

BIBLIOGRAPHIC NOTICES.

Remarks on the Use of Vivisection as a Means of scientific Research ; in a Letter addressed to the Earl of Caernarvon, President of the Society for preventing Cruelty to Animals.
By RICHARD JAMESON. 1844. Pamphlet.

THIS is a very clever letter, defending the expediency of performing experiments on living animals to elucidate physiology and surgery, and to improve the manual dexterity of the surgical operator. It was written in consequence of the Society for the Prevention of Cruelty to Animals having passed a severe censure on those members of the Profession who had performed experiments on live animals. We cannot wonder, where the utility of the end is often so obscure, that the casual observer should view with disgust and abhorrence the mutilations which the practised physiological investigator performs, with such apparent *sang froid*, on animals that have such claims on our sympathies—as the faithful dog or the noble horse—when, even to those who see clearly the end and aim of those slicings of the brain, those prickings of the nerves, those incisions down to lungs, heart, or intestines, the sight is repugnant, and the science of medicine appears in a garb far from attractive, when it demands the sacrifice of such hecatombs of poor, inoffensive brutes, by means which would appear to the uninitiated wanton devices of cruelty to increase the natural pain attending dissolution, by wounds short of death, by tearings and breakings of limbs, by starvation, or the exhibition of deleterious food, and by the excruciating tortures of poison. But most men of science will allow that medicine has been materially benefited by experiments on living animals, and that Cooper, Harvey, Haller, Hunter, and others have arrived at important truths by such means, unattainable by any other. But while we allow this, and fully agree with Mr. Jameson, that bad as the means may seemingly be, they are often fully justified by the aim to be attained, we cannot but feel, that, in Paris especially, it has been overdone, and that the return has not been commensurate with the outlay

of animal suffering and animal life. And though the Society for the Prevention of Cruelty to Animals have gone rather far, as most enthusiasts do, yet as enthusiasm generally obtains some portion of its object, so this Society will doubtless have some effect in making the ignorant pause before they cut.

Mr. Jameson is a very hard hitter, and we think our readers will not fail to be amused with the manner in which he handles the Rev. Mr. Styles. With regard to the great exaggeration of the number of experiments performed on living animals, he says:

“Foremost in the list of exaggerators, I must place the Rev. John Styles, D. D. He is the author of an Essay which gained a prize of £100, as being *the best out of thirty-four* papers sent into the Committee of your Society, and may, therefore, fairly be regarded as the chosen champion of its cause; I shall bestow as much space as the limits of this letter will allow, in exposing some of his most glaring misrepresentations. But few words are necessary to do this, for the statements refute themselves by their very absurdity.

“He informs the public, ‘that every surgeon’s apprentice thinks himself entitled to find his way into the arcana of nature, by scalping cats and rabbits to see where their brains lie.’ ‘The transactions,’ he adds, ‘of the college of the medical craft in this sense would convict them before a convocation of Ashantees.’ Very likely—and why? Because the Ashantees, like Dr. Styles himself, are unable, from ignorance of physiology, to appreciate the end for which vivisections are employed. Supposing that the Ashantees *would* be shocked at experiments on living animals, how much more horrible would they think a surgical operation performed on one of themselves! What needless cruelty to wrench a fine firm tooth out of a poor child’s jaw; or stab him in the arm with a poisoned weapon; or make a gash in the thigh of a man who has only a little swelling behind his knee; or, when a person has been stunned by a fall, what wanton barbarity to cut his scalp and saw off a piece of his skull! ‘How could such cruel experiments answer any good end?’ would be the wise remark of some Ashantee Doctor of Divinity.

“Where Dr. Styles collected his information about the ‘surgeon’s apprentices’ I know not, but it looks exceedingly like a hoax practised on his credulity by some waggish student, who thought to satisfy the Doctor’s love of the marvellous, by telling him horrible stories of ‘what they did at the hospital.’ I will give one or two proofs how excessive his credulity is. He gravely asserts, that ‘oxen are compelled to travel *for many days* without food, *their hoofs worn off, and on bleeding stumps.*’ He might just as well have said at once, ‘with their *heads* worn off.’ The only instance at all parallel to this of the hoofs, is to be found in the ‘Surprising Life and Adventures of Baron Munchausen.’ He had a famous greyhound that ran till he wore his legs away, but was not useless even then, for being a staunch dog, says the Baron, he made a capital pointer.

"Again at page 128, we are told that 'the hippopotamus or river-horse, which, when overcharged with too great a quantity of blood, strikes himself against the point of a sharp reed until he has bled sufficiently, and then rolls into a particular kind of mud until the bleeding is stanch'd, supplied the ancients with the original idea of phlebotomy.' An animal whose hide is proof against a bullet, bleeding itself with the point of a reed!!

