Physical sketches, or, outlines of correctives, applied to certain modern errours in physick / by John B. Davidge.

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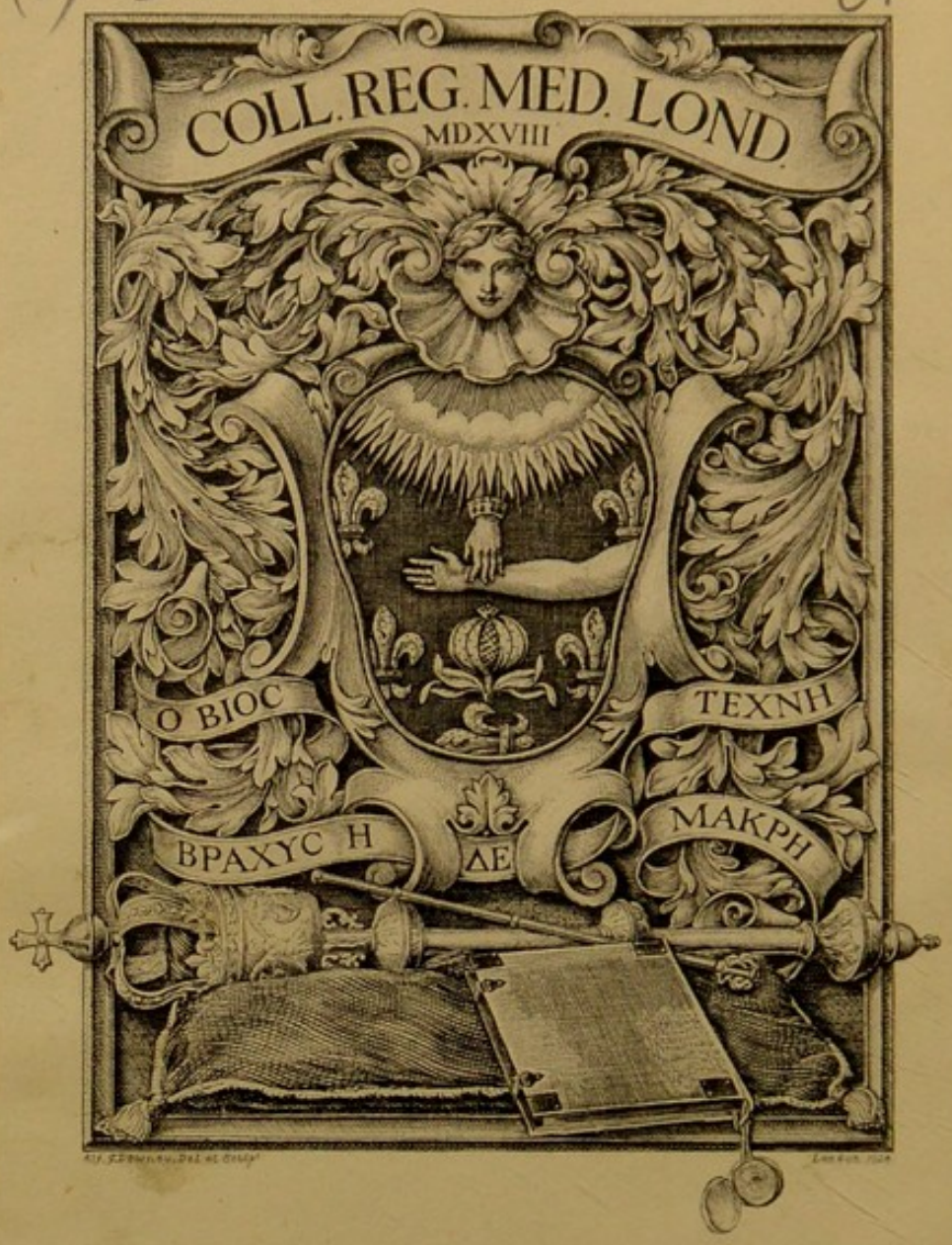

To D^r [redacted] of London,

with the respectful con-

sideration of the Author

Jan^y 15^th 1815

PHYSICAL SKETCHES,

OR

OUT LINES OF CORRECTIVES,

APPLIED TO

Certain Modern Errours in Physick ;

BY

JOHN B. DAVIDGE, A. M. M. D.

Professor of the Institutes of Physick and of Anatomy in the University of Maryland.

Quantula sunt hominum opuscula examen solum fatetur.

BALTIMORE :

PRINTED BY WILLIAM WARNER.

.....

1814.

DISTRICT OF MARYLAND, ss.

BE it remembered that on this Thirteenth day of December, in the Thirty-Eighth year of the Independence of the United States of America; John B. Davidge, of the said district, hath deposited in this Office, the Title of a Book, the right whereof he claims as Author; in the words and figures following, to wit—

SEAL.

"Physical Sketches, or out lines of correctives, applied to certain modern Errours in Physick; by John B Davidge, A. M M. D. Professor of the Institutes of Physick and of Anatomy in the University of Maryland. Quantula sunt hominum opuscula examen solum fatetur."

In conformity to the Act of the Congress of the United States, entitled, "An Act for the encouragement of Learning by securing the copies of Maps, Charts, and Books, to the authors and proprietors of such copies during the times therein mentioned."

And also to the Act, entitled, "An Act Supplementary to the Act, entitled, "An Act for the encouragement of Learning, by securing the copies of Maps, Charts, and Books, to the authors and proprietors of such copies during the times therein mentioned," and extending the benefits thereof to the arts of Designing, Engraving, and Etching, Historical, and other Prints.

PHILIP MOORE, *Clerk,*

of the District of Maryland.

INDEX.

PREFACE.

IN the present volume the author takes some notice of the opinion of writers on the menstruous operation in the human female, and endeavours to show that little or nothing of correct science had enlightened the path of the philosopher until 1793, the period at which the author's sentiments were first given to the world.

However respectful the manner may be in which the author refers to Hunter, Denman, &c. it is very much of a fact that he received from the authorities alluded to but slender aid, their ideas being rather indeterminate and equivocal, by no means moulded into the form of a doctrine, or assuming the order of a principle. They appear to be rather an incidental conjecture than a digested result of a scientifick view.

The author brings before the reader some of the many errours of writers, especially those of his own country, on the Yellow fever, not omitting to point out as he passes on, the facility with which even learning and character are influenced by the magick power of a name.

The gratuitous doctrines of Dr. Jones of London, on the Pathology of wounded Arteries, is

not passed by in silence; nor does the writer forget the suppositious Aneurism of Philadelphia—that in which so much is said to suggest to a prudent understanding the advantage and propriety of always, with care, noting the marked distance between *opinion* and *fact.*

The author intends to pursue his plan of furnishing sketches or out lines of correctives on certain errours of modern works; and although his talents may not be fully adequate to a successful management of the vast mass of materials provided to his hand, he has some hopes arising from his industry and application.

It is not to be understood that his hopes are very great of adding much to the fertility of the fair field of medical science, by directing through it many copious streams of knowledge from new and unexplored sources: he will have the measure of his expectations filled up, if he can succeed in giving a proper diversion to the inundating tide that has been for years setting in upon it. To act negatively is not at all times to act uselessly.—To be disabused of old errours is to be put into the way to acquire new truths.

DISSERTATIO PHYSIOLOGICA

DE

CAUSIS CATAMENIORUM

QUAM

ANNUENTE SUMMO NUMINE

ET AUCTORITATE DIGNISSIMI VICE-CANCELLARII

ARCHIBALDI DAVIDSON, S. S. T. P. P.

Et Collegii Glasg. Praefecti:

NEC NON

Amplissimi Senatus Academici consensu et nobilissimae Facultatis Medicæ decreto;

PRO GRADU DOCTORIS

Summisque in medicina honoribus et privilegiis rite ac legitime consequendis,

IN COMITIIS

UNIVERSITATIS GLASGUENSIS,

Eruditorum examini subjecit

Joannes Beale Davidge, A. M.

Marylandiæ civis

SOCIET. AMER. PHYS. EDIN. SOC.

ET PRÆSES ANNUUS

Ad diem xxii Aprilis. hora locoque solitis GLASGUÆ.

M,DCC,XCIII.

MEDICIS

JACOBO, ET GULIELMO MURRAY

SUB QUORUM AUSPICIIS,

PRIMUM FUNDAMENTUM JECIT,

EXERCITANTIBUS MEDICAM ARTEM

DEBITA LAUDE

De Annapolis

SUMMÆ CHARITATIS PERTENUE SIGNUM,

HOC OPUSCULUM

DEDICAT

CONSECRATQUE

AUCTOR.

DE CAUSIS CATAMENIORUM.

INTER plurima, de re medicâ, argumenta, quod ex iis, hac in dissertatione, seligerem, quodque, optimo cum successu, tractare possem, diu et multum dubitavi. Omnibus vero rite et considerate perpensis, pauca de catameniis proponere mecum statui. Quibus ex causis oritur hic sanguinis fluxus, certo ætatis tempore, vel quid sit naturæ huic respondens consilium, in corpore fœminino, nemo adhuc, sive medicus, sive physiologus, pro certo dicere ausus est. Inter facultates aut functiones corporis humani nulla est de quâ difficilius ratio reddi potest. Multi quidem physiologi, tum veteres, tum recentiores, ad hanc rem explorandam, operam dederunt: ibi vero, me saltem judice, labor eorum omnis effusus est, si quas in lucem ediderunt hodierni observationes excipiantur.

Nihil quidem, de hac re, novi, pauca tantum, quibus sententias Hunteri, Denmani, aliorum que medicorum stabilire et confirmare possumus, in animo est mihi proferre. Hoc modo igitur, ut spero, ex memoria hominum detrahere et oblivioni æternæ tradere istas opiniones falsas æque ac absurdas quæ tamdiu animis eorum arrisêre, nonnihil valebo.

Inter tot tamque diversas opiniones, quarum nonnullæ a viris illustribus traditæ sunt, quid sit verum, quid falsum, ut juvenem et tyronem decet, explorare conabor. Rem vero religioni non habebo, quod interdum a sententiis medicorum, etiam clarissimorum discederem. Me hisce verbis utente, quin invidiam, forsitan et contumeliam mihimet paravero, haud incertus sum. His autem magis ire obviam, quam falsum pro vero positum indecoro silentio præterire volo. Hujus rationem fluxus reddere vel potius attentionem nominum ad rationes exquerendas incitare, mihi consilium est. Propter sententias ab aliis diversas, hac in dissertatione prolatas, me facere insignem, si possit fieri, non in animo est. Ne quis igitur vitio vertat, si opinionibus aliorum non semper accederem. Quâ modestia viriumque suarum diffidentiâ uti juvenem decet, suam de rebus omnibus non minus quam medicis, sententiam in lucem prodentem, non omnino ignoro. Hic vero recordari nos oportet, tyrones sæpe ac imperitos, quæ maxi-

mo sunt usui vitæque humanæ accommodatissima, multa feliciter invenisse. Hanc tamen rem adeo obscuram non confidentissime aggredior. Quod si quae dicenda sint, ad justas causas mensium fluxus, vel, ut jam dixi, animos hominum eas explorare quoquomodo ducant, satis habeo.

Non ab re alienum videbitur, observationes tradere perpaucas de sententiis eorum qui de catameniis scripserunt. Imprimis autem, de signis et mutationibus quæ primum mensium fluxum præcedunt et comitantur, pauca præmittere necesse erit.

Vix dicere possum, an alia ulla in natura sit operatio, varios inter ordines sive plantarum sive animalium, quæ tam varios tamque diversos edit effectus, quam quas subit puella mutationes tempore pubertatis. Totum fere corpus mutatur. Forma puellæ universa magis fit gracilis, crescit magnitudine uterus, pubes venit, mammæ efflorescunt, pulchrior renidet vultus, splendent oculi, incessus, gestus, sermo, omnia denique fiunt magis decora.

Tempus, quo primum menses erumpunt, non omnibus idem est; variat enim regionis temperie, vitæ conditione forsitan et multis aliis causis. Maturius pubescunt puellæ in regionibus calidis, in frigidis vero tardius. In regionibus calidis, menses quoque multo citius quam in frigidis sese manifestant. Hoc puellis Asia-

ticis annum ætatis octavum vel nonum agentibus, accidere dicitur: quo tempore, fœminæ fiunt, priusque matres factae sunt, quam annum ætatis decimum compleverint. In regionibus frigidis multo serius accidit, neque, exceptis paucissimis exemplis, primum se ostendit ante annum ætatis vicessimum.

Quantum variat tempus quo primum eveniunt menses, in diversis regionibus, tantum variat quantitas sanguinis ejecti. In regionibus frigidissimis, tres uncias, in calidioribus usque ad sexdecim, per mensium fluxum, mulieres amittunt. Certam pro regionis situ proportionem ratam spectat hujus fluxus quantitas. In his regionibus, nempe in Britannia, sex vel octo unciæ sanguinis, singulis mensibus, hoc modo, fluere censentur.

Puellæ Britannicæ, sana valitudine fruentes, circiter annum ætatis decimum quartum vel quintum, primum mensium fluxum patiuntur. Quo tempore, languor, et lassitudo insolita, pandiculatio, dolor lumborum, ventriculi, et capitis, vertigo, nausea, interdum et vomitio, tumor mammarum aliquando et dolor, aliaque his signa similia puellam fatigant. Hæc signa jam recensita mensium fluxum in genere præcedunt, durantque nisi per intervalla brevia, donec sanguis ex cavo uteri prorumpit. Si vero, hoc tempore, aliquid exitui ejus per uterum obstet, fluxus est ex naribus, pulmonibus, ventriculo, ano, oculis, gingivis, mammis, umbilico, minimo manus

digito, aliquando et aliis insuetis corporis partibus.*

Sanguinem, ex utero, tempore mensium fluentum, esse sincerum, putaverunt nonnulli. De hac re, autem, maxime dubito: alii enim, hunc sanguinem menstruum non esse congulabilem, neque ullam habere similitudinem sanguinis sinceri naturæ, nisi colori ejus, asseruerunt. De his, autem, mox plura dicenda sunt.

Hic sanguinis fluxus, mulieribus per magnam vitæ partem naturalis est, et valetudini earum maxime necessarius, singulis mensibus occurrit, si uterum non gerunt, quo tempore, rarissime se manifestat. Non desunt qui, menses, more solito, per totum graviditatis tempus, permansisse memoriæ tradiderunt. Sed paucissimis in exemplis, quibus res ita sese habuerunt, tale profluvium pro hemorrhagiâ morbidâ censeri debetur: forsitan et ex arteriis uterinis, minime effunditur, sed ex vasis in vaginam hiantibus. Satis enim constat, quo tempore, conceperit mulier, os uteri, materiâ mucosâ, occlusum esse. Sanguis igitur, more mensium solito, ex cavo uteri gravidi, fluere non potest. Hujuscemodi profluvia, maxima, tenero embryoni, incommoda, mortemque, fortasse ipsam, afferrent.

Menses quo ex tempore, mulieribus primum contingunt, certis intervallis redeuntes, perque

* Vide De Graaf, page 129.

trium, quatuor, aliquando et sex dierum spatium fluentes, vix ante quadragessimum quintum et fere ante quinquagessimum ætatis annum desinunt. Sed quibusdam in casibus ad provectiorem ætatem fluere perstant. Non desunt exempla, quamvis rarissima, quibus menses sub pubertatem proruperunt. Medicus ille Alexander Monro pater, de quâdam puellâ, mentionem fecit, cui menses occurerunt, tertio quidem anno ætatis.*

DE CAUSIS EXCITANTIBUS.

De mensium causis, varias multumque inter se diversas opiniones, in scriptis medicorum invenimus. Multa quidem vana et futilia, tum de causis, tum de effectibus eorum, tradiderunt physiologi. Causæ eorum, ut quibusdam visum est, ex cursibus lunaribus deducendae sunt, Alii, autem, probare conati sunt, eas in bile existere. Plerique, vero, inter recentiores, medici, de hâc rê disceptantes, vel ex congestione sanguinis uterini, vel plethorâ corporis universali, vel partibus corporis jam evolutis, vel arteriarum densitate, causas quæsiverunt. Hodie quidem, multi, menses oriri, ex unâ vel alterâ harum causarum quas nunc enumeravi, se habent persuasos. Nostro autem judicio, qui putant, menses esse secretionem ex arteriis uterinis, quæ pendet ex conditione ovariorum peculiari, proprius ad veritatem accesserunt! Nunc vero, pauca, de rationibus, quibus, suas, de

* Vide, page 422.

hac re, opiniones confirmare potuerint medici, in lucem proferam.

Vix opus est verbis, opinionem refellere, de lunæ effectibus in mensibus ciendis. Si enim luna hoc efficere valeat, mulieribus omnibus, eodem fere tempore, menses contingerent. Sane res ita non fiunt. Namque, omnibus cursus lunaris temporibus, hunc mensium fluxum pati solent mulieres. Igitur, quam medicus Mead male finxit, de vi lunæ pro mensium causa opinionem ut falsam æque ac absurdam rejicere debemus. Quare autem rogare volo, menses non fluunt ante annum ætatis decimum quartum vel quintum, atque post quinquagessimum desinunt, quoniam corpus foemininum, per totum vitæ spatium, vi lunæ eadem semper afficiatur? Hic, ut mihi videtur, homines ad vana fingenda, quam ad rerum naturam investigandam proniores opinionibus tam absurdis fidem habere adducti sunt.

Alii vero physiologi, hac de re, disceptantes, hoc errore relicto, in alium abierunt. Scyllam cupientes evitare, in Charybdim inciderunt. Opinionem, menses originem ducere ex nimia bilis copia, vel ex natura bilis peculiari, non vero rationibus validioribus, neque observationibus magis certis, quidem protulerunt. Sed ut supra dixi, plura facere verba, has opiniones esse falsas ostendere, non erit necesse.

Ad aliam, de mensium causis, opinionem, animum advertamus. Menses ut quibusdam videtur, ex congestione sanguinis in uteri vasis, oriri censentur. Medici vero hanc opinionem proferentes, quo pacto fit, ut hæc sanguinis congestio, quæ, parvis incrementis in molem facta, certissimis intervallis prorumperet, non nobis dixerunt. Pro certo habeatur, ut plethora in aliqua corporis parte partialis brevi fiet universalis. Nunc autem de hâc plenitudinê corporis universali, a plerisque hominibus pro mensium causis habita, fusius dicam.

Pauci quidem ante tempus praeclari Culleni, hanc opinionem ita investigaverunt, ut rei adeo obscuræ multum lucis attulerunt. Cullenus vero ipse, quamvis, acumine ingenii solito, hanc rem explicare conatus est, tamen caligine atra obvolutam reliquit. Sententiam ejus pro mera hypothesi habere oportet; tamen lubentissime concedam esse hypothesin ingeniosissimam. Asserit ille uterum esse evolutum post alias corporis partes; et igitur hæ partes sanguini a corde demisso resistentes, in laxa vasa uteri eum nempe sanguinem determinant; hæc vasa cedentia impetui sanguinis impingentis, donec tandem rumpuntur, exitusque ad sanguinis fluxum datur; atque, ut ait ille, alia fit accumulatio per mensis spatium. Sic ille disseruit. Sed haud scio unde oritur hæc nimia sanguinis abundantia quæ mulieribus omnibus in eadem regione degentibus, eodem ætatis tem-

pore, congeritur, et singulis mensibus fluere perstat, donec arteriæ uterinæ tam firmæ densæ que factæ, sanguinis exitum omnino impediunt.

Ad opinionem eorum qui putant mensium causas oriri ex plenitudine corporis, quæ a quadam conditione ovariorum pendet haud facile accedam ; neque aliam plenitudinem præter naturam, in corpore existere, credere possum.

Ceterum affirmat Cullenus, arterias uterinas non esse equales, quoad firmitatem vel densitatem, arteriis aliarum corporis partium, sed laxiores et infirmiores esse: atque satis constat, incrementum omnis corporis partis ex toto pendere a fluxu sanguinis in eam demisso. Uterus vero, putat ille, tardius evolvitur aliis corporis partibus, et hoc tempore, determinatio est sanguinis ad uterum, cujus arteriis impetui sanguinis cedentibus, menses fluunt, et fluere perstabunt, solitis intervallis, donec arteriæ uteri tam densæ et firmæ fiunt, ut sanguini ab aliis corporis partibus demisso resistere valeat, quod accidit cum menses desinunt.

Nunc autem observandum est, quam maxime erravit Cullenus, cum rationes, de hac re redderet. Nam si aliæ corporis partes firmæ ac validæ factæ impetui sanguinis resistunt, et cum in vasa uteri laxiora et infirmiora determinant, non possit fieri, ut hæ partes, figuram justam, et firmitatem adeptæ, post mensuim

fluxum magis increscerent. Res vero aliter se habent.

Satis enim constat, totum fere corpus muliebre, postquam menses acciderint, magna incrementa accipere, et quibusdam in casibus, quoad formam, figuram, et firmitatem multum mutari. His igitur rite perpensis, fidem opinioni Culleni habere non possum, menses a successione evolutionis, ut dicit ille, pendere.

Ad quam protulit Charletonus, de hac re, opinionem, nunc animum advertamus. Putat ille, menses originem habere a quodam spiritu nitro aereo. Ejus verba sunt, " *Utut hæc res* " *se habeat, mihi quidem verisimile videtur, spi-* " *ritum nitro aereum in hoc opere choragum esse;* " *habemus itaque tandem jam verisimilem saltem,* " *si non verum menstrui ardoris in utero mulie-* " *bri originem.*"

Valde miror, quomodo fieret, ut Charletonus, erroribus et falsis sententiis Hyppocratis, Galleni, aliorumque antiquorum, ab illo in lucem prolatis, repudiatis, opinioni tam absurdæ fidem haberet. Hic vero nobis attulit certissima documenta, quo facilius est hominibus, errores aliorum exponere quam veritatem investigare: male facere, qui vult, nusquam non causam inveniet.

Hisce igitur observationibus de sententiis aliorum jam traditis, rem, ut se habet, quoad po-

tero, in animo est mihi tractare. Atque ut solent sapientissimi mortalium, quum in rebus dubiis et obscuris investigandis versantur et viam ad veritatem tramite recto, persequi nequeunt, quam habent aliis rebus magis apertis et perspicuis analogiam invenire conantur. Horum exemplo adductus de re adhuc satis obscura, meam quoque sententiam in medium proferam.

Neminem latet, omnes humani corporis secretiones ex toto pendere a conditione organi secernentis perfecta, et prius organum ad conditionem perfectam pervenit, quam secretiones illi proprias perficere potest: cæterisque paribus, quo magis alicujus organi conditio, eo melius suas præstat functiones. Exempli gratia, conditio peculiaris hepatis, testium, et mammarum, pro bilis, seminis, et lactis, secretione, maxime necessaria est. Haud scio, quibus ex causis oriuntur menses, si non signa censentur certa, quibus certiores facti sumus, uterum esse perfectum et ovaria evoluta, atque ad munera sibi propria perficienda, apta. Menses semper fluxerunt, ante quam mulier uterum gerit. Nostro autem judicio, non pro causis, ut nos docuit professor Simpson, sed tantum pro signis plenæ pubertatis hic fluxus haberetur.

Ratio igitur in promptu erit, cur menses mulieribus non citius contingunt. Quare, rogare volo, neque semen secernitur pueris, neque lac puellis, quinto vel sexto ætatis anno? Huic res-

pondeatur, quia testes et mammæ, organa istos humores secernentia, ad conditionem perfectam, qua suis propriis muneribus fungantur, non adhuc pervenerunt.

Sed, ut supra memoravi, nonnulli auctores, tum veteres, tum recentiores se habent persuasos, menses oriri ex plenitudine corporis vel universali vel partiali, quæ, tempore pubertatis maxime valet. Quo fit, ut omnibus puellis certa ætate, inque eadem regione, menses acciderint, difficile erit ex hac opinione rationem reddere. Satis enim constat, ut puellae, quamcunque vitam agant, vel qualicunque victu utantantur, hunc mensium fluxum patiuntur. Et si quando menses, solito tempore, non fluxerint, tamen naturæ conamina ad hoc efficiendum non omnino deficiunt.

Unde igitur oritur hæc corporis plenitudo quæ longe lateque grassatur ; cui obnoxiæ et opportunæ sunt omnes puellæ, cujuscunque temperamenti, sive vitam in urbe aut in rure agerent, sive laute viverent, aut tenui et parciore victu uterentur ?

Si autem menses neque ex lunæ imperio, neque bile, neque spiritu nitro aereo, neque ex plenitudine corporis proficiscuntur, quibus ex causis oriuntur? Qua ex causa oritur seminis secretio ex vasis spermaticis? Certe ex conditione testium fæcundante. Eodem quoque mo-

do, mensium fluxus a conditione ovariorum fæcundante pendet. Fluxus iste igitur censeri debet secretio ex arteriis uteri quibus stimulo est conditio ovariorum perfecta. Joannes Bell eandem opinionem prælectiones suas anatomicas audientibus tradidit.

Professor Munro, medicus celeberrimus, cum sententias de hac re, in suis prælectionibus utilissimis proferret, menses a peculiari ovariorum conditione pendere, asseruit. Idem vir illustris addere vult, hanc conditionem ovariorum plenitudinem corporis universalem efficere.

Certe scio, cum discederem a sententiis hujus viri clarissimi, ut homines arrogantiam, fortasse et ignorantiam, mihi attribuent. Mihi vero non est consilium, si fieri possit, aliquid detrahere nomini ejus, quem plurimi facio, et maximum et solertissimum medicum lubentissime agnosco. Non cupidus novi, sed rerum causas cognoscendi, ab illo dissentio. Et si in hoc erraverim, omnes bonos ac candidos iterum iterumque oratos volo, ut ne mihi vitio vertant, et me amore veritatis captum in errorem adductum esse meminissent.

Cum Professore Munrone, de hac re, doctissimo et ingeniosissimo Professori Jacobo Gregory in sententiis prœlectionum suarum traditis, convenire placet. Putat ille quoque plenitudinem corporis, ex qua oritur mensium fluxus,

quadam conditione ovariorum effici. Sed quomodo fit hæc plenitudo in corpore, neque hic, neque ille, demonstravit, et quomodo fiat confiteor me omnino ignorare.

Diffidens animi hanc rem aggredior ; namque bene scio meas vires eam tractare, ut vellem, non sufficere. Tamen nos oportet cavere ne addiceremur in verba jurare ullius magistri ; me enim judicante, multum est periculi, et haud ignarus sum, ut medici et philosophi nomina et auctoritates magnorum virorum pro validis rationibus sæpe adducunt, et nos opinionibus eorum magnis nominibus et auctoritatibus tantum firmatis assentire volunt. Non quidem est tyronis, sine ulla investigatione fidem habere sententiis aliorum. Si enim res ita esset, vera scientia semper fuisset inculta ; de circulatione sanguinis, de curatione variolæ, aut de vasis lymphaticis, nostras ad aures nihil unquam pervenisset.

Nunc igitur in animo est mihi meas sententias, de mensium causis, proferre, ut alios majore ingenii acumine dotibusque animi præstantioribus præditos operam dare huic rei investigandæ, excitem. Quamvis enim nonnulli putaverunt rem tam obscuram nunquam in lucem proferri posse ; tamen sperare possum, tempus non multum abesse quum erit manifesta ; et fortasse invenietur, menses oriri ex conditione ova-

riorum fœcundante, quæ est stimulo arteriis uteri, ex quibus iste humor secernitur.

Multa quidem animum ad hanc opinionem ducunt. Nullas novimus puellas viro tempestivas, quibus menses non accederint. Aliis quoque animalibus lex est hujusmodi.*

In priore parte hujus opusculi, jam observavi menstruum sanguinem non esse coagulabilem more sanguinis sinceri, et huic sanguini nullam habere similitudinem nisi rubedini ejus. De hac re, autem, nunquam mihi dabatur occasio ulla experimenta faciendi. Ex experimentis aliis institutis hoc accepi. Quamvis autem sanguis menstruus esset sincerus, ex hoc demonstrari non potest, plenitudinem adesse, vel quomodo arteriae uterinae partem sanguinis a generali massa separare possunt.

Si vox plethora audit justam et ratam sanguinis quantitatem, quae distentioni et actioni vasorum necessaria est, non ut vulgo significat conditionem corporis morbidam, in hoc sensu vox est absurda. Plethora in posteriore sensu tantum notat conditionem morbidam et laxitatem solidorum corporis indicat. Si accuratius dicere volumus *vi* aut *stimulo* uti debemus, quo facilius lis dirimi possit.

Satis constat, actiones omnium arteriarum a sanguine in iis contento ex toto pendere. Iis

* Vide Spalanzani.

sanguis est stimulo, quo functiones earum rite praestent. Haec quantitas sanguinis vasa stimulat in eodem modo ut calor, et haec quantitas sola necessaria est ovaria stimulare ad munera propria perficienda. Ovaria sunt stimulo arteriis uterinis quae humorem menstruum se cernunt.

Quo tempore primum menses erumpunt puellis, pubes venit, mammae turgent, incalescit uterus, genitalia suis functionibus apta sunt, quæ omnia nobis sunt certissimis signis jam esse viro maturas. Si quando, ut supradixi, fluxus sanguinis per uterum impeditur, fluxus est ex ventriculo, pulmonibus, oculis, aliisque corporis partibus. De his autem mox fusius dicam. Nunc pauca de recursibus mensium periodicis proferam.

Plura dicere, de opinione eorum qui mensium fluxum a lunae imperio pendere putaverunt, supervacaneum est. Quod dicendum est, de plenitudine partiali, æque ad universalem spectat. Omnes enim affectiones hujusmodi primo partiales, tandem fiunt universales. De hisce igitur opinionibus, hoc in loco, simul agere in animo est ; utpote sunt opiniones omnium fere medicorum, nonmulli quorum ante tempus Culleni plenitudinem esse corporis universalem demonstrare conati sunt.

Cullenus de plethora hypothesin finxit ingeniosam. Asseruit ille, primum mensium flux-

um a densitate et firmitate arteriarum aliarum corporis partium, quæ sanguinem ad uterum determinant. Arteriæ uteri jam evoluti laxiores et infirmiores impetui sanguinis cedentes, tandem rumpuntur et sanguis effunditur. Per intervallum mensis, alia est congestio, aliusque fluxus; atque ut solet accidere aliis hemorrhagiis, recursus hujus fluxus, certis intervallis consuetudine confirmatus est.

Cullenus autem, quo fit, ut hic fluxus tam accurate recurrat mensis intervallo, aut omnibus fere foeminis in eadem regione, eadem essent intervalla, non demonstravit: nec quidem demonstravit, quomodo fit recursus alicujus hemorrhagiæ certis intervallis.

