtomed on the one hand to

verbal analyſis, which forms a moſt important part of genuine *logic*, or the *theory of reaſoning*, it may not be improper to add the following remarks. An hoop, or any round object, eſpecially if hollow within, might ſupply the notion of a circle; but moſt probably a wheel with its ſpokes, and the central block of wood into which the interior extremities of the ſpokes are fixed, furniſhed the idea of the figure of the radii and centre; radius, we know, ſignifies a ſpoke: a ſignification previous, no doubt, to that of lines drawn from the centre to the circumference of a circle. All accurate ideas, founded upon meaſurement or careful compariſon, muſt neceſſarily be poſterior to approximations, ſuggeſted by a caſual or a diſtant ſurvey of objects; and a good deal of perplexity, when perſons firſt engage in the ſtudy of mathematics, ariſes from the reſult of vague obſervations,

ſervations, mixing itſelf with the reſult of the exact experiments employed in the reaſoning of mathematics.

Centre is ſimply *a point*, *a dot*, *a mark*, ſhewing the common origin of the radii. This is the ſecondary ſignification of the ſubſtantive κεντρον, derived from the verb κεντεω *pungo*, *ſtimulo*, provided the etymologiſts of Holland have not reverſed the natural order of derivation, and I ſhall hereafter endeavour to render it probable, from ſome very ſimple conſiderations, and even from data which they themſelves afford, that they have. Their reſearches, however, even ſo, retain a great value. In theſe caſes, the beſt ſervice a perſon can render to letters, next to writing the truth, is to write exactly the reverſe.—At all events, the firſt ſignification of κεντρον, is a *goad*, or any thing ending in a ſharp point; and hence the mark left by preſſing

this point againſt any yielding ſurface. If a man were to begin to ſtudy geometry, independently of all ideas of viſion, he muſt have the points whence lines are to be drawn, the centre of a circle, &c. marked by ſome ſharp projecting point or a ſmall hollow.

In Lennep's Etymologicum, we have the following account of the origin of κιρκος and κυκλος.

Κιρκος, *circus* (item *circulus*, et *annulus*) ortum eſt a κὶρω, quod vicinum verbo κειρω. Communis autem origo eſt κεω, κιω, cujus *propria* notio poſita eſt in *motu*, qui fit *impellendo allidendove*. Et κιρω quidem vicinum videtur verbo γυρω, cujus origo γυω. Eâdem autem ratione, ut a γυρω ortum γῦρος, ſignificat motum rei in ſe redeuntis, ſic noſtrum κιρκος circum ſignificare videtur.—Again, κυκλος, cyclus, circus . . . . . repeten-

repetendum eſt a κυλιω, vel formâ quâdam vicina, id autem eſt *volvo*.

The *poſtulates* prefixed to Euclid's Elements, preſent themſelves in a form of which, perhaps, Mr. Ludlam felt the awkwardneſs, when he obſerved, "that the "poſtulates are not to be ſo underſtood, as "if Euclid required a practical dexterity "in managing a ruler and pencil. They "are here ſet down, that his reader may "admit the *poſſibility* of what he may here-"after require to be done. To ſhew that "what he thus requires contains no ab-"ſurdity, no repugnant ideas, Euclid in "the courſe of a demonſtration, requires "you to produce a terminated ſtraight line. "Was this as impoſſible in idea, as it is to "take a greater number out of a leſs, the "whole demonſtration muſt fail; for the "ſteps of a demonſtration, like the links "of a chain, hang by one another; Eu-

"clid,

" clid, therefore, in this place, enumerates " all the operations required in his future " demonſtrations and problems; requiring " their poſſibility to be here acknowledged, " and thus precludes all future objections " on this head." (*Rudiments, Ed. 3d. p.* 137.) Theſe poſtulates were, indeed, probably introduced by cavils, or the dread of cavils: but if that which is demanded had been denied, the author would have been ſo thoroughly convinced by his ſenſes, that he could produce a ſtraight line to any requiſite diſtance, or deſcribe a circle with any radius, that he would have gone on with perfect aſſurance in his reaſoning. Should any one object to a demonſtration, " but how am I ſure that theſe things are " true, of lines reaching from the earth to " the moon, or of a circle that ſhould be " deſcribed from the ſun as a centre, and " at the diſtance of Sirius?" It is impoſ-

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ſible to convince ſuch an objector, by direct ſenſible teſtimony, or rather it is impoſſible to make him own a conviction, which we are ſure he muſt feel. It is then idle to clog an elementary book with forms or matter intended to obviate ſuch objections. All that ſhould be inſerted, is juſt enough to ſatisfy a perſon, that will, *bonâ fide*, make uſe of his faculties.

Moſt of the AXIOMS were probably introduced, in conſequence of that perverſion of the human underſtanding, which the ſtudy of *generals* occaſioned. "The rules "eſtabliſhed in the ſchools, ſays Mr. "Locke, that all reaſonings are *ex præ-* "*cognitis et præconceſſis*, ſeem to lay the "foundation of all other knowledge in "theſe maxims, and to ſuppoſe them to be "*præcognita*, whereby, I think, are meant "theſe two things, firſt, that theſe axioms "are thoſe truths that are firſt known to

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"the mind; and ſecondly, that upon them "the other parts of our knowledge de-"pend." (B. iv. c. 7. §. 8.) He goes on to ſhew, 1. That theſe axioms are not the truths firſt known; and, 2. That the other parts of our knowledge do not depend upon them; and then challenges any ſcholaſtic man, to lay before him the whole frame and ſyſtem of any ſcience ſo built upon axioms, that they cannot be ſhewn to ſtand as firmly without any conſideration of them. Theſe axioms then ought to be expunged from books of geometry; 1. as unneceſſary; and, 2. as tending to give the beginner wrong notions of the foundation of knowledge, and the means by which we render ourſelves certain in any caſe of doubt. Conſidering what commentaries we have lately had from no deſpicable hands upon the *veritès premiéres* of Pere Buffier, it is not yet ſuperfluous to apprize

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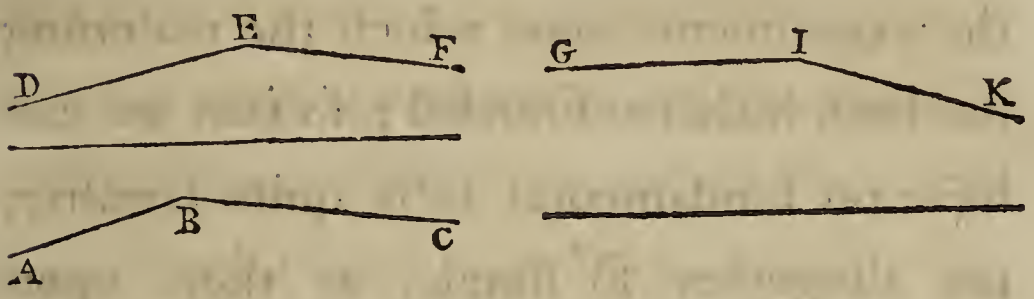
the ſtudent where human knowledge begins, and how certainty is acquired.

In the 35th definition, and the 12th *axiom*, as it is improperly called, we have expreſſed the reſult of our common obſervation of parallel lines. This part of Euclid has been the occaſion of infinite but needleſs perplexity to editors and commentators. Had they been aware of the nature of geometrical reaſoning, they would have entertained leſs anxiety on account of the foundation upon which the doctrine of parallel lines is to be built; for it is remarkable, that though all, as far as I know, from Savile to Ludlam, object to Euclid, and each propoſes an aſſumption at ſetting out different from that of his predeceſſors, yet all are perfectly ſatisfied with the concluſions; and there never probably was a perſon, who after paying proper attention, doubted of the truth of the 27th, 28th,

28th, 29th, or 30th propoſition of the firſt book of Euclid, or of the moſt remote conſequence deduced from them. The fact is, the ſenſes give deciſive evidence of theſe properties. All the difference in the projects for correcting Euclid's method conſiſts in this, that one writer propoſes to ſet out from one reſult, another from another; all of them true, and more or leſs obvious. Dr. Auſtin propoſes to ſet out from the equality of the alternate angles; a fact more remote from immediate inſpection, than any of the others; but even to this method there can be no objection, provided the pupil be ſhewn, that this equality does actually exiſt. Other writers begin with properties leſs remote from vulgar obſervation. Dr. Simſon propoſes theſe *axioms*, "a ſtraight line cannot firſt go nearer to another ſtraight line and then go further from it, before it cuts it, nor recede,

recede, and then approach, nor keep the ſame diſtance for a part of its courſe, and then recede or approach;" that is, in ſhorter and clearer terms, that a ſtraight is not a crooked or a bent line; as for inſtance, in



the lines above, that a ſtraight line can never take the direction of ABC, or DEF, or GIK: he afterwards goes through five tireſome demonſtrations, as if the elements of Euclid were not already tedious enough.

Mr. Ludlam, as we ſhall preſently ſee, propoſes a ſhorter, and therefore a better way, to demonſtrate the 29th propoſition.

A very ſimple reflection will make it appear, why ſo many different facts have been ſuggeſted on the foundation of propoſitions concerning parallel lines. We do

not

not uſually in the courſe of life, treaſure up any exact reſult upon this ſubject; nor can we ſatisfy ourſelves here, by bringing one figure and laying it upon another, or meaſuring it in any very ſimple way, as in the experiments upon which the reaſoning has been hitherto founded; ſo that we can have no fundamental facts quite familiar, nor altogether ſo ſimple, as thoſe upon which we have hitherto proceeded; but the want of familiarity, the fruit of paſt obſervation, may be eaſily made up by preſent attention; of a truth of which the ſenſes are perfectly equal to take cognizance, we ſhall, after due attention, feel the ſame conviction, whether we happen to find it out ourſelves or have it pointed out to us. And the reaſoning concerning parallel lines, though neither beginning in ideas ſo ſimple or ſo familiar, will never-

 theleſs

theleſs prove equally concluſive with the reaſoning that has gone before.

On an hundred different occaſions, in our playful and in our ſerious moods, we have obſerved the coincidence of ſtraight lines, and meaſured lengths; our obſervation of ruts traced by wheel carriages, as alſo of frame-work in which bars laid alongſide of each other, gives us a diſtinct idea of ſtraight lines that perpetually preſerve the ſame diſtance: and we ſhall find alſo upon calling to mind our paſt obſervations, and holding them ſteadily in view, that if lines do not preſerve the ſame diſtance, they muſt, if produced far enough, at laſt meet on one ſide or other of a ſtraight line, which croſſes them, let them converge ever ſo ſlowly *.

The

* *Parallel ſtraight lines are ſuch as are in the ſame plane, and which being produced ever ſo far both ways do*

The truth of the reſult, expreſſed in Euclid's 12th *axiom*, as it is ſo improperly called,

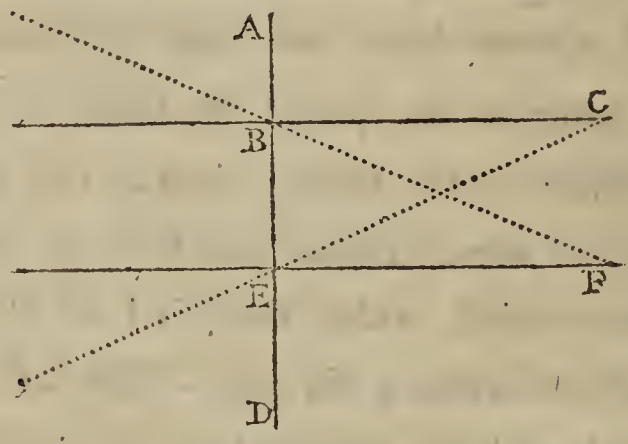
not meet. Thus the 35th definition ſtands in Simſon's Euclid. It has been objected to, "as a vulgar and "inaccurate conception, containing no given finite "quantity, and furniſhing no meaſure or ſtandard, "from which an equation might be made; and that "nothing could be expected from ſuch a definition in "a ſcience, *all* whoſe axioms expreſs only equati-"ons." (Auſtin's Examination of Euclid, p. 13.) This definition may, in one ſenſe, be regarded as a verbal definition, an explanation of the word *parallel*; which word need not be uſed in the elements of geometry, juſt as for triangle we might always uſe the phraſe, *a plane figure contained by three ſtraight lines*. Thus in the enunciation of propoſition the 29th, we might ſay, *if a ſtraight line, falling upon two other ſtraight lines, makes the alternate angles equal to one another; theſe two ſtraight lines*, being produced ever ſo far both ways, ſhall never meet, inſtead of, *ſhall be parallel*.

The 10th axiom in Simſon, proves Dr. A's concluding remark not to be juſt. Moreover, it appears to me that the

called, may be clearly ſhewn by the following experiment, and he may have in-

the idea of lines that never meet, is one of the cleareſt and moſt ſatisfactory the ſenſes bring to the mind; and it is, beſides, abundantly ſimple. But, in order to remove all uncertainty, if the caſual obſervations from which the term *paralleliſm* is derived, ſhould have left any, fix two pencils at a convenient diſtance in a piece of wood; let one end of this piece of wood be terminated in a plane, large enough to keep true to the face of a ruler. By this ſimple apparatus, a child at a very early age, might be convinced, that lines traced by theſe pencils, would never meet, ſince they muſt always have the ſame diſtance between them, as long as the pencil continue to be moved along the face of the ruler. And in this manner we ſhall have an experimental proof of the above 27th propoſition, againſt which the cavil of Proclus will not be able to raiſe a ſcruple.—The difficulties, in ſhort, that have been propoſed on the ſubject of parallel lines, and the contrivances to obviate them, equally evince the experimental nature of mathematical reaſoning.

ferred it either from ſome ſuch experiment made or imagined; or from knowing, that the angles of a plane triangle are equal to two right angles. The *axiom* is; "if a "ſtraight meets two ſtraight lines, ſo as to "make the two interior angles on the ſame "ſide of it taken together leſs than two "right angles, theſe ſtraight lines being "continually produced, ſhall at length "meet upon that ſide on which are the "angles which are leſs than two right "angles."

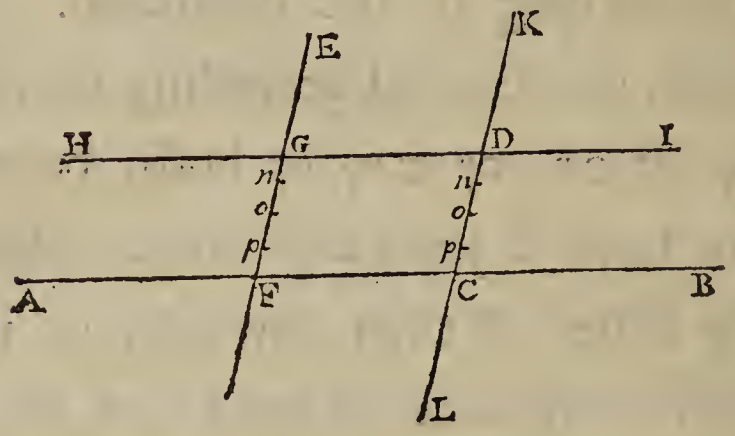


Let a ſtraight line ſo fall upon two other ſtraight lines, that the angles ABC and CBD, are equal to one another, and alſo the

the angles AEF and FED, (which may be done either by experiment or by former propoſitions) then the lines CB and FE ſtand upright with reſpect to AD, or the angles, being adjacent and equal, are right angles according to the definition. Now in a model conſtructed according to this diagram, let CB, or EF, or both, be moveable, or have a joint at B and E, then by turning either of them inwards, or in the direction of the dotted lines EC or BF, they will meet and form a triangle, if produced. So that if theſe angles, which by the conſtruction are equal to two right angles, become at all leſs, *the ſtraight lines, continually produced, ſhall at length meet upon that ſide, on which are the angles which are leſs than two right angles*; which was to be ſhewn.

The following experiments would furniſh a ſatisfactory demonſtration of the lead-

ing properties of parallel lines; let parallel lines be ſhewn to be ſuch as always to preſerve the ſame diſtance; hence all the ſtraight lines meeting them internally at equal angles ſhall be equal. Thus let AB be a ſtraight

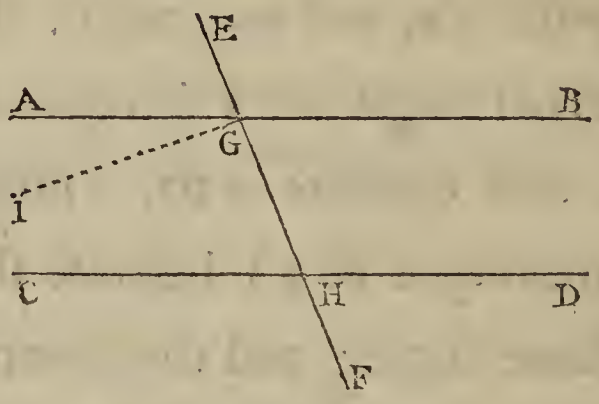


line; and let DC make with it the angle DCB. At the point F make the angle GFB equal to DCB, and the line FG equal to DC, and through G and D draw the line HI. Now the angles DCB and GFB being equal, and GF equal to DC, the lines AB and HI are parallel. Now let us ſuppoſe a model made according to this diagram, ſo that the line HI might move along GF and DC, upon each of which certain ſmall equal

diſtances

diſtances are marked off, as *n*, *o*, *p*. It is evident, that if HI were brought down through theſe equal ſmall diſtances ſucceſſively, it would be parallel, for F*n* is equal to C*n*, and ſo on; and at laſt it would coincide with AB, and not cut it. Therefore, the internal angle DCB is equal to the external, and oppoſite KDI, (ſuppoſing DC to be prolonged to K) becauſe they would be the ſame angle; and the alternate angles GDC, DCB, would be equal, as being now the vertical angles DCB and FCL; and the interior angles, IDC coinciding with BCL and BCD with IDK, would, by propoſition the 13th, be equal to two right angles; and any line parallel to AB would be parallel to HI, prop. 30th. On ſeparating the ſtraight ſtrips again, it would appear, that if the alternate angles be equal, the exterior equal to the interior and oppoſite, and both interior equal to two right angles,

the lines would keep parallel, and if any of theſe circumſtances change, the angles would not be parallel, for the lines forming the equal angles, would become unequal.



A courſe of experiments on parallel lines may begin in a manner different ſtill. Mr. Ludlam propoſes to demonſtrate the equality of the alternate angles, for which alone the 12th *axiom* is needed, by the following experiment, which he miſcalls an axiom. *Two ſtraight lines, meeting in a point, are not* BOTH *parallel to a third line.* For, ſays he, if AGH be not equal to GHD (the alternate angle) one muſt be greater; let

AGH

AGH be the greater. At the point G, make IGH equal to GHD, (by prop. 23. I.) then IG is parallel to CD by 27. I. But by ſupp. AG is alſo parallel to CD, therefore AG and IG, meeting in a point at G, are both parallel to CD, which is impoſſible by the axiom, (i. e. it will be found ſo upon trial.) *Rudim.* p. 144.

This is perfectly ſatisfactory. In a word, when we come to reflect upon our perceptions, we diſcover a conſtant equality of diſtance, or the impoſſibility of their meeting to be a character of parallel lines; but this is not ſufficient, for we muſt have a meaſure of this equality; and by continuing our attention a ſtep further, we perceive that this meaſure may be either the equality of lines making equal interior angles with one of the parallel lines, or what is a particular caſe of the ſame reſult, the equality of lines falling at right angles upon the

the inſide of parallel lines. I have no other reaſon for preferring either of theſe meaſures, than that it ſeems to be the firſt accurate reſult we arrive at in conſidering parallel lines. Any other clear inference from manual or mental experiments, will afford an equally firm baſe for the reaſoning; and any of the firſt properties attributed by Euclid to parallel lines, may be employed for this purpoſe. They are all about equally diſtant from our previous knowledge, and all may with nearly equal eaſe, be proved by experiment. In Euclid's Elements, the truth ſeems to me to be ſo frequently obſcured by demonſtration, and ſo much diſguſt is often excited by his tedious method of proceeding, that were it not a violation of that loyalty which we owe to our maſters, the Greeks, I wiſh the ſhorteſt poſſible method might be followed in teaching the rudiments of mathematics

matics by the help of ſimple ſatisfactory experiments. And if there be any one who ſhould have learned geometry in this way, let him be aſſured that he holds his proficiency by a firm tenure. In this ſcience there is no tranſcendental road; but I imagine a *royal* road might be ſtruck out, though Euclid was of a different opinion. The ſooner too we quit the geometrical for the algebraic method, the better. Not only has algebra all the general advantages aſcribed to the ſtudy of mathematics, by Bacon and Locke, but one peculiar to itſelf. Not only "if the wit be dull, does "the analytic method ſharpen it; if too "wandering, fix it; if too inherent in the "ſenſe, abſtract it;" but it confers the power of invention and combination beyond any other ſtudy; in geometry indeed, compared with algebra, the mind may be ſaid to be paſſive. The power of readily calling

calling up poſſibilities before the imagination, of contraſting them with realities and with one another, and of deciding on their reſpective merits, appears to me the higheſt ſtate of perfection, at which our faculties can arrive. A perſon, poſſeſſed of this talent, is prepared equally to excel in thought and conduct; and the reſources of his mind will be inexhauſtible. Now an acute obſervation, and juſt conception of things actual, joined to an habit of comparing and combining them, not unlike that caſting about of the thoughts which takes place in the ſolution of algebraic problems, is the only way, I believe, in which this ineſtimable talent can be acquired.

Should it be objected, that if we appeal as directly as poſſible to the ſenſes in teaching the elements of geometry, we ſhall impair the power of abſtraction, I anſwer, that this method will confer the power

of

of abſtraction in a ſuperior degree. The ſenſes will deliver more diſtinct ideas to the memory; and the more firmly the memory holds ideas, the more eaſily will the imagination and the judgment be enabled to perform their functions. Natural ſuperiority of intellect can ariſe only from an happier organization of the ſenſes, or the *ſenſorium**. Now by exerciſing the ſenſes pro-

* Hence the long agitated queſtion concerning original determination, or innate diſpoſition, towards any particular purſuit, may be decided with perfect preciſion. That the ſmalleſt tendency towards mathematics, poetry, &c. exiſts at the time of birth, is a chimæra too abſurd to be ſeriouſly combated now. It is ſufficient to obſerve an infant, or to be acquainted with the origin of ideas, to diſmiſs from the mind this ſuggeſtion of ignorance and indolence. Beſides, why are not geniuſes for arts or ſciences born among ſavages, as frequently as in nations where theſe arts and ſciences flouriſh? If theſe germs of particular talents pre-exiſt

properly during the firſt period of life, we may make up, by continued attention, for their deficiency of original acuteneſs. And where nature has beſtowed her nobleſt gifts, ſuperior powers of perception, retention, and combination, we ſhall thus turn them to the beſt poſſible advantage.

Should

exiſt in the mind, we might expect to ſee them occaſionally vegetate, and at length produce fruit, independently of the ſtate of ſociety; but we ſee evidently, that particular eminence is always engrafted upon general talents, often at a late period in life, and in no caſe the ſpontaneous produce of the intellect.—A peculiar organization, however, may be conſidered in one ſenſe as an original determination towards a particular purſuit. Thus a peculiar organization of the ear, which perhaps experiment would ſhew to be only a quicker ſenſe of hearing, and ſo to differ in degree, not in kind, diſpoſes to inſtrumental muſic: A good ear with an agreeable voice to vocal muſic alſo. A well-organized and very pliant hand may determine to occupations

Should theſe reflections be juſt, and ſhould it alſo be admitted, that in demonſtrative reaſoning, we proceed purely by experiments, as will, I hope, hardly now be doubted; it would be advantageous, in order to beſtow the habit of obſerving with attention, to conſtruct a geometrical ap-

cupations requiring manual dexterity. In powers of vociferation a man, to the annoyance of his neighbours, often feels a vocation to become an orator.—The relative ſtructure of the organ, by which we combine ideas, ſeems to be in every reſpect independant of thoſe by which we receive them. Though the eye or ear be well or ill organized, it does not follow that the brain ſhall be well or ill organized. And to the combining as well as the retaining faculty, it ſeems perfectly indifferent, what ſort of ideas it has preſented to it; whether of ſounds, colours, mathematical, poetical, or any other. Except therefore thoſe purſuits, which are immediately connected with external organization, we cannot be ſaid, in any ſenſe whatever, to receive from nature any particular determination.

paratus, which might be employed in the early part of education. Should ſuch an apparatus be preſented to children, without any of the forbidding airs of pedantry, I imagine that in this mode of ſtudying geometry, they would find no leſs relief than improvement; they would find at leaſt employment for their ſenſes; and every thing we can obſerve in children, conſpires with every thing we know of the human underſtanding, to ſhew that nature intended us during the firſt period of life, to be chiefly employed in exerciſing them. The ſoul of a child, (from his earlieſt infancy, when he will always, while awake, be obſerved to be collecting ideas, by his hands and eyes in particular,) may be ſaid eſſentially to reſide in his ſenſes. At a riper age the combination of theſe ideas engages a conſiderable ſhare of attention; and at an advanced age, when the ſenſes are blunted,

the

the exerciſe of the internal faculties will ſtill more occupy a mind previouſly well ſtored.

But according to the modern practice of education, inſtead of ſuffering children to follow the active tendency of their nature, or gently directing it, we forcibly debar them from the exerciſe of the ſenſes, and condemn them to the horrible drudgery of learning by rote, the conceits of a tribe of ſophiſts and ſemi-barbarians, to whom it is no reproach not to have entertained juſt ideas either concerning words or things. Next to actual blind-folding and muffling, to oblige children to learn the terms in which theſe conceits are couched, is the happieſt contrivance imaginable, for keeping their minds unfurniſhed; by long continuance of ſedentary confinement, we hold the perceptive faculties, as much as poſſible, in a ſtate of perfect inaction; at the

ſame time we employ the organs of ſpeech in pronouncing, and the memory in retaining, none but ſounds inſignificant; ſo that from the commencement of a liberal education, one might be led to conclude, that the following is the only ſentence, ever written by Mr. Locke, of which his countrymen have attempted an application; "if it were "worth while, no doubt a child might be "ſo ordered, as to have but a very few, "even of the ordinary ideas, till he were "grown up to a man;" and that nothing might be wanting to ſatisfy us, that our apparent cruelty is *real kindneſs*, it has been clearly proved, that the principal rules laid down in our grammars are falſe, and the exceptions groundleſs! Let the moraliſt, when he has verified this fact in the writings of Mr. Tooke, and his fellow labourers in the philoſophy of language, determine whether it be an act of

greater

greater humanity, to preſerve the Africans from ſlavery, or deliver children from *grammar* *.

The

* Thanks to Mrs. BARBAULD, who has entitled herſelf to the everlaſting gratitude of parents and children, one great point in education has already been gained: as long as the ſuperintendance of the mother laſts, books calculated to improve them in every reſpect, are now commonly put into the hands of children. Theſe books are equally intelligible and amuſing.

But no ſooner have the boys croſſed the threſhold of the grammar ſchool, than they experience the moſt dreadful reverſe. And during a great part of their continuance at ſchool, they are ſubjected to the moſt exquiſite mental torture that can well be deviſed. The following untranſlatable character, by Valckenaer, (*Obſerv. ad Orig. Græc. p.* 4.) may ſerve to ſhew how much boys are likely to ſuffer, and how much to gain, while they are toiling through their Greek grammar: and no one will accuſe Valckenaer of rating the knowledge of the Greek language below its value.—"Recentiorum *grammaticæ*, ut vocantur, *Græcæ* ad prima lingua rudimenta facili methodo diſcenda vix ac ne vix

The illuſtrations already given, would, I preſume, enable the moſt inexperienced reader in other caſes, to diſcern the reſults of experiment upon which mathematical reaſoning muſt always proceed. But ſo

quidem accommodatæ, REGULIS INEPTIS, TORTUOSIS ANOMALIARUM TRICIS, ET VANORUM AC NUGANTIUM HOMINUM SPINOSIS DELIRAMENTIS ſunt refertiſſimæ." Our Latin grammars are, I preſume, little leſs plentifully provided than our Greek with FALSE AND FOOLISH RULES, PERPLEXING EXCEPTIONS AND THE DISGUSTING RAVINGS OF VAIN AND EMPTY TRIFLERS. I, who believe in the impreſcriptible right of boys and girls to a good education, think parents in conſcience bound to exert themſelves ſtrenuouſly, in order that children be taught ſomething very different from theſe odious ſubtleties. One thing they may reckon upon as certain, that though occaſionally a degree of attention may be requiſite beyond what a child will chearfully beſtow, yet if education goes on, upon the whole, unpleaſantly, it is ill-conducted. Children commonly acquire *ideas* with pleaſure, and pride themſelves in the acquiſition.

much

much has been written concerning the 5th definition of the 5th book, that I may be permitted to add a few obſervations upon this particular difficulty. The terms of this definition ſo exactly reſemble the terms in which "*every curious noſtrum and choice* "*receipt*" is delivered, that one may wonder how Mr. Harris himſelf could fail to perceive, in this inſtance at leaſt, that mathematical reaſoning is founded upon experiment or experience, which, as he juſtly obſerves, is no more than "the reſult of many repeated experiments." The firſt of four magnitudes, we are told in this definition, has the ſame ratio to the ſecond, that the third has to the fourth, when any equimultiples of the firſt and third *being taken*, and any equimultiples whatever of the ſecond and fourth, if when the multiple of the firſt is leſs than that of the ſecond, the multiple of the third is leſs than

the multiple of the fourth, if when equal, equal; and if when greater, greater. I know not how it is poſſible more directly to appeal to experiment, than firſt to tell us to double certain magnitudes upon themſelves as often as we pleaſe, and then to obſerve whether the firſt and third having been ſo doubled upon themſelves an equal number of times reſpectively, exceed or equal the ſecond and fourth treated in the ſame manner, or fall ſhort of them.

Euclid's definition is doubtleſs embarraſſing; beginners do not eaſily catch the ſenſe; beſides the new terms it contains, and the neceſſity it impoſes of transferring the attention to alternate magnitudes, it involves another conſiderable difficulty. It expreſſes a reſult which ſcarce any beginner will be able to draw from the review of his experience. And yet it is abſolutely neceſſary, that before he proceeds, he

ſhould

ſhould be ſatisfied of its truth. It has been idly obſerved, that though no ſuch thing as a circle or ſtraight line ſhould exiſt, our reaſoning concerning them would be equally valid. But did not our ſenſes apprize us of their exiſtence, and enable us to diſcover their properties, it is very certain that we ſhould never have reaſoned at all concerning them. Every *definition*, as they are perhaps not happily termed, either expreſſes an object that lies immediately within the ſphere, and can be recognized by a ſingle application, of the ſenſes; or elſe a reſult which requires ſeveral ſucceſſive applications of the ſenſes. In the firſt caſe the object, if not already well known, muſt be exhibited; in the ſecond, we muſt make ſucceſſive appeals to the ſenſes. In order "to underſtand Euclid's fifth definition, ſays Mr. Ludlam, inſtances of its applica-

tion in particular caſes muſt be given *." The reſult, deſcribed in the definition, was probably deduced by its author from a train of mental experiments, which he omitted to ſet down, as Sir Iſaac Newton has ſometimes done. Every commentator has laboured to ſupply theſe reflections or experiments. And this muſt be done by the teacher, for there will be few beginners ſagacious enough to deduce it for themſelves.

* P. 167. In another paſſage, however, the ſame author ſets out with this groſs miſtake. "The buſi-" neſs of ſcience, ſays he, is from a few general prin-" ciples, to draw a great number of particular con-" cluſions." Mr. Ludlam's book having, in the courſe of a few years, come to a third edition, muſt have been put into the hands of many young people; and one may, on this account, lament the more that his notions are ſo confuſed as they often appear to be.

The

The difficulty may be eaſily overcome: let us recollect, that the only way of explaining words denoting ſimple perceptions, is to excite thoſe ſimple perceptions; and again, that the only way of explaining general terms, is to enumerate the ſigns of thoſe ſimple perceptions which the general term comprehends; and then, if neceſſary, to explain the ſimple terms, by ſhewing the correſponding objects. Now *proportion* ſignifies a ſeries of ſimple perceptions. Upon applying a meaſure to two diſtant objects, and finding one to be ten times as tall as the other, we ſay that the height of A has to that of B, the proportion of ten to one; and with reſpect to height, we conſider B as the tenth *portion* of A. But proportion is uſed to expreſs a more indirect and complicated compariſon of quantities, when we can neither bring them together, nor meaſure them ſeparately.

Thus

Thus when of two perſons varying in ſize and ſtature, I ſay of one (A) that he is as fat as another (B), I may not mean that the weight of A's fat equals the weight of B's fat: but that if the bones and muſcles of A are half as heavy as the bones and muſcles of B, the fat of A ſhall be half as heavy as that of B. If Britain among eight millions of inhabitants, have 200 perſons eminent in ſcience and letters; and France, with 25 millions of inhabitants, have 625 perſons equally eminent in ſcience and letters, then the Engliſh, you would ſay, is as learned and ſcientific a nation as the French. And for the reſult of this indirect kind of compariſon, we uſe the term *proportion* *:

and

* For want of being acquainted with the analyſis of terms, which indeed in the extent to which it has been lately carried, may be conſidered as a new ſpecies of knowledge, Dr. Price, I think, has maintained, that

the

and whenever we find four magnitudes, of which we perceive one to be exactly as great, when compared with the ſecond, as the third is when compared to the fourth, we expreſs this obſervation, by ſaying, that the firſt has to the ſecond, the ſame ratio that the third has to the fourth. With reſpect to numbers, when according to Euclid's

the idea of proportion is derived from the underſtanding, and not from the ſenſes. He might as well have maintained, that the ideas of *a*, *b*, *c*, as uſed in algebraic equations, are derived from the underſtanding, and not the ſenſes. Abſtract terms partake very much of the nature of algebraic terms, and if Dr. Price had happened to view them in this light, he muſt have ſeen his error. The higheſt and moſt perfect abſtraction is exhibited in the terms of algebra. To many abſtract words of common language, ſomewhat of their individual corporeal ſignification adheres; and even when this is quite loſt, they can only repreſent a certain ſort or number of ideas, which being in frequent

Euclid's 20th definition of book the 7th, the firſt number is the ſame multiple, or part of the ſecond, as the third is of the fourth; we ſay, that the firſt bears the ſame proportion to the ſecond, that the third bears to the fourth. In making experiments upon any four magnitudes, of which we perceive the firſt to be exactly as great, when compared to the ſecond, as the third compared to the fourth; we ſhall

quent demand, it is convenient to have ready bundled up for exchange in converſation, to ſave the trouble of counting them out, one by one. In algebra the ſigns are arbitrary; and hence their force is perfectly clear. The ſenſe is not obſcured by the intruſion of any collateral ſignification, nor do we pre-ſuppoſe that knowledge of the import of the terms, which often with reſpect to complex terms, does not exiſt, but aſſign it preciſely beforehand.

Mr. Ludlam informs his readers, "that the idea of ratio is a ſimple idea," p. 166. Such is his apprehenſion of the nature of terms.

find

find, that when we double the firſt and third upon themſelves any equal number of times, they will for ever be reſpectively as great, when compared to the ſecond and fourth, either remaining as at firſt, or alſo doubled upon themſelves any equal number of times; ſo that having clearly conceived this by the help of trials upon various magnitudes, we ſhall be aſſured, that if the firſt and third of four magnitudes, be taken any equal number of times, and the magnitudes thence reſulting ſhall not be exactly as great, when reſpectively compared to the ſecond and fourth, or their equimultiples, the one as the other, then the original four magnitudes cannot have been exactly as great, the firſt compared to the ſecond, as the third compared to the fourth. From this conſtant correſpondence in the increaſe of proportional magnitudes, it will appear, upon due conſideration, that whatever

ever equimultiples be taken as aforeſaid, the firſt multiple will be, when compared with the ſecond, what the third is when compared with the fourth *.

Con-

* Under the title of "*A Demonſtration of the 5th Def. B. V. of Euclid*," Mr. Robertſon, public Lecturer in Geometry at Oxford, has proved by a very ingenious train of experiments, the truth of the reſult aſſumed by Euclid. He ſhews, that if we have four magnitudes, the firſt being to the ſecond as the third to a magnitude leſs than the fourth, (for inſtance, 6, 4, 9, 7,) then it is poſſible to take certain equimultiples of the firſt and third, and certain equimultiples of the ſecond and fourth, ſuch, that the multiple of the firſt ſhall be greater than that of the ſecond, but the multiple of the third not greater than that of the fourth. Thus:

$(6\times6=)\ 36\ (8\times4=)\ 32\ (6\times9=)\ 54\ (8\times7=)\ 56$;

Again he ſhews, that if the firſt be to the ſecond, as the third is to a magnitude greater than the fourth, then certain equimultiples can be taken of the firſt and third, and certain equimultiples of the ſecond and fourth;

Concerning the language uſed in deſcribing proportion, there prevails an erroneous notion. Sir Iſaac Newton has tranſmitted it to the writers of compendiums of mathematics and natural philoſophy; it reſpects

fourth; ſuch, that the multiple of the firſt ſhall be leſs than the multiple of the ſecond, but the multiple of the third not leſs than the multiple of the fourth. Thus, to illuſtrate the ſenſe of this propoſition alſo, by an arithmetical example, take 6, 4, 9, 5, and repeat the alternate numbers the ſame number of times.

Let $(6\times3=)$ 18 $(4\times5=)$ 20 $(9\times3=)$ 27 $(5\times5=)$ 25; the magnitudes, being doubled upon themſelves, increaſe ſo, that when the 4th is leſs when compared with the 3d, than the 2d when compared with the 1ſt, the multiple of the 2d comes to exceed or equal that of the 1ſt, when that of the 4th is leſs than that of the 3d, and *v. v.* when the 4th is greater compared the 3d, than the 2d with the 1ſt.

By taking different pieces of tape, of which the 4th is made firſt greater and then leſs, when compared to the 3d, than the 2d when compared to the 1ſt; a boy

ſpects the meaning of the word AS. The truth is, AS has no peculiar ſignification as has generally been aſſerted after him. It may always be exchanged for *it*, *that*, *what*, or, *that which*, alike in common and philoſophical diſcourſe. Thus, *New-*

will ſoon perceive, that by being doubled itſelf, while the reſt are alſo doubled upon themſelves in the manner directed by Euclid; this 4th will be at ſome period of its multiplication, equal to the 3d, or leſs, or greater, when the multiple of the 2d is not equal, or greater, or leſs, than that of the firſt. By this exerciſe he will catch the point of view, under which to conſider the proportion of magnitudes accurately. The appearance of abſtruſeneſs, which is ſo repulſive to beginners, ariſes ſolely from their having been unaccuſtomed to compare magnitudes with attention in this way. The idea of teaching the elements of geometry by lengths of tape, may ſeem ridiculous; but I ſuſpect, that thoſe who are accuſtomed to give inſtruction in this part of the ſcience, are not entitled by their experience of the ſucceſs of the uſual method to be ſcornful.

ton

ton is to other philoſophers, as Homer is to other poets; or what *Homer is to other poets, Newton is to other philoſophers, viz. the greateſt of all.* So in ſtating proportions, as 3 is to 4, as 9 is to 12, or 3 is to 4, what 9 is to 12; and A is to B inverſely or reciprocally, *what* C is to D; and *as* or *what* ſhortly expreſs *as much greater or leſs than*, or *equal to*. See the Επεα πτερ. p. 283, and the following pages, where the article or conjunction *as*, is ſo far explained, though its original *corporeal* ſenſe is not given.

