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Presented by Dr. H. A. Matthews
Oliver Lee Allen
REVIEW *Boston*

OF
"An Essay on the Bilious Epidemic Fever,

PREVAILING IN THE

STATE OF NEW-YORK:

BY

CHRISTOPHER C. YATES."

WITH

ADDITIONAL REMARKS,

BY

A PHYSICIAN.

Doctor Low



ALBANY:

PRINTED BY E. AND E. HOSFORD, FOR THE AUTHOR.

1813.

INTRODUCTION.

WHEN a writer treats of a disease which is epidemic, uncommon and alarming, the interests of community require a faithful detail of facts, and a judicious selection of remedies. Professional jealousies so common to the Faculty, should ever yield to the vital importance of the subject. A mind obscured with prejudice is extremely ill qualified to investigate truth and to detect error. The primary object is always lost in a solicitude to detract from individual reputation, and the author is consequently involved in absurdities and contradictions.

These remarks were occasioned by the perusal of a pamphlet entitled "An Essay on the Bilious Epidemic Fever, by Christopher C. Yates."

The course which he has adopted, imposes on me the humiliating necessity of descending to a refutation of the numerous errors which he has attempted to propagate.

I feel myself constrained to beg pardon of the friends of science, for the remarks which I may bestow upon this publication, and also for that infringement of decorum which the occasion imperiously demands. A course strictly decorous would render this a very inefficient and inapposite review of the Pamphlet before us.

From a peculiar aversion to this species of controversy, and a decided conviction that the erroneous

view which Dr. Yates had taken of the subject, was too obvious to escape notice or to require exposure, and also from a hope that some abler pen would have been wielded in defence of truth, I had for a long time resolved to be silent; but when I discovered the editor of the Albany Register and his *erudite* correspondents lavishing high encomiums upon this publication; when I saw the same commendations reiterated in the Utica Patriot, and ascribed to Dr. Alexander Coventry, of that village, and the effect which was thus produced in the minds of some Medical gentlemen in favour of the pamphlet, I hesitated whether to suspect a universal delusion, or to doubt the evidence of my own senses: I however, charitably adopted the opinion that the gross errors every where conspicuous, were concealed in the importance and novelty of the subject; and that a solicitude to acquire information of the Epidemic, precluded the discovery of palpable contradictions. Had these however been of minor importance and not essentially influenced the treatment of a disease, which in injudicious hands was marching with rapid strides to a fatal issue, I should have persevered in my determination.

But when I witness in this pamphlet the boasts of extraordinary success, the positive prohibition of every remedy except emetics and cathartics, the revival of a proximate cause of fever, which has been long since exploded from the annals of Medicine as absurd and ridiculous. When I hear of the uncommon mortality of the Epidemic where this treatment has been rigidly pursued, and the lingering debility

of those who finally convalesce, I am impelled by every sense of duty and humanity, no longer to withhold my disapprobation of such opinions and such practice.

Should I fail in producing full conviction in the public mind, of the truth of the allegations I have made, I trust I shall not wish to avert those just denunciations of their displeasure which I may thence incur.

I extremely regret the necessity of exposing the errors of the author at the expence of his veracity, but the public good requires that such an exposure should be made, however it may affect the moral or medical reputation of any individual, and if, in his zeal to acquire celebrity, he has been regardless of the effects which these may produce upon community, *the rod of correction cannot be too freely applied.*

I cannot omit this opportunity of expressing my decided disapprobation of those Medical Journalists, who review publications of this nature without exposing their prominent defects. This unmerited indulgence gives currency to error, and stamps imperfection with a degree of importance to which it never can be entitled.

While the opinions of reviewers are considered decisive of the merits or demerits of a work, they certainly should be expressed with critical accuracy, and the most rigid impartiality; by pursuing this course, the review will obtain a reputation which it never can acquire from an indiscriminate profusion of applause.

In reviewing Doct. Yates' pamphlet I shall adhere to the order which he has pursued and select a few prominent passages for critical remarks. Grammatical errors are of minor importance and too numerous to be noticed. They must occur to the observation of every correct reader in almost every page of his publication. As I am collecting materials, from authentic sources, not founded on "vague rumour," for a correct history and treatment of the Epidemic, I shall therefore render this review as brief as possible. It is merely a temporary *caveat* to the public to prevent the general diffusion of error. Enough is said to produce conviction of its inaccuracy, while much is reserved for the reader to examine. When the subject is resumed, the base on which Doctor C. C. Yates has erected his *immense* fabric will be so perfectly demolished by authentic documents, that "not one stone shall be left upon another."

REVIEW, &c.

DR. YATES says, "In the summer and fall of 1812, a considerable number of deaths occurred in the encampment at Greenbush, of this prevailing Epidemic." That about the first of May, 1813, the disorder continued to prevail in Albany with undiminished malignity, and that in other counties and states where it had not before appeared, it commenced with the opening of spring.

It may be remarked that the Dr. has advanced an opinion that this Epidemic is not a winter disease, but that it is equally prevalent at all seasons of the year. I will not positively assert that he designedly mistates facts for the purpose of supporting this opinion, but I can freely declare that I have never heard of the Epidemic prevailing in the vicinity of Albany during any of the summer months. I have heard a very respectable and intelligent surgeon of the army say, that the disease which first appeared among the soldiers in the fall of 1812, was highly inflammatory, resembling a *peripneumonia notha*, that the blood when drawn was always indicative of that inflammation, and that no permanent relief could be obtained without recourse to this remedy—but that after the commencement of the extreme cold of winter, the type and symptoms were totally changed. Solici-

tious to establish the position he has taken, the Dr. might easily mistake this for his favourite Epidemic. Such errors are unpardonable in an author who attempts to found thereon important principles in medicine.