Verbi gratia, fingamus plenitudinem adesse, atque octo uncias sanguinis per mensis spatium accumulari, et ex utero fluere. Cum ratione conveniet, si sexdecim unciae sanguinis, die decimo quarto ante tempus solitum, per venae sectionem auferantur, non erit fluxus antequam duo menses elapsi fuerint, vel mensis cum semisse. Si igitur causa hujus fluxus occasionalis ex congestione per mensis spatium facta originem habet; duplice sanguinis quantitate per mensis intervallum detracta, triplex fere temporis spatium elabi ante alium fluxum, necesse erit: quippe quod tempus duorum mensium ad congerendam sanguinis quantitatem detractione missam requiritur et prius pars tertii mensis

elapsa fuerit, quam congestio et fluxus iterum acciderent. Sanguis autem, hoc modo, missus, neque tempus neque quantitatem mutare potest. Medicus Simpson, ni fallor memoriae tradidit, decem libras per venæ sectionem missas, quantitate et intervallis menstrui fluxus iisdem manentibus.

Ex his igitur, ut mihi videtur, menses a plenetudine corporis non pendere, manifestum est. Aliud autem est argumentum ex eo quod accidit foeminae cui per errorem ovaria excidit Percival Pott. Ante hoc tempus huic foeminae menses rite et copiose fluxerunt. Postquam autem res tam infelix evenerat, mammae subsiderunt, omniaque signa solita mensium fluxum comitantia desiverunt, seque nunquam postea ostenderunt. Hisce igitur animo volventibus, dubitare non possum, quin menses majis pendeant a conditione ovariorum, quæ est stimulo utero, quam a plenitudine corporis.

Nunc quomodo redeunt menses certissimis intervellis explicare oportet: Confiteor tamen, ut rationem hujus rei obscurae satis validam reddere non possum; quamvis me habeo persuasum ut explicari possit et rationem non multum abesse. Cur non redeunt gratæ vices diei noctisque intervallo hebdomadis vel mensis et non diei? quia legibus hominibus aliisque animalibus impositis, stimulus ad suas functiones perficiendas necessarius, paucis horis diminui-

tur, et quies et sopor ad eum reficiendum singulis diebus, requiruntur. Eadem ratione, hic fluxus singulis mensibus contingit foeminis. Si enim semel per anni spatium tantum accidisset, guadia hominis multum imminuta essent, et si nunquam accidisset, foeminae nunquam uterum gessissent: quia mensium fluxus ad secernendos humores faecundos necessarius esset. In aliis animalibus humor istiusmodi qui ex utero fluit non omnino deest.

Quo fit ut arteriae uterinae per paucas dies singulorum mensium hunc humorem peculiarem secernunt? Eodem modo quo secernitur lac ex arteriis mammarum post omnem partum. Putant nonnulli rem esse supra fidem tantum hujus humoris ex arteriis uteri secerni, quantum paucis diebus ex utero fluit. Non mirum est, si credere possumus quantum seminis secernitur ex spermaticis canis pro rato tempore in coitu.

Menses, ut observatum fuit a nonnullis auctoribus ex aliis corporis partibus fluxerunt; eodemque modo, quibusdam in casibus, bilis per vesicam transit. Hic, hepatis, ille, uteri, morbidam conditionem demonstrat. Interdum quoque bilis per ductum cholidochum impediatur.

Nunc pauca dicenda sunt, cur menses circiter annum aetatis quadragessimum quintum vel quinquagessimum amnino desinunt. Qui opinati sunt menses ex plenitudine oriri, quædam

argumenta hoc ex fonte deduxerunt. Illi putant, menses cessare hoc tempore, ex firmitate arteriarum uteri quæ sanguinis impetui resistunt. Vix credere possumus, arterias omnium foeminarum eodem tempore aetatis, aeque esse firmas ac densas, aut fieri minus capaces quo sanguinis fluxum impediunt. Haud quidem veri simile est. Alia ex causa oritur, ut mihi videtur, nempe, quia, hoc tempore, ovaria conditionem faecundantem omnino omittunt. Namque providens natura posuit legem, ut menses fluere cessarent, et ovaria conditionem quae est stimulo utero amitterent, cum mulieres non diutius nutrimentum faetui præbere possunt.

DE CAUSA FINALI.

Adhuc quidem in dubio est, quibus usibus inserviant menses. Multi putant esse nutrimento faetui. Nonnulli, tum veteres, tum recentiores, hanc opinionem protulerunt. Quia menses desinunt, dum uterus est gravidus, putatur, sanguinem qui erumpere solebat, per mensium fluxum pro nutritione embryonis, deteneri.

Per aliquod temporis, postquam mulier conceperit faetus in utero fabæ aut vespae vix aequalis est magnitudine. Hoc tempore igitur minima nutrimenti quantitas satis esset. Quo vadet nunc ista pars sanguinis quæ nutrimento faetus superest? Si sanguis menstruus huic usui dicatus fuerit, principio graviditatis

nimis abunderet, ad finem vero multum deficeret. Sunt autem multa alia animalia quae menses non habent; tamen faetus eorum non orbantur nutrimento. Cur igitur auctores credideruntmenses esse nutrimento faetui in humano genere, cum aliter res se habet in aliis animalibus? Ratio in promptu est, quia veras mensium causas non investigarunt quidam auctores putaverunt menstruum sanguinem esse foedum et perniciosum. In legibus Leviticis, de hac re, nonnulla sunt praecepta. Hac vero, ut mihi videtur, ad munditias populi istius, quam ad aliquid immundum vel lethale in menstruo sanguine magis spectant.

Igitur, ut res mihi apparet, consilium est naturae in mensibus ciendis, ut certa sint signa uterum esse maturum. Ex hoc explicare possumus, cur menses desinunt, dum uterum gerit mulier; quia hoc tempore, ovaria amittant conditionem faecundantem, quae est stimulo arteriis uteri. Itaque menses tempore graviditatis omnino desinunt, nisi in paucissimis casibus hisque morbidis.

Quam finem dicendi fecerim, ante meo amico et socio Joanni Thomae Shaaf medicinae Doctori, aliisque omnibus amicis maximas agam gratias, ob multa et magna beneficia in me collata.

" Ante leves ergo pascentur in aethere cervi
" Et freta destituent nudos in littore pisces ;
" Ante, pererratis amborum finibus, exul
" Aut Ararim Parthus bibet, aut Germania Tigrim
" Quam nostro ipsorum labatur pectore amor."

TANTUM.

OF THE

CAUSES

OF THE

MENSTRUAL ACTION.

Which of the various subjects, appertaining to Physick, and suitable to my present purpose I should chuse, and that on which I could enter with the greatest prospect of success, I for a considerable time and seriously doubted. But, maturely and calmly weighing all things, I have resolved to offer a few remarks on the Menstruous Fluid of the human female. From what causes this sanguineous effusion has its origin; or what is the particular end of the economy of nature in establishing the menstrual operation in the female body, neither Physician nor Physiologist, has as yet ventured confidently to assert. Among the various faculties and functions of animal life there is none more difficult of explication. Many Physiologists, it is true, as well ancient as modern, have turned their attention to this subject, but in my estimation, if we

except a few of the present day, they have stopped very far short of success; their labour has been lost.

To adduce any thing altogether new on this subject is not my intention; my object reaches no farther than to advance a few arguments, which may tend to give stability and firmness to the sentiments referred to by Hunter, by Denman, and by one or two other medical men. In this manner, I flatter myself, I shall be able, in some measure to loosen the attachment of gentlemen from those false and absurd notions, which have so long amused their fancies, and diverted their judgments, and to turn over to eternal oblivion those speculations themselves.

As becomes a young man and a learner, I shall endeavour to discriminate between the true and the false of the numerous and various opinions which have come down to us; some from illustrious characters. But I shall not hold it to be a point of conscience never to depart from the path of other Physicans, even the most celebrated. In adopting this language, I know that I shall procure to myself censure, perhaps contumely: but I had rather run the hazard of all such consequences than silently to permit errour to occupy the place of truth. My object is an enquiry into the nature of the menstrual operation; or rather to awaken the attention of others to the research. But I have no expec-

tations of acquiring fame by a mere opposition of sentiment. Therefore let gentlemen be sparing of censure although I should not accord exactly in opinion with those who have preceded me. I am not now wholly to learn that it is becoming a young man not only in medical, but all other matters, to come forward with a proper degree of modesty, and distrust of his own powers ; nevertheless we must never lose from our recollection, the important fact, that some of the most valuable and useful discoveries have been fallen on by young, and even ignorant men. However I shall approach a subject so obscure with no slender caution, and if what I shall say may lead to a discovery of the true cause of the Menstruous fluid, I shall feel satisfied.

To deliver a few remarks on the sentiments of those who have turned their attention to the menstrual operation, may not be deemed irrelative to our subject: and especially it may be necessary to premise a few observations on the signs and phenomena which precede and accompany this eruption.

It is highly problematical, whether there be in the vegetable or animal kingdom, any natural operation in which there is so remarkable a revolution as that to which a young maid is subject at the period of pubescence. Almost the whole body is changed. The universal form of

the virgin becomes more soft and winning, the uterus enlarges, the marks of puberty appear, the bosom evolves, the countenance is more beautiful, the eyes sparkle, the step, the carriage, the speech; in short all is grace and elegance.

The time at which the menstruous action takes place is not the same in all. It varies according to climate, conditions of life, and various other causes. Girls in the warmer regions are pubescent sooner, those in the colder later. The same is also true of the eruption of the menstrual fluid. This change is said to occur with the girls of Asia so early as during the eighth and ninth year; at which time they are women, and become mothers before they shall have completed their tenth year. But in the colder latitudes, this change does not occur until much later; nor indeed, except in very few instances, previous to the twentieth year.

The periods at which the eruption takes place in different latitudes, do not differ more than the quantities discharged. In the very cold, about three ounces, in the very warm, to the extent of sixteen, are thrown off. The quantity bears relation to the latitude. In this climate, viz. that of Britain, from six to eight ounces of blood are supposed to be the mean quantity discharged.

The girls of Britain, enjoying good health, for the most part; experience this function about the fourteenth or fifteenth year of their age. At this epoch they are more or less annoyed by unusual languidness, and weariness, a disposition to yawn, pains of the loins, of the stomach, and of the head, giddiness, inclination to vomit, and sometimes absolute vomiting; tumefaction, and, at times, pain of the breasts, and other similarly uncomfortable feelings. Those sensations, in a greater or less degree, continue at intervals, until the fluid begins to pass off from the womb. But should any impediment oppose its passage through the womb, at this time, it has been observed to flow from the nose, the lungs, the stomach, the fundament, the eyes, the gums, the nipples, the navel, the little finger, and other unusual parts.

The red fluid which flows from the womb during these particular periods has been supposed by some to be pure blood. But of the correctness of this supposition we very much doubt. For it has been advanced and maintained that it is not coagulable, and that, except in colour, it has none of the sensible properties of pure blood. But of this more presently.

This dicharge of red fluid is proper to women during a considerable part of life, and is very necessary to the maintenance of health. It occurs monthly, the periods of gestation be-

ing excepted, at which periods it very rarely takes place, but instances have been furnished where this monthly discharge has regularly, in its usual form, occurred during the whole term of gestation. The instances, however, are very few in number where matters have been so uniform and regular; and, when they do arise, should be viewed as morbid: and perhaps in those rare cases the fluid may more properly be imagined to be furnished by the vessels of the Os Tincæ opening into the Vagina than by the uterine vessels belonging to the hollow of the womb. For it is beyound a doubt that, at the time of conception the mouth of the womb is closed by mucous matter. Therefore the fluid, in the customary way in which it passes off, cannot be discharged from the inside of the womb. An effusion in the usual way would necessarily bring a serious incommodity on the child, if indeed it did not endanger its life.

This menstruous fluid, from the period at which it first flows, returns periodically every month, and continues at each return, about from four to six day, does not wholly cease until the forty-fifth or fiftieth year of the woman's age. There are not wanting examples, although extremely rare, where this menstruous operation occurs under puberty. Dr. Murno the father, makes mention of a little girl, in whom this operation took place even at the tender age of three

years, and continued regularly thereafter through the ordinary space of time.

OF THE EXCITING CAUSES.

We meet with various and greatly different opinions in the writings of Physicians on the causes of the menstrual operation. Indeed Physiologists have amused themselves with many conceits and visionary speculations as well on the causes as on the effects of this operation. Some have traced the causes up to an imaginary influence of the moon, and others have laboured to prove that they resided in the qualities of the bile. But most of the moderns who have treated of this matter have attributed the causes either to a local congestion of blood in the uterine vessels or to a general plethorick condition of the whole body, or to an unequal evolution of the different parts of the body, and density of the arteries. At present indeed there are but few who do not consider the menstrual action to depend on the one or the other of the causes recited. But in our estimation those who view this fluid as a *secretion* from the uterine vessels under peculiar ovarious influence, approach much nearer the truth. And we will now detail a few of the reasons, by which, the advocates of this opinion might establish their sentiments.

A few words may suffice to set aside the conjecture of the menstrual operation being attributable to lunar influence. If the moon ruled, in this affair, it is presumable that all women would be under the operation at the same moment. This is not the fact. For at every period of a lunation some one or other is under this operation. Therefore this visionary notion of Dr. Meed is to be rejected both as unscientifick and futile. We would ask, on this head, why does not the menstrual action take place before fourteen or fifteen or why does it cease after fifty years, seeing that the female is during the whole of life equally exposed to lunar power? Here, as it appears to me, we have a striking illustration of the greater disposition to indulge in absurd and ridiculous conceits than to engage seriously in the sober investigation of the real nature of things.

But other Physiologists, keeping void of one errour, have fallen on its opposite; desirous of avoiding Scylla, they have been wrecked on Charybdis. The opinion advanced by certain gentlemen that the menstruous operation derives its origin from too great a quantity or a peculiar quality of the bile rest upon ground as untenable and precarious as the sentiment noticed above. And to say more to demonstrate the incorrectness of those opinions would certainly be a waste of words.

We will direct our attention to another hypothesis touching the subject. The menstrual operations have been conceived to depend on a congestion of blood in the uterine arteries. But the Physicians who have favoured this conjecture have not instructed us how it comes to pass that congestion accumulating by slow degrees into a considerable mass should burst forth at given intervals. And it may be received as an incontrovertible fact that every partial plethora, in any part of the body must in a short time become general. Therefore we shall speak some what at large on the general fulness held by almost every physiologist as the unquestionable cause of the menstrual operation. Few indeed antecedent to the time of the eminent Cullen had so investigated this opinion of plethora as to throw any considerable light on the subject. And Cullen, although not deserted by his usual acumen, unsuccessfully essayed this difficult point having found it immersed in great obscurity. Indeed we are permitted to grant this opinion of Cullen no higher rank than a hypothesis yet we most willingly acknowledge it to be an exceedingly ingenious one. He lays it down as his premise that the uterus is the last part of the body which is evolved, and consequently that the blood sent out from the heart, being resisted by the superiour strength of the arteries of the other parts, is determined into the laxer vessels of the womb, and these gradually

yielding to the force of the impinging mass ultimately give way, and an effusion of blood is the result. This course of things in the course of a month is repeated. Thus he taught. But I am as yet to learn whence this too great abundance of blood, in all women in the same climate, and at the same epoch of life is collected and continues to pass off, at every four weeks until the arteries shall become so firm and dense as altogether to resist its egress.

To the sentiments of those who suppose this plenitude of the general body to arise out of a peculiar condition of the ovaries I cannot readily accede; nor can I grant that the body at these periods possesses any preternatural repletion. But Cullen affirms, that the uterine arteries, as to firmness and density, are unequal to the arteries, of the other parts of the body; that they are more lax and feeble: and it is very obvious that the augmentation of bulk in every part of the body is entirely owing to the quantity of blood conveyed to it. The womb, as he supposes is of slower growth than the other parts, and, at the period of inequality, there is a determination of blood to this viscus, the arteries of which giving way there is an efflux of blood, which efflux continues to recur at certain intervals so long as the weaker or laxer condition of the arteries shall permit or so long as they shall

be overcome by the force of the blood sent from other parts; when the balance is established the menstrual action ceases.

We must now notice the egregious errour of Dr. Cullen in his philosophy on this point. If the other parts of the body, having acquried their tone and firmness, resist the force of the impinging blood, and determine it into the more lax and weak vessels of the womb, it could not happen that those parts already possessed of their proper figures and tone could acquire additional volume and power after the effusion of this fiuid. But the fact is otherwise. For it is notorious that, almost uniformly, the whole body, subsequent to the eruption of the menstrual fluid, does receive great increase of bulk, and, in most cases, as to form, figure, and tone, does undergo great change. Therefore we cannot, when maturely considering these facts, assent to the opinion of Cullen, that this fluid is ascribable, in its cause, to any succession of evolution of the several parts of the body.

We now turn our attention to the opinion of Charlton on this subject. He supposed that the menstruous fluid, in its causes, was attributable to a certain nitro-aerial spirit. His words are: "howsoever the thing may be, I will not determine, but it appears propable to me that a nitro-aerial spirit is the chief furnisher or setter forth in the work; therefore we shall consider it

as highly probable if not an absolute certainty that we have arrived at the true source of the meustrual ardour."

We are not a little surprised to find Charlton, after rejecting the errours and incorrect notions of Hippocrates, of Galen and of others among the ancients, by him so carefully introduced, admitting an opinion so positively absurd. But we are hereby furnished with the most unquestionable documents of the fact with how much greater facility we can successfully engage in the overthrow of the errours of others than in the establishment of truth on our own part. It is an easy matter to pick a hole in any man's coat.

Having advanced so far in discussing the sentiments of others, I shall now proceed, as my powers may enable me, to give my own views of this subject.

And as the most learned in investigating that which is doubtful and obscure, when they could not proceed by a direct and known path, have endeavoured by the analogous and that which approaches most to the open and plain. So I, conducted by their example, shall state my opinion on this very obscure and difficult subject.

It is palpable and obvious that all the secretions of the human body depend entirely upon the matured condition of the secreting organ,

and that every organ before it can perform the function proper to it, must arrive at a state of maturity; and, other things being equal, that in proportion to the maturity of an organ will be the perfection of its function. For instance, a mature condition of the liver, of the testicles, and of the breasts, is altogether necessary for the secretion of the bile, of the seminal fluid, and of the milk. Nor can I conjecture any other source of the menstruous fluid, except it is to be viewed as signs by which we are to understand that the womb is so prepared and the ovaries are so evolved as to be suited to the affairs for which they are destined. The menstrual action always takes place before a woman can conceive. But, in my opinion, is not so much to be considered as the cause, according to professor Simpson, as sign or result of full puberty.

We therefore feel at no loss about the reason why the menstruous fluid does not occur sooner in women. Why, we will ask, is not the seminal fluid secreted by boys, or milk by girls, at the age of five or six years? To this it may be answered that, at this age, neither the testicles nor breasts, the organs concerned in these functions have arrived at that degree of perfection necessary to the performance of these offices.

But as above remarked, there are certain writers, as well ancient as modern, who have been

induced to believe that the menstrual operation is attributable to a partial or general plenitude of the body which in an especial manner prevails at puberty. Although the premises be granted, it will be difficult to understand how it comes to pass, that all girls at a fixed period of life and in the same climate, should have this monthly discharge. For the truth is that all girls without exception, what life soever they lead or food they use are subject to this operation. And if it should so happen that the menstrual fluid does not pass off at the usual period the efforts of nature to accomplish such an end will nevertheless come into operation.

Whence arises this fulness of body which is so general and uniform, and to which all girls are so obnoxious, of whatever temperament they may be, whether they pass their lives in the city or in the country; whether they live luxuriously or whether they live on a spare and slender diet?

But if the menstruous flux be not attributable to the influence of the moon, or to the bile, or nitro-aerial spirit, or to a fulness of body, from what causes does it arise? From what causes proceeds the secretion of semen in the spermatick vessels? Assuredly from the impregnating capacity of the testicles. In the same manner, also does the menstrual action depend on a condition of the ovaries to be impregnated.

Hence this flux is to be received as a *secretion* from the uterine arteries acting in obedience to an excitement derived from a matured condition of the ovaries. In his prelections Jno. Bell, delivered sentiments of this cast to his pupils.

Professor Munro, a physician of the greatest celebrity, when discoursing to his pupils on this subject, asserted that the menstruous action depended upon a peculiar condition of the ovaries. This same illustrious man added that this condition of the ovaries produced in the female body a general plethora.

I am perfectly aware that, when I dissent from the opinions of a character so celebrated, I may be charged with arrogance, and perhaps ignorance. But it is not my intention if the thing were possible, to detract from the name of him, whom I most willingly acknowledge to be of the first standing, and, of physicians the most learned and sagacious. Nor is it from a fondness of novelty that I differ from this gentleman, but solely for the sake of enquiring into the real cause of the thing And should I err in this, I entreat all good and honest men, again and again, that they do not censure me but bear in mind, that captivated by the charms of truth, I have gone astray.

With Professor Munro, the very ingenious Professor Jas. Gregory agrees, on this head, as may be seen in his lectures. He also imagined

that the fulness of body, on which the menstrual action depended, arose from a peculiar condition of the ovaries. But how this plenitude of the body was produced, neither Munro nor Gregory has demonstrated, and how it can take place I confess myself totally at a loss to conjecture.

On giving my own opinion I now enter with diffidence, for I well know that my powers are far inferior to my undertaking. Yet it becomes us to be careful to avoid too strict a regard for the authority of our teachers, for in my opinion, and of the position I am thoroughly satisfied, both Physicians and Philosophers are often disposed to substitute names and authorities for solid reasoning, and are solicitous that we should admit their opinions because they are coupled with great names and high authorities. But it is not the province of a learner, without investigation, to pay too much respect to any opinion. For if so, true science would ever have remained rude and uncultivated: of the circulation of the blood; of the cure of the small pox; of the lymphatick vessels, no knowledge would have been handed down to us.

I now therefore shall bring forward my sentiments that I may at least rouse, those of better genius and more enlarged intellect to an investigation of the matter. For although some despair and believe a scrutiny into so obscure a subject must ever remain fruitless. I cannot be persuaded that the period is distant when this mat-

ter will be developed; and perhaps it will be discovered that the menstrual fluid does depend on a disposition of the ovaries to be impregnated, and is a *secretion* from the uterine vessels acting in obedience to this ovarious influence.

Many reasons indeed urge us to the adoption of this opinion. We know that girls are not marriageable until this operation shall have taken place. Even some inferior animals are subject to a similar law.

In the former part of this treatise, I remaked, that the menstruous blood was not, like genuine blood, coagulable, and that it possessed no character in common with pure blood but its red colour. On this, however, I have never had occasion to experiment. I rest upon the experiments of others. But even admitting it to be genuine blood it would not follow that a fulness must be present; nor is it clear how the arteries of the womb could regularly separate a given quantity from the general mass.

If the word *plethora* means a just and proper quantity of blood, such as is necessary to the distention and action of the vessels, and not, as is commonly understood, a morbid condition of the body, it is absurd. Plethora, in the latter and proper sense, indicates only a morbid and relaxed condition of the solids of the body. If we would speak more circumspectly, and, when treating of the quantity of the blood, use the

word *power* or *excitement*, we might more readily come to a mutual understanding.

It is sufficiently obvious, that all the arteries must depend entirely on the blood contained in them. This blood serves as a source of excitement to them, enabling them to perform their proper offices.* A given quantity of blood stimulates vessels in the same manner the heat does. And this given quantity alone is necessary to stimulate the ovaries to the performance of their function. The ovaries are a source of excitement to the uterine arteries which *secrete* the menstruous fluid.

At the period at which the menstrual action first accours young girls are pubescent; their breast enlarge, the womb is excited, their genitals are evolved, and all the attendant phenomena give assurance of nubile maturity. If at any time, as above hinted any impediment be opposed to its egress from the womb, it flows from the stomach, lungs, eyes, or other parts of the body. But of this matter I shall speak more at large presently. I will now say a word or two on the periodical recurrences of this fluid.

To dilate on the opinion of those who imagine the menstrual action to depend on the influ-

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*The author wishes it also to be understood that the blood is the material out of which the secretions, subservient to the animal economy, are made.

ence of the moon would be superfluous; and what could be said of partial might also be urged in behalf of general plethora; for all partial affections of this sort, must of necessity become general. It is therefore my intention to treat of both these opinions at the same time; especially as almost all the attempts of Physicians, some before the days of Cullen, have tended to prove the position of the general fulness of the body being the cause.

The opinion of Cullen is ingenious. He affirms the effusion to be referrible to a given density and tone of the arteries of the general body, which determine the blood in upon the womb, the arteries of the evolved uterus, laxer and weaker than those of the body in general, yield and, ultimately giving way, a hemorrhage is the result. In the space of a month another congestion is established, and a second effusion takes place, and so on as with other hemorrhages: thus the menstruous flux, in its recurrences, becomes established by habit! Yet Cullen has not ascertained to us how it is that this flux recurs so precisely at the end of every month in every healthy female body, in the same climate, nor indeed is it proven by him that any hemorrhage does return at fixed periods with undeviating precision.

We will admit, for the illustration of the thing that the quantity of eight ounces is accumulated

in the course of four weeks, and passes off. It is presumable that, if sixteen ounces were taken away by art, there would not be a discharge until at least two or two and a half months should have elapsed. If therefore this occasional effusion be deducible from a monthly accumulation, a double quantity being artificially removed in the space of a month nearly triple the time must go its round before a second effusion, seeing that the space of two months will be required to recover the quantity lost by art, and a part of the third before the congestion could be complete and the effusion take place. But blood abstracted in this way and to this amount will neither change the period nor alter the quantity. Professor Simpson, if my recollection be correct, asserts that he knew ten pounds to be removed by the lancet, and yet the menstruous fluid remained the same in quantity and flowed at the customary intervals.*

From these facts, it is very clear, in my estimation, that the menstrual action cannot depend on plethora. And in addition to what has been said, the circumstance of the young woman from whom Percival Pott by mistake removed the ovaries might be adduced. Antecedent to the operation, this young woman had had her men-

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* The author, from much and extensive observation since the above was written, is decidedly of the opinion that the ground occupied is fully tenable, and to the doctrine no respectable fact can be opposed.

strual operations regularly and at each time copiously, but after this unhappy affair, her breasts wasted away, and all the phenomena attendant on those periods disappeared and never afterward returned. Attentively weighing these matters I cannot hesitate to make up my mind in favour of the opinion that this effusion is attributable to a peculiar condition of the ovaries serving as a source of excitement to the vessels of the womb, rather than to the doctrine of repletion of the body.

It now becomes my business to explain how it is that the menstrual action recurs at fixed periods. Yet I confess there will be a difficulty in giving an explanation clear of exceptions. Although I am fully persuaded that an elucidation can be furnished, and that I am not much out of the way in my present view. Why do not agreeable vicissitudes of day and night return weekly or monthly instead of once in twenty-four hours? Because by *established* and *immutable* laws imposed on man and the inferior animals, the excitement necessary to the performance of the functions of life is more or less diminished in a short space of time, and rest and sleep are required every twenty-four hours for its restoration. The same reason holds with regard to this operation in women. For if it did not recur more than once annually, human pleasure would be abridged, and if it never occured, woman, according to her present constitution, would be barren, seeing that this operation

is absolutely necessary to the aptitude for fecundation. In some of the inferior animals there is something analogous.

How is it that the uterine arteries take upon themselves to secrete this peculiar fluid for a few days each month.* Upon the same condition that those of the female breast secrete milk after each parturition. Some have brought themselves to believe that it is improbable the arteries of the uterine could secrete, in a few days, so much as is perceived to pass off. It will be no longer wonderful after we satisfy ourselves of the immense quantity the dog will furnish in a measured space of time during coition.

It has been remarked by some writers that the menstruous fluid is not unfrequently discharged from other parts of the body than the uterus.† It is also a fact that the bile, at times, passes off by the urinary bladder. This points out a state rather morbid in the liver, that an unhealthful condition of the womb. The bile is also sometimes arrested in its passage through the duct common to the liver and the gall-bladder.

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* By fixed, fundamental laws of the female economy.

† This is congruous with the ordinary fact, that any set of arteries in the body may perform a vicarious office. They all in turns, assume to themselves the function of ossification.

We now turn our attention to the circumstance of the total cessation of the menstrual operation about the forty-fifth or fiftieth year. Those who have favoured the opinion of this action depending on plethora have not been inattentive to this fact. They supposed that this discharge ceased at this epoch, because the arteries of the womb had become so firm as to be competent to afford an adequate resistance to the impulsion of the blood. We can scarcely be induced to admit that the uterine arteries of every woman would at this age be uniformly and equally so firm or so diminished in their capacity as to present an insurmountable barrier to the current of blood. The thing is far from being probable. This cessation is in my estimation, deducible from some other source. That is, at this period, the ovaries lose altogether their disposition to fecundity. For nature, wise in her provisions, has by an unchangeable law ordered that the menstruous action should be discontinued, and the ovaries should lose their capacity to be impregnated, which served as an excitement to the womb, at that period when women could no longer afford nutriment to their offspring.

Of the end or ultimate purpose of the Menstruous Fluid.

To this day it remains a question, to what purposes this fluid is destined. Many believe that it is intended for the nutriment of the child. There were of the ancients as well as the moderns who countenanced this opinion, and for the reason that during gestation the menstruous fluid is discontinued, and was detained, as was imagined, as aliment to the embryon.

For some time subsequent to conception the fetus does not exceed, in volume, a bean or a wasp. And during this time can require but a small quantity of nourishment. What now becomes of the superfluous quantity? If the menstruous fluid be intended for this purpose, in the earlier stage of gestation it would be too abundant; in the later insufficient.* Added to this there are animals not provided with this fluid, but yet their young do not want aliment. Why therefore would writers look to this as nourishment for the child when it is obvious that the

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* The whole of the menstruous fluid for nine months, the term of gestation, would not amount to more than five or at most six pounds; a child, at full time, together with the secundine, weighs from eight to ten or twelve, or fifteen, or even twenty pounds. Hence it is irrefutable that the menstruous fluid cannot be intended for fetal organization. Nature could not act upon principles so inadequate as to have her means thus incommensurate to her ends.

young of the lower animals have no such resources? we need not be at much trouble to furnish the reason they have not looked out and explored the real and true cause of the menstrual operation.

Certain authors have believed the menstruous flood to be offensive and hurtful. In the Livitical law some commands are to be found on this head. But, as the thing appears to me, more on account of the uncleanliness of that people, than from a regard to any thing unclean or hurtful in the fluid itself.