"But not content with single absurdities of this kind, the Doctor groups into one sentence a whole cluster of them. Speaking of the small amount of pain inflicted by prædacious animals upon others, he says: 'they obey an instinct which destroys the life of their victims at the least possible expense of pain. It is usually in the night-time and in the hour of sleep that they sink under the fangs of their destroyers; twenty strokes sent home in one instant to the sources of life afford no time to reflect that they are about to lose it.' If Dr. Styles had studied the animal creation before writing about it, or if he had kept his eyes employed as he walked in the fields, or even if he had only sat by the fireside and watched his cat, he never would have written such nonsense as this. Do hawks and kites kill sleeping birds, or foxes sleeping rabbits, or swallows sleeping insects, or do spiders subsist upon somnambulent flies? Had the Doctor's cat depended for her sustenance upon sleeping mice, who, while in that state, are always in their holes, she would have fared but poorly. And when she does catch a mouse, is its life destroyed 'at the least possible expense of pain?'

"But one of the Doctor's greatest exaggerations, and one which most appropriately belongs to this the first division of my subject, relates to the *number* of animals experimented upon. Not only does 'every surgeon's apprentice' think himself entitled to destroy them, but 'in this country many thousands of animals of all descriptions, from the worn-out horses and asses regularly bought for the purpose, to the most minute insect, are dissected alive.' This is a very vague way of talking: it is easy to say—'many thousands;' but I do not believe that *one* thousand animals, large and small, are used for vivisection in the course of the year throughout Great Britain. But as the Doctor is so fond of great numbers, I will just remind him that King Solomon, his pattern of humanity, killed *in one day* twenty-two thousand oxen, and as many sheep. These animals, it will be said, were killed to the glory of God. And, I ask; do not the discoveries of physiology advance his glory?"

Mr. Jameson gives a slight cut or two at Mr. Drummond, whom, however, he lets off rather easily, as he also does Mr. James Macauley, another essayist on cruelty to animals. This last writer derives his chief strength from numerous quotations from the Scriptures; and we shall see by the following quotations how Mr. Jameson disposes of him and King David at the same time:

"It is remarkable that among the numerous passages which Mr.

Macauley cites from the writings of King David, there is not *a single precept** which enjoins humanity to brutes! All the quotations from the Psalms are descriptive either of God's power over the animal creation, or of the cruelty of men towards each other.

He demonstrates the incorrectness of the popular notion of the sensibility of insects:

"See a child catch a 'daddy-long-legs;' the insect escapes by leaving a leg or two in his pursuer's grasp; and an instant after observe the mutilated animal feeding as quietly as if nothing had taken place. The ichneumon fly deposits her eggs in the body of a living caterpillar: the young are hatched there, and the caterpillar continues to feed uninterruptedly while the intruders are actually devouring its living tissues."

"To ascend from insects to animals whose nervous system is more completely developed—to birds and quadrupeds. Here, no doubt, as they are subject to fewer casualties, and are furnished with more perfect means of escape, we find an increased susceptibility of pain. But how absurd to pretend, even in these creatures, that the pain resulting from surgical operations is as great as in man! How trifling comparatively is the mere physical pain of an amputation to that caused by the anticipation of it, or by the consideration of its results! Contrast the situation of an idiot who has lost a hand, with that of a surgeon or an artist similarly mutilated. Would Raffaele or Michael Angelo, or any other great painter, have grieved over the mere severing of so many inches of bone and sinew; or would not the feeling that from henceforth all his glorious conceptions—all the visions of grandeur and beauty on which he had mused for years—must fade away for want of the power to embody them, have caused a pang infinitely more severe than any bodily torture? What would have been the state of mind of John Hunter, brooding over a vast system of physiology, and trusting to his industry and manual skill to work out and prove all his theories, if compelled to lose his right hand? The idiot and the brute no doubt suffer from wounds, or from the various causes which may bring about their death, but, as Dr. Styles observes (and for once he is right), 'that fatal moment is not embittered to them by any of the feelings which render it so painful to most of the human race, regret for the past and solicitude about futurity. They feel the pang of nature but not of mind.'"

Mr. Jameson then exposes the popular fallacy of most of the writers of the Society, that wounds of the brain are painful,

* "The *practice* of this monarch was notoriously the very reverse of humane. When he conquered the Syrians, he 'houghed all the chariot horses.' (2 Sam. viii. 4.) Nor was he less cruel to his human captives: 'Whosoever getteth up to the gutter and smiteth the Jebusites, and the lame and the blind, that are hated of David's soul, he shall be chief and captain.' (2 Sam. v. 8.) 'And he brought out the people that were in it (Rabbah) and cut them with saws, and with harrows of iron, and with axes.'" (1 Chron. xx. 3.)

and mentions a case related by Sir C. Bell, where after a pistol ball had passed through a man's head, he could enter the fingers deep into the anterior lobe, without the patient complaining of anything but of the integument. But is not this integument always more or less extensively divided, particularly in experiments on the cerebellum? The truth is, perhaps most of the experiments on animals are necessarily attended with much suffering to the poor brutes, those by poison especially so; we look, therefore, on this ground of argument, adopted by Mr. Jameson, as very weak; it is much preferable to allow that much pain and misery are inflicted in the great majority of cases, and to rest their apology on the only true ground of defence, that of the great benefits afforded by them to mankind.