The only apology that I can offer for this precipitate declaration is, that his mind is more prone to adopt contradictory opinions upon any evidence, than to investigate truth upon its true basis. Had he affirmed this fact by the testimony of ocular demonstration or the citation of competent authority, it would have been entitled to credence; but when it rests upon no better ground than vague rumour, the Dr. will excuse my incredulity. With respect to his assertion that the Epidemic continued to prevail in Albany on the first of May, 1813, with undiminished malignity, I have no hesitation to deny the position, and shall even presume to adduce the Dr. himself in evidence. I do confidently affirm that at this time but few cases remained, and that by the middle of June not one well marked new case was to be found, unless the Doct. pleases to denominate every "ache and indisposition a case of the Epidemic." That he perfectly coincides with me in this opinion, may be fairly inferred from his taking advantage of this healthy season to make a pleasurable excursion to New-York—would the Dr. be so inhuman as to disregard the "agonizing cries of his patients' for relief," and abandon his duty at this critical juncture for a pleasant sail in the steam-boat? The Doctor's heart has certainly been insensible to the calls of humanity, or he has been guilty of a gross mistatement.

of facts. This inference acquires additional strength from the consideration that two other physicians of extensive practice were absent from Albany on a visit to New-York at the very same time, when Dr. Yates says the Epidemic prevailed with undiminished malignity. Not so in the winter. Not a physician dared then to desert his post; in confirmation of this I will state one fact. A Mr. Nehemiah Pratt on the western turnpike, three miles from Albany, was seized with the Epidemic in January; with much intreaty he was able to procure but three or four visits from different Physicians in Albany; his messengers repeatedly afterwards requested and urged every practitioner they could find to visit Mr. Pratt, but in vain. The sick man was suffered to languish four days without any medical aid, when he died. No party of pleasure, no calls of humanity could then entice any practitioner from Albany; not even Dr. Yates himself. At any other time twenty Physicians would have vied with each other to obtain this patient. But no sooner does warm weather commence and with it "*an increased malignity of the disease,*" than the Physicians leave the city almost destitute of medical aid.

The correctness of these deductions I trust the Dr. himself will not controvert.

It is a remarkable fact which clearly designates the character of this fever, that it appeared last winter, earliest in those climates which are distinguished for the greatest severity of cold.—Hence in Vermont, and in the northern parts of this state, it occurred at the commencement of

winter. In Albany about the first of January; in the southern parts of this state and Connecticut, the last of February and first of March, and in Philadelphia not till the first of April; south of this I have not received any correct information. This fact unequivocally proves that whatever the cause may be, it requires a certain degree of cold to enable it to produce its deleterious effects upon the system; and that where the cold is severe, activity is immediately communicated to the agent, or the body is rendered more susceptible of its operation. But where the cold is less severe a longer time is requisite for the same cause to produce its effects.

But we will proceed, in the confident hope that other parts of this pamphlet will be better supported.

“The Physicians of the army treated it as a highly inflammatory complaint, while those from the city who were occasionally called in to assist, treated it as a disease putting on the *character of typhus fever*; by the first the lancet was used with a liberal hand; by the *latter*, brandy and laudanum were exhibited from the first attack with freedom and without measure!”

The Dr. here designates a complete discriminating line between the Physicians of the city and army, and arranges the former *nolens, volens*, in battle array against the latter. To say the least of this it is extremely uncharitable and reflects no great honour upon the Doctor's discernment. It is an evidence of his disposition to adopt general sweeping clauses without embarrassing himself with individual opinion.

How extensively a difference in opinion prevailed

among the surgeons of the army, I am not prepared to decide; but this I can with confidence assert, I have conferred with a very respectable Physician of the army, and we perfectly concurred in the symptoms and cure of this disease, and according to the Doctor's own account, his opinion at this period was still fluctuating between bleeding and stimulants.

I now beg leave to assure the Dr. that ever since the appearance of the Epidemic in our city, I have never entertained but one opinion upon this subject. My mind has never been subjected to that variety of changes which he ascribes to his own, and which, I think, from its present tone, will have to experience greater vicissitudes, before it arrives at the stationary point of truth.

Whenever I have been called to consult with those Physicians who were the advocates of the stimulating treatment, I have uniformly opposed their liberal use of stimulants; and certain I am that other practitioners in the city concurred with me in this opinion. I have therefore the strongest assurance to believe that those physicians who used exclusively the stimulating treatment, were very limited in number, though "extensive in practice." The confidence with which they enjoined the use of brandy had an imposing influence upon popular opinion; and the well known case which appeared in the fall, in a respectable family in Lion-street, and succeeded under the operation of the most powerful stimulants, conduced much to sanction this practice.

Dr. Yates informs us that one of the physicians,

who attended this case, afterwards abandoned his stimulating practice, and added "if he had not done so he would have lost his patients." He further informs that this physician had as yet lost no patients. It may be gratifying to know how near these patients approached death before the Dr. discovered his error, and how many recovered under the use of stimulants, and how our author will reconcile the success of that practice with his theory. These Gordian knots he will solve in the same manner and with the same facility as did Alexander the great.

But his remark relative to typhus fever is equally exceptionable and an additional evidence of his aversion to particulars. Does the Dr. administer brandy and laudanum with freedom and without measure whenever a disease "puts on the character of typhus fever?" Does he pay no regard to the diversity of symptoms which it assumes? Or is his treatment of the typhus equally simplified with his treatment of the epidemic? If so, brandy will probably constitute that "unity of remedy" which required the boldest effort of his genius to discover, and which will perpetuate his memory to everlasting fame.

"They felt as if they had to contend with a new and unseasonable enemy."

Is the bilious fever "a new enemy"? The Dr. has for a moment forgotten the levity with which he treats the symptoms "of this misnamed terrible fever," in other places.

To prevent the epidemic, "brandy was *cried up* as a sovereign remedy."

The Dr. will please to explain how he will apply a "remedy" before the existence of the disease it is intended to cure.

"In this state of things I found much to lament and much to rejoice at. While some physicians were so unfortunate as to lose many with the fever, I felt grateful that I had as yet lost none; I felt happy that I stood not alone in this *situation*; two other physicians of extensive practice had been equally fortunate, their mode of treating the disease was generally the same with the one which I had adopted, and until the 26th of January had lost but one patient each."