We therefore must, according to my conceptions, consider that nature in furnishing the menstruous fluid intends nothing farther than to present to us certain signs of the maturity of the uterine system. From this view we may explain why it is that the discharge ceases during pregnancy; that is because at this period the ovaries lose their capacity to be impregnated which serves to rouse the arteries. Hence it is that we never, except in a few instances and these cases of disease, find this discharge to continue while the woman is pregnant.

Before I come to a final close, I must present my thanks, grateful and sincere, to my friend and colleague Dr. John Shaaff, and to others, from whom I have received acts of friendship, for the many favours and civilities enjoyed by their courtesy.

Th' inhabitants of seas and skies shall change,
And fish on shore, and stags in air shall range:
The banish'd Parthian dwell on Arab's brink,
And the blue German shall the Tigris drink:
Ere I, forsaking gratitude and truth,
Forget their kindness to my rising youth.

When the author presented to, and sustained before the University of Glasgow, as his Inaugural Thesis, the foregoing little work, he contemplated adding to it a short account of his views of the office of the Placenta: but finding himself anticipated by the learned professor Jaffrey, who, he understood, had employed his able pen on that subject, he abandoned his intentions. Yet it may not, even at the present moment, be out of place to state briefly the opinion entertained at that period*, and which he now, after much examination and reflection, believes to be well founded.

The Placenta has generally been thought to be a mere medium of connexion, or of circulation between the mother and child. To this the author has many, and, as he apprehends, serious objections to offer. But passing by, for the present, all laboured discussion, he will only state that, to his views, the Placenta holds the rank and performs the office of a secretory organ, a species of Mamma, and that its function is the secretion and separation from the mother's blood, of the aliment and oxygen necessary and appropriate to the fetal wants and demands.

* 1793.

A TREATISE

ON THE

Autumnal Endemial Epidemick

OF

TROPICAL CLIMATES,

COMMONLY CALLED THE

YELLOW FEVER;

CONTAINING ITS

ORIGIN, HISTORY, NATURE, AND CURE;

TOGETHER WITH

A few Reflections on the

PROXIMATE CAUSE OF DISEASES.

BY

JOHN B. DAVIDGE,

A. M. M. D.

ALTERAM PARTEM AUDI.

BALTIMORE: Printed by WILLIAM WARNER.

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1813.

INTRODUCTION.

IT is not my intention in the following pages, to unfurl the banners of personal opposition. Dr. Rush professes truth to be the object of his inquiries—truth is the end of my researches. The expanded mind of Dr. Rush, receives more sincere and real pleasure in the perception of one truth, than in all the fulsome incense that could ascend from a thousand flatteries: His great mind, I hope, is closed to the poisonous nutriment of boyish vanity.

The object of my labours and studies, for several years past, has been, in a great measure, an inquiry into the nature and etiology of diseases. And although, not unfrequently, I could meet with the opinion, that contagious diseases did often originate in marsh exhalations, yet as this opinion was accompanied by suspicious circumstances and wanted the support of probability, I treated it more as the fondness for novelty and innovation, than as the candid result of experience. Nor should I at present attempt to arrest its prevalence, had it not found too able an abettor in the learned and elegant Rush. Writers, of little note, cannot give tone to opinions. Falcons do not feed on flies. Great names alone can introduce great errors.

Who among us would have imagined, that the human mind was nothing but a flux of ideas, had not David Hume given currency to this hypothesis? Or that thought was a peculiar configuration of the combination of material atoms, had not Dr. Priestly introduced it to our acquaintance? But, to come nearer home, who could have supposed that the water-spout, that huge rising of the ocean, was effected by a power of suction resident in the clouds, without the hint of Dr. Franklin.*

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* The true and real cause of the water-spout is, a column of electrick fluid descends from the clouds; this electrick column rarefies the air to a given distance; the rarefaction of the air causes an unequal pressure on the water: the equal pressure being removed, the water immediately rises, in bulk corresponding to the space of the rarefied atmosphere. This column of water ascends in proportion to the levity or thinness of the air, sometimes to an amazing height; and in its ascension acquires a whirl occasioned by the circumambient air rushing to a focus to fill up the partial vacuum and restore to itself its former equilibrium.

There is a species of water-spout, which, it is said, at times occurs on land. This we imagine is produced by a condensation of the aqueous vapours, constituting the cloud, in consequence of a sudden loss of electrick fluid and elementary heat. Nearly upon the same principle, it is probable, as hail is formed in the hotest weather of summer. This phenomenon, to our understanding, is obviously produced by rapid evaporation occasioned by an almost instantaneous escape of a large quantity of electrick fluid, either from two clouds, the one plus and the other minus, meeting, or from a cloud highly charged passing within striking distance of some elevated district of land.

Great names have a species of magick accompanying them. Mr. John Hunter gave, for a time in London, tone to the doctrine of digestion. His opinion, that it is the combined action of friction and fermentation, has been successfully combated by being put in competition with one better founded. Spallanzani, by a series of well managed experiments, has exhausted the subject of all doubt. According to this indefatigable man, it is nothing more complex than a simple chymical process; such as takes place in the union of sugar and water, where the attraction of combination overcomes the affinity of aggregation: That which is proper nourishment is taken up, by the lacteals acting as capillary tubes, what remains is thrown off as excrementitious refuse.

Names frequently are more propitious to books than their contents are. And many works, of sterling value, too often instruct none but their authors.

This comes forth, neither connected with name, nor clothed with title; I leave it to recommend itself. If it prosper, well; if not, it will only, suffering the mishap which overtook one of David Hume's best productions, fall dead-born from the press.*

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* When the above was written the author did not intend to have affixed his name to it, but for particular reasons he changed his determination.

The reader will bear in mind that the following few imperfect pages constitute the first english production in which the author's pen was ever engaged*: that he was not at liberty, to imagine himself employed in merely dressing a brilliant period of polite literature, or polishing a trope in rhetorick, but seriously occupied in disquisitions of a much higher, and a more important nature; and also recollect, that, it was written a few months after the author had taken on himself the discharge of the highly responsible duties of his profession. Keeping in sight those points, the Reader, it is hoped, will generously touch with a light hand, and view with a pardoning eye, the many inaccuracies and the juvenility of the style.

* It was first published in 1798.

TREATISE, &c.

CHAPTER I.

THE ORIGIN, HISTORY, AND NATURE OF THE YELLOW FEVER.

THE yellow fever, synonimous with la Maladie de siam, or la Fevre matelotte of the French, and vomito prieto of the Spaniards, is the majority or acmé, as the intermittent is the infancy, of the remittent bilious fever. It is to the common bilious fever, what the confluent, is to the common mild, small-pox. They are in kind the same, a specifick difference only exists between them. It is conceived in the same matrix, and quickened by the same sun.* It is indigenous to America, and all other warm climates. It is the great outlet, to Americans and Britons, from life to the grave.

The rays of the sun, diffused and scattered, are innocent and salutary, but collected and condensed into a focus are dangerous and hurtful.

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* "Without the matrix of putrid vegetable matters, there can no more be a bilious or yellow fever generated amongst us, than there can be vegetation without earth, water, or air." Rush vol. 3, p. 168.

Thus the concentrated effluvia of marshes are venemous and deadly; but scattered and diffused, float innocently among us.

The generality of the French writers call it la maladie de Siam from a false notion that it was originally from Siam, a country in the east. This contains as much truth as the opinion that it attacks sailors only: whence they call it la fevre matelotte. It is, in all probability, the Causus or Tebris ardens of Hippocrates, of Aretœus and of Galen. Trallian and Lommius appear to have seen this fever.

Ulloa makes mention of the vomito prieto prevailing, in a most horrible and destructive form, in Carthagena, in the year 1729 and 1730*. It made its inroads in Barbadoes in the year 1696† a time long prior to the visit of Dr. Warren to that island.

Marsh effluvia appear to be either the decomposition of vegetables or water; but whether inscrutably combined with something else, or insulated, I leave for farther investigation. In the decomposition of either, hydrogene is produced in considerable quantity. Water, in its liquid state, is a compound of hydrogene and oxigene, with an addition of calorick and a little

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* Voyage to South America, B. 1, C. 5.

† Hughs' History.

common air. Its decomposition is affected by the matter of heat inserting itself between the liquid pherules until their separation is such as to annihilate the attraction of combination between the hydrogene and oxygene. In effecting this decomposition, however, there must be a third agent. Whether then an union, betwixt the oxigene and light, comes to pass, remains to be discovered: this I strongly suspect. The hydrogene being disengaged and insulated, and very much accumulated, or peculiarly combined, with some subtle poison, perhaps becomes the peccant agent.

It is worthy of observation that the bilious, or yellow fever does not generally prevail during the heat of the summer. This may be owing to the greatness of the heat dissipating and scattering the hydrogene, or marsh effluvia, so as to enfeeble and render it or those innocent.

That a combination between the oxygene and light happens is likely, first from their natural affinity to each other, and secondly from a phœnomenon observable during ignition. The rays of light falling immediately upon the ignited combustibles, cause the flame to become faint, and ultimately will extinguish every particle of the fire. The probability is, that this phœnomenon is occasioned by the rays of light attracting and combining with the oxygene of the atmosphere, and thereby interrupting the process

which was going forward between the oxygene and combustible bodies To this there is an accession of additional strength from what takes place in vegetation.

A vegetable, kept in the shade, becomes white, and sickly: when it is exposed to the light it revives, and becomes healthy. This I apprehend arises from the light acting as a stimulus, and at the same time attracting from it its oxygene, with which it is necessarily charged in decompounding water for its nutrition. Vegetables when analyzed yield more or less of hydrogene. Hydrogene is that gas which in its struggle to ascend, meets with the electrick fluid of the atmosphere and forms what, in vernacular vulgarism, is called jack-o-lanthorn —or when it has gained the superior regions and formed the upper strata of the air, comes into contact with the electrick fluid, and effects what is indicated under the appellation of aurora borealis or northern lights*. It is also the principal agent in the motion of the aerostatick machines.

The yellow fever made its first appearance in the city of Baltimore in the last of August. The common bilious fever prevailed at the Point from June. A lady from Philadelphia, bring-

* It is presumable, that, in those meteorous phenomena, phosphorus may be more or less concerned.

ing with her the seeds of the disease, which were brought into action by the fatigue of the journey, was severely attacked with it in Charles-street, she had the genuine black vomiting, which resembled ink and coffee-grounds mixed, for two nights and a day, and miscarried on the sixth night of the disease.—She notwithstanding recovered. No person in the family, or neighbourhood had it during the whole season.

This, together with the number of cases of violent bilious fever at Fell's Point, threw the city, generally, into a combustion. The committee of health requested that the physicians would convene in order to give a report of the city; upon the meeting of the physicians, it appeared, from their joint testimony, that the above-mentioned lady was the only person, labouring under the fever, in the west end of the city, called the town in contradistinction to the Point. [I shall in what follows make use of the distinction of town and Point.] The committee received a letter from Dr. Coulter, a physician of great respectability at the Point. Dr. Allender, in person, waited on the committee, and gave information of that part of the city. It was requested by the committee of health, that some of the physicians of the town would visit the Point. Pursuantly to this request, doctors Goodwin, Moores, and Davidge politely went, and waited on the sick individually; they were piloted by a gentleman who lives with Dr. Al-

lender, together with a student of Dr. Coulter's. Their report was; "that there was nothing more than a violent remittent bilious fever prevailing." This perfectly accorded with the sentiment which dropt from one of those physicians during his report to the mayor; "his reason (he said) why he did not report the case of the lady of Philadelphia, was a full persuasion, that the yellow fever, being of the same origin with a bilious fever, could not be multiplied, by an intercourse of bodies, under any circumstances whatever."

The above report is a strong and prominent feature of their discernment, and indifference to popular prejudices. A little after this period, the disease, rising from the rank of a bilious, to that of the yellow fever, mounted its chariot of death, and spread dismay and mourning wherever it approached. Conveyed by the north-east wind, it diffused itself all along Federal-hill, and west end of the bason. Whichever direction the miasmata (arising from the stagnant water and marshes about the Point and wharves) controled by the winds, took, the disease closely pursued. It evolved, in an horrid and dismal manner, its venemous characters in the south end of Hanover-street, and its vicinity. After a short interval these deadly effluvia penetrated into the centre of the city, and many who were not near the Point nor wharves, those exuberant fountains of mischief, were diseased

either in their houses, or in the streets. Not an inconsiderable number of those who were at the launching of the frigate took the disease; several of whom died of it afterwards up in the town; but, fortunately for the citizens, no person was infected from them. A very considerable part of the Point fled into the country; and some from the town removed; a temporary desert was effectuated.

I scarcely need mention the daily deaths; the reports of the committee of health are inadmissible of an accession of testimony, to give weight to their authenticity and accuracy.

The greatest number, in the town or west end of the city, in any one day, was seven or eight; the list of the deaths at the Point was, for some time, considerable. This fever began to cease about the first of October, and was nearly or quite extinct on the first of November.

All endemical epidemicks, as they depend on a peculiar constitution of the air, must of necessity cease whenever this condition is destroyed, whether it be by frosts, or rains.

The yellow fever, like all other epidemicks, delights in solitude. An epidemick, whether endemical or contagious, depends on a general peculiarity of the air;* which general peculiarity

* This peculiarity of air is attributable in the one instance to a general diffusion of vegetable, and, in the other, of animal effluvia.

will have a general influence within its own dominion, and communicate a general aptitude to all bodies, within this jurisdiction, to receive its action. It chases away all others of less strength, as is justly observed, first by Diemerbrook, secondly by the Sydenham, thirdly by Pouppé Desportes, and after them by Rush; who is perfectly correct, where he says, that no two epidemicks, of unequal force, can prevail at the same time. How is it possible for two general and opposite constitutions of the air to exist at the same time? with equal propriety we would say that a cord can possess two distinct oscillations at the same instant; or that two particles of matter can occupy the same given space in the same division of time. But this every tyro should know.

Some of the first traces of the yellow fever, in America, are to be found about sixty or seventy years back. A physician, in conversation, the other day told me that he had met with the yellow fever, in Baltimore ever since he had lived in it, which is fifteen or twenty years. It is violating all obligations of decency and truth to say that it is of recent date. The town of Baltimore, in proportion to its inhabitants, is less subject to this autumnal remittent or yellow fever, than the low situations about the Potowmack. A gentleman, who for some considerable time was one of the principal directors of the Potowmack business, informed me that one season they lost a considerable number of their

workmen by the above fever. And that in no instance did it spread by contagion.

Every country has diseases proper to its climate and situation. Some diseases are common to every country and climate: Accident and particular circumstances will create sporadick diseases, in every country, not peculiar to them respectively. A disease, proper to one country, can, by the medium of intercourse, be carried into another and there propagated.

Britain has its scrofula, and typhus; the vicinage of the Alps has its goiter: the East has its plague; the West-Indies, America and other countries, within or near the tropicks, have their remittent bilious fever, and hepatick affections. It is not the import of this paragraph, that those diseases are exclusively generated in these countries respectively. The reverse is incontrovertible. But as those complaints, although they may originate, or be produced, in every country and under every climate, most commonly and generally appear in those countries in the manner above-mentioned, I have taken the liberty to style them proper to those individual climates. It is a concurrence of circumstances, and not given latitudes, that is requisite for the production of diseases.

The seeds of the remittent bilious yellow fever are produced in putrid vegetables and stagnant water, and are quickened by heat and dry-

ness. Those who live in the neighbourhood of marshes, in warm climates, and during hot and dry autumns generally suffer more or less. The more elevated situations, and those distant from such sources in the West-Indies and America, most generally escape this deathful malady, except when the atmosphere becomes universally surcharged with the effluvia emanating from those exuberant fountains of mischief and poison; under such occurrences they unavoidably participate of the evil. This is confirmed by the united voices of writers and practitioners.

Males, by being more exposed to these effluvia floating in the air, to the violent rays of the sun, to night-dews, and to the various, sudden, and great vicissitudes of the weather, (these operate differently according to their respective natures) are more subject to this, and all endemical epidemicks, than females, whose business is naturally within their houses, where the poison is blunted and rendered inert by the fires and smoke, and has itself dissipated by striking against buildings.

The skirts of most cities are occupied by the poorer class of inhabitants; their houses are exposed to the first and most violent assaults of all endemical epidemicks; but as those epidemicks invade more the vitals and heart of a city, they are dissipated, enfeebled, and disarmed.

The remittent bilious yellow fever neither pities helpless infancy, nor reverences the decrepitude of age; but its chief delight is to jostle, from the stage of life, the young and vigorous. The many-headed monster, armed with destruction and wo, wantons in the citadel of life: and now, Anthropophagi like, without warning or premonition, implants his merciless dagger; and now under the garb of innocence, gambols in the blushing cheek of health. The yellow fever is as fatal when with the investiture of an intermittent, as when habited in its regular dress. Having explored its cause and origin, we may trace out its nature.

Dr. Rush.

"The fevers generated by putrid cabbage, mentioned by Dr. Rogers, and by putrid flax mentioned by Dr. Zimmerman, were both contageous. Dr. Lind ascribes the yellow fever every where to marsh or vegetable exhalations; and this fever, we know spreads by contagion. Dr. Lind, jun. establishes the contagious nature of the marsh fever which prevailed in Bengal in the year 1762. I shall transcribe his words upon this subject. "Although marsh miasmata (says he) first bring on the disease, yet contagion presently spreads it, and renders it more epidemick. Thus the Drake Indiaman continued free from the disorder for two weeks together, when she had no commu-

The Author.

An endemick is a disease, that afflicts several people together in the same country where it reigns, arising from local circumstances, or a peculiar condition of the air. It cannot be carried from one country to another, by the means of bodies and cloths. It affects more or less all within its own periphery. It cannot be carried, beyond its own atmosphere, by bodies diseased with it. No disease, arising from marsh effluvia, can be communicated beyond the atmosphere charged with these effluvia. Every disease arising from marsh effluvia, from the gentle intermittent to the furious yellow fever inclusive, then is endemick.

It does not appear, that any of the cases, in the opposite

nication with the other ships; whereas as soon as the disorder was brought on board, many were seized with it within a few days in such a manner as to leave no room to entertain the least doubt concerning its pestilential nature."

Dr. Clark mentions a contagious malignant fever from marsh miasmata, which prevailed at Prince's Island in the year 1771, and which afterwards infected the Grenville Indiaman. The contagious pestilential fever in France, so accurately described by Reverius, was produced by an exhalation from putrid vegetables, particularly hemp and flax. Even intermittents, the most frequent and most numerous offspring of the marsh exhalation, are contagious. Of this there are many proofs in practical authors. Blanchi describes an intermittent which was highly contagious at Wolfenbuttle in the year 1666. Dr. Clark mentions a number of cases in which this mild species of fever was propagated by contagion.

Dr. Cleghorn has established the contagious nature of intermittents by many facts. After mentioning numerous instances of their having spread in this way, he says, "These tertians have as good a right to be called contagious as the measles, small-pox, or any other disease." &c. *Vol.* 3, *p.* 160.

column, communicated themselves by means of the diseased bodies beyond the circumference of the atmosphere impregnated by the vegetable exhalations; then if they did not, which is pretty obvious, act beyond the limits of the inquinated atmosphere, the probability is, that the vegetable or marsh effluvia wafted through the air did the mischief, and not the intercourse of bodies.

That a ship's crew was free from a disease this week, is no just argument, if they be infected next, that it must be by means of one diseased body communicating it to another; the contaminated air may have enlarged its limits—a particular direction of the wind may have conveyed the bad air to the ship. It happening posteriorly to a communication with the diseased, is no argument, except at the same time it be proved that this crew was without the control of the ill-conditioned air. Marsh and vegetable effluvia smite at the distance of miles, this no physician doubts.

"These diseases (speaking of intermittents) make their first appearance in February and August particularly; tho' sometimes they appear sooner or later, according as the air is more or less disposed to produce them, which, of course renders them more or less epidemick." P. 51, Dr. Sydenham.

A question of importance and magnitude here arises and invites our attention; it may serve to awaken a little our judgments and elucidate the present stage of the business. Why is it that the intermittent fever has never clothed itself in its contagious habiliment in America. It is rather problematical that it has ever done more mischief, or been more common and violent in any other country than in America. Here is a mystery equally dark on all sides; how are we to decipher the enigma?

"*Intermittentes.*

Febres, miasmate paludum ortae, paroxysmis pluribus, apyrexia, saltem remissione evidente interposita, cum exacerbatione notabili, et plerumque cum horrore redeuntibus, constantes: paroxysmo quovis die unico tantum." Dr. Cullen, Tom. 2, Synop.

Dr. Cullen, the luminary of the medical world, has been careful to inform us, that he has never met with contagious diseases arising from vegetable putrefaction.

Sydenham, styled by Dr. Rush, "the incomparable physician," positively asserts that an intermittent becomes epidemick from the *air*, and not by contagion. Dr. Jackson says that the intermittents and remittents become epidemick and spread by the marsh miasma being dif-

fused through the atmosphere, and not by contagion (vide Jackson on the diseases of America). Dr. Gilchrist mentions in his tracts on sea voyages, that the marsh effluvia being scattered through the air become the source of popular diseases. That diseases, arising from marsh miasmata, are incapable of becoming contagious has been considered, by the generality of physicians, as a fact almost self-evident. And I believe that few will long hesitate to determine between the authority of Sydenham and Cleghorn.

The unscientifick notion of intermittent and remittent bilious fevers being contagious, is too ridiculous to find access even to the easy credulity of the unread peasant: but yet it is embraced by philosophick refinement. And what absurdity, though ever so enormous, has not, at one time or another, been countenanced by proud, arrogant, fastidious philosophy? Organized absurdities, have long conspired against the progress of science, and tyranized over the tender claims of more luminous systems.

Endemick is the antithesis of contagious. Endemick and contagious establish two opposite categories.

Contagion is the emission, from body to body, by which diseases are communicated. A contagious disease arises originally from human

effluvia, can be carried, by means of the infected bodies, or cloths, from country to country, from city to city, from town to town; in fine, from any one place to another: The yellow fever has not its origin from human effluvia, cannot be spread, by means of diseased bodies, or wearing apparel, or bed-cloaths, from country to country, from city to city, from town to town, nor from one place to another—the yellow fever therefore is not contagious.

Either an endemick or contagious disease, becoming general, and affecting a whole country, or great extent of territory, claims, in technical language, the appellation of epidemick. It is not whether a disease arises from vegetable or human effluvia, but whether it has a general or universal action, that constitutes it an epidemick. It is the universality of its action, and not the nature of the source whence it arose, that stamps its character. Hence two classes of epidemicks—the endemial epidemick, and the contagious epidemick. The yellow fever ranges under the former, the plague under the latter.*

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* So far as accessible authority will justify a conclusion, the author is the first writer of America, who has publickly advanced the opinion that the Yellow Fever is not a contagious disease. And he would feel no inconsiderable degree of pleasure in perceiving many of the most impassioned opposers of his sentiment among its warmest admirers, were it not, that these learned gentlemen, with his Treatise in their hands, pretend and assert (with how much justice may be left to the reader), that the discovery is the *result of their own observations*.

Dr. Rush.

"It has been remarked that this fever did not spread in the country, when carried there by persons who were infected, and who afterwards died with it This I concieve was occasioned, in part by the contagion being deprived of the aid of miasmata from the putrid matter which first produced it in the city, and in part by its being diluted, and thereby being weakened by the pure air of the country. During four times in which it prevailed in Charleston, in no one instance, according to Dr. Lining, was it propagated in any other part of the state." Vol. 3, page 157.

The Author.

When the intercourse of bodies, labouring under a fever, cannot support and multiply the said fever, the fever is not contagious; the yellow fever cannot simply and without the addition of the effluvia arising from vegetable putrefaction support and multiply itself, the yellow fever therefore is not cantagious.

A little attention to the nature and operation of the small-pox, measles, or jail-fever, when conveyed into the purest atmosphere, will, in some measure, obviate the difficulty which obscures the above fact in relation to the yellow fever. It is found upon experiment, that, uniformly the small-pox, or measles, or jail-fever, requires nothing more than its own presence, and virulence to perpetuate itself. This is clearly apparent from the many melancholy instances of whole families being precipitated to their graves by the unfortunate introduction of servants, purchased from on board ships, infected with the typhus or jail-fever. Out of fifty persons, who might visit a patient labouring under the latter stage of the small-pox or measles, for-

ty-nine, in all probability would be infected. I have selected those diseases, as they are common and known to every body.

In these diseases, which are essentially and absolutely contagious, no heterogeneous aid is necessary. In the yellow fever the addition of the marsh miasmata seems to be the *sine qua non*, in multiplying and propagating the complaint, and the efficient, occasional cause of its rise, independently of any assistance from bodies: and without the help of these miasmata, the bodies can neither generate nor spread the disease; hence it appears that the miasma produces the disease whenever and wherever it may occur, and not the bodies labouring under the complaint.

It is not my expectation to turn, upon the axis of a syllogism, the whole esculapian world. Prejudice may for a time bar the understandings, but time and accident will eventually unlock the judgments of men.

Dr. Rush.

"Let it not be inferred from the enumeration of the means of preventing the contagion of this fever, that I admit a contagious nature to be one of its characteristick marks. Far from it. It is an accidental circumstance produced chiefly by the concurrence of the weather." And on the same page "It is in no instance contagi-

The Author.

A disease which is not essentially, and in its own nature contagious, is not, in strict propriety, contagious at all. The yellow fever is not essentially, and in its nature contagious—the yellow fever is therefore not contagious at all. A disease, which is not intrinsically and in its very nature, radically and inherently contagious,

ous in some cases." Vol. 4, page 61.*

naturally repudiates the idea of contagion. Fire, elementarily, contains heat; and light is, intrinsically, luminous.

If the vegetable effluvia do, and can, of themselves, produce, perpetuate, and multiply the yellow fever: and if the human effluvia cannot and do not in any instance, of themselves, produce, perpetuate, and multiply this fever, upon their fortuitous union; which can be supposed to effect the mischief? The utmost precipitancy cannot endanger the decision.

The bodies diseased certainly produce an effluvia, which, by disturbing directly or indirectly the bowels, stomach, or sensorium commune, may destroy the equipoise of the animal system, and thus prove an exciting cause, in like manner with drunkenness or night-exposure; hence the aptitude of those who have to nurse, and wait on the sick in the yellow fever, to be diseased. This is not by receiving it by way of contagion: but under those circumstances the equilibrium of the body is destroyed, the œconomy is thrown into disarray, the vigour is unnerved, and an advantage is afforded the miasma to bring itself, with all its deathful consequences, into full operation.

The government of a country is called good or bad, according to its nature; the best govern-

* This paragraph, for its interpretation, requires something above the author's comprehension.

ment becomes hateful and detestable from bad administration; but the administration is not physically or morally consecutive of the government. In this instance we are to depose our minister, not change our civil system. Thus if we remove the patient, in the yellow fever, from the inquinated atmosphere, he is no longer dangerous or hurtful to the attendants.

The christian system, the substratum of hope and felicity, has become hostile to the peace and safety of nations, by its administration, not by its nature, its essence is peace, and life, and immortality.

Many things, in their nature, elude the most vigorous effort of the human intellect, and tantalize the grasp of genius itself. They present to us their modes, and qualities, and affections; these, operated on by the instrumentality of the senses, advertise us of their respective existence. It is from the effects of human, and marsh effluvia, that we can have any clue to the secret of their nature. Like causes will always produce like effects, provided they operate upon the same order of patients. The human effluvia produce one order of diseases, the marsh effluvia another, these orders never unite—they have no connection. Of these identity forms the curve, and those effluvia the asymptotes, they apparently approximate, but they can never come into contact. The human effluvia can

never produce the remittent bilious or yellow fever, nor can the marsh effluvia ever give origin to the typhus or jail fever.

Is it good logick, that a fever can arise from one source to-day, and from another, diametrically and physically opposite, to-morrow? This is certainly at variance with common ratiocination. Yet this must come to pass, if the yellow fever, originally, has its origin from vegetable putrefaction, and is afterwards perpetuated by contagion. Some have asserted that heat and cold, though different, produce the same effects. Cold is a relative term—it has no absolute existence—and that which has not an absolute existence cannot possess a positive action.

Sir Isaac Newton, who was a man of no humble genius, is of the opinion, that it is incompatible with the principle of philosophizing, in the explication of any phœnomenon, to adduce more causes than are true, and absolutely necessary for its solution.

After the above theoretical observations, we shall direct our attention, a little, to the authority of experience, and minute inquiry, of some of the most respectable writers and practitioners.

It is an unquestionable truth; a verity of the most publick notoriety, that not one, of the very great numbers who have left the cities and towns, some of whom have died and some have

recovered, has communicated the yellow fever to those who have attended. Not one solitary fact has ever reached me; and my scepticism is such as to lead me into a persuasion, that there has not existed one unequivocal, well analyzed fact of a patient, going to the country, and there multiplying this fever.

"It affects the inhabitants of cities, and not of the country, as in Charleston in the years 1732, 1739, 1745 and 1748. And in Philadelphia in the year 1793." That is, it could not be propagated, by the diseased bodies, in the country. But that it can affect inhabitants of the country and also originate in the country is fully defensible. I know that my opinions here, as in other parts of this essay, are the antipodes of those generally received. But while I have facts, and those of the most stern and inflexible nature, I shall not be over solicitous about received whims whether popular or professional.

On the Potowmack Bottoms, and along the Monokocy, I have seen the most unequivocal cases. It is also, from the very respectable authority of Dr. Watkins, very common in different parts of Kentucky. Dr. Watkins was in Baltimore during the prevalence of the fever this autumn, and, with me, waited on the sick. He, after being provided of all necessary requisites to form an opinion, declared it to be precisely similar to that of Kentucky. He farther

observed that the fever in that state frequently was attended by the black vomiting, &c. He said that it was considered there as an endemick arising from the marshes, and in no instance contagious.

It is true that Dr. Rush mentions an instance of this fever spreading in the country; but we want graver authority than inaugural theses afford. Theses, we all know, generally, are nothing but the echo of the prelections of a preceptor.

In the flourishing and growing city of Baltimore, we have had the most stubborn, and irrefragable proofs, of the yellow fever being incapable of supporting itself, in the cases which have occured about the wharves and Fell's Point. After those cases were removed up into the city, they had their virulence to die with them, those who died; and from those, who recovered, all mischief and supposed contagion evanesced into the empty air, which bore it to the pages of medical writers, not to the bodies of healthy attendants. This was the result in 1794 and 1797.

The unfortunate case, of the very respectable Dr. E. Johnson, with several cotemporary incidents, afforded a short-lived triumph to those who were wedded to the contagious system; but when their opinions, armed with all the address and subtlety of the authors, came in collision, with those of more erect and manly aspect,

they felt their vacillating uncertainty, and ceded in candid contest.