III.

Mr. Locke defines *knowledge* to be *the perception of the connexion and agreement, or diſagreement and repugnancy, of any of our ideas.* In this alone, ſays he, it conſiſts. "Where this perception is, there is knowledge; and where it is not, there, though

we may fancy, gueſs or believe, yet we always come ſhort of knowledge; for, when we know that *white is not black*, what do we elſe than perceive, that theſe two ideas do not agree?" He goes on, through the firſt chapter of his fourth book, to illuſtrate his definition. But his definition appears to me far from happy, and his commentary far from ſatisfactory. It would perhaps have ſpared him ſome unneceſſary words and vain diſtinctions, if, as knowledge ariſes from the perceptions of ſenſe, he had made it to conſiſt in the conſciouſneſs of thoſe perceptions; but I ſuſpect his favourite example of the equality of the three angles of a triangle to two right angles, which he alſo produces upon this occaſion, led him to adopt expreſſions not ſufficiently comprehenſive. When he obſerved, B. II. ch. 1. §. 19. that "it is the "affectation of knowing *beyond what we*

"*per-*

" *perceive*, that makes ſo much uſeleſs noiſe " and diſpute in the world :" he ſeems to give his ſanction to the moſt ſimple and ſatisfactory deſcription of *knowledge*, that can be comprehended in a few words.

In the next chapter, Mr. Locke treats of the degrees of knowledge: of theſe he enumerates the *intuitive*, the *demonſtrative*, and towards the end, with ſome heſitation, as if conſcious of inconſiſtency, he mentions the *ſenſitive* knowledge of particular exiſtence *. The intuitive he makes to conſiſt in the immediate perception of the agreement or diſagreement between two

* It is evident, that if we perceive only ideas, we can have no perception of particular exiſtence. Mr. Locke's embarraſſment proceeds from one or both of theſe cauſes: 1. He ſeems deſirous to explain the mechaniſm of thinking; and 2. To give ſuch an explanation, as ſhall comprehend both memory and perception; hence he talks only of the perception and compariſon of *ideas*.

ideas, without the intervention of any other. "Demonſtrative knowledge," he goes on to obſerve, "is where the mind "perceives the agreement, or diſagreement, "of any ideas, but not immediately." "The reaſon," he ſoon afterwards adds, "why the mind cannot always perceive "preſently," (i. e. at once) "the agree- "ment or diſagreement of two ideas is, "becauſe thoſe ideas, concerning whoſe "agreement or diſagreement the enquiry "is made, cannot by the mind be ſo put "together as to ſhew it *. In this caſe "then, when the mind cannot ſo bring

* Would it not be more ſimple to ſay, that we cannot contrive any ſingle experiment, which ſhall exhibit the reſult in queſtion? Thus we cannot ſhew at once, the equality of the angles of a triangle to two right angles. Whether the mind can or cannot bring the ideas together, depends entirely on the power of the eye.

"its

" its ideas together, as by their immediate
" compariſon, and as it were, juxtapoſition,
" or application to one another, to per-
" ceive their agreement or diſagreement,
" it is fain, by the intervention of other
" ideas . . . to diſcover the agreement or
" diſagreement, which it ſearches; and this
" is what we call reaſoning. Thus the mind
" being willing to know the agreement
" or diſagreement in bigneſs, between the
" three angles of a triangle and two right
" ones, cannot by an immediate view and
" comparing them, do it: *becauſe the three*
" *angles of a triangle cannot be brought at*
" *once, and be compared with any one or two*
" *angles*; and ſo of this the mind has no
" immediate, no intuitive knowledge. In
" this caſe the mind is fain to find out ſome
" other angles, to which the three angles
" of a triangle have an equality; and find-
" ing thoſe equal to two right ones, comes
" to know their equality to two right ones.

"Thoſe intervening ideas, which ſerve to "ſhew the agreement of any two others "are called *proofs*, and where the agree- "ment or diſagreement is by this means "plainly and clearly perceived, it is called "*Demonſtration*." (B. IV. ch. ii. §. 2.)

In §. 7, he gives this deſcription of the proceſs of demonſtrative reaſoning. "*In* "*every ſtep* reaſon makes in demonſtrative "knowledge, there is an intuitive know- "ledge of that agreement or diſagreement "it ſeeks with the next intermediate idea "which it uſes, as a proof. For if it were "not ſo, that yet would need a proof; "ſince without the perception of ſuch "agreement or diſagreement there is no "knowledge produced; if it be perceived "by itſelf, it is intuitive knowledge: if it "cannot be perceived by itſelf, there is "need of ſome intervening idea, as a "common meaſure to ſhew their agree- "ment or diſagreement. By which it is

"plain

" plain, that every ſtep in reaſoning that " produces knowledge, has intuitive cer- " tainty."

There is only a ſlight difference in terms between this theory and that which I have endeavoured to explain, and to confirm by examples. And this difference of terms is owing to the independant origin of the latter. It vaniſhes altogether, as ſoon as it is obſerved, that intuitive and experimental knowledge are one and the ſame thing. The reader, I ſuppoſe, will not in this caſe think it neceſſary to attend to the common diſtinction between *obſervation* and *experiment*; to our perceptions it is of no conſequence, whether we diſpoſe objects on purpoſe to obſerve them, or are mere ſpectators. The perception is the ſame by whatever agency the objects are preſented to the ſenſes; though in geometry, we meet with infinitely more reſults of *experi-*

ment than of *obſervation* *, for we go on perpetually making experiments and accumulating reſults.

Clear as it is, no uſe has, I believe, yet been made of Mr. Locke's account of demonſtrative evidence, either to ſolve difficulties or to improve the method of teaching in geometry: the ſhame, however, will be divided between ſo many culprits, and ſome of them ſo illuſtrious, that the ſhare of each will be exceedingly ſmall. For the ſubject has not fallen into the hands of ordinary compilers merely. Among the commentators upon Euclid, one might enumerate men of comprehenſive views and various

* We have theſe reſults of *obſervation* in other paſſages of Euclid, beſides the *axioms* of the firſt book. The firſt, ſecond, and other propoſitions of the fifth book are *ſelf-evident*, that is, we aſſent to them as ſoon as we underſtand the terms, which could not be, if we were not acquainted with appearances, warranting the reſult.

infor-

information; and authors, who aſpire far beyond the commentator's higheſt praiſe, have formally diſcuſſed the nature of mathematical evidence.

The great celebrity of a modern ſyſtem of pneumatology, conſpires with the opportunity I have had of conſulting it, ſince the beginning of this eſſay was ſent to the preſs, to induce me to alter the reſolution I originally formed, to avoid the quotation and diſcuſſion of different opinions. The author of this ſyſtem is Mr. Kant, Profeſſor at Koenigſberg in Pruſſia. Mr. Kant has raiſed to himſelf throughout Germany, a reputation ſuperior to that of Wolf, and at leaſt equal to that of Leibnitz. Among his numerous followers he reckons men of eminence, who having firſt violently oppoſed his doctrines have, by a tranſition not uncommon, become his zealous advocates: nor does that oppoſition

which

which new opinions always have to encounter, ſeem to have ſerved any other purpoſe than to diffuſe thoſe of Mr. Kant. They have already been publicly taught at Jena, and there is reaſon to believe that they are making their way at Gottingen *. At a period when full liberty of ſentiment is happily eſtabliſhed throughout the republic of letters, ſpeculations which have ſo powerfully influenced independant minds, are an object of rational curioſity; and an inquiſitive perſon might wiſh, that an explicit account of Mr. Kant's doctrines were publiſhed in Engliſh, which the terminology would render a difficult undertaking.

Mr. Kant, as might be expected in "*a critical examination of reaſon*," treats at

* See Gotting. Anzeig. p. 201. where in giving an account of Mr. Schultz's commentary upon Mr. Kant's moſt celebrated work, one of the Gottingen profeſſors half avows his converſion to the new ſyſtem.

large

large of the nature of demonſtration. The following quotation may ſerve in ſome meaſure to ſhew, in what light he conſiders it. I do not expect that it will do him much credit in this country, but it is unfair to judge an author ſo celebrated from a ſingle paſſage; a great work may doubtleſs contain much truth blended with a good deal of error. For my own part, I think that after Mr. Locke's diſcoveries, and thoſe relating to language, what chiefly remains to be done is, to digeſt the whole into an uniform ſyſtem, and to apply this ſyſtem to improve, or more properly ſpeaking, to create *the art of inſtruction.*

Mr. Kant aſſerts, "that we are in poſ-"ſeſſion of knowledge *a priori* *." In proof of his poſition he obſerves, "that in "order to make this appear, we have but

* Kritik der reinen Vernunft. 1787. p. 3.

"to

" to diſcover a criterion by which to diſ-
" tinguiſh between *pure* and *empirical* know-
" ledge. Now experience teaches us, that
" a thing is ſo, but by no means that it
" cannot be otherwiſe. If therefore there
" be, 1. A propoſition, which, when con-
" ceived, is accompanied with an idea of
" its neceſſity; this is a judgment *a priori*.
" Any propoſition too, deduced from ano-
" ther, which is itſelf a neceſſary propo-
" ſition, is altogether *a priori*. 2. Expe-
" rience never gives to its poſitions a real
" and ſtrict, but only an aſſumed and com-
" parative univerſality; i. e. one derived
" from induction. So that in propriety
" we ſhould ſay, that *as far as we have*
" *hitherto obſerved, this or that rule admits*
" *of no exception*. If therefore a propoſi-
" tion be conceived as ſtrictly univerſal, or
" in ſuch a manner, that no exception is
" allowed poſſible, this is not derived from

" expe-

" experience, but comes *a priori*. Empi-
" rical univerſality is therefore only an ar-
" bitrary extenſion of that which holds in
" moſt caſes to all caſes, without excepti-
" on; as in this propoſition, *all bodies are*
" *heavy*. On the contrary, where ſtrict
" univerſality eſſentially appertains to any
" judgment, this circumſtance points out
" a peculiar origin of ſuch judgments, viz.
" a power of knowledge (*ein vermoegen des*
" *erkenntniſſes*) *a priori*. Neceſſity there-
" fore and ſtrict univerſality are ſure crite-
" rions of knowledge *a priori*; and they are
" inſeparably connected. But as in the ap-
" plication of theſe criterions it is ſome-
" times eaſier to point out the empirical
" limitation of knowledge, than contin-
" gency in our judgments, and again, as
" the unlimited univerſality we attribute
" to any judgment (or propoſition,) is often
" more obvious than its neceſſity, it is, for
" theſe reaſons, adviſeable to uſe ſeparately

" theſe

" theſe two criterions, of which each is " by itſelf infallible.

" That ſuch neceſſary and univerſal, and " conſequently pure judgments *a priori*, " may be found in human knowledge, it " is eaſy to ſhew. If you chooſe an ex- " ample from ſcience, I refer you *to all the* " *propoſitions in mathematics:* if you would " have one from the moſt common exer- " ciſe of the underſtanding, the propoſi- " tion, that *every change muſt have a cauſe*, " may ſerve for this purpoſe. Indeed the " idea of a cauſe, ſo obviouſly contains the " idea of the neceſſity of connection with " an effect, and a ſtrict univerſality of the " rule, that it (the idea of a cauſe) would " be entirely loſt, if with Hume we were " to deduce it from the frequent aſſocia- " tion of ſomething that happens with " ſomething that precedes, and from an " habit thence acquired of connecting the

" ideas,

" ideas, conſequently were to reduce it to " a ſubjective neceſſity, or a neceſſity re- " lative to the obſerver *."

This paſſage, as I apprehend it, includes a conſiderable number of miſtakes; ſome more, ſome leſs, immediately connected with

* Modern writers, in our own language, expreſs an opinion, ſimilar to that of Mr. Kant, and inevitably fatal to their philoſophy of mind. " In ſtating the " argument for the exiſtence of the Deity, ſays Mr. " Stuart, ſeveral modern philoſophers (as Dr. Reid) " have been at pains to illuſtrate that law of our na- " ture, which leads us to refer every change we per- " ceive in the univerſe, to the operation of an effi- " cient cauſe;" (or as he juſt before terms it a *metaphyſical* cauſe, i. e. " ſomething which is ſuppoſed to " be neceſſarily connected with the change, and with- " out which it could not have happened.") " This " reference, he continues, is not the reſult of reaſon- " ing, but neceſſarily accompanies the perception; " ſo as to render it impoſſible for us to ſee the change, " without feeling a conviction of the operation of " ſome

with the ſubject of mathematical reaſoning. At the riſque of a little apparent digreſſion, I ſhall endeavour to point out the principal of theſe miſtakes. To conſider Mr. Kant's ſecond example firſt;—*cauſe*, from being the name of a particular object, has become, in conſequence of the obliteration

" ſome cauſe, by which it was produced; much in " the ſame manner in which we find it impoſſible " to conceive a ſenſation, without being impreſſed " with the belief of the exiſtence of a ſentient being." *Philoſophy of the H. Mind*, 1792, p. 73. Dr. Reid, I think, always miſtakes our *habits* of obſervation for *original laws* of thought, and ſeems not ſufficiently acquainted with the nature of complex terms. In the inſtance here quoted, the term *ſenſation* comprehends *ſentient* beings, as *pride* does proud beings, and *yawning* yawning beings; or, as *two* includes *one* and *one*. When we therefore conceive the meaning of theſe terms, we find that like all others, they include their own ſignification, or ſignify what they do ſignify, which, I ſuppoſe, is all the remark amounts to.

of

of that original ſignification, a remarkable abbreviation in language. If a perſon aſks himſelf, what is a cauſe? he will find that he has no idea affixed to the term; and can only explain it by the ſubſtitution of a particular object taken from a particular fact, or by ſaying that it means "ſomething which precedes ſomething elſe." Thus when electricity is ſaid to be *the cauſe* of thunder and lightning, we mean that thunder and lightning will never be obſerved, unleſs the electrical fluid be firſt unequally diſtributed. As ſoon as we trace back the word to its original meaning, (when only we can be ſaid to have an idea of it) we ſhall find that it includes no idea of neceſſity; and in this manner the etymologiſt will generally correct the errors of the metaphyſician. Thus, cauſſa *ſeu* cauſa, ſays Mr. Scheid, a *καυσω futuro thematis καυω proprié* pungo, ſtimulo, *unde*

κᾶυσις, cauſa, *ut a* πᾶυσις, pauſa. Cautes c. q. καυ∫ης, *unde* cauterium, *a* κεκαυ∫αι, *th.* καυω, pungo (*Etymol. p.* 1183). Lennep. (ibid. p. 380.) obſerves, that in καιω, the ſame word with καυω, only differently ſpelled, the ſignification of burning is ſecondary to that of pricking. Thus, καιω, uro: Ortum eſt *a ſimpliciori* καω, *cujus notio propria quærenda eſt in* motu rei impulſæ & ſtimulatæ. Thus we ſee the term *cauſe* originating in the motion produced in living animals, by the application of a pointed body occaſioning pain. People in general are as little conſcious of *cauſe* having any proper ſignification, as of one of the letters of the alphabet having a proper ſignification; and could language have been formed after the knowledge of its principles, we might as well have taken one of the letters of the alphabet for ſuch an abbreviation. And then I ſuppoſe we ſhould never

never have heard of an idea of cauſe, independent of obſervation and experience. It is curious to remark, how we uſe *cauſe* in a ſenſe not only remote from its proper ſenſe, but often at croſs purpoſes with it: thus we may either ſay, *corpulency is the cauſe of indolence*, or, *indolence is the cauſe of corpulency*. Inſtead of cauſe, it would ſeem ſtrange if we were to read, *ſtimulus*, *ſpur*, or *goad*.

Cauſes or *events preceding other events*, may perhaps be arranged, with reſpect to our manner of obſerving them, in three claſſes. 1. Thoſe of which we are mere inactive, though attentive ſpectators; of ſuch cauſes aſtronomy, geology, medicine and phyſiology, ſupply abundant examples. 2. Thoſe of which we acquire the knowledge, by diſpoſing objects purpoſely to obſerve the ſucceſſion of events, as in certain phyſical inveſtigations. 3. Thoſe of which

we acquire the knowledge by our moſt ſimple and earlieſt obſervations and experiments, as of impulſe producing motion. This laſt claſs differs from the preceding, only in date, and in being unaccompanied with the remembrance of meditation. But the ſpontaneous and early origin of the knowledge of theſe laſt cauſes, has deceived the philoſophers. They have taken away from experience the credit of our inſtruction, for want of perceiving how early ſhe begins her leſſons.

Neceſſity, like *cauſe*, is a mere abbreviation, and we have no idea of neceſſity more than of cauſe: neceſſity is ſo looſe a term, that by obſerving various phraſes, in which it occurs, we ſhall perceive that it ſtands as a ſubſtitute for the moſt diſſimilar ideas. When a maſter ſays to his ſervant, "it is neceſſary that you be more ſober henceforward," he means, "unleſs you be more ſober,

ſober, I ſhall diſcharge you :" or a phyſician to his patient ; " it is neceſſary you take this medicine ;"—he means, " if you do not take this medicine, be aſſured you will not recover ;" that is to ſay, when reſolved farther into experience, " he has " ſeen, or others have ſeen, the medicine " cure the diſorder." Thus it is, that the forms of ſpeaking have uniformly deceived metaphyſicians ; and thus all words, and combinations of words, may be referred to perceptions of ſenſe, and none can mean any thing beyond obſervation.

To obſervation and to induction alone, whatever Mr. Kant may imagine, it is eaſy to ſee that we owe our knowledge of the abſolute neceſſity or ſtrict univerſality of geometrical truths : one cannot indeed be ſurprized, that the induction of particulars ſhould have eſcaped thoſe whom the nature of the reaſoning has totally eſcaped. Every

one of us, by calling to mind the train of his own thoughts, may aſſure himſelf that, in ſtudying Euclid, however rapid, and however eaſy the proceſs of induction, we have at leaſt, in imagination, varied the forms of the diagrams, and finding the reaſoning equally applicable to all imaginable varieties, have aſſented to the truth of the propoſitions. A beginner, eſpecially if he be young, will not be ſatisfied even in the ſimpleſt caſe, till he has gone through the proceſs of induction. He will actually ſtop to vary the interſections and approximations of two ſtraight lines, till he is convinced by experiment, that they cannot incloſe ſpace; at leaſt, he will ſtop to imagine theſe ſimple experiments. One inſtance I have myſelf ſeen, and I ſuppoſe they are not uncommon, of a ſtudent drawing a figure anew upon a different ſcale, in order to ſatisfy himſelf, that the demonſtration

ſtration was not confined to the particular figure before him: and teachers, I believe, at ſetting out, ſeldom fail to call the attention of their hearers to this induction. "You ſee," they remark, "that the rea-"ſoning does not concern this or that par-"ticular ſhape or ſize of triangles, &c." A little conſideration ſatisfies the pupil, that the induction is full and perfect; and hence the ſtrict univerſality of mathematical truths.

The propoſition, quoted by Mr. Kant, *that all bodies are heavy*, does not ſupply a fair object of compariſon with geometrical propoſitions. Plane triangles, parallel lines, and the various kinds of ſolids, are definite things; clear perceptions fix the meaning of the term. But this is not the caſe with *all poſſible bodies*.

Mr. Locke has already obſerved, that intuitive is clearer and more certain than de-

monſtrative knowledge; and this, in as far as they differ, is unqueſtionably true; that which is immediately before the ſenſes, impreſſes us always with the moſt lively conviction. The ſphere of viſion when diſtinct is very limited; we are obliged to view things in ſucceſſion; and I ſuppoſe that the memory cannot bring more knowledge before the mind at once, than the eye can at once receive; moſt certainly no memory can offer at once that chain of experiments, by which it is ſhewn, that the ſquare of the hypothenuſe is equal to the ſquares of the two other ſides of a right-angled triangle. Every one muſt be conſcious, that if this truth could be exhibited to the eye at once, could it by any mechanical contrivance be made the firſt propoſition in geometry; our conviction would be more lively than that which we obtain from a number of reſults. What we ſee will

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ever ſeem more certain than what we remember to have ſeen; and when we have drawn out a long chain of conſequences, ſome latent apprehenſion will remain, that we may not have examined each ſucceſſive link, in a ſtate of perfect freedom from illuſion of ſenſe and diſtraction of thought. It is not then with much propriety that the anatomiſts have been reprehended for profaning a term, which ſome metaphyſicians would conſecrate and ſet apart to mathematical certainty. What is *ſhewn* to us by anatomy, we are juſt as ſure of, as of that which is *ſhewn* to us by geometry.

Mr. Locke felt that demonſtration is not limited to quantity. "It has been gene-
"rally," ſays he, "taken for granted, that
"mathematics alone are capable of demon-
"ſtrative certainty. But to have ſuch an
"agreement or diſagreement, as may in-
"tuitively

" tuitively be perceived, being, as I ima-
" gine, not the privilege of the ideas of
" *number*, *extenſion* and *figure* alone, it may
" poſſibly be the want of due method and
" application in us, and not of ſufficient
" evidence in things, that demonſtration
" has been thought to have ſo little to do
" in other parts of knowledge, and been
" ſcarce ſo much as aimed at by any but
" mathematicians. For whatever ideas we
" have, wherein the mind can perceive the
" immediate agreement or diſagreement
" there is between them, there the mind
" is capable of intuitive knowledge; and
" where it can perceive the agreement or
" diſagreement of any two ideas, by an
" intuitive perception of the agreement or
" diſagreement they have with any inter-
" mediate ideas, there the mind is capable
" of demonſtration, which is not limited

" to

"to ideas of extenſion, number, figure, "and their modes." (IV. 6. §. 9.)

Wherever we have clear perceptions, there doubtleſs we have the full evidence of demonſtration. It is as certain to me, that ſugar is ſoluble in water, as that two ſtraight lines can never encloſe a ſpace; or in Mr. Locke's more complicated language, my mind perceives as perfectly the agreement of the ideas of ſugar, and of ſolution in water, as it does the diſagreement of the ideas of two ſtraight lines, and incloſed ſpace. I may not ſo readily recognize ſugar and water, as I do certain figures of plane geometry, nor with ſuch certainty as I do all the plane figures and ſolids of geometry without exception. Now on this *facility* and *certainty* of recognition muſt depend the facility and certainty of the application of our knowledge. And as we can apply knowledge more readily, the more

more perfectly do we ſeem to poſſeſs it; in whatever light therefore we view it, we diſcover the abſolute dependance of mathematical knowledge upon ſenſe. And what our ſenſes are ſo perfectly adequate to convey, they have been ſuppoſed not to convey at all; juſt as we many times hardly perceive ourſelves doing, what we do with perfect eaſe. In comparing phyſical with mathematical knowledge, we ſhall find certainty ariſing, in the latter caſe, from the perfect competency of our ſenſes, in the former from their incompetency. What for inſtance is it, that prevents me from being as certain, that water conſiſts of hydrogene and oxygene airs, as of any propoſition in Euclid?—nothing ſurely but the incompetency of my ſenſes. In the firſt place, I cannot perceive whether theſe airs do not previouſly contain a large quantity of water: ſecondly, the heat that appears,

pears, and of which I have no adequate perception, perplexes me; and thirdly, the occaſional appearance of an acid in the water. Now if I could perceive the ſmall quantity of azotic air preſent, ſeparately uniting with a certain portion of the oxygene air to form acid, while the hydrogene air unites with the reſt to form water; if I could ſee that the airs previouſly contain only a little or no water beforehand, and if there was no heat and light, I ſhould have demonſtrative evidence—and that juſt as clear as that mucilage of gum arabic conſiſts of water and of that particular gum, or that ſuds conſiſt of ſoap and water.

Thus when the ſenſes ſerve us imperfectly, and make us continually feel our dependance, we are ever ready to acknowledge the obligation. But where they inſtantly deliver ſuch clear and full perceptions

tions to the memory, that we have no more occaſion to recur to them, we forget or deny the ſervice they have rendered us. —It is ſaid, that the ſtateſman, after the final ſucceſs of his intrigues, is apt to ſpurn away the ladder by which he has mounted to power. In like manner the metaphyſician, when he feels his mind elevated to what he calls *abſtraction*, or when his thoughts are employed about objects, of which the remembrance is almoſt as diſtinct as the perception, is apt to betray an inattention equal to the ſtateſman's ingratitude.

Pure phyſical ſcience ſeems faſt approaching to the fullneſs and clearneſs of mathematical ſcience; an obſervation of which more examples will be found in Mr. Lavoiſier's Elementary Treatiſe of Chemiſtry, than in any other book I am acquainted with. By contrivances calculated to convey the moſt diſtinct perceptions, he every where

where impreſſes a degree of conviction, which, not many years ago, would have been thought unattainable in the ſtudy of impalpable ſubſtances *. The different kinds of air may perhaps be conſidered, with reſpect to our ſenſes, as occupying a middle place between palpable bodies and the *ætherial* fluids, if any ſuch exiſt. Perhaps

* Mr. Condorcet, who poſſeſſes vaſt extent as well as accuracy of knowledge, and whoſe mathematical ſkill is well known, has lately expreſſed himſelf in the following manner concerning mathematical, compared with other, truths.—Mr. Buffon etablit cette opinion, que les verités mathematiques ne ſont point des verités reelles, mais de pures verités de definition; obſervatíon juſte, ſi on veut la prendre dans la rigueur metaphyſique, mais qui *s'applique egalement alors aux verités de tous les ordres*, deſqu'elles ſont preciſes & qu'elles n'ont pas des individus pour objet. Si enſuite, on veut appliquer ces verités a la pratique et les rendre dés—lors individuelles, *ſemblables encore en cet egard* a des verités mathematiques, elles ne ſont plus que des

haps another Lavoiſier, by bringing theſe as much within the ſphere of the ſenſes, may exhibit almoſt mathematical evidence of the qualities of *fire*, *electricity* and *magnetiſm*.

To ſolid geometry we do not come any thing near ſo well prepared by obſervation as to plane. The difficulty of *imagining* (which

verités approchées. Il n'exiſte reellement qu'une ſeule difference, c'eſt que les idées, dont l'identité forme les verités mathematiques & phyſiques ſont plus abſtraites dans les premiéres, d' ou il reſulte que pour les verités phyſiques, nous avons un, ſouvenir diſtinct des individus, dont elles expriment les qualités communes ; & que nous ne l'avons pour les autres : mais la veritable realité, l' utilité d'une propoſition quelconque eſt independante de cette difference ; car on doit regarder une verité comme reelle toutes les fois que, ſi on l'applique a un objet reellement exiſtant, elle reſte un verité abſolue, ou devient une verité indefiniment approchée.—*Hiſtoire de l'Acad. des Sciences, pour l'année*, 1788. p. 64. *Paris*, 1791.

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(which always depends on the want of opportunity, or of power to *perceive*) the interſections of ſolids, is always very ſenſibly felt. And here it is almoſt as neceſſary as in mechanics, to exhibit the objects, whoſe qualities are to be taught; and to call in the joint aſſiſtance of the hands and eyes. So far, if experiment mean the diſpoſal of

In ſtating the only difference which he believes to ſubſiſt between mathematical and other truths, Mr. Condorcet is not, in my opinion, perfectly accurate. The individual objects, of which we retain the remembrance, muſt excite a number of perceptions; and all or ſeveral of theſe, joined together, form the character. Now objects, preſenting but one or two circumſtances to notice, can ſcarce have an individual character; we think no more of the individual glaſs of water upon which we have made any obſervation, than upon the individual triangle or ſquare. So alſo in a great number of phyſical truths beſides, we have *not* any diſtinct recollection of the individuals, whoſe common properties theſe truths expreſs.

objects for the purpose of observing them, most teachers, doubtless, find it not less necessary to make experiments formally in the science of quantity than in that of motion.

Mathematics, in fine, teach either to measure or to count. The simplest and the shortest way we can acquire either of these arts, the better, I believe, in all respects. We cannot possibly set about to learn either of them otherwise *, than by the use of the senses. And it is by no means impos-

* Children may be observed to learn to count with great labour, and it is long before they learn the terms of number perfectly.—If you lay a very small number of similar objects before a child, that has been learning to count for some time, and ask how many there are, he will be obliged to stretch out his hand, tell them over slowly. The eye, which takes its other lessons so admirably from the touch, never attains much readiness in the discrimination of number. No person, I apprehend, can distinguish eleven from twelve similar objects at a glance.

sible,

ſible, that there may be a method of applying the ſenſes, in geometry at leaſt, to far greater advantage than any practiſed at preſent; a method at once agreeable, expeditious, and calculated to invigorate every mental faculty. When ſuch a plan of education ſhall be generally adopted, (and its outline is not extremely difficult to trace) that more knowledge than the learned and ſcientific now uſually poſſeſs at forty, may be acquired by twenty, ſuch a method of teaching geometry will probably form part of it.

If *all we know* be, as it aſſuredly is, juſtly reduced by Mr. Locke to intuition, (or perception, for demonſtration is but a ſucceſſion of intuitions or perceptions), *all we believe or expect* muſt be founded upon what others have reported concerning their intuitions, in addition to our own. And all evidence may therefore, I think, moſt con-

veniently be referred to two general heads; either, 1. What we ourſelves have perſonally experienced, or *direct evidence:* or, 2. What other perſons aſſert, that they have experienced, or *indirect evidence.* Thus we are confined within the circle of ſenſe by a ſpell caſt upon every individual of the human race, and ſuch as we can never by any efforts diſſolve.

Under the two heads of direct and indirect evidence, there are an infinite number of gradations of credibility; in our opinion concerning almoſt each particular, whether falling under the cognizance of our ſenſes or teſtified by others, we may acquieſce with a different degree of conviction. Should any one aſk, But how, ſince our ſenſes are fallible, ſhall we then attain to certainty? It can only be replied, that certainty is not among the privileges of our nature, except *that certainty*

tainty which is attainable by ſenſe. Infallibility was never, as far as I know, guaranteed to man; nor is there any danger leſt, like the Children in the Wood, we ſhould lay us down and die, leſt we ſhould fall into the pit of error. We differ from one another in every circumſtance of conduct, taſte, and ſentiment with perfect ſelf-ſatisfaction; and opinions for which he has only weak indirect evidence, each man entertains with the fulleſt aſſurance, notwithſtanding he has againſt him the bulk of his ſpecies. Often as human proneneſs to error is bewailed, it does not appear that many among us feel a ſincere anxiety for that degree of certainty, any more than for that extent of knowledge, of which we are perfectly capable.

Opinions were unfortunately, long prevalent in the world, of which the direct tendency was to deprive men of the moſt valu-

able, as being the moſt certain, portion of knowledge; namely, that which we acquire from our intuitions. Now there is, I am diſpoſed to believe, no abſurdity of opinion which is not productive of ſome pernicious practice. Even when the opinions are renounced and forgotten, the practices, into which they have deluded mankind, may ſtill prevail for ages. Of this truth, I either fancy or find a deplorable proof in the common conduct of liberal education. And if Mr. Locke * has in vain diſcovered the principles upon which education ought to proceed, and in vain applied them with great, but not unexceptionable ſkill, I attribute his want of ſucceſs, very much to

* I by no means forget Mr. Locke's great predeceſſor, Bacon, of whoſe *Advancement of Learning* I wiſh to ſee a new 8vo. edition. The common old thin 4to. is not adapted to modern delicacy in books, and it is not indeed a pleaſant book to read.

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the deep impreſſion left by the *Antient Metaphyſics*. We know how ſtudiouſly Plato depreciated the body, the ſenſes, and the informations of ſenſe; how his excommunication of our perceptive powers was confirmed by the peripatetic *phantaſms*, and how both were amalgamated with the fantaſtic religious opinions, that ſo long bewildered and brutalized mankind; as alſo what authority this monſtrous mixture of heterogeneous reveries maintained during a long ſucceſſion of ages. "The Platoniſts," ſays Mr. Harris *, "conſidering ſcience as "ſomething aſcertained, definite and ſteady "would admit nothing to be its object "which was vague, indefinite and paſſing. "For this reaſon they excluded all indivi- "duals or objects of ſenſe, and, as Am- "monius expreſſes it, raiſed themſelves in

* Treatiſes, p. 341.

" their contemplations from beings parti-
" cular to beings univerſal, and which
" from their own nature, were eternal
" and durable——.

" Conſonant to this, was the advice of
" Plato, with reſpect to the progreſs of
" our ſpeculations and inquiries, to deſcend
" from thoſe higher genera, which include
" many ſubordinate ſpecies, down to the
" loweſt rank of ſpecies, thoſe which in-
" clude only individuals. But here it was
" his opinion that our enquiries ſhould
" ſtop, AND AS TO INDIVIDUALS LET
" THEM WHOLLY ALONE; becauſe of
" theſe there could not poſſibly be any
" ſcience."

Such were the ravings of the parent of myſticiſm. And as the Barbarians of the Weſt could not but ſurvey with an enthuſiaſm, bordering on adoration, the fine compoſitions of the antient writers, this

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ſentiment greatly contributed, by an obvious aſſociation, to their baneful effect.

What has been the progreſs of phyſical and moral ſcience, ſince their cultivators have gone directly contrary to the *advice* of Plato, is well underſtood. The ſcience of grammar has been juſt created upon preciſely the ſame principle; but moral, phyſical, and grammatical inſtruction, which, as well as diſcovery, muſt in order to be efficacious, proceed from the obſervation, or, if you pleaſe, the exhibition of particulars, is ſtill conducted after Plato's own heart; and were he now to viſit our ſeminaries, there is every reaſon to preſume, that this contemplator of *beings univerſal*, would be ſatisfied with our conformity to his injunctions. For aſſuredly, we neglect, as much as in us lies, the cultivation of all ſuch knowledge as the ſenſes convey, and we let individuals wholly

alone.

alone. The conſequence is, that when the term of education has elapſed, the greater number find themſelves utterly deſtitute and helpleſs; without an outline which they can afterwards agreeably employ themſelves in filling up, and with few or no acquiſitions which they can apply to the ſervice of others. The few, who feel the pride of ſuperior powers, have nothing upon which to pique themſelves, but certain pretenſions to *taſte*.

Theſe pretenſions, I believe, ſhew, on the one hand, how, according to Mr. Locke, men, after eſpouſing certain well-endowed or faſhionable opinions, "ſeek argu-" ments, either to make good their beau-" ty, or varniſh over and cover their de-" formity;" while, on the other, they furniſh an inſtance, where more than ordinary penetration is required to make a diſcovery, or more than ordinary ingenu-

ouſneſs

ouſineſs to produce a confeſſion; one may expect indeed, that a man will keep ſuch a ſecret as long as he can from himſelf as well as from others; for however much he may have laboured, and however regularly he may have paſſed through the forms, he muſt either ſummon up reſolution to begin anew, or perpetually carry about with him a moſt humiliating conſciouſneſs.

Where the taſte has been almoſt excluſively cultivated, the character will be without energy, and its moſt prominent feature will be that *delicacy of feeling*, againſt which Mr. Hume has entered ſo juſt a proteſt. Gray, ſtripped of his genius, is a tolerably fair model of a man of mere taſte; and nothing can be well imagined leſs deſireable, than Gray's ſickly conſtitution of mind. Nothing, I think, affords a more lively repreſentation of intellects thus puny and paſ-

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ſive than thoſe maſſes of animated jelly, which one ſees at times ſcattered along the ſea ſhore, without bone or tendon, that quiver to every blaſt and ſhrink at every touch.

The futility of this plea in defence of a method, according to which more time is conſumed and more drudgery undergone, than would be ſufficient to learn half the Encyclopedia in addition to all that is really acquired, plainly appears from the aſſociation of the moſt exquiſite taſte with the greateſt proficiency in phyſical knowledge. Such an aſſociation has frequently been ſeen in our times; in Haller, Born, Buffon, Murray, Darwin, and twenty others *. It will more and more frequently be

* Mr. Heyne, whoſe taſte and judgment will not be diſputed, ſpeaks always in as high terms of *real* or phyſical knowledge, as Bacon himſelf.——" Naturæ vero cognoſcendæ ſtudium, omnis veræ philoſophiæ

be ſeen, as we diſcard more and more of the inveterate errors and inadequate inſtitutions of our anceſtors. It is impoſſible to doubt that out of every hundred of liberally educated perſons, whatever be the extent of their capacity, ninety *might* have acquired as correct a taſte and infinitely more knowledge than they poſſeſs.

As claſſical literature is not the whole, nor the moſt important part of that which ought to be taught in the courſe of a good education, ſo even to acquire this, ſome better method than that which we at preſent follow is wanting *. In fact, many of thoſe,

phiæ fundus, humanæ mentis fax certiſſima—*ad ea quoque, quæ extra ſenſibilem naturam poſita eſſe dicuntur.*" Opuſc. iii. 204.

* Hemſterhuſius, ſays Mr. Ruhnken, *commentitias anomalias, quibus grammatici omnia perturbaſſent, exploſit, denique tenebras linguæ (Græcæ nempe) per tot ſæcula of-*

fuſas

thoſe, who are made to devote years to the purſuit, approach no nearer to the object, than

fuſas ita diſcuſſit, ut qua linguâ nulla eſt neque verbis neque formis copioſitor, eâdem JAM *nulla reperiatur ad diſcendum facilior.*

Our ſchool boys, I am much afraid, know nothing of the conſummation announced by Mr. Ruhnken's JAM, however devoutly they may wiſh for it. They feel what Lennep ſays will be the caſe, as long as the Greek language is taught according to our barbarous grammars—*nihil triſtius ejus ſtudiis invenietur; nihil quod poſſit juventutem ab ejus linguæ culturâ, deterrere magis.* We are unacquainted with thoſe few and ſimple rules, *ad quas omnia in linguis, tanquam ad normam certiſſimam, exigi poſſint; quas ex ipſâ linguæ naturâ ductas, & ratione ſuffultas, memoriæ infigere, & infixas conſervare diutiſſime poſſis* (p. 4.).