This reminds me of a pamphlet which Dr. Moses Willard wrote a few years since, to prove the success of his practice in a fever which then prevailed in Albany. "Of seventy-six patients which he attended," the Dr. states, "that he lost none, while other physicians were so unfortunate as to lose many."

Dr. Willard will pardon the comparison.

But I trust our author will give as much credit to the success of other practitioners in Albany as he does to those whose names he has cited. I will mention only two, Dr. Montgomery and Dr. Sherman, both of whom have declared in the public papers, that they have not lost a single patient during the whole time the epidemic prevailed in Albany. From the public manner in which this declaration was made, our author must certainly have known of these two instances of extraordinary success, and as the evidence on which they rely is precisely the same which Dr. Yates adduces in his own support, he cannot therefore refuse to them his most implicit confi-

dence. Notwithstanding all this, the Doctor's partiality has totally excluded them from a participation of that honor which he so lavishly accumulates upon his two friends.

A treatment which insured a success even superior to the Doctor's himself, certainly merits a distinguished place in the third edition of his *Book*, and will unquestionably afford a valuable addition to his repository of facts.

At a time when the stimulating treatment was in its highest repute, I have heard one of its most zealous advocates affirm in vindication of that practice, that without the liberal use of stimulants no patient ever could recover, that he had invariably pursued that practice and "had not then lost a single patient." Dr. Yates, therefore, has more company in the successful treatment of the epidemic, than he is willing to admit. But none, it appears, can ever expect to participate in his honors who do not implicitly adopt his practice and his opinions.

During the prevalence of the epidemic it had become so common for practitioners to boast of their success, that I have frequently heard it reported of some who were notoriously the most unfortunate, "that they had not lost a single patient."

Disgusted with the daily repetition of such unfounded rumours, I adopted an opinion which I found to be generally correct that those who were most vociferous of their own success, were such as had lost the most patients, or had none to lose.

The report of those physicians who were examined before a committee of the legislature, relative to the state of the epidemic, is a curious evidence of

this fact. By this report the proportion of deaths to the number of cases was one to forty ; but no sooner did the Board of Health require the name of each individual under their care, than this proportion was astonishingly changed. I heard one physician declare, that at this period his number of new cases had very suddenly declined.

“ *When I sent him on the 26th of January the following communication.*”

The Doctor’s knowledge of philology will enable him to explain the meaning of this short sentence— I am unable to decide when his communication was sent.

“ His eyes appeared to indicate an increased and inflammatory action in the system, which was contradicted by the pulse; this inflammatory appearance induced me to bleed him.”

It is astonishing that the Doctor should have had recourse to such a powerful remedy as bleeding, upon an indication so delusive as an inflammatory eye, especially when that indication was contradicted by that true unerring test of all inflammation, the pulse. Has not this experience exhibited to his view a thousand instances of such deceptive symptoms in the last stages of a typhus gravior? Does he not know that this turgid appearance of the blood vessels of the eyes does not originate from increased excitement, but merely from debility, and that it is a common indication of approaching death? If he does not he has another “ important lesson to learn in medicine.”

The following is the Doctor’s history of the treatment of this patient.—“ I took eight or ten ounces of blood, gave him jalap and calomel, applied a

blister plaister, prescribed nitre, calomel and tartrite of antimony, gave him sweating draughts, wine, camphor, laudanum and bark, under this treatment the frequency of his pulse increased, he grew delirious and died on the seventh day."

And is it possible Dr. that "under this treatment, your patient *grew* delirious and died?" Because bleeding and stimulants were unsuccessful in this case, he very deliberately condemns both.

"My patients told me the chills were different from any they ever felt before.—The pulse was peculiar and new to me."

Notwithstanding all this he contends, it is nothing but a common "bilious fever."

"The pulse exhibited every mark that would deter a prudent physician from bleeding." Yet the Doctor bled.

"Full inspirations were not prevented by acute pain but a deadly suffocating pressure on the air vessels."

I know not from what sources the Dr. derived his information, but certain I am that I have never known more severe, acute pains about the thorax, in any disease, not even in pneumonia, than I have witnessed in some cases of the epidemic.

"Every case of this *misnamed terrible fever*, which has come under my observation has yielded to this treatment." When the Dr. recollects the unprecedented mortality that occurred in a certain house in state-street, he may be induced to expunge the whole of this sentence from his *immortal* work.

"All we know is that the morbid matter is creating from time to time."

The Dr. will please to inform us what "the mor-

bid matter is creating." In his flights of imagination he often leads us into his "wide field of conjecture," where we are left to roam without a guide.

The Doctor's theory of the "deleterious particles entering the blood-vessels and destroying the vital principle, thence producing the dark, purple colour of the face, depression of spirits, prostration of strength," &c. &c. &c. is as ridiculous as it is erroneous. If he had been more of a philosopher, he would not have fabricated a theory founded wholly on hypothesis. Facts alone should constitute the foundation of every important theory in medicine. But he assumes the position that the vital principle is destroyed by deleterious particles, without ever evincing in any manner whatever that such particles do actually exist in the blood, or how they produce this effect. Is the vital principle decomposed by any chemical affinity, or do these particles by any power of locomotion, insinuate themselves between the blood and the oxygene in the lungs and thus prevent the absorption of that principle? Does he not know that all these symptoms may occur from an incapacity in the lungs to make full inspirations, without any reference to a deleterious principle? If he does not, let him apply a ligature to the trachea, and he will soon be convinced that his laboured theory is the result of a "plodding" imagination.

"I now had to receive my most important lesson in this complaint, though at the expense of my patients' lives."

Comment on this paragraph is totally unnecessary: Our author's mind which has rung to all the changes in the treatment and theory of the epidemic,

is now destined to experience another very important revolution. A recapitulation and condensed view of all these vicissitudes may not be unimportant.

1st. He commences with bleeding, sweating, evacuations and stimulants.

2d. He omits bleeding and uses evacuants and stimulants.