Let us see how he treats another of the writers opposed to vivisections:

"A paper-war on the subject of vivisection has, I perceive, been carried on for some time in the pages of the *London Medical Gazette*. Among the most violent of the anti-vivisectionists is a Dr. Hull, who deals about his blows with the blind fury of a raw recruit, rather than the steady skill of an old soldier. He calls those who make physiological experiments by some very hard names; they are 'diabolical,' 'savage,' 'infernal,' 'damnable;' and he quotes as a great authority a Mr. Mac something, 'a surgeon of operative and literary fame, who has been long impressed with the inutility of vivisections.' Neither the fame of Dr. Hull, nor of his friend, has yet reached the city where I am now writing, and I must be excused for the present if I prefer to either of these gentlemen's opinions, those of Harvey, Haller, Hunter, Bell, Dupuytren, Cooper, Orfila, and Hope, who have all declared (and proved) that vivisection was absolutely necessary to enable them to attain the important objects of their research. Another in the *Medical Gazette* recently quoted a number of the *Idler* against vivisection, as if the opinion of Dr. Johnson could be of the smallest weight on the subject of which he was utterly ignorant. The following are some of the passages quoted:

"Among the inferior professors of medical knowledge is a race of wretches, whose lives are only varied by varieties of cruelty, whose favourite amusement is to nail dogs to tables and open them alive; to try how long life may be continued in various degrees of mutilation, or with the excision or laceration of vital parts; to examine whether burning irons are felt more acutely by the bone or tendon; and whether the more lasting agonies are produced by poison forced into the mouth or injected by the veins' . . . 'he that burned an animal with irons yesterday, will be willing to amuse himself by burning another to-morrow.'

"Here we observe the common fallacy of unscientific writers on the subject of vivisection—that those who practise it do so 'for amusement!' Of course any persons who could derive the slightest gratification from inflicting pain, either on their fellow-men or on

the lower animals, would be most justly stigmatized as 'wretches : but where are such monsters of absurdity to be found amongst medical men ? At the very time that Johnson, like a 'good hater' as he was, composed the 17th No. of the *Idler*, Haller (at least as good, as conscientious, and as pious a man as Johnson himself, and certainly not one of 'the inferior professors of medical knowledge') was engaged in a series of experiments on living animals, perhaps the most extensive that has been ever performed ; and was laying the foundation of that system of patient investigation and impartial research which has produced such important results, and has raised medicine from rude guess-work to the rank of philosophy.

"I revere the memory of Dr. Johnson, but I do not love his faults ; nor can all my admiration of his independent character, his noble self-reliance, his unflinching integrity, and honest warmth of heart, make me forget that he was of all bigots the most prejudiced, of all controversialists the most obstinate and virulent."

In favour of his favourite position, Mr. Jameson next goes on to prove that some of the most extensive experimenters on animals were men of undoubted piety, as Haller, Robert Boyle, Rev. Stephen Hales, Dr. Durham, and the famous Boerhave, "a firm believer in the revelations of Christianity," and lastly Dr. Hope.

"I wish as much as your Lordship or any member of your Society that there were no such thing as pain in the world ; but we must take the world as we find it, with its good and its evil, its pain and its pleasure, its joy and its sorrow, endlessly mingled and alternating. A golden age such as Dr. Styles describes, in which beasts of prey and the weaker animals lived together in harmony, could no more have had an actual existence than those 'impenetrable scales' with which his liberal imagination has encased the whale.

"We judge of God's will by his works. What we see done we conclude he willed to be done. Now if we contemplate his works in Nature, we see a never-ending process of destruction and reproduction of life going on, the former frequently attended by *pain*. Whether we regard the great destroyer man, incessantly taking away the life of other creatures to support his own, or the beasts of prey roaming the desert and the forest, or the countless tribes of fishes devouring those weaker than themselves ; or when, as the insect tribes escape our search by their minuteness, we aid our sight with the microscope, and see that the very film that floats on the stagnant pool is one scene of slaughter and devastation ; whichever way, in short, we turn our eyes, we must be convinced that the amount of pain in the world is great beyond calculation.