Aſſuredly, if the ſpirit of the claſſics be ſo ſalutary to the youthful mind, we ſhould infuſe it as effectually as poſſible. Now, beſides thoſe quoted above, there is other high authority for ſuppoſing, that our methods are not ſo effectual as thoſe practiſed elſewhere.

than children when they give chace to the extremity of the rainbow. Nor is any thing more

——Some years ago, the king, ſuppoſing that our ſchool editions of the claſſics, might be uſeful in his German Dominions, ordered a collection of the books uſed at Weſtminſter and Eton to be ſent to Mr. Heyne, at Gottingen. Of theſe editions Mr. H. has publiſhed a review, (Gottingen Magazine, 1780, No. 6, p. 429, &c.): he marks the greater number with a ſtrong note of diſapprobation. He is ſtruck by the metrical part of our Latin grammar; obſerves, that it muſt needs be very crabbed and obſcure; and ſeems to doubt, whether we are quite ſo abſurd as to force children to learn it by rote. It is, indeed, to be hoped, that this moſt painful inſtrument of grammatical torture will ſoon be generally laid aſide. Our claſſical ſcholars would perhaps wiſh, that the whole of this Review might be tranſlated; and were Mr. H.'s remonſtrances likely to produce an alteration, it would be a work of humanity to tranſlate it: but I ſuſpect that the immediate effect would be to alarm our pride, rather than correct our errors, as the following expreſſions,

more common than to ſee the ſchool and college books, finally conſigned over to the damps and cobwebs of the dark cloſet, the moment their poſſeſſor becomes *ſui juris*.

preſſions, which may be conſidered as a ſummary of this celebrated profeſſor's opinion on our method of inſtruction in the Latin, may ſerve to ſhew: "Ex- "perience, he ſays, proves, that good Latin is no "where more uncommon than among the Engliſh. "Their beſt ſcholars (*Humaniſten*) often write a Latin "ſtyle, full of ſoleciſms and barbariſms. Even in "ſome ſchool-books the preface and additional mat- "ter are expreſſed in very bad Latin. At this no one "will be ſurprized, when he ſees how they are in- "troduced early in life, to the knowledge of Latin."

Mr. Heyne ſpeaks in the moſt contemptuous terms of that, which Mr. Harris calls Dr. Clarke's "*rational edition of Homer.*" The tranſlation, he ſays, is barbarous, and a diſgrace to the poet—the grammatical obſervations either falſe or trivial, the thouſand times repeated references to which he thinks intolerable: the notes, he beſides obſerves, ſeldom afford the illuſtration wanted, &c. &c.

It was partly in order to ſtrengthen, if poſſible, thoſe arguments that have been urged in favour of a plan of education which ſhall pay ſome attention to the ſenſes and the underſtanding, by many illuſtrious writers from Locke to Condorcet *; partly to take away from the revivers of exploded abſurdities, that ſupport which they have been deſirous to gain, by forcing into an unnatural alliance with their cauſe, ſo reſpectable a ſcience as mathematics; and partly to ſhew what falſe meaſures of objects are taken by thoſe who have no better rule than *antient metaphyſics*, that theſe remarks are offered to public conſideration. Had it not been for ſuch collateral views, that eminent patron of lite-

* In his memoirs on *public inſtruction*, in the *Bibliotheque de l'homme publique*.

rature, whoſe name may be read at the bottom of the title page, ſhould never have riſqued upon them the expence of paper and printing.

THE END.

NOTE I.

On the Syſtem of the Greek Language, propoſed by *Schultens*, *Hemſterhuis*, their Diſciples, and by Lord *Monboddo*.

Neque enim ad grammaticorum regulas linguæ fuerunt conditæ; ſed ex linguis multo uſu populorum jam tritis et excultis, regulæ tandem ſunt formatæ.

Lennep. de Analogiâ, p. 55.

IT ſeems to me hardly poſſible for a mere claſſical ſcholar to make any diſcovery of importance concerning the ſtructure of language. The little ſucceſs with which the long-continued efforts of ſuch ſcholars have been attended, and a conſideration of the Greek and Latin languages themſelves, concur in countenancing this opinion. The Greek language (and the Latin is ſcarce any thing but a dialect of the Greek), has its ſurface ſo highly varniſhed, and its joints ſo cloſely fitted, that the acuteſt ſurveyor could hardly ever have aſcertained the original materials of which

it is compoſed, or have diſtinguiſhed the ſize and ſhape of the pieces that are thus nicely adapted to one another. Had not Schultens and Ten Kate deduced ſome juſt ideas from the ſtudy of languages, whoſe ſtructure lies more open to inſpection, I cannot eaſily perſuade myſelf, that Hemſterhuis, or his diſciples, Lennep and Valckenaer, would have contributed any thing material towards the philoſophy of language. Their merit would, in all probability, have been confined to the purification of the Greek grammar from a few of its abſurdities, and to the interpretation and amendment of paſſages in Greek authors.

That I may not be accuſed of lightly advancing this unfavourable ſurmiſe, I ſhall endeavour to render it probable, that their knowledge of Greek has ſerved but to bewilder the Dutch Etymologiſts. The more, indeed, I conſider their ſyſtem of *analogy*, the more does it appear to me to be repugnant to every kind and degree of evidence, as well as productive of particular errors without end. Where one can judge of their poſitions with any ſort of confidence, there they ſeem palpably erroneous; and in other caſes, contrary to all probability. Nevertheleſs, what they have collected, when properly arranged, will be found

to confirm the true theory of language, by many very ſtriking facts.

I have already looſely obſerved, that their ſyſtem ſuppoſes a regular derivation of the language from a few ſhort primitives. Lord Monboddo, more conſiſtent and more ingenious, reduces without heſitation, the Greek primitives to the five duads already quoted, and had he turned his attention to the manner in which the vowels are formed by the organs of ſpeech, and perceived their near affinity, he might with juſt as much propriety have reduced all his primitives to one.

The Dutch etymologiſts do not all ſeem to have ſo much narrowed the baſe of their building. They aſſume triliteral and quadriliteral as well as biliteral roots, and are doubtful whether there are not quinqueliteral. *Inveſtigari poterit*, ſays Valckenaer, *quot probabiliter a* primis ſapientibus illis linguæ conditoribus, *ſimplicia primitiva fixa fuerint & conſtituta—Verba primitiva ab* A *incipientia in antiquâ & ſimplici linguâ fuerunt tantum duodecim nempe*, ἀβω, ἀγω, ἀκω, ἀλω, ἀμω, ἀνω, ἀπω, ἀρω, ἀσω, ἀτω, et ἀω.—— *Ab* ακω, ακαω, ακιω, ακιω, ακοω, ακυω—and ſo on till you come by a regular increaſe, even to ακρωτηριαζω.—Again,

Ab αγω, αγαω, αγαζω, αγαιω, αγαλλω, αγανω.—Mr. Scheid, when he calls the five biliteral, the moſt

ſimple roots, (Etym. p. xlvi and xlvii), ſeems to come nearer to Lord M. as does alſo Lennep, when he obſerves, *manifeſtum itaque eſt, extitiſſe in antiquâ linguâ Græcâ* has quinque *formas ſimpliciſſimas* (viz. the duads), *eaſque adeo merito* primas *Græcæ linguæ* origines *dici.*

In this manner, then, by inſertion of letters, and the conſequent enlargement of words, was the whole language formed from a few roots. And, as Valckenaer further remarks, *ex verbis primitivis, tanquam totidem ſtirpibus et radicibus, amæno linguæ horto inſitis, derivatorum verborum, tanquam totidem ramorum fœcunda propago emicuit et propullulavit.* And not only was the mode of the *derivation*, but that alſo of the *compoſition*, of words fixed by theſe ſame wiſe founders, who beheld in ſpirit the future fortunes of their verbal progeny.—Verba *autem cum* verbis, & nomina *cum* nominibus *modo tam vario et tam artificioſo componi queunt, & ſibi invicem innecti et illigari, ut componendi et glutinandi rationes, quas* PRIMI CONDITORES *linguæ admirabilis fixiſſe videntur, nullis regulis includi poſſe videantur*, (p. 22.)

How little probable, or rather how impoſſible, any ſuch formation of a language is, the hiſtory of mankind evidently ſhews. Upon what grounds can ſuch a degree of foreſight and taſte be imagined to have exiſted

iſted among the Greeks before the hiſtorical times? and the inconceivable enterprize of laying out a language according to a regular plan, could not have eſcaped both hiſtory and tradition. The motives and the means of execution defy conception. If Ariſtotle and Plato could not ſee this artificial ſyſtem, when the language lay actually before them, ſhall we believe that their remote and forgotten predeceſſors could call it up before their imagination? In truth, ſuch a reach of thought is not the affair of untutored and uninſpired mortals, who, as they had a language of art to form, muſt have ſpoken a rude language. Now in the condition of ſociety, implied in this ſuppoſition, human life is divided between ſenſation, ſleep, liſtleſſneſs, and phrenzy; for the tumultuous ſallies of the exalted imagination of a ſavage, fall nothing ſhort of phrenzy. Such an effort of generalization one may therefore as reaſonably expect from the remote anceſtors of Homer, as an *Eſſay on Human Underſtanding* from an Hottentot.

If the other member of the dilemma be preferred, and theſe CONDITORES be placed in an age ſo cultivated as to have a written ſpeech, the garden of language will, it is evident, be already occupied; nor

will there be ſpace for theſe new roots, and the wide-ſpreading foliage of the trees which they produce *.

If we ſurvey a collection of Greek words analogically arranged †, and admit their affinity, we may form two ſuppoſitions; 1. That the long words are formed from the ſhort by extenſion; or, 2. The ſhort from the long by contraction. Now as we have no poſitive hiſtorical evidence to determine which of theſe

* *Conſideramus linguam Græcam, tanquam hortum ſimplici ac naturali pulchritudine amœniſſimum, gratâ florum diverſiſſimorum copiâ cultiſſimum, et mirificum in modum variegatum; ſed cujus horti præ ceteris hortis natura duplicem ab rationem eſt admirabilis*; primum, *quod ſi ſpectemus horti amplitudinem & capacitatem, arbores ferat numero pauciſſimas, arbores vero per eoſdem ordines directas; arbores, quorum proceritas & late ſpatioſa et in longum diffuſa ramorum amplitudo ſpatium horti totum opacet.* Deinde *id arboribus hujus horti peculiare eſt, quod truncus omnium arborum ſit ſimillimus, quod rami in omnibus arboribus numero pares ad eandem lineam videantur educti, quodque in ſingulis ramis omnium truncorum totidem reperiantur folia, nullis lineamentis diſparia, adeo ut qui unius rami folia, unius trunci ramos, oculo curioſo et philoſophico conſideraverit, ac probe perſpexerit, is eâdem operâ mox noverit, ſingularum arborum rami quot foliis gravati luxurientur, quot rami e ſingulis truncis propullulaverint.* (Valkenaer, p. 27.)

† Valckenaer, (p. 58, 59.) has an alphabetical arrangement of an hundred words derived from ἀχω.

is

is the true ſuppoſition, ſo neither in itſelf does the ſyſtem of analogical abbreviation appear more improbable than that of analogical elongation.

And the former ſyſtem is countenanced by all experience in languages. After they are written, and doubtleſs alſo before, they ſhew a ſtrong tendency to drop letters. If there be exceptions to this rule, they are afforded by a few words of infrequent uſe, or by words adopted from languages already in eſteem, a cauſe that never diſturbed the natural tendency of the Greek language, or by ſome very peculiar circumſtance: as our adoption of *unleſs*, inſtead of *leſs*, uſed by many of our old writers, was perhaps determined by our taking *leſs* for a term of compariſon. Here we neglect cauſes which operate to ſuch a ſmall extent, as well as changes made merely *euphoniæ gratiâ*: Had the Greek language afforded this ſeries,

If, gif, yef, give, yeve, gin, given,

Mr. Valckenaer would doubtleſs have fixed upon *if* as the primitive. Yet wherever there is an opportunity of obſerving ſucceſſive alterations in words, we ſhall find abridgment and not extenſion; whether in the different periods of an improving language, or, (where it is exceedingly remarkable,) in the difference between the long concatenation of articulations in barbarous tongues,

and

and the monoſyllables that form the baſe of written languages. We cannot but ſuppoſe, that this latter change has taken place by degrees; it is not very eaſy to believe, that words have the property of ſhutting up all at once, like pocket teleſcopes. Lord Monboddo, I know, pretends, that *languages of art* muſt be made by *men of art*, that is, by thoſe "who have made a "particular ſtudy of words and things, with a plan "or ſyſtem, proceeding in a certain method, and ac-"cording to certain rules for the attainment of that "end;" but, as uſual, he forgets to prove all this *.

He

* Here is a ſpecimen of his Lordſhip's manner of arguing, &c. "It may be thought—that though there be, no doubt, a great deal "of art in language, yet it may have ariſen by degrees from "experience, obſervation, and vulgar uſe; and that in this way "even a language of art may have been formed without any re-"gular plan or ſyſtem. If this be true, I think it muſt be true "likewiſe, that all the other arts, liberal as well as mechanical, "muſt have been invented in the ſame way; *and that painting, "muſic, architecture, muſt have been the work of the* MERE VUL-"GAR!" II. 483. Some philoſophers have been as liberal in imputing *art*, or ingenious contrivance, to rude nations, as Lord M. (who quibbles not a little about the term *art*) is niggardly. Thus the *trial by jury* has been imputed to profound political wiſdom, as if a review had been taken of all *actual* and all *poſſible*

He informs us further, that "the inventors of the "art of language were men of authority among a ſa- "vage people." (II. 497.) Now let us hear what taſk theſe ſavage men of authority had to perform: firſt, "A ſyſtem of etymology was to be formed, by "which the whole language was to be derived from "certain primitive ſounds, or radical words. Then "caſes, genders and numbers were to be invented, "which anſwered a double purpoſe, both of expreſſing "different relations and other circumſtances of things,

modes of adminiſtering juſtice; and this had been preferred upon compariſon. But it is, in fact, a very obvious ſuggeſtion of our ſenſe of juſtice, and follows immediately from that principle of human nature, which is ſo admirably unfolded in the *theory of moral ſentiments, by Dr. A. Smith.* Jurymen are but arbiters under another name; and *arbitration* is the moſt natural, as well as the moſt excellent way of compoſing differences. The only difficulty perhaps was to transfer this method from civil to criminal cauſes. But after all, in eſtabliſhing and regulating a mode of diſpenſing juſtice, which was dictated by feelings lodged in every boſom, there is nothing which we need imagine placed beyond the reach of minds little inured to reflection. Several nations, we know, inherited this invaluable bleſſing from their rude anceſtors; and probably all enjoyed it in ſome form or other, till it was wreſted from them by their oppreſſors.

"and

" and of connecting words together in Syntax. Then " tenſes and moods of verbs were to be contrived, " by which the circumſtance of time, and the affec- " tions and diſpoſitions of the human mind with re- " ſpect to the action of the verb, were to be ex- " preſſed." (II. p. 489.) The language was beſides to be made muſical; and before every thing, it was neceſſary to have accompliſhed " that great work of " ſcience, viz. analyſis."

The univerſal *tendency to contraction*, is not leſs apparent in the Greek than in other languages: and the writers whom I have quoted in a thouſand inſtances, both prove and allow it, though in the formation of the language they ſeem only to think of the contrary proceſs; a proceſs without cauſe or motive, unleſs in very rare caſes, and therefore contrary to nature. Lord M. gives us this gradation. From τυπτεμεναι, (the original infinitive, according to him,) come

τυπτεναι
τυπτεμεν
τυπτεεν
τυπτειν (II. p. 507.)

So alſo, εμμεναι, εμεναι, εμμεν, εμεν

Lennep. δρασω, δραω, δρῶ
εξελασω, εξελαω, εξελῶ.
for, εις, ει — ης, η

Valckenaer

Valckenaer allows contraction in such cases as these, πελαω — πλαω, — τεραω, — τραω — and hence we might easily have πλῶ and τρῶ.

Mr. Scheid supplies a great number of contractions both in Greek and Latin—as φακος, *fax, a torch* *. πακος, πᾰξ: of *edo*, I eat, he observes, declinatio verbi per *es, est esse, estur*, hoc est, *edis, edit, edere, editur*, p. 342, where he explains why the *d* takes place of the *s*.—In a few instances, the perverted order of derivation, which I lay to the charge of these etymologists, is palpable. Thus, *a* κύω osculor, *formatum est futurum* κύσω, et σ geminata, κὺσσω. Now in this word, imitative of sound, every one's ears will satisfy him, that κυσσω must have been before κυω, as being so much more expressive †.——Scaliger had derived "*mitto*" *I send*, from μίτος, a *thread*, "quoniam qui mittit, elongat."—No, says Mr. S., it comes from

* His ingenious observation on the very general word *facio*, deserves to be quoted: *facio* proprie *in lucem profero, emitto, ostendo. . . . . Unde jam statim intelligi poterit, quâ de causâ verbum* facere *adhibeatur tantum de* externis *&* luci *quasi* expositis *actionibus; alterum vero,* agere *de* internis *proprie dicatur*, p. 328.

† Πτισσω, pinso, *fortè pro* πισσω, a πιω, (see the Etymologicum,) is surely an example of the same perverted etymology, and many others might be enumerated.

μίω,

μίω, which is akin to μόω, and therefore ſignifies any motion, *quo* quid *elaxetur* et *projiciatur*. The ſame ſort of miſtake perpetually occurs in the *Etymologicum*, where all the primitives have a *general* meaning aſſigned to them, and the derivatives a particular. Now it is obvious, that all ſignifications muſt ariſe from particular perceptions, and the meaning of all words be analogically extended; we muſt firſt have a given motion, and then the term for *motus*, quo *quid*. Hence when it is ſaid, that μευς, μυς, a *mouſe*, comes from μεω *penetro*, this order muſt be the reverſe of right, unleſs a word for *penetrate* had been derived from ſome other individual phænomenon.

But what overturns this whole ſyſtem of analogical elongation at once, is a diſcovery of Mr. Scheid, to which Lennep contributed an hint, and the eaſtern languages a direct analogy: this diſcovery affords by far the moſt important elucidation of the ſtructure of the Greek language, of any thing contained in the three volumes, of which he has been the publiſher. Indeed, the obſervations of Mr. Scheid are, in general, much ſuperior to thoſe of his predeceſſors, eſpecially to thoſe of Valckenaer; and the ſuperiority probably depends on his great knowledge of the oriental languages.

Theſe

Theſe languages, as he remarks, univerſally form the perſons of the præterite, by adding certain ſyllables of the ancient pronouns to their infinitive forms. Thus *Sjchabh* in Hebrew ſignifies *decumbere*, and *atta*, *tu*, and *attem*, *vos*; hence by coaleſcence and contraction, *sjchabh*-TA, *decubuiſti*, and *sjchabh*-TEM, *decubuiſtis*. In like manner he ſhews, that from the coaleſcence of εγω, εμις, εμι, αμις, (which as well as εμος and εμυς all ſtood for the firſt perſon ſingular,) with certain other words or ſounds, were formed the Greek verbs in -ω and -μι; as

Ἄω or ἄμι, for ἄεγω or ἄεμι *I breathe*, from the coaleſcence of the firſt vowel, (originally perhaps ſeveral times repeated, in imitation of the ſound of breathing or panting ἄ ἄ ἄ,) and εγω.—So alſo, from πῦ, a ſound imitative of ſpitting and the ſame pronoun Πτύω or πῦμι for πῦεγω, or πῦεμι. He goes through the other perſons of the verb, and ſhews, that they are compoſed of *adforming* pronouns and other ſounds. The Latin verbs in -m, (*inquam*, &c.) he obſerves, correſpond to the Greek in -μι. The infinitive is formed in the ſame way by the addition of -ειν to ποιε, &c. This is common ſenſe; here we have ſatisfactory reaſons aſſigned for each change; whereas Valckenaer and Lord Monboddo have both been led,

by

by their ſyſtem of derivation, and each independently of the other, to aſſign a fortuitous and cauſeleſs origin to the verbs in μι, and alſo to miſtake their relative antiquity. "There are at preſent in Greek," ſays Lord Monboddo, "two kinds of verbs, one ter-
"minating in -ω, the other in -μι: but it is evi-
"dent, that theſe laſt verbs are derived from the
"circumflex verbs in -ῶ; and *that they were a variety*
"*in the form of the verbs, introduced in later times,*
"*and no part of the original conſtitution of the language.*
"Accordingly in the moſt antient dialect of the Greek,
"I mean in the Latin, they are not to be found." (II. 515.) Nearly ſo Valckenaer— *a verbis in* οω *manarunt verba in* ωμι, *a verbis in* υω, *verba in* ψμι (p. 16.). *In Homero*, ſays Lennep on the contrary, *longe plura hujus generis verba* (in μι) *quam in reliquis ſcriptoribus Græcis reperiuntur* (p. 113): he adds, that the Æolic dialect, having undergone leſs change than the Attic (that is, in fact, being older), has far more verbs in -μι.

The whole theory of the formation of perſons and tenſes, by changing letters and ſyllables, muſt be as falſe as it is unſatisfactory, and it aſſigns the reaſon of nothing. All the Greek perſons and tenſes and oblique caſes muſt be explained by the anatomy of the

the words, as Mr. Scheid has explained them in ſome inſtances. I might juſt as well ſay, that A *ſcalp* is derived from TO *ſcalp*, by changing TO into A, as that ξευγνυμι is formed from ξευγνυω, by changing ω into μι, and ſo on in all tenſes, moods, and caſes. The argument from the analogy of the Greek words actually diſſected, corroborated as it is by the analogy of other languages, which like the Engliſh (as *I think, we think, you think, they think*) *prefix* inſtead of ſuffixing, in order to form the perſons of their verbs, is perfectly deciſive: nor is there any thing better than mere aſſertion to oppoſe to it. And Lennep eſtimates the aſſertions of grammarians at their full value, when he obſerves: *Id nimirum ſemper tenendum, in hoc ſtudiorum genere, parum aut nihil fere vidiſſe grammaticos, tam veteres quam recentiores; adeoque, nullo modo eorum placitis eſſe ſtandum, niſi tum quando ex ipſâ linguæ naturâ petita eſſe ea appareat* (p. 55.). Indeed, except as to the ſound of a language, it is indifferent whether theſe modifying words are prefixed or ſuffixed. In Swediſh, the articles are annexed to the end of the words; and this, I believe, is one reaſon, why that language is ſo much the moſt melliflu-ous (and of courſe the only one abounding in operas) of the whole northern ſiſter-hood. We may be aſ-

ſured, that ω was never metamorphoſed, as Valckenaer ſuppoſes (p. 44.), by any grammatical magic into εις, ει to make perſons, nor into ος or ρος to make nouns, as αχω, αχος, αχρος: the difference muſt depend upon the appoſition of different words, and the extermination perhaps of the laſt ſyllable of the firſt, and the firſt ſyllable of the laſt, of the coaleſcing words.

Hence it appears that Lord Monboddo, as well as the Hemſterhuſian ſchool, were groſsly deceived in ſuppoſing the *verb* or verbs, to be the parent word of the whole Greek language; the verbs or words which we conjugate, were of late formation; and aroſe as the language was ground down by uſe. Nouns, or the names of perceptions, are, in truth, the only true part of ſpeech: from them all was derived. Several years ago, a gentleman in Oxford, whoſe minute ſkill in languages and general juſtneſs of views, are not ſufficiently known beyond the circle of his friends, ſhewed me by inſtances, like that adduced by Mr. Scheid, *that the Hebrew language has no verbs*. In the ſame ſenſe, the Saxon languages are without verbs; and the Greek verbs are compoſed of nouns. Accordingly, Mr. Tooke very juſtly obſerves, that "a conſideration of *ideas*, or of the *mind*, or of "*things*, relative to the parts of ſpeech, will lead us

"no

" no farther than to *nouns*; i. e. the ſigns of thoſe " impreſſions or names of ideas. The other part of " ſpeech, the *verb*, muſt be accounted for from the " neceſſary uſe of it in communication. It is in " fact, the communication itſelf; and therefore well " denominated Ρημα, *dictum*. For the verb is *quod* " *loquimur*, the noun, *de quo*." (p. 71.): to make a verb, or to communicate, we put together the name of a thing and of an action or paſſion. The mode of expreſſion of children and Africans frequently exemplifies this remark.

To conclude a conteſt with chimæras, which affords no immediate ſatisfaction, and in its moſt favourable iſſue, can confer little honour; I ſhall quote from Mr. Scheid a paſſage, in which he finally rejects the whole *analogy* of his maſters; and if we had none but præternatural means offered to our choice, I ſhould, with him, prefer revelation to the ſuppoſition of rude men falling upon this milliner's method of faſhioning a language, by ſticking letters in the middle or at the end of little primitives, in order to puff them out. *Equidem fateor, ſubtilem nimis mihi ſæpe viſam fuiſſe iſtam quam diximus, derivandi rationem*; *quaſi vero antiquiſſimi Græci normam hanc* de communi conſilio, in ſermonis ſui amplificatione, ſibi legiſſent;

ut ab ἄκω inſertâ aliquâ vocali de quinque *vocalibus, efformarent* ἀκαω, ακεω, ακιω, ακοω, ακυω, *ab* ἔλω, ελαω, ελεω, ελιω, ελοω, ελυω. *Rudibus mortalibus, (ſi vel maxime linguas humanum opus eſſe exiſtimes,* quod nunquam ego crediderim) *antequam lingua jam conſtituta eſſet, eam tribuere* ἀκριβειαν, *quæ vix a philoſophis, novam forte linguam condituris, expectari poſſit, id vero nimium mihi videtur eſſe, in hoc antiquitatis ſtudio; neque id voluiſſe Lennepium, ſummoſque ipſius præceptores, Hemſterhuſiam et Valckenarium,* facile inducor ut credam (p. 491.) Mr. S. ſuppoſes ακει the old dative of ακη, which is formed from ακω, to have given riſe to ακεω, and ſo forth.

The phyſical conſideration of the vowels will go a great way towards explaining the facts upon which this ſyſtem of analogy is founded. In the different languages of Europe, A, E, I, O, U, repreſent ſome ſcores, and perhaps ſome hundreds of ſounds, varying by imperceptible gradations; and it is evident from anatomy, that ſuch ſounds may be varied without end. From the affinity of the vowels, and of ſeveral of the conſonants *, it happens that the ſame ſound is in

* Lennep's idea, if he be the author of it, of arranging the letters of the alphabet, according to their affinity, is ingenious. He places, for inſtance, G next to K, B to P, D to T, and ſo on.

writing

writing repreſented very differently. Before the invention of the art of printing, it was perhaps impoſſible to introduce an uniform orthography; though the more a language was written, the more it would approach towards uniformity in this reſpect. In moſt of thoſe caſes therefore, where the vowels are ſuppoſed to have been ſeverally inſerted, we have, in fact, only one word differently ſpelled: and this difference of ſpelling will be more firmly eſtabliſhed, the longer the ſtates, that ſpeak a common language, continue independant of one another. Had all Germany groaned for ages under the tyranny of one deſpot, we ſhould not have had the difference of orthography, which ſtill prevails between the Saxon, Swiſs, Pruſſian and Auſtrian writers. Moreover, in every thing we do, we proceed by analogy, and imitation is the mould in which we are all caſt. Now it is a circumſtance of great ſingularity and importance, that in the improvement of the Greek language, and in the gradual contraction of Greek words, this invariable tendency of human nature was not diſturbed by the influence of any more poliſhed language. Theſe ſeveral cauſes will, I think, fully account for thoſe groups of reſembling ſounds which are obſerved in that language. And ſimilarity of conſtruction, being

founded upon a principle, common to all mankind, muſt prevail to a certain extent in other languages as well as in the Greek; and as Lennep juſtly obſerves; analogia, *ex ipſa hominis natura manans, in omnibus omnino linguis, pro diverſâ indole earum, diverſa quidem, in ſingulis tamen, æquali modo, per omnes earundem partes, regnat conſtantiſſimè.* It may appear extraordinary, that a perſon, who makes ſo many juſt ſingle obſervations, ſhould adopt a ſyſtem ſo improbable and ſo deſtitute of proof. But the phænomenon is not unprecedented. The facts, adduced by other theoriſts, have ſometimes been ſubverſive of their ſyſtem.

NOTE

NOTE II.

On the Spirit and Tendency of the Doctrines of the Επεα Πτεροεντα, *and on the Merit of the Author as a Diſcoverer.*

CONſidering the long deluſions which words have ſupported, the deadly animoſities, public and private, to which they have given riſe, and how much genius they have rendered uſeleſs or pernicious to mankind, *grammar* may be numbered among the moſt important of all purſuits. That work, therefore, in which Mr. Tooke has revealed ſo much of the ſtructure of language, appeared to me, on its firſt publication, one of the moſt valuable, as well as one of the moſt ingenious productions, that ever iſſued from the preſs; except Mr. Locke's Eſſay, I conſider it as that which has moſt contributed towards the theory of our intellectual faculties. Whatever may be thought of their *value*, no one, I believe, capable of under-

ſtanding the proofs, will heſitate to admit the *truth* of the doctrines it contains.

But moſt fortreſſes have ſome weak or unguarded ſpot. When a theory is eſtabliſhed by incontrovertible evidence, the uſual mode of hoſtility is to diſpute the author's claim to diſcovery. Should he be in any way obnoxious, this kind of attack is not likely, on that account, to be carried on with leſs than ordinary vigour.

It has accordingly been ſaid, that although his philoſophy of language be true, yet little or no credit is due to Mr. Tooke, ſince it had previouſly been taught by others. If ſo, Mr. T. was either acquainted with their works, or he was not. According to the latter ſuppoſition, we may conſole him by ſome ſlight commendations of his ingenuity, but, in a ſhort time, we ſhall think no more about him. Our tribute of admiration and of gratitude, will all be paid, as juſtly due, to his predeceſſors. But if he was acquainted with their diſcoveries, then notwithſtanding any trifling corrections or additions he may have made, his attempt to deceive us, and defraud our inſtructors and benefactors of their reward, will ſuppreſs every ſentiment but indignation.

The

The indictment againſt Mr. T., may be laid in the name of Skinner and his brethren, of Lord Monboddo, or the Dutch ſchool of etymologiſts.

The ſhare in the Επεα Πτεροεντα due to Sanctius, Voſſius, Skinner, or Junius, who are always, I believe, fairly quoted, will vaniſh altogether upon a juſt eſtimation: nor will any equitable critic, acquainted with the progreſs of diſcovery, dwell a moment upon this charge. It has always happened, that certain facts belonging to an extenſive theory have been noticed, and certain hints, which when they have *afterwards* been adverted to, might be ſuppoſed to ſuggeſt it, have been thrown out, long before the theory itſelf was fully formed. This can hardly fail to be the caſe with all juſt theories. A man need not poſſeſs a very obſervant eye, nor a very generalizing mind, to notice a few out of a multitude of facts, which he has every day before him, and to ſuſpect ſome connection between them. The predeceſſors of Harvey were ſo perfectly acquainted with the proofs from which Harvey deduced the circulation of the blood, that Haller wonders how they could have failed, eſpecially one of them, to draw the concluſion. But Haller does not the leſs, on this account, acknowledge the genius of

Harvey.

Harvey. If it be true that Skinner's IF ſpoke ſo plainly and ſo loud in behalf of himſelf and his kindred, why was the cry loſt upon Johnſon, Lowth, Harris, Monboddo, as well as our other grammarians and lexicographers of high and low degree. It was becauſe Skinner's derivation, if it ſhines at all, ſhines with a reflected light—it was becauſe it is not every man, who has eyes, that can perceive.

Lord Monboddo will not infringe even ſo much as Skinner upon the grammatical reputation of Mr. Tooke. Skinner's merit does amount to ſomething, though the ſum be ſcarce large enough to be appreciated. Lord Monboddo's is a ſolitary cypher. The following paſſage occurs in his ſecond volume, publiſhed in 1774, and is that upon which his claim muſt be founded. "I prefer that diviſion of the "parts of ſpeech, that has been given both by Plato "and Ariſtotle, into *noun* and *verb*; and I will en- "deavour to ſhew, that all the other parts of ſpeech "above-mentioned, (viz. the eight commonly enu- "merated) may be fitly referred to one or other of "theſe two." In a note his lordſhip informs us, that "Ariſtotle has given this diviſion in his book "of interpretation, and Plato in the Sophiſt. It is "true,

" true, that Ariſtotle . . . . . in his popular work " upon poetry, ch. 20. has given us another diviſion, " more ſuited to the capacity of thoſe for whom he " wrote that book, viz. into *noun*, *verb*, *article* and " *conjunction*." (II. 28.) To write falſhood and nonſenſe for the edification of the people, has indeed been a practice common enough ſince the days of Ariſtotle, and probably before him. I do not, however, in the preſent caſe, give Ariſtotle the credit of any ſuch laudable intention. I rather ſuppoſe that he was not acquainted with any one of thoſe phænomena which betray the nature of language; if ſo, his fluctuation between different ſurmiſes will appear natural enough. His expreſſions would undoubtedly have been preciſe and uniform, had he entertained but a ſuſpicion of the nature of *particles*. Lord Monboddo certainly repeats the opinion without comprehending the ſubject any better than Ariſtotle, and no where in his very prolix *Analyſis of the formal part of language*, which, after all, is no analyſis, nor any thing in any degree approaching towards an analyſis, does he offer either proof or preſumption in favour of his hypotheſis. He even adjures the reader by his candour, not to impute to him any ſuch deſign. " The

" candid

" candid reader, ſays he, will not underſtand that I " mean to ſay, that conjunctions, prepoſitions, and " ſuch like words, which are rather the pegs and " nails that faſten the ſeveral parts of the language, " than the language itſelf, are derived from verbs, or " are derivatives of any kind, but he will underſtand " that I mean the names of things, which are pro- " perly the words of a language." (II. p. 188.) This is then what his lordſhip's doctrine amounts to, compared with that of former grammarians. They, many of them, reckoned eight parts of ſpeech; the *noun* and *verb* among others. Lord M. reckons two only, all the while merely repeating Plato and Ariſtotle; —and theſe are the *noun* and *verb*. As to the particles, wherein lay all the difficulty, he ſays, they are *pegs* and *nails, not the language itſelf, not properly the words of a language.* And when he conjectures, that the verb is the parent word of the whole Greek language, he muſt mean of the whole eight parts of ſpeech, ſix excepted. Whatever may be Mr. T.'s dexterity, as a literary thief, he never ſtole an atom of his doctrine from Lord Monboddo. Nor do I imagine his lordſhip would venture an oath, or even an affirmation, that any article in the Επεα Πτεροεντα is his pro-

property, except what is duly marked with his lordſhip's name *. When he comes to explain the pronoun, this is his philoſophy. "If either the ſpeaker "or the hearer be the ſubject of the diſcourſe, there "is no more ado than to invent two words to deſign "and diſtinguiſh them from one another. And "theſe words are called *pronouns:* viz. of the firſt "and ſecond perſon." In explaining the origin of the pronoun of the third perſon, he is equally luminous. Having premiſed, that if there be an object in queſtion, of which the hearer knows nothing beforehand, we can convey no knowledge of this object to him but by ſhewing it; "but ſuppoſe the object, he "continues, had been mentioned before in the diſ-"courſe, and that in this way he has come to the "knowledge of it, any word marking a reference "to the object before-mentioned, and denoting that "it is the ſame with the object now mentioned, will "be ſufficient to ſingle out and diſtinguiſh that ob-

* The only poſſible exception is in the latter part of this ſentence. "The art of language appears to conſiſt in four things: "1. In expreſſing accurately and diſtinctly all the conceptions "of the human mind. 2. *In doing this by as few words as poſ-"ſible.*" (II. p. 6.)

"ject

" ject from others. And here we have another pro-
" noun of the third perſon, which ſerves to diſtinguiſh
" ſubjects of the converſation that are not preſent."
(II. p. 49.) According to Lord M.'s doctrine, when a man was in want of a pronoun, there was no more ado than to invent one; but *how* he ſet about this invention! why truly, ſagacious reader, this trivial and acceſſory problem he leaves to thee to ſolve. It ſeems clear, from his own terms, that the pronoun is not any more than the conjunctions, &c. one among the parts of ſpeech which he would derive from the verb; and nothing, for I will not cavil about trifling inaccuracies, would have been wanting to the theory, if, as he has entitled the particles, the *pegs* and *nails*, it had pleaſed him to chriſten the pronouns, the *finger-poſts* of language.

Mr. Harris accounts for the obſervation of Plato and Ariſtotle in this manner. " Plato in his Sophiſt
" mentions only two (parts of ſpeech), the *noun* and
" the *verb*. Ariſtotle mentions no more, where he
" treats of propoſitions. Not that thoſe acute philo-
" ſophers were ignorant of the other parts, but they
" ſpoke with reference to *logic* or *dialectic*, conſider-
" ing the eſſence of ſpeech as contained in theſe two,
" becauſe *theſe alone* combined make a perfect *aſſer-*

" *tive* ſentence, which none of the reſt without them " are able to effect. Hence therefore Ariſtotle in his " *treatiſe of poetry*, (where he was to lay down the " elements of a more variegated ſpeech) adds the *ar-* " *ticle* and *conjunction*, &c." In a note Mr. Harris quotes Priſcian and Boetius (Hermes, p. 33.); the former obſerves, *hæ ſolæ* (*part. orat. nempe*, NOMEN *et* VERBUM) *etiam per ſe conjunctæ plenam faciunt orationem*; and the latter declares it to be Ariſtotle's meaning, that a ſimple enunciative ſentence may be expreſſed by *noun* and *verb* alone. I know not whether this ſuppoſition will reſcue Ariſtotle from the imputation of a diſcovery, but we may be certain, I think, that he did not ſpeak with any knowledge of the ſtructure of language. And if there be any thing in him or in Plato, (for we muſt quit Lord Monboddo, and —*integros accedere fontes*) capable of throwing light upon the ſubject, why was it left for ages unapplied? Their works have been long enough before the world, and been more than ſufficiently ſtudied. Conſidered merely as a commentator of Ariſtotle and Plato, Mr. T.'s merit will, I conceive, be juſt as great, as if we ſuppoſe, that his opinions were ſuggeſted by his own obſervation of language.

It

It is impoſſible to think of the Dutch etymologiſts without a lively ſenſe of gratitude. Yet if we conſider Mr. T.'s freedom from the hypotheſes with which they are encumbered, that important application which he makes of his doctrine to the illuſtration of Mr. Locke's Eſſay, and the light he every where throws on collateral ſubjects, they will appear to be at leaſt half a century behind our countryman in the philoſophy of language. Nevertheleſs, they have collected many uſeful materials; and ſome architect, more ſkilful than themſelves, would have arranged them into a juſt ſyſtem at ſome future period.

Before I proceed to ſhew how little probable it is that he ſhould have had any knowledge of their reſearches, let me remind the reader, that within the laſt twenty or thirty years, perſons, in different countries, have oftener than once fallen upon the ſame, and that a very important and unexpected, diſcovery. Nor in theſe caſes has there been any ſuſpicion of foul play on either ſide.