3d. He abandons stimulants and adopts emetics and cathartics alone, and reprobates the use of stimulants and tonics even in the convalescent stage. In the last opinion his mind appears to continue stationary to the end of his first edition; but no sooner does his second edition appear, than his pregnant mind is delivered of a totally novel and wonderful discovery. It is no less than a "unity of remedy" for all diseases. Astonishing fœcundity of imagination! *fortunati nos!* we have lived to witness that happy era, when the "jargon of the schools" is abolished—when we shall be no longer perplexed with a four years study to qualify us for the practice of medicine; we have only to learn Doctor Yates' "unity of remedy," and we shall be perfectly competent to protract life to mature old age—to meet disease in its mildest or most malignant form.

But as if to tantalize the medical world with disappointed hopes, or perhaps aware that it could not sustain the shock of such a sudden and total revolution in science, the Dr. has neglected to disclose this omnipotent remedy. As the whole of his materia medica is now reduced to emetics and cathartics, we may reasonably infer that one of these will ultimately be designated as his catholicon. The good women

will again be restored to their former station in science, and grateful for his exertions in their behalf, they will bestow on the Doctor the highest tribute of applause.

But as we proceed in this wonderful pamphlet we find even in his proximate cause something to excite our curiosity.

“The proximate cause appears to be a secretion of morbid acrid matter, or vitiated bile.”

To ascertain the verity of this hypothesis, we must have recourse to dissection; the following one selected from the New-England Journal, and performed in Boston, will afford satisfactory evidence that bile is not the cause of the disease. It was a well marked case of the epidemic:

“The body was examined the day after death—the brain was very firm—its superficial veins were shrunk, but exhibited satisfactory marks of having been very much distended. The Tunica Arachnoidea had many spots of coagulated lymph, by which the hemispheres were more closely connected than common—some water was found in the sentricles—*ventricles* the heart was in a sound state—the left lung much discoloured, and partially hardened—the right lung was discovered lying in the upper part of its cavity covered by yellow coagulated lymph, its substance remaining entire. The lower part of the same cavity was occupied by a kind of cyst formed by a recent concretion of lymph, about a quarter of an inch thick, on all sides, soft and readily torn and enclosing in its cavity, more than three quarts of thick and discoloured serum. The anterior surface

of the abdominal organs, the liver, spleen, stomach, and intestines were covered with a most extraordinarily thick coat of very yellow lymph, which cemented all these parts into one mass, and united them to the anterior parietes of the abdomen. This lymph was in many places an inch thick, and might be separated into large masses. The whole peritoneal surface of the organs, and of the parietes of the abdomen was in a greater or less degree covered with lymph. By the union of the different organs, the appearance of these parts was rendered as confused as is possible to conceive. In various places the adhering parts formed cysts of different sizes, which were usually filled with serum, containing a portion of semi-purulent lymph. The peritoneum and pleura were generally very tender, and in some parts had the aspect of approaching gangrene."

Had the Doctor's proximate cause the least foundation in truth, some of this vast quantity of bile, would certainly have been detected in a case so carefully and judiciously dissected. But instead of this we find a very thick coat of yellow lymph or slime covering every viscus and membrane of the abdominal and thoracick cavities,—which the Doctor denies ever to have discovered in a single case.

However he may have availed himself of extraneous aid to complete his compilations, I had no expectation that he would have gone back more than three hundred years and borrowed from the ancients their proximate cause of intermittents, and transferred it to the epidemic of this season. Being more familiar with ancient than modern opinions, perhaps he is still

ignorant that his proximate cause has been repeatedly disproved by subsequent improvements in medical science, and consequently long since exploded from the annals of medicine. He will therefore permit me to refer him to Wilson on Febrile Diseases, 91st page, where he will find the following quotation.

“It is one of the many opinions which have generally prevailed respecting the proximate cause of intermittents, that they arise from an *increased secretion or vitiated state of the bile*. The particular state and frequent redundancy of the bile in intermittents, and still more in the remittents of warm climates, gave rise to this hypothesis.”

The Doctor's reading has perhaps never extended to the objections which subsequent writers adduced against this theory, or he never would have adopted that for his proximate cause which is evidently nothing but an effect of the disease. In proof of this opinion I will cite a few of the many conclusive authorities.

Sir John Pringle says, “But after all the bile seems to be more the effect than the cause of intermittent fever. For whenever these fevers come to fair intermissions, they give way to the bark, a medicine which as far as we know has no direct influence upon this humour. All therefore that can be said in favour of the ancient doctrine is that although the bile is not the first cause, yet from its *redundance and depravation*, owing perhaps to the fever, it frequently becomes a secondary cause of irritation.”

Seneca observes, "If the bile be the cause of intermittents it must have acquired some particular properties, for it is often discharged both upwards and downwards without inducing any fever of this kind: Nor are those who labour under jaundice more subject to agues than others, and in these circumstances if ever, the bile should induce fever since the whole fluids of the body are mixed with it."

Wilson adds, "It would be superfluous to attempt any addition to what these and others have said respecting this hypothesis, which *unfounded* as it is, is very *generally blended with the writings of medical authors.*"

But the reasoning and experiments of Dr. Saunders are perfectly conclusive upon this point. After all his critical analysis of the nature and qualities of the bile he very decisively concludes that the *increased secretion* or *vitiating quality* of bile, is productive of no disease but what may be easily remedied. The following are his own words. "I do not however mean to deny that many and great inconveniences are found to arise from the prevalence of bile in the primæ viæ, but I am firmly persuaded that a diminution of its natural quantity would produce diseases of a more permanent and alarming nature. It is more difficult to supply the *defect* in the quantity of this fluid than to carry off its *excess*. It is even more easy to diminish its *acrimony* than to increase its power."