Which is most consonant with probability, for gentlemen, going to an atmosphere, charged with poison and deadly effluvia, an atmosphere immediately wafted from the Point and concentrated under a hill, where many had heen forced, by its deathful influence, to pass the bourn of life, and others to perceive death, with its black wings, to hover about them, to take it from those exanimous and dying bodies, or this noxious air? Taking into view, at the same time, the fact, that none of those gentlemen, several of whom died up in the city, communicated, to any of their families or attendants, the disease.

Some even point out the luckless moment, in which the relentless malady seized them. "At a particular juncture," says one, "I perceived something singularly offensive—in three hours after that inauspicious point of time, I became unwell." Another was taken ill a day or two posteriorly to some ill-omened hour, in which the breath of the patient was breathed directly into his face; it was then he inhaled, with the pabulum of life, the fermenting leaven of death. Who, after these and such like melancholy tales, can hesitate to believe the yellow fever contagious?

I would no longer quarrel with the sentiment of contagion, had one of the above occurrences

taken place, in people, who were not, at the time of the imagined infection, in an impure atmosphere, or had not lately been exposed to air impregnated with the destructive miasmata. The fact is, those persons were all in this ill-conditioned air. I met with two cases, in two young men, who had been at the Point at the same time, and were both attacked, on the same day, with all the regular symptoms of the fever, where the miasmata did not come into action until the eighteenth day—they were not, during the interval, exposed to either sick bodies, or vegetable effluvia, having been in a healthy country the whole time.

Whatever affects most our senses, we are prone to attribute our evils to: This is excusable in men unaccustomed to thought, but it is an incongruity in the common order of reasoning, and is at total variance with the notions of men of science.

Logomachy is as much at variance with my habits, as any two opposites in nature are inconsistent with each other. But I do not consider a war of sentiments, fraught with mischief or good, a colluctation of principles, tending either to support or destroy the commerce of a nation, as the idle jar of words. It is no new act for men to force the most opposite and contending principles into the most cruel and unnatural union. Tradition, let it militate ever so much with com-

mon sense; and an association of ideas, bearing no affinity nor cognation to each other, press us, too frequently, into most absurd beliefs and habits of thinking. I might here cause to pass in review before the reader's mind the great Lavater's philosophy of faces, that finished and polished physiognomy of folly. And where is a philosopher, bating a few, who does not stuff his works with the infinite divisibility of matter? who has dared to dispute the corrollary, of the great Locke, that the human mind is, at first, as a blank sheet of paper, passive to the characters of chance? and where are the friends of David Hume, who do not, to this day, believe, that, if an ass were placed between two cocks of hay, which impressed equally, he would starve, being unable to make a choice? But I am straying: it may be well to obey the suggestion, that we should tread lightly on the ashes of the dead; I bow obedience, and may David rest in peace, and his errors sleep in eternal silence.

Now were I to plunge into the vacuum of metaphysicks, or enter the lists in pneumatology; I should believe, with the peerless Reid of Glasgow, that the human mind possessed, inherently, action, vigour and choice; that it operated upon surrounding objects, and was not the passive sport of incidental impression. Whence comes genius, the innate perspicacity of the human intellect; genius, that heavenly offspring,

at all times impatient of the trammels of control, and indifferent to the habits of education? not from the impressions of the beauties of Thompson, nor the more sublime of Homer. It is inborn. Did the mountains of Switzerland, give exertion to the mind of a Haller, or the banks of the Potowmack, infuse divinity into a Washington? that encyclopedia of virtue and greatness, in whom may be found every ornament of human excellence; into him who first boldly dared, to rend asunder the strong ligaments of prejudice, to control the imperious tide of ancient usage; who magnanimously despised the boisterous torrent of vulgar obloquy, and challenged the herald of recording time?

This apparent digression is to shew, that no name is above truth, that our care and solicitude should be the investigation and conservation of truth, not the support and protection of names and traditions.

Doctor Rush.

"An aptitude or predisposition from season, climate or constitution must concur to render the contagion of this, as well as other malignant fevers, sufficiently active to produce disease; as well might a traveller attempt to describe the climate of a new country, from the history of a single season, as a physician to fix the character of an epidemick from its appearance in one season, or one country." Vol. 4, page 62.

The Author.

"The one (the yellow fever) is evidently caused by marsh effluvia, heat, violent exercise in that heat, thick, hot, moist atmosphere; night air and dews, and the abuse of spiritous liquors. The other (the boullam fever) on the contrary, is caused by contagion alone. This is certainly the most remarkable difference; and constitutes an obvious, clear, and indisputable diagnosis." Chisholm, page 147.

"But I never could observe any one instance, where I could say that one person was infected by, or received the fever, (yellow fever) from another person who had it." Hillary, page 145.

Dr. Jackson calls the yellow fever of Jamaica an endemick, and no where mentions its being contagious; who also is of the opinion that most epidemicks spread by means of the *marsh effluvia*, and not by contagion.

Dr. Mosely styles it (the yellow fever) an endemial causus, page 391. And ridicules the idea of its being malignant, pestilential, and contagious, as asserted by Dr. Warren, page 412. Dr. Warren, as Mosely justly observes, had scarcely any idea of this fever at all, except in its description. Towne calls it febris ardens biliosa, but does not add contagiosa, and asserts it to be an endemick of the West-Indies.

That it appeared, at Barbadoes, anterior to the time (1725) fixed by Dr. Warren is evident from Dr. Gamble, who well remembered it to be very fatal, in the island, in the year 1691. That Warren's account of the disease is altogether fabulous is certain: this man, wrapt in hypothetical clouds, was carried about by every wind of errour.

Pouppé Desportes speaking of the la maladie de Siam, says; "la régularité avec laquelle elle se reproduit, semble devoir la faire regarder

comme une de ces maladies dont il faut chercher la cause dans la *constitution de l'air.*" Page 191, Tom. 1.

Dr. Cullen supposed this disease to arise from human effluvia and therefore, placing it under the section of contagious diseases, called it typhus Icterodes—but it is certain Dr. Cullen never saw the disease, and equally certain that he borrowed his idea from Dr. Warren—Dr. Cullen is consequently to be left out of the present discussion.

As well might you contend, that a skilful gardener could make a tree flourish in a soil unnatural to its growth, as physicians ingraft contagion on marsh exhalations.

In the almost endless chain of cases, wherein the clothes of those who have died, in the West-Indies, from the yellow fever, were brought to America, contagion would be explored in but one instance, even by the indefatigable industry of the sagacious Rush. A gentleman's clothes being returned in a trunk to his friends; a young gentleman, upon opening the trunk, became immediately unwell, but no other person suffered in the least. Those clothes, beyond a question, were damp, this dampness was by heat converted into effluvia, corresponding in every particulur with marsh miasmata. These effluvia, from the closeness of the trunk not be-

ing able to escape, remained in their offensive and concentrated state: And, upon the trunk being opened, applied themselves in their full force to the excitable system of the young man. In this manner, from the water in the clothes being changed into effluvia similar to those of marshes, and not from the clothes acting as the vehicle of the human effluvia, was this gentleman diseased. Now if the supposed contagion was so very vivacious as to deproduce itself after so long a lapse of time, how is it that the recent emanations from a diseased body treated with such great tenderness the attending friends, as not to interfere in the smallest degree with their healths?

The idea of contagion is indirectly injurious to commerce, and directly to society. Under the influence of the persuasion, that there is one of the most violent of contagious diseases prevailing in several of the sea-ports of America, can we suppose that foreign ports will suffer our vessels to enter? A long and dangerous quarantine must be performed; the damages, accruing from such delay, the merchants too sensibly feel to be ignorant of. Even when our papers do not proclaim the melancholy tidings; seeing that such disease has frequently visited our cities, is it not probable that foreigners will guaranty their own safety, by prohibiting the entrance of our vessels? will they not naturally say? the Americans have seen their

error in rendering publick their diseases—their silence is a piece of policy—the diseases exist: interest, the main spring of human action, forbids their publicity.

From slender causes great events come to pass. Few could have supposed that the interest France took in the American struggle would have laid the corner stone of her ruin and overthrow?—The government I speak of. Athens was ruined solely by Pericles giving countenance and support to a despicable tribe of stage-actors. Upon a slender pivot play weighty matters.

Our extraneous interruptions are not the only inconveniences we labour under, domestick commerce is subject to arrestation, a general calm and stagnation in business depresses our prosperity.

Has it ever been known that the yellow fever has been propagated through Baltimore, or any other city, from a person bringing from Philadelphia this malady, even in instances where it has proven fatal? The archives of America will never record such a fact.

Do not the grave proclamations, and serious resolves of city-corporations excite our astonishment? What are these proclamations and resolves for? To prevent, what, from the earliest dawn of lapsing ages, has never, and to the latest eve of expiring time will never, come to pass.

Our interest and commerce fall a sacrifice, and are immolated at the shrine of our folly. As long as my reminiscence will recover to me a knowledge of the proceedings of the health-committee of the city of Baltimore, I shall admire and esteem their judgment and good sense.* Has the yellow fever ever been imported, from the West-Indies to America? Report says it has; but where are proofs? smothered in impenetrable obscurity; they fly the presence and converse of investigation. Has this fever ever been carried from the West-Indies to Britain?†

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* This, with several other passages refer to the circumstances of the time, and cannot be understood but by those who may be possessed of a knowledge of those circumstances.

† On this subject Dr. Wilson in his valuable work on febrile diseases, has furnished us with a most important fact. "Dr. Lind, remarks he, mentions a fact, for which it is still more difficult to account, that the same fever brought into this country in several American ships, *attacked those only who had been on board the ships*, others remaining uninfected, notwithstanding the *freest intercourse* with the sick on shore." Vol. 1, page 178.

It is not a little surprising that a man of understanding and science should be at a loss to account for this fact. If we were to admit the ships to be foul and highly charged with putrid vegetable effluvia, might we not thereby be provided with a clue to the explanation of this mystery?

The persons who went on board the ships came into contact with the poisonous effluvia which at first produced the disease; those who remained on shore were exposed only to the human effluvia issuing from the sick and the dying.

The conclusion here is inevitable; the disease does not spread by contagion, but is produced by putrid vegetable matter only.

The poor unfortunate subjects of disease, flying from the cities, find the doors and windows of the country barred against them. The children leave their parents to die, and parents their children, their minds being jostled by the sound of contagion, from their proper seats. The lonely hearse solemnly conveys the dead to the dreary repository of the sleeping multitude, where reign silence and death. Having considered the history and nature of the yellow fever, we shall pass on to a consideration of its symptoms, first premising a few thoughts on the proximate cause.

CHAPTER II.

Proximate cause Analyzed.

THE fabrick of the pathology of diseases, has for more than two thousand years floated on the varying ocean of incertitude, the sport of winds and tide. When the microscopick eye traverses the hemisphere of medicine, it beholds theories hurled on theories, fancies crushed by fancies, and less errors smothered by those of greater bulk and effrontery. From the auspicious days of Hippocrates, we gently glide down the silent tide of time, collecting as we move the shattered wrecks of crazy systems, until we arrive at the fluctuating variety of modern hypotheses. Hippocrates, wrapt up in the flattering pretensions of his humoral pathology, and balancing between heat and bile, a long time swayed the sceptre of the medical world. From an attentive perusal of this author's works, heat or bile, or plethora or obstruction (for in different passages he speaks of all these) seem to constitute the proximate cause of fever. His successors soon perceived the futility of this foundation, and attempted to fabricate others more probable.

Diocles of Carystus, a physician, who flourished at an early period, and a man of conside-

rable eminence, asserted that fever was not so much a primary disease, as secondary and dependent on some more hidden disorder. In order to avoid the force of his doctrine, after physicians established the diversity of symptomatick and idiopathick fevers. Presently after Diocles, Erasistratus, a physician at the court of Antigonus, invited the attention of the world, his proximate cause resided in an error loci. Next Asclepiades, the Bythinian, stepped on the stage and rudely grasped the reins of government; he, adopting the doctrine of atoms, handed to the Greeks by Democritus of Abdera, attempted to account for the difference of types by a difference in the size of the corpuscles, which he supposed to be formed by a combination of indivisible atoms. Here emerges the doctrine of the obstruction of the permeable canals of the body, and its consequence modern viscidity and lentor, so famous in the schools. Asclepiades was the father and patron of the sect of the Methodicks.

Themison vibrated between strictum et laxum, and on those two pillars reared his pathology of diseases, here are the first traces of spasm, afterwards laboured by Hoffman and matured by the great Cullen: this hypothesis claimed the ascendency, at Rome, for more than an hundred years. At last Galen, the impassioned admirer of Hippocrates, exhumed and reanimated the humoral errors.

Athenœus ventured to resuscitate the doctrine of the putrescency of the blood (this is to be found in the writings of earlier authors) and putrescency has not made a very despicable figure in the world. Avicenna expressly defines fevers to arise from a preturnatural heat of the heart.

The Galenists prevailed until the beginning of the sixteenth century; about which time Aureolus Phillippus Theophrastus, commonly known by the name of Paracelsus, began to make a figure. This man assailed the Galenical party with all the engines of effrontery and resources of unimproved chymistry. Here commences the period of medical romance, so fraught with the struggles between the mechanical and chymical modes of reasoning, these eventually neutralized, and ushered in the chymico-mechanical philosophy.

The furious archeus of Van Helmont, differently modified, is the efforts of nature, so celebrated by Campanella and Sydenham, and autocrateia of Stahl. If we except Mundy, Borelli and Cole are the only writers previous to the time of Hoffman, who considered the nervous system as directly affording a seat for the proximate cause of fevers: Here in more positive terms is expressed the doctrine of spasmodick stricture. This idea of Hoffman, Cullen has elaborated to its utmost perfection. Who has taken the proximate cause from the heart, and

fixed it on the superficies of the body, in an atony and spasm of the capillaries.

After the above recital the mind is restless and in eager expectation waits to embrace a knowledge of the true nature and seat of the proximate cause. I shall, leaving the anterior whims to slumber with their authors, they being, by one obliterating stroke of the pen of Dr. Rush, sentenced to perpetual silence, take the liberty of putting the opinion of the celebrated Philadelphia Professor into the crucible of analytical inquiry. The opinion of Doctor Rush is the latest that I have met with in the writings of physicians. His words are "having premised these general propositions, I go on to remark, that a fever (when not misplaced) consists in a morbid excitement and irregular action in the blood-vessels, more especially in the arteries." "This irregular action is in other words, a convulsion in the sanguiferous, but more obviously, in the arterial system." Page 134, Vol. 4. "From the facts and analogies which have been mentioned, I have been led to believe that irregular action or a convulsion in the blood-vessels, is the proximate cause of fever." Page 139, Vol. 4.

There incontrovertibly is a difference between a fever and the proximate cause of a fever; a fever cannot consist in, or be made up of (these are synonimous) a convulsion in the blood-ves-

sels, and a convulsion in the blood-vessels be the proximate cause of a fever. An effect and the cause of that effect cannot be the same. If the fever consists in, or is made up of, an irregular action or convulsion of the blood-vessel, what is the proximate cause? and vice versa. It is illogical to identify cause and effect.

The proximate cause, of which writers say quae presens facit, sublata tollit, mutata mutat, after being hunted from one part of the body to another, and metamorphosed from one thing into another, at last takes refuge in the ignorance of its friends; having for its associates Phlogiston and the four elements; and an honourable triumvirate they form, each having enjoyed its apotheosis.

In a disease there are three essential causes. The predisposing, or an aptitude of the body to be acted on; the occasional, which acts directly or indirectly on the seat of life and action, and the exciting, or that which destroys the equilibrium of the nervous energy, and by this destruction of the equipoise of the system gives the occasional cause an opportunity of making an impression. The causation or modus operandi which takes place between the occasional cause and the living principle is not accessible even to the most vigorous efforts of the human mind.*

* Notwithstanding the generous and enlightened suggestions of the able and ingenious editor of the New-York Repository,

An offending entity (the remote or occasional cause) assails the tranquility of the healthy body, a particular, though inscrutable, infraction of the harmony of the animal œconomy is caused, evidencing itself by a chain of symptoms more or less unequivocal; these constitute the symptomata of writers, that (the primary disarray or infraction) the disease; here we observe a regular and simple concatenation of cause and effect, and the evidences of such an effect. We cannot apply the name of disease to an arrangement of symptoms, no more than we can the appellation of matter to an assemblage of qualities, or the epithet of spirit to a combination of modes,

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the prepossessions of the author against the *possibility* of a proximate cause in any disease are still inveterate and incurable. So sceptical, indeed, is he on this head, that he apprehends there is no one thing more grossly absurd or completely unintelligible than the conception of agency or operation between the action of the remote cause on the living principle and the result, the disease, farther than the modus operandi or rule of action. And in his estimation it argues no great soundness of intellection to magnify a rule of action into a distinct agency.

All that has fallen in his way is so disgustingly puerile that he would feel relieved and gratified on seeing any thing assuming the form of rationality on the subject.

In his objections to the reality of a proximate cause the author is strongly fortified by the following sentence from the very learned Dr. Gregory.

"Haec quum ita sint, non mirum esse poterit multa medicorum commenta de causis *proximis* variorum morborum prorsus *futilia* esse, et non modo *falsa*, sed adio *confusa et obscura* ut vix possint intelligi, neque fortasse ab ipsis eorum auctoribus unquam *satis intellecta fuerint.*" Consp. Tom. 1, page 29.

but to that particularly morbid state of the body giving origin to such an arrangement of symptoms.

Chills, fever, pain, prostration of strength, discolouration of the tongue, &c. are symptomatick of a hidden and essential disarray (a disease) of the nervous power. Figure, divisibility, extension, and solidity are indicative of an inscrutable material substratum. Passion, memory, and judgment are proofs of an immaterial essence, the nature of which the labour of the human mind cannot develop.

The cherishing beams of philosophy have begun to dawn, and I hope soon will enable us to proceed with more certain step in the healing art. It is time that we should divorce from our minds the petitio principii, and, like Pyrrho, disrobe ourselves of credulous facility. This epoch demands self-evident premises or proven data for the ground-work of our inductions. Jurare in verba magistri, is the motto of unthinking hebetude; a master's nod ought not to block up the avenues of research.

CHAPTER III.

DIAGNOSIS.

THAT assemblage of symptoms, which generally are the appendages of any disease, and establish a barrier between it and all others, constitute its pathognomonicks or diagnosticks. The general characteristicks, which disjoin the yellow fever from all others, are the following:

In most instances, a prostration of spirits and an inaptitude to motion, a sense of uneasiness and great fatigue; pain and uneasiness through the limbs, as if from riding. It some times, without any premonition, impugns the guardians of life. It will in one instance assume the dress of the tertian, and in others clothe itself in all the characters of a cold. But let what will be its harbingers, it soon hangs out its own colours, and demands a tribute. The eyes become more or less affected by inflammation, accompanied with an acrimonious or burning epiphora; the head feels itself molested by pain and giddiness, and a sense of congestion; the tongue is indifferently white, yellow, blue, red, brown or black; in the first days of the disease it has an oily feel. A pyrexia attends; the skin is one while hot and dry, at other times preter-

naturally cold and clammy. The præcordia is much oppressed, attended by a great inclination to vomit. Vomiting not unfrequently, or a cholera morbus or a diarrhœa gives notice of the approaching calamity. The stomach in the latter stages of the disease labours under a sensation of having in it something which it cannot digest; this sensation they attribute to whatever they have swallowed: A flatulency and hiccough help to fill up the train of evils. There is pretty uniformly a paucity of urine, and what is voided is very high coloured. A black vomiting or purging, hemorrhages from every part of the body, especially the stomach, uterus, bowels, nostrils, and the incisions made by the lancet in bleeding; carbuncles and numerous little biles, more or less, act their part in this tragical scene; the black matter and hemorrhages seldom appear until after the fourth or fifth day; yet they sometimes occur earlier. The countenance has a peculiarly ferocious look. The eyes are with the redness, tinged with a croceous colour; this yellowness frequently diffuses itself through the whole superficies of the body.

There is in some cases, about the fifth or sixth day, a cessation of the fever, and all the violent symptoms, every thing becomes apparently favourable, and the physician will augur auspiciously; but this is a mournful circumstance; it is the powers of life ceding, and not a relaxation

of the disease; especially if yellowness and hemorrages co-exist. There is frequently a metastasis to the testicles.

In puking, the patient some times throws up nothing but what is taken into the stomach rendered a little ropy, at other times a black liquid, resembling a mixture of soot and water, is ejected.

The blood when abstracted is seldom covered with a buffy coat, but generally is what we call, a dense, red blood; it rarely is dissolved. An obmutescence, and deafness are among the last marks of an approaching dissolution, they are truly prophetick of death. The sensibility of the surface of the body is exceedingly morbid; and on the least touch communicates uneasiness; this preternatural excitability I have met with in a surprising degree.

Not unusually a considerable degree of delirium accompanies this most prominent in the black catalogue of human ills. Perhaps there is no complaint, from the effect of which patients are so long convalescing. Small purple spots very often variegate the arms, breast, and neck; they are ominous of peril.

By the dissection of defunct bodies, we get a view of the dreadful ravages of this relentless malady: We behold the stomach disfigured with sphacelated spots, and characters of inflamma-

tion. The liver swollen, and exhibiting every mark of phlegmasia: The spleen preternaturally flaccid: The gall-bladder turged with black and acrid bile; the whole of the primæ viæ, when a natural diarrhœa co-operates, is manifestly affected with erysipelatous inflammation, which, by the way, is the species of inflammation that attacks the stomach, and this, probably, is the reason why the blood, when abstracted, is not sizy; were it of the phlegmonoid species the size on the blood would uniformly appear; in some cases this species of inflammation does attend, and in those the pleuritick coat, pretty uniformly, proclaims its presence. The erysipelatous species is generally too rapid in its progress to mortification to communicate the buff to the general mass of blood. That the buffy coat is an inseparable and infallible sign of inflammation when accompanied by fever, and vice versa: See the experience of the most enlightened and assiduous practitioners; also the professors of Edinburgh; under whose wings, were folly and ignorance to deluge creation, learning and science would find shelter.

The lungs not unfrequently show marks of inflammation. The encephalon is pretty uniformly implicated in the testimony of the general destruction occasioned by the yellow fever operating on the animal frame: Its meninges

are found inflamed, the cortical and medullary substance is unusually red.*

This disease attacks sometimes with such violence and severity, as, either from its force or the feebleness of the patients, to supercede, by death, most of the above-mentioned appearances.

The black vomiting, hemorrhages, cerebral affections, yellowness of the eyes and skin, purple spots, and pyrexia being the most conspicuous and inseparable diagnosticks of the complaint, merit more seriously our particular discussion.

The bile, in its natural and healthy state, is as bland and mild as any secretion in the human body, but when the liver is affected by any specifick action, its secretory function, like similar physiological processes, is subject to vitiation; and that this black matter, discharged indifferently upwards or downwards, is vitiated bile, depending on a morbid action in the secretory organ, is obvious. 1. From the great quantity which is found upon dissection, in the gall-bladder. 2. From its great acritude.* 3. From its analogy to other disordered secretions: For instance that of the kidney in diabetes, of the stomach in dispepsy, of a sore when it becomes,

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* Vide Pouppé Desportes, Hillary, Mosely, Rush, &c. &c.

as we generally phrase it, vitiated. Laudable pus is a secretion, so is the acrimony escaping from a vitiated, a cancerous, or a scrofulous ulcer. the atrabilis of the ancients perfectly accords with the black vomiting of the moderns. Some physicians have persuaded themselves that this black matter is owing to an admixture of blood, and that there is an absolute want of secretion in the yellow fever; this opinion, I must believe, has a more intimate affinity with prejudice, than with reflection.

Hemorrhages seldom overtake the patient in the early stage of the disease, except under the form of epistaxis, which is not commonly considerable. When they do occur, they are the indubitable evidence of an atony or paralysis of the blood-vessels and this atony in its turn depends on a destroyed vigour of the nervous power; this destruction of the nervous influence is effectuated by the peccant agency operating directly and immediately on the sensorium commune. It is the result of a violent action on the immediate seat of life, and not of the vascular, or arterial system; the blood-vessels can only be acted on secondarily. If we could timously diminish the quantity of the hostile power, we should infallibly prevent those sanguineous fluxes. Moderate blood-letting, by lessening the volume of the blood, will also contribute considerably to that end; but too profuse bleedings, by robbing the system of its sources of recovery

from so violent a shock, precipitate the unhappy sufferer to his grave. That a violent action on, and not a smothered or incontrollable exertion of the system, lays the foundation of those hemorrhages, is obvious. 1. As they do not accompany phrenitis or pneumonia where the vascular exertion is much more fierce, active, and oppressed. 2. Blood-letting after the second or third time, except in particular habits, when it is pretty copious, so far from relieving the imaginarily oppressed pulse, rather renders it more feeble and yielding. 3. In the most violent attacks, where there exists the greatest degree of indirect debility, the propriety and safety of immoderate phlebotomy are in the inverse ratio of this indirect debility. I speak from my own experience, and that of Hillary, and Pouppé Desportes who carried the lancet to its greatest extremes, and possessed the most ample opportunities of seeing its advantages and disadvantages; not for one season only but for fifteen or twenty years, during which time he practised in the West-Indies; as also of several of the most respectable physicians in the city of Baltimore. Lastly, hemorrhages are more apt to occur in those who have been copiously, than in those who have been moderately bled; provided untimely death, superinduced by those large evacuations, does not obviate them. So is mortification about the orifices made by the lancet. Pouppé Desportes, speaking of those hemorrhages, and mortifications, remarks

" Dans plusieurs les saignées se rouvrent, et le sang, malgré le nombre des compresses, pénetre, cette hemorragie est souvent accompanée d'une gangrene charbonnée, qui se forme autour de la saignée, et dont on ne peut, arrèter le progrès." And a little below.—" Cet accident arrive ordinairement à ceux qui ont été trop saignés."* I shall speak more at large on this when I arrive at the therapeutick division of this publication.

The Encephalon although subject to inflammation and partial infarction, cannot labour under general congestion; that is, the brain-case cannot contain more at one time than another; except the bones and sutures become firm and ossified, and in cases where the sutures are afterwards destroyed by diseases. The medullary substance of the brain is incompressible,† and the case itself is unyielding: When a congestion of the right side of the head happens, there is a simultaneous, and equivalent diminution of the areæ of the vessels of the left. In case of hydrocephalus there is a general invasion of the areæ of the cerebral vessels. In some instances the cortical substance is worn away by attrition. If a general congestion of the brain could take place, a partial vacuum would of consequence be possible; and if a partial vacuum took place,

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* Vide Tom. 1, page 200.

† Vide professor Munro's incontrovertible experiments.

the plates which are behind the eye-balls would unavoidably be forced in, and instant death be the result by the weight of the external air: The columns of which, in weight amounts to about forty-pounds on each eye, allowing fourteen pounds to each square inch.* The posterior plate that supports the eye is frequently so thin as to be quite diaphanous. The inference is that either an inflammatory or partially congested state of the brain must give support to that delirium, vigilance, &c. we observe in the yellow fever and many other complaints.

This recieves accession of certitude from the circumstances attending the decapitation of an animal; when the head of an animal is severed from its body, all the blood discharged is from the external parts of the head, not one drop escapes from the internal. This is substantiated by the following experiments. Dissect the vessels to their exit from the skull, then secure them firmly by a ligature, this being done, divide the case and you will find every vessel regularly filled and replete with its contents having suffered no evacuation.

Again, take a glass-globe with two opposite orifices, fill it with water, then put your finger upon one, and turn the other downwards; no water will escape until you remove your finger

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* Vide Hale's experiments.

from the superior orifice. Nor can the contents of the skull be either decreased or augmented, except the case previously be exposed to violence.

The yellow or brown colour of the eyes and skin is owing to an absorption of the bile or brown matter after it is secreted. There is sometimes a tempory yellowness of the skin, this may be produced by a peculiar action of the blood-vessels.*

The purple spots constitute a pathognomonick, they are neither the production of an over action of the system, nor a dissolved state of the blood. They are to the yellow fever what the red spots are to the cynanche maligna, or the eruptions of the skin to the measles; they are symptomatick, and no regular consequence of the general state of the fluids or condition of the solids.

Fever is a convulsive action of the arterial system, as mentioned by Dr. Rush, accompanied by more or less of a peculiar and indiscribable heat and dryness of the skin: The dryness and heat though are not always present; as in the febris typhodes. This convulsion of the arterial system is the result of a more inscrutable and hidden morbidness of the source and seat of life and action, the nervous power. The mus-

* Vide Rush.

cular fibre possesses no vis insita, its sensibility and contractibility are feudatory of the vis nervia.*

The source of life and action must be originally and primarily concerned in all the operations, whether morbid or healthy, of the animal body. The nerves are the "seat and throne" of all diseases.

Whether a fever be the result of a mere mechanical action, or the provident effort of the vis medicatrix naturæ, that power in the body to heal its own maladies, I leave to the more learned to determine. But there is a something about a fever which is more easily recognized than developed.

A peccant entity, acting either mediately as in the form of fracture or wound, produces fever indirectly; or immediately as in the condition of human or marsh effluvia, &c. produces fever directly. In both cases the fever is the consequence, and symptomatick: In neither is it strictly idiopathick. It is nothing but a symptom of a more primary and essential disturbance of the nervous energy.—This primary and essential disturbance is uniformly the disease; whatever succeeds is only indicative of this first disturbance of the quiet and health of the animal system.

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* Vide Monro's nervous system.

CHAPTER IV.

CURE.

WE now arrive at the division of this essay which is the most interesting and merits the most serious and unbiased investigation. And leaving the slippery declivity of hypothetical incertitude, we introduce our readers to the more unequivocal and inflexible data of practical experience: where feeble theory is supplanted by more certain practice, where the sick bed triumphs over the reveries of the closet.

Dr. Rush, in manfully and successfully labouring to stem the torrent of error and preposterous madness, which had diffused themselves throughout, and woven the tissue of general practice; and in calling the medical mind back to the almost antiquated system of depletion so fortunately pursued by Sydenham, Cullen, Monro, Gregory, Botallus, Pouppe Desportes &c. has attached immortal honour to himself, and, using a gallicism, deserved well of mankind. The doctor's exalted dignity elevated him above the mean wiles of plagiarism. He, with the candour proper to great minds, frankly acknowledges his obligations to preceding writers. I am, however, considerably persuaded that in his strenuous exertion to crush the growing folly of medical pre-

judice, which took root in the execrable writings of Brown of Edinburgh, and Kirkland of England, he has suffered himself to be hurried within the embraces of the opposite extreme. It is beyond contradiction that the experience of some of the most scientifick and best informed physicians, will not warrant the extremes of depletion inculcated in his learned works.