The diſcovery of oxygene air, which was made about the ſame time, both by Dr. Prieſtley and Mr. Scheele, affords one remarkable example of this kind. And theſe coincidences will occur more frequently,

as

as the human mind is rouſed to activity in a greater number of different countries.

Now Mr. Tooke's *letter to* Mr. Dunning, was publiſhed in 1778. That letter, I am told, has been long out of print, and I have never ſeen it. But the author informs us, that it contains the ſubſtance of the 6th, 7th, 8th, and 9th chapters of his later work. Theſe chapters treat of the word *that*, of *conjunctions*, of the *etymology of the Engliſh conjunctions*, and of *prepoſitions*. And they offer beſides, a great variety of the moſt acute and ingenious obſervations, together with examples of *reſolution*, ſuch as it would be in vain to ſeek in any other writer upon language.

No perſon will heſitate a moment to believe, that the author had formed his ſyſtem of language at a period, when he had explained all that was difficult in the particles. What *time* he will allow for the formation of this body of grammatical knowledge, every reader muſt judge from his own experience in diſcoveries, or the beſt obſervations he may have been able to make upon the ſpeed of others in diſcovery. One thing I hold as certain, that when a man of genius has ſatisfied himſelf of any new truth, he is, in general, very loath to begin to mould it into a ſhape fit for public inſpection, and advances very ſlowly with

this taſk. This averſion to the drudgery of drawing out a theory in form, whether it be pride, or habit, or indolence, ſeems ſometimes for a long time, and ſometimes finally, to have withheld valuable diſcoveries from the world.

Now this date of the letter to Mr. Dunning, to neglect every conſideration, cuts off the pretenſions of Mr. Villoiſon's publication of an extract from a MS. work of Lennep; for Mr. Villoiſon did not, I think, publiſh Longus's Paſtorals, till 1778 *; and before

* Mr. Villoiſon had deſired Mr. Scheid to procure him a MS. copy of part of Lennep's Lectures. "Ejus rei exitus tunc fuit, ut approbatâ a te quam maxime," ſays the latter, addreſſing himſelf to the former, Hemſterhuſianæ ſcholæ diſciplinâ, digniſſimum haberes Lennepii opuſculum, cujus partem haud ignobilem—— tuæ Longi Paſtoralium editioni inſereres, *ut non Belgarum tantummodo noſtrorum privatis uſibus Lennepiana hæc inſervirent, ſed ad cæterarum quoque gentium Gallorum præſertim, Hiſpanorum et Italorum* (ipſa tua verba ſunt) *pervenirent notitiam.* (*Dedication of Valckenaer's Obſervations*, &c.) Mr. Burgeſs afterwards reprinted the ſame extract in Dawes's *Miſcell. Critica.* In the prolegom. to the Etymologicum, p. 1. Mr. Scheid expreſſes his conſciouſneſs, how little all theſe reſearches were known out of Holland. A ſurreptitious edition of Lennep *de Analogia* was printed, I know not where nor when, never having ſeen it. But it obtained no ſort of attention in foreign countries, and was not valued even in Holland.

before this date, the Dutch Analogy had made, as they own themſelves, little or no noiſe abroad.

In the numerous quartos of Schultens, ſome excellent obſervations on *particles*, ſeem, indeed, to have lain hid. I would not ſwear, for I cannot know, that Mr. Tooke never, in his reſearches, ſtumbled upon theſe paſſages. But againſt this ſuppoſition a preſumption ariſes from the ſize and number of the volumes of Schultens, as well as from the little attention the obſervations in queſtion obtained abroad; no particular marks of imitation are pointed out; we have only a bare poſſibility. And againſt this we have ſtrong internal evidence in the peculiar features of Mr. T.'s work. The air and manner of the *Diverſions of Purley*, ſtrike me at leaſt as altogether original. The property of the moſt valuable parts of the work, which ſeem entirely to be overlooked in the charge, is not diſputed; and the whole work is ſo much of a piece, that the author of the beſt part was undoubtedly ca-

Holland. *Neque multo melior conditio fuit libri Lennepiani; quî et ipſe, ſæpiſſime, ab imperitis, negligentibuſve aut ſtupidis hominibus ita deſcriptus eſt, ut vix jam cum jucunditate aliquâ legi poſſet nauſeamque crearet eodem uſuris. Teſtis eſto editio optimi hujus libelli prima, quæ ſub falſo Londini nomine . . . . . fædiſſimis mendis commaculata fuit, atque adeo merito contemtui eſt habita.*

pable of being the author of the reſt; and not only ſo, but if, as he ſays, and as is abundantly probable, his philoſophy of ideas, independently of etymology, afforded him a glimpſe of his philoſophy of language; and if he had long conſidered the greater part of Mr. Locke's Eſſay, as a treatiſe upon words, he *muſt neceſſarily have diſcovered the nature of particles*, though not a ſingle one had ever been inveſtigated before. Paſſages innumerable in his work, to which there is nothing ſimilar in the works of theſe other writers, prove that he was equal to the whole diſcovery, and that he was in the train to make it. Nothing indeed can appear more evident, than the etymology of the greater number of particles, now we have been taught how to trace them to their ſource; nor need any one be ſurprized that he, who ſailed after a chart ſo much more correct, ſhould have made ſo much greater progreſs.

From the former part of this, and from the foregoing note, it may be collected how different Mr. Tooke's general views of language are from the views of all thoſe writers whom I have quoted beſides. In order fully to enable the reader to judge, between the ſpirit of his philoſophy and that of Lord Monboddo, I have extracted from each a paſſage, treating of the

ſame

ſame part of ſpeech. This experiment of juxtapoſition, I can aſſure him, is fairly made; and it will ſhew whether the one has gone beyond the commonplace futility of grammar, and how widely the other has extended his views beyond mere etymology, and what an admirable rationale he gives of the *manner of ſignification* of words.

LORD MONBODDO.

"Prepoſitions I likewiſe claſs under verbs, as they denote relations of things; not abſtractedly, for then they would be nouns, but inherent in their ſubjects, ſo that they are qualities which are not conſidered as having a ſeparate exiſtence. The chief uſe of them, as it appears to me, is to expreſs relations, which could not be conveniently expreſſed by the caſes of nouns, ſuch as place, ſituation, order, and many other connections of things, which are obſerved by grammarians, in the ſignifications they give to the ſeveral prepoſitions. *They are of very great uſe in ſyntax, and govern a caſe, whereby we know the word to which they refer.* . . .

To know the preciſe meaning of the prepoſitions in the Greek language, and to be able to diſtinguiſh the proper from the figurative ſignification of them,

is

is a matter of great nicety . . . . . the uſe of them in compoſition gives a peculiar beauty and accuracy of expreſſion to the Greek language. There are commonly enough two of them, and ſometimes three, in compoſition with their verbs, by which they deſcribe ſo minutely the action of the verb, that it is really a kind of painting. Thus Homer, in deſcribing water coming out of the foot of a rock, uſes the word ὑπ-εκ-προ-ρεειν, by which is deſcribed, firſt its coming from *below*, then its coming *out*, or *guſhing*, and laſtly, its running forward.

The adverb, as the name imports, is a ſort of adjunct of the verb, and appears to me to be ſuch a ſupplement of the verb, as the prepoſition is to the noun.

As ſingle words are connected together by the means of caſes and prepoſitions, it is fit alſo that ſentences, and members of ſentences, ſhould be connected together; and for that purpoſe, a ſet of words have been invented, called *conjunctions*, which though they may ſeem often only to connect words, yet it is truly ſentences that they connect. . . . . I ſhall only obſerve, that though they all go by the name of *conjunctions*, ſome of them *connect* by disjoining, not by joining." II. 175-8.—For this laſt obſervation,

and many ſuch beſides, I ſuppoſe every ſchool-boy, who ſhould not happen to have them in his grammar will be ready to acknowledge his obligations.

MR. TOOKE.

" As the neceſſity of the *article*, or of ſome equivalent invention follows from the impoſſibility of having in language a diſtinct name, or particular *term*, for each particular individual *idea*; ſo does the neceſſity of the *prepoſition*, or of ſome equivalent invention, follow from the impoſſibility of having in language, a diſtant *complex term* for each different *collection of ideas*, which we may have occaſion to put together in diſcourſe. The addition or ſubtraction of *any one* idea to or from a collection, makes it a different collection: and, if there were degrees of impoſſibility, it is ſtill more impoſſible to uſe in language a different and diſtinct *complex term* for each different and diſtinct *collection of ideas*, than it is to uſe a diſtinct *particular term*, for each particular and individual idea. To ſupply the place therefore of the complex terms which are wanting in a language, is the prepoſition employed, by whoſe aid *complex* terms are prevented from being infinite or too numerous, and are uſed only

only for thoſe collections of ideas which we have moſt frequently occaſion to mention in diſcourſe. And this end is obtained in the moſt ſimple manner in the world. For having occaſion in communication, to mention a collection of ideas, for which there is no one ſimple *complex* term in the language, we either take that complex term which includes the greateſt number, though not *all*, of the ideas we would communicate, or elſe we take that complex term, which includes *all*, and the feweſt ideas *more* than thoſe we would communicate; and then by the help of the prepoſition, we make up the deficiency in the one caſe, or retrench the ſuperfluity in the other.—For inſtance,

1. *A houſe* WITH *a party-wall.*

2. *A houſe* WITHOUT *a party-wall.*

In the firſt inſtance, the complex term is deficient: the prepoſition directs to add what is wanting. In the ſecond inſtance, the complex term is redundant: the prepoſition directs to take away what is ſuperfluous.

If to one of our modern grammarians, I ſhould ſay—A *houſe* JOIN—he would aſk me—JOIN what? but he would not contend that JOIN is an indeclinable word, and has no meaning of its own; becauſe he knows

knows that it is the imperative of the verb, the other parts of which are ſtill in uſe; and its own meaning is clear to him, though the ſentence is not compleated. If inſtead of JOIN, I ſhould ſay to him—A *houſe* WITH, he would ſtill aſk the ſame queſtion—WITH *what?*" Mr. T. then ſhews from etymology, that his inſtances ſtand thus:

"1. *A houſe* JOIN *a party-wall.*

2. *A houſe* BE-OUT *a party-wall.*"

Now this full and clear diſplay of the manner of ſignification of words, by which the plan, begun by Mr. Locke, has been nearly compleated, conſtitutes, in my apprehenſion, the great and diſtinguiſhing merit of the Επεα Πτεροεντα, above all other works on language. I find none of theſe comprehenſive views in the Dutch etymologiſts; and though they have contributed much to unfold the manner of ſignification of the hitherto ſo imperfectly underſtood particles in the Greek and Latin, the Oriental and Dutch languages, they have but ſupplied materials to the *philoſopher*; their labours may be conſidered as bearing to the theory of *language*, the ſame relation which the noting down of individual appearances in the heavens bears to the theory of aſtronomy. The only

juſt principle they were able to deduce I have quoted above (p. 6, *note*), and this principle they were ſo little able to apply, that as we have ſeen, they have reverſed the progreſs of language, both in the formation of words and the genealogy of ſignifications. As to Lord Monboddo, his doctrine of the operations of mind is directly contrary to that of Mr. Locke, as it has been corrected by the author of Επεα Πτεροεντα; who juſtly obſerves, "that his Lordſhip and his fau-" tors will do well to contend ſtoutly and obſtinately " for their doctrine of language, for they are menaced " with a greater danger than *they* will at firſt appre-" hend; for if they give up their doctrine of lan-" guage, they will not be able to make even a battle " for their metaphyſics: the very term *metaphyſic* " being nonſenſe; and all the ſyſtems of it, and con-" troverſies concerning it, that are or have been in " the world, being founded on the groſſeſt ignorance " of words, and of the nature of ſpeech." If therefore Mr. Tooke had not diſcovered a ſingle etymology, which till it is proved, every impartial perſon will beg to be excuſed from believing; if his admirable reſolution of THAT, and the proof he has given of the ſteadineſs of ſignification of words in the caſe

of

of FOR (a word, which had ſo much perplexed the lexicographers *,) were ſtruck out of his book, it would ſtill retain an immenſe value. It would have the merit, 1. of purifying learning from the laſt blemiſhes and obſcurities left upon it by the ſtudy of *univerſals*. 2. By expoſing the futile falſhoods of grammar, it will contribute to the amendment of education, a work of mercy which will redeem a multitude of ſins; and, 3. It will eventually deliver mankind from thoſe fierce and fatal contentions, for which ignorance of the foundation of knowledge, and of the ſtructure of language, ſupplied the pretext. The more information, indeed, we acquire on this and other ſubjects, and the more pains we take to form opinions by the help of our own unbiaſſed reflections, the more ſhall we poſſeſs of that equity of mind, on which depends the charity of thought and action. Solid knowledge

* Greek ſcholars will now be ſaved the trouble of ſtudying the fatiguing *minutiæ* of Vigerus, Hoogeveen, &c. This mode of reſolution, (the *corporeal* ſignification of *particles* being given, and Mr. Scheid will ſupply that), will do away the ſuppoſed variation of ſenſe in the ſame word, and the caprice of idiom; than which no ſtudy, I imagine, can be more perplexing, more burdenſome to the memory, or more barren of uſeful inſtruction.

humanizes

humanizes, without enfeebling or rendering effeminate, the character.

I may be allowed to add, that I have no other reason for respecting Mr. Tooke, than as he is the teacher of truth. The temptations to decry him are obvious enough: I know of none to praise him beyond what he deserves; and if it shall be proved that he has been guilty of the greatest crime, which, as the writer of a work on language, he could commit, I shall not only conceive proper detestation of his conduct, but shall be grateful to any person who shall inform me, to whom my respect is due; for this sentiment I shall assuredly ever retain for the discoverer of truths so important.——As to Mr. Tooke, if after reading these remarks, he should require an apology for the freedom which a stranger has taken with his name, I confess myself unable to make him one that will be satisfactory, unless it be already made.

THE END.

ERRATA.

P. 19, l. 17, for *a such*, r. *such a*. P. 23, l. 9, for *that*, r. *as to*. P. 26, l. 7, for *angels*, r. *angles*. P. 33, last line, for *extremity*, r. *extremities*. P. 47, l. 16, for *on*, r. *as*. P. 49, after *bars*, insert *are*. P. 51, l. 15, for *pencil*, r. *pencils*. P. 54, l. 2, dele *to*. P. 108, after *former*, insert *uncertainty*. P. 140, l. 13, for *quoted in a thousand instances*, r, *quoted, in . . . instances*. P. 146, l. 15, dele *true*.

A

LETTER

TO

ERASMUS DARWIN, M. D.

ON

A NEW METHOD

OF TREATING

PULMONARY CONSUMPTION,

AND

Some other Diſeaſes hitherto found Incurable.

By THOMAS BEDDOES, M. D.

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A

LETTER *to* DR. DARWIN.

DEAR SIR,

NOTHING but compleat ſucceſs in the treatment of Conſumption could afford me greater ſatisfaction than to find that my theory of this deſtructive diſeaſe appears by no means deſtitute of plauſibility to a Phyſician of your experience and ingenuity.

You aſk whether I have any new facts to add to the probabilities which I have collected in my MEDICAL OBSERVATIONS: (*a*) and you

 expreſs

(*a*) See *Observations on the Nature and Cure of Calculus, Sea Scurvy, Conſumption, &c.* 8vo. Murray, London. 1793. page 113—148.

expreſs a wiſh for information concerning the apparatus which I have juſt cauſed to be conſtructed for the purpoſe of reducing my theory to practice. I ſhall endeavour fully to ſatisfy your polite enquiries. Liable as they muſt be to miſconception and miſtatement, I am indeed very deſirous to make my real views generally known. For although it is evident that none but beneficial conſequences can reſult to the public from the proſecution of my deſign, yet it requires very little knowledge of the world to perceive the danger to which I am expoſing my reputation. It is impoſſible to engage in a new and arduous undertaking without incurring ridicule and obloquy: Of courſe I muſt expect to be decried by ſome as a ſilly projector, and by others as a rapacious empiric. Secrecy, as you well know, and promiſes unreſtrained by ſenſe or ſhame, conſtitute the *eſſential character* of empiriciſm: Now I truſt that the Book, to which I have juſt referred, will bear ample teſtimony that on medical ſub-

jects

jects I have no diſpoſition to ſecrecy. The moſt inattentive reader muſt perceive that I have poured forth my ideas freely and without reſerve; and I know not any thing I have ſo much to dread from the judicious critic, as cenſure for not taking more time to reconſider and arrange them more advantageouſly.

As to the new method, in which I propoſe to treat Conſumption, as well as ſome other mortal diſorders, I cannot but be aware that it would be equally impolitic and diſingenuous to ſpeak of it with greater confidence than it deſerves. Its real efficacy muſt in a ſhort time be aſcertained either by myſelf or by others. I ſhould not therefore fail finally to be a loſer by injudicious boaſting. And I flatter myſelf that I am above all temptations deſignedly to inſpire falſe hopes, and ſo to add diſappointment to the ſufferings of the ſick. Thus every conſideration, you ſee, induces me to keep my expreſſions rather below the tenor of my expectations: And if I ſtrictly obſerve this

law, ſurely no perſon of candour will reflect upon me for employing the beſt means that can, I think, till the progreſs of Science ſhall have furniſhed ſome new hints, be imagined for attaining one of the moſt deſirable of all purpoſes---even though I ſhould not at laſt be more ſucceſsful than others have been before me. And with this reflection, I ſhall for the preſent conclude my apology for aſpiring to be uſeful.

THE OLD MEDICAL WRITERS, and indeed modern writers on the continent, divide Conſumption into a great multitude of ſpecies or varieties. It is no wonder that we, who are certainly their ſuperiors in the logic of Medicine, ſhould reject diſtinctions founded upon ſtates of the body merely imaginary, or ſuch as are not characterized by ſigns obvious to the ſenſes. I am nevertheleſs diſpoſed to think that there exiſts a more eſſential difference between different caſes of Phthiſis Pulmonalis, than the Britiſh practitioners in general ſeem willing to admit.

admit. This difference appears to be clearly indicated by the ſymptons; and it would probably be completely aſcertained by diſſection, if it were more the cuſtom to open the bodies of perſons who die conſumptive. Two varieties or two ſpecies have lately appeared to me well marked; they have indeed been noticed by ſeveral of the beſt obſervers in our own and foreign countries; and it is not unlikely that they may require a very different method of treatment during their firſt ſtage.

To the firſt, which may perhaps be termed *the Florid Conſumption*, the reaſoning in my book of Obſervations, is excluſively applicable. It is in this ſpecies, eſpecially during its approach and towards its commencement, that we perceive at the time of the acceſſions of feveriſhneſs, the vivid redneſs of the cheeks, as well as an extraordinary permanent redneſs of the lips, of the tongue and fauces. Indeed theſe parts, ſince I have been at pains to examine them very attentively, have uniformly

ſuggeſted

ſuggeſted to me the idea of meat reddened by ſaltpetre. The eyes, too, in ſuch caſes are remarkable for their vivacity. The blood, diſcharged by Epiſtaxis or Hæmoptyſis, has a colour evidently more florid than uſual. In the latter indeed, which in healthy perſons is a rare occurrence, we have not many opportunities of remarking this difference in the colour of the blood, but in the ſpontaneous bleedings of the noſe that ſo frequently take place both in incipient and confirmed Phthiſis, we ſee the blood much brighter than when it flows from healthy perſons, in conſequence of accidental violence.

It is not my intention to repeat either what I have myſelf written concerning the hyperoxygenated ſtate of the ſyſtem in Conſumption, or to tranſcribe from elementary books, a detail of the ordinary progreſs of the ſymptoms. But you will think it not improbable, from the attention I have been lately paying to this diſorder, and the opinion I entertain concern-

ing

ing its nature, that I ſhould be led to remark ſome appearances not commonly known or attended to. Theſe I ſhall from time to time ſubmit to the conſideration of Medical Philoſophers, as ſupplementary to the knowledge accumulated by former experience. In juſtice however to myſelf, as well as to my readers, I muſt remark, that I do not at preſent bring forward any of the following obſervations as *laws* to which the ſyſtem, while affected by florid Conſumption, is invariably ſubject. I have not yet had opportunities enough to enable me to determine whether they *generally* take place, or only accidentally occurred in ſome patients whom I have ſeen ſince the formation of my theory.

1. It is an evident conſequence of my leading opinion, that a phthifical patient would take a longer time than another perſon in being drowned, or in being ſuffocated in moſt of thoſe airs that are unfit for reſpiration. I ſay *moſt*, becauſe I ſuſpect that nitrous air

might furniſh an exception. Being already provided, as I ſuppoſe them to be, with a larger proportion of that principle which reſpiration introduces into the body, they muſt be able to continue for a longer time without the neceſſity of a new ſupply. And as the left cavities of the heart ſeem to be more irritable in ſuch patients, it is probable that they would be more eaſily recoverable from accidents of this kind, than perſons in an ordinary ſtate of health. This conſequence of my theory is much countenanced by the ſubjoined caſe of the ſon of our common acquaintance, Mr. Crump, ſurgeon and apothecary at Albrighton, in Shropſhire. You will find that he not only breathed with eaſe air of a very low ſtandard, but that he even much preferred it to atmoſpheric air. But I do not pretend to ſay that this fact deciſively proves the greater independance of conſumptive perſons on the function of reſpiration. The air he inſpired was partly hydrogene air. And it ſeems pretty well aſcer-

tained

tained that aſphyxia is leſs ſuddenly produced by hydrogene, than by any other ſpecies of air, unfit for reſpiration.

I lay rather more ſtreſs upon the following, which is the converſe of this obſervation, I imagine that in conſtitutions injured by exceſs of ſpirituous liquors, and more particularly while they are under the primary operation of ſuch liquors, there exiſts a deficiency of oxygene; at leaſt of oxygene in a ſtate of combination ſo looſe as it is found in the circulating blood and moving fibres. A variety of conſiderations with which I will not detain you at this moment, ſeem to me to confirm this opinion. You recollect, among others the caſe of the inflammable woman of Coventry, as deſcribed by Mr. Wilmer. This woman ſeems to have reduced herſelf by dram-drinking to ſuch a ſtate as to be capable of being ſet on fire by a ſpark, and of burning like very combuſtible matter. And one is juſtified by all the known facts relative to combuſtion, in ſup-

poſing, that where the ſubſtance of the body was ſo eager to combine with external oxygene, there muſt have been an internal deficiency of this element. I premiſe this conjecture for the ſake of relating that I once ſaw a ſot, while under the influence of liquor, though not nearly intoxicated, reduced in a very ſhort time to a ſtate of inſenſibility by breathing air of an higher ſtandard than that which Mr. G. Crump uſed to breath with pleaſure for an indefinite and certainly a much longer time. I have twice in my life ſeen a perſon habituated to drinking much affected, and indeed on the verge of aſphyxia, by breathing for a few ſeconds a mixture in which the atmoſpheric could not have been to the mephitic air in a proportion leſs than that of five or ſix to one. I need not apologize to you for the want of preciſion in theſe obſervations. No perſon can or ought for a moment to think of aſcertaining ſuch points preciſely by experiments upon Man. I hope, however, ſoon to be able

to determine by experiments upon animals, whether the faculty of living in air of a reduced ſtandard is really impaired by the influence of ſpirituous ſtimulants; and when you conſider how nearly all animals of warm blood reſemble each other in this function, you will not, I dare ſay, heſitate to transfer the reſult to the human ſpecies.

Before I paſs on to another obſervation, I will ſtop to aſk you if you do not think it probable that Divers would be able to continue much longer under water, if before immerſion they were for ſome time to breathe air of a much higher than the ordinary ſtandard, or pure oxygene air itſelf.

2. The French writers inform us that Conſumption is leſs common in the ſouthern than in the northern diſtricts of France. From all that I have been able to collect by perſonal enquiry and by reading, I infer that in the the North of France compared with England

this diſeaſe is more rare than would ſeem to follow either from the difference of climate, or any cauſe I have hitherto heard aſſigned. In conformity with my principles, and indeed with what I have expreſsly ſtated (*Observations page* 141), I have lately been led to conjecture that this difference may partly depend on the difference between the ordinary diet of the French and Engliſh people. As far as the articles of food are the ſame in both countries, the French mode of cookery tends to diveſt them of oxygene, or to combine the oxygene more intimately with the other conſtituent parts. The fermentation of their bread is carried much further; hence muſt not a larger quantity of oxygene be carried off from the dough in the carbonic acid air?---Every body knows how much more the French dreſs their meat and vegetables than we do, and how much more oil they add both to the one and the other; all circumſtances which render the food leſs apt to carry oxygene into the ſyſtem.

If

If I ſhould add that the greater ſummer heats even of the North of France may poſſibly contribute ſomething to the greater freedom of the inhabitants from Conſumption, you will perhaps think that I carry my principles to an extravagant length---and yet may not the effect of the temperature be to occaſion the oxygene to enter, not only more rapidly but in larger proportion, into the ſaline combinations, formed in animal bodies, particularly the perſpirable matter? And does not this hypotheſis afford a better ſolution than has hitherto been given of the different effects of animal and vegetable diet in very hot countries? It ſeems to me well eſtabliſhed that the latter in ſuch countries is much more wholeſome than the former; and as the natives---thoſe of Egypt for example---do not think themſelves in health unleſs they are perſpiring profuſely, may not more oxygene be wanting for the various purpoſes of the animal œconomy than reſpiration can ſupply? eſpecially as the atmoſphere both

from

from its higher temperature and the greater quantity of water it holds diſſolved, probably contains in an equal bulk rather leſs oxygene than in more temperate regions.

3. In the florid Conſumption, I think I have lately remarked, before the period of extreme debility and emaciation, a much greater acuteneſs of the ſenſes than is uſual in health. Soon after I became acquainted with the effect of placing upon the tongue a bit of lead in contact with a bit of ſilver, or of placing one under and one above the tongue and then connecting them by a conductor of electricity, I deſired each perſon in a pretty large company to repeat the experiment upon himſelf. To ſome the ſubacid taſte was not ſenſible, others could perceive it diſtinctly; one perſon preſent was in a confirmed Conſumption; and ſcarce had he applied the two metals to his tongue, than he put them out of his mouth with ſigns of diſguſt, declaring the acid taſte to be ſo diſagreeably ſtrong as nearly to

ſet

ſet his teeth an edge.---This opinion as far as it relates to the ſenſes of ſmell and hearing, has been confirmed to me by perſons who have accurately obſerved others ſuffering under the ſame complaint. But at the ſame time it ſhould be recollected that the hearing becomes quicker in ſeveral diſorders; and we know that in different ſtates of the body equal impreſſions on the ſenſe of ſmell will produce very unequal effects upon the ſenſorium. Perhaps this is alſo true of impreſſions of taſte. I muſt therefore leave it to you and other attentive obſervers of animal nature, to determine whether the opinion is in any degree juſt (*b*), and by what limitations it is to be reſtricted.

4. In the indiſpoſition preparatory to Conſumption, I am inclined to ſuppoſe that a remarkable ſymptom occurs rather more fre-

 quently

(*b*) Cullen has an obſervation of an import ſomewhat ſimilar. Hæmoptyſis "happens to perſons of a delicate ſlender make; to perſons of *much ſenſibility and irritability, and therefore of quick parts.*" FIRST LINES II. 340.

quently than is commonly underſtood. This ſymptom is a convulſive cough, often of ſuch alarming violence, as to be likely to perplex and diſconcert inexperienced practitioners. It ſeems to alternate and intermix with the well-known ſhort, dry cough. You will recollect that I had once occaſion to deſcribe to you this convulſive cough from nature. I find it occaſionally mentioned, but not much inſiſted upon, by authors. Thus Dr. Murray, lately Profeſſor at Gottingen, and author of the *Apparatus Medicaminum*, in deſcribing the firſt ſymptoms he obſerved in his brother who died of Conſumption, informs us: *Tussis* VEHEMENTER *ingravescebat, quæ non catarrhalis unice videbatur, sed simul* ſpaſtica.---*Tussis antea sicca, humida fiebat.* (*Murray de phthsi pituitosa apud Baldingerum in sylloge Opusculorum*: *V.* 316.) Raulin (De la Phthiſie pulm: Paris 1782. p. 44) mentions a perſon *qui fut pris au mois d'Aóut* 1771 *d'une toux seche, qui devint plus forte & plus frequente: au mois d'Avril,*

1772,

1772, *elle etoit* convulsive *sans crachats; elle augmenta au mois de Mai: il survint quelques crachats un peu sanguinolens; & des frissonnemens au dos, &c. &c.* I suppose this kind of cough must occur in but a small proportion of cases; otherwise it would be more distinctly described, and be the subject of more speculation in books than we find to be. From my own observations I suspect this cough to be produced by the same irritating cause (as tubercles, for instance) which produces the short, dry cough or whatever other cough may happen in any given case to be the forerunner of Consumption. Upon the most attentive examination I could discover no other cause of irritation. And can you not allow yourself to conceive that some, and that perhaps a small alteration in the power of the cause or in the habit of the patient, may produce such changes in the action of the muscles, or of the stimulus that excites them to act?

5. It is an opinion entertained by many perſons converſant in the hiſtory of medicine, that certain diſeaſes are now much more prevalent, and certain others much leſs ſo, than formerly: Some indeed, that made great havoc in this iſland centuries ago, have now totally or nearly diſappeared. Theſe variations have been imputed, and with great appearance of probability, to the entire change that has taken place during the lapſe of ages in almoſt every circumſtance of the manner of living. But in many inſtances we are not able to fix upon the particular alteration to which the introduction or the extirpation of any given diſeaſe is to be aſcribed; and ſtill leſs can we preciſely determine the effect of the alteration upon the functions of the living body. If it be true, however, as ſo many medical practitioners believe, that Conſumption is now much more frequent (c), it is eaſy according to my ſyſtem, to underſtand whence this has happened. The inhabitants

(c) I know ſome old practitioners who are perſuaded that Conſumptions have become more frequent within their own memory.

inhabitants of this country, almoſt without exception, breathe a freer and a purer air than their anceſtors. Nor do I believe that there is any particular in which the difference between the preſent and ſome paſt generations, is ſo remarkable. You ſee then that the ſubjects of our Edwards, and our Henrys, and of good Queen Beſs may have found, in being more free from ſo formidable a diſeaſe than their delicate and airy poſterity, ſome compenſation for the confined air and filth in which they paſſed their exiſtence.

6. I have mentioned *(Obſervations, p.* 146*)* a caſe of phthiſis where the ribs on one ſide were preſſed quite inwards, and a cavity formed on that ſide, in conſequence no doubt of the deſtruction of the correſponding lobe of the lungs; and perhaps a ſubſequent ſoftening of the ribs. If your curioſity was in any degree raiſed by this ſingular occurrence, I hope it will be gratified by the following communi-

cation, for which I am indebted to a Gentleman, perfectly well known to you.

Letter from Mr. Yonge, Surgeon at Shiffnall in Shropshire.

SHIFFNALL, April 15, 1793.

DEAR SIR,

You will find ſubjoined a brief ſtatement of the caſe of Miſs Farmer, though I fear not ſufficiently accurate or diſtinct to afford you much ſatisfaction; it is formed from memory, and the information of ſiſters who attended her during the whole time of her illneſs, aided by reference to preſcriptions, and a few imperfect memorandums. It ſeems unneceſſary to detail at length the gradations of her incipient diſeaſe--they were the ſame as in other phthiſical caſes---or the ſucceſſion of inefficacious medicines employed. I ſhall only remark that during the few firſt weeks, the patient was more reduced in fleſh and ſtrength than the apparent ſeverity of the ſymptoms, or the means uſed

for

for her relief led me to expect; both however were ſomewhat reſtored before ſhe relinquiſhed the uſe of medicine in 1775, though without any abatement of the cough or expectoration. I am unable to diſcover at what time the depreſſion of the cheſt commenced, or the exact period of its increaſe or termination.

Your's Sincerely,

To W. YONGE.

Dr. BEDDOES,
Briſtol Hotwells.

" Mary Farmer, of a ſpare habit, and hectic " appearance was attacked at the latter end " of the year 1773 with cough, pain in the " left ſide of the thorax, and ſlight feveriſh " ſymptoms.---Theſe complaints were allevi- " ated by bleeding, ſaline medicines, and bliſ- " ſters----but after a few weeks introduced " phthiſis pulmonalis, which though mild in its " appearance, was diſtinct and perfectly formed.

" During

" During the period between this attack and
" the middle of the year 1775, the medicines
" chiefly employed and repeatedly varied
" were---gentle emetics of ipecacuanha, opi-
" um, Peruvian bark, vitriolic acid with con-
" ſerve of roſes, and fixed air taken into the
" ſtomach, none of which gave her permanent
" relief. In this interval alſo it was obſerved
" that ſome diſtortion of the thorax had taken
" place, from depreſſion of the ribs on the left
" ſide. The expectoration was not more than
" three ounce meaſures every twenty-four hours,
" of perfectly purulent matter, untinged with
" blood. The night-ſweats and pain conſtant,
" but not ſevere.---Theſe circumſtances con-
" tinued until the year 1779, without variation,
" when ſhe tried the air and waters of Briſtol
" without advantage, and in 1780 repeated
" ſome of her former medicines with no better
" ſucceſs. The depreſſion of the left ſide of
" the thorax was now very great, but did not
" increaſe after this time; and the diſeaſe in-

" creaſing

" creaſing by almoſt imperceptible gradations,
" ſhe died in September 1785, without the
" intervention of any unuſual circumſtance."

In this caſe, you ſee, the patient lived between ten and eleven years after the depreſſion of the ribs. Mr. Portal, the French anatomiſt, relates a caſe where the progreſs of Conſumption was remarkably ſlow, and where *la cavité droite de la poitrine fut trouvee absolument depourvue de poumon, en sorte que les trois lobes en etoient entierement detruits. La bronche droite etoit racornie et desseché comme un ligament. On doit remarquer qu' il n'y avoit aucune espece d' epanchement dans cette cavite; ce qui indique que cette destruction du poumon s'etoit faite depuis long temps & vraisemblablement depuis quatre ans.* (*La Medecine eclaireé, par Fourcroy I.* 207.)

You muſt have ſeen inſtances where the progreſs of Conſumption has been remarkably ſlow. I do not know whether you have remarked any correſponding peculiarity, internal or external.

The other variety of Conſumption ſets in at leaſt with very different ſymptoms, though I believe in its progreſs the higher oxygenation of the ſyſtem takes place: The ruddineſs, at leaſt, of the tongue is conſpicuous at an advanced period of the diſeaſe (*d*). This variety has been called, on account of the prodigious expectoration of mucus at its commencement, the *pituitous* or *catarrhal* Conſumption. I have ſeen the countenance pale and bloated, and ſeemingly but not really œdematous, in the firſt ſtage. In one recent inſtance where a very copious expectoration of mucus preceded for a long time the appearance of pus and the formation of the regular hectic fever, this fever vaniſhed together with the purulent expectoration; and a true anaſarca, not a mere œdematous

(*d*) *Accedit* tandem *febris primum erratica, horroribus calori crebro intersertis, dein plerumque post pastum accedens, tandem uno ferè tramite decurrens, post meridiem et vesperam versus exasperata. Quæ postquam impetiit, macies celeri gradu increscit, siti haud parum molestâ, linguâ purâ quin* rubicundâ, *sputis subinde adeo tenacibus ut suffocationem minentur.*

Murray de phthiſi pituitoſâ, § 7.

œdematous ſwelling of the feet from weakneſs, came on. You know what has been written on this variety of Conſumption, and for the preſent I ſhall content myſelf with referring readers, not ſo well informed, to Huxham (*e*) Chalmers (*f*) Duncan (*g*) and Macbride (*h*).

Several years ago a firm perſuaſion ſettled upon my mind that the ſyſtem might be as powerfully and as variouſly affected by means of the lungs as of the ſtomach. And the more knowledge we have acquired of the properties of elaſtic fluids, the more has my opinion been ſtrengthened. Of all the functions, Reſpiration is, I think, the beſt underſtood; and it will alſo be found in ſkilful hands to be the moſt eaſily manageable. It is impoſſible to doubt that we are nouriſhed by the lungs as truly as by the ſtomach; and that what we take in at the for-

(*e*) De aere and morbis.

(*f*) Diſeaſes of South Carolina.

(*g*) Medical caſes.

(*h*) Theory and Practice of Phyſic.—See alſo De Haen R. M. xv.

mer entrance becomes like our food a part of the ſubſtance of our ſolids as well as of our fluids. By the lungs we can alſo introduce effectual alteratives of the blood, and by conſequence of all the parts nouriſhed by the blood.

Early in the courſe of my medical ſtudies I met with the writings of Mayow(*i*); and not long afterwards I became intimate with Dr. Edmund Goodwyn, to whom we owe a moſt maſterly experimental

(*i*) In the abſtract which I publiſhed of Mayow's phyſiological and chemical writings, I expreſſed myſelf in the following manner: "That the blood departs from the left or poſterior cavities of the "heart, richly laden with nitro-atmoſpherical ſpirit (oxygene) and "returns deſtitute to the right, ſeems ſufficiently eſtabliſhed; "and that it is more impoveriſhed, as it approaches nearer the "end of its circuit, is highly probable. From the frequency of "reſpiration it is evident that much of this principle is received; "and much therefore is left behind for the conſumption and uſe "of animal œconomy; and ſurely it ſerves other purpoſes beſides "keeping up the play of the left ventricle of the heart. If we "conſider further that the fœtus, though it can live without "reſpiration, muſt nevertheleſs borrow from the maternal blood, "by means of the placenta, a ſupply of the ſame principle—that, "as our author obſerves, the tranſmiſſion of the blood can never "be the ſole object of reſpiration, ſince this object might have "been accompliſhed by continuing the organization of the fœtus—

experimental inveſtigation of the function of reſpiration. Theſe circumſtances led me to reflect with peculiar earneſtneſs upon the action of the atmoſpheric air on the blood during its paſſage through the lungs; and at Edinburgh I made or witneſſed many experiments upon animals, tending to illuſtrate this important ſubject. Being afterwards appointed to the Chair of Chemiſtry in the Univerſity of

 Oxford,

" and how much blood goes to the ſecretory organs and the
" brains—from the whole we may ſafely infer that theſe data
" will one day conſpire with future diſcoveries in chemiſtry to
" unravel the myſterious operation of theſe organs. And the
" MAYOW of another age, by explaining, and perhaps alſo by
" teaching how to manage their intricate and multifarious
" machinery, may ſee that art, which can reſt firmly upon no
" other foundation than a juſt theory of the functions of the
" body, riſing under his hands into a beautiful and ſolid ſtructure.
" Nor, however remote MEDICINE may at preſent be from
" ſuch perfection, do I ſee any reaſon to doubt that, by taking
" advantage of various and continual acceſſions as they accrue to
" ſcience, the ſame power will be acquired over living, as is at
" preſent exerciſed over ſome inanimate bodies; and that not
" only the cure and prevention of diſeaſes, but the art of pro-
" tracting the faireſt ſeaſon of life and rendering health more
" vigorous, will one day half realize half the dream of
" Alchemy."