If then the bile in its most vitiated state, cannot produce a single paroxysm of an intermittent, sure-

ly none but our *erudite* author could ever metamorphose it into the proximate cause of a continued malignant fever. Doctor Christopher C. Yates is here completely at issue with Dr. Saunders, and he has no alternative left to support his reputation, but to controvert this authority; a task, which with all his borrowed aid and *profound erudition*, he is totally incompetent to perform. He may have recourse to his ancient authorities for opinions which have long since been exploded. By these authorities he may prove the existence of morbid matter, the whole system of the humoral pathology, and even disprove the circulation of the blood. But the Dr. must excuse me if I do not concur with him in the verity of these opinions. I am one of those who believe that the moderns have essentially improved Medical Science, and that the work of Saunders upon the Liver, is infinitely superior to all the visionary obsolete theories of the ancients upon this subject.

But however the Dr. may be enveloped among the musty records of antiquity in search of authorities to prove his theory, I defy him to corroborate it by any modern work of greater celebrity than his own pamphlet.

I have hitherto admitted a principle which he primarily assumed without the least color of evidence, that the bile is in a *vitiated state*, and have proved conclusively from the most established authorities, that bile even in this state never can produce an intermittent or continued fever.

It then necessarily follows that if bile in its vitiated state, never can be the cause of fever, in its mild

and natural state, it is perfectly innocuous. That this is the state of the bile till after the actual inception of the Epidemic, I do positively assert; and I defy Doct. Christopher C. Yates with all his ability to distort facts, to adduce a particle of proof to the contrary.

How ridiculous then does the Dr. appear with his proximate cause, on which his whole superstructure and hopes of future fame were substantially founded.

But ludicrous as it is, the opinion advanced in the public papers, by Doct. Alexander Coventry, of Utica, to relieve Dr. Yates from the embarrassment to which his proximate cause was subjected, has left the latter far in the back-ground.

Dr. Coventry was aware that the increased secretion of vitiated bile, is peculiar to warm climates, and the consequence of long continued heat upon the hepatic system. He therefore reminds our author of this circumstance, and of some modern experiments and discoveries of which he considers him as totally ignorant, and advises him to go back to the summer or autumnal months of last season, for the origin of his proximate cause, the generation of bile and its accumulation in the primæ viæ, where it has remained dormant and perfectly innocuous till it suddenly explodes in the production of the epidemic. Elated with this visionary suggestion, our author abandons his former *remote cause*, which he had placed in the atmosphere, and embraces this in his second edition with extreme avidity. Having adopted principles without being duly informed of their

accuracy, his mind is continually experiencing the greatest vicissitudes; he precipitately adopts and as hastily rejects his theories.

But the Doctor has probably seen cases of the epidemic during the spring, in which the patients enjoyed perfect health during the fall and winter preceding, till they were suddenly attacked in the spring. Will he be kind enough to reconcile this vast accumulation of bile for the long period of nine months, with the fruition of uninterrupted health? surely this humour cannot be so noxious as he would wish us to believe. But it is incumbent on him previously to prove the actual existence of this vast quantity of bile.

This subject is too absurd and ridiculous to require any further refutation: I shall therefore submit this difficulty to the solution of those adjunct theorists. Their mutual aid will, perhaps, relieve their mutual embarrassments, and procure a mutual *dedication* to their respective pamphlets.

However these gentlemen may eulogise the splendid talents and productions of each other in gratitude for an *epistle dedicatory*, I never shall expect any logical reasoning from either. Buffoonery is their fort, to which they may never expect men of science to descend. If Dr. Yates had placed his proximate cause in the liver or in the glands in general, he might have rendered his theory more consistent: But when he places it exclusively in the bile, he has evidently committed a gross error, and the whole system of cure which he has founded upon this hypothesis, is consequently erroneous and ab-

surd. What if the Dr. perseveres in emetics and cathartics till every particle of his proximate cause is evacuated, will the disorder then cease? No, the morbid irritation of the liver still continues and the bile is secreted with more rapidity than before. Upon the principle which he adopts, the disorder never can terminate, for his evacuants become continued exciting causes of the secretion of bile, and with the accumulation of this his proximate cause, the disorder must in the same ratio acquire strength.

Without reflecting upon this effect of emetics on the hepatic system, he has founded his whole system of cure upon the discovery of bile thrown out by evacuants.

To prove that this bile was not in the stomach before, but that it was emulged from the biliary ducts by the remedies he employs, and that his hypothesis of the whole system of cure is consequently erroneous, I shall cite a few competent authorities.

Dr. Cullen says, "frequent vomiting emulges the biliary ducts and throws out much bile."

Dr. Saunders—"The secretion of bile is frequently increased and hurried by causes acting on the stomach, such as sea-sickness and *emetics*, the discharge of bile by vomiting is therefore no proof of its having existed in the stomach before the exhibition of the vomit, or of its having been the primary cause of nausea and indigestion, it is *only the effect of direct action on that organ*. In the bilious fever of the West Indies, the nausea and vomiting which arise from some slight degree of inflammation near the pylorus and upper surface of the duodenum, in-

vite bile into the stomach which has no tendency to produce the fever: it is only the *effect* and *not the cause of the disease.*”

Doct. Jackson is an authority which Dr. Yates quotes to prove his theory, he therefore will not deny the following *correct* quotation from that author.

“It is well known,” says Dr. Jackson, “that a continuance of nausea, or that a repetition of the action of vomiting, increases the determination not only to the stomach, but likewise to the parts which are near it. Hence the secretion of bile is preternaturally increased by the ordinary effect of vomiting.” “But besides this,” bilious fevers “often originate from our own treatment, viz: from the repeated use of emetics or cathartics which are violent in their operation.” “The accidental appearance of bilious vomitings in the fevers of hot climates, furnished medical authors, with a pretence of forming a new theory and of directing the mode of practice to a particular view. Influenced by this appearance they assume it as a fact, that a *vitiating quality* or *redundant quantity* of bile constitutes the essential cause of the disease, and on this foundation adopt the plan of repeatedly evacuating both upwards and downwards; a practice which evidently *increases* the secretion of bile. Hence a disease or the symptoms of a disease arises wholly from this mode of treatment.”

Dr. Jackson has so minutely described the theory and practice of Dr. Yates, that we are induced to suspect that the latter has derived much of his infor-

mation from this source, without being in the least benefitted by the strictures of the former.