In the management of the autumnal remittent or yellow fever there are four therapeutick intentions.

1. To dimish the violent action of the general system and remove as far as possible the inflammatory disposition of the liver, stomach, &c.

2. To take off the stricture of the superficies of the body.

3. To discharge the acrid bile as quickly as it is excreted.

4. To restore the vigour of the frame as soon as possible after the fever has subsided.

The first intention is best accomplished by a judicious and proper use of the lancet, together with a speedy introduction of mercury, either in the form of calomel or ointment, into the system. It will too often happen from an over excitability of the stomach that the calomel cannot be used. When this occurs the mercurial ointment must be freely applied to the insides of the

thighs, legs, and arms ; those parts being the most abundantly, of all the external parts of the body, supplied with the lymphatics.

Calomel is the most efficacious and powerful of all medicines in the resolution of inflammations of whatever kind they may be. But in those of the liver its salutary effects are peculiarly deserving our notice. For this valuable information we are principally indebted to Dr. Gilchrist of Scotland and the practitioners of the East-Indies.

When we enter the room of a patient in this fever our first attention should be to the state of the eyes, the degree of pain in the head, oppression about the præcordia, and the fullness of the pulse. This is seldom or never hard ; indeed the stroke of the artery is more deceitful in this fever, than in any other disease I have ever met with. If the eyes be much inflamed, or labour under a sense of protrusion from the sockets, or an unweildiness in their motion ; or an acrid epiphora frequently distils from the eyes ; together with the above if the head complains much, the pulse appear full to the application of the fingers, and be frequent, we must instantly have recourse to the lancet. Even should the tongue at the same time be blue or brown. The colour of the tongue is not to be in general confided in. It is sometimes blue or brown from the first days of attack. So long as the pain of the head, or back continues

with a very frequent or full pulse the lancet must be resorted to. This however will rarely be the case after two or three good bleedings, except in particular habits. Some habits will bear the lancet to the fifth or sixth repetition, especially where the indirect debility of the system is not great and where there is great heat of the surface of the body, and the tongue white.

A timous use of the lancet more effectually than any other remedy, tends to prevent the rapid progress of the inflammation of the different viscera. When it has taken place blood-letting promises more liberally than all other remedies. But when bleeding is carried to too great extremes, it exhausts the general system and prostrates the powers of life in so great a degree, that the animal frame can never renew its functions ; it hastens, by robbing the blood-vessels of internal support and nourishment, that atony and palsy of the vascular system which lays the ground-work of those melancholy hemorrhages.

When blood-letting is had recourse to, it should be practised within the first three or four days. It may, under particular circumstances, be performed at later periods, but not with such propitious consequences. I have practised it, as late as the tenth and fifteenth day of the disease, in instances where the patient's strength had not been sapped by evacuations.

Experience, together with the writings of most of the respectable practitioners of the West-Indies, establish the following.

Those who were not bled suffered from the neglect of the lancet,

Those who were bled largely died from the abuse of the lancet.

Those who were bled according to the pain of the head, fullness of the pulse, and oppression about the præcordia, or in other words moderately, in a much greater proportion recovered. Bleeding is by no means a new remedy in the yellow fever.

There were two young gentlemen, who in a visit to the Point contracted the disease : They were both taken ill a few days after their return to the Town. One of them was blooded six or seven times, and died. The other was bled once only and recovered. The violence of the attacks was apparently equal.

A little after, there were two others who took the disease by going to the contaminated atmosphere ; one of whom was bled six or seven timse within about forty-eight hours, was put into the cold bath, had injections of cold water ; he died, amidst the hands of the operators, during the third injection of cold water, The other who was delerious almost from the first onset of the

complaint, was not bled at all, yet after a severe and dangerous illness recovered.

There were two other cases, one of whom lost about as much as would be taken away at four common bleedings, and recovered. Though no patient could be worse than she was to recover. The other was bled twelve or thirteen times, his arm mortified, and he departed from among the living. Out of six who took the disease at or near Bowley's wharf, five, some of whom were so much evacuated that their friends threatened to interfere, died. The one, Mr. Waters, who escaped was bled but twice and that moderately. There were some instances of recovery after those profuse evacuations, but they were relatively few. Where one, after such immense losses of blood escaped, there were ten who were either not bled at all, or but once or twice, that recovered. Out of all the blacks, for negroes by no means escaped this complaint, whom I attended from the first attack of the disease, and they were not few in number I lost none. I bled but one, and him only to the amount of six ounces. I do not recollect that I met with a single instance of hemorrhage in a black person.

Calomel not only is the most effectual medicine that can be used in the first stage, but is also the only one in which we can have confidence to remove the stricture of the surface of the body ; especially when assisted by the warm bath,

either generally or partially applied. I have seen the ephidrosis produced to that extent by a liberal use of calomel, as to require three or four changes of linen in twenty four hours.

Calomel not only cures by acting as a diaphoretick and antiphlogistick, but also by establishing in the system an opposite action to that of the fever. No two general actions can exist at the same time : So that when the mercurial action takes place the morbid one must, of necessity, cease. The establishment of this mercurial action is confirmed to the practitioner by a free ptyalism. Whenever a free salivation takes place the patient is safe. Perhaps no person ever died after the full establishment of this discharge from the gums. Not that the salivation, strictly speaking, is of any service in itself. It is in the yellow fever, as in the lues venerea, the unavoidable consequence of the general mercurial action of the system, and of no farther service than informing the physician of this general action.

That the local pain is of no advantage is evident from the following. Let the gums become ever so much inflamed, pained, and swollen, if a very free spitting should not succeed, the sick reaps no advantage, but, on the contrary this state of the gums is ominous of approaching death.

To invitethe mercury to the surface of the body, the tepid bathshould be used, or in its place the pediluvium, and local applications of flannel, wrung out of hot water, to the regions of the liver, stomach, &c. Those should be frequently repeated, and continued, at least half an hour each time.

The cold bath has been very strenuously recommended by Jackson. But it does not appear from his writings that he ever used it with success without its being preceded by the warm bath: This together with its ill effects whenever it has been introduced into practice in America,* render it probable that Jackson attributed to the cold, what belonged to the warm bath. If it has ever been of the slightest utility in cases in America, these cases have not as yet found their way to the public. I say from the cold bath, in the practice of Dr Jackson, being uniformly preceded by the warm, and its general failure in America when used alone, I am fully persuaded that it not only is a useless, but a dangerous application in the yellow fever.

Emeticks even of the gentlest kind are inadmissible in every stage of the fever.† No preparation of antimony, from the tendency in those

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* Vide Rush.

† Some practitioners have given emeticks in the instant of attack with supposed advantage.

articles to excite vomiting, can be used with safety. The justly celebrated James' powder is here of too precarious operation. All neutral salts are too inert and uncertain.

Whether, upon being called to the patient, we bleed, or not ; we must instantly order a mercurial purge, which should be repeated every day or every second day, so as to produce four or five stools daily, this number he at least ought to have. To assist the operation of the catharticks ; the lower part of the intestines must be opened by means of glysters. A glyster-syringe will with great propriety and utility be kept constantly in the patient's room, that by the use of this the purges when they are too slow may be quickened and invited downwards. For as above mentioned the sick should never have less than four or five passages a day during the strength of the fever. Catharticks are of all remedies the best and most useful. They are not to be omitted even in cases where the pulse is feeble, and intermitting : If purges cannot relieve the patient, his chance is truly melancholy.

In support of the great necessity of constant purging consult Moseley, Hillary, Pouppé Desports, and the learned Rush. I had my patients to call for the close-chair from three to ten and fifteen times a day. And those who purged most, when not accompanied by puking, recovered soonest. In one case which I was called to, four

grains of calomel produced thirty-five stools : after which evacuation the patient began to mend and recovered. A patient, to whom I was called, took a little broth which rendered him much worse than he had been the preceding day. I ordered him thirty grains of jallap and ten of calomel (my usual dose) which operated twelve times ; and from that instant he began to recover. It was to the repeated use of catharticks, and mercurial diaphoreticks that I trusted the blacks to whom I was called.

Of all catharticks, I prefer gamboge and calomel, or jallap and calomel or senna in powder and calomel. By the timous use of the former of these I prevented the regular course of *this* fever, in at least thirty patients, who upon the first appearance of the disease called on me.

The fourth and last intention is employed principally in selecting those articles which will the most readily restore the exhausted strength of the patient. From habit and prejudice, in this stage of the disease, physicians generally fly to well known powers of bark and wine, opium and æther, colombo, and quassia, &c. But although these medicines are in appearance our hope and reliance ; a just pathology and experience will quickly evince the impropriety and hurtful tendency of all tonick and stimulant remedies. The snake-root itself is too great a bitter, and must not, except under particular circum-

stances, be exhibited. Our sole trust and dernier resource is in a well regulated dietetick plan, with now and then a purge to keep the bowels open. Nor must the inexperienced suffer the weakness of the patient to frighten them from the free use of purgatives.

The habits & desires of the stomach are chiefly to be consulted after the fever has subsided. But during the height of the fever, neither flesh nor any thing made of flesh can be allowed. Vegetables are the only articles of food that the patient can be indulged in while the fever possesses any considerable strength.

The patient must never take any beverage stronger than barley-water, lemon-ade, cold water, water with a toast in it, and such like mild potables. Some physicians are too cautious in giving cold water, and lemonade during the use of mercury. But I am satisfied from daily practice that no drink is more innocent and beneficial in a fever, especially the yellow fever, than lemonade, and cold water : nor are we to discontinue the citrick acid even during the use of calomel. Chymical affinities might induce us to believe that upon an union of the acid and calomel, the latter would, by decompounding the former, and attaching to itself part of oxigen, convert itself into the oxiginated muriate of mercury, or what is vulgarly called corrosive sublimate. This, however in fact does not happen.

If any acid possessing a greater affinity, to the base of calomel, than the muriatick, be used, the muriatick acid will be precipitated, and a new neutral, formed by this more powerful acid, and the base of the calomel viz. mercury, will be the consequence.

Blisters are sometimes useful; particularly when applied to the epigastrick region. They quiet the disturbance and excitability of the stomach. They are not unusually applied to the extremities and other parts of the body but perhaps more from custom and fashion than conviction of their real use.

Opium may, in the last stages when there is no fever present, be given in very small quantities, thereby to take off for a few hours the great irritability of the stomach. But all opiates must be followed, within six hours after they are given, by catharticks; other wise their stimulant qualities will far more than counterbalance any advantages the patient may at first receive from them.

During the dying state of a patient, I have made use of æther and musk, but my views contemplated the removal of particular symptoms, such as hiccough, twitches of the tendons &c. which very much distressed what few friends there were who had courage enough to come near the departing sufferer; and by no means the cure of the complaint. They serve under

such circumstances to smooth the passage to the grave but they cannot deliver from the grasp of the fell malady.

When hemorrhages do overtake the unhappy sufferer, general experience has painfully convinced us of the impossibility of managing them by astringents whether internal or external, except in those of the uterus where applications of cold vinegar for the most part answer. We are here still to pursue our general plan.

All applications to the carbuncles which oft accompany this fever, are useless. They may do mischief but we can expect but little benefit from them. Nor does opening of them seem to answer any good end. It is best to leave them to nature.

APPENDIX.

DURING the time that the foregoing Tractate was at the press I accidentally met with the answer of the Physicians of Philadelphia, to the request of the governor, the hon. Thomas Mifflin—in relation to the yellow fever. A perfect and harmonious coincidenee unites our sentiments, so far as they respect the origin and cure. For the satisfaction and benefit of the public, I will insert the letter.

SIR,

"IN compliance with your request, the subscribers have devoted themselves to the investigation of the origin, progress, and nature of the fever which lately prevailed in our city; and we have now the honour of communicating to you the result of our enquiries and observations.

We conceive the fever which has lately prevailed in our city, commonly called the yellow fever, to be the bilious remitting fever of warm climates, excited to a higher degree of malignity by circumstances to be mentioned hereafter.

Our reasons for this opinion are as follows:

1st. The sameness of their origin, both being the offspring of putrefaction. Of this there are many proofs in the histories of the yellow fever in the West-Indies. Where there are no putrefaction the West-India islands enjoy a perfect exemption from that disease in common with nothern climates.

2d. The yellow fever makes its appearance in these months chiefly in which the bilious fever prevails in our country and is uniformly checked and destroyed by the same causes; viz. heavy rains and frosts.

3d. The symptoms of the bilious fever are the same in their nature. They differ only in their degree. It is no objection to this assertion that their is sometimes a deficiency or absence of bile in the yellow fever. This symptom is the effect only of a torpid state of the liver, produced by the greater force of the disease acting upon that part of the body. By means of depleting remedies this torpor is removed and the disease thereby made to assume its original and simple bilious character.

4th. The common bilious and yellow fever often run into each other. By depleting remedies the most malignant yellow fever may be changed into a common bilious fever and by tonic remedies improperly applied, the common bilious fever may be made to assume the symptoms of the malignant yellow fever.

*5th. The common bilious and yellow fevers are alike contagious, under certain circumstances of the weather and of predisposition in the body. That the common bilious fever is contagious we assert, from the observations of some of us, and from the authority of many Physicians who have long commanded the highest respect in medicine.

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* This argument, the spirit of which states this and the bilious fever to be contagious, appears to be the offspring of conjecture and a biased education. Whatever the exertions and perspicacity of some individuals of Philadelphia may have explored I cannot determine. But, in my candid opinion, the observations, which deceived them into the persuasion that the bilious fever is infectious, are, somewhat defective. Many physicians of eminence have asserted, in their writings, the above fever to be contagious ; but they have neglected to accompany those conjectures with tributary facts to support and give strength to them.

There is not, so far as my reading extends, one solitary case of a contagious bilious fever in the records of medicine : I say not one case wherein contagion has been, or can be proved. And here, reader, my opinion is propugned and countenanced by Cullen, Sydenham and many more of the most learned and experienced of physicians. Bilious fevers spread by what those learned gentlemen in the latter part of the following paragraph

6th, The yellow and mild bilious fever mutually propogate each other. We conceive a belief in the unity of these two states of fever to be deeply interesting to humanity, inasmuch as it may lead patients to an early application for medical aid, and physicians to the use of the same remedies for each of them, varying those remedies only according to the force of the disorder. It is no objection to this opinion that, that state of bilious fever called the yellow fever, is a *modern* appearance in our country. From certain revolutions in the atmosphere as yet observed only but not accounted for by Physicians, diseases have in all ages and countries alternately risen and fallen in their force and danger. At present a constitution of the atmosphere prevails in the United States which disposes to a fever of a highly inflammatory character. It began in the year 1793. Its duration in other countries has been from one to fifty years. It is not peculiar to the common bilious fever to have put on more inflammatory symptoms than in former years. There is scarcely a disease which has not been affected in a similar way by the late change in our atmosphere and that does not call for a greater force of depleting remedies than were required to cure them before the year 1793.

7th. And lastly. The yellow fever affects the system more than once, in common with the bilious fever. Of this there were many instances during the prevalence of our late epidemick.

The fever which lately prevailed our city appears from the documents which accompany this letter to have been derived from the following sources:

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call "a constitution of the atmosphere which prevails in the United States and disposes to a fever (the bilious or yellow fever) of a highly inflammatory character. It is this constitution, effectuated by the marsh effluvia or vegetable putrefaction being diffused through the air, which in the first place gives rise to, and afterwards renders epidemick or general the bilious or yellow fever, as those gentlemen in what follows of their reply wisely allow. Those who wish to be satisfied that this fever is not contagious, will do well to pay particular attention to the cases instanced in the above letter.

1st. Putrid exhalations from the gutters, streets, ponds and marshy grounds in the neighbourhood of the city. From some one of these sources we derive a case attended by Dr. Caldwell on the 9th of June—one attended by Dr. Pascalis on the 22d of July, and by two cases attended by Dr. Rush and Dr. Physick on the 5th and 15th of the same month; and also most of those cases of yellow fever, which appeared in the northern parts of the city, and near Kensington bridge, in the months of August, September and October. We are the more satisfied of the truth of this source of the fever from the numerous accounts we have received of the prevalence of the same fever and from the same causes, during the late autumn in New-York and various parts of New-Jersey, Pennsylvania, Maryland, Virginia and South Carolina, not only in sea ports but inland towns. The peculiar disposition of these exhalations to produce disease and death was evinced early in the season by the mortality which prevailed among the cats, and during every part of the season by the mortality which prevailed in many parts of our country among horses. The disease which proved so fatal to the latter animals is known among the farmers by the name of the yellow water. We conceive it to be a modification of the yellow fever.

2ndly. A second source of our late fever appears to have been derived from the noxious air emitted from the hold of the now Navigation, capt. Linstroom, which arrived with a healthy crew, from Marseilles on the 25th of July, and discharged her cargo at Latimer's wharf, after a passage of eighty days. We are led to ascribe the principal part of the disease which prevailed in the south end of the city to this noxious air, and that for the following reasons:

1st. The fever appeared first on board this vessel and in its neighbourhood, affecting a great number of persons nearly at the same time, and so remote from each other that it could not be propagated by contagion.

2dly. There was in the hold of this vessel a quantity of vegetable matters, such as prunes, almonds, olives, capers, and several other articles, some of which were in a state of putrefaction.

3dly. A most offensive smell was emitted from this vessel after she had discharged her cargo, which was perceived by persons several hundred feet from the wharf where she was moored.

4thly. A similar fever has been produced from similar causes, in a variety of instances: we shall briefly mention a few of them.

At Tortola a fever was produced in the month of June, in the year 1797, on board the ship Britannia, capt. James Welch, from the noxious air generated from a few bushels of potatoes, which destroyed the captain, mate, and most of the crew, in a few days.

Two sailors were affected with a malignant fever, on board the ———, capt. Thomas Egger, in the month of March, 1727, from the noxious air produced by wine that had putrified in the hold of the ship, one of whom died soon after her arrival in Philadelphia.

In the month of June, 1793, the yellow fever was generated by the noxious air of some rotten bags of pepper on board a French Indiaman, which was carried into the port of Bridgetown, by the British letter of marque Pilgrim. All the white men and most of the negroes employed in removing this pepper, perished with the yellow fever and the foul atmosphere affected the town, where it proved fatal to many of the inhabitants.

On board the Busbridge Indiaman a yellow fever was produced in the month of May, 1792, on her passage from England to Madras, which affected above two hundred of the crew. It was supposed to be derived from infection, but many circumstances concur to make it probable that it was derived from noxious air. The absence of smell in the air does not militate against this opinion, for there are many proofs of the most malignant fevers being brought on by airs which produced no impression on the sense of smelling. This is more frequently the case when the impure air has passed a considerable distance from its source and becomes diluted with the purer air of the atmosphere.

Several cases are related by Dr. Lind, in his treatise upon fever and infection of the yellow fever, originating at sea under circumstances which forbade the suspicion of infection, and which can only be ascribed to the impure air generated from putrid vegetables.

So well known, and so generally admitted is this source of yellow fever in warm climates, that Dr. Shannon, a late writer upon the means of preventing the diseases of warm climates, in enumerating its various causes expressly mentions "the putrid effluvia of a ship's hold."

We wish due attention to be paid to these facts, not only because they lead to the certain means of preventing one of the sources of this fever, but because they explain the reasons why sailors are so often its first victims, and why from this circumstance the origin of the disease has been so hastily, but erroneously ascribed solely to importation.

The fever which prevailed along the shore of the Delaware, in Kensington, and which proved fatal to Mr. Joseph Bowers and two of his family, we believed originated from the noxious air emitted from the hold of the ship Huldah, capt. Wm. Warner. This air was generated by the putrefaction of coffee which had remaind there during her voyage from Philadelphia to Hamburgh, and back again.

* In the course of our enquires we were led to suspect one source of our late fever to be of foreign origin. The sails of the armed ship Hinde, on board of which several persons had

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* Relatively to the persons affected after receiving the sails of the armed ship Hinde, there is not the smallest probability that they were infected by the sails acting as fomites. The explanation given by those gentlemen is perfectly satisfactory. And had not the snow Navigation been in the vicinity, we should not have been at a loss to account for their indisposition without admitting the idea of contagion. The sails emitting an offensive smell is an irrefragable proof that they were damp; and what must be the state of the water and deleterious gas which were enveloped in their folds? This putrid state of the water is the very postulate to render it destructive.

died of the yellow fever, on her passage from Port-au-Prince, and which arrived on the 4th of August, were sent to the said store of Mr. Moyse. Four persons belonging to the loft were soon afterwards affected with symptoms of a bilious yellow fever. We shall not decide positively upon the origin of the fever in these cases; but the following facts render it probable that it was not derived from the persons who had died of it on board the suspected vessel. 1st, The sails emitted an offensive smell; 2d, three of the cases of the persons affected in the sail loft were of a mild grade of the fever; 3d, the fever was not propogated by contagion from any one of them; 4th, the sail loft was within the influence of the noxious air which was emitted from the hold of the snow Navigation, being not more than fifty yards, and was in the direction of the wind which blew at that time over her. The extent of this air has not been accurately ascertained, but many analogies give us reason to believe that it may be conveyed by the wind, in its deleterious state, from half a mile to a mile.

In support of the opinion we have delivered of the origin of our late fever, we must add further, that in that part of the city which lies between Walnut and Vine-streets, and which appeared to be free from the effects of exhalation and the noxious air of the ships, there were but few cases of the fever which appeared to spread by contagion, even under the most favourable circumstances for that purpose.

Having pointed out the nature and origin of our late fever, I hope we shall be excused in mentioning the means of preventing it in future.———These are,

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It is impossible that Philadelphia, amidst the numerous sources of vegetable exhalations could have one street or lane free from this miasma. And the dispersed cases which happened between Walnut and Pine-streets were produced by a less quantity of these vegetable effluvia.

There is not in this, nor any other writings of the Physicians of Philadelphia, a single case accompanied by even the probability of contagion. And there is no axiom more evident than that the bilious or yellow fever is not contagious.

* First. A continuance of the present laws for preventing the importation of the disease from the West-Indies, and other parts of the world where it usually prevails.

Secondly. Removing all those matters from our streets, gutters, sellers, gardens, yards, stores, vaults, ponds, &c. which by putrifaction in warm weather afford the most frequent remote cause of the disease, in all countries. For this purpose we recommend the appointment of a certain number of physicians whose business it shall be to inspect all such places in the city, the nothern liberties and Southwark, as contain any matters capable by putrefaction of producing the disease and to have them removed.

Thirdly. We earnestly recommend the frequent washing of all impure parts of the city in warm and dry weather, by means of the pumps, until the water of Scuylkill can be made to wash all the city ; a measure which we conceive promises to our citizens the most durable exemption from bilious fevers of all kinds, of domestick origin.

Fourthly. To guard against the frequent source of yellow fever from the noxious air of the holds of ships, we recommend the unlading all ships cargoes liable to putrifaction at a distance from the city, during the months of June, July, August, September and October. To prevent the generation of noxious air in the ships, we conceive every vessel should be

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* This disease can only be imported from the West-Indies or any place by vessels arriving from those places with putrid water or vegetables in their holds, &c. As this fever uniformly arises from the unsound vegetables or putrid water, and never from clothes as the vehicles of contagion, or bodies which may be diseased on board such vessels; our chief attention, instead of being confined to the persons, should be directed to the state of the cargoes; which by the by ought always, during the sickly months, to be examined before they are suffered to come so near a city as that the noxious air can injure the inhabitants.

I have not annexed this appendix solely for the purpose of adjusting the etiquette of science, but in order to preserve the excellent preventive observations contained in the reply to the request of the governor of Pennsylvania.

obliged by law to carry and use a ventilator, and we recommend in a particular manner the one lately contrived by Mr. Benjamin Wynkoop.—We believe this invention to be one of the most important and useful that has been made in modern times, and that it is calcutated to prevent not only the decay of ships and cargoes, but a very frequent source of pestilential diseases of all kinds in commercial cities.

In thus deciding upon the nature and origin of our late fever we expect to administer consolation to our fellow citizens upon the cause of our late calamity, for in pointing out its origin to the senses, we are enabled immediately and certainly to prevent it. But while the only source of it is believed to be from abroad and while its entrance into our city is believed to be in ways so numerous and insidious as to elude the utmost possible vigilance of health officers, we are led in despair to consider the disease as removed beyond the prevention of human power or wisdom. It has been by adopting measures similar to those we have delivered for preventing pestilential diseases, that most of the cities of Europe, which are situated in warm latitudes, have become healthy in warm seasons and amidst the closest commercial intercouse with nations and islands constantly afflicted with those diseases. The extraordinary cleanliness of the Hollanders, was originally imposed upon them by the frequency of pestilential fevers in their cities. This habit of cleanliness has continued to characterize those people after the causes which produced it have probably ceased to be known.

In thus urging a regard to the domestick sources of the yellow fever, we are actuated by motives of magnitude, far beyond those which determine ordinary questions in science. Though we feel the strongest conviction that the value of property, the increase of commerce and general prosperity of our city, will be eminently forwarded by the adoption of the foregoing propositions, yet these are but little objects in our view when compared with the prevention of the immense mass of distress which never fails to accompany a mortal epidemick. We consider ourselves morever as deciding upon a question, which is

to effect the lives and happiness not only of the present inhabitants of Philadelphia, but of millions yet unborn in every part of the globe.

We are with the greatest respect,

Sir,

Your very humble servants,

Benjamin Rush.
Charles Caldwell,
William Dewees,
John Redman Coxe,
Phillip Syng Physick,
James Reynolds,
Francis Bowes Sayre,
John C. Otto,
William Boys,
Samuel Cooper,
James Stuart,
Felix Wise,
Joseph Strong.

His Excellency
THOMAS MIFFLIN.

From Dr. Davidge to Dr. Barton.

DEAR SIR,

I AM entering on a compartment to which I am no stranger. Its topography is familiar to me: I have traversed its fields; passed through its walks; and explored its most intimate recesses, with great industry, care, and minute observation. At least every day of my life, I am engaged in the contemplation of the objects of this divison, and ought, in some measure, to be acquainted with them.

To be qualified to judge accurately, and well, of what is without the scheme of nature, we should be well acquainted with what is within her plan. A partial view of her operations will lead us into error, and inconclusive deduction. When I cast my eyes over the vast scenes of nature, I with much delight observe her plan; I see animals, vegetables, and minerals: animals and vegetables living, and reproducing; minerals growing, each after its own kind.

When I look into the body of a human female, I behold organs of higher and lower importance: the lungs to oxidate the blood, and the stomach to digest the food, the kidneys to secrete the urine, the uterus to bear the young &c. The

uterus and the ovaria, with their appendages, are, equally with the stomach and lungs, provided by nature. They are not morbid productions. Each organ by nature is destined to certain functions, or offices. Am I deceived in this? Can the ovaria and uterus be organs of nature, and yet conception and gestation be "grades of disease?" Can any human mind, in its calmer moments, when relieved from the importunate entreaties of a new-born theory, conceive any thing so incongruous, as that the ovaria and uterus are organs of nature, and yet conception and gestation are gradations of disease? Let us examine into this opinion.

In every perfect female body, we find organs of generation, as well as organs of digestion and respiration. Our venereal appetencies are as regular as our appetites for aliments of life, or our demands for the renewal of the oxigenous stimulus to our blood; and it is as natural to gratify the one as the other. If we cease to gratify our appetites for food, or demands for respiration, we die; if we cease to indulge our appetites for venery, our kind becomes extinct. The former are death to the body; the latter is death to the race.

Now, seeing that the organs of generation, with their appetencies, are strictly conformable to the original intentions of nature, with what modesty or apology do we say, that the result

of these appetencies, and the functions to which the organs are destined, are gradations of disease? Can an organ with its instinct be natural, and nevertheless the only function of which it is capable be morbid, or a gradation of disease? Whenever a function is performed agreeably to the fundamental and immutable laws of nature, and without which intermediate function the grand scheme of nature becomes broken and discontinuous, we intelligibly and with understanding argue such function to be natural, or within the plan of nature. Of the grand though inscrutable catenation of human reproduction, that pregnancy is a distinguished link, no man in the possession of a sound mind can entertain the most extenuated doubt. If, then, a link connecting and alone appending the subsequent to the precedent generation, how can it be said to be a disease? A disease is a mere contingency; a contingency declarative of an aberration from the healthy economy of the animal body. Disease is an accident to which nature is liable, but no part of her economy can it constitute. But conception and gestation are the very work of the maturity and health of the animal body.

Were pregnancy a disease, or, as gentlemen please to phrase it, a *grade* of disease, then were pregnancy, in its inception, progression, and termination, truly fortuitous and incidental. The sublime order of the universe would be forced from its connection, its great design be marred,

ruined, and a second chaos involve its beauties. Pregnancy is within the control of uniform, determinate laws, consecutively subject to the common government of the economy of the body. The uniformity and regularity of utero-gestation are inferrible from the fixedness and constancy of its term.

That pregnancy is laid out and planned, in the deep, unsearchable projection of nature, is deducible from the accordance of the marriage state, bearing children, with the most perfect health. Not unfrequently, indeed, the stimulus of matrimony bears the body above diseases, not otherwise manageable by our art. That women, while pregnant, should be favoured with a total exemption from disease, is not at all consequential of the position, that gestation is a condition of nature. Nature may as readily be encumbered by disease, its various ramifications entwining about her springs, in one part of her motions as another. Dyspepsia at times invades the stomach, but still digestion is a natural function; tubercles may be diffused through the lungs, interrupting their play, nevertheless respiration is a healthy animal operation.

Can a part of the economy of nature be so ameliorated, by medical aid, as to be better suited, in its relations and operations, to the purposes of its office, than it is by its original constitution? I answer in the negative. Then why shall we

break in upon nature's works with our agency? Gentlemen not only upturn the foundations of the physical world, but they dream of suspending the very denunciations of Heaven on the point of their lancet.

During gestation, nature may, in her economy, be encumbered by disease; and the plan of her procedure be disturbed. But, as the cause of this disturbance is not uniform in its specifick relations, neither can the means appealed to be without variety in their kind. To the particular character of the disease, with which the pregnant female may be oppressed, the remedies, in extent and nature, must be accommodated. If the powers of life fall low, and she is unnerved by languor, arising from luxury or poverty of diet, let her be advised to suitable nutriment, cordial beverage, exhilarating company, and regular exercise. If the stomach be distressed by dyspepsia, she is directed to magnesia, alkalies, emeticks, tonicks, or lime-water; to which we add exercise, the first and best mode of infusing vigour into the system, with all its organs. If the bowels be slow, aperients are pressed into service. If the body be raised in its action by too high stimulation, general or local, the circulation is to be tamed, the system tranquillized, the rigid fibre relaxed, by the abstraction of blood; which abstraction, in degree, will be according to the accidents of each individual case, the general hurry, or local impediment. All general

rule is inadmissible; there can be no uniformity of usage, in things of themselves contingent.