Chemical Experiments and Opinions, 1790, *p.* 60, 61.

Oxford, I found myſelf obliged to acquire as minute a knowledge of the properties of elaſtic fluids as poſſible.

From the moment I became acquainted with the effects of pregnancy in ſuſpending the progreſs of Conſumption, I conceived hopes that by combining this fact with the diſcoveries daily making in pneumatic chemiſtry, a ſucceſsful method of treating this diſeaſe, in ſome of its ſtages at leaſt, might be deviſed. During the ſame interval the idea of turning theſe diſcoveries to the benefit of conſumptive patients occurred to others: And ſeveral attempts, not abſolutely unattended with ſucceſs, were made to palliate their ſymptoms. But they were random trials, guided by no fixed principle; and, if we except thoſe of Dr. Fourcroy, it was impoſſible that they ſhould have been ſucceſsful, even if the means, properly employed, had been adequate.

In

In the mean time, my own reflections, aided by information obtained from various quarters, ſuggeſted to me an opinion concerning Conſumption, which leads to a method of practice totally different from any hitherto employed. And you, I am ſure, will not judge more unfavourably either of the theory or practice on this account. Juſt before the publication of my OBSERVATIONS, in which the theory is ſtated at length, an opportunity offered of bringing it in ſome degree to the teſt of experiment. The writer of the following letters, (who, you know, has been long extenſively engaged in the practice of medicine) brought me his ſon, at that time far gone in a Conſumption of the lungs. I propoſed that he ſhould breathe a mixture of airs, and be put upon a diet, conformable to my views. The father, convinced of the utter inſignificance of all the ordinary means of cure, chearfully conſented. An apparatus in itſelf very imperfect, but as good as the time and place would admit, was accordingly

ingly ſet up in an apartment of a common dwelling-houſe. But as the perſons, who uſed it, had never been accuſtomed to the proceſſes of pneumatic chemiſtry, it was unfortunately often out of order; and as it was neceſſary to ſend ſeven miles (to W. Hampton) to have it repaired, the patient was often three or four days, or even a week deprived of his modified air: And he paſſed upon the whole four or five times as many days entirely without it as with it; and when he did uſe it, the ſupply of air was infinitely more ſcanty than I could have wiſhed. Yet even ſo, the effect exceeded my moſt ſanguine expectations. And I cannot but ſuppoſe that if the effect of a larger ſupply had kept any proportion with the effect of the ſmaller, and if I had fallen upon the ſame plan of medical treatment and of diet that has ſince occurred to me, his life, even in that advanced period of his diſeaſe, might have been preſerved.

Letter

Letter from Mr. Crump, Surgeon and Apothecary, at Albrighton, in Shropshire:

ALBRIGHTON, May 29, 1793.

Dear SIR,

I am very ſorry it was not in my power to give you any ſort of hiſtory of the caſe of my ſon by the meſſenger you ſent, not having committed any thing to paper relative to him, but the opinion of ....., given about four years and a half ſince, at which time he recovered in ſome meaſure at leaſt from his fever, pain in his ſide, &c. The cough, however, never left him, but was of little conſequence, except he took freſh cold; and then his fever and expectoration always returned with violent fits of coughing---but in general ſoon ſubſided by his obſerving rules nearly ſimilar to what had been previouſly followed---until ſuch time I brought him to you; and I hope you will not think me guilty of flattery, when I tell you that I am very well ſatisfied, that if your

 method

method of living with the free uſe of the mixed air had been entered upon ſooner, we ſhould have ſaved him---I can only add that whenever we could procure the air properly, he was always moſt wonderfully relieved not only by diminiſhing the expectoration, *but evidently the fever, &c. On thoſe unfortunate days that we did not ſucceed (which was too often the caſe either from my other engagements, as alſo thoſe of my aſſiſtant when in the purſuit of unavoidable buſineſs---or at other times from our lutes failing, ſo that we could not keep the air confined) the poor boy uſed frequently to aſk me for ſome of Dr. Beddoes's breath---for the little

* If he was without the air for two days the fœtid ſtate of his breath was ſure to return with a conſiderable increaſe of the matter coughed up, which ſeemed always to be in a declining ſtate ſo long as we could procure the air---and even continued ſo to the laſt without variation---his appetite and reliſh for animal food conſiderably better during the time he inſpired.---Previous to his leaving the inſpiring off, he was ſeized with a violent fit of coughing, which brought up a quantity of red blood, which was ſucceeded by a great profuſion of matter, which was literally pumped up, and ſo much reduced him that he was not able to ſit up, and we could not convey the air into his lungs in bed.

little he had was almoſt gone. And I cannot in juſtice to you do otherwiſe than inform you that his dyſpnæa and expectoration of matter were always relieved by two or three inſpirations---and more ſo than by any medicine I ever gave him, or ever ſaw adminiſtered to others labouring under the ſame complaints.

Another thing I muſt obſerve, that the poor boy always regretted the loſs of blood---as you ſeemed to think it an improper mode of proceeding in Pulmonary Conſumption.

I wiſh very much to hear from you at ſome future period upon this buſineſs, and if you will be kind enough to point out any defect in the information I here ſend you, I will gladly anſwer you, as I can ſend you to Briſtol any further account in franks---at the ſame time I ſhall be happy to know if you have made any further trial of this promiſing plan for relieving this dreadful malady.

An obſervation I made during the time G. Crump inſpired the air, is perhaps worth notice---at leaſt I ſhould wiſh to know if it produced the ſame effects in others, that is, in a morning previous to his uſing it his breath was moſt abominably fœtid---that after uſing it two or three times it became not only very eaſy, but alſo very ſweet, and continued ſo, ſometimes for two or three days---he alſo told us at the firſt inſpiration, whether it would relieve him or not, by ſaying he would not tire himſelf, for that it was not of the right ſort---though the bell ſhaped inſtrument roſe with the uſual regularity in the water: I am not chemiſt ſufficient to explain this †. I beg you will excuſe the haſty manner in which I have ſent you the above account; and hope you will

† The hydrogene air having been procured ſometimes by means of heated iron, and ſometimes by means of charcoal, it is propable that ſome impurity, (as a proportion of ſulphur in the iron larger than ordinary, or of bitumen in ſome pieces of charcoal not well burned) might have produced that air, which the patient felt not to be of the right ſort.

T. B.

will remember that I ſhall have a pleaſure in anſwering any further enquiry you may wiſh to make---at the ſame time beg leave to tell you, that I have a heart replete with gratitude for your kind aſſiſtance to my poor boy,---and remain,

Dear Sir, your's truly,

To GEORGE CRUMP.

Dr. BEDDOES.

Extract of a ſecond Letter from Mr. Crump:

ALBRIGHTON, June 2, 1793.

Dear SIR,

I conceive, as nearly as we can trace back by the medicines my ſon took, &c. that it might have been about a year and a half that he laboured under the ſymptoms of Phthiſis Pulmonalis. The quantity of factitious air was as nearly as poſſible as one to two of atmoſpherical----ſometimes, I think a little

more atmoſpheric air; but towards the laſt of his uſing it, it was almoſt equal or it did not pleaſe him.----I imagine the immediate eaſe he always received from it made him extremely anxious to have it, what he called good.

I am, Dear Sir,

Your moſt obedient ſervant,

To GEORGE CRUMP.

Dr. Beddoes.

Mr. Yonge curious to ſee the effect of ſo ſingular a method of treating conſumption, once accompanied me when I viſited this patient, and has ſupplied me with the following memorandum:----"The account, given by Mr. "George Crump of his relief from the uſe of "the pneumatic machine was: *that on the days* "*of its application the cough, dyſpnœa, and* "*expectoration were invariably and greatly re-* "*lieved---and his nights less reſtleſs and feverish.* "*Indeed his eagerneſs for the renovation of his*

"*diſordered*

" *disordered apparatus left no doubt of the fidelity* " *of his testimony, so fully confirmed to us by* " *his father.*"

W. Yonge.

I do not know whether you will allow that the circumſtances of this caſe offered a reaſonable inducement to proſecute this method of treating conſumption. With me they were particularly calculated to weigh, as they ſeemed to give as much countenance as could poſſibly be expected to my reaſonings; in which I had even ventured to ſuggeſt that it would be more advantageous to lower the atmoſpheric air with hydrogene than with azotic air, becauſe the hydrogene has been obſerved in experiments made upon blood out of the body " to have the power of darkening its colour" (*Observations p.* 141), becauſe blood abſorbs a good deal of this air, and becauſe when inſpired, it is found to " poſſeſs no irritating " quality," a fact, which I hope to eſtabliſh to your ſatisfaction below. The experience of

Dr.

Dr. Percival, who found that carbonic acid air alleviated the ſymptoms of conſumption, and ſtill more that of Dr. Fourcroy, who obſerved them in not leſs than twenty caſes to be exceſſively aggravated by the inſpiration of oxygene air, contributed to induce me to try fully whether this method, which I now certainly knew to be ſafe and more palliative than any other, might not alſo in ſome ſtages hitherto hopeleſs, effect a real cure. After ſome heſitation, Briſtol Hotwells was fixed upon as the moſt proper place to erect the neceſſary apparatus, becauſe this reſort of invalids ſeemed more likely than any other ſituation to furniſh patients in all the various gradations of Conſumption.

In the conſtruction of the apparatus three eſſential objects were to be accompliſhed: 1. To provide the means of procuring the ſeveral ſpecies of air, at leaſt azotic, hydrogene, carbonic acid, and likewiſe oxygene, for I had no intention to confine myſelf to one incurable diſorder,

diſorder, as you will infer from my former ſpeculations, eſpecially what I have ſaid concerning Fever. 2. It was neceſſary that the reſervoirs ſhould be ſo large that the patients might be ſupplied with any quantity their ſymptoms might require. 2. It was alſo neceſſary to have it in my power to mix theſe airs with one another as well as with atmoſpheric air in any proportion.

Theſe objects have been compleatly attained by a conſtruction not very diſſimilar to that employed in the gazometers of Mr. Lavoiſier and Dr. Van Marum. I once thought it would be deſirable to introduce patients into rooms filled with modified air; and if this plan could be eaſily carried into execution, it would be preferable to inſpiration through a pipe or mouthpiece. But there would be ſome difficulty and much expence in fitting up ſuch rooms; nor I am ſure that they would be ſo totally exempt from danger; beſides experience has convinced me that patients very ſoon learn to inſpire

through a tube, held in the mouth, and to expire through the noſtrils. They are indeed at firſt apt to reſpire with a greater effort than ordinary ; but even in this particular they very ſoon correct themſelves.

I once thought of annexing drawings of my apparatus to this letter. But as I am in hopes of being ſoon furniſhed with materials for another publication upon the ſame ſubject, and as in the mean time the apparatus will hardly fail to receive conſiderable improvements, I concluded that it would be more adviſeable to defer the engravings for a few months. I have however provided a model for the inſpection of ſuch perſons on the ſpot as may be intereſted in acquiring an accurate idea of the method of treatment. I have alſo found it neceſſary to provide a portable apparatus for the uſe of patients unable to quit their beds or their apartments : And of this alſo I intend to publiſh a drawing and a deſcription. It may perhaps be ſaid that I ſhould have done better if

I had delayed all public notification of the exiſtence of the apparatus, till I had certainly determined the efficacy of the practice in which it is deſigned to be employed. But in the firſt place, it will be neceſſary for me to have the care of a conſiderable variety of caſes before I can determine its efficacy with certainty; and I could not expect this advantage till the nature of the deſign was generally known through the kingdom. Secondly, I did not chooſe to truſt to unauthorized rumour to make it known; and, in the third place, I imagine there are many practitioners of medicine ſo convinced of the futility of all former methods of treating Conſumption, as to be deſirous that their patients ſhould have the chance of any new method that affords hopes of relief, and is aſcertained to be ſafe and not diſagreeable; a ſentiment in which I ſuppoſe many patients will concur: And in proportion to the efficacy of the method, will be the number of lives preſerved by making it ſpeedily known. I may add, that the

expence of keeping ſuch an apparatus in uſe is not inconſiderable; of courſe I muſt wiſh to be able as ſoon as poſſible to form a judgment whether it would be prudent to lay it aſide, or to extend it.

Of the three Airs above mentioned, I hope the moſt from the employment of hydrogene to reduce the air of the atmoſphere to a lower ſtandard. From a number of trials it appears to be ſo totally free from all irritating and poſitively deleterious properties, that it may be breathed unmixed with impunity for a much longer time than any other of thoſe airs that are incapable of ſupporting life, except perhaps azotic. Thus Mr. Scheele tells us that having by a ſtrong expiration expelled from his lungs as much atmoſpheric air as poſſible, he was able to make twenty inſpirations without inconvenience. Pilatre de Rozier frequently repeated the ſame experiment; and when it was objected to him that he did not empty his lungs of atmoſpheric air, he proceeded in the following

lowing manner. He inhaled a large quantity of hydrogene air, and then, applying his mouth to a tube, he blew out the air and fired it at the end of the tube, ſo that he appeared to breathe flame. In this caſe, he obſerved, if there had been any admixture of atmoſpheric air in his lungs, a detonation muſt have taken place in his mouth and cheſt. I have myſelf felt the pulſe of Mr. Sadler, the Engliſh Aeronaut, while he has inſpired, for above a minute and half at a time, pure hydrogene air from ſteam and heated iron. From the rate of eighty-four in a minute it increaſed to an hundred and ten in fifteen ſeconds, and became ſoft and weak. A ſtout florid young woman breathed the ſame air for two minutes, during which ſhe made twenty-ſix inſpirations. She was not in the ſmalleſt degree affected, till ſhe had walked down a flight of ſtairs. She then felt ſome ſickneſs, and her head became a little dizzy and continued ſo for two hours, after which ſhe felt perfectly well. Two other per-

ſons on the ſame occaſion made ſeveral ſucceſſive full inſpirations of this air without the ſlighteſt inconvenience. It was inhaled through a tube, and in conſequence of preſſure on the reſervoir, a ſtrong current ſet into the mouth. The noſtrils were not cloſed in any of theſe inſtances. A ſmall quantity of atmoſpheric was therefore probably inhaled with the hydrogene air.----Hydrogene is ſaid, when animals are immerſed in it, to be as ſuddenly deleterious as carbonic acid air: *(Priestly's Exp. New Ed. I.* 229*)* and as ſuch experiments, if accurately made, are certainly the moſt deciſive, I have perhaps expreſſed myſelf too ſtrongly in aſſerting "that it ſeems pretty well aſcer-"tained that hydrogene will not ſo ſoon pro-"duce aſphyxia as other mephitic airs." Nevertheleſs in ſome inſtances which I have known of inſenſibility produced by the fumes of charcoal, I conceive that the ſufferers muſt have breathed a mixture containing more atmoſpheric air than the perſons above mentioned.

tioned. Carbonic acid air may be ſuſpected ſomewhat to accelerate aſphyxia by the irritation it produces. Nitrous air evidently does ſo by forming nitrous acid: When Dr. Prieſtly put fiſhes into water impregnated with nitrous and other airs, he found the former to have a more immediately deleterious effect. The difference, if any exiſts, might be aſcertained by obvious experiments on ſome of the ſmaller of the warm blooded animals.

It is a curious, and to me an agreeable circumſtance that animals may be gradually inured to air with leſs oxygene than uſual. In the caſe of phthiſis related above, the patient continually demanded more and more hydrogene air; and can you doubt that the human frame poſſeſſes the ſame power of adapting itſelf to a change of circumſtances that has been diſcovered in inferior animals, which reſpire in the ſame manner? *(Priestly III. 257.)*

Since my Apparatus has been at all fit for uſe, I have employed hydrogene air in one pulmonic caſe; and the reſult has been ſatisfactory. A perſon of fair complexion and dark eyes had been for ſome time paſt ſubject to attacks of hæmoptyſis attended with dyſpnœa, ſevere coughing, and pain in the thorax. On a late ſeizure of this kind I adviſed the uſe of modified air. The pulſe was hard, from an hundred and four to an hundred and eight. The countenance much flushed; the ſkin dry and hot, with conſiderable febrile rigour. All her feelings the ſame as at the commencement of former ſeizures. She began by breathing three hundred and fifty-ſix cubic inches of air; of which forty-four (nearly one-eighth) we hydrogene from ſteam and red hot iron. This quantity ſhe conſumed in fifteen minutes. When ſhe ſat down to inſpire, ſhe had very acute pain under the ſternum; which entirely ſubſided while ſhe was breathing. The pain afterwards returned, but with leſs violence; and

upon

upon inſpiring from ten to fifteen minutes at a time ſeveral times a day for three days, it entirely diſappeared with the other ſymptoms.-- It had before occurred to me that air of a reduced ſtandard would be extremely beneficial in ſthenic inflammation; and I am confirmed in the opinion by this inſtance.

I have ſeveral times ſeen the ſhort dry cough ſtopped at once by air ſimilarly modified. But without further experience I dare not affirm that it was owing to the medicinal power of the mixture, ſince we ſo frequently ſee morbid affections of the body ſuſpended by a change in the ſtate of the mind.

Should I find it neceſſary to ſubſtitute azotic in the place of hydrogene air, the accidental diſcovery (which I made a few years ago, and which is announced in Dr. CRELL's *Chemical Annals* for 1790) of a ready method of procuring this ſpecies of elaſtic fluid by efferveſcence will, I truſt, prove ſerviceable. Nothing but the want

of a few more experiments to aſcertain which of ſeveral variations of the proceſs is the beſt, and what quantity is afforded by the different proceſſes, has prevented me from ſubmitting an account of my method to the Royal Society; and whether I ſhall have occaſion for this air for medical purpoſes or not, I hope ſoon to be able to bring my experiments to a concluſion.

After ſecuring a full ſupply of oxygene air, the firſt thing I undertook, was to attempt to throw ſome light upon the nature of Conſumption by an experiment upon myſelf. Not having any thing of the phthiſical conformation or the ſlighteſt hereditary claim to the diſeaſe, I thought I might venture very far in oxygenating myſelf without any great riſque; and it was impoſſible for me to obſerve the effects ſo minutely in another perſon. I accordingly reſpired air of a much higher than the ordinary ſtandard, and commonly ſuch as contained almoſt equal parts of oxygene and azotic air

for

for near ſeven weeks with little interruption. I breathed it upon the whole ſometimes for twenty minutes, ſometimes for half an hour, and ſometimes for an hour in the day, but I never continued breathing for above four or five minutes at any one time. I felt, at the time of inſpiration, that agreeable glow and lightneſs of the cheſt, which has been deſcribed by Dr. Prieſtly and others. In a very ſhort time I was ſenſible of a much greater flow of ſpirits than formerly, and was much more diſpoſed to muſcular exertion. By degrees, my complexion, from an uniform brown, became fairer and ſomewhat florid. I perceived a carnation tint at the ends of the fingers, and on all the covered parts of the body the ſkin acquired much more of a fleſh colour than it had before. I was rather fat, but during this proceſs I fell away rapidly, my waiſtcoats becoming very much too large for me; I was not ſenſible however, of any muſcular emaciation, but rather the contrary. My appetite was good;

and I eat one-third or one-fourth more than before without feeling my ſtomach loaded. In no long time I obſerved in myſelf a remarkable power of ſuſtaining cold. Except one or two evenings when I was feveriſh, I never once experienced the ſenſation of chilline ſs, though cold eaſterly winds prevailed, during great part of the time I was inſpiring oxygene air. I not only reduced my bed clothes to a ſingle blanket and cover-lid, but ſlept without inconvenience in a large bed-chamber, looking to the N. E. with the window open all night, and with the door and windows of an adjacent ſitting room alſo open. About the expiration of the above-mentioned time, I perceived ſome ſuſpicious ſymptoms. It was uncomfortable to me to ſit in a room at all cloſe. I frequently felt a ſenſe of heat and uneaſineſs in my cheſt; and my ſkin was often dry and hot with burning in my palms and ſoles; my pulſe, which had hitherto ſeldom exceeded eighty, was above ninety in the evening. At this time

I took

I took a journey of about 170 miles, the greater part in a mail coach in the night, the reſt on horſeback. The roads were uncommonly duſty, and ſeveral circumſtances concurred to harraſs and fatigue me. On the way I met with a medical friend, who was much ſtruck with the fluſhed appearance of my countenance, and upon feeling my ſkin and pulſe, which varied from an hundred and four to an hundred and twenty, imagined that I was become hectic. I had now, though but ſeldom, a ſhort, dry cough; but the ſenſe of irritation to cough required an almoſt conſtant effort to ſuppreſs it; this ſenſe of irritation was, as you will ſuppoſe, attended by dyſpnœa. I had alſo frequent bleedings at the noſe, an event almoſt unprecedented with me; the blood was of an unuſually bright colour; which was alſo ſeen in blood forced from the gums. Whenever I pierced the ſkin in ſhaving, the blood flowed in greater abundance than uſual, and was ſtaunched with difficulty.

Theſe ſymptoms indicate a tendency to hæmoptyſis (*k*), and they, of courſe, ſo ſtrongly corroborate my theory of Conſumption, that I am almoſt afraid leſt they ſhould be conſidered as imaginary or fictitious. I am glad therefore to have it in my power to appeal, for all thoſe circumſtances which could fall under the notice of ſpectators, to teſtimony that will weigh ſo much with you as that of Mr. William Reynolds, at whoſe houſe at *Coalpit Bank* I was when moſt indiſpoſed, of his brother, Mr. Joseph Reynolds, and of Mr. W. Yonge. By keeping quiet and cool, and by the aſſiſtance, as I ſuppoſe, of a diet in which ſweet oil, butter and cream bore a large proportion to the other articles, I very ſoon recovered my health. My inſenſibility to cold, as I had an opportunity of aſcertaining, during the evenings we have lately had

(*k*) Hæmoptysis: *genarum rubor; moleſtiæ aut doloris, et aliquando caloris, in pectore ſensus; dyſpnœa; titillatio faucium; tussis aut tussicula ſanguinem floridum, sæpe ſpumoſum rejiciens.* (Cullen Noſolog. II. 156).

had (June 21ſt----24th) ſtill continues; and the change of complexion will probably be permanent; it may at leaſt, I doubt not, be rendered ſo by inſpiring ſmall quantities of oxygene air from time to time.---Is there not alſo room to conjecture, as well from this experiment as from the remarkable tendency of oxygene to generate heat, whenever it enters into combination, that many invalids may be advantageouſly fortified againſt the cold of winter by breathing atmoſpheric improved by the addition of oxygene air? We ſhall not, I hope, be long without a convenient ſmall apparatus for procuring and containing oxygene air; and ſuch an apparatus, I ſhould expect, would ſoon come to be ranked among the ordinary articles of houſehold furniture. In this caſe I think it probable that oxygene air will ſuperſede all other coſmetics; one deciſive advantage it will certainly have over them all, for while it improves the looks, it will, if judiciouſly adminiſtered, amend the

health alſo.---In true aſthmatic fits its beneficial effects have been already many times experienced. No ſooner does it touch the lungs than the livid colour of the countenance diſappears, the laborious reſpiration ceaſes, and the functions of all the thoracic organs go on eaſily and pleaſantly again.

As ſo many more diſeaſes ariſe from the defect than the exceſs of vital energy, I expect eſſential benefit from oxygene air in a conſiderable variety of diſeaſes; and I look upon it as fortunate that the inſpiration ſhould be attended with agreeable ſenſations. A mixture of air containing about thirty-five parts in an hundred of oxygene may be employed with probable advantage and with undoubted ſafety in the following diſeaſes: 1. In Typhus, but not perhaps where the excitement of the vaſcular ſyſtem is too great, as long as this ſymptom continues. 2. In Hyſteria, and ſimilar nervous affections. 3. In Anaſarca and Hydrothorax, after the evacuation of the water; in theſe caſes we find

find the extremities, eſpecially the feet, to be habitually cold, almoſt to inſenſibility; oxygene, I think, would give life and activity to the remoteſt abſorbents, and prevent a freſh accumulation of water. Indeed I ſuſpect that in conſequence of this operation it will be found the moſt powerful and ſalutary of all diuretics. 4. In Diabetes, may not an addition of the acidifying principle to the blood correct the tendency of the kidneys to ſecrete ſaccharine urine; and hence cure this infrequent but fatal diſeaſe? 5. I am not without hopes of being able to repair the injury ſuſtained by the liver from too free a mode of life. In one inſtance where redneſs of the noſe alternated with heavy internal pain about the region of the liver, the natural colour of the noſe has been for ſome time permanent and the pain has diſappeared; but a ſufficient interval has not elapſed, ſince theſe changes took place, for any deciſive opinion to be given upon the caſe. 6. In ill-conditioned Ulcers, eſpecially

of the legs, and in ſome of the affections termed ſcrophulous. 7. In Palſy. 8. Does not the leaden complexion in Schirrus indicate a deficiency of oxygene? Superficial readers will perhaps be tempted to charge me with inconſiſtency for imputing diſeaſes, marked by ſuch different ſypmtoms, as ſcurvy, typhus and ſchirrus to a deficiency of the ſame conſtituent part of animal bodies; but you will allow that it is perfectly conſonant to analogy and therefore probable, that diſeaſes, widely differing in form, ſhould be produced as this principle is more or leſs withdrawn, and as it is withdrawn more or leſs rapidly.----Having mentioned ſchirrus, it is ſuperfluous to ſay that oxygene air ſhould be tried in cancer alſo. 9. In Hydrophobia and ſome other deſperate diſeaſes, no one, I ſuppoſe, would object to the trial of various mixtures of air, even though we are totally deſerted by theory.

It is needleſs to continue this enumeration. Every perſon, who underſtands the principles,

will

will be able to continue it for himſelf; and ſucceſs in one will encourage to a trial in other diſeaſes. In a future letter I hope to preſent you with a catalogue of diſeaſes in which I have effected a cure. The power of the various elaſtic fluids, and of a diet and medicines calculated according to the theory, which preſcribes a particular mixture of airs in any given caſe, will I hope, ſoon be determined. There are many diſeaſes in which neither patients nor practitioners have much reaſon to be ſatisfied with the ſtate of medicine; and multitudes will, no doubt, concur with me in endeavouring to put it upon a better footing. Many circumſtances, indeed, ſeem to indicate that a great revolution in this art is at hand. We owe to PNEUMATIC CHEMISTRY the command of the elements which compoſe animal ſubſtances; Now it is difficult not to believe that much depends on the due proportion of theſe ingredients; and it is the buſineſs of PNEUMATIC MEDICINE to apply them with caution and

intelligence to the reſtoration and preſervation of health.---And if you do not, as I am almoſt ſure you do not, think it abſurd to ſuppoſe the organization of man equally ſuſceptible of improvement from culture with that of various animals and vegetables, you will agree with me in entertaining hopes not only of a beneficial change in the practice of medicine, but in the conſtitution of human nature itſelf.

THOMAS BEDDOES.

Hope-Square,
BRISTOL HOTWELLS,
June 30, 1793.

It is proper to mention, and it may be ſatisfactory to ſome readers to be informed, that the preceding remarks on the varieties of conſumption were printed before the receipt of the following letter: And as I had never any opportunity of knowing his ſentiments on this point before, I could not but be gratified on finding that I had formed nearly the ſame opinion with a Philoſopher, not leſs diſtinguiſhed by the talent of obſervation, than that of invention.

DERBY,

DERBY, Jan. 17, 1793.

Dear SIR,

Your treatiſe on Conſumptions I have read with great pleaſure, and am glad to find you are about to combat this giant-malady, which has hitherto baffled the ſkill, and withſtood the proweſs of all ages; and which in this country deſtroys whole families, and, like war, cuts off the young in their prime of life, ſparing old age and infirmity.

The few obſervations I have made on this diſeaſe, ſince you requeſt them, are at your ſervice; as I wiſh to contribute even a mite to your great deſign. I hope you will be led to try a variety of experiments with mixtures of airs; your very ingenious reaſonings from the ſcarlet colour of the blood in conſumptive patients, and from the inflammatory ſize or coagulable part of it, and from the delay of the progreſs of conſumption in pregnant women, indicate indeed hyperoxygenation to

be the cauſe of this fatal diſeaſe. But by inſtituting other experiments with different kinds or proportions of airs, you will, at leaſt, if your firſt experiments ſhould not ſucceed to your utmoſt wiſh, hold out hopes to thoſe unfortunate young men and women; who, if they knew the general fatality of their diſeaſe under the preſent modes of treatment, would deſpond at the commencement of it, or wiſh to try ſome new kind of medicine: For though catarrhs are ſometimes miſtaken for conſumptions by the ignorant, or are deſignedly called ſo by the crafty, and are hence ſuppoſed occaſionally to have been cured; yet it is well known to attentive practitioners, that thoſe patients, who are truly conſumptive from pulmonary ulcers, which have ariſen ſpontaneouſly, (and are thus diſtinguiſhable from a ſingle ulcer owing to violent peripneumony, or the wound of a ſword)---whether they are kept on vegetable or on animal diet; and whether, with the uſual quantity of wine and beer, or with water and

milk

milk alone---at length ſubmit to fate; and that generally with many of their relations by heriditary prediſpoſition, or with their neareſt friend by contagion.

The immediate caſe of Pulmonary Conſumption conſiſts in ulcers of the lungs; theſe ulcers, whether they ariſe ſpontaneouſly, or from previous infection, may vary in reſpect to their ſeat, as well as to their remote cauſe; but I ſuppoſe the immediate cauſe of theſe ulcers to conſiſt in the inirritability of either the lymphatic or of the venous ſyſtem. Firſt, if the mucaginous fluid, which is poured into the cells of the mucous membrane of the lungs be not perfectly abſorbed, it will produce ulcers by its accumulation, or by its chemical change. Secondly, if the blood brought to the internal ſurface of the bronchia by the bronchial arteries be not perfectly abſorbed, or taken up by the correſpondent bronchial veins, hæmoptoe will be induced, and ſmall ulcers ſucceed in conſequence.

Hence

Hence there has always appeared to me to be two kinds of Pulmonary Conſumption, one which begins with ſlight hæmoptoe and which is generally ſeen in dark-eyed people with large pupils; and the other, which commences without hæmoptoe, and which is generally ſeen in light-eyed people with large pupils. The aperture of the iris in both theſe kinds of Conſumption is generally large, which evinces the inirritability of the eye, and thence perhaps in conſequence the inirritability of the whole ſyſtem. The former of theſe conſumptions is generally heriditary without any appearance of ſcrophula; and the latter with appearance of ſcrophula in the preſent, preceding, or third generation upwards. The former commences more certainly between ſeventeen and ſeven and twenty; the latter attacks people of all ages.

I believe both theſe Conſumptions to be infectious to thoſe who ſleep with ſuch patients in the laſt ſtage of the diſeaſe; as I have obſerved

obſerved a huſband in two caſes begin to be diſeaſed ſoon after the death of his wife; and in one a wife, who became conſumptive ſoon after the death of her huſband; in all which caſes there was no reaſon to ſuſpect hereditary prediſpoſition.

The hæmoptoe generally begins at its firſt attack during ſleep, for as reſpiration is in part a voluntary action during our waking hours, and as the power of volition is totally ſuſpended in perfect ſleep, the blood in the lungs of irritable or weak people is liable to accumulate for want of the aid of the voluntary power at that time, and hence hæmorrage enſues; and the ulcers are produced in conſequence of the rupture of the bronchial veins, and of ſome part of the effuſed blood ſtagnating in the air-veſſels, and undergoing a chemical change.

Large abſceſſes frequently exiſt many weeks---ſo long as they are precluded from any acceſs

of air without occaſioning hectic fever; but on their ſurfaces being expoſed to the contact of the air by opening them, hectic fever is occaſioned in a very few hours. From this cauſe ariſes the advantage of opening large abſceſſes by means of a ſeton paſſed through them. Hence where a wound is required to heal by the firſt intention (as it is called) as in compound fractures, it muſt carefully be confined from any acceſs of air. And hence laſtly the reaſon, why ulcers of the lungs are ſo difficult to heal, viz. becauſe they are perpetually expoſed to a current of air. What part of atmoſpheric air is hurtful to ulcers, whether it be the oxygene in too large a quantity, as your ingenious reaſonings ſeem to countenance; or too great a proportion of azotic air, I hope your cautious experiments and your particular attention to conſumptive patients will ſoon decide.

One agreeable circumſtance attending your application of different kinds of air to conſumptive

ſumptive people is, that they may at the ſame time perſiſt in any plan of medicine or diet, which theirſelves or their friends for them are ſolicitous to purſue, without interfering with your remedy which is immediately applied to the ſeat of the diſeaſe, and the ulcerations of the lungs.

Go on, dear Sir, ſave the young and the fair of the riſing generation from premature death; and reſcue the ſcience of medicine from its greateſt opprobrium.

Your's, &c.

To E. DARWIN.

Dr. BEDDOES.

P. S. Dr. Wood of Newcaſtle has juſt publiſhed a theory of Typhus, varying but in terms from that which I had before given in the book ſo often quoted. He thinks that typhus is produced by "the *accumulation* or *over-pro-*"*portion* of carbone and hydrogene, and an exhauſted ſtate "of irritability." I ſuſpect that ſuch an accumulation would require more time than the production of this fever

ever takes; and I ſtill think it much more probable that the oxygene is ſuddenly diminiſhed by the action of contagion, or of the ſtimuli, that operate like contagion. This phyſician from experience recommends nitre in typhus. We know how much nitre was a favourite medicine with the Stahlian ſchool; and though it has been of late almoſt entirely diſregarded, I have reaſon to believe that it will prove a medicine of great uſe, now we have a rule for its adminiſtration; and I will not loſe the preſent opportunity of repeating the wiſh I formerly expreſſed, that nitre and acid of tartar might be both employed in ſea ſcurvy.

Carbonic matter long ſince preſented itſelf to my mind as likely to be ſerviceable in diſeaſes, where we ſhould deſire to deprive the ſyſtem of oxygene. Its great attraction for oxygene in high temperatures has long been known; and the experiments of Mr. Lowitz, and ſtill more the very ſurpriſing ones of Dr. Kels *(Crell's Annalen St.* 3, 1792*)* and of Dr. Buchholz *(Gren's Journ. der Physik B.* v. *p.* 3.*)* ſhew that at a temperature conſiderably below that of warm-blooded animals, carbonic matter is by no means ſo inert a ſubſtance, as it has hitherto been reputed. Dr. Moench *(V. d. arzney-mitteln p.* 221*)* aſſures us that he has given it largely with ſucceſs; and the *spongia usta*, though in my opinion a bad preparation and in general too ſparingly adminiſtered, has been thought ſerviceable

viceable in ſcrophula. I have lately given it in a form more capable of being acted upon by the powers of digeſtion than that in which it commonly appears: And, as I believe, with effect. One patient in this neighbourhood had a ſhort, dry cough, pain in the thorax, heats with occaſional ſhiverings, amenorrhœa, fluſhed countenance at times, quick pulſe, and with other ſigns of tubercles threatning ſuppuration---at leaſt of imminent phthiſis; of which diſeaſe ſome of the family had died. I propoſed the inſpiration of modified air; but as the apparatus was not ready for uſe---and it will not indeed be *perfectly finished* before the end of the preſent week---I attempted to retard the approach of the diſeaſe by carbonic matter. The patient grew gradually better, and for ſome weeks has had no phthifical ſymptom. I am now employing the ſame means in confirmed phthiſis; and as ſoon as I can form an opinion concerning the beſt mode of preparation, and the efficacy, of the medicine, I will publiſh it.

Since this letter has been in the preſs, a patient whoſe principal ſymptoms are too great ſecretion of bronchial mucus, and dyſpnœa on walking up even a ſlight aſcent has been reſpiring air containing about thirty-ſix parts of oxygene air in an hundred. He is already ſenſible of ſome relief in his breathing, though he has hitherto reſpired only for five minutes at a time twice a day. And the hands and covered ſkin, which were before too pale, are, he thinks,

becoming more coloured. There can ſcarce indeed be a doubt concerning this effect of oxygene air. Dr. Fourcroy expreſsly mentions the heightening of the complexion in the patients who breathed it under his inſpection; and this might have been fully expected from the change which blood undergoes, when brought into contact with oxygene air; which, if ſuch an expreſſion were allowable, deſerves to be conſidered as the true *sweetner* or *purifier* of the blood. Mr. Chaptal experienced great benefit from this air in a caſe of humoural aſthma* ſimilar to that which I have mentioned laſt.---To what I have before ſaid of the convulſive cough preceding phthiſis, it may be added that in Dr. Fourcroy's patients when the diſeaſe returned with increaſed violence, the firſt ſymptom was a dry, convulſive cough: It is remarkable that both Fourcroy and Chaptal uſed oxygene air unmixed with atmoſpheric. This can ſcarce be proper in any caſe---Inſtances may occur in which an eaſy method of oxygenation might be practiſed. We draw in at every inſpiration about twelve cubic inches of air at a medium. By a full inſpiration after a careful

* Believing that this air would be beneficial whenever *le poumon est engorgè par des humeurs pituiteuses, J'ai essayè*, ſays Mr. Chaptal *de le faire respirer à un asthmatique qui en effet a ete* PRODIGIEUSEMENT SOULAGE. *(Annales de Chemie* iv. 22.) It ſhould not be omitted that in one of Mr. Chaptal's phthiſical caſes, the firſt effect of oxygene air was ſuch that in three weeks the patient was able to take *pretty long walks*, though before ſo weak that he could not quit his bed. In this and another caſe it produced great animation and gaiety *(l. c.)*.

careful expiration we can inhale two hundred cubic inches. Hence by a moderate effort we can take in from ſixty to an hundred cubic inches. Thus we ſhall bring more air than uſual within the attracting diſtance of the blood; and by continuing this proceſs, oxygenate the ſyſtem. The effect will be promoted by ſlow inſpirations and quick expirations. I have thus in a few minutes often produced a more general glow and higher colour on the cheeks than I could impute to the mere increaſe of muſcular exertion.----I ſubjoin a few queries.---1. Does not the iris generally become more contracted juſt before and juſt after phthiſis? 2. Though it is impoſſible not to perceive the acuteneſs of the argument from the habitual dilatation of the pupil to the inirritability of the ſyſtem, do not ſome appearances ſhew more deciſively ſtill that the ſyſtem about the beginning of conſumption is highly irritable? Do we not for inſtance find that ſtimulants, as fermented liquors, ſet ſuch perſons all on fire? I have ſometimes obſerved a meal of ſolid food without even ſmall beer produce a flow of ſpirits apparantly greater than a moderate quantity of wine would in health. In florid conſumption in ſhort, before great debility comes on, do not weak ſtimuli induce diſagreeable burnings, hard pulſe, &c.?---Even ſo, I do not underſtand why in an irritable ſtate of the body, the iris ſhould be unirritated. 3. Do not Dr. Fourcroy's caſes compared with thoſe above related render it probable that the atmoſpheric oxygene, and not its azote, is prejudi-

cial

cial to ulcerated lungs, and ſo far ſolve Dr. Darwin's problem? 4. It is common in France, and, I believe alſo in Ireland, to lodge phthiſical patients in cow-houſes, on the ſuppoſition that the ſweet breath of the cow is healing and balſamic. *L'air des etables de bêtes á cornes*, ſays Dr. Metherie, *est assez pur pour que la medicine ait cru pouvoir le conseiller dans les maux de poitrine (Airs. 1788. II. 13.)* But if it be beneficial, is not the effect much more probably owing to the ſubtraction of oxygene by the reſpiration of the animals?