"What is it that reconciles us to all this but a conviction that it is necessary ? and that this seemingly wanton waste of life is, in reality, a most wonderful provision for preserving organic matter within the limits of the animal kingdom, and thereby economizing, as it

fessor Owen in his recently published Lectures on Comparative Anatomy, where he speaks of the important office which the infusoria perform 'in preventing the gradual diminution of the present amount of organized matter upon the earth. For when this matter is dissolved or suspended in water, in that state of decay which immediately precedes its final decomposition into its elementary gases, and its consequent return from the organic into the inorganic world, these wakeful members of nature's invisible police are every where ready to arrest the fugitive organized particles, and turn them back into the ascending stream of animal life. Having converted the dead and decomposing particles into their own living tissues, they themselves become the food of larger Infusoria, as the Rotifera, and of numerous other small animals, as fishes; and thus a pabulum, fit for the nourishment of the highest organized beings, is brought back by a short route from the extremity of the realms of organized matter.'"

The next argument Mr. Jameson expands upon is the degree of necessity which exists for these vivisections, to acquire or impart knowledge. The first example is that of Hunter, who performed numerous experiments on dogs before he ventured to perform the operation for the popliteal aneurism, now generally used, viz., tying the vessel at a distance from the tumour. Sir Astley Cooper also, before tying the aorta in man, operated several times on dogs, he also ascertained by numerous experiments on animals, the exact process of union in broken bones, and the causes which principally retard it.

"I have yet to notice another illustrious example of talent and industry, who has employed vivisection to a greater extent than perhaps any of his contemporaries: I mean Professor Orfila. The objects which he, and others who laboured in the same field, had in view were these:

"1st. By carefully and repeatedly observing the effects of poisons on living bodies, to ascertain with precision what organ or set of organs were specially affected by certain substances:

"2ndly. To determine what antidotes would best counteract such poisons, and how these antidotes could be most efficiently administered:

"3rdly. To apply the knowledge thus obtained to cases where death might occur under suspicious circumstances; and to ascertain, by *post mortem* examinations, and the use of chemical tests, whether poison had been administered, and, if so, what the nature of the poison had been: points of the utmost possible importance in a legal inquiry, as involving the life of a fellow-creature who may have been falsely accused of murder.

"I bring forward experiments of this kind the more readily, as they determine at once the question of the lawfulness of vivisection, by proving that the end for which they were instituted was a great and important good, and attainable by no other means. These experiments, XXV. NO. 75. 3 o

periments were nearly all painful, many acutely so, and lingering in their operation: if wantonly employed, therefore, for mere curiosity or for amusement, they would have been frightfully cruel. But what has been the result? I refer in answer, to the treatment of persons poisoned, as it existed even at the commencement of the nineteenth century, contrasted with that of the present day. With what happy promptitude in one case is the stomach-pump applied, instead of time being wasted, and the patient at last sacrificed by relying upon supposed antidotes; and how certainly, in another case, does the physician employ the means which chemistry has furnished him to neutralize the deadly drug! To experiments on animals carefully conducted and repeated over and over again, and to nothing else, can we attribute the happy change from the fruitless trifling of the old herbalist to the energetic practice of the modern physician.

"In a late number of the 'Quarterly Review,' the illustrious writer whose experiments I have alluded to is mentioned in a note, as having 'sacrificed the lives of 10,000 animals in the course of his researches into the actions of poisons,' and he is accordingly classed with what the Reviewer is pleased to call 'the Frenchified, butcherly school of anatomical experimenters.' I should like to know how many thousand lives have been sacrificed with no other object than *mere amusement*, by those hard-riding gentlemen whose exploits the Reviewer elsewhere takes such pains to celebrate. The man who devotes years of study to learn how he may best alleviate the pain or save the lives of his fellow-creatures, is called a 'butcher.' If he had quitted his study for the field, had dressed himself like a mountebank, and had ridden his horse to death, or had killed ten times ten thousand hares and rabbits, the Reviewer would have seen nothing wrong in his conduct, and instead of a 'butcher,' would have styled him 'a gallant sportsman.'

"The whole question of the lawfulness of the experiments of Orfila and others, is well stated by Sir David Barry, who devoted much time and labour to an inquiry into the actions of poisons on living animals, with a view to improve the treatment of poisoned wounds. 'Others,' he says, 'talk of needless cruelty. If any useful knowledge is to be obtained by an experiment, none of the means necessary to arrive at that knowledge can be useless, and none else can be adopted without defeating the purpose aimed at; therefore, in useful experiments, there never is needless cruelty, or, in other words, unnecessary pain inflicted.'"

The importance of vivisection in physiology is next insisted on, and to the opinion that the performance of any experiment once is sufficient, and that it will do others to have the results stated to them, he objects:

"Thousands of dead bodies have been dissected, and there are anatomical works without end which contain the results of such dissections, and yet each student has to go through the same processes, to impress things on his memory. So if there be any thing seen on opening a living animal which is important to be seen and to be re-

membered, each must use his own eyes, and not content himself, any more than in questions of human anatomy, with the written reports of others."