As I advance, I find that I have some matters of etiquette in science to adjust with an ingenious gentleman of Wilmington, Dr. Vaughan, and also with a learned lecturer of Philadelphia. While I use towards these gentlemen every personal civility and courtesy, I must be permitted to indulge in liberal criticism on their sentiments. Error in youth is excusable, but in teachers should be chastised.

I proceed, in the first place, to notice, in detail, some propositions from the pen of Dr. Vaughan, a gentleman of much character and ingenuity, and who, by writing, has become a teacher in the art of Midwifery. This gentleman lays it down, as a part of his ground-work, that "several pounds of blood are retained with the mother, and transformed into fœtal organization, and that this fluid is the menses, reserved during ten lunar months."

Dr. Vaughan promised, that, "after giving Mr. White's opinion, with its authorities, in his own language, he would reply to them in detailed order." He then subjoins: "It is immaterial to the present question, whether the catamenia be occasioned by general plethora or not; if a given quantity of the sanguiferous fluid, ordinarily discharged by essential laws" (surely

not a morbid hemorrhage dependent on essential laws), " and retained in the pregnant state, the consequences are the same." But if this given quantity be unequal to the sum of the child's weight, the consequences will be very dissimilar: and that it is unequal to it, we infer from the respectable testimony of Dr. Vaughan himself.

In his foot-note, he says, that " the weight of the full-grown fœtus, and its appendages, so far exceeds the ordinary sum of the catamenia in ten lunar months, that other excretions must be lessened in a considerable degree." Rep. v. 6, p. 152.

This foot-note gives to me all I contend for; it maintains, in the face of all opposition, that there cannot possibly be " a retention or accumulation," in either the general or uterine system, during the latter months of pregnancy. And especially if there is this contribution laid on other excretions, what occasion can there be for the use of the lancet?

The Doctor then refers us in a summary way, to the opinions of Dr. Rush: opinions always respectable, and entitled to the highest deference from the medical world. But opinions and detailed arguments are not the same, in my view.

At this moment my pen is arrested by the melancholy tidings, that my able and worthy

correspondent has paid the great debt of mortality. In his mind burned the lamp of science; from his heart rose the incense of piety; and through his actions flowed the warm stream of benevolence. But, alas! nature has her demands, and in a tone and style too forcible and intelligible to be misunderstood, speaks to her son: "dust thou art, and unto dust shalt thou return."

The menses are, with me, a natural secretion from the arteries of the womb under ovarious influence. They are the first to premonish even the tender virgin herself of nubile maturity. They inspire their lovely authoress with new desires, admonish her to new hopes, and throw about her air all the ornament and force of irresistible captivation, attraction, and grace.

When this conceptious female, from sexual intercourse with her manly companion, becomes pregnant, the uterine arteries cease their functions; a new, though temporary, organ begins its office. The placenta, endowed with the function of a gland, provides nutriment for the fœtus. There are solitary cases, in which a species of morbid hemorrhage, or vicarious menses, continues throughout pregnancy. Is this efflux of blood from the vessels of the os tincæ?

But to return, and fairly examine into the merits of the proposition, giving it the fullest

possible bearing on the subject. I will, for the moment, concede the point before us. Let the menses be formed, and be retained, according to the pleasure of the writer. The total sum of fluid, amassed during nine calender, or even *ten lunar* months, would not be, upon the most liberal estimate for fœtal organization, more than five pounds of aliment, allowing six ounces to each menstruous period. An ordinary child weighs from ten to fifteen pounds: that is, from five to ten pounds more than the total sum of the fluid retained.

How far does this go to demonstrate, that fœtal organization derives its sources from the menstruous fluid? And here I might, not inaptly, subjoin a second problem: If the human fœtus be made up of the menstruous fluid, whence are the materials for the organization of the young of the lower animals furnished? They have no menses.

The Doctor's proposition extends much farther. It not only insinuates, but directly maintains, that this fluid is retained, and yet is transformed into fœtal organization! What! is the menstrual fluid retained with the mother, constituting a source on which we may advantageously draw with the lancet, and moreover is transformed into fœtal organization? This is a species of logick, to my understanding, neither forcible nor clear.

The doctrine is wholly wrong, both in its premises and conclusion; and, of all hypotheses within the reach of a sprightly fancy, the most unhappy in its deductions for the general rule of blood-letting, with which it was proposed to quadrate.

Still further to illustrate his favourite doctrine of blood-letting during utero-gestation, this gentlemen refers us to the concentration of excitement in the gravid uterus, and the extra-vitality of impregnation; to the tenacity of pregnant females for life, and the surviving existence of the fœtus in utero, after the death of the mother. I am not prepared to determine, whether an impregnated female is more vivacious than an unimpregnated one. There are not sufficient documents before the public, for us to enter on this disquisition. That a fœtus in utero survives five minutes, or even two, the death of the mother, is what the facts hitherto furnished by faithful obstetrick record directly oppose. The child ceases to live, immediately after it ceases to receive, through the mediation of the placenta, oxigen from the mother's blood. The chick may have its blood oxidated through the shell with which it is encased, but the human fœtus cannot have this necessary supply through the walls of the abdomen.

Obstetrick history expands to our view many instances of judicious, well-managed efforts to

ve the child on the death of the mother; but eir ill success has devolved the iteration of ese efforts on those who can believe, that the tus can live without the constant renovation the oxigenous stimulus.

We have no materials of which the theory of e continuity of circulation, between the moth- and child, can be constructed. Injections, inner than red-blood, have not, as yet, found eir way from one to the other. And, were e circulation continuous, and immediate, a feless mother could not support a living child. he alimental supply before birth, as before ob- rved, is by means of the placenta, acting as gland, as it is subsequently furnished by the ammæ.

Dr. Dewees, in his thesis, the mirror from hich all the features of his doctrine are reflect- d to us, adopts the hypothesis, "that they pregnancy and parturition) ought to be consi- ered as diseases, according to the opinion of r. Rush, one of the greatest ornaments of me- icine, in the present or any antecedent age. his he infers from the necessity, in too many nstances, a few cases only excepted, of our be- ng obliged to mitigate *their* violence, or shorten *heir* duration*."

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* "Pregnancy," "Though a *natural* alteration of the animal conomy, which every female seems originally formed to under- o, and hence *not* to be considered as a state of *disease*," &c. *Encyclopædia.*

To this I can affix no definite, determinate idea. "Shorten their duration!" And are we obliged, in the general, to shorten the duration of pregnancy? The Doctor cannot possibly be serious in this; yet he says, that, "we are *obliged*;" it is not a point of choice, "to mitigate *their* violence, and shorten *their* duration," viz., the *duration of pregnancy* and parturition. Then pregnancy has no fixed, legitimate term! It is as salutary and regular at six, as at nine months! Would the extraction of a given quantity of the blood of the chesnut-tree improve the maturation of its nut, or aid the evolution of the burr?

A rose, whether cultivated in America, or transplanted to the soil of Russia, howsoever altered in its foilage, its efflorescence, or the tints of its petals, continues to be one of the chief *natural* ornaments of the parterre. Pregnancy remains, amidst all the mutations of climate, and cultivation of civilization, a natural condition; an indefeasible right of nature.

"The uterus is a hollow viscus, in which the great *object* of conception is performed." To my understanding, this sentence is extremely obscure; it is wholly unintelligible. "The uterus is a hollow viscus, in which the great *object* of conception is performed." The author certainly does not wish to convey the idea, that the uterus is the organ of conception; and yet to me,

the sentence is unsusceptible of any other interpretation. It leaves no room for his brilliant discovery of the passage of the second ovum, in case of superfœtation, along the fallopian tube; its separation of the membrana decidua from the parietes of the uterus, and getting within this hollow viscus. Of all the discoveries in the mystery of generation, this is by far the most splendid*. An embryon, with its water and involucra, the whole not equiponderant with two grains, forcibly breaking up the attachment of the membrana decidua, and making for itself room in the cavity of the uterus! Does this embryon force its way by mechanical powers, or melt down the band of attachment between the lining membrane and wall of the uterus, by certain chemical properties?

The sentence in itself is a handsome display of the ability with which some gentlemen are favoured, of making a pretty arrangement of words,

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* "This resistence will, however, be soon overcome; either by the ordinary efforts of the tube, or by the ovum resting unusually long, and beginning to develope, obliging the mouth of the tube to open," &c. *Museum*, V. I, No. 2, p. 172.

What efforts are these, that can overcome the resistence offered by the attachment of the lining membrane of the gravid uterus? The more the ovum developes, the less the probability of a passage through the mouth of the tube. The ovum, under these circumstances, must remain in the tube, and constitute an extra-uterine fœtus. Perhaps the ovum would travel into the uterus, as Dr. Harrison's semen would travel out of it.

See Museum, V. I, No. 1, p. 39.

without infusing into them the power of making a definite impression on the reader's mind.

But again to the thesis. "However easy the act of child-bearing be, among savage tribes, and certain individuals in various states of society, we find it, among others, an operation of great pain, and frequent danger."

Here is an assumption of principles, upon the begged question; a gratuitous assumption of the very points at issue. The sentence, with the following parts of the paragraph, of which it is a member, embraces as acknowledged facts, two points: the one, that the parturient act with the savage is uniformly easy, without pain or difficulty; the other, that, with the civilized woman, civilization and refinement have produced difficulty, pain and danger. Neither of these positions are true.

The first rests upon the insufficient grounds furnished by the reports, vague and highly questionable, of travellers. Travellers are privileged men; in an especial manner so, when they undertake to write and speak of things, to which they could not possibly have access. Whatever is uncommon, or without the usual course of things, is, by a savage, uncultivated people, in a high degree deserving of notice; is among the first objects of communication to a stranger. The reports, in themselves, convey to me the

fullest satisfaction that they are not the facts of observation; that they are mere unusual, extraordinary matters, which, from their infrequency and being out of the usual course of things, become the marvellous points of communication. But even the authors of these wonderful tales do not tell us, that the labour with the savage is without pain. From these persons we collect the information, that, among this unsettled sort of people, there are no persons, male or female, devoting themselves to the obstetrick art. If this be correct, whence is their information? Have they made it a matter of private enquiry with the individual Squaws?

Are there no instances of preternatural presentation among the Calabrian societies; such as that of the arm, back, or belly? Would these too be without pain or difficulty? These tales of uncivilized life are told to us in a most uncivil manner. They are an indecorous attack on the understanding of every man.

The savage, the negro, and the poorer sort of peasantry, are in their condition nearly similar. They present not dissimilar phenomena to the eye of observation. And with them there is naturally, and originally, in relation to the commencement of labour, an equal degree of difficulty, pain, and danger. Nature, in the general, is upon the same scale, and is safe; I speak of the negro, the peasant, and the savage. Mis-

chief is the result of rude and clumsy art, in the hands of adventurous ignorance.

In fine, we have no authorities upon which we can, with safety, proceed in our investigations into the state of parturition, amidst savage life. And what are we to think of the narratives of men, totally without the means of instruction, in respect to the affairs of the woman of the forest, when a lecturer can publish to the world, "that the labours of the brute are not generally attended with pain, or difficulty." Have any of these intelligent travellers been at the couch of the lion, or the lair of the wild-hog, the den of the wolf, or the hole of the fox? The female horse may die with her foal in her matrix; there are birds of prey and quadrupeds, fierce and carnivorous: who is to save from the talons of the one, and the jaws of the other, the objects of our research? In speaking of the brute, the Doctor cannot certainly allude to such as browse our meadows, bound along our plains, or to the trusty animal that faithfully guards the shepherd's flock. Such news, bearing the stamp of authenticity, would be joyous tidings to the farmer.

The Abbot Raynal wrote a book, the object of which was to prove to the credulous world, the deterioration of all animals, even man, in this western world. From whom did the Abbot get his information? From travellers. From

whom do learned gentlemen get their information? From travellers.

From the most correct view I can get of the subject, I am inclined to believe, that natural labours (all labours are natural in themselves, and only unnatural in their circumstances) are as frequently interrupted by rigidity of the os uteri, with or without inflammation, among the laborious negroes of the farm, or the peasants of the thatched hut, as among the most delicate ladies, who live amidst all the luxuries of civilization and refinement; indeed, much more so. My opportunities, on this head, are not very limited.

It avails nothing to say, that we have departed from the life of the savage, and that an equal departure from the simple dictates of nature, in search of a corrective, is necessary. With the savage and with the civilized woman, the mechanism of labour is the same. There is a certain compound resistence to be overcome by the cooperation of given forces of expulsion. A change from the savage mode, to that of civilized life, does not, and cannot, alter the relation between this sum of compound resistence and these forces of expulsion. It neither abstracts from the aggregate of one, nor diminishes the efficiency of the other. If the one be altered, the other is equally altered. They are equally dependent on the same economy of the general whole.

Can we believe, with the lecturer, that "the man (a specifick term, by which an individual is put for the kind) of the civilized world, has lost much of his original strength," &c., and yet, that this man, thus plundered of his powers, is more subject to inflammation, &c.? Sir, what do you think of that philosophy which points out the lax fibre, the body with ruined energies, as the proper subject of rigidity, the very object eligible for the lancet? Can your ingenuity unfold to you, how the delicate lady, whose bed is down, and whose life is inaction, can be more obnoxious to inflammation, rigidity, &c., and better able to bear large abstractions of blood, than the wild savage, whose body, like that of the hardy rustick, is braced by exercise, whose blood is pure and rich, from a simple, yet substantial diet?*

But, to fill up the measure of this singular philosophy, the lecturer declares, that, although

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* We generally find the women of the country more obnoxious to it (pain) than those of *cities*. *Museum*, Vol. I, No. 3, p. 280. *Dewees*.

And this is, I suppose, a logical deduction from the proposition, that "pain is produced by civilization and refinement!" Then the hard-working woman of the city, and the laborious of the country, are the civilized; and the delicate, *refined* lady of the city is the uncivilized woman. Brydone certainly did not add to the facility of birth with the Sicilian women, that they were also "savages." In the Museum, all the advantages are with the lady of the city. In the essay, she has lost every thing "by civilization and refinement." See *Dewees*, p. 43, &c.

the man of the civilized world has lost much of his original strength, "the circular muscles, the heart and intestines, as far as we can determine, have lost nothing of their primitive powers." Now, if the circulation, which depends on the heart, and the digestion, the office of the alimentary canal, be in the vigour they were in during the days of our first parents, the man of the civilized world cannot have fallen off much. Where there is a vigorous circulation, and strong digestive powers, the muscular energies of the body cannot be low. I allude to those muscular energies (I believe the long, straight muscles to be concerned) which qualify the Hibernian, in proud contempt, to poise his shilelah, the nervous Englishman to shake the very walls of the theatre of pugilism with loud huzzas, when he has pushed his unequal antagonist on some unresisting spot; which qualified the proud Roman to project the coit, or throw the javelin; the alert Greek, covered with sweat and dust, to carry off the prize in the gymnastick circles; or brawny American to turn, in coarse exultation, from his fallen competitor.

The doctrine of the circular muscles is new; I am not prepared to receive or admit it. I believe it to be wholly inadmissible, from our present anatomical knowledge, and social observation.

Dr. Dewees talks as lightly and familiarly of labours "without pain,"* as if they were the objects of daily observation. In the examination of matters of science, we are necessarily restricted from all loose modes of expression. We are not to be indulged in tropes, and figures, and flowers of rhetorick, by way of decoration to our subject. I am a good deal sceptical about labours without pain; and, when I look at the immense volume of female experience, as it is unfolded in every age, and every nation, I am led still farther to doubt. The world should, at least, have furnished *one* case, where the throes of labour have been passed through without pain. I have not, as yet, read one honest account of such a case, except where the sensibilities were benumbed by stupor, suspended by syncope, or annihilated by death.

The ancient doctrines of Boerhaave make but an awkward appearance in their new American dress: doctrines that enlightened science had committed to the stream of time, that they might

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* "And of women delivered without pain, it would be idle to cite them, as they must occur in every man's practice." *Dewees, Essay*, p. 43.

I have never met with one of these labours without pain; and, to my recollection, neither Smellie, nor Hamilton, nor Denman, nor Baudelocque, nor La Motte, nor Levret, nor Louverjat, nor Walker, &c., &c, speak of such a thing, where the body retained its sensibilities. These men write of easy labours with little pain, and yet their practice was not among savages. Were these painless cases of the Essay among civilized people?

no longer reproach the understandings of professors. That pregnancy is a state of disease, was taught by Boerhaave to his pupils, and commented on by Vansweiten, in his illustrations of Boerhaave's Aphorisms.* It is like many other discoveries that are now-a-days fallen on.

The first few pages of Dr. Dewees's pamphlet excited my surprize; but, when I arrived at the article upon blood-letting, I was overwhelmed with astonishment. He gravely tells us, that in labours attended by rigidity of the os uteri, with or without inflammation, with irregular contraction, &c., the utility of the lancet originated in his own observation, and with himself! That "in diminishing pain, disposing the os uteri to dilate, the external parts to unfold, &c., blood-letting originated, as far as I am acquainted, with myself."† Are all preceding writers

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* "Morbi gravidarum." "Postquam gravida est fœmina, plurimis afficitur malis ex sola graviditate oviundis. *Boer. Aph.* 1293.

"Facile patet, hic tantum agi de illis morbis, qui a graviditate, tanquam causa, pendent, non autem de illis, qui gravitatis tempore contingunt quidem, verum aliis causis originem dederent." *Vansweiten.*

With parity of reason Boerhaave might have said, that the function by which the blood is oxidated is a state, a gradation of disease, because, in the first acts of respiration, children utter cries of apparent alarm and pain. Certainly such things can only point out to us the present general state of suffering humanity.

† Essay, p. 63.

on general midwifery silent on this head? Or is the Doctor's reading limited to his own writings? Each writer must speak for himself.

"In lingering labours, when the parts are *rigid*, if the patient is of a plethorick habit, with quick strong pulse," says Smellie, "the contrary method (opposed to cordials) is to be used, such as *venesection*, antiphlogistick medicines,"* &c. Here is the doctrine recognized by old Smellie, an unfashionable writer it is true, in all its principles. He advises venesection in rigidity of the softer parts. For what purpose? Surely "to dispose them to dilate, to unfold" before the head of the child, to prevent or cure, as the case might be, inflammation. He could not, nor could any other rational man, suggest to us blood letting, merely for the ceremony of the thing. He must have intended it to be in its extent commensurate with the circumstances of the case.

Thus, where Baudelocque remarks, that "a bleeding made to the purpose (a happy expression), with emollient, mucilaginous injections the warm-bath, &c, * * * * can relax the *rigidity* of the neck of the womb, and render its di

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* Smellie, Vol. I, p. 221.

† "*Une saignée faite à propos*, des injections émolientes mucilagineuses, les bains * * * * pour *affoiblir la roideur du col de la matrice, et en faciliter l'ouverture.*" Tom. I, par. 114. *Baudelocque.*

latation easy." He does not restrict us to any given quantity. There is, however, but one thing to be understood. The bleeding is to be "*à propos ;*" suited to the circumstances of the case, as to extent, and done in time. What reasonable man could have said more ?

In the same strain, we observe the opinions of Denman to run on "fever and local inflammation" (rigidity with inflammation). This excellent accoucheur says, that "It does not seem necessary to bleed every patient on the accession of labour, and for some it would be highly improper. But whenever the feverish symptoms become violent, it (blood-letting) is, I believe, *universally* proper ; the quantity of blood taken away, being suited to the degree of fever, and to the constitution of the patient."* If the fever arises from local inflammation, as we understand from the head of the section, and as there must be *pain* where there is local inflammation producing fever, he assuredly prescribed blood-letting "for the diminishing of pain."

A little farther on, a few pages only, the same judicious man adds: "For the prevention of such difficulties as may attend the first act of parturition, in those who are advanced in age, we have been advised to order frequent and small bleeding towards the conclusion of pregnancy."

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* Denman, Vol. II, p. 50, 59.

For what is this advice? For no other purpose that I can devise, than "to dispose the rigid os uteri to dilate, and the external parts to unfold."

To those already adduced, I will add Hamilton, who tells us, that in "crampish spasms of the belly," or viscera within the belly, we are to have recourse "to *venesection*, glysters, &c. And in the subsequent paragraph he subjoins, that "inflammatory diathesis, in young subjects of strong rigid fibres, and plethorick habits, must be obviated by *venesection*, repeated glysters,"* &c.

It will now appear that blood-letting has been used, by those writers that are well entitled to a place in the library of every medical gentleman, "for diminishing pain, disposing the os uteri to dilate, the external parts to unfold," &c.; and that these writers recognize and enforce the doctrine of blood-letting in its fullest extent, so far as rational men can go. The doctrine, with its principles, is all that can be contended for. The application must rest with the judgment and discretion of the practising accoucheur.

In the quotation of the 1960th paragraph of Baudelocque, there is great injustice done the learned Frenchman, as it is cited in the thesis. Why were the "proper methods," so hastily

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* Hamilton, p. 156.

passed over? Are they undeserving of notice? What are those "proper methods," that are to have place before the accoucheur is to think of the cutting instrument? Turn to paragraph 1145, and you will see it to be nothing less than what the learned lecturer claims as his discovery, "blood-letting," &c.

It may not be amiss to remark, that there are two states of unyielding os uteri: the one from an inflammatory disposition, the other from a cartilaginous state. How the lancet will succeed in real cartilage, we are scarcely prepared to determine. The bistoury should be the last thing thought on by the accoucheur. This is Baudelocque's doctrine.

That, by a loss of blood to any extent whatever, the practitioner will prevent the disease called the "swelling of the lower limb of the lying-in woman," Dr. Dewees will not again assert, when he shall have taken a better and nearer view of that disease. It is as common in feeble, exhausted habits, as in robust, plethorick ones. I write from observation. See also Charles White, of Manchester, Denman, &c.

Of cicatrices I have no experience; but I can have no very exalted opinion of that remedy, which, after several repetitions, leaves the case to be relieved by the forceps. The lancet would be proper without question, so far as irritation

and inflammation might be threatened from the effects of long-continued, violent throes.

From what has been premised, I deduce the following conclusions:

1. That pregnancy, whether with the savage or civilized woman, is a state of nature, and parturition an act of nature, but subject to interruption by disease.

2. That the doctrine of pregnancy being a disease is inconsistent with the order of nature, a variance with common observation, in itself indefensible, and of the school of Boerhaave.

3. That the rigid os uteri, with or without inflammation, is a rare occurrence, there not being more than ten cases in a thousand of ordinary practice, where the accoucheur would be called on for the aid of his art.

4. That blood-letting has been considered and prescribed as a remedy, "for diminishing pain disposing the os uteri to dilate, the external parts to unfold," &c., by Smellie, by Denman by Hamilton, and by Baudelocque.*

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* "I am aware that this remedy (blood-letting) is not a new one to a certain degree; but, I believe it has never yet been advised nor used in the copious manner, nor regulated by the morbid phenomena" (rigidity, local inflammation, unyielding disposition of the soft parts, &c.) "of child-bearing, which have been mentioned." *Rush. Rep.* vol. VI, p. 20.

I will close this letter, with a short admoni-
on to young practitioners. It would be well for
entlemen, when they enter the room of a lying-
n patient, to carry with them, not their lancet,
or I have understood that those who wear the
word are apt imprudently to use the sword, but
good stock of composure and patience, and
onfidence in the resources and abilities of na-
ure; and, above all, a sound, discriminating,
ell-instructed judgment. Thus provided, I
hink that I can assure them, they will seldom
eave the room with feelings ungrateful to them-
elves, or a reputation disadvantageous to the
art of the accoucheur.

In my next speculation, I shall confine my pen
o the mechanism of labour. I shall endeavour
o demonstrate, that if the lancet will relax the
softer parts through which the child is to pass, it
will also reduce the efficiency of the throes, ne-
cessary to the detrusion of the child; and, that

"This remedy (blood-letting) is by no means a new one, in labour; but employed for the express purpose of diminishing pain, and subduing the various species of rigidity just spoken of, and carried to an extent that will ensure these objects, that is, diminishing pain, disposing the os uteri to dilate, the external parts to unfold, &c., originated, as far as I am acquainted, with *myself*." *Dewees, Essay*, p. 62.

If, sir, you will compare these two paragraphs, I think you will coincide in opinion with me, that, were they in different languages, the one might, with the strictest justice, be considered as a free translation of the other. Did Dr. Dewees ever read Dr. Rush?

where one ordinary case will be benefited, one hundred will be injured, by the lancet.

I am yours, &c.,

JOHN B. DAVIDGE.

Baltimore, March 25th, 1807.

From Dr. Davidge to Dr. Barton.

DEAR SIR,

IN my last I gave a promise, that, in this my subsequent speculation, I would restrict my pen to labour; to the symptoms proper to labour; and to the forces by which it is effectuated: that I would endeavour to demonstrate labour to be morally and physically painful; and this, from the present condition of humanity, to be of necessity, and not of contingence. I moreover promised, and in this the promise chiefly consisted, to demonstrate the inadmissibility, in ordinary, natural labours, of the use of the lancet; that, in ninety-nine out of the hundred ordinary labours, it must be productive of unpleasant consequences to the parturient patient, and, when copiously used, it must retard labour. I now take up my pen to fill up the measure of my promise.

We will suppose the woman to be entering on her ninth month, as, until the first ten days of that shall have elapsed, there is no symptom of approaching labour. Within about three weeks of the period of actual parturition, the woman begins to percieve a subsidence in the epigastrick and hypochondriack regions. This subsidence is not a mere chimerical deception of the sense of the woman; it is an actual change, and becomes more and more the subject of regard, until the period of confinement. This subsidence has been a source of serious contemplation to the woman; she has mistaken it as indicative of the death and waste of her infant. With some accoucheurs it has been a subject of idle animadversion, being construed into the collection and dispersion of wind in the stomach and bowels; whilst with others, better trained in the school of observation and experience, it has constituted a part of the uniform design of nature.

To those who are acquainted with the regular evolution of the gravid uterus, I need scarcely remark, that this evolution is in divisions; that of these, the fundus may be considered the first division, and is the first in evolving; the corpus the second, and has the second place in suiting itself to the increased dimensions of the child; and the cervix as the third, and the last in yielding to the growth of the contained fœtus. This last division does not lend its aid until towards the eighth month, from which time it in regular

progression evolves to meet the expanding dimensions of the fœtus.

When this is nearly completed, the child actually occupies a lower part of the pelvick region than during the sixth and seventh months; and, about the completion of this evolution of the neck of the uterus, the evolving action of the abdominal muscles begins to change, and give place to the powers of expulsion resident in these muscles, and the longitudinal muscular fibres of the uterus. The uterus, from the interest it has in the general sympathies of the system at large, commands into its private services the aid of the neighbouring muscles. By an absolute impress of foreign agency, in co-operation with its own powers, it begins and carries on the function of expulsion; and, in the preparation of these muscles for more serious action, we discover the first cause of the first premonitory sign, the subsidence of the abdomen, of approaching labour.

At full time, when nine months shall have completed their round, the throes of child-birth come on. They may fortuitously be provoked into untimely action, and produce abortion, or miscarriage; but their legitimate term is generally conceded to be that of nine calendar months. If it were necessary, in addition to what I have said in my former letter on this subject, to adduce evidence in attestation of the fixedness and

uniformity of the term of labour, I would invite your attention to the highly interesting fact, that, if a fœtus be extra-uterine, the throes, at full time, will come on with the same force and regularity as if the fœtus were within the uterus. Here we have another argument strongly militating against the preposterous doctrine, that "pregnancy and parturition are grades of disease." The throes to expel an extra-uterine fœtus will not be argued to be derived from the stimulus of mechanical distention operating on the accumulated excitability of the uterus. We assuredly deduce these throes from the laws of the constitution of the mother; for, in this case, the child is not within the womb, and, of consequence, can by no luxuriancy of fancy be supposed to excite into action that organ.

The term proper to the maturity of the child being elapsed, we discover farther and more decisive marks of labour to present themselves. But, before I proceed to the physical properties of the womb, I must say something on the moral necessity of pain.

What I mean by the moral necessity of pain, it may be proper, in a succinct manner, to explain. Notwithstanding the general sympathies of the body may, and to my apprehension do, grow out of the common constitution and economy of body, and would, without the transgression of man, have been attended by plea-

sureable sensations in the various operations of which they are the source, yet I find no difficulty to concieve that those sympathies, in their laws the same, may be accompanied by unpleasureable sensations in the functions or conditions to which they give origin.

In the first period of conception, upon the supposition that Eve had remained in her original paradisaical estate, I can conceive that all the associated actions would have afforded pleasure ; that the very associated action of the stomach, which, in the present condition of woman, causes distress, could, under the state alluded to, have produced happiness.

The body has been, for some time previous to conception, accustomed to have separated from its common circulating mass a peculiar liquor to a given quantity. Upon conception in the general, this particular excretion (the menses) ceases to be thrown off; the body no longer, from its altered condition, throws off this excrementitious fluid, no longer can it need the same supply of aliment ; and hence is the general inappetency of pregnant women for food during the first two or three months, until, by the bulk of the child, greater demands shall be made on the general constitution, and, of course, on the stomach.

From this I wish it to be understood, that the sickness and uneasiness of breeding, as it is usu-

ally phrased, is a moral consequence, while the inappetency for food is, from a law of the female habit, under the control of ovarious influence. Therefore, when we speak of the diseases of pregnancy, I hope that it will be always conceived, that we do not have allusion to this constitutional association between parts, nor to the distress consequent on this associated action, which, in itself, in a higher or lower degree, howsoever opposed by human skill, is inseparable from the present condition of humanity, and forms one among the many irrefutable facts on which rests the authenticity of the bible.

CASTELLIO'S VERSION.

"Deinde ad mulierem: Ego te multis doloribus, inquit, aerumnisque afficiam, tu natos *cum dolore* paries." *Gen.* cap. iii. v. 16.

From this serious part of my letter, you are not to anticipate a formal discourse on theology. This excerption from sacred history is at present necessary to, and may have an advantageous place in, our discussion. Were it a light matter which at this moment solicits our attention, I should most sedulously have avoided an appeal to that authority, the sacredness of which renders it inaccessible, either in language or writing, except when in our solemn, serious meditation, in the stillness of spiritual contemplation, we approach, with a trembling dread and awful apprehension, to enquire into its momentous doctrines, to hear the great ever-living En-

tity unfold, in justice, wisdom, and celestial dignity, his irreversible purposes to deciduous man!

It is palpable, and bears with irresistible conviction on the mind, that the above address to Eve was not directed and limited to her as an individual; it was addressed to her with a meaning diffusive and general, terminable only by the limits of her descendants; as woman; as the mother of all human females.