T. B.

5th JULY, 1793.

N. B. The few typographical errors in the preceding pages are such as every reader will be able to correct.

LETTERS

FROM

Dr. WITHERING, of *Birmingham*,

Dr. EWART, of *Bath*,

Dr. THORNTON, of *London*,

AND

Dr. BIGGS, late of the Isle of *Santa-Cruz*;

TOGETHER WITH

SOME OTHER PAPERS,

SUPPLEMENTARY TO TWO PUBLICATIONS

ON

Asthma, *Consumption*, *Fever*,

AND OTHER DISEASES,

BY

THOMAS BEDDOES, M. D.

BRISTOL:

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TO

JOSEPH BLACK, M. D.

PROFESSOR of CHEMISTRY

In the Univerſity of *Edinburgh.*

DEAR SIR,

I HAVE always, ſince our firſt acquaintance, found gratification in avowing the reſpect I entertain for a character ſo eminent as yours for genius and candour. But your late adoption of Mr. LAVOISIER's ſyſtem has greatly added to the force of this ſentiment: And the recollection of ſo ſignal a proof, that neither years nor celebrity—the bane of vulgar minds—have had power to blunt your ſenſibility to truth, affords me greater pleaſure than I ſhould otherwiſe have felt in dedicating to you the following ſmall collection of obſervations.

Theſe obſervations principally relate to a ſubject of unſpeakable importance; and one, in which our own countrymen are more deeply intereſted than almoſt any other portion of the human race.—The *invariable fatality* of Pulmonary Conſumption is among thoſe notions which obſervation and reading render moſt familiar to the minds of medical men. Many other perſons are, however, by no means fully apprized of this melancholy truth. For "*Catarrhs*," as a great phyſician has obſerved, "are ſometimes miſtaken by the ignorant for "*Conſumptions;* or deſignedly called ſo by the crafty. "Hence they are *ſuppoſed* to have been occaſionally cured." Dr. Cullen uſed to mention in his lectures *one* inſtance of recovery from what he ſuppoſed to be a real conſumption. But he profeſſed himſelf unable to form any conjecture how this almoſt miraculous event was brought about; and of courſe he could not apply it to the benefit of other patients. I have heard of no other credible inſtance of recovery from well-aſcertained conſumption, except thoſe mentioned in the following communications; yet the different practitioners whom I have queſtioned reſpecting the reſult of their experience muſt, I ſhould imagine, have ſeen ten or, perhaps, twenty thouſand patients. It may very ſafely be aſſumed that at leaſt ninety-nine out of every hundred perſons, ill of conſumption, are cut off, notwithſtanding the very earlieſt adminiſtration of the various compoſitions proclaimed by advertiſements, or of the means adviſed in books, for their relief. Now what ſort of remedies muſt thoſe be, under whoſe ope-

ration nineteen patients die out of twenty, or even four out of five? Do they deſerve the name of *remedies?* Credulity might, in this inſtance, take a leſſon from the reply made to a boaſtful Pagan prieſt; who, in order to ſurpriſe a traveller into admiration of the power of his Deity, produced a liſt of the names of perſons whom He had preſerved from ſhipwreck, in conſequence of the vows they had offered to Him; "very well," ſaid the traveller, "ſo far, ſo good; now let "me ſee the liſt of thoſe who periſhed in ſpite of their vows."

If the means in uſe for the cure of conſumption uniformly fail, the means of prevention are alſo lamentably deficient. Every body knows the diſeaſe to be dangerous; the ſigns therefore that indicate its approach commonly excite alarm, and, on their firſt appearance, few except the needy neglect to call in the aid of medicine, and many, doubtleſs, ſteadily purſue the directions they receive. Nevertheleſs, rich houſes are every day diſcharging into the grave victims to this dire diſeaſe.

Deluſion of every kind will, I imagine, on calculating its effects, be found injurious to ſociety. The prevailing degree of perſuaſion, that Pulmonary Conſumption has been and may again be cured either by quack medicines or by any other of the uſual means, is obviouſly productive of two bad conſequences; 1. As it enables a moſt pernicious ſpecies of impoſtors to fatten on the produce of fraud; and 2. As it renders phyſicians leſs active in ſeeking, and the public leſs urgent in requiring, an efficacious method of treatment. Are theſe evils outweighed by the common-place plea in favour of deception? Of this plea, which is ſuggeſted ſometimes by real, and ſometimes by affected humanity, I, for my own part, queſtion the validity: in the firſt place, becauſe I have obſerved phthiſical patients, under full aſſurance of their fate, ſuffer leſs than others, who have only ſuſpected their danger (as they ſeldom fail to do), and who in conſequence were agitated by inceſſant viciſſitudes of hope and terror;—and, ſecondly, will not every ſanguine patient, however firmly convinced that the *true conſumption* is inevitably fatal, perſuade himſelf that his own is not a caſe of *true conſumption?*

Such, if I know myſelf, would be my opinion, were I a diſintereſted ſpectator of thoſe ſcenes of domeſtic miſery, which Conſumption is every hour producing. The deſire—a reaſonable deſire, I hope—of ſeeing my own project fully and ſpeedily carried into execution, may render me more eager to diſſipate any rival deluſion. But I am confident, from the temper of the preſent age, and from ſeveral peculiar circumſtances, that it will be tried in every poſſible form. Patients themſelves, or their friends for them, will ſoon learn

to

to ask their medical attendants these two simple questions: "Have you had so much favourable experience of any other "method as to advise me to trust my life to it? Do you know "the method, newly proposed, to be inefficacious?"—The following testimonies, must also have the greater weight both with the public and with the members of the medical profession, as they come from perfectly impartial and well-informed persons. Many other of the most respectable practitioners and improvers of medicine have expressed the most earnest wishes for the execution of the design, as well as great anxiety for further information. And were there no other hope, those young men, to whom YOU communicate ardour and information, would, I am sure, prevent this chance of relieving otherwise irremediable misery from being lost to mankind. The sooner, however, its pretensions are examined, the better; in order either that the benefit may be disseminated, or, in case of total failure, that ingenuity may strike off in quest of other improvements. For who will deny that the art of medicine needs improvement, while so many and such frequent diseases remain incurable?

The *pneumatic* practice is about to be introduced into one hospital—another source of expectation. But an appropriated hospital, under the management of an able and impartial physician, would soonest try this practice, and improve it, if it be worthy of prosecution. Such an establishment, with rooms proper for containing modified airs, might be provided for a sum which, when set in competition with a small probability of greatly promoting the public welfare, must appear contemptible. And an individual, who from inexperience of the world, should suppose mankind in general, open to conviction and alive to their true interests, might imagine that the attention of the opulent would infallibly be arrested by considerations like the following:

"Some exterminating maladies infest, almost exclusively, "the habitations of the indigent. But Consumption does "not confine its ravages within such narrow limits. Nor "has wealth yet been able to provide materials for erecting "a barrier, capable of resisting its invasion. The young, "the beautiful, and ingenious are its ordinary prey—and how "often have you to lament that it fastens upon the objects "of your fondest attachment; after whose loss this busy "world will seem to you as a cheerless desert?—I am aware "of the interest which a child, consuming by a slow decay, "must excite in the bosom of a parent. Full allowance, "however, being made for the effect of compassionate affection on the imagination, it will often appear, that the most "amiable individuals of a family are really singled out by "Consumption.

"Self

" Self-preſervation comes in to ſecond the dictates of " parental affection; for it is certain that the number of " perſons, who die of conſumption at an advanced period of " life, infinitely exceeds the common computation.

" In compariſon with ſo unceaſing and diffuſive a calamity, " how inconſiderable are the effects of thoſe epidemical diſ- " orders, that occaſionally excite ſo much conſternation " among us? Why then heſitate to accept the aid of Science, " when ſhe offers agents endowed with *great* and *peculiar* " powers, advantageous in their application, and, as there is " ſome reaſon for ſuppoſing, adapted to our neceſſities? Is " a full trial of their efficacy too expenſive? At what rate " then do you eſtimate the chance of learning how to " preſerve from otherwiſe inevitable deſtruction thoſe whom " their underſtanding or diſpoſition may have rendered your " pride or your delight? How many times a larger ſum may " you have to beſtow without receiving in return any chance " of their preſervation?—But you have heard the project " vilified. So would a Panacea be. So was the Peruvian " bark; and Inoculation; and every great improvement of that " art, from which, according to its ſtate, all in their turn ſhall " experience good or harm. Beſides, are you ſure that thoſe " who paſs this ſentence are uninfluenced by prejudice, " pride, or the thirſt of gold? Recollect that to decry what " we do not underſtand is an obvious expedient of ſelf-love; " conſider therefore whether the information of theſe men " is ſuch as may enable them to judge from analogy, or " whether they ſpeak from actual experience: For opinion " can have no ſolid baſe but in analogy or experience, ſince " an intuitive perception of the powers of nature is not " among the faculties of man. Authority, equal to any that " can be oppoſed, is adduced in favour of the propoſal. " Many conſiderations concur to render it plauſible. The " few trials, hitherto made, have anſwered beyond expec- " tation. There is nothing, for example, in the authentic " records of medicine ſimilar to the caſe of *florid* conſump- " tion related in one of the following letters. The relapſes " ſerve but to render more evident the connection between " cauſe and effect. The ſame obſervation applies to the caſe " of putrid fever, related in another letter."———

I flatter myſelf that the art of medicine will find great reſources in OXYGENE or VITAL air. Its powers, as far as I have hitherto tried them, have exceeded my previous conceptions. But as every ſubſtance, worthy of being regarded as a medicine, muſt be capable of doing much miſchief when miſapplied, I am under ſome apprehenſion leſt miſmanagement ſhould bring this ſpecies of air into diſrepute.

Whenever

Whenever it is adminiſtered to perſons whoſe conſtitutions are not much reduced, nor their ſtrength much impaired, it ſhould at firſt be diluted with three times its bulk of atmoſpheric air; nor ſhould this mixture be inſpired above five minutes at a ſitting three or four times a-day. The ſubjoined caſe of epilepſy, in which its effects did not correſpond to our wiſhes, will ſerve to enforce this caution. Within theſe few days another confirmation of this rule has occurred to me: An aſthmatic patient, finding great relief from atmoſpheric mixed with oxygene air, unadviſedly, determined to attempt to ſubdue his diſeaſe at one attack. By largely uſing oxygene air little diluted, he brought on ſome ſingular ſymptoms, but, I hope, without doing himſelf permanent miſchief. I do not enter into further particulars at preſent, as I ſhall probably have an opportunity of laying this caſe before the public in the words of the patient himſelf.

In ſeveral experiments with animals that had reſpired diluted oxygene air, I have found them upon immerſion in water much more vivacious than ſimilar animals that had breathed atmoſpheric air. Of theſe experiments I intend ſoon to give an account, together with a drawing and deſcription of a chamber-apparatus for procuring and containing elaſtic fluids. In the mean time, it were to be wiſhed that a number of perſons would engage in this promiſing inveſtigation. It might perhaps be determined, whether phthiſical patients vitiate the air more than perſons in health?—whether aſthmatic patients, during a fit, vitiate it leſs, as Mr. Chaptal, I think, aſſerts?—An inſtrument for meaſuring the capacity of the lungs in different people might eaſily be contrived; and ſuch an inſtrument might poſſibly be uſeful as well as curious. But heads of inquiry will occur to any one who conſiders this copious ſubject.

" Suppoſing the proportion of ingredients in the atmoſ-
" phere to be that beſt adapted to the average ſtate of health,
" is it not likely that there may be certain deviations from
" this ſtate, where that fluid body contains too little vital air,
" and other deviations, where it contains too much?" Your encouragement of the inquiry, will, I hope, aſſiſt in furniſhing the ſolution of a problem, which is certainly one of the moſt important in phyſiology and pathology.

I am, dear Sir,

Your affectionate Friend,

Thomas Beddoes.

Hope-Square, Briſtol Hotwells,
Dec. 24, 1793.

CONTENTS.

OBSERVATIONS *on the Alteration produced in the* AIR *of* PLACES *where a* GREAT NUMBER *of Perſons are aſſembled.* By Mr. Lavoiſier.

THIS Paper is taken from the Memoirs of the Paris Society of Medicine, a Work that does not fall in the way of ordinary readers. It is valuable in itſelf, and intimately connected with the ſubject of Diſeaſes that may be cured, or relieved by breathing different airs. I am indebted for the tranſlation to the kindneſs of a Friend. The chemical terms are rendered conformable to the new French nomenclature, which did not exiſt in 1785, *when this paper was read; and the degrees of Fahrenheit's ſcale are ſubſtituted in the place of thoſe of Reaumur, but the weights and meaſures are French.* T. B.

MODERN Chemiſts have diſcovered that, beſides the common reſpirable air, there is in nature a variety of fluids which agree with it in its moſt obvious properties. Like the air of the atmoſphere, they are colourleſs, and ſo perfectly elaſtic, fluid, and tranſparent, that they would eſcape the Sight and Touch, if their reſiſtance and the poſſibility of confining them, did not in many inſtances convince us of their exiſtence. But though they bear a conſiderable reſemblance to common atmoſpheric air in their external, or what may be called their phyſical qualities, yet they are found to differ eſſentially when chemically examined; viewed in this light, ſome are diſcovered to be nothing more than the ordinary alkalis or acids in a ſtate of vapour; others are neutral ſubſtances of a very ſingular nature, and there are others again whoſe properties have not yet been aſcertained. Accurate and profound reſearches into the nature of aeriform fluids, have ſhewn that they are indebted for their elaſtic ſtate to the matter of heat, which enters into their compoſition: that all volatile ſubſtances whatſoever are ſuſceptible of *Evaporation*, and are tranſformed into a ſpecies of air by a certain quantity of heat: that the upper ſurface of the mercury in the barometer, for example, being at its mean height, (or about 28 Paris inches above that of the mercury in the baſon) water aſſumes an aeriform ſtate at the temperature of 212° degrees of Fahrenheit's thermometer; and Spirit of Wine at that of 75°: that theſe fluids, thus rendered elaſtic and aeriform, are capable of being confined under glaſs bells, or other receivers: that they may be transferred from one veſſel to another, and

ſubjected to all the experiments that can be made on permanently elaſtic fluids. This aeriform ſtate, or that of an elaſtic fluid, is nothing therefore but a modification, and the words *air* or *gas* are mere generic expreſſions characteriſing a certain claſs of bodies, but not appropriated to any particular ſpecies. The common air of the atmoſphere is conſequently only an individual of this numerous claſs.

Theſe general conſiderations might induce us to conſider the atmoſphere not as a ſimple but as a compound ſubſtance: it may be a mixture of all the various ſubſtances capable of aſſuming the ſtate of air at the degree of heat, and under the preſſure in which we live. Experience has confirmed this conjecture which was ſuggeſted by analogy. Chemiſts having ventured to analyſe the air of our atmoſphere, they have ſucceeded in diſcovering that it conſiſts of about 27 or 28 parts in 100 of an air perfectly fit for the purpoſe of reſpiration, and now known by the name of oxygene air, and of 72 or 73 parts of a mephitic fluid, abſolutely incapable of ſupporting the combuſtion of Bodies, or the reſpiration of Animals, which has lately been denominated azotic air.

In the proportion juſt mentioned, of 72 parts of azotic air, to 28 of oxygene air, the number of cubic inches occupied by each in a cubic foot of the common atmoſphere, is found to be as follows:

Oxygene air, - -	484	inches,
Azotic air, - -	1244	
Total	1728	inches = cubic foot.

I have found by a number of experiments, of which I ſhall hereafter give an account, that when the barometer is at the heighth of 28 Paris inches, that is to ſay, at its mean heighth, and the thermometer at 52 degrees, the cubic foot of atmoſpheric air weighs - - - - - oz. 1, gros. 3, grs. 3

The weight of a cubic foot of oxygene air is	1	4	0
And of a cubic foot of azotic air - -	1	2	48

Hence it follows that a cubic foot is compoſed

	Inches.		grs.	grs.
Of Oxygene air, - -	484	weighing	3	26
Azotic, - - -	1244		7	49
Total	1728		1	3 3

Amongſt the different ſubſtances of which the atmoſphere is compoſed, none beſides oxygene air is eſſential to reſpiration: the azotic air contributes nothing towards it: ſo that, in fact, any other mephitic fluid might be ſubſtituted in its place; and, provided this ſubſtituted fluid poſſeſſes no irritating or deleterious quality, and is combined with oxygene air in the proportion

proportion of 72 parts in 100, ſuch a mixture would conſtitute a fluid equally ſalutary, and reſpirable with the common air of the atmoſphere.

Such is the knowledge of the compoſition of the air we breathe, which the Science of Medicine has derived from Natural Philoſophy and Chemiſtry.

But what are the changes produced in air thus formed in the various circumſtances of Life? what the influence of theſe circumſtances on the organs of reſpiration? what diſeaſes in the Animal Œconomy may hence ariſe? and what are the methods of preventing or remedying them? To anſwer theſe queſtions is the object of my preſent undertaking; and of theſe I ſhall give an account to the Society from time to time in different papers.

It is a fact which has been long known, that reſpiring animals live only for a given time in a given quantity of atmoſpheric air; they ſoon become faint, and ſink into a kind of ſlumber: this ſlumber, though compoſed at firſt, is ſucceeded by great agitation: the reſpiration becomes quick and difficult; and the animal expires in convulſions. Theſe events ſucceed each other with greater or leſs rapidity in proportion to the quantity of air in which the animal is confined, and in proportion to its general bulk, and to the comparative ſize of its lungs: The vigour of any given animal may likewiſe contribute ſomewhat to prolong its exiſtence for a ſhort period, but in general it may be conſidered as an eſtabliſhed fact, that a man cannot ſubſiſt longer than an hour in a quantity of air equal in bulk to five cubic feet.

In order to obtain an adequate idea of the ſpecies of injury which the air ſuſtains by being reſpired, I introduced a Guinea-pig under a glaſs bell inverted upon mercury, which contained 248 cubic inches of oxygene air. I ſuffered the animal to remain in theſe circumſtances about an hour and a half; at the end of which time, I removed it, by the ſame way in which it had been introduced, by paſſing it through the mercury. I did not perceive, that in either of its paſſages it had been in the leaſt injured.

In order to facilitate our future reaſonings, I ſhall ſuppoſe that the quantity of oxygene air in which the Guinea-pig was confined, amounted to a cubic foot, or to 1728 cubic inches, and I ſhall reduce by calculation all the reſults of my experiments to this ſtandard.

When the Guinea-pig was withdrawn from under the bell, the 1728 cubic inches of oxygene air were found to be reduced to $1672\frac{3}{4}$; the diminution of bulk was conſequently $55\frac{1}{4}$ cubical inches; in the mean time there were formed $229\frac{1}{2}$ cubic inches of carbonic acid air. Of this fact I ſatis-

fied myſelf by introducing a quantity of cauſtic alkali into the bell; the air remaining after this operation was perfectly pure oxygene air.

Conſidering theſe portions of air with reſpect to their weight, we ſhall have for the quantities remaining under the bell after the animal had been withdrawn, the following proportions:

	oz.	grs.	grs.
Oxygene air, - -	1	2	$1\frac{3}{4}$
Carbonic acid air, -	0	2	15
	1	4	$16\frac{3}{4}$

In this experiment the air appears to be diminiſhed in bulk about 1-32d part: but its abſolute weight augmented: hence it evidently reſults, 1ſt. That the air derives from the lungs during the act of reſpiration, a portion of carbonic acid air: But it muſt be remarked that this augmentation of weight which appears to be only 21,87, is in reality much more conſiderable than it appears to be at firſt ſight. The experiment which I have juſt related, produced no more than $229\frac{1}{4}$ inches of carbonic acid air; now according to very exact experiments which I have deſcribed elſewhere, 100 parts of carbonic acid air in weight are compoſed of 72 parts of oxygene air, and 28 of charcoal. The $229\frac{1}{2}$ inches of carbonic acid air obtained in this experiment contained therefore of

	grains.
Oxygene air, - -	114,84
Carbone, - -	44,66

The 114,84 grains of oxygene air amount in cubic inches to $229\frac{1}{2}$ inches; if then no more oxygene air had been employed than was neceſſary to form the carbonic acid air, the quantity remaining after the operation ſhould have been $1728-229\frac{1}{2}=1498\frac{1}{3}$; it was only 1443 2-3ds. the deficiency is=54 2-3ds.

It is evident from this ſtatement, that independantly of the portion of oxygene air which has been converted into carbonic acid air, another portion of that which has entered the lungs has not returned in an elaſtic ſtate; and it follows that one of theſe two effects takes place during the act of reſpiration; either that a portion of oxygene air is united with the blood, or that it is combined with a portion of inflammable air, and compoſes water. I ſhall diſcuſs in other papers the reaſons which may be adduced in favour of each of theſe opinions. But allowing, (which there is ſome reaſon to do) that the latter is the preferable ſuppoſition, it is eaſy, from the above experiment, to determine the quantity of water which is formed during reſpiration, and to aſcertain the quantity of hydrogene extracted from the lungs.

In fact,

In fact, since to produce 100 parts of water it is necessary to employ 85 parts by weight of oxygene air, and 15 of hydrogene gas, it follows that the 54 2-3ds. inches of oxygene air which have not been accounted for, must have formed $32\frac{1}{4}$ grains of water, and that 4 5-6ths. grains of inflammable air have been disengaged from the lungs of the animal. The same experiment repeated in common air, affords similar results: a diminution of the bulk of the air: an augmentation of its absolute weight: a formation of carbonic acid air, and of water: a disengagement of carbone, and of a small portion of inflammable gas from the lungs: but the azotic air which remains, and which mixes with the carbonic acid air, and with the portion of oxygene air not entirely consumed, renders the result more complicated. At the time therefore, that the respiration of the atmospheric air has been continued as long as may be, and animals can no longer remain in it, except at the risque of losing their lives within a few seconds, it is found to be composed of nearly the following proportion in each cubic foot, I say nearly, for great variations are observable in these circumstances, and particularly in the quantity of carbonic acid air. A cubic foot contains therefore in these circumstances,

Of oxygene air, - -	173 inches.
Carbonic acid air, -	200
Azotic air, - -	1355
Total	1728

Which gives in weight,

	oz.	grs.	grs.
Oxygene air, - -	0	1	14
Carbonic acid air, -	0	1	66
Azotic air, - -	1	0	26
Total	1	3	34

I ought to take notice, that these results were determined by means of respired air after it had been cooled, and had deposited the superabundant humidity which it had acquired in passing through the lungs. Air thus exhausted by respiration, proves that the limits within which it is possible to vary the proportions of oxygene and azotic, in order to produce respirable air, are not very extensive, and that consequently it is no wonder that the air should be found sensibly injured in a great variety of circumstances. In the experiment made upon the Guinea-pig confined in oxygene air, which I have just related, I perceived that the animal suffered considerably towards the conclusion. It is however evident, that in this case a very small portion only

was abſolutely vitiated, that is, converted into carbonic acid air, and that there remained of oxygene air a quantity much more than was neceſſary to conſtitute a ſalubrious air. This circumſtance had been already obſerved by Dr. Prieſtley, but the object which I propoſe in this paper, required a repetition of a part of his experiments. My operations were generally performed upon Guinea-pigs. The oxygene air which I made them breathe was nearly pure; and did not contain above five or ſix parts of azotic air in 100 of the whole portion. Now though theſe animals lived much longer in a certain quantity of this air than they would have done in an equal quantity of the air of the atmoſphere, they periſhed long before it was completely vitiated, while another animal of the ſame kind introduced into this vitiated air did not appear, for ſome time at leaſt, to ſuffer any conſiderable inconvenience. It was not therefore for want of reſpirable air that the animals periſhed; it was rather owing to ſome pernicious quality in the oxygene air, a proof that the admixture of a certain portion of azote with oxygene is required to render it ſalubrious. M. Bucquet, whoſe name at this moment muſt renew the public regret, aſſiſted me very kindly in ſome of theſe experiments, and we opened together the animals which had fallen victims to our reſearches; they all appeared to have died of a burning fever or ſome inflammatory diſeaſe. Their muſcles, upon inſpection, were found to be very red; the heart livid and full of blood; eſpecially the right auricle and ventricle: the lungs were but little inflated, but were red even externally, and gorged with blood. A truly ſalubrious air therefore is compoſed of an adequate mixture of oxygene and azote: and it is of conſequence to reſpiring animals, that this proportion, which is commonly 28 parts of oxygene to 72 of azote, ſhould never vary in any conſiderable degree. This difference however is obſervable, that when the oxygene is ſuperabundant, the animal only ſuffers ſeverely: when it is deficient, the conequence is immediate death. Since, therefore atmoſpheric air ſupports life for a certain period only, and ſince it becomes the more vitiated the oftener it is reſpired, we may venture to conclude that the wholeſomeneſs of the air muſt be more or leſs diminiſhed in all public places, in hoſpitals, and wherever a number of perſons are aſſembled; eſpecially if the air circulates ſlowly or with difficulty. I thought it of ſome conſequence to determine to what extent this vitiation could poſſibly be carried, for which purpoſe I choſe the loweſt ward in the General Hoſpital, which appeared to me more crouded and unhealthy than the reſt. I went thither at day-break, I was admitted the inſtant the door was opened,

and

and filled two phials with the air of the room; one I filled from the lower part of the room nearly on a level with the floor, and the other from the upper part, or as near as poſſible to the cieling. The former of theſe two portions of air, or that which was taken from below, was but little vitiated; it contained in two portions, in bulk,

Of Oxygene air,	- -	25
Carbonic acid air,	-	4
Azotic air,	- -	71
		100 Parts.

The air taken from the top of this ward had ſuffered much greater injury. It contained,

Of Oxygene air,	- -	$18\frac{1}{2}$
Carbonic acid air,	-	$2\frac{1}{2}$
Azotic air,	- -	79
		100

Atmoſpheric air taken the ſame day in the open air contained,

Of Oxygene air,	-	27
Azotic air,	-	73
		100

I attempted the ſame experiments on the air of a theatre. The French comedians were at that time in the palace of the Thuilleries, and I performed my operations in that building. I choſe a day in which the number of ſpectators was unuſually great, and taking with me two phials full of water, I emptied one at the top of the theatre, in a box which had been kept ſhut during the whole of the performance, and the other at the bottom of the pit, a few moments before the concluſion of the play. It is eaſily conceived, that this ſecond part of my operation was attended with ſome trouble and difficulty: the leaſt appearance of any thing extraordinary, would have occaſioned diſturbance in the pit, and might have put a ſtop to the performance. I was obliged therefore to be ſatisfied with coming in gently a few moments before the end of the play, and placing myſelf near the centinel, whom I had informed of my ſcheme, emptying my phial in that aukward ſituation. But the air which I thus obtained was taken too near the door, and the water through which it paſſed in order to enter the phial, muſt have abſorbed a portion of its carbonic acid air. On this account, the experiment did not give me any reſults ſenſibly different from thoſe made with the external air; but this was not the caſe with the air collected at the top of the theatre. In 100 parts of this air there were found

Of

Of Oxygene air, - -	21
Carbonic acid air, -	$2\frac{1}{2}$
Azotic air, - -	$76\frac{1}{2}$
Total	100

Whence it is evident that the quantity of oxygene air had been diminiſhed in the proportion of 27 to 21, or nearly one fourth. It is to be wiſhed that theſe experiments could be repeated more at large and with a more convenient apparatus. The waſhing of the air, at the time of collecting it, ſhould be above all things avoided. This might eaſily be effected by means of tin pipes communicating from the outer to the inner parts of the building, to whoſe extremities ſhould have been previouſly fitted balloons exhauſted by the air-pump.

In this manner it would be eaſy to procure a quantity of air ſufficient to determine its ſpecific gravity: the experiments might alſo be conducted on ſo large a ſcale as to render even minute differences very ſenſible; and they might be repeated a ſufficient number of times to render the inaccuracies which in all delicate experiments are unavoidable, nearly evaneſcent, and make them compenſate one another. Such experiments cannot be well carried on except under the ſanction of Government; but undoubtedly we ſhould derive from them valuable information with reſpect to the conſtruction of theatres, hoſpitals, and every other building, in which people aſſemble in great numbers.

However imperfect my experiments may be, we may collect by comparing them with others made on a ſmaller ſcale, under glaſs veſſels, that the air of the atmoſphere which is originally compoſed of only two fluids, or very nearly ſo, is compoſed of three in all places which contain numerous aſſemblies; in conſequence of the converſion of a part of the oxygene air into carbonic acid air: that theſe three fluids are not mingled in equal proportions in every part of the room, but on the contrary tend to arrange themſelves according to their ſpecific gravities: that the azotic air, as being lighter and favoured by the heat which expands it, naturally mounts upwards; and thus a ſpecies of circulation is produced which ſupplies the place of the mephitic air, which eſcapes at the top, with freſh air flowing in from the lower avenues.

This circulation takes place more or leſs in every theatre; and frequently in ſpite of the architect who directed the conſtruction: unleſs this was the caſe, unleſs the air was thus renewed, the ſpectators would be expoſed to the moſt fatal accidents long before the concluſion of the performance. To convince ourſelves of this truth, nothing more is neceſſary than to take the example of a theatre, ſuppoſe of 30 feet

long,

long, 25 feet wide, and 30 feet high. A room of theſe dimenſions would be equal in bulk to 22,500 cubic feet, and might contain about 100 ſpectators: now ſince each perſon conſumes, as I have mentioned above, about five cubic feet in an hour, it follows that the air of the theatre (if it were not renewed) would be rendered completely mephitic in four hours and a half: and it is likewiſe probable, that the greater part of the ſpectators would be ſeriouſly incommoded or even periſh before the end of that period.

The ſame calculations applied to low and cloſe places of reſort, of which I could mention many inſtances, will explain how it happens that on crouded days the attention of the audience cannot in ſuch places be kept awake above two or three hours, where a mechanical impatience is brought on by a certain uneaſineſs and phyſical anxiety, of which it is difficult to diſcover the cauſe. In ſuch circumſtances unfortunate is the reader to whom have been allotted the laſt moments of the ſitting; an intereſt in his ſubject is no longer communicable to his audience: he is no longer liſtened to with complacency, or even with attention: and he receives none of thoſe tributes of applauſe or gratitude, which in more favourable circumſtances he had a right to expect.

When I began the preſent paper, my intention was to have given ſome account of the various ſpecies of injury which the air is capable of receiving in the ordinary circumſtances of life. But I perceive that I have as yet done no more than ſketch one point in the plan which I had adopted, and am obliged to refer to a ſecond diſſertation the remarks I have to make on the vitiation of the air produced by the burning of lamps, wax, tapers, candles, coal, by freſh plaſter, oil-painting, &c. but as this part of my work is nearly finiſhed, I ſhall ſoon have it in my power to preſent it to the Society.

There will remain to be treated of in a third paper, atmoſpheric air conſidered not as an elaſtic fluid ſuſceptible of decompoſition, but as a chemical agent capable of taking up, in the way of ſolution, miaſmata of various kinds. It is ſomewhat alarming to conſider how often in a large aſſembly, the air which each individual breathes, has paſſed either wholly or partly through the lungs of all thoſe who are preſent. It muſt take up in each caſe exhalations more or leſs putrid. But of what nature theſe exhalations are: to what degree they vary in different ſubjects: in age or youth: in health or ſickneſs: what diſeaſes we are capable of receiving by this mode of communication: and what precautions may be employed to neutraliſe or deſtroy the dangerous influence of theſe miaſmata—there are none of theſe ſubjects which may not afford ground of inquiry, and ſurely there are none

of more importance to the human race. While every art is advancing rapidly towards perfection; the art of living with comfort in ſociety, of preſerving in health and vigour perſons obliged to meet in large aſſemblies, of rendering cities and great towns healthier, and the communication of contagious diſorders leſs general, is unfortunately yet in its infancy.

The immenſe labour which might be founded on this important object, muſt be undertaken by Societies of learned men only; no individual can flatter himſelf that he poſſeſſes knowledge ſufficient to complete without aſſiſtance, a plan ſo complicated and extenſive; and it is from reliance on the advice, the information, and the aſſiſtance of this Society, that I have now undertaken to cultivate ſome few portions of this immenſe field. §

§ Mr. L. has, I believe, publiſhed nothing further on this important ſubject: And his incomparable talents are, I fear, now loſt to Science and Humanity. T. B.

LETTER

From Dr. WITHERING

To Dr. BEDDOES.

DEAR SIR,

THE deſign you have conceived is an important one; the philanthropiſt cannot but be intereſted in its ſucceſs; the phyſician muſt rejoice at the probability of learning how to cure or eſſentially alleviate a frequent, a cruel, and an hitherto hopeleſs diſeaſe; philoſophers will urge you to proceed, from a conviction that ſhould you fail in your higher aims, you muſt extend the boundaries of ſcience, and throw new light on the laws of the living machine; and ſhould your endeavours ultimately be crowned with ſucceſs, and the moſt amiable, not to ſay the moſt beautiful individuals of our ſpecies, be thus ſnatched from a premature fate, numerous private families will be indebted to you for their greateſt comforts, and Society at large for its brighteſt ornaments. It is from ſuch conſiderations as theſe, as well as at your particular deſire, that I now am about to communicate to you ſuch obſervations, as have occurred during many years' attention to the phthiſis pulmonalis; but I muſt con-

fine

fine myſelf to thoſe circumſtances which more immediately tend to ſupport or to invalidate your opinions, otherwiſe I ſhould write a volume inſtead of a letter.

Catarrhs, cauſes of Conſumption.

Young people themſelves, as well as thoſe who have the direction of them, cannot have it too ſtrongly enforced upon their minds, that a *cough* merely the conſequence of a cold, ceaſes of itſelf in eight or ten days; that if it continue beyond that period, there is danger that a conſumption may be the conſequence. Bleeding, ſpare diet and the other uſual modes of obviating inflammation ſhould be immediately purſued untill the cough ſhall entirely ceaſe; and particularly bleeding by leeches, or cupping on the part where any pain ſhall be felt in the cheſt.

Cauſes of Catarrhs. (Obſervations p. 156, &c.)

A ſudden change from cold external air to that in a heated room, is certainly a much more frequent cauſe of inflammatory affections of the lungs, &c. than has hitherto been generally ſuppoſed; it is I believe the moſt general cauſe, but ſurely it is not the only cauſe of taking cold! I am perſuaded that a ſudden tranſition from a warm to a cold apartment or to a ſtream of cold air, will produce this effect. We do get colds in Summer when no fires are lighted in our ſitting rooms, though not ſo frequently as in Winter. Horſes and cows get colds, though they never experience much ſudden change from cold to hot in the temperature of the air they breathe, whilſt the dog, who from the temperature of the coldeſt ſeaſons inſtantly on entering the houſe, lies down cloſe the hotteſt fires, and vice verſa, ſeems little if at all liable to catarrhal affections. Horſes ſometimes die conſumptive, cows often; dogs I believe never.†

Kinds of Conſumptions. (Obſervations p. 112.)

The different kinds of conſumptions ſhould be better diſtinguiſhed than they have been; not only as influencing prognoſis, but as directing to a more ſucceſsful practice. I agree with you that patients have reaped no advantage from the prevalent idea that moſt conſumptions have a ſcrophulous origin. One ſpecies you have happily named the *florid*, and it is readily diſtinguiſhed. There exiſts alſo a *truly ſcrophulous* conſumption, but it is a rare, and not an incurable diſeaſe, if the treatment be properly adjuſted to its nature; but the treatment which I have repeatedly found ſucceſsful here, would only haſten the florid conſumption to

† I do not here forget the epidemical contagious catarrh or influenza, to which Dogs are ſubject. W. W.

its fatal termination ‡. When the ſcrophulous conſumption cannot be traced by any known family diſeaſe; or by the more obvious ſymptoms in the conſtitution of the patient, it may ſometimes be aſcertained by knotted cords of lymphatics running down the neck, and dipping under the clavicle into the cheſt.

Subſtance of the Lungs deſtroyed. (Obſervations p. 146, letter p. 25.)

The exiſtence of this fact is not diſputed, but it muſt be a very rare occurrence. Would not the falling in of the ribs in the caſe you mention, be equally explicable on the ſuppoſition of a caries induced in their bony ſubſtance from a diſeaſe in the pleura? Or might not the increaſing debility &c. of the patient be alone ſufficient to produce ſuch an appearance, which often occurs in ricketty children? At one period of my life I had opportunities of accurately examining the Lungs of many who died conſumptive, but I never met with any thing like the deſtruction of them. It was once my intention to have given theſe obſervations to the public, but the utility of that deſign was fully anſwered by the publication of the works of the late Dr. Stark, becauſe nothing that I had obſerved, had eſcaped the attentive ſearches of that ingenious and indefatigable man, by whoſe early death ſcience was deprived of one of its moſt active votaries.

Who exempt from Conſumptions.

It is a prevalent opinion that the workmen employed about Limekilns never become conſumptive; and it is uſual for the affected with the diſeaſe, to repair to ignited kilns to breathe

‡ Medical practitioners will probably be ſurpriſed at this paſſage. Having had an opportunity of converſing with Dr. Withering ſince his letter was written, I requeſted an explanation. He informed me that the practice he had found ſucceſsful in what he conſiders as the truly ſcrophulous Phthiſis, is peculiar to himſelf: he mentioned to me what it was, but deſired I would not anticipate the account of it he himſelf deſigns to publiſh.