After considering its importance to the surgeon previous to performing a great operation, he next objects to indiscriminate or careless use, and makes the following powerful remarks :

" In a passage which Dr. Styles quotes from Dr. Millengen's *Curiosities of Medical Experience*, the author says that vivisection should not be made ' a public exhibition or a student's pastime.' This remark is most just. Among the precautions to be observed by the vivisector, none is more essential than *the avoidance of display*. When an important end is to be gained (as was the case in the experiments I have previously noticed, by Orfila, Dupuytren, Sir A. Cooper, Bell, and others), the means indispensable to that end do not constitute cruelty. In employing these means the experimenter is justified by stern necessity, and, if duly impressed with the importance of his researches, can no more feel an inclination towards display than a conscientious surgeon would during a critical operation.

" Those who consider all infliction of pain on the lower animals unjustifiable, may charge me with carrying the principle of expediency too far. To such I would reply, by asking on what ground but the supposed necessity to an end is the punishment of death tolerated in the present day ? The feeling of revenge, which originally prompted it, is out of the question in a civilized community. It is inflicted on offenders to deter others : ' On ne corrige pas,' says Montaigne, ' ce-luy qu'on pend ; on corrige les autres par luy.' " Whether the proposed end be really attained is doubted by many enlightened persons : no doubt can exist, however, that the means are shocking, infinitely more so than any vivisection of brutes. In killing the latter mere pain is the result ; take the most agonizing process by which a creature's life can possibly be extinguished, still it is so much bodily suffering : we destroy a creature without moral sense, and incapable therefore of doing right or wrong ; we prevent no virtue in this life, we hasten no punishment in another.

" How different is the case of a criminal ! Who can say if his life were spared that he would not repent ? It is not likely, perhaps, but it is possible. He may have been seduced by evil example, the strongest temptation may have assailed him, he may never have had (how often is this the case !) the benefits of education. If that education and moral training were begun even now, his whole nature might be changed, he might yet become a good and happy man : by killing him you prevent all his possible virtues, you make him *die wicked*. And if we take the more solemn view of the question ; if we believe that his future fate *through all eternity* depends upon his life *here*, what a responsibility do we take upon ourselves ! By making a change of life impossible, we seal his everlasting doom."

The natural repugnance one feels to inflict pain on the lower animals is not a fair argument against these experiments, be-

cause we feel an equal repugnance to inflict pain on our fellow-men, and yet we perform the most painful operations, the scooping out of an eye for instance, convinced that by so doing we purchase for the sufferer future benefit, far, far beyond the temporary pain. Mr. Jameson concludes by dilating on this proposition:

“That while necessity alone compels us to take away the lives of animals, our accusers are daily accessory to their wholesale destruction, for the mere gratification of luxury, or as an amusement to while away the passing hour.”

This *tu quoque* sort of argument is not exactly to our taste, and, as we have said before, we would prefer taking the thing on its merits or its necessity, than setting up a sort of oblique defence by accusing the accusers of doing as bad or worse. Mr. Jameson, with great talent, animadverts on the painful killing of animals for food; the mutilation of them to improve their flavour; bulls and rams gelded, sows splayed, cocks converted into capons; geese confined to one spot close to a fire, and stuffed with food, until a disease of the liver takes place, which converts that organ into fat for *patés de Strasbourg*; turkeys crammed by main force; lobsters boiled alive, &c. He vigorously attacks the inconsistency of the members of the Society boasting of having put down bull-baiting and vulgar pastime, while nothing is attempted against the nearly equally cruel, but royal amusement of stag hunting. “Did the poor bull’s suffering arise from the social position of his persecutors? Or do you suppose the stag feels less agony because his tormentors are lords and gentlemen? Prince Albert’s hounds bite as sharply as the butcher’s bull-dog.” We should be happy could we find space to present our readers with Mr. Jameson’s illustrations of how badly the patrons of the Society for the Prevention of Cruelty to Animals, exhibit by their conduct the feeling which their position would promise. Beginning with the Queen (the chief patron) who baits hares with beagles, he then brings forward the Duke of Cambridge, “who killed with his own gun about sixty head of game;” the Duke of Devonshire and the Duke of Buccleugh killing grouse, pheasants, and hares in abundance; Lord F. Egerton, another patron, who gave the clerks and agents in his employment a day’s coursing, when they killed twenty-two hares. But we have made quotations enough to convey some idea of the talent and interest of this letter, and we shall therefore conclude with Mr. Jameson’s final observations:

“Before I bring my letter to a close, allow me once more distinctly to repeat my declaration, that in charging your patrons with

the grossest inconsistency and injustice, I do not mean to accuse them of *wilful* oppression and tyranny. I have no doubt that the hunters and shooters of Windsor and Chatsworth think they are simply advancing the cause of humanity in punishing the baiters of Westminster and cock-fighters of Hillingdon. The persecutors do not see the wrongfulness of their own pursuits, simply because it has never been fairly pointed out to them. The sportsman shoots and hunts because his father and his friends hunted and shot before him ; the possibility of his amusements being cruel has never once occurred to him. Just so it was in Great Britain a century ago with respect to slavery. Englishmen had been accustomed to buy and sell Negroes just as they did pigs or poultry ; and yet those Englishmen were not perhaps worse people than their grandsons. Whitefield (a sincerely pious Christian, if ever there was one) bought Negroes and worked them, and at his death bequeathed them to—‘that elect Lady, that Mother in Israel, that mirror of true and undefiled religion, the Right Honourable Selina, Countess Dowager of Huntingdon.’ In his will the Negroes stand just midway between his ‘lands’ and his ‘books and furniture’ ! But one by one, and little by little, men began to see that the slave-trade was wrong : Sterne said something, and Granville Sharp, and Clarkson, and Wilberforce, said more, and at last most Englishmen were ashamed of what they and their fathers had done as a matter of course ; and now every child will tell you that slave-dealing is a sin. When the thing was pointed out to the people they saw it, but not before : having once clearly seen it they can never lose sight of it again.

“ If the Society over which your lordship presides be really anxious to act up to its title, let it throw aside all that reverence for rank which at present checks its efforts or renders them ridiculous. If cruelty is to be punished, let it be condemned *for its own sake*, not because its effects are sometimes painful to the beholder who happens to have weak nerves : and let it be punished alike in all. In the mean time, let the Society make a better use of its funds than to give away a hundred pounds for an ‘Essay on the Animal Creation,’ by one who is ignorant of the commonest facts connected with it. Above all, let it beware how it attempts, by well-meant, but ignorant interference, to check the progress of a science, whose noble aim it is, by mitigating disease, to prolong the lives, increase the happiness, and promote the social welfare of mankind.”

On Dysmenorrhœa, and other uterine Affections, in Connexion with deranged Assimilation. By DR. RIGBY.

IN the short work before us Dr. Rigby has taken a good deal of pains to shew an intimate connexion existing between deranged assimilation and dysmenorrhœa, with some of its consequences and complications. Influenced by his admission that

the investigation of the affections, to which his observations refer, is still far from being so perfect as he could wish it to be, we shall rest satisfied with laying before our readers some of the points on which he particularly dwells, and for further information refer them to the work itself. It is divided into two parts, the first being confined to a brief consideration of assimilation with its derangements, and the effect of these, as evidenced by a vitiated condition of the blood, and faulty secretion, with an altered state of the functions of the skin and mucous membranes. Into the details of these consequences of mal-assimilation we shall not follow him further than to state, that having shewn

“ That a very intimate connexion exists between the assimilation of the albuminous principle, and the function of the mucous membranes, it will be equally manifest that a healthy or unhealthy condition of the one will determine a corresponding character of function in the other. The secretion from those membranes is of a more or less modified albuminous character, and experience shews, as a general rule, that in mal-assimilation of the albuminous principle, this condition of the mucous secretion usually holds a pretty exact correspondence with the quantum of lithic matters discharged by the kidney.”

Dr. Rigby further dwells on the altered state and action of the mucous membrane, as remarked in the rheumatic and gouty diathesis, wherein their circulation, but more especially the venous, becomes much congested, and the membrane assumes a relaxed, swollen, and deeply injected, even purplish appearance; at times coming on suddenly, and manifesting the same erratic character as gout in other parts. As an illustration of this he refers to attacks of gouty asthma. The occurrence of hæmorrhoids he also adduces as a further illustration of the disposition to congestion of the mucous membrane in these habits, with also the increased secretion of an albuminous transparent mucus, “ particularly distinct in the rectum, and seemingly closely analogous to the albuminous discharge from the cervix uteri and vagina, in certain uterine affections.”

The great tendency in gouty habits of the mucous membrane (but especially of the rectum) to secrete gas, is another point of importance, which will be found to bear out the analogy he wishes to demonstrate as existing between such gouty affections consequent on mal-assimilation and the uterine affections, of which we come now to speak. These he, “ in default of any more appropriate term,” designates uterine rheumatic gout, meaning thereby to imply

“ A certain series of local phenomena or symptoms preceded or attended by a corresponding state of the general system; they are

chiefly of a congestive or inflammatory character, or at least in some degree resembling the phenomena of inflammation, being attended with local vascular excitement of a more or less acute nature, with the chief features of inflammation, viz., heat, swelling, redness, and pain; or of a chronic form, with much venous engorgement, swelling, induration, and ultimately alteration of structure. The first form is more sudden in its attacks and recessions, more erratic in its movements; the latter more gradual, but fixing on the part with a firmer hold, and relinquishing it with proportional difficulty. The acute form is usually seen in connexion with dysmenorrhœal attacks, or with the uterine excitement which is generally observed in such cases, at the half-way time between the menstrual periods. The other is mostly attended by chronic leucorrhœal discharge, and chronic or subacute inflammation of the cervix uteri, followed by induration and organic disease. If not arrested by proper treatment, the acute sooner or later passes into the chronic form, in which case they may be looked upon as different stages of the same disease. The majority, however, of these affections are of a chronic or subacute nature; whether so originally, or from the circumstance that the change from the acute to the chronic form frequently takes place at an early period of the disease, is not very easy to determine."