This heavy denunciation was fulminated against woman, in the same spirit, under the same circumstances, and for the same high crime, as against Adam was launched the terrible menace, "pulvis es, et in pulverem redibis."

This was uttered against Adam as man, the father and head of all living. Both were levelled against our kind, and not restricted to early ages, particular nations, or certain individuals. A few men, by miracle or special favour, have had, in relation to themselves, this malediction rescinded, but we know of no women or race of women, savage or civilized, to whom the curse does not extend, "with pain shalt thou bring forth children." When men cease to be mortal, women will be blessed with painless births.

The text in the English version is very obscure and indefinite; I therefore have preferred that of the learned Castellio, who, in the opinion of the most able criticks, stands among the

foremost in Hebrew literature. And it affords me great pleasure to find that the sense of Castellio is confirmed by the Greek* and French.†

Superadded to what this laborious Hebrician gives in the text, he assures us, in his foot-note, that the words, strictly translated, are "est dolor pariendi." And to it, I am sure, all women, who have borne children, and are in the possession of a sound mind, will most willingly subscribe.

Dr. Dewees indulges in a most curious annotation on the text as it stands in the English version. He most gravely labours to prove, that the word "sorrow" does not, in any part of the sacred writings, mean an uneasy corporeal sensation. It would have been great gratification to all biblical scholars, had the Doctor dilated a little more on the general sense of the text, and given us a full commentary on "I will greatly multiply thy sorrow and thy *conception;* in sorrow shalt thou bring forth children." Perhaps it would have appeared, in the course of his discourse, that woman, in consequence of her defection, had become more prolifick; for to mul-

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* "In the Greek version, or Septuagint, the word *sorrow* is rendered λυπας, which signifies pains or sickness," &c. *Vaughan. Med. Rep.* vol. 6, p. 31.

† "Dieu dit aussi à la femme: je vous affligerai de plusieurs maux pendant votre grossesse; vous ne mettez au monde des enfans *qu'avec douleur.*" *French Version.*

tiply "*conception*," is without doubt to increase fertility.

What can be inferred from "I will greatly multiply thy conception," &c.? Nothing, but that the whole is incorrect, and a stupid blunder of the translator. The Doctor says, that it was not a punishment for transgression, but the mere result of accident or civilization. And yet the very version, and every other version, on which he offers his annotation, expressly declares, "because thou hast done this," &c., &c.

The tonick and clonick powers of the uterus, together with the contractions of the abdominal muscles and diaphragm, are the only actions alleged and recognized, by the concurrent authority of accoucheurs, as contributing their agency to the expulsion of a child from the gravid uterus. If there be others, I am unacquainted with them. All voluntary effort is both useless and improper.

THE TONICK POWER.

It appears to be understood among accoucheurs, yet not with the full and expressed consent of anatomy, that there is in the uterus a double set of fibres; the one circular, the other longitudinal. The circular fibres are diffused equally throughout the uterus; its fundus, its corpus, its cervix. In these fibres is resident

the tonick or elastick power. It perpetually urges the gravid uterus to recover upon itself, as the French would speak ; and no sooner do the throes, co-operating with the circular fibres of the fundus and corpus uteri, discharge the liquor amnii, than the whole of the walls of the uterus, the circular fibres of the cervix, with those of the other parts, close about the child, and in this state would the hapless fœtus, immured within its narrow habitation, remain, until death and putrefaction should waste it away, were not the revival and reiteration of the throes to impel it to the world.

The circular fibres of the cervix are forced into obedience by the superior power of the throes, of which I shall presently speak. The tonick or elastick power never ceases to act when once called on by the laws of the constitution, and provoked by a source of distension within the cavity of the uterus ; and no sooner is the child expelled with its appendages, than this tonick power gathers the uterus up nearly to its original dimensions. Of this tonick action the woman has no consciousness : it perpetually urges, yet without pain.

" 1. That the circular fibres may contract to almost any degree, without being attended with pain. 2. That their contraction alone, however violent, does not forward the child. 3. That they do not possess the power of alternate con-

traction in the same degree as the longitudina fibres; and, that they may exert this power, i is necessary at first to have them distracted b some force or other," concludes Dr. Dewees from previous facts or propositions.

Permit me respectively to invite your attentio to these words from the essay of your lecturer and impress your mind with their value: I in tend, from them and their consequences, to es tablish the important fact, that the lancet, in n *ordinary*, *regular* labour, can afford the practi tioner any possible aid, but must of necessity embarrass the natural progression of the partu rient act, and disappoint the accoucheur wh has unwarily trusted to it.

In no instance whatever has a child been de livered by the tonick power of the womb. I the case recorded by Levret, and the few other collected by writers, although the child was de livered after the death of the mother, its hea must have been in the smaller basin of the pel vis, that is, without the os internum, and in th vagina, otherwise it is very obvious, from wha Dr. Dewees writes, and every other man mus believe, that it must have remained with th mother.

THE CLONICK POWER.

This power of alternate contraction and re laxation, and acting at certain periods or inter

vals, is a property of the longitudinal fibres of the womb, associated with a synchronous action of the diaphragm and abdominal muscles. The abdominal muscles and diaphragm are brought into service by a constitutional sympathy between them and the uterus, such as obtains between them and the stomach in paroxysms of excessive sickness. These alternate contractions and relaxations, or clonick actions, constitute the real throes or pains of labour. They are uniformly, except in stupor, accompanied by an uneasy corporeal sensation, and hence are termed throes or pains. The word throe itself is from the Saxon term, to suffer. No throe can have place without the consciousness of the woman, except, as above remarked, under an affection of the common sensibilities of the body; and, without a throe, the gravid uterus can never disincumcer itself of its burthen.

In every throe there is more or less of an uneasy sensation of the loins, haunches, or uterine region. At times, the lower extremities are distressed with most unpleasant feelings. In cases of convulsion during labour, the convulsive affection simulates very much, in its periodical recurrences, the genuine labour-pains.

From the antecedent premises, I deduce the fact, that, as "the tonick power, however violent, does not forward the child," and as there is no third agency alleged or recognized by phy-

siologists or accoucheurs in the expulsion of the child, there cannot possibly be an act of parturition without throes or pains.

Assuredly the most enthusiastick theorist, who dreams out his cases in his closet, is not prepared to admit or maintain that throes can be present, and yet the woman feel no corporeal distress. This were an absurdity too monstrous for any sane mind to contemplate, but with disgust and abhorrence.

I shall now proceed to the examination of that philosophy which instructs in the abstraction of blood from a general system of vessels, in order to relax a particular set only. This general system, we may observe, goes off from the same heart; is mutually in its parts dependent, and between which parts there is a constant reciprocation of good offices, or injuries, with a continued, uninterrupted circulation of the same common mass of fluid. The circulation through this general system being continuous, and, as it were, in a circle, any quantity of blood abstracted must immediately, and in the ratio of the diameter of each vessel, influence each and every part of the general whole equally.

If any loss of blood will relax the os uteri, vagina, and loca muliebria* (I speak of these parts

* Leci muliebres, vel loca muliebria, are by some classick writers preferred, when they speak of the genital parts of the human female, while they rather restrict the expression puden-

in their ordinary state, free from inflammation, rigidity, or spasm, such as ninety-nine of a hundred labours will be the case), will it not likewise prostrate, in a higher or lower degree, the expulsive forces of the uterus, abdominal muscles, and diaphragm, and equally in their relation? It must unquestionably, and leave the resistance to be overcome, and the powers of expulsion, in the same relation to each other that they were previously to the use of the lancet.

Whatever be the quantity of blood, its relative effects must be the same. If even the woman be reduced to syncope, the child remains in the womb; for where there is syncope, there can be no clonick action, and the child will not be born until the woman shall be restored to her powers.

During utero-gestation, if a woman has incautiously, by excessive exercise, roused the heart and arteries, or the circulation has been accelerated by intellectual emotions, or a fever has been kindled up, the accoucheur abstracts blood: but for what? to relax the parts through which the child is to pass in its birth? by no means. His object is to bring the body more or less to a state of relaxation or syncope, and thus

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da to the genital parts of the female brute. But, perhaps, they are equally proper, and may be used according to the taste or choice of the writer.

to lower the powers of expulsion, to quiet the parturient throes, and save his patient from miscarriage. If, then, during utero-gestation, we are to prevent premature birth by the abstraction of blood, why, and how, are we to facilitate parturition by the lancet at full time? Will not the effects generally be the same? Undoubtedly so.

We think correctly, when we believe that nature has as wisely ordered the relation of her powers in the act of parturition, as she has regulated the graduation of the stimulus of the blood to the irritability of the heart. Not indeed to a mathematical balance, but, according to her own liberal scheme, admitting of some extravagance and aberration. Therefore, every light disease, or improper interference of art, cannot throw her from her design.

Any given quantity of blood, whether from the arm or uterus, should, to my understanding, affect the general system, and, of consequence, every part similarly; and a relaxation of the body, or complete syncope, must be equal, in what manner soever induced. Hence I infer, that no woman should die from flooding; at least without being first disburthened of her load: nor indeed could she possibly die of flooding before the birth of her child, if the doctrine, embraced by the principles inculcated by the friends of syncope being the immediate path to easy and

rapid labour, were founded in the laws which regulate nature.

I write from observation; I am an accoucheur, and have witnessed the effects of bleeding in ordinary labours: it is true, not in my own patients, except such as have become so secondarily. As to labours attended by morbid phenomena, I have already spoken of them in my first letter. The use of the lancet, in such, is recommended and enforced almost by every writer on general midwifery, for fifty or a hundred years back. My present letter only relates to ordinary labour, and neither of them to preternatural or laborious labour.

When we talk of relaxing the vagina and external parts by the lancet, in common labour, we use a sort of language too mechanical, and in no respect applicable to the affair of which we speak. This relaxation, as we are pleased to term it, of the soft parts, is a peculiar and inscrutable evolution or unfolding, which may suggest to us a high veneration for that wisdom which ordered the plan of the parts and their functions; and certainly we may be content with the knowledge of the fact, without entangling ourselves in useless researches after physiology; researches that will ever be unsuccessful, and will only serve to teach us humility.

At best, the science of physiology is a science of conjecture. We know the organs, and we

learn the result of their operations; but of the causation, the physiology of the thing, we know nothing, whether we speak of the uterus, the liver, the brain, or any other organ.

This same unintelligible, mechanical language has been as fruitlessly applied to the explication of the evolution of the gravid uterus. Like the gravid uterus, the vagina and external parts evolve by the particular physical properties of the parts themselves. These properties or laws are a part of the wonderful economy of nature; they are called into service by powers of the female constitution, and should have taught professors that parturition is not a "grade of disease." So soon as disease fully takes place, this natural evolution ceases, and art must open its resources.

I may add, before I close, a few animadversions on the general though reprehensible practice among accoucheurs of artificially supporting the perinaeum during the last act of the parturient operation, as if nature called more for adscititious aid in one step of her course than in another. Can there be any thing more gross and exceptionable in the eye of enlightened philosophy than the supposition that more aid from art would be required to facilitate the passage of the child through the os externum than would be demanded for its passage through the os internum? Surely gentlemen are not serious in their advocation

of this relick of folly, this remain of mistake. It were as rational and becoming to renew the pleadings and reasonings for the baths and macerations to relax the ligaments of the pelvis that it might enlarge in its diameter and afford facilities to the descent of the child, as to appeal to art to aid its egression from the Vagina.

Are those efforts to sustain the perinaeum and save it from laceration the dictates of experience —the doctrine enjoined on the practitioner by the language of observation? certainly not. If left to itself the evolution of the external, soft parts will go on as regularly and safely as that of the mouth of the womb. How difficult it is for the accoucheur to learn a negative science, to know that in the general he has no duty to perform beyond the simple acts of receiving the child, securing the cord, and taking the afterbirth from the vagina! An humble office it is true, yet one with which modest intelligence is content. The man of science reserves himself for scenes of danger and trial, where nature is forced from her ground, or is failing under a load of difficulty. Then by a dextrous application of the important rules furnished by his art, he comes to her relief and guides her through her perils.

I am yours, &c.

JOHN B. DAVIDGE.

Baltimore, April 10th, 1807.

From Dr. Davidge to Dr. Smith.

DEAR SIR,

TO your polite favour I should have returned an earlier answer, but for a disability of my right hand, occasioned by a slight and transient casualty. That you "will derive pleasure" from a correspondence with me, is both flattering and gratifying; but when you demand "improvement," you will permit me to admonish you to make your drafts on other banks than those over which I have control, lest, from a deficiency in credit on my part, your bills return dishonoured.

The *nosology* of a wounded artery is interesting and highly important. Its range in consequences, in all serious, indeed trivial wounds is great, and well entitled to the attention of the surgeon.

You will not, my friend, be startled at the sound of the word *nosology*, the object of which is, an inquiry, you know, into the *nature*, the *symptoms*, and *consequences* of diseases; except indeed, you have become a convert to the new philosophy of the day—except you consider nosology as an unfit subject for the present enlightened period, believing that in dark periods of ignorance, when gross night overshadowed the

chools, such stupid themes had an apology in he dull, unimproved state of the times. But ow that a beam of new light has awakened the nedical world from its long and deep slumber, ou may imagine that a subject more ennobled y novelty, would be less unbecoming. Be it o; and let my present mention of it find an exuse in my early prejudices. But I widely misalculate if I do not in some little speculation, which, if I can rouse myself from my indolence, intend to send you, *demonstrate* that where noology ends, medicine ceases to be a science. But to the subject of the present letter, lest you hink that I have designedly strayed to hunt up he errors of great men.

A sword passes through the fore-arm, and ome small arteries, with a larger one, the raial, ulner, or interosseous for instance, are ivided. The blood pours furiously out for a ime, but is, by coagulation of the effused fluid, nd the coarse means externally applied by the amily, or some generous neighbour, stanched. The surgeon arrives, and finds all things quiet, lthough in homely order. He feels reluctant o open anew the vessels by breaking up the dressing; and begins to hope and persuade himself that the hæmorrhagy will not recur. How ain and illusory!

In a day or two the hæmorrhagy breaks out gain, and is again checked; and thus I have

seen matters proceed, for two and three weeks, until the hand, wrist, and fore-arm have been completely injected, and the patient exhausted to the extremest degree. And what does all this arise from? From one of three things.

The surgeon has read, or heard, for all our surgeons do not read, that if the lips of the wound in an artery be coaptated, whether by good luck or artificial means, they will unite after the manner of a vein, the gluten being poured out by the vasa vasorum. Or, if the artery be completely divided, it will contract up to its first branch. Or, a coagulum without and within, will form, and serve as a stopple to the artery. And in this last, countenanced by the doctrine of the vitality of the blood, he begins to imagine to himself, or talk to those about him, how this coagulum forms its new vessels, and how they stretch out towards the walls of the artery, and how, by uniting with the sides, the plug is secured. Here are three sources of error in the surgeon, and danger to the patient.

I hold it as an axiom in the surgery of a wounded artery, that no hæmorrhagy from an artery will cease, except the sides come into close contact and union.

In the smaller arteries, I conceive, that, by the effused and coagulated blood without and about the sheath of an artery, its sides are forced into union; that is, that they come into con-

tact; that the vessels of the internal coat are excited, and take on that action which brings about what surgeons call the union by the first intention.

What is it in those smaller vessels which, after the patient is put to bed—his heat restored—and the re-action established, causes the recurrence of the hæmorrhage? Plainly and palpably the increase of the current of blood forcing open the mouths of the divided arteries, in opposition to the mechanical pressure without; not the heat or any other cause relaxing the contraction of the extremity of the arteries.

And what is it in the larger arteries which causes them again and again to pour out their blood? To my understanding, the putrefaction and melting down of this coagulum, to which the surgeon unhappily trusts. And as often as the coagulum forms, the hæmorrhage is stopped; and as often as it begins to putrify, and melt down, and fall away, the hæmorrhage returns. If there be a coagulum within at all, it is too insignificant to be noticed; and, it will ultimately deceive the surgeon. I do not speak from conjecture; nor do I rest on the highly respectable authority of Mr. John Bell, who appears to be destined, in his luminous course, to give light to the universe of surgery. But, as I had the pleasure of mentioning to you, I have had an opportunity of dissecting two stumps, at the dis-

tance of thirty odd days from the period of ope
ration. In which, when the vessels were neat
ly and fairly exposed, they were found complete
ly cylindrical and open down to one eighth of a
inch of the very extremity. The wall that clos
ed up and formed the end, was but very little
if any, thicker than the sides measured at any
place above in the most healthy parts.

Here was no coagulum, no plug, no conver
sion into ligament up to the first branch. Bu
the lumen arteriæ was open and free down t
the very point where the ligature closed upon
the walls. This dissection was nearly forty
days from the period of operation, and yet th
artery had all the characters of health—its coat
having no marks of thickness in any part.

There were several medical gentlemen pre
sent during this dissection, and among them my
ingenious and learned colleague, Dr. Cocke; a
gentleman well known to you for his politeness
suavity of manners, and intimate knowledge o
the healing art; and whose mind, we may sup
pose, was well prepared to detect any circum
stance that might militate against the doctrines
of the schools in which he had been educated—
those of London and Philadelphia; and in those
schools the doctrines of contraction and coagu-
lation have certainly not been without friends.

But from what I have advanced you are not
to infer, that I deny the possibility of a union of

the sides of the vessel, up to the first braneh. I admit it; but not from contraction, nor coagulation, but from the walls of the artery being forced into contact by mechanical agency; and this would happen not only up to the first branch, but up as high as the mechanical force acted.

At first, immediately on tying of the vessel, there may be, and really is, a coagulum within—yet this soon melts down, and is mixed with the common circulating mass. And here the surgeon is deceived. He takes the column of jelly, in a few weeks to be melted away, for living substance, filling up the tube.

Yourself and every other gentleman accustomed to the knife, knows, from repeated observation, that when an artery is divided, it retracts, and draws itself up within its cellular sheath; but the contraction of a divided artery even in a slight degree is to be conceded with caution. If an artery is to be provoked by adventitious stimulus, it will contract; and this so long as its excitability shall be exposed to foreign stimulation. But it is not on this that the surgeon is to depend. It is uncertain, irregular, and of small compass.

When an artery is divided, lies exposed, and has its calorick with the fluid, which fills its vasa vasorum, to fly off, it is possible that its canal may be diminished below the natural state of its systole; though this is too unimportant to be

brought into account; and I am induced to consider the contraction of an artery an imaginary thing, equally illusory with the coagulum within the artery, the plug or stopple of which surgeons delight to speak.

Mr. John Bell says, "that an artery, in the moment of being divided, may be made to re-unite, I hold to be an absolute fact, not because it is proved by the authority of those who have prevented aneurisms by a skilful compress; for in such cases the aneurism being prevented, we are still left in doubt, whether the artery was actually wounded! but the aneurismal varix ascertains the fact." vol. i. page 211.

Most singular indeed! that an aneurismal varix ascertains the fact, that the lips of a wound in an artery adhere, if laid well together at the moment the artery is wounded. Or should we not say, that of all facts, the aneurismal varix gives the most decided proof that the wound has not united: for, in the aneurismal varix, the lips of the wound of the artery adhere to the fascia below, while those of the vein adhere to the fascia above, by means of the intermediate cellular substance. If the lips of the artery were united, there could no longer exist an aneurismal varix, as the current of blood from the artery which constitutes the disease, would be cut off by the closing of the aperture in the artery. Even Ulysses would not at all times pass the arrow

through the ring, were Minerva to leave his side.

My opinion is, that we should secure every injured artery by the regular compress or ligature, doing by art what must be done by accident, if the hæmorrhage cease; for I do not hesitate to lay it down as a grave and serious fact, that the walls of every artery must come into contact, and its lumen be obliterated, when a wound in an artery is successfully managed. I am fully aware, that many, and able surgeons, are of a different opinion; and that arteries are kept to show the gelatine up the tube, and consolidation up to the first branch. This gluten, no doubt, with all its appearance of commencing organization, would have been gradually carried away, had the patient lived but a few weeks. The serum and red globules, we all know, quickly run off, and after a time, as assuredly, the more tenacious or viscid gluten is dissolved. This we see when clots of blood are laid on healthy granulating wounds—when long compressed in the uterus, &c.

And the consolidation up to the first or second branch, is equally obvious. The walls are in contact; a slight action, approaching to that of inflammation, takes place, and a cord of more or less thickness, is left to deceive the surgeon.

The case, so frequently referred to, and so much confided in by surgeons, by Lambert, of

Newcastle upon Tyne, is far from deciding even the possibility of the re-union of the lips of a wounded artery, much less that it is to be generally expected.

When in a numerous series of experiments, one only is to appearance successful, we should be extremely careful how we extend our confidence. I am, in all these solitary instances of success, much inclined to doubt, and feel it my duty to guard myself from deception, by critically and severely examining into the circumstances of the case.

There are against Lambert's case many weighty objections. He tells us, and upon his words I shall take the affair, that, " a small steel pin, rather more than a quarter of an inch long, was passed through the two lips of the wound in the artery, and secured by twisting a thread round it, as in the hair lip. This was found to stop the bleeding," &c.

Whether the discharge of blood was arrested by the pin, with the thread about it, approximating the lips, and wholly closing the aperture in the artery, is to me very questionable. I rather conceive it to have been from the bulk of the pin and thread pressing, so as mechanically to close up the diameter of the vessel. And this pressure was undoubtedly increased by the binding up the arm: for in a wound so extensive as to expose the brachial artery at the fold of the

elbow, the consequent inflammation and swelling could not have been inconsiderable. The pressure must increase momentally with the inflammation and tumefaction. And, in fact, if at first the anterior wall of the artery was not in contact with the posterior, it must have been so about the fourth day, the period at which "the wound was first drest."

This, my conjecture, is strongly corroborated, by the fact of the pin coming away with the dressings on the fourteenth day. Here it is certain that the anterior part of the artery must have been affected with high inflammation, and even gangrene and sloughing, or the pin could not have come away with the dressing. It otherwise would have remained for the surgeon to draw out, having previously disengaged the thread. It is clear, and beyond a question, that the front of the vessel mortified and sloughed. Can you conceive it probable, that in a vessel so small, and pressed on as this must have been, the anterior portion of its circle could inflame, gangrene, and fall away, and yet its lumen to be preserved, and a current of blood to continue down through it? If so, your conception is more sprightly and ingenious than mine.

But the surgeon says, that "the pulse was very little altered immediately after the operation," "and so strong and equal, that he had no doubt of the blood continuing to circulate freely

through it." What! is there no way but the great trunk for the blood to get into the radial and ulner arteries? Are there no anastamosing branches which might possibly convey it down, and thus support the pulsation of the radial or ulner artery? I have seen, and any surgeon may see when he pleases, the upper portion of the temporal artery beat freely and strongly when its trunk below has been absolutely cut asunder by the shoulder of the lancet; not surely by a continuance of the circulation through the trunk, but by the return blood through the anastamosing branches.

Superadded to the chance by the collateral branches, is it not possible, and indeed highly probable, that in this solitary case of success, the great trunk may have divided, high up in the arm, and of consequence a large portion of the blood would go off, and thus support the pulsation below?

That the diameter of the artery was completely annihilated, all the circumstances of the case tend to prove. And as to the pulsation below, I can readily conceive it to be kept up by the circulation through the anatomising branches, as already remarked, or the possible lusus naturæ, the branching off, high up the arm, of the radial trunk. And had the arm upon the death of the patient been injected, we have every reason to believe the canal of the artery would have been

found obstructed. Vide Med. Observ. vol. i. p. 360.

There is another circumstance of arterial hæmorrhagy, which at present solicits my attention. I allude to the frequent recurrence of hæmorrhage after the operation, for the popliteal aneurism, when the operation is performed in the front of the thigh. There is nothing in the nature of a femoral artery aneurismal in the ham, which should make it subject to hæmorrhagy. The fault, without doubt, must be solely in the manner of securing the vessel. I need not enter minutely into detail of the various methods of *doing* this plain affair—the ligatures of reserve —the metallick plates—the modes of confining loops, &c. They all tend more or less to break up the connections of the vessel with the surrounding parts. They deprive it of nourishment, by cutting off its channels of support. The very solicitude to secure the vessel, together with its being left continuous and undivided, has been the source of all the evil.

What could be requisite to produce hæmorrhagy in an artery, more than to have it insulated —cut off from all the neighbouring parts—bedded in pus and coagulated blood? Surely nothing. How could its private œconomy be maintained, when the very instruments of support were destroyed, when the blood could no longer flow to its bare and exposed walls?

And after John Bell had writen so ably and intelligibly about a plain fact, which, to be understood, needed only to be pointed out, how could he suffer his trancendent pen to encumber itself with that mongrel, equivocal philosophy, that so unintelligibly discourses of "the artery being eroded by the foul pus?" The idea of the sides of an artery being eroded by foul pus, is so far below that clear and luminous philosophy which so vividly shines through every part of Bell's great work—the greatest of surgical works—that I can scarcely consent that it shall stand in the splendid copy I possess.

What more need he have said, than that the vessel was insulated, and without support? Consequently its walls must die, mortify, and fall away; and thus would the blood burst out. To Bell we owe much in many things, especially for what he has written on the arteries. To what he has said about the manner of managing the affair, I can add nothing.

Baltimore, July 25th, 1809.

From Dr. Davidge to Dr. Smith.

DEAR SIR,

WHEN I did myself the pleasure of addressing to you my former letter, on the pathology of wounded arteries, I had not seen the work of Dr. Jones on that subject, and all I knew of the work, I collected from the desultory conversation I had with you. Not having seen the work, I could have no specifick objects against which I might direct my observations. The principles of Jones I conceived to be erroneous, and, at the moment, offered a few reasons, on which I thought them inadmissible ; I further added to those reasons, in my letter to you, and now having, by your courtesy, the work before me, I shall present, in full, my objections to his doctrines.

The Doctor has managed his subject with much ingenuity ; his language is easy, perspicuous, and rises to elegance ; the plan and order of his experiments are natural and unexceptionable ; but I cannot grant my approbation to the manner in which he has found it convenient to represent the various results of his numerous experiments. With the results, as they stand, I am particularly gratified. Nor have I the slightest wish to deduct from their force. I ad-

mit them all their influence ; but I cannot agree to the interpretation the Doctor has put upon them. I must assume the right of inquiring into, and minutely weighing the experiments in their circumstances ; I expect to show a preponderance different from that given by the Doctor, to the publick. Nothing is more palpable to my understanding than that, in his laboured work, Jones has ascribed more importance and efficiency to some of the circumstances than can justly be attributed to them ; and that he has overlooked the more immediate relations between them and the results.

If I can, without offering violence to the general tenor of the facts, or shading any one part to the disadvantage of another, satisfactorily explain to you that the Doctor has not been happy in his deductions, I shall consider myself fortunate ; but yet I shall do no more than justice to my undertaking.

In some place of Bacon's elaborate work he has remarked that, in the explication of any phenomenon, more causes than are adequate and necessary should not be adduced. This grave apothegm is the suggestion of a cautious and circumspect philosophy ; a philosophy luminous and observant. This caution arose out of a clear conviction, furnished by years of accurate and attentive observation, and it should ever be kept in view by those who may come before the pub-

lick, engaged either in the illustration of old principles, or the establishment of new hypotheses.

There is nothing more usual in the institution of experiments, for the ascertainment of any fact, than for the experimenter to enter on the affair with his mind more or less bent up to his purpose. In his closet he anticipates what he wishes to find in his experiments, and in his experiments he is sure to discover what he has already anticipated. We first enlist our prejudices and affections in behalf of our doctrines, and then the illusions of the circumstances not unfrequently flatter us, that our experiment has afforded the clearest verification of our hypothesis.

I do not premise this, to excite any undue prepossession against the deductions drawn from the experiments of Dr. Jones; far from it: I wish these experiments to be open to a fair analysis and honourable discussion.

I would not trouble you with this criticism, had not Dr. Jones laid open anew the avenues of error, which had been so judiciously narrowed, and in a great degree closed by Pouteau and Bell. For, although to surgeons of experience and accurate observation, to say that we are to trust to spontaneous contractions of divided arteries; or to clots blocking up the canal of an artery; or finally and permanently mending an aperture in the side of a punctured vessel, would

be unproductive of serious consequences; yet to minds untrained in the school of practice, those reveries of the closet serve as most fatal lessons. Upon the first authority, I can assure you that we are not without example in this city of the application of this doctrine of spontaneous contraction and natural coagulation, to the great disappointment of the surgeon, and fatal result to the patient. Indeed, to me this return to the obsolete doctrines of Petit and Kirkland, is little better than the revival of the old affair of the wonders wrought by stypticks, &c. upon the altar of which so many thousand lives have been immolated. But that I may not appear to wish to preoccupy your mind in behalf of what I am about to say on the experiments of the Doctor, I will present them, in their prominent and most characteristick features, to you, along with my remarks.

To be particular, which I intend to be, I must unavoidably be a little prolix; too much so for a letter; but the importance of the subject must serve as my apology.

Preparatory to the discussion of the main points more immediately the subjects of consideration, it may not be amiss to premise a few observations, on the structure of arteries, and also on their general function, viz. their process in receiving and distributing the blood. On this general function, the Doctor has been rather silent.

"The substance of which arteries are composed is divided into distinct parts," says the Doctor, "which have been called tunicks or coats. Three coats, which have received various names, can be readily demonstrated, and may be simply and clearly distinguished by the terms, internal, middle, and external.

"The internal coat, although extremely thin, is very close in its texture, and gives to an artery a smooth and polished lining; it is elastick and firm, considering its delicate structure, in the longitudinal direction, but so weak in the circular as to be very easily torn by the slightest force applied in that direction. The morbid changes which have been observed in it, prove that this coat is vascular, and some experiments have been related to show the probability of its being sensible.

"The middle coat, which is thickest, is formed by numerous layers of firm, compact, fleshy fibres, of a pale red colour, passing in a circular direction, but appearing rather obliquely connected and interlain with each other, than forming complete circles. These fibres are of a peculiar nature, are well supplied with nerves, and resemble, in form and disposition, muscular fibres, but differ from them in possessing a remarkable degree of elasticity. Their elasticity keeps a dead artery open and circular; for this coat, when detached from the internal and ex-

ternal coats, still preserves the cylindrical form, whilst they, on the contrary, in a state of separation, become flaccid, and collapse. As this coat has no longitudinal fibres, the circular fibres are held together by a slender connection, which yields readily to any force applied in the circumference of the artery. The middle coat is intimately connected with the internal and external by a very short and fine cellular membrane.