A few remarks resulting from my own experience will evince the application of these authorities to the Epidemic. For the truth of these remarks I appeal to every medical gentleman who has been much conversant with the disease. Dr. Yates will please to consider himself excepted from this appeal, as his observations have hitherto been extremely partial and limited.

If an emetic is exhibited immediately after a sudden attack of the disease, very little if any bile is evacuated; neither does the skin at this time exhibit any indication of the existence of bile in the blood, as it ought to do if the Doctor's theory be correct. It is only till after a considerable duration of the disease, or the exhibition of a second emetic, that much bile is thrown from the stomach, or the skin exhibits a yellow aspect. The Dr. readily admits the last fact, probably without discovering the fatality which it threatens to his theory.

From these facts it satisfactorily appears that the disease has actually commenced its ravages upon the system, before the secretion of bile has increased, and consequently before the Doctor's proximate cause begins to exist.

It is extremely easy for innovators to find symptoms perfectly subservient to their theory. Hence the Dr. has invariably found "a pain more or less severe in the right side in the region of the gall-bladder passing up towards the neck and settling between the shoulder blades." It seems he is determined to

advance opinions so diametrically opposed to my observation of the symptoms, that a coincidence can never be expected.

I have been so unfortunate as not to have witnessed the pain "invariably" in that region. It has appeared as often in the left side as in the right, and in the breast as often as in either side: Finally, no part of the thorax and in some cases no part of the body, has been exempt from the pains. I presume every patient who has had a severe attack of the disease, will bear testimony to the truth of this assertion.

With his proximate cause always in view, the Doctor says, "in *all cases* he first prescribes an emetic." We are here deprived of all discretion—we are not even permitted to inquire into the contra-indications of this remedy; but must in *all cases* prescribe an emetic.—It may not be improper however to inquire whether the Doctor's practice perfectly corresponds with the mode of treatment which he recommends.

In one of his notes he says, "In all the cases which I have had, with the exception of less than ten, I have given emetics."

Why the Doctor has not complied with his prescription in these "less than ten cases" he does not condescend to inform us. That he should establish an unexceptionable rule for the government of others and then deviate from that rule himself in practice, is an inconsistency that he is required to explain. Had he admonished us of any symptoms that would render the exhibition of emetics improper, his prac-

tice would have been more consistent, but when with a peculiar emphasis he says they must be given “*in all cases* ;” although symptoms should occur that would render their operation extremely hazardous, he certainly merits the severest censure.

But his reasons are obvious—he was aware that if the disease should be cured without any evacuation, his favourite theory would be prostrated. In this he has evinced more consistency than usual.

But the Doctor’s astonishment will no doubt be highly excited when he is informed, that many eminent practitioners whose success has been at least equal to his own, do not depend on evacuations as a *sine qua non*. They use them occasionally as in other diseases, to prevent a constipation of the bowels; while their chief dependence is placed upon calomel, opium, sudorifics, &c.

A respectable practitioner in Troy depends much upon the operation of calomel upon the salivary glands, another in Saratoga succeeds in almost every case by the use of opium, another in Canaan cures his patients exclusively with sudorifics—and some yet continue the use of stimulants; while Doctor Yates decidedly condemns all these modes of treatment, and prescribes nothing but emetics and cathartics. This is only an evidence of the difference in sentiments which prevails among practitioners, and of the success which they experience under their respective modes of cure.

But how will the Doctor explain the cures produ-

ced by these different means without any copious evacuation. His proximate cause in these cases must be completely confined in the *primæ viæ*, and in the utmost hazard of being totally suffocated. Can that vitiated bile which is capable of producing such sudden and powerful effects upon the system, as to endanger life in a few hours, be so perfectly changed in its quality as to become entirely harmless? Will it in one hour prostrate the strength and energy of the body, and in the next conduce to a restoration of that energy?

Dreadful alternative for the Doctor's proximate cause. I have a presentiment that between Scilla and Charybdis, it will ultimately sink into the whirlpool of oblivion.

To extricate himself from this wonderful dilemma he will undoubtedly consult his co-adjutors in science, and their combined decision will result in a total denial of the facts. Should he have recourse to this his usual expedient of refuting unanswerable arguments, his reputation will never sustain the shock of that irresistible torrent of authorities, which rush in from every quarter.

The more clearly to elucidate the doctor's character as a consistent writer, it may not be improper to designate a few passages for the reader to compare. These few I trust will stimulate him to a more diligent enquiry, and I beg leave to assure him that if he examines the whole publication, with critical accuracy, he will scarcely find a page exempt from the most ridiculous inconsistencies.

The doctor says in the 23d page of his 1st edition,

“The discharge from the stomach is mixed with or followed by a slimy, whitish, compact substance,” and in the 46th page, that “in not one solitary instance have I observed *gelatenous slime or any thing that resembled it.*” If he intends any difference between “the slimy compact substance” and “gelatinous slime,” it must be that the former is, as its name imports, a perfect slimy solid, and as I have never seen any solid substances ejected from the stomach, I must differ with him in this particular. But I have more charity for the doctor than to believe he intended to say, that his emetics ejected any *perfect solid* substances. I have, on the contrary, the strongest assurance to believe, that, in his description of symptoms he candidly admitted the existence of a “slimy substance,” because he had frequently seen it discharged from the stomach, and that he would not have denied this “gelatenous slime” in the other place, had it not been enumerated by another practitioner, in his description of symptoms, whom he wished to contradict.

His intemperate zeal to be in the opposition, has therefore in this instance induced him to contradict himself.

“The morbid bilious secretions continue *pouring* into the gall bladder, giving rise by distending that viscous to the pain in the right side.”

If the distention of the gall bladder is the sole cause of the pain in the right side, I wish to know what produces it in the left side and in other parts of the thorax. Will the Doctor give his gall blad-

der ubiquity, or does he believe there are as many gall bladders as pains.