Dr. Rigby's experience goes to prove, that these symptoms are mostly met with in systems more or less predisposed from long continued dyspeptic and other gastro-enteric derangements, not unfrequently, when the acute form is present, ushered in by an attack of rheumatic fever or gout; or, the general diathesis being gouty, by exposure to cold during a menstrual period or an early abortion, constituting "in fact an attack of dysmenorrhœa in a gouty habit." These are the cases in which exudations are now and then met with, but not necessarily so. The cause of this exudation Dr. Rigby declares his inability to state with certainty, but inclines to the opinion of its inflammatory origin.

"If there be any fact," he says, "which seems to be common to all the cases of dysmenorrhœa which have been attended with exudation, it is the co-existence of some local inflammatory action of a neighbouring organ. In some it has been the kidney, in others (and on the whole more frequently) the ovary, which may easily be presumed from its close connexion with the uterus, and from the generally received opinion of its being essential to the function of menstruation. In others the os and cervix of the uterus itself have been the seat of inflammation."

The chronic form of this so called uterine rheumatic gout is for some time, our author states, preceded by increasing derangement of the assimilating functions, causing, as already alluded to, vitiated circulation, unhealthy action of bowels, with

lithic deposits in the urine, headach and much depression of mind, with more or less atonic leucorrhœa. The disease, in many instances, seems chiefly confined to the vagina and rectum, and is, therefore, unattended with dysmenorrhœa. There are at times symptoms evidencing a state of congestion of the pelvic region (coming on and going off suddenly), as a sense of fulness, weight, heat, and throbbing, with extreme sensibility, so as to require the greatest caution in sitting down.

“On examination, the labra and nymphæ are usually found swollen and flabby, and copiously moistened with a thick, creamy, albuminous discharge. Occasionally, however, they are hot and turgid; the vagina is in a state of soft, flabby tumefaction, its parietes in close contact with each other, and its calibre much diminished by the swelling. The mucous membrane is swollen, and shews evident marks of venous congestion; it is every where thickly covered with the above-mentioned white, or yellowish-white discharge, and not unfrequently the canal is so exquisitely sensitive as to render the introduction of the finger very painful, and sometimes even impossible.”

But the most remarkable symptom is the frequent discharge of flatus from the vagina, apparently the product of secretion from the mucous membrane of this canal, or of the uterus itself. This phenomenon, Dr. Rigby is of opinion, never occurs, except in uterine affections of a rheumatic, gouty character, and, so far from being rare, he believes that uterine derangement, in connexion with this diathesis, seldom or never occurs without the presence of this peculiar symptom; the reason of its not being noticed, he attributes to delicacy on the part of the patient:

“It varies,” he says “much in its appearance; at times taking place to a very considerable extent, being formed very rapidly, and escaping on the slightest motion of the patient; at other times it only occurs towards evening, or is expelled on making any violent exertion; while in some instances, it is only observed at the menstrual periods.”

The considerable excess of urea, uniformly observed in those cases he had constant and regular opportunities of examining, tends, our Author thinks, still further to establish the identity of these uterine affections with gout.

A hæmorrhoidal diathesis is also met with in a large majority, with a state of the mucous membrane of the rectum analogous to that of the vagina already alluded to, with increased secretion of mucus, and at times, flatus.

These attacks of pelvic congestion, though at first transitory, eventually become permanent, and the disease may go on until “symptoms threatening the commencement of scirrhus are but too distinctly established.” The darting, lancinating pains so

generally deemed characteristic of scirrhus, Dr. Rigby thinks not correctly so considered. " Darting pains, however," he says, " cannot be pronounced to be peculiar to this disease, even in its earliest stage of induration." He thinks they are merely neuralgic, and are called into action by the sudden paroxysm of congestion taking place in a gouty condition of the part, bearing a close analogy to the twinge of a gouty foot, and to the darts of pain in *tic dolooureux*, depending on gouty or dyspeptic irritation. This is plausible, but seems a little hobby-horsical.

Thus has Dr. Rigby, following out the views of Dr. Prout, and applying them to his own peculiar department of practice, endeavoured to draw an analogy between these uterine affections and gout, and so refer them to a mal-assimilation. We have abstained from giving any opinion as we passed along, but cannot conclude without stating, that we think the whole has been much overdone, and not by any means satisfactorily borne out by the seven cases he gives. We had expected to have been furnished with some additional information as to the treatment, resulting from the views of these affections taken by the Author, but find ourselves doomed to disappointment. The treatment, as laid down, contains nothing new, resolving itself merely into the round of aperients, tonics, and alteratives, with the usual modes of relieving local congestions. As to the exhibition of *guaicum* and *colchicum*, Drs. Dewees and Locock had already advised such. Dr. Rigby concludes with a tabular view of the analysis of the urine, as taken in three cases, and a few directions, by attending to which, he says, the practitioner may be enabled to observe the more important phenomena presented by the urine in these diseases, and turn such knowledge to profitable account. For these we refer the reader to the work itself, fearing we have already occupied too large a portion of his time.