" The external coat, anatomically considered, is so simple, that many authors have thought it sufficient to say, that it is formed of condensed cellular membrane, which, by becoming gradually of a looser texture, connects the artery with the surrounding parts ; but the importance which is attached, in a surgical view, to this coat, renders a more particular account of it highly necessary and interesting. Although ultimately resolvable into cellular membrane, yet it derives from the particular arrangement of its component fibres, a characteristick appearance which distinguishes it from cellular membrane and entitles it to be ranked as a proper coat of an artery. Internally, or next to the middle coat its texture is close and smooth ; externally, more open and rough, in consequence of the cellular membrane by which it is connected with an additional covering. The whole is remarkable for its whiteness, density, and great elasticity. *If an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by*

it as they can be by a knife, whilst the external coat remains entire ; a fact which will be commented upon in another part of this treatise, and shown to be connected with important circumstances. The strength, therefore, of any artery depends chiefly on its external coat, which answers in some respects, the purpose of a strong fascia."

The above quotation I consider to be necessary, not because you and I could not recollect what anatomists have advanced on the structure of arteries, or what we ourselves have demonstrated it to be, but because, being in language peculiar to, and admitted in doctrine by Dr. Jones, I can the more easily show that his physiological and pathological observations are not always on the best terms with each other.

According to its structure, as above developed, if an artery be wounded transversely, the edges of the wound recede to a certain distance from each other ; if it be wounded longitudinally, the lips of the incision in the same manner, for the fibres of the middle coat are nearly circular, retire from each other, leaving a longitudinal chasm ; and if an artery be completely divided in its transverse direction, the extremities retract, the one towards the breast, the other from it, to a considerable distance ; but the diameter continues the same, for the " elasticity of the fibres, circular in their direction,

of the middle coat," we are assured by Dr Jones, "keeps a dead artery open and circular.'

In page 114, Dr. Jones, detailing the immediate effects of wounds of arteries, as they respect the form and appearance of such wounds tells us that "the longitudinal appears to produce the slightest possible, or perhaps scarcely any separation; the oblique occasions a separation proportioned to its extent; and the transverse, however small, seems to produce a circular aperture in the parietes of the artery.' And in note C, refers us to an opinion of John Bell, expressed on a case of Deschamp, which decides a principle.

Have the circular fibres of the middle coat no power or action? When the circle is violated by the slit-like cut up and down the wall of the artery, what prevents the fibres of the middle coat from acting? Will they not retract and gather up on themselves? They have the same elasticity that the longitudinal fibres have. I am astonished at the facility with which Bell has suffered himself to be led astray. With Dr. Jones there is more ample apology; he is evidently, from his writings, not a practical surgeon. Do we not see, too frequently, in unhappy operations with the lancet in the common affair of abstracting blood from the arm, that in those slit-like wounds the lips do not remain in complete contact and apposition? In operating

on the basilick vein, if we penetrate so deep as to touch the artery, we for the most part make the incision pretty nearly parallel with the longitudinal fibres of the humeral artery, descending through the fold of the elbow, just before it divides into the ulnar and radial trunks. And yet who ever understood that such wounds would not open in obedience to the elastic power of the circular fibres of the middle coat? Is there no separation of the lips in those longitudinal cuts, when a new wave of blood is impelled along, forcing the artery into a state of diastole? Surely, my friend, hypothesis has some share in this grave doctrine.

But to put all conjecture to rest, I have seen, and demonstrated, in a living artery, a longitudinal incision to the extent at least of half an inch: and in this the edges receded very considerably from each other, and exposed, in a great degree, the opposite internal surface, although the vessel was kept in an extended state, thus mechanically favouring the approximation of the edges, and counteracting the natural retraction of the circular fibres. Six or eight medical gentlemen witnessed this.

Were I not writing to Professor Smith, I would take into account, in my behalf, the opinion of Monro the father, in his great work, where he says, that if an artery be but slightly wounded, the edges instantly retract, and have

an aperture as if an artist's punch had passed through.

But this error, as to the state of the lips of a longitudinal cut in the parietes of an artery, so directly in the teeth of the anatomy and obvious physiology of the artery, is not the only thing of this quotation I have to notice. I have also to call your attention to the effects of a ligature thrown round an artery. The Doctor says, that "if an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by it as they can be by a knife, whilst the external coat remains entire." If this position, which the Doctor has dignified with the rank of a fact, is intended to convey to the mind of the reader the idea that this division of the internal and middle coats is the usual consequence of a ligature applied in the ordinary way by a surgeon, and it can have no other surgical import, I am far from admitting it. It is, in my view, altogether gratuitous, and not deducible from any data furnished by the Doctor's experiments. But I shall postpone all further consideration of this until I come to the experiments instituted for its illustration. I now advance to the experiments of the first chapter.

CHAPTER I.—SECTION II.

Experiment 1.—"The hæmorrhage did not cease (from the carotid of a horse) before its

death. *The circular membrane surrounding the divided portions of artery was very much filled with blood;* both extremities of the artery were considerably *contracted*, particularly the one next to the head, but there was no coagulum," &c.

Experiment 2.—"The animal bled to death, &c. the truncated extremities of the artery had retracted about an inch and a quarter, and were very much *contracted*, but their mouths were open; *the arterial sheath and cellular membrane were much distended with blood*," &c.

Experiment 3.—The cut extremities of the artery were separated nearly an inch; both appeared to be *very much contracted; the surrounding cellular membrane and sheath were very much filled with blood*," &c.

It were a waste of time to recapitulate in detail, all the particulars of the nineteen experiments of this second section. It may suffice to say that in each one, the above prominent facts were fully ascertained, viz. *the contraction of the mouth of the divided extremity, and injection of the cellular membrane, surrounding the artery and sheath, with coagulated blood and lymph*, and the mutual recession of the ends of the separated vessel.

There can be no dispute about the retraction of the extremities of a divided artery, therefore I shall pass on to the other points.

By every stroke of the heart a wave of blood is urged forward into the artery, by which the artery is dilated, or is forced into what is termed its diastole ; the elasticity of the tunicks of the artery being by this wave of blood called into operation, for the coats are put into a state of distention, restores the artery from this state of dilatation to its ordinary quiescent state, or condition of systole ; and in this condition of rest or systole it would continue, were it not again altered by a succeeding wave of blood from the heart. In this play of systole and diastole consists the pulsation.

It is necessary here not to mistake this systole of the artery, this natural quiescent state, for what the Doctor calls the *contraction;* for if the artery were dead and dissected out of the body, and laid by itself on a table, it would retain this systolick condition. Elasticity is equally the property of living or dead matter. The Doctor wishes to convey a far different idea to the mind of his reader ; he is desirous to impress him with the persuasion that the artery acted on by some unexplained laws, or spontaneous causes, is, by way of provision against approaching ruin, able to contract, and really does contract the mouth of the severed extremity.

The experiments uniformly present to us two points ; the one is the reduction of the diameter of the end of the artery, or what Dr. Jones terms

the *contraction;* the other is the infarction or injection of the cellular substance surrounding the artery. Now if this infarction or injection of the circumjacent cellular substance is fully adequate to the explanation of the reduction of the diameter, or lumen of the artery. why should we be over tenacious of this *contraction?* why should we so solicitously look out for the support and maintenance of this child of the imagination?

When the arterial sheath is injected with blood, and the surrounding cellular membrane much filled with lymph, as the most pointed testimony of each experiment unequivocally establishes, either this cellular membrane surrounding the vessel must be enlarged, forcing apart and displacing all the superincumbent fat and muscle, and causing the volume of the limb, if it be in an extremity, to be increased; or the parietes of the artery must be urged into smaller compass, viz. the diameter of the artery must be lessened. The arterial tube is empty, or if filled, it can only contain coagulated blood, a material soft, and yielding, and readily giving place to a force from without.

Can rational, enlightened minds hesitate a moment about this alternative? It is not more plain that two and two amount to four in common arithmetic, than that if there be an internal force of one thousand, operating against a

counteracting force of five, the internal force still accumulating, that the counteracting force of five must give way.

From the nearest and best view I can take of the subject, I am convinced that the cellular membrane surrounding the artery is highly injected; that the end of the cut artery is reduced in its diameter; but this is done mechanically, and not by any spontaneous contraction in the vessel itself; nor do I see any insurmountable impediment to this conclusion, except it be the very respectable authority of Dr. Jones, who, with all the serious gravity imaginable, assures us, in the eighteenth experiment, that "there was a considerable layer of coagulated lymph, in which the artery was completely enclosed, but *not compressed* by it, although it was impossible to conceive a more complete engorgement of the cellular membranes surrounding it." And, to enforce with additional energy the doctrine of the text, our author adds, in a foot note, "I make this observation because the state of this artery forms an insuperable objection to Pouteau's doctrine, that the suppression of hæmorrhage depends on the engorgements of the cellular membrane." Then the Doctor does verily believe that, "although the portion of artery next to the head was so completely surrounded by coagulated lymph, that it was impossible, from an external view of it, to say exactly where it terminated," the whole force of this ac-

cumulated and accumulating lymph spent itself in displacing the superincumbent fat and muscles, and even putting the integuments themselves on the stretch, without at all compressing the parietes of the artery! Had Pouteau laboured seven years he could not have hunted up an experiment more to his purpose. And to what state of the artery Jones can possibly allude when he says it forms an insuperable objection to Pouteau's doctrine, I cannot conjecture. If the subject were not the adjustment of an important point of science, I should believe the Doctor was sporting with the easy faith of his reader.

The conical state of the extremity of the divided artery we also deduce from this coagulated blood and lymph compressing the sides. Why it is conical is very obvious; the coagulation immediately at the point is greater than it is higher, consequently the extreme end is more diminished in its volume than farther on.

In section the third of the same chapter, Dr. Jones proceeds to show that we are not only to expect, in the natural process for the suppression of arterial hæmorrhage, a contraction of the ends of the vessel; but, in addition to this spontaneous contraction, that we are uniformly to meet with an external and internal coagulum of blood, together with a coagulum of lymph interfused between those sanguineous coagula.

No surgeon of any reading or observation ca hesitate to admit the existence of an *external* co agulum of blood; it is met with on every exam ination, and in every surgical writer. We can not open a fresh wound without seeing it. Th evidences however, of the uniform occurrenc of an internal coagulum are not so great, and s generally within the reach of our senses, nor o that genuine, irrefragable sort which are to b collected from the faithful history of unadulte rated cases. The coagulated lymph, on whicl the Doctor lays so much stress, is certainly t be found, but when he undertakes to show tha it serves as a coagulum, or stopple, to close up the tube of the artery, he undoubtedly become too mechanical in his idea; but more of thi presently.

In conceding the presence of an *external* coa- gulum in the sheath, and attached to the mouth of every recently divided artery, provided the artery be not of the larger class, I must not be understood as granting also its permanent utili- ty. Its advantages are transient and illusory. By mechanically filling up the sheath left vacant by the retraction of the extremity of the vessel, and attaching itself, by means of the cohesive property of the coagulable lymph incorporated with it, to the end of the artery, it stops the hæ- morrhage, but no sooner does this coagulum be- gin to liquify, and melt down, than this whole mechanical support gives way, and a new dash

of blood disquiets the patient, and alarms the surgeon.

I have seen this coagulation about the mouth of the vessel, this suppression of hæmorrhage, and this liquifaction and falling away of the mechanical transient support, again and again occurring, and it will constantly overtake the steps of the surgeon so long as he shall pursue the deviating track which keeps in sight the permanent aid of this external coagulum.

An *internal* coagulum of blood is not always to be met with; so rarely indeed does it occur, that many of the best and most experienced surgeons have searched for it in vain, for it has been a moot point nearly an hundred years. Nature in her processes, whether acting by the cooperation of many means, or simple causes, is, and must be uniform: a thing that is contingent is not of the regular process of nature.

White, in his cases, says, "I next laid open the arteries to their extremities, and found them entirely closed (sides completely in contact and consolidated) near an inch from the end of the stump; but from that point upwards *their capacities were not at all diminished*, nor was there any *coagulum or clot of blood* in the vessels or any where near them."

Pouteau remarks, that he "very seldom found any traces of internal coagulum; that in four ex-

aminations, one of a stump of an arm eight days after amputation; two of legs, one ten days, and the other three weeks after the operation; and of a thigh four weeks after amputation, he found no coagulum."

It is very extraordinary, if this internal coagulum be a part of the process of nature, that those two able and observing surgeons could discover nothing of it in those five or six cases. But Dr. Jones, anxious to break the force of these cases bearing so immediately on this disputable point, anticipates the conclusion of the reader, by reminding him that artificial means were resorted to. It is true they were; but will Dr. Jones, or any other gentleman for him, demonstrate that this artificial aid has prevented, or can prevent such a process, if it be a work of nature? I confess that in this suggestion of the Doctor, there is, in my opinion, more of evasion and subterfuge, than argument. What can be the difference between the subsequent operations of the extremity of an artery when a ligature or compress is used, and when this extremity is compressed by a coagulation round the parietes, thereby forcing them into contact? The external coagulum as readily forms in the artificial suppression as in the natural, and the effusion of the lymph is the same; for the effusion of lymph and the external coagulation are totally independent of the means, whether natural or artificial, made

use of for the suppression of the hæmorrhage. The effusion of the lymph is produced by the excitement in the exhalents, caused by the disturbance in their economy by the cutting instrument, and the coagulation is a mere property of dead matter, for a time acting in the coagulable lymph or gelatine: and the effusion of lymph is uniform, internally as well as externally, but it is not seen after a few days, because it is partly taken up by the absorbents of the internal coat, and partly carried away by the circulating fluid. But to proceed.

"After he (a large horse, the subject of the experiment) was killed," says Gooch, "we dissected the thigh, and found that the bleeding was not suppressed by coagulated blood, but by the vessels being close contracted (their walls mechanically compressed) for near an inch or more from their extremities."

"When hæmorrhage stops of its own accord," adds John Bell, "it is neither from the retraction of an artery, nor the construction of the fibres, nor the formation of clots, (internal coagula of Petit) but by the cellular substance, which surrounds the artery, being injected with blood" Here Bell repeats to us the doctrine of Pouteau, which is firmly established by the experiments of Dr. Jones.

On this opinion however, of John Bell, Dr. Jones indulges in the following criticism: "We

have therefore only to discuss the difference between a *clot of blood* and *coagulated blood*, to discover the principal difference between this offspring of Mr. Bell, and what he has been pleased to call Petit's sickly child."

From respect to Dr. Jones I would not say that he has willingly misrepresented Bell, but most unquestionably he has egregiously misunderstood him. When Bell says, " nor the formation of clots," he, to the understanding of every man, plainly conveys the idea of the *internal* coagula of Petit, for against Petit's doctrine he is contending, and cannot be supposed to have in his view the *external* coagulation or injection of the surrounding cellular membrane, which is the true and real cause of the suppression. Dr. Jones, in his next edition will, I hope, leave both his candour and understanding unincumbered by suspicion.

That there is now and then an internal coagulum, I admit to be probable, yet I cannot grant it to be any part of the process of nature for the suppression of an arterial hæmorrhage ; when it occurs, it can only be considered as a casualty, transient in its consequences, and of no permanent duration.

On the division of an artery, there is, at first, a dash of blood, but, if the artery be small, the effusion gradually lessens until the blood poured out shall coagulate, at first at a distance, and

by degrees it approaches the mouth of the vessel, and finally closes its tube by bringing the sides into contact. The irritation occasioned by the wounding instrument, after a few hours, causes the exhalents, both without and within the canal of the artery, to throw out an increased quantity of lymph; for the artery, in fact, inflames: the sides being compressed from without, are forced into contact and coaptated like the lips of a wound, a thin stratum or almost imperceptible quantity of lymph is effused between the parietes of the vessel, through which fine arteries, with all the apparatus of organization, pass, mutually receive each other, or anastamose, and ultimately constitute a continuous organized substance. Thus do the living walls, by these intermediate vessels, become permanently united; the blood circulates from side to side, and nature's grand object is attained. But yet, like an artist who, when he has completed his building takes down his scaffolding, nature, by apparatus of her own, gradually removes this effused lymph from the inside, from between the tunicks, and from without, and the whole artery, down to the coalesced points, is restored to its healthy natural appearance and condition. The effused lymph serves merely as an appropriate bed for the tender vessels to generate in, and no sooner is the organization accomplished than this now useless matter, is carried away by the industrious absorbents. Thus stood the fact in the arteries dissected by myself, referred

to in my former letter; thus stood the fact in the cases of Pouteau; and thus stood the fact with the accurate and observant White. Nor in my opinion will the careful dissecter ever find it otherwise if he waits until the whole process of nature shall be gone through. As to internal columns of blood or lymph either becoming in and of themselves organized, sending their vessels out to meet those of the sides, and ultimately constituting with the sides an organized living part of the animal system, it is a flight of imagination against which I seriously and warmly object,

Whether the sides of an artery close by a single line, as was the case with my subject, or are united for half an inch or two inches, it matters not; in either case the union depends on the sides being more or less pressed into contact, and kept so by external agents, or the cohesive property of the lymph, until they shall firmly grow together.

So far as respects partially divided or punctured arteries, I delivered my sentiments in my former letter, and in Dr. Jones I meet with nothing that alters in the slightest degree my opinion as you then received it. I am not conversant with the pathology of wounds in the arteries of the lower animals, but according to the success of the Doctor in brutes, the anatomy between their wounds and similar ones in the human kind

must be very different; at least, if the testimonies of surgeons are to be received.

In the third chapter, Jones treats of the immediate effects of the ligature, used I suppose in the ordinary way, of surgeons. He herein endeavours to show the direct consequences of the ligature to be a total separation of the internal and middle coats of the vessel. Early in his work he announced as a fact this consequence of the ligature, and this chapter is pretty much devoted to illustrate it by apposite experiments. The whole that I can gather from the most attentive perusal of the experiments is, that, when a ligature was applied round the vessel, and in a short time thereafter removed, there was found an effusion of lymph, sometimes in the form of a transverse septum cutting the upper from the lower part.

These experiments, as they are detailed, are extremely inconclusive, and, to my mind, quite unsatisfactory. To them there are several things to object. We, in the first place, have to ascertain whether, in the application of the ligature to the artery of the brute, more force was not exerted than is ordinarily used by surgeons; in the second place there was no demonstration that these coats were divided at all, it remains a thing of inference from the effusion of lymph; and conceding, which we do not, that a separation of the fine vessels necessarily preceded the

effusion, it does not follow that the immediate effects of the ligature was the division of the internal and middle coats, for the ligature might mechanically so affect the susceptible constituent parts of these tunicks as to induce inflammation, gangrene, and sphacelus of the whole circle within the loop. Every surgeon knows that the circle within the loop uniformly dies and falls away; but this is a secondary operation, and does not take place until after the sides, immediately above the edge of the mortifying circle, have united and become firm.

But, in the third place, my chief objection is, that when a ligature is applied, even but for a short time, the interruption to the circulation in the vasa vasorum of the internal and middle tunicks, and the bruising they receive, must excite more or less of irritation, which quickly runs on to inflammation. In this state of excitement, innumerable facts assure us, that the exhalents pour out a vast quantity of coagulable lymph. Nor is it requisite that the integrity of the fine vessels should be violated, either for the production of the lymph, or absolute union of the surfaces. We could instance the copious effusion of lymph, along the internal surface of the trachea, in cases of the croup; also, in inflammations of the pleura, the numerous occurrences of a permanent union of the pleura costalis with the pleura pulmonalis; and to these may be superadded the various attachments be-

tween the several duplicatures of the peritoneum in cases of abdominal inflammation.

The Doctor certainly would not argue that, because the lymph was effused, or because internal surfaces coalesce, there necessarily must be a division of vessels or an ulceration of surfaces.

I now close my long, and, I am apprehensive, uninteresting letter. I have endeavoured to be plain and intelligible in all its parts: I have objected to no opinion that I did not think seriously objectionable; and I have in no place quoted Dr. Jones's sentiments, so as, by dissociating them from the general text, to make them appear to a disadvantage. I hate disputation, and I abhor a war of words.

Baltimore, October 27th, 1809.

A review of Mease's case of Aneurism, by J. B. Davidge, M. D.

MEDICAL cases, collected with assiduity, examined with intelligence, and promulgated with ingenuousness, are ample lessons of instruction, but when they are permitted to be vehicles of errour, or channels affording facilities to undijested, hasty speculations, they call irresistibly on our attention, and rouse our vigilance; they should be scrutinized with rigour, yet with justice, and admitted with the extremest caution; it becomes a duty to control their influence, and repress their extravagance. Were it so that they would fall into the hands of men of science and observation only, we should give ourselves but little concern about them, but as they are intended to convey, doctrines and principles, to minds incompetent to determine on their nature, it would amount to culpable dereliction of the duty we owe both the publick and our profession to let them pass unnoticed. In an especial manner are we bound to subject such cases, as are here alluded to, to strict and critical analysis, where they come forth clothed with the authority of character, and enforced by the recommendation of that celebrity which already and deservedly possesses the publick confidence.

Had the above case, to be found in the sixth number of the Philosophical Journal and Review of New-York, from the pen of Dr. Mease, been given to the world unaccompanied by the *preface* and *conclusion* by Dr. Dorsey, it would have excited little interest and awakened less curiosity. There is so much of common character about it; so much of what every professional man, at all in the habit of using the lancet, must frequently meet with, that it would have called forth few other sentiments than those of gratitude for the benevolent intentions of the respectable author, and, perhaps, surprise at the minuteness of a detail without the force of conviction or ornament of variety.

Upon reading the prelude, on which the editor of the Journal in a foot-note, makes a judicious remark, we acknowledge we anticipated something curious and valuable; such as, if it did not illustrate what the writer wished to establish, would at least have afforded entertainment to the practitioner and information to the pupil; but, in this our expectation, we were disappointed. The first point, in the detail of the case, deserving of remark, is "The blood at first appeared *very dark*, for I had been in high health."

That the blood when it first flows from a vein, recently opened, should be *very dark*, may be attributed to almost any circumstance rather

than to *high health.* We presume the author means by very dark, *unusually* dark.

The blood may be very (unusually) dark from any disease which may so affect the lungs in their function as to prevent the blood from being properly oxigenated ; or from the air being cut off from the lungs by submersion, or a cord round the neck. Contrary to what is the import and meaning of the writer's language, the blood is never very, or in any eminent degree, dark, exclusively from high health. It is true it may be very dark when the arm has been bound up any considerable time before the vein be opened, although the persons have been in high health. And we are not a little surprised that a gentleman of the professional respectability of Dr. Mease, should have permitted an idea so unphilosophick to have escaped from his pen.

"In about ten or twelve seconds it (the blood) changed to a florid complexion, and was discharged in jets."

From this the writer wishes it to be inferred that the former quantity, the very dark, was from the vein, and the latter from the artery. And to fortify this suggestion still more, he is careful to mention that it was discharged in jets.

If a ligature be passed round the arm for a short time and then the vein be opened, the blood first discharged will be dark in proportion

to the time the ligature was on previous to the opening of the vein; if it be permitted to flow for a minute or two it becomes of a colour more or less florid; and if the vein opened be the basilick, running immediately over the artery, the blood will be discharged in jets. Nor will those circumstances occur casually; they are almost always uniform.

The blood is dark because it is detained in the veins of the arm, as it were out of the general circulation; it becomes of a florid complexion because there are fresh supplies round from the arteries; and it is discharged in jets because the great artery in the fold of the arm is strongly beating under the lower wall of the basilick vein. There is nothing in all this which can *decisively* point out the artery to be wounded, or should have led a practical man to have suspected such an accident.

"I felt the pulsation of the blood, while flowing from the artery into the vein."

Of this, no doubt, the Doctor felt satisfied, but he will excuse us, we hope, when we acknowledge that we can have no satisfactory conception how, or in what way, he could determine whether the pulsation he felt were of the blood flowing from the artery into the vein, or from the blood flowing along the channel of the artery beating against the blood effused from the vein. We may repeat, that to determine

the pulsation felt to have been from the blood flowing from the artery into the vein, required more nicety and accuracy of touch, than in our estimation could have been boasted by any man, how well soever trained his sensation might have been; and we are still disposed to propound the question: Is the pulsation of the blood while flowing from the artery into the vein obviously different from the pulsation of the blood while flowing in the tube of the artery? and, If it be not indicated by some marked variety, might not the Doctor have been imposed on?

"The *pulsation* of the *blood*," is an unusual expression. We understand the *pulsation* of an *artery*. Was there also a tube, possessed of the properties of an artery, between the vein and artery? If not there could not have been a pulsation produced by the passage of the blood, upon the supposition of a communication.

"And perceived the thrilling noise from the same cause whenever I put my head under the bed-cloathes."

How strongly soever the Doctor was persuaded that he perceived a thrilling noise whenever he put his head under the bed-clothes, we must be indulged in the opinion that there was more of *imagination* than *reality* in the perception. Fancy is sometimes extremely unruly, and at no time more so, than when a man had become

persuaded that the main artery of the arm was wounded. Particularly when the subject of this supposed injury is a physician, well acquainted with the consequences of such a wound.

Why could not the thrilling noise be heard except when the Doctor was in bed? Is *darkness* favourable to sound? It is to the exercise of the imagination. Or are vibrations propagated better under the bed-clothes than in the open air of the room? It does not appear that this thrilling noise was heard by either Dr. Physic or Dr. Dorsey.

"I was careful to use my arm as little as possible, for besides the continuance there was a small swelling about the size of a small filbert in the injured part; but about five or six weeks after the accident I imprudently exercised it for half an hour, when the part swelled to the size of a common filbert, became painful and looked red. The pain and redness however soon subsided, but the swelling did not diminish for upwards of two months."

Now we have before us the plain and unadorned fact; the simple description of an ecchymosis compounded with an inflamed and thickened tendon; such as we have all met with so often that we have almost ceased to notice them.

In the genuine varicose aneurism, the blood passes freely and uninterruptedly from the arte-

ry into the vein, especially so when a thrilling noise can be heard. If then there be a liberal and ready passage of the blood from the artery into the vein, and afterwards an easy progress on through the vein to the heart, how could an increase of swelling, a redness and pain occur? This pain, this redness, this increase of swelling, argues very strongly, to our understanding, that the case was an ecchymosis combined with inflammation and thickening of the circumjacent substance. Indeed except there be a prompt and ready passage of the blood from the artery into the vein, the vein can never become varicose; and if there be this ready passage of the blood we cannot understand how, at the distance of six weeks after the injury occurred, inflammation could take place; but that it did take place is obvious from the phenomena of redness, pain, and swelling.

"But, says the Doctor, the swelling did not diminish for upwards of two months."

If the case were a genuine case of varicose aneurism, what could cause the *swelling* to diminish at all, until the aperture in the artery were closed? The vein being once varicose, would continue to be so, and, indeed, increase in volume, so long as the blood should have free access to it from the artery.

"Doctor Physick having carefully examined the arm, was perfectly convinced of the existence

of varicose aneurism, and did not entertain the faintest hope of a spontaneous cure."

The opinions of Dr Physick are respectable, and we are at all times gratified in paying regard to them; but we are desirous it should be understood that the opinions of that gentleman, like those of every other gentleman of science and observation, must be considered as opinions, *mere opinions*, and can by no means be substituted for facts. And we are the more unwilling, in the present instance, to admit the substitution of *opinions* for *facts*, as we are informed that the able Physician referred to had not the faintest hope of a spontaneous cure. That is, neither his reading or experience had ever furnished him with a spontaneous cure of a varicose aneurism, and conceiving this to be one, he had no ground on which he could bottom his hope.

If neither the fair pages of surgical history, nor the honest experience of any judicious Surgeon, can furnish a solitary case of a spontaneous cure of a varicose aneurism, or indeed any other, without the obliteration of the diameter of the artery wounded, ought there not to be some little hesitation in making up a decisive opinion in the present case? But even granting that the artery was wounded, that it was an unequivocal aneurism, what evidence has Dr. Dorsey, from which he can reasonably or justly con-

clude that "the artery ever since has been pervious."

"At this time and ever since the cure, the artery has been pervious, and pulsates like that in the other arm at the elbow and wrist."

In this concluding sentence Dr. Dorsey indulges himself in an unreserved expression of a positive opinion on a subject, respecting which he could have had no decisive or positive knowledge. We will grant however to show more plainly the inconsiderateness of the Doctor's conclusion, that the artery was wounded, and that there has been a spontaneous cure. But does it follow as a necessary consequence from a spontaneous cure that the artery must be pervious? There are numerous instances of spontaneous cures of aneurism on record, but in all there was an obliteration of the cavity of the artery. On the contrary there is no case where the wound in the wall of the artery has been closed by accident or by nature, leaving the artery *pervious*.

The lumen of an artery may be obliterated to a certain extent and yet, in consequence of the touch by the finger, being confused by the pulsation of the artery above, and below the point of union or consolidation of the sides of the artery, no satisfactory or unexceptionable deduction can be established.

With regard to the pulsation at the wrist, on which some stress is laid, we hope it is unnecessary for us to suggest, to the learned gentleman, that the perviousness of the main artery at the fold of the arm is by no means necessary. He well knows, as every surgeon must know, that an artery pulsates, when supplied by anastamosing branches, its trunk being tied or cut asunder, as regularly and nearly or quite as forcibly, as when its diameter is free and open throughout.

It appears to be forgotten that there was, and that too for a considerable time a compress on the tumour. What prevented the sides of the artery from being forced into contact by a compress continued for a series of days? And if forced into contact, what prevented a union?

But Dr. Dorsey decisively and unhesitatingly avers that the artery is pervious. And by what sensible means has he ascertained this? we know of but one mean, unequivocal and satisfactory, by which this point can be settled, that is *injection* and *dissection*.

Surely gentlemen must be very solicitous to sustain the notion that wounds in arteries can be cured spontaneously without a closing of their cavites, and that the surgeon need not trouble himself about the ligature or compress which we declare to be *necessary* in all wounded arteries.

We will conclude the present remarks, with hinting to these gentlemen, and those who may fall in with their cases. that, if they do not obliterate the diameters of wounded arteries by compress or ligature, the fate of their patients will remain as sad memorials of the imperfect condition of the surgical art; and we will subjoin that we exceedingly regret, because we highly respect Dr. Dorsey, that, without *positive* proof of any kind, or *unequivocal* evidence of any sort, or even *probability*, he has permitted himself to publish the following paragraph: he should have recollected that a sufficient number of lives have already been lost from wounded arteries left to "heal without any obliteration of their cavity."

"In compliance with my promise, I have the pleasure to subjoin the case of aneurism which I consider a *decisive* proof, that *wounded arteries may heal without an obliteration of their cavity.* I fully concur with you in the opinion that such a termination is extremely rare; but those writers who pronounce the thing impossible, are impossibly, are surely mistaken"!!

FINIS.

Deed 6/83

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