Not many weeks before his ſudden death, the late Mr. Benjamin Colbourne, of Bath, told me that he had diſcovered a medicine which he had reaſon to believe not leſs efficacious in certain diſeaſes of the urinary paſſages than his aerated alkaline water is in calculus. The patients whom he had treated complained of difficulty of retention of urine, which often came away involuntarily in ſmall quantities with a ſenſe of irritation in the urethra, and was fœtid and *alkaline*, as he ſhewed me by dipping into ſome of it paper tinged by litmus, and afterwards reddened by an acid. The effect of his medicine was to remove the above-mentioned diſtreſſing ſymptoms, and to change the quality of the urine, as he alſo ſhewed me by the effect of the urine of another patient who had been under his care for ſome time, upon teſt paper. The urine of this patient was diſtinctly acid, and not more offenſive than ordinary urine.—His deſign was to try his method in a few more caſes, and, if he was ſucceſsful, to publiſh it. He had never communicated it; I am told that no account of it has been found among his papers. I hope however that further ſearch will diſcover his preparation; otherwiſe the loſs to humanity will be truly deplorable. T. B.

breathe the vapour iſſuing therefrom. This rude mode of adminiſtering atmoſpheric air deprived of part of its oxygene and combined with a portion of carbonic acid, has not under my obſervation ever cured a patient, but ſtill I am diſpoſed to believe that opinions generally prevalent have ſome ſort of foundation. In looking about for the cauſes which promote or retard the frequency of conſumptions, different ſituations and occupations become of courſe objects of my attention; and the only claſſes of men I have yet obſerved exempt from the diſeaſe, are butchers*, and makers of catgut. They both paſs much of their time amidſt the ſtench of dead animal matters, the latter very much ſo; the former live chiefly on animal food, and are much expoſed to the inclemencies of the ſeaſons, whilſt the latter live as other manufacturers, and work under cover, in cloſe and rather warm buildings. Theſe people are always ſleek, often fat, and the roſy bloom of health adorns their cheeks. Theſe facts but ill accord with our theoretical notions of putrid diſeaſes.

Progreſs of Conſumptions ſtopped. (Obſ. p. 113.)

The effect of pregnancy in arreſting the progreſs of conſumption has long been known, but it was reſerved for you, Sir, to turn this remarkable fact to advantage. Should your idea concerning the effect of the impeded action of the diaphragm ſtand its ground, the application of a compreſſive bandage upon the abdomen cannot fail to preſent itſelf to your imagination.

But the progreſs of conſumption is alſo ſtopped by inſanity. This is a circumſtance well worthy your attention: A young woman in the laſt ſtage of phthiſis ſuddenly became furiouſly inſane. After three months the inſanity ceaſed, the phthiſical ſymptoms returned, and ſhe died in a few weeks.

A young gentleman whoſe father died conſumptive, conſulted me about a troubleſome cough, pain in his cheſt, hectic fever and emaciation. I had no expectation of his living, but wiſhed him to winter in a warmer climate: on his return the following ſpring, the phthiſical ſymptoms had no exiſtence, but there was an unuſual oddity in his manner, which very ſhortly ſhewed itſelf in a confirmed inſanity. For ſeveral weeks he was furious, but that ſtate gradually gave way to an abſtracted melancholic caſt: after ſome years he grew more comfortable, and ſo continues, but is far from well.¶

* Nevertheleſs, I have at preſent under my care a Butcher from Wrington, in Somerſetſhire, who has been for ſome time in a true Conſumption. He is much relieved and entertains ſanguine hopes of recovery. T. B.

¶ I was apprized of the ſuſpenſion of Phthiſis by inſanity. It is noticed in Cullen's *Firſt Lines*. I have myſelf mentioned a caſe where the pneumonic ſymptoms and quick pulſe of Phthiſis were ſuſpended by anaſarca. Dr. Percival

Diet of the Conſumptive.

Every body has ſeen the inefficacy of milk, fruit, and a vegetable diet, with more or leſs abſtinence from fermented liquors. For three ſucceſſive years the oppoſite method was purſued in a great number of caſes, by a practitioner within the ſphere of my obſervation. His patients were ſupported upon animal food, ſtrong gravy broth, and porter or port wine. According to the moſt candid judgment I could form, theſe great variations in diet had no eſſential effect upon the diſeaſe; and then it was that my hopes of finding a cure for it firſt forſook me—but you, Sir, have revived theſe hopes.

Vitriolic acid. (Obſ. p. 135.)

In the florid conſumption, and in hæmoptoe, the uſe of this acid has been generally approved; but I think its effects are very problematical. The patients generally like the medicine at firſt, but I have repeatedly obſerved that in a few days, it has occaſioned an increaſe of oppreſſion, a ſtraighter cough, more heat, and if perſiſted in, an hæmoptoe, though none had appeared before.—This alſo favours your theory.‖

Carbonic acid Air. (Obſ. p. 128.)

In the caſe which I ſaw perfectly cured by means of this air, and which I communicated to Dr. Percival many years ago, the expectorated matter was very copious, and very offenſive. It was with a view to correct this fœtor, and by that means to diminiſh the hectic fever, that I thought of directing its uſe. I found the patient in the ſtate juſt now mentioned, and had the ſatisfaction of ſeeing her cured. Further and more mature obſervation has long ſince convinced me, that this was a caſe of vomica, and not a true phthiſis. Accordingly I took the firſt fair opportunity of confeſſing my error, as may be ſeen in the appendix to my account of the foxglore. But though my hopes founded on this firſt trial proved deceptive, I am ſtill very much deceived if the inſpiration of carbonic acid air has not greatly prolonged the exiſtence of many truly phthiſical patients. My mode of uſing it is more effectual than you may have ſuppoſed. I order the patient to ſleep in a ſmall room, take care to have the chimney on one ſide of the bed, and place an earthen veſſel which will contain two or three gallons on the

tival relates a caſe where it might at firſt ſight appear that *Phthiſis* was relieved or removed by *hydrocephalus internus*; but a careful peruſal of the narrative of this eminent phyſician will, I think, ſatisfy the reader that the ſymptoms of the former diſeaſe only yielded as the powers of the ſenſorium were gradually deſtroyed. (Med. Facts and Obſ. I. 131.) T. B.

‖ Dr. Withering has ſince informed me of a caſe where Spitting of Blood and Stricture of the Cheſt repeatedly ſucceeded the uſe of this acid. His obſervation, if well founded, is of the greateſt importance in practice. T. B.

the oppoſite ſide, on a level with the pillow. Things are ſo managed, that the efferveſcence goes on ſlowly, and continues for great part of the night; the vapour as it riſes paſſing over the patient's bed. If the ſick are ſo ill as to be confined to the houſe, the ſame proceſs goes on through the day. You will probably find other aeriform fluids better adapted to the cure of the diſeaſe, but I think you will obſerve good effects from this, particularly when the expectorated matter is fœtid.

Vapour of Gums and Roſins.

Many people are perſuaded that conſumptive patients have found good effects from inhaling the vapour of reſinous or gummy reſinous ſubſtances. The powder of theſe ſubſtances is directed to be ſprinkled upon a fire in a chafing-diſh, and the patient inhales the vapour as it riſes. But here a queſtion preſents itſelf, whether the benefit ſhould be attributed to the vapour of the medicine, or to that of the burning charcoal? Japanners are conſtantly breathing the vapours of reſinous ſubſtances, but I never could obſerve that they were more or leſs ſubject to phthiſis than others; caſters of fine braſs work very often die conſumptive, much more ſo than any other ſet of artiſts in Birmingham. They duſt their moulds with powdered roſin, the vapour of which riſes copiouſly when the melted metal is poured in. But the miſchief can hardly be attributed to this vapour, otherwiſe the Japanners would be affected; nor yet to the flowers of zinc, which are copiouſly diffuſed through the work-ſhops, becauſe the caſters of large braſs work are not peculiarly liable to become conſumptive. I ſuppoſe the Phthiſis in theſe inſtances to be cauſed by the mechanical action of the powdery matters which float in the air in great quantities in theſe fine caſting ſhops, and are neceſſarily taken in with the breath. Whilſt flints for the potteries were pounded in mortars, the people ſo employed univerſally died conſumptive, and the grinders of needles now often experience the ſame fate. **

Effects of Diet on Reſpiration. (Letter, p. 12.)

The experiments you wiſh for on this ſubject have in part been made. The late Mr. Spalding, who did ſo much in improving and uſing the diving-bell, was a man of nice

 obſervation,

** Linnæus, or Ullholm, mentions a very curious experiment on the penetrating quality of this powder. *Quanto vitio pulvis lapidoſus pectus oneret, apud Orſenſes Dalecarliæ videre licet, qui ex teneriori lapide arenaceo cotes ſuas rotatiles ſecant, et ante annum tricſiemum Phthiſici plerumque moriuntur. Quin et lapicidæ Stockholmienſes, tantum non omnes, aut calculo pulmonum, aut phthiſi, aut hæmoptyſi enecantur; quanquam pulvis ille tenuis lapidoſus adeo penetrabilis eſt et volatilis, ut veſica urinaria, inflata ſuſpenſaque in officinis illorum poſt exactum annum, aliquot ſcrupulos pulveris tenuis lapidoſi intus continere deprehendatur. Amoen. Acad.* viii. 159. T. B.

obſervation, and had he not fallen a ſacrifice to the negligence of drunken attendants, would have thrown much additional light upon more than one branch of ſcience. He particularly informed me, that when he had eaten animal food, or drank fermented liquors, he conſumed the air in the bell much faſter than when he lived upon vegetables, and drank only water.†† Many repeated trials had ſo convinced him of this, that he conſtantly abſtained from the former diet whilſt engaged in diving.

Carbonic Matter. (Letter, p. 68.)

Its effects upon living animals are yet but little known. Many people mix it with the food of their poultry, and think it contributes to fatten them. This is much in favour of your opinion that it does abſorb oxygene in the heat of the animal ſtomach.——

Theſe, Sir, are ſuch remarks as occurred to my mind on reading your obſervations on conſumption, and reflecting on the many ingenious ideas you have ſuggeſted. I ſhall be happy if they can be made ſubſervient to the great cauſe in which you have engaged. An individual ſo occupied has a right to claim every aſſiſtance which his brethren can afford him: mankind have much to gain, and nothing to loſe by ſuch enquiries.

I remain, dear Sir, your's,

Wm. Withering.

To
Dr. Beddoes.

P. S. In conſtructing the apparatus I have no doubt but you will contrive to ballance the air-veſſel inverted in water, ſo that the patients may inſpire perfectly at eaſe. The reſiſtance given to inſpiration by the column of water, low as it is, in Mudge's inhaler, is ſo great that even a healthy perſon cannot long perſevere in breathing through it, and I have never ſeen a patient uſe it ſo as to draw the air through the water as its author intended ſhould be done.

†† I had inferred that "the faculty of living in air of a reduced ſtandard is "impaired by the influence of ſpirituous ſtimulants." This obſervation of Mr. Spalding, which ſeems equally new and intereſting, adds much probability to the opinion. I intend to aſcertain whether it be true or not, by direct experiments on animals. T. B.

LETTER

LETTER

From Doctor EWART

To Dr. BEDDOES.

DEAR SIR, BATH, *November* 14, 1793.

I CAN have no objection whatever to comply with your requeſt of ſtating to you in writing ſuch of the particulars, as my memory diſtinctly retains, concerning the two caſes of Phthiſis Pulmonalis, in which I have employed the inhalation of mephitic air, with ſeeming advantage; and I give you leave to make what uſe of them you pleaſe. I am ſorry however, that not having kept a regular journal of the caſes alluded to, I muſt now confine my obſervations to general circumſtances, and to their general reſult.

I accompanied the late Hon. Col. Cathcart, when he ſailed from England in the year 1787, on an embaſſy deſtined for China. This Gentleman had from his infancy been ſubject to frequent and alarming pulmonary complaints; and at the period above-mentioned, being then 28 or 29 years old, he was threatened by ſuch ſerious ſymptoms of Phthiſis, that little hope was entertained of his recovery but from the effects of a ſea voyage to a warmer climate. There was ſome proſpect of this hope being realized, during the firſt part of the voyage; but after paſſing the Cape of Good Hope we were forced into a high Southern latitude where the cold was intenſe, and in which all the former ſymptoms of Phthiſis returned upon him with redoubled violence. An almoſt inceſſant cough, a copious expectoration of matter, judged both by its appearance and ſmell to be of a purulent nature, and mixed occaſionally with ſtreaks of blood, a fixed pain in the breaſt affecting his breathing, together with a rapid emaciation and hectic fever, left no doubt of the confirmed and dangerous form of the diſeaſe. All the common remedies were employed to moderate theſe ſymptoms, with little or no benefit.

I thought myſelf juſtified in having recourſe to any means, recommended by experience though unuſed in general practice, that offered a poſſible chance of relief in a caſe ſo deſperate; and I therefore determined, "without being enlightened, I confeſs, by the grateful dawn of any probable theory" on the ſubject, to propoſe the inſpiring of mephitic air, as mentioned on very reſpectable authority, in an appen-

dix to one of Dr. Prieſtley's volumes on air, to have been tried in ſimilar caſes, with ſome degree of ſucceſs.

It was impoſſible to conſtruct at ſea ſuch an apparatus as might have been wiſhed, for the purpoſe of determining accurately the proportions of mephitic and atmoſpheric airs uſed in the experiment. Having however on board one of Dr. Nooth's glaſs machines for impregnating water with fixed air, I removed the upper part of it, as of no uſe for my purpoſe, and inſerted a flexible tube, which I happened to have in my poſſeſſion, through a cork, fitted to the ſuperior orifice of the middle chamber of the machine, through which tube I meant my patient to inhale mephitic air. I filled this chamber of the machine nearly one-third full of pure water, with the view of arreſting any particles of marble or vitriolic acid (the ingredients I uſed to obtain fixed air) which might be carried up along with the air from the lower chamber.

After the mephitic air had continued to aſcend through the water, till I could perceive its peculiar odour iſſuing from the extremity of the flexible tube, I allowed my patient to take a full inſpiration of it, and made him repeat the ſame, after an interval of one or two inhalations of atmoſpheric air between each, for a quarter of an hour or twenty minutes ſucceſſively; taking care always to ſupply a briſk ſtream of mephitic air from below, by adding more marble and vitriolic acid when wanted. This operation was renewed three, four, and ſometimes five times a-day; and no inconvenience or uneaſy feeling was occaſioned by it to the patient. On the contrary, he expreſſed himſelf ſomewhat relieved after it, and wiſhed to repeat it oftener than I choſe to venture. The cough ſeemed to be rendered leſs frequent and leſs violent; the matter expectorated aſſumed more of the conſiſtence and appearance which denote laudable pus; the breathing became more free; and I thought the hectic fever was ſenſibly mitigated. Still however there was a progreſſive decay, and none of the ſymptoms were ever entirely ſuſpended. The patient died after uſing the mephitic air, in the manner above deſcribed, for ſix or ſeven weeks, ſatisfied to the laſt that it contributed in a conſiderable degree to alleviate his ſufferings. It is unneceſſary for me to remark that before recourſe was had to this remedy, the texture and even the ſubſtance of a great portion of the lungs were in all probability deſtroyed. It may be worthy of notice, what I had more than one opportunity to obſerve in this caſe, that the ſymptoms were milder aſhore, and more diſpoſed to be troubleſome at ſea, although the greateſt part of our voy-

age

age was within the tropics, where of courſe no expoſure to cold or to the common cauſes which increaſe the diſeaſe, could account for the circumſtance.

The other caſe in which I employed the inhalation of mephitic air, was that of a lady aged about 22 years; who nearly two years and a half ago, was ſeized in Ruſſia with ſymptoms of a violent pleuriſy, after incautiouſly eating iced cream when over-heated. Notwithſtanding bloodlettings and other evacuations, the inflammatory ſymptoms ſeem to have run into a rapid ſuppuration, for eight or ten days after the firſt attack, and after a ſevere fit of coughing, almoſt immediate relief followed the ſudden expectoration of a large quantity of what was deemed pure pus, ſlightly intermixed with blood. But though the pain and dyſpnœa now abated, ſtill a frequent cough and a very copious expectoration of a ſimilar matter to that diſcharged at firſt, remained; and ſoon her fever aſſumed a hectic form. She was in this ſituation recommended to come to England, but experienced no benefit either from the ſea voyage or from the uſe of the Briſtol hot waters, which ſhe drank during ſome months. So much of her caſe I give from her own report. From Briſtol ſhe came to Bath in the beginning of laſt January, when I firſt ſaw her, eighteen months after the commencement of her illneſs. The ſtate of circumſtances then was, very conſiderable and *progreſſive* emaciation, an almoſt conſtant hectic fluſh on the countenance, the pulſe always quick, with regular and ſtrong exacerbations of fever towards evening, which again abated before morning, and were ſucceeded by profuſe ſweats; the cough was very frequent, and the expectoration ſo profuſe as completely to wet many handkerchiefs daily. She began now to inſpire mephitic air, pretty nearly in the ſame manner as Col. Cathcart had formerly done. She not only repeated, however, the inhalations from the machine oftener, and continued them longer each time than was done in his caſe, but even while ſhe was not inſpiring through the tube, the machine generally remained on a table near her, emitting the fixed air which was continually extricated from the mixture of calcareous earth and vitriolic acid it contained, ſo that I ſeldom entered her apartment without perceiving mephitic fumes in a greater or leſs degree. The apartment being cloſe and of no great extent, I ſometimes thought it prudent to have a window opened for the purpoſe of clearing it of theſe fumes.——Particular circumſtances rendered it neceſſary that I ſhould inform the lady's relations without reſerve, what chance I ſaw of her recovery; and in the beginning of my attendance I did not heſitate to expreſs my deſpair of doing her any good, or of ever ſeeing her better.

Such however was ſoon the abatement of all her ſymptoms under the above treatment; ſo entirely for ſome weeks did the hectic fever diſappear; and ſo evidently did ſhe gain during the ſame period both fleſh and ſtrength, that not only her relations acquired new and ſanguine hopes of her recovery, but I began ſeriouſly to flatter myſelf with a diſappointment of my predictions, although I durſt not venture to avow it. The firſt check given to this amendment, which proceeded for four or five weeks, was occaſioned by an over exertion of her lately recovered ſtrength, during a fatiguing walk, the latter part of which was up a pretty ſteep aſcent. A return of pain in the breaſt and dyſpnœa, a tinge of blood in the expectoration, together with an accelerated pulſe, made me have recourſe to bloodletting, bliſters applied to the cheſt, &c. which greatly relieved theſe ſymptoms, but at the ſame time reduced the general ſtrength. The inhalation of mephitic air was interrupted during the period of this freſh inflammatory attack, from an uncertainty how it might act rather than from any obſervation of its diſagreeing; but it was repeated as before, after the ſymptoms of inflammation had abated, and again ſeemed to produce the ſame beneficial effects. A ſecond relapſe however occurred ſome weeks afterwards from a ſlight indiſcretion, the throwing off part of her accuſtomed garments. This was removed much in the ſame way as the former one, and the mephitic air was again reſorted to with ſimilar ſucceſs. After each of theſe inflammatory attacks, and after one or two others which happened ſubſequently, there remained for ſome time a conſiderable increaſe of cough and expectoration, and a permanent hectic, which however gradually abated under the uſe of the mephitic air. But theſe repeated relapſes from ſlight cauſes, notwithſtanding the conſtitution rallied aſtoniſhingly afterwards, and ſoon ſeemed to regain all it had loſt, renewed my fears that the diſeaſe would ſoon run the uſual and rapid courſe of confirmed phthiſis. The patient left Bath in the month of May laſt, to take advantage of the ſummer ſeaſon for trying another voyage by ſea, ſtill bent on continuing the inhalation of mephitic air. I deſpaired of hearing much longer any favourable accounts of her; but have been repeatedly and agreeably diſappointed, in learning that her health has ſince gained inſtead of loſing ſtrength. By a letter received within theſe few days from Peterſburgh, where ſhe has paſſed the ſummer, it is reported to me, "that ſhe is wonderfully recovered by the Balſam of Mecca, which ſhe got from the Turkiſh ambaſſador." Whether ſhe has all along continued the mephitic air, I cannot undertake to aſſert; but I believe in the affirmative, from her intentions

at

at the time of leaving this country. To whatever cauſe her preſervation is owing, it is the firſt caſe of ſo fully formed, and ſo far advanced a phthiſis that I have met with, in which the progreſs to diſſolution has been ſo long reſtrained, or ſo ſucceſsfully repelled.

I recommended to the parents of a young lady, who died of phthiſis at Briſtol about a year ago, to make trial of this method of exhibiting fixed air; and her father aſſured me afterwards that he had attempted it, but found her lungs could not bear it, as it excited irritation and coughing. From his account however I ſuſpected that the operation was clumſily conducted, and that the coughing was produced by a ſtream of air ruſhing too ſuddenly from the tube into the fauces without an act of volition. Both of my patients experienced this inconvenience at firſt, owing to my very imperfect apparatus; but after they acquired the management of it, no ſuch irritation was excited.

I ſhall be rejoiced to hear of your purſuing thoſe inquiries on the effects of reſpiring different kinds of air, in which you have already diſplayed ſuch happy invention in theory, with equal ſucceſs in practice; and ſhould the expectations ſuggeſted by an ingenious hypotheſis be too ſanguine, yet much advantage, I truſt, will ariſe from this application of the recent diſcoveries in pneumatic chemiſtry, to the improvement of pathology and the cure of diſeaſes.

I remain, dear Sir, &c.

John Ewart.

——Accounts from Peterſburgh of a late date ſtate the amendment of this lady to be more conſiderable than I ventured in my laſt letter to repreſent it. It was her intention to paſs the winter in the South of Ruſſia, but ſhe now thinks herſelf ſo well as to be able to remain with impunity at Peterſburgh. The expreſſions of her father in a letter to her ſiſter are, "She has recovered progreſſively ever ſince ſhe "returned here, regains fleſh and ſtrength, is free from fever, "and ſuffers very little from her cough, but continues to ſpit "immoderately, though with eaſe." No mention is made in this letter whether ſhe perſiſts in reſpiring fixible air.

Bath, Dec. 15, 1793. Your's, &c. J. E.

LETTER

From Doctor THORNTON,

To Dr. BEDDOES.

SIR, LONDON, *December* 7, 1793.

I SHALL be happy at all times to communicate to you the result of my various trials of factitious air. I more readily entered into your ideas, as to the change to be wrought on the blood by different airs, and consequently the removal of many otherwise incurable disorders, as I had formerly chosen for my thesis at Cambridge, "that all animal heat arises from the decomposition of air," or in other words, that the blood in the lungs receives from the air *oxygene in combination with matter of heat*:—and that in its passage through the body, the oxygene meeting with another superior attraction, forsakes the caloric; and the matter of heat being thus disengaged, (as with neutral salts whose basis is withdrawn from them) assumes its well known active character.

I was naturally led next to the consideration of the different mediums through which this matter of heat or caloric passes, and I found it pervade most rapidly bodies already saturated with oxygene.† In local inflammations I therefore forsook the old practice, and hastily withdrawing all oily applications, I substituted in their room the best conductors of heat. Having learnt from the experiments of Dr. Black, the different capacities for heat in water and steam, after long exercise I always recommended warm tea, or whatever else might produce perspiration. I was enabled to support the remarkable heats of last summer in a surprising manner by wearing a *fleecy hosiery waistcoat*; and since my first using this under garb, I am not subject to catch cold, as formerly, from vicissitudes of weather.

Four years ago I was at Bristol Hot-Wells, the sorrowful companion of a near relation, and I observed every consumptive person, I knew there, in time swept away by the *giant-malady* as Dr. Darwin most justly calls it. Being seized myself afterwards with every symptom of phthisis, it was likewise recommended to me to try the Bristol waters, but that my spirits might not get depressed, and not relishing repeated bleedings, the plan of cure then pursued, I retired to the Isle of Wight; and living chiefly on fried fish and animal food, with

† Is this true of all bodies, metals included? T. B.

with much freſh butter, I recovered to the ſurpriſe of every one. This, Sir, was previous to your new method of treating that fatal diſeaſe, and before I could be biaſſed by your theory.

Laſt September, when I went down to Pewſey in Wiltſhire, I found my valuable and very learned friend, the Rev. Mr. Townſend, labouring under a moſt dreadful fever, ſuch a fever as a few years back carried off 63 of his pariſhioners. His tongue was black; his breath putrid; his countenance ſunk; ſeveral white ſpecks were formed about his fauces; he had the ſubſultus tendinum and ſingultus; his pulſe was quick and feeble. By the adminiſtration of bark aud Port wine every two hours, and food in the intervals, theſe alarming ſymptoms vaniſhed, but the difficulty of breathing ſtill continued. Having opened the windows and ſprinkled oxygenated wine (vinegar) like a fine dew over the apartment, the thermometer fell nearly four degrees, and the effect of a purer and colder air was ſuch, that in a few moments after, he breathed as he ſtyled it, like a ſucking child, through his noſtrils, and generally afterwards grew compoſed to ſleep.*

About ten days ſince, I was called to a patient, a child 13 years old; ſhe had a fever, which had already attacked two other perſons in the houſe. Mr. Murdock, the father of the child, told me that my medical ſkill could avail but little, as his child was at the point of death; and that all he expected from me was in ſome meaſure to palliate her ſufferings. For three days and as many nights every thing taken into her ſtomach had been rejected. During this time ſhe had had no ſleep. Much watery liquor paſſed from her bowels, and ſhe had an almoſt conſtant deſire to go to ſtool. The two laſt nights glyſters of mutton broth had been adminiſtered. When I entered the room ſhe had juſt been convulſed, was ſpeechleſs, and gaſping for breath. Her eyes were fixed and ſunk, and ſurrounded with a circle of a darkiſh brown colour. The muſcles of the face ſtill quivered. I immediately opened the window, for the room had but one, and ordered the fire to be put out. I removed ſome portion of the flannels, with which ſhe was covered, and took off one blanket. I then adminiſtered factitious oxygene air, and to the aſtoniſhment of the beholders an acute pain in the left ſide firſt abated and then altogether ceaſed. Her ſpeech was reſtored. As ſhe ſeemed exhauſted for want of food, I took the white of an egg, which of all nutritious ſubſtances I judged the leaſt ſubject to corruption, and mixing it with white wine, warm water,

* Mr. Townſhend himſelf, the celebrated Spaniſh traveller, lately deſcribed to me the relief he experienced from the air of his apartment, charged with the fine ſpray of vinegar. The language he uſed was ſuch as medical practitioners are accuſtomed to hear, when the patient is ſuddenly delivered from the moſt intenſe pain or diſtreſſing anxiety. T. B.

water, cinnamon, and afterwards with calves' foot jelly a little acidulated, I gave it her in ſmall quantities, and finding that it remained, I ſoon after tried the bark and red wine, ſtopping whenever the leaſt inclination to vomiting came on. The child was recovering faſt by this treatment, when ſome officious female interfered. The conſequence was, that the child was again ſeized with convulſions and became ſpeechleſs. But in leſs than five minutes ſhe was reſtored by breathing pure air. She is now out of danger, and doing well.

That hyper-oxygenated air is an admirable coſmetic, and the acquirement of colour attended with no diminution, but generally with an increaſe of health and ſpirits, I could adduce many reſpectable teſtimonies to ſhew. With electricity I make no doubt it will be found to be the moſt effectual cure for chloroſis.—The good and bad effects from the transfuſion of blood (as formerly employed) may be now accounted for. As the poſt is on the eve of departing, I ſhall detain you with but one obſervation more. The caloric imparted from oxygenated blood appears to be the ſtimulus moſt eſſential to the animal œconomy. It is pleaſing to obſerve that the power of being irritated in the nervous ſyſtem keeps exact pace with the quantity of this ſtimulus generated in the animal body. Hence the reaſon of the long life of the heart of fiſhes, as it is called, and of all animals whoſe blood is cold. What advantage may be derived to the ſick by increaſing or diminiſhing this natural ſtimulus may be eaſily conceived!

I have the honour to be, &c.

R. J. Thornton.

*Extract of a letter from Dr. ———.**

I HAVE lately tried purified *(hyper- oxygenated)* air in the caſe of the —— of Mr. ——, the celebrated ſurgeon in ——. This young lady has for two years been ſubject to repeated ſpaſms, and has found no relief whatever from medicine. She has been conſiderably better ſince her firſt breathing purified air. Yeſterday, juſt before ſhe was to imbibe it, a ſpaſm came on, ſuch as terrified all around her. She had not breathed the portion of air I judged a proper doſe, when to the aſtoniſhment of Mr. ——, ſurgeon, the ſpaſm ceaſed.—I am daily more and more convinced of the juſtneſs of your ideas on conſumption. I have been to Haſlar Hoſpital to obſerve the ſea ſcurvy.—A lady in the ſtate of pregnancy, whom I have juſt left, is drinking vinegar, which ſhe could not before bear. She imagines her frequent tooth-aches to proceed from the four apples ſhe has lately ſo much indulged in. May not the qualms in pregnancy be removed by purified air?

Dec. 19, 1793. Your's. &c.

To Dr. Beddoes. —— ——.

* I receive this letter at the moment of delivering the laſt parcel of MS. to the Printer. The communication was not made for the ſake of publication, and I have not time to requeſt permiſſion and further particulars. So I muſt ſuppreſs names. But I may venture to aſſure the reader that he need not doubt the authenticity of the accouut a moment. To ſay nothing of the writer, the parties mentioned in the letter are, ſome of them, well known to the public. From the ſlight intimation given of the caſe, it appears to be one of thoſe nervous affections where opium in large doſes often does ſervice. T. B.

Caſe of Dyſpnoea, approaching to Orthopnoea, in a letter from Benjamin Biggs, M. D.

IT is proper to premiſe that the following caſe could not at the time Dr. B. was at Briſtol be referred to any diſeaſe for which we have an appropriated denomination and definition in the common books of Noſology. It came neareſt to what is called an humoural aſthma; the nocturnal acceſſions of difficulty of breathing did not obſerve the courſe of the paroxyſms of the true aſthma; nor were they preceded by drowſineſs, yawning and the other ſymptoms uſually preceding ſuch paroxyſms; nor did they, like aſthmatic fits, occur from time to time, leaving the patient free in the interval. Dr. B. inſpired one part of oxygene air mixed with three of atmoſpheric three times a-day at firſt, for five minutes at a time. He afterwards inſpired the ſame mixture for twenty minutes at a time. Notwithſtanding the effect of this proceſs on the dyſpnoea, the cough and expectoration, continued much as before.

DEAR SIR, OCTOBER 14, 1793.

I HAVE for near two years been ſubject to a cough with ſpitting of mucus and very conſiderable difficulty of breathing, the attacks of which reſembled aſthmatic paroxyſms in coming on in the night, often after my firſt ſleep. They very frequently obliged me to riſe out of bed and walk about the room. I was always forced to ſleep with my head conſiderably raiſed. For theſe ſymptoms I had employed various antiſpaſmodic remedies, which afforded only relief for the moment and not always that. After breathing the mixture of airs you directed, I found this difficulty of breathing much relieved in three days, and before the expiration of eight days, it had entirely ceaſed; and has never returned ſince.* Before this time, I had been ſubject to coldneſs of the extremities, which now went off. I could even ſleep with fewer bed-cloaths. I had alſo a greater flow of ſpirits.—— I can hardly doubt, from my own feelings, that this kind of air will be highly beneficial in that very diſtreſſing diſeaſe, the aſthma; and in diſeaſes of languor alſo. I had tried various climates, the Bath and Briſtol waters, in vain; I had conſulted at leaſt twelve phyſicians in Europe, the Weſt Indies, and America. I am, &c.

Benjamin Biggs, M. D.

To
DR. BEDDOES.

* This was written about three weeks after Dr. B. had ceaſed to inſpire the mixed air.

Caſe of Epileptic Affection.

ABOUT three years ago a young man (aged 20) after an excurſion on the heights of the Alps, during which it is probable he experienced ſome terror, was ſeized with a fit in the night. He had dreamed of falling from a precipice. There appeared evident marks of his having been ſtrongly convulſed. This attack was at firſt conſidered as the night-mare; and valerian with other medicines, called *nervous*, were in vain adminiſtered. Sea-bathing diſagreed with him; and cold bathing in freſh water rendered him ſuddenly worſe, inſomuch that his fits, which at firſt occurred only once or twice a-week, increaſed to the number of 28 in 24 hours. They afterwards diminiſhed in frequency, and for a long time have not exceeded 12 in the day and night. They differ in degree, if not in kind.—In the more violent, he is inſenſible. Theſe fits continue from one to three minutes, and he is comatoſe for about ten minutes afterwards. They occur only in the night.—The ſlighter fits occur both in the day and night, but more frequently in the night. He has often only one or two by day, and eight or ten by night. Theſe laſt from 10 to 15 ſeconds, during which time the patient is ſenſible and often ſpeaks with perfect knowledge of what is paſſing, though ſomewhat indiſtinctly. The inſtant they are over he is quite well, or rather relieved. If he is ſeized while on his legs, he falls with force; many of his muſcles become rigid and others convulſed. On his chair, he may have a fit without the knowledge of a perſon ſitting in the ſame room. For a long time he was continually drowſy; he could neither look up to any height nor down from it; he could neither read a ſingle line nor exert the ſmalleſt effort of attention, without bringing on a fit. But none of theſe circumſtances now affect him. His appetite and ſpirits he has always retained; nor are his faculties impaired.

Having paſſed through the hands of many phyſicians, he had exhauſted the materia medica. The laſt phyſician he had conſulted, had conceived the deſign of putting a ſtop to his fits by large doſes of opium, adminiſtered towards evening and during the night. The firſt two grains however of this drug produced a frantic delirium, which required the aſſiſtance of eight perſons to ſecure the patient. This ſtate of violent excitement or intoxication laſted 18 hours, during which indeed there was no fit: but the fits were rather more ſevere than uſual during the weakneſs that followed. A ſubſequent trial of opium alſo failed.

Some time afterwards it was ſuggeſted that the inſpiration of modified air might be ſerviceable. Upon being conſulted concerning the probable ſucceſs of this plan, I could give little encouragement. The only hope I conceived aroſe from an analogy which will preſently be mentioned: Although therefore I believed we might manage ſo as not to do permanent miſchief, I thought it due to the patient and myſelf to declare that the event might poſſibly be to a certain degree unfavourable. It is not ſurpriſing that this conſideration ſhould have been ſuperſeded by the wiſh to be delivered from ſo diſtreſſing a ſtate.—No trial having been before made in a ſimilar caſe with air containing either more or leſs oxygene than the atmoſphere, I had only analogy for my guide. The following probabilities determined my choice. 1. Animals breathing air of too low a ſtandard fall into convulſions. 2. The cold bath had permanently aggravated the complaint; but cold is only the abſtraction of heat, and the abſtraction of oxygene might, I feared, be prejudicial. 3. The great phyſician, who ſuggeſted the uſe of air of a reduced ſtandard in the preſent caſe, obſerves in a work which will ſpeedily be publiſhed: "If the excitability of the ſyſtem depends on "the quantity of oxygene abſorbed by the lungs in reſpi- "ration, ſleeping in an atmoſphere with leſs oxygene might "be of great ſervice in epileptic caſes, and in cramp, and "even in fits of the aſthma, where their periods commence "from the increaſe of irritability during ſleep." Now the ſlighter fits in the preſent caſe came on chiefly, perhaps in the proportion of 8 to 1, during ſleep; and the ſeverer fits always; and I had found that in aſthma the nocturnal fits were prevented by air with exceſs of oxygene. This I thought a ſtrong analogy. 4. I believed, and it appears ſtill probable to me, that there is a great difference between tendency to ſpaſm or convulſion, and ſtrength in muſcles: I hoped that oxygene, by ſtrengthening Mr. ——'s muſcles, would diminiſh their too great mobility. 5. The patient is of that temperament, to which laxity of fibre is aſcribed. 6. His youth was an objection to this mode of treatment, but he was rather fat for his age, whence I inferred that he had not already an over-proportion of oxygene in his ſyſtem.

From theſe conſiderations he was deſired to inſpire a mixture of three parts of atmoſpheric and one part of oxygene air, for ten minutes on going to bed. As no effect was perceived, the time of inſpiration was next night (Sunday night) extended to twenty minutes: after which he felt an agreeable glow in his cheſt. On Monday night at three intervals he inſpired for half an hour: and by way of precaution a ſaline draught with antimonial wine was ordered for him, and his

diet

diet was a little lowered. On Tuesday night he inspired for about twenty minutes: on Wednesday the air was omitted. On Thursday as no sensible effect followed, and as he passed good nights and had had no fit during two of the preceding days, the mixture of air was made a little stronger with oxygene, nearly as one of this species of air to two of atmospheric: He inspired for half an hour, and felt uncomfortably hot afterwards. In the morning his pulse was 72, and of natural strength. He coughed slightly, but found himself very well. He had no fit during the day, and the friend who accompanied him, and who had observed him with great attention ever since the commencement of his indisposition, thought him so much better than he had been for some time past, as to write a favourable account to his distant friends. Towards night he was unusually lively, but perfectly composed. This night the inspiration was omitted; as I had originally determined to interpose an interval of several days, as soon as any effect, good or bad, should appear. He had scarce lain down when he was alarmed with what I conceive from his description to have been a starting of the abdominal muscles. This soon ceased, but I found him flushed and somewhat feverish, with a pulse above 100 and rather strong. He had a strong tendency to muscular motion, but was easily persuaded to lie quiet. He appeared as if a little intoxicated, and at the same time alarmed at his situation. One of his slighter fits supervening increased his apprehensions, for he had conceived some hopes that this would prove the crisis of his disease. As he had had no motion the preceding day, a gentle cathartic was prescribed and operated as was wished. During the course of the night he had a kind of drunken delirium, similar to that which opium had produced, only far milder and accompanied with singular muscular agitations: The toes sometimes moving like the fingers of a person playing on the harpsichord, and the lower extremities being frequently in action. But the motion of his arms was the most constant; and this was of a very curious kind. It exactly imitated the gestures of a person who very gracefully drives a pair of horses from a phaeton. To this exercise the patient had long been accustomed for three or four hours every morning, but he had lately discontinued it for about a fortnight. These gestures lasted till some time on Monday, when all the other movements had subsided. He frequently declared them to be involuntary, and at breakfast on Monday, when he was quite composed, was rather amused with his own inability to restrain them. He had only five or six of the slighter fits in twenty-four hours; but did not sleep till Saturday night, when he fell into a profound sleep and had the

usual

uſual number of fits with a delirious acceſſion early on Sunday morning. Early on Monday morning, he had a ſimilar, but much fainter paroxyſm, which was the laſt. During the reſt of Sunday night he ſlept as before very profoundly, which he had alſo done in the day-time. The muſcular agitations were at this time gentler during ſleep and confined to the fingers principally —The muſcles that move the joints had been ſo much in action as to produce that general ſtiffneſs and ſoreneſs which follows unuſually ſevere exerciſe. The pulſe ſoon became feeble, and was ſometimes 108. He was full of apprehenſion during this whole time, but his fears gradually ſubſided. At times he appeared to be torpid.—It is remarkable that not only the prevailing ſtate of the mind was the ſame during the action of the opium, but that it was occupied by the very ſame ideas on both occaſions. The whole effect of the opium I was informed, totally diſappeared in leſs than 40 hours; that of the oxygene air laſted 12 hours longer; the excitement of the ſenſorium was far more violent and continued in the former caſe; but this was compenſated by the extraordinary muſcular agitation in the latter caſe. This agitation has indeed been in a ſlight degree ſince obſervable during ſleep; a gentleman, who lately watched the patient all night for the ſake of making obſervations, has alſo had reaſon to believe that the ſame apprehenſions, which he expreſſed during his periods of excitement, recur in his dreams. In his waking hours and in other reſpects he has appeared at leaſt as well as before he inſpired oxygene air.—I was not prepared to expect any thing like intoxication from an exceſs of oxygene, eſpecially as in inſtances where I have known more inſpired in the ſame time, nothing beyond a ſenſation ſimilar to the alertneſs of healthy children was felt.—The muſcular agitations of this patient contribute to render it probable that the difference of muſcular irritability in different perſons partly at leaſt depends on a difference of oxygene in the muſcles. This phœnomenon is, in my opinion, to be claſſed with the increaſed vivacity of the ſyſtem in animals that have reſpired air of an higher than the common ſtandard. The preſent caſe ſhews alſo that if any one ſhould attempt to reſtore or increaſe the irritability of his muſcles by vital air, he ought to conduct the proceſs very gradually; and perhaps in this way the progreſs of old age may be arreſted: and much of that liſtleſsneſs prevented, which renders the decline of life ſo comfortleſs and ſad.