“ I hardly recollect a case in which a cathartic did not operate on its first exhibition. I have in no disease found the bowels more susceptible of being moved by a single dose.”

I believe the Doctor stands entirely alone in this opinion. Every practitioner with whom I have conferred upon this subject, uniformly concur with me, that in no disease have they found such an obstinate constipation of the bowels. In some cases it was even impossible to procure evacuations, and I have heard a very respectable and venerable physician of this city declare, that he had been in consultation with Dr. Yates in one case where the Doctor had administered two drachms of tartrate of antimony without producing the least operation. Still the Doctor persists “ that he has never found the bowels more susceptible of being moved by a single dose.” I trust it is unnecessary to add more on a subject so perfectly clear and well known to every practitioner, to every patient, and almost to every person who has witnessed the Epidemic. That he should hazard such an assertion against such a mass of testimony, is therefore truly mysterious, and totally inexplicable. I however, would by no means impeach the Doctor’s motives. They were no doubt perfectly *honest* and *conscientious*. His zeal to become *useful* to his fellow citizens has been prematurely disclosed

in the adoption of errors, which reflection and his well known *candour* will prompt him to retract. I cannot, however, sufficiently express my admiration of that elevation of sentiment which the Doctor evinces in his fortunate selection of tropes and figures. When he compares the Peripneumonia Typhodes to "a *White Black Sheep*" he exhibits an unerring test of a mind dignified with *exalted* ideas, and which soars at least as high as this elevated *Figure*.

In the 49th page, in a note, the Doctor says, "he has condemned both stimulants and bleeding," and then inquires ironically, "what is to be done when the prominent symptoms are so nearly balanced that prudence would dictate the use of neither of these classes of remedies? The patient is here left suspended between pneumonia and typhus, without any advice for his relief, his situation is truly singular."

Although the Doctor intended this note as one of his severest sarcasms for another practitioner, his want of penetration has prevented his discovering the applicability of it to himself. It will be perceived that in the first of the note he condemns both bleeding and stimulants, by his own confession then the residue applies to himself with peculiar force and propriety.

This note is not dissimilar to Hudibrass' gun,

"Which recoils and kicks the owner over."

It seems the Doctor is doomed to embarrassments of this nature.

“This invariable ejection of viscous slime resembling in tenacity and colour the white of an egg, has as invariably escaped my observation and that of other Physicians in this city as emetics have been administered.”

The Dr. has here made a positive assertion that neither he nor other Physicians in Albany ever discovered any slime that resembled the white of an egg. Unfortunately for his veracity, however, all the Physicians in this city who have given publicity to their opinions totally disagree with the Doctor in this respect. In confirmation of this fact I beg leave to select from the Medical and Philosophical Register, a quotation from the communications of Doctors Low and Eights.

Dr. Low says “the bowels are generally costive and the stomach oppressed with a thick, glairy, tenacious matter, resembling the *albumen ovi*.”

Dr. Eights says “I confess I have not discovered those strong bilious symptoms as described by some Physicians. Many of my Patients to whom emetics have been administered evacuated little or no bile, but on the contrary a *tough, glairy matter, in*

some respects resembling the albuminous part of the egg."

To these gentlemen as residents of Albany, Doctor Yates makes a solemn appeal for the truth of what he states. Whether his veracity will derive much support from their testimony the public must decide.

I beg leave to refer the reader to those communications, where he will find the symptoms described by these Physicians materially variant from those enumerated by Dr. Yates.

Jaundiced indeed must be those opticks which instead of this vast accumulation of viscous slime, or glairy matter, can discover nothing but vitiated bile.

When I commenced with his notes it was my intention to have bestowed upon each in succession a few critical remarks; but the more I examine them the more I am disgusted with the attempt.

They are so perfectly destitute of consistency, of importance, and of common decorum, that I must beg the reader to excuse me from accompanying him through this series of unprovoked invective.

I trust, however, that he will not deem this character of the work so perfectly satisfactory, as to prevent a critical investigation of its truth.

In his attempts to depreciate Dr. Mann's communication he has treated the proceedings of the general administration with indecent asperity. He very unwarrantably introduces their policy into a medical publication, for the purpose of bestowing upon it, a liberal portion of his censure. Such a course is unworthy a scientific performance.

But to return to the 27th page, "I have said that the symptoms of this disease partook of the bilious gaol and malignant fever, but more particularly of the latter."

Does the Doctor find in this epidemic, the brown or black tongue, the dark and fætid sordes about the teeth, the livid flush of the countenance, and the acrid and more intense heat of the skin, that always characterize a malignant fever? If he does, he has been very deficient in not enumerating them among his symptoms of the disease.

But he ought to be informed that a malignant fever is nothing more or less than a *typhus gravior*. The conclusion that necessarily results then is, that he considers the epidemic as a *typhus gravior*, from its commencement. In this opinion he will find but very few to concur; neither do I believe that he intended to adopt it himself; but it is the necessary consequence of his premises. This is another dilemma which his sagacity never led him to suspect.

He has a peculiar *typhus phobia*, for although he labours with so much industry to prove this to be a malignant fever, he derides the idea of its having any connection with a typhus, or a typhoid state of the system. His zeal has again surpassed his prudence, and exposed him to a variety of obvious inconsistencies and absurdities.

To go into a minute exposure of all these, more time is requisite than I have leisure or inclination to bestow. My only object, at present, is to excite the public to a more critical examination of the work. If the errors are discovered the injuries may be averted.

I must therefore earnestly entreat the reader critically to examine the authorities which he has adduced in support of his opinions. And I have no hesitation to assure him, that he will find some of them mutilated and misquoted, so as to convey a meaning in some respects different from what the authors intended.

That they should have escaped notice and obtained currency among the profession, is a subject of surprise, and another evidence of the facility with which any opinion may be propagated.

“ Since the publication of this essay in February, I have been informed that one of these gentlemen has complained of the illiberality and unfairness of my notes. This complaint might have had some colour of reason to support it, had I only given extracts from, or garbled their publications. I gave them entire. That the public might judge of the fairness or liberality of my notes.”

I shall refer to only one instance to test the truth of this solemn appeal to the public. In the second communication, in the first sentence of the description of symptoms, it will be perceived that he has changed “unequally” as it was originally, into “equally” which conveys an idea totally the reverse of what was intended by the author. It will also be seen that he has framed a note for the purpose of ridiculing the sentiment which he has fabricated for this paragraph.—He has erected a shadow to accommodate the weapons with which he had to contend. He found nothing in the original communications on which he could exercise that *poignancy* of wit and satire for which he is peculiarly distinguished, and has therefore permitted his “plodding imagination” to conceive the bold idea of inserting them “entire” in his pamphlet for the purpose of altering the words, so as to convey a meaning which he could refute with the most success.

For this and numerous other evidences of his prowess in science, the Doctor richly merits some noble distinction.

In his second edition the Doctor complains with much asperity, that any practitioner should differ in opinion with him and still use emetics and cathartics.

I never before suspected that he claimed an exclusive right to the use of emetics and cathartics, or that he even pretended to be the author of this mode of treatment. Such however now appears to be his object, and his ire is most dreadfully excited because this practice is pursued without conferring on him the credit of the discovery.

I have previously informed Doctor Yates that ever since the commencement of the epidemic I never entertained but one opinion of its nature and cure. The treatment which I then adopted I have since invariably pursued, except in one instance where I was obliged to deviate to accommodate a consulting physician—and if I were disposed like him to boast, I could say, that although materially different from his, it has been crowned with equal if not superior success.

This treatment consisted internally of emetics, cathartics, diaphoretics, calomel, alkalies and opiates, and externally of the warm bath, spiritous fomentations and epispastics, varying these remedies according to the diversity of symptoms which occurred. Although emetics and cathartics were of infinite utility in the cure, I have not like Dr. Yates confined

my treatment exclusively to these, nor even like him prescribed both of these, where contrary indications rendered them improper.

Neither do I concur with him in explaining their useful effects. This I conceive to be an important point of difference, and one that will essentially controul the cure.

He considers them useful only in proportion to the quantity of bile which they evacuate: such also is the reasoning of every superficial observer, who is ignorant of the œconomy of the human frame.

If the emetic discharges much bile they consider it as having completely attained the object for which it was prescribed, and I have heard some practitioners express their astonishment, that, although their emetic had discharged no bile and very little of any thing, still their patient was essentially benefitted by the operation. A similar remark, I presume must have been made by every practitioner in medicine.

This is an evidence that emetics have a much more extensive operation than merely to evacuate the stomach. And those who are influenced solely by superficial and visible effects, must also be convinced of the truth of this fact, by the universal sweat which they generally produce.

But we have the most satisfactory evidence from authority and observation, that the effects of emetics and cathartics pervade the whole system and are peculiarly operative upon the glands and lymphatics. The healthy action of the liver is often restored by a single dose.

Let those effects be once produced; the healthy

action of the glands be once restored, and that fluid which Dr. Yates is continually recruiting his forces to combat, will vanish like a charm. You will hear no more of his proximate cause, the absorption of vitiated bile, or its accumulation in the alimentary canal.

In prescribing emetics and cathartics therefore, I have this object in view, equally with the evacuations which they produce; and on this principle we may satisfactorily explain the successful operation of the various and apparently opposite modes of treatment that have been pursued.

In different ways they ultimately produce the same effects, they remove the morbid irritation of the glands and the torpor of the stomach and intestines.

Stimulants succeed, by producing a general and more equal excitement through the system. Diaphoretics and calomel have a limited operation upon the glands and lymphatics, and emetics and cathartics have an immediate effect upon the alimentary canal and an indirect one upon the whole system.

From all these considerations, therefore, it evidently appears that the disease is general, affecting in degree all the secretions in the body; and that the morbid state of the liver is more obviously perceived than any other gland, originates solely from the magnitude of this organ, and the various and important uses to which its secretions are appropriated.

Superficial physicians are therefore induced to overlook every other morbid affection, and to regard the liver and its secretions as the only object of cure.

And I have no doubt that the difference in opinion upon this subject among physicians originates from that remissness to a diligent investigation of the symptoms which peculiarly distinguishes some of the profession: If they discover any indications of a redundancy of bile, they are perfectly satisfied of the nature of the complaint, and their enquiries are abruptly terminated in an immediate prescription of emetics and cathartics, to evacuate this fluid.

In most cases the action which evacuants produce upon the alimentary canal, and their consequent indirect effect upon the whole system, are sufficient to procure a healthy secretion in all the glands. But where these prove incompetent, the aid of other means is evidently requisite. In this respect also I essentially differ with Dr. Yates.

Having experienced the utility of emetics and cathartics in only two instances, he seems elated with the discovery and hastily concludes to adopt them in every case to the exclusion of all other remedies.

He therefore rigidly proscribes all other means of cure and says that "*Doctor Wendell and Doctor Bay, two of the most respectable physicians in Albany*" perfectly concur with him in opinion.

If the partial, limited and incorrect view which Dr. Yates has taken of the epidemic, has influenced other practitioners to adopt the same error, it is certainly a subject of extreme regret. I had hoped that this number would have been small and limited entirely to those physicians who had not an opportunity of being very familiar with this disease.

But if it is approbated by these two practitioners, and yielding to Dr. Yates his claims to respectability, we may thence fairly infer that the system which I oppose is advocated by at least three of "*the most respectable physicians in the city of Albany*" which affords the strongest evidence of the unequal terms on which I enter the field of controversy. Unequal as they are, I pledge my reputation, never to surrender the defence of truth.

When the Doctor talks "of plucking out his heart and throwing it to the dogs" he uses a language to me totally mysterious, and as far as I can judge strongly indicative of a *Typhomania*. I trust however that his case is not entirely hopeless, but that by a judicious application of his "unity of remedy" he will be so far restored as to be able to communicate this grand Catholicon, this philosopher's stone in the third edition of his inimitable work, "for the benefit of the Albany Humane Society."

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