Excitement uſually follows the application of intoxicating ſtimulants more ſpeedily than in this inſtance. But ſuppoſing the muſcular movements to have ariſen from the increaſed proportion of oxygene in the muſcular fibres, the blood would

would take ſome time to feed them with this ſuperabundant quantity; and perhaps the delirium was only ſymptomatic of the agitation; which took place firſt, and which, I am certain, did not proceed from any ordinary ſtimulant. The patient on that day, by my deſire, had even dropped his ordinary allowance (four glaſſes) of wine. Any medical inferences that may be deducible from theſe facts, the reader ſhall draw for himſelf. For my own part, my want of ſucceſs induces me to wiſh that I had followed the ſuggeſtion of the phyſician whoſe opinion I have quoted.

What was the immediate cauſe of theſe ſingular muſcular movements? Could they be excited by the blood, rendered unuſually ſtimulating, as it traverſed the muſcles, rendered unuſually irritable—by an overcharge of oxygene?

I have already ſaid that this caſe is now publiſhed by way of caution. As I am perſuaded that many ſick people, harraſſed by diſeaſe and tired of medicines, will themſelves ſuggeſt the trial of elaſtic fluids, I hope the caution will be regarded. The credit of the *Digitalis* ſuffered from its being given in doſes twenty times too large: ſo would that of opium, mercury, and antimony, if they were now firſt about to be introduced into the materia medica.—N. B. It is about ſeven weeks ſince this caſe occurred.

Abſtract of Mr. Vauquelin's experiments on the liver of the Ray or Skate fiſh.

THE Skate has a very large liver in compariſon with its heart and its organ of reſpiration. The liver in this fiſh is of a very fat nature, as is well known to cooks, who always obſerve that it affords, upon being boiled, a great deal of oil, which continues liquid in the ordinary temperature of the air.——Of ſkate's liver Mr. V. reduced 1 oz. $4\frac{1}{2}$ gros (drachms) by pounding it in a mortar to a ſort of pap, on the ſurface of which were ſeen to float particles of white oil; 4 oz. of cold diſtilled water readily combined with this pap; the mixture was whitiſh, and on adding more water, became as white as milk. The liquor being paſſed through a fine ſilk ſieve, nothing remained behind but the inveſting membrane. In a few hours there appeared on this emulſion, a yellowiſh cream like that which is ſeen on diluted milk or on an emulſion of almonds; it was doubtleſs ſome of the oil which ſeparates and carries up a little of the parenchyma. This milky liquor is decompounded by even the weakeſt acids, they produce curds or coagula, which riſe to the ſurface, as when ſoap is decompounded by an acid.—The above-mentioned cream, being ſkimmed and agitated long in a mortar, did not yield butter like the cream of milk, but only an oil of a thicker conſiſtence than that which was procured by heat in a ſubſequent experiment. Paper, on which the liver of a ſkate has lain, becomes tranſparent and oily.—4 oz. of liver, covered with its membrane, after being bruiſed, were heated ſlightly in a pipkin: on the firſt impreſſion of the heat, a coagulation took place and much yellow oil ſeparated; the heat was applied as long as any ſteam aroſe: then the oil was paſſed through fine linen, and a ſtrong preſſure applied to ſeparate it from the parenchyma, which afterwards weighed 4 gros. 36 grains, but ſtill retained a good deal of oil; the collected oil weighed 1 oz. 7 gros. theſe together make 2 oz. 3 gros. 36 grains; hence the water evaporated muſt have been 1 oz. 4 gros. 36 grains. The 4 gros. 36 grains of parenchyma afforded, on combuſtion, 8 grains of aſhes, which proved to be phoſphate of lime.—Upon 2 gros of oil from ſkate's liver there was poured oxygenated marine acid, till the acid ceaſed to loſe its peculiar ſmell immediately. The oil became white like greaſe, but it had the ductility of wax that has been ſqueezed between the fingers. Mr. V. found that upon blowing on the ſurface of this oil, twelve hours after its expreſſion, a white pellicle

pellicle was formed; this pellicle broke into fragments, which diffuſed themſelves through the oil. When the oil was ſpread thin on glaſs, it ſoon became denſe and opake. "Theſe experiments," adds Mr. V. "prove that ſkate's liver "contains above half its weight of oil completely formed. "The fluidity of this fatty matter ſhews the influence of the "very inconſiderable reſpiration of this animal upon the "conſiſtence of its ſubſtance, and eſpecially upon the charac- "ter of its fat. The liver of man and of quadrupeds ſome- "times on being cut or torn, ſhews traces of oil, but it is "far ſhort of the quantity found in the claſs of ſwimming "*amphibia*. In certain diſeaſes of the abdomen, in diſeaſes "of the liver, this viſcus ſwells, becomes almoſt white or "greyiſh like that of the ſkate, and at length grows very oily. "The livers of birds, eſpecially of geeſe that are kept in a "warm place and fed with milk, exhibit the ſame appear- "ance. Probably the blood, paſſing through the ſyſtem of "the meſenteric, ſplenic and hepatic arteries, undergoes "great changes; whether, as ſome phyſiologiſts with little "probability ſuppoſe, it diſſolves the abdominal fat, or "whether, as I am inclined to think, as it ſlowly traverſes "theſe regions, the carbone it contains attracts to itſelf all "the oxygene which had been only introduced among all its "particles as it traverſed the lungs; and hence the blood "itſelf, immediately before it returns to the thorax, acquires "from the ſuperabundance of hydrogene an oily nature, "which it imparts to the organs it nouriſhes. *(a)* If this "takes place at all in man and quadrupeds that reſpire much, "and in whoſe veſſels the circulation of the blood is very "rapid, it ought to be far more ſtriking in thoſe ſingular "animals, which are capable of living long in mud or the "moſt offenſive mire, and which have a very limited reſpi- "ration, ſince in proportion to their ſize their reſpiratory "organs are very minute, and of courſe admit but very little "air; which from the ſlowneſs of their circulation, is not "imparted to their whole maſs of fluids till long after it is "received. Hence thoſe animals are all ſoft and cartilagi- "nous; pallid and colourleſs throughout, and little ſenſible "or alert. I attribute therefore the pre-eminence of the "liver in this claſs of animals to their deficiency of reſpi- "ration; as alſo the liquid and oleaginous ſubſtance of "their brain."—So far Mr. V.—I add 1. That if one of his laſt remarks be juſt, oxygene in ſome form or other may be expected to cure that ſtrange diſeaſe, *the emollition of the*

 bones.

(a) I intend to examine, whether the blood of the venous ſyſtem of the abdomen, collected in the *vena portarum*, does not contain carbonic acid, or more carbonic acid than the reſt of the blood, ſuppoſing this to contain any. V.

bones. 2. The whole tenour of theſe facts tends to confirm the connection between a certain deficiency of oxygen in the human ſyſtem and the formation of fat. As animal chemiſtry is improved, the art of fattening animals will alſo be rendered more cheap and expeditious.—Should opulent people in general acquire a taſte for knowledge, many experiments upon a large ſcale will be executed, of which the reſult will have a very ſalutary re-action upon medicine and phyſiology. For inſtance, if ſkates and other animals of this claſs were long kept in reſervoirs of water in contact with oxygene air, their ſtructure and, conſequently their nature, would probably undergo a great change. 3. Dr. Withering has communicated to me a fact towards the confirmation of my conjecture, that fat is generated at the expence of muſcle. In Portugal, where Dr. W. ſpent the laſt winter, hogs are fed much fatter than in England. He informs me that he obſerved in one inſtance in particular where the hog was more than ordinarily fat, that there was no muſcle on the cheek, and very little on the ham.

In the *Monthly Review* for November 1793, the following among many other acute remarks occurs: "It is aſſumed "by Dr. Beddoes that the blood of pregnant women has a "diminiſhed proportion of oxygene; but pregnant women "have the ſame circumſcribed ſpot of florid acid in their "countenances, which is apparent in hectics: if then the "preſence of this colour be ſufficient to prove an exceſs of "oxygene in the one caſe, it muſt have the ſame weight in "the other."

Undoubtedly, the theory cannot be put to a more proper teſt; for, if the fluſh of pregnancy and of florid conſumption be the *ſame*, my opinion concerning the hyper-oxygenation of the ſyſtem in florid conſumption loſes all the ſupport it ſeemed to receive from this appearance: nor can the two ſtates be oppoſite with reſpect to the proportion of oxygen. Let obſervation therefore decide.—To me it has appeared that the fixed ſpot on the cheek of pregnant women is dark-coloured in compariſon with the fine crimſon of the hectic fluſh: and I have thought it more nearly to reſemble the complexion of ſome elderly people, or that hue which is occaſioned by cold, both which I ſuppoſe are owing to a paralyſis or relaxation of the cutaneous capillaries, particularly of the veins. The varicous ſwellings, vibices and livid ſpots in pregnancy ſeem to indicate a ſimilar cauſe. It may at leaſt as a friend has ſuggeſted to me, deſerve to be conſidered whether theſe appearances are produced ſolely by

mechanical

mechanical preſſure.——Typhus, I am informed on good authority, has ſuſpended conſumption. This fact is perfectly conſiſtent with my opinions. I originally expreſſed the moſt ſanguine expectations from oxygene air in malignant fevers, and the opinion has been corroborated by facts.—It may be ſaid, in the language of the late Mr. Hunter, that, in all theſe inſtances, phthiſis is ſuſpended by a *new action* being induced. The anſwer is true, no doubt; but we ought not to reſt ſatisfied with it, ſince it leads to nothing uſeful in practice. But it would be uſeful to aſcertain the nature of the new action or, at leaſt its cauſe; and to this my inquiries have tended.

I once hoped that this collection would have been enriched by the caſe of Dr. JAMES CURRIE of Liverpool, drawn up by himſelf. Other occupations, however, have prevented Dr. Currie from executing his obliging intention towards me. As this ingenious phyſician laboured under the moſt alarming phthiſical ſymptoms, the obſervations he made upon himſelf muſt be highly intereſting, and it is to be hoped that he will one day make the particulars public. He has favoured me with the following ſummary. "My caſe con-
"tains nothing that ſeems to me applicable to your theory.
"The ſole inferences to be drawn from it are, that in the
"florid conſumption a change of air from the ſea-ſhore to an
"inland and mountainous ſituation is highly uſeful; and
"that the hectic paroxyſm on its approach, may be prevented
"by the ſwing, in ſome inſtances, and by exerciſe on horſe-
"back in ſtill more, to which laſt, perſevered in with a de-
"gree of pertinacity that is not common, I chiefly impute
"my own recovery."

In a letter, dated Jan. 2, 1794, Dr. Thornton informs me of an inſtance of great and unexpected relief from the exhibition of atmoſpheric air mixed with a ſmall proportion of hydrogen air in the laſt ſtage of conſumption. "Mr.
"C——ns" he ſays, "beſides the ordinary ſymptoms of con-
"ſumption, had an oedematous ſwelling of his feet and
"ancles; that laſt fatal ſymptom, a laxity of his bowels, had
"even come on. Upon being called in, I reſolved upon a
"very cautious trial of hydrogen air, which I employed at
"firſt in not more than the proportion of one part of hydro-
"gen air to thirty of atmoſpheric air. Under this treatment,
"to my great ſurprize, he has gained ſtrength; the diarrhoea
"ceaſed and the oedematous ſwelling diſappeared. And it
"deſerves to be mentioned, that ſome particular circum-

"ſtances having at times prevented his being ſupplied with "the mixed air for ſeveral days, he has become worſe, and "gained ground, after inſpiring it again." Dr. T. adds at the cloſe of his letter this juſt obſervation—"Should any "practitioner, bold through ignorance, do eſſential injury by "an injudicious adminiſtration of air, the unhappy event "would be blazed through the kingdom; and the benefit "that will otherwiſe probably reſult to mankind from your "propoſal, perhaps be excluded for ever." The ſame chance of falling into unmerited diſcredit awaits every ſubſtance endued with active properties, on its firſt introduction into medicine.

THE following communications are not noticed in the table of contents, becauſe they were received after that table was printed off. The publication of the pamphlet was delayed in order for their reception. They tend to ſhew that the adminiſtration of factitious airs in certain diſeaſes is SAFE *and* PROMISES ADVANTAGE *to Society; and that the deſign ought to be proſecuted, which is all that I maintain in this or any preceding publication.* T. B.

SECOND LETTER from Dr. THORNTON.

Great Ruſſel-ſtreet, Bloomſbury,
Jan. 4, 1794.

DEAR SIR,

I HAVE lately found vital air of great uſe in the removal and alleviation of certain ſpaſmodic diſeaſes, as the aſthma and hooping-cough. One ſpaſmodic caſe, that came under my immediate care, deſerves, I think, your particular attention.

An amiable young lady, nearly related to ſome gentlemen of the firſt eminence in the medical world, has been for the laſt two years dreadfully afflicted with violent ſpaſmodic ſeizures. Opium had been largely adminiſtered, but it ceaſed to have the deſired effect. Nothing gave relief but water impregnated with carbonic acid air. Previous to her ſecond trial of breathing a purer atmoſphere, a violent ſpaſmodic ſeizure came on, which ſeemed particularly to affect the diaphragm. All who were around her were alarmed. Her brother-in-law inſtantly urged me to adminiſter the medicinal air. She had ſcarce breathed it three minutes, when to the ſurpriſe of all who were preſent, the ſpaſm left her. It returned however with diminiſhed violence. From daily

daily breathing certain portions of this elegant, and ſafe remedy (if judiciouſly adminiſtered), ſhe has had fewer attacks; and theſe leſs violent, and much ſhorter in their duration.

I cannot at this time forbear mentioning the inſtance of a clergyman, who laboured under dyſpepſia and depreſſion of ſpirits. He had taken the tincture of bark without experiencing much benefit. As nothing conduces more towards good ſpirits and digeſtion than a clear pure air, I adminiſtered the vital air blended with atmoſpheric. The load on his cheſt, as he called it, was removed. His appetite was quickened. His ſpirits were raiſed even to the pitch, I call, gaiety; and as he informed me, he felt a ſtrong inclination to go to the play, to which he had not been this winter, and he ſays he is fully convinced that no inducement could have got him thither, had he not previouſly breathed a more exalted atmoſphere. Languor, you know, liſtleſsneſs and inactivity are characters of hypochondriaſis; and dyſpepſia is its frequent attendant.

Concerning the other kind of air, whoſe properties are diametrically oppoſite to the laſt, I am now adminiſtering it to a gentleman, who when he came to me, appeared greatly emaciated from conſumption: his cough was troubleſome, his voice was gone, his ancles were ſwelled, and a diarrhœa was on him, which laſt ſymptom towards the cloſe of this diſeaſe baffles the power of every known medicine. He was uncommonly weak—but his appetite, as often happens, was good.—As flannel frets the ſkin, however the impreſſion may be weakened by repetition, yet as exciting the ſyſtem without any juſt reaſon, I recommended it to him to change his flannel for fleecy hoſiery, which equally with flannel abſorbs the perſpiration, and as being a bad conductor of heat, hinders us from feeling any changes of the weather. For milk in the morning and at night, I ſubſtituted patent cocoa, and ſome ſlices of cold boiled leg of pork. I adviſed for dinner inſtead of vegetables meat well done, chiefly mutton chops, and French bread. His medicines were ſuch as attract oxygen, as a moderate uſe of wine, opium, and almond milk, with the addition of oil of almonds, of which he took a great quantity in the day. I deſired him to avoid whatever tended to oxygenate the blood, as ſtrong exerciſe, acids, &c. and he breathed at firſt atmoſpheric mixed with hydrogen air. Afterwards I preferred azot, combined in a certain proportion with atmoſpheric air; the reſult was, theſe very formidable ſymptoms ſoon diſappeared, and what makes me give ſome ſhare of credit to the air, is, that when he has left off breathing it for a few days, he finds himſelf worſe;

and

and he always declares himself better, when he has breathed it again for a few days successively. I am in great hopes the sequel of this case will prove as flattering as the commencement. In the last letter I wrote to you, I mentioned some cases, in which I was about to try your medicinal airs. I am now waiting for an opportunity to employ the oxygen air, for the immediate recovery of persons in syncope.—Would not this air, my dear Sir, be found of great service if it were let loose in mines, in churches, and in crowded rooms, but more especially in the bathing-rooms at Bath, where great faintness is often brought on the patient by breathing a reduced atmosphere from the extrication of azot out of those waters. I am, &c.

R. J. Thornton.

On

On the uſe of Yeaſt in putrid fevers. By the Rev. Edmund Cartwright.

A copy of this paper was firſt ſent to Dr. Pegge at Oxford, at the Doctor's deſire. The author, afterwards hearing that the communication might be acceptable to me, very obligingly and humanely tranſmitted it without delay.

T. B.

ABOUT ſeventeen years ago I went to reſide at Brampton, a very populous village near Cheſterfield. I had not been there many months before a putrid fever broke out. Finding by far the greater part of my new pariſhioners much too poor to afford themſelves medical aſſiſtance, I undertook, by the help of ſuch books on the ſubject of medicine as happened to be in my poſſeſſion, to preſcribe for them. In the courſe of my practice I attended a boy about 14 years of age, who was attacked by a fever: what its appearances were in the firſt ſtage of it I forget. He had not been ill many days before the ſymptoms were unequivocally putrid. I then adminiſtered bark, wine, and ſuch other remedies as my books directed. My exertions, however, were of no avail; his diſorder grew every day more untractable and malignant, ſo that for more than a week I was in hourly expectation of his diſſolution. Being under the neceſſity of taking a journey, before I ſet off I went to ſee him, as I thought, for the laſt time; not, indeed, with the ſlighteſt degree of hope to be of ſervice to him, but ſolely for the purpoſe of preparing his parents for the event of his death, which I conſidered as inevitable, and of reconciling them, in the beſt manner I was able, to a loſs which, I knew, they would feel ſeverely. While I was in converſation on this diſtreſſing ſubject with his mother, I obſerved in a corner of the cottage a ſmall tub of wort working. The ſight brought to my recollection an experiment I had ſomewhere met with, of a piece of ſtale meat being made ſweet by being ſuſpended over a tub of wort in the like act of fermentation. The idea inſtantly flaſhed upon my mind that yeaſt might poſſibly be of ſervice to my patient: without a moment's pauſe or reflection I gave him two large ſpoonfuls. I then told the mother, if ſhe found him no worſe for what I had given him, to repeat the doſe every three hours. I then took my leave, ſomewhat precipitately, I own; for I began to think it poſſible the yeaſt might ferment ſo violently as to bring on an immediate ſuffocation. I ſet off upon my journey, and was abſent about a fortnight. Being told on my return the

boy was recovered, I could not repreſs my curioſity to ſee him immediately. Though fatigued with my journey, and night was coming on, I went directly to where he lived, which was three miles from my houſe, in a wild part of the moors. I found the boy, as I had been told, perfectly well. On inquiring of his mother the manner and progreſs of his recovery, ſhe told me, I was ſcarcely out of ſight before the boy ſaid to her, "mother, I think I am getting well:" and from that time he continued to mend as faſt as poſſible. The ſucceſs of this experiment determined me in every caſe of fever, not obviouſly inflammatory, to adminiſter yeaſt, not omitting at the ſame time ſuch other remedies as the nature of the diſorder might ſeem to call for. In the ſpace of two years afterwards, while I continued my reſidence at Brampton, I make no doubt I attended nearly fifty poor people in fevers of the low putrid kind. What will appear ſingular, I did not loſe one patient in all that time. It is to be obſerved, however, I had an advantage which more regular practitioners have not; as my advice and remedies were adminiſtered gratis, I was uſually conſulted on the firſt attack of the diſorder, ſo that its progreſs was ſtopped before it had time to become ſo dangerous as otherwiſe it might have done.

After I left Brampton I went to live in Leiceſterſhire. My pariſhioners there being few and opulent, I dropped my medical character entirely, and did not even preſcribe for my own family. One of my domeſtics falling ill, the apothecary was ſent for. His complaint was a fever, which in its progreſs became putrid. Having great reliance, and I believe with reaſon, on the apothecary's penetration and judgment, the man was left ſolely to his management. His diſorder kept daily gaining ground, till at length the apothecary conſidered him in very great danger. At laſt finding every effort to be of ſervice to him baffled, he told me he conſidered it as a loſt caſe, and that, in his opinion, the man could not ſurvive four and twenty hours. On the apothecary thus giving him up, I determined to try the effects of yeaſt. I gave him two large ſpoonfuls. Recollecting the very ſudden effect I was told it had on the firſt patient I adminiſtered it to, I laid my watch upon the table, and took the man's pulſe into my hand. In about ten minutes I perceived an alteration in it ſenſibly for the better. I then aſked the man if the medicine I had given affected him in any particular manner, ſuch as making him ſick, diſordering his bowels, &c. his anſwer, which I give in his own words, was ſtrikingly emphatical and expreſſive; "I perceive no "effect it has, but to make me feel *ſtrangely lightſome.*" In fifteen minutes from taking the yeaſt, his pulſe, though ſtill

feeble,

feeble, began to get compoſed and even. He then obſerved, that not having been out of bed for many days, it would be great refreſhment to him to get up, if only for the purpoſe of having his bed made. In thirty-two minutes from his taking the yeaſt he was dreſſed, and walking about his room. At the expiration of the firſt hour I gave him a doſe of bark in a glaſs of wine, which I waſhed down with a quarter of a pint more. At the expiration of the ſecond hour I gave him a baſon of ſago, with a good deal of lemon, wine, and ginger in it; he eat it with the appetite of a man in health: in another hour I repeated the yeaſt: an hour afterwards I gave the bark as before: at the next hour he had food of ſome kind or other, but what I do not now recollect; at the third hour, which was nine o'clock at night, he had another doſe of yeaſt, and then went to bed. I went to him the next morning at ſix o'clock; he told me he had had a good night, and that he felt himſelf perfectly well. I, however, gave him another doſe of yeaſt. He then got up, and went about his buſineſs as uſual.

About a year after this, as I was riding paſt a detached farm-houſe at the outſkirts of the village, I obſerved the farmer's daughter ſtanding at the door, apparently in great affliction. On inquiring into the cauſe of her diſtreſs, ſhe told me her father was dying. I diſmounted and went into the houſe to ſee him. I found him in the laſt ſtage of a putrid fever; his tongue was black, a ſanious ichor was oozing out of the corners of his mouth, his pulſe was ſcarcely perceptible, and he lay ſtretched out, like a corpſe, in a ſtate of drowſy inſenſibility. I immediately procured ſome yeaſt, which, being ſtale, and conſequently thick, I diluted with a little warm water to make it potable, and alſo to ſet it into a fermentation, and poured it down his throat. I then left him with little hope, as reaſonably may be imagined, of his recovery. I returned in about an hour and found him perfectly ſenſible and able to converſe. I inquired of him the effects of the medicine. The preciſe words he made uſe of I forget; his anſwer, however, was exactly to the ſame effect as the anſwer to the like queſtion my ſervant gave. I then gave him a doſe of bark. He afterwards took, at a proper interval, ſome refreſhment. I ſtaid with him till he repeated the yeaſt, and then left him, with directions for him to be treated in the ſame manner as I had treated my ſervant. I called upon him the next morning at nine o'clock. I found him apparently well, walking in his garden. He was an old man, upwards of ſeventy, of a thin ſpare habit. He was alive laſt year, and then nearly ninety years old.

About

About a year and half ago, a gentleman's ſon, in the neighbourhood of Doncaſter, was attacked by a putrid ſore throat and fever. He had been ill and in conſiderable danger before I heard, which was by accident, the nature of his complaint. I immediately communicated the above facts to the apothecary who attended him. It happened his diſorder the evening before had taken a favourable turn, and conſequently a change of medicine would not have been juſtifiable. In the courſe, however, of a few days, the nurſe-maid, who waited upon the child, was ſeized with the ſame complaint, and was treated in the ſame manner, but with different ſucceſs. The apothecary then gave the yeaſt. She recovered with a degree of rapidity which he told me he ſhould have conſidered as incredible had he not been an eye-witneſs of it.

Though the very ſpeedy operation of the yeaſt in all the caſes I have enumerated may at firſt ſight appear ſingular, yet if we conſider the principle upon which it operates, it is reaſonable to conclude, whatever its operation may be, it muſt be immediate, as it will begin to part with its fixed air almoſt as ſoon as it is received into the warm ſtomach.

In caſes of external mortification it might be applied to the part affected, as well as given internally. It probably might be found of ſervice in cancers, if what Dr. Buchan affirms be true, that by means of antiſeptics alone he kept a confirmed cancer at bay for ſome years.

In corroboration of the above facts, relative to the medical virtues of the yeaſt, I add the following one, communicated to me by Mr. Williams, a reſpectable clergyman of Pinner in Middleſex, to whom I had been mentioning the ſucceſs with which I had adminiſtered that remedy.

When a young boy he was ſeized with the ſmall-pox, and was thought in imminent danger. By the advice of an old Welch clergyman, who viſited at his father's, he drank a hearty draught of beer out of the vat, the yeaſt being previouſly beaten in. His bad ſymptoms very ſoon diſappeared, the puſtules roſe kindly, and he got through the diſorder, in every reſpect, in the moſt favourable manner. Mr. Williams perfectly recollects his recovery being always ſpoken of in the family as owing to the preſcription of their Welch friend. This event muſt have happened not leſs than forty years ago.

Caſe communicated by Dr. PARRY, *of* BATH.

WHEN the following caſe occurred, the pneumatic chemiſtry had been reduced to no ſyſtematic form. It had indeed long been known that blood was capable of becoming red by contact with atmoſpherical air; and Dr. Prieſtley had found that this happened even though the ſubſtance of a bladder was interpoſed, and that the change was moſt ſpeedy and conſiderable when the experiment was made with dephlogiſticated air. But although from theſe and other facts Dr. Prieſtley had drawn a very important concluſion as to the uſe of reſpiration, yet the application of this branch of chemiſtry to phyſiology had not much occupied the general mind. Hence it aroſe that ſome ſymptoms of the caſe which I am going to deſcribe, and which was preſerved principally with a view to inveſtigate the nature of the hydrocephalus internus, were not ſo minutely related as they would have been had the inquiry taken a different turn, or had its object been more general. With regard alſo to the diſſection, a very great embarraſsment was thrown in our way by a ſtrange tenderneſs of the patient's friends, who choſe to have a man ſervant continue in the room during the anatomical examination. This obliged us to content ourſelves with what imperfect information we could gain by haſtily examining the heart in ſitu; and prevents my ſpeaking with poſitiveneſs as to the non-exiſtence of a canalis arterioſus, the number of the pulmonary veins, and ſome other circumſtances.

On the whole however, this caſe, imperfect as it is, is one of the very few in which the mal-conformation of the pulmonary veſſels affords a ſtrong preſumption that the red colour of the blood is owing to the oxygen which it receives during the act of inſpiration.

The Hon. Miſs V. was firſt put under my care in the ſpring of the year 1786. She was then between 13 and 14 years of age, of a placid temper, moderately tall, thin, and of a ſmall make. The moſt ſtriking appearance of deviation from the healthy ſtate of the conſtitution was a lividneſs or bluiſh purple hue, which in ſome degree affected the whole ſkin, but was moſt intenſe where the tinge of the blood is uſually moſt apparent, as in the cheeks, the noſtrils, the lips, the ends of the fingers beneath the nails, and other ſimilar parts. She conſtantly ſuffered more or leſs of palpitation of the heart, irregularity of the pulſe, and hurried reſpiration; and theſe ſymptoms were much aggravated by any muſcular exertion, though of the ſlighteſt kind, but became extremely painful in conſequence of any ſtronger exerciſe. From

going up ſtairs, however gently, the livid blood became accumulated about the face and head, the pulſe was accelerated to 120 or 130 beats in a minute, the irregularity of pulſation which I have deſcribed became more apparent, and a very quick and laborious reſpiration was induced. At all times, but more eſpecially after the exertion of walking, the beating of the heart was more diſtinctly felt on the right than on the left ſide of the thorax. There was alſo this peculiarity in the circulation, produced, ſo far as I could find, by muſcular exertion only, that the pulſation of the right carotid artery was very perceptible to the touch, while that of the left was extremely obſcure; and that the number of pulſations in the left radial artery was ſmaller than of thoſe in the right; the ſtroke in the former being ſometimes miſſed, and at other times imperfectly performed, while the correſponding pulſation in the latter was diſtinct and ſtrong.

The ſymptoms of diſeaſe which I have deſcribed came on without any obvious cauſe; nor could the young lady's parents aſcertain at what period they had firſt been obſerved. This however was certain, that they had been conſiderably aggravated during the ſix years laſt preceding.

In addition to theſe complaints, Miſs V. was occaſionally liable to head-achs, evidently connected with flatulency, coſtiveneſs, and other marks of indigeſtion, which ſometimes went ſo far as to produce ſickneſs and vomiting. Emetics had therefore occaſionally been given; and her mother was of opinion that they had, for ſome time, relieved her ſtomach complaints, head-ach, and dyſpnœa.

Miſs V. had never had the catamenia. Her appetite was tolerably good, and ſhe was free from cough.

On the lower part of the os frontis, about the middle of the forehead, there was a tumor as large as a pigeon's egg cut through the ſhorteſt diameter, hard, immoveable, and of the ſame colour and ſenſibility as the ſkin near it.

I regret that I made no memorandum as to the heat of her ſkin, or the proportion which the number of reſpirations bore to that of her pulſe.

It required no great medical acuteneſs to diſcover that in this caſe there was a conſiderable deviation from the proper ſtructure of the large veſſels about the heart. I therefore apprized the friends of the patient, that nothing more could be done than to alleviate ſymptoms which would probably one day prove fatal. I deſired that all violent exertion ſhould be carefully ſhunned, but that gentle exerciſe, eſpecially on horſeback, ſhould be aſſiduouſly uſed. At the ſame time Miſs V. was adviſed to abſtain from all full meals, and from every ſort of food which could produce plethora or flatulency;

lency; to clothe herſelf warmly, eſpecially about her legs and feet, and conſtantly to remove coſtiveneſs by means of an aloetic pill. To theſe meaſures was added the internal uſe of ſmall quantities of the Bath water.

This plan, continued more or leſs through the winter, in a great degree removed the ſymptoms of dyſpepſia, and ſomewhat leſſened thoſe of undue circulation.

As the ſpring advanced, her friends wiſhed to fix on ſome ſituation beneficial to her health, in which ſhe might ſpend the ſummer; but previouſly to any deciſion on this ſubject they complied with my earneſt requeſt that they would go to London for the purpoſe of conſulting Mr. Hunter, from whom, in the month of April, 1787, I received the following letter.

"DEAR SIR,

"I HAD the honour to ſee your patient Miſs V. There is certainly either diſeaſe about the heart and lungs, or an original bad formation of theſe parts, the laſt of which I am moſt inclined to believe. If the blood paſſes through the lungs without receiving the benefit of the air, it will come back to the heart venal blood; or if it has any collateral paſſage into the aorta, before it paſſes to the lungs, in either caſe the parts where the blood is expoſed will be livid; and as this has been more or leſs a ſymptom ever ſince ſhe has been born, it is natural to conclude it to be owing to a natural formation of parts. A caſe of that kind is publiſhed in the Medical Tranſactions of the College by Dr. Pulteney, which I ſaw, and where the ſymptoms were very ſimilar to Miſs V.'s, only, I think, more violent. However, as I can conceive diſeaſe, and moſt probably the ſcrofula, to produce ſimilar ſymptoms, I think ſhe ſhould not loſe any chance of being relieved on ſuch an idea. After ſaying that every thing which increaſes the ſymptoms ſhould be avoided, and that every thing which ſerves to keep them quiet ſhould be ſtrictly adhered to, I propoſed her bathing in the ſea-water. But it ſhould be made ſo warm as not to give the leaſt ſhock at firſt going in; and this to be purſued according to circumſtances.

"As this is a caſe which, I think, will terminate ill, I wiſh you would take notes of all the ſymptoms; for probably a time will come when the parts will be inſpected by ſomebody, which will afford valuable information when attended by the hiſtory of the caſe. The pulſation being in one arm and not always in the other at the ſame time, is a curious fact, &c."

Not long after this Miſs V. went to the ſea-coaſt and having purſued the plan which had been recommended, returned to Bath in the autumn, in much the ſame ſtate of health as when ſhe left it. During the beginning of the winter I ſaw her three or four times, but found no alteration in the ſymptoms.

On

On the 4th of January, 1787, I was ſent for, and viſiting her at two o'clock in the afternoon, found her lying in bed. She had complained for two or three days of pain in her head and ſome diminution of appetite, and on the evening before had been ſeized with vomiting, which had continued more or leſs till the time I ſaw her. She had taken no food, and had not ſlept during the night. What ſhe had vomited an hour before my viſit was fluid, ſlightly green, and of an acid ſmell. The pain of her head was not violent, nor, ſo far as I learnt, was it confined to any particular ſpot. Her tongue was ſlightly furred, there was no unuſual appearance about the eyes, and her pulſe, heat, colour, and reſpiration ſeemed to be in their natural ſtate. During two or three days immediately ſucceeding, ſhe had been coſtive; and had taken at the beginning ſome magneſia, which had produced ſcarcely any ſenſible effect on her bowels, and had not relieved any of the ſymptoms. She was ordered to take a ſcruple of calcined magneſia every hour till it operated.

At eight o'clock I found that four doſes of the magneſia had been given without moving the bowels; but the vomiting had ceaſed after the firſt doſe. It was ſaid alſo that ſhe had been aſleep two hours, for which reaſon Lady —— begged that I would not diſturb her by then going into her chamber, but told me, that, previouſly to her going to ſleep, Miſs V. had been delirious, and attempted to get out of bed. I ordered a purgative glyſter to be injected, and the magneſia to be continued.

Repeating my viſit at ten o'clock at night, I was informed that the glyſter had been imperfectly injected, without effect, and that ſhe ſtill continued to ſleep. I begged however to ſee her, and found, on attempting to rouze her, that ſhe was almoſt ſenſeleſs. She was not affected by any noiſe, but ſeemed uneaſy when a candle was brought near her, and the pupils were much dilated. She made an inarticulate ſound with her voice, ſat up of herſelf, and attempted to get out of bed, but ſeemed to have no conſciouſneſs of what was paſſing around her. Her reſpiration was quick and laborious, her ſkin in general rather hot, her feet cold, her face pale, and her pulſe upwards of a hundred in a minute, extremely full, hard and labouring.

By my deſire Dr. Falconer was called into conſultation, and we met at half paſt eleven at night, when all the ſymptoms laſt deſcribed continued, but in a greater degree.

She was ordered to loſe four ounces of blood, to have the purgative glyſter repeated, to take a draught with a few drops of tinctura thebaica, to have the feet, legs and abdomen fomented with tepid water, and afterwards ſinapiſms applied to the feet.

Previouſly

Previouſly to the bleeding ſhe had two fits, in which ſhe was convulſed in various parts of the body, and particularly about the throat, as in the hyſteria, and cried out with great violence. The glyſter brought away at firſt a conſiderable quantity of hardened fæces, and afterwards a copious looſe ſtool. After this evacuation and the bleeding, ſhe ſeemed ſomewhat relieved with regard to her breathing and power of ſenſation.

The glyſter was ordered to be repeated, and ſome broth occaſionally given.

Jan. 5. Eight in the morning. She had had an evacuation from the glyſter, but had paſſed a very bad night. Her breathing was ſtill more laborious, her ſkin very hot; her pulſe 136 in a minute, extremely ſtrong and hard; the pulſation of the right carotid unuſually full and bounding, her face ſtill pale, but the pupils more dilated. Now alſo, for the firſt time, there appeared conſiderable ſtrabiſmus. No urine had been made for upwards of twenty-four hours.

A repetition of the bleeding was ordered, and it was directed that ſhe ſhould be put into a tepid bath.

She was bled, and the bath employed, notwithſtanding a conſiderable quantity of urine had previouſly been made. She then ſeemed again eaſier; but the ſymptoms ſoon rapidly increaſed, and at night ſhe died.

On opening the body the following day, the cranium appeared to be unuſually hard. The ſwelling in the forehead was found to be a tumor or thickening of the os frontis itſelf, which when the ſcalp was removed, was rough and of a livid ſpotted appearance, as from divided varicoſe veſſels. The ſame rough tumor extended itſelf the inſide of the cranium; but no particular diſeaſe was obſervable in the dura mater lining that part, or in the portion of the cerebrum immediately under it.

The dura mater itſelf was very tough and adhered ſtrongly to the cranium.

The veſſels of the pia mater were extremely turgid with blood.

A conſiderable quantity of water was found in the right lateral ventricle.

On the right ſide of the thorax, the ribs were very much depreſſed, while thoſe on the left were in a natural ſtate. The lungs on the left ſide were free from adheſion and any other appearance of diſeaſe. The right lobe of the lungs was ſo thin as to reſemble nothing more than a plexus of membranes, it ſtrongly adhered to the pleura coſtalis; but was free from tubercles, inflammation, or ſuppuration. The right pulmonary artery was ſmall in proportion to the defect of the right lobe of the lungs.

The

The heart was conſiderably larger than natural, and the coronary arteries were full of blood.

Every thing about the arch of the aorta, and the carotid arteries, was in a natural ſtate; nor was there any other unuſual appearance about the heart or large veſſels.

The mentum was void of fat; but all the viſcera of the abdomen were free from diſeaſe.

C. H. Parry.

BATH, *Jan.* 17, 1794.

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