On Paracentesis Thoracis, with Cases. By H. M. HUGHES, M. D., and EDWARD COCK.*

On Paracentesis Thoracis as a curative Measure in Empyema and inflammatory Hydrothorax. By HAMILTON ROE, M. D., Physician to the Westminster Hospital.†

Account of a Case of Empyema, which recovered after Punctures of the Pleural Sac. By THEOPHILUS THOMPSON, M. D., Visiting Physician to the Hospital for Consumption, and Diseases of the Chest.‡

Empyema, and its Cure, Medical and Surgical: the Result of original Observations. By Dr. ALBERT KRAUSE, Physician to the Royal Lying-in Institution at Danzig.§

Clinical Lectures—Paracentesis Thoracis. By PROFESSOR SCHÖNLEIN.||

IN the April Number of Guy's Hospital Reports for 1844, we find a long Paper on Paracentesis Thoracis, by Dr. Hughes and Mr. Cock; the former treats of the medical, and the latter of the surgical part of the question. In Dr. Hughes' lucubrations we find nothing that is not sufficiently well known already to our readers: it is composed of a few trite remarks that have appeared over and over again in various forms in different lectures and treatises published on the subject, and we were not a little surprised to find many of them introduced as if the result of the author's own observation, and advanced in a manner not only calculated to mislead an inexperienced reader into the belief, that previous to the appearance of Dr. Hughes' essay, the Profession was immersed in midnight darkness on the subject, but also to lead him to suppose that the writer had made some important discoveries, which would soon remedy this excessive ignorance. Our space is too limited to enable us to give many passages from the paper to support these statements, but we confidently refer to the Essay itself for confirmation of them. Is there any thing new, we would ask, in the following summary of the indications calling for the operation of paracentesis thoracis?

"The indications, then, for paracentesis in empyema, or chronic pleuritic effusion, appear to be, in the first instance, the presence of a large quantity of fluid in the pleura, rapidly effused; in the second, the distress of the patient, dependent on the great accumulation of fluid; and in the third, the existence of a considerable amount of

* *Guy's Hospital Reports*, Second Series, No. 111, April, 1844.

† *Lancet*, May 4th, 1844.

‡ *Ibid.*

§ From a Review in "*The London and Edinburgh Monthly Journal*," June, 1844.

|| *Lancet*, for 1844.

effusion, together with such a state of constitution, or of general health, or such other circumstances, as would render a prolonged purely medical treatment injurious, or undesirable."

In other parts of his paper Dr. Hughes introduces remarks that, notwithstanding their new garb, we must claim as old acquaintances; thus, at page 58, our author, speaking of the diagnosis between a phthisical cavity and pneumothorax, very confidently puts forward, as the result of his own observation, the following points of difference:

"The diagnosis, in a vast majority of cases, may, notwithstanding, be effected with facility, and, in circumstances of difficulty, may be assisted by the following considerations: In simple phthisis, the tympanitic resonance and the metallic tinkling (not common in any degree) are rarely so well marked as in pneumothorax; while succussion is so very unfrequent in the former, as never, with certainty, to have been heard by myself, or by any one with whom I have communicated on the subject. Laennec is reported to have heard it on one occasion. In pneumothorax the chest is *generally* enlarged; in simple phthisis it is almost always contracted. In the former affection the patient usually lies on the affected side; in the latter, upon either side, or upon the back indifferently. In pneumothorax the cavity is commonly at the lower part of the chest; in simple phthisis the chief excavation is almost universally at the upper part."

Seeing that all the important points of diagnosis in the above passage had been for some years before the Profession in Dr. Stokes's work, we did expect that our author would have made some sort of allusion to the researches of the latter gentleman, particularly as he admits in various places having perused that excellent treatise; if so, we cannot help wondering how the passage should have escaped his notice. It alludes to the diagnosis between a phthisical cavity and pneumothorax, and runs thus:

LARGE CAVITY WITHIN THE LUNG.	PNEUMO-THORAX, FISTULA, EMPYEMA.
" 1. Metallic phenomena much less developed.	1. Metallic phenomena intense.
" 2. Signs supervening gradually.	2. Phenomena suddenly developed.
" 3. Side not dilated. It may be contracted.	3. Side generally dilated.
" 4. Sound on percussion dull, or with resonance of a cavity.	4. Percussion exactly indicating the extent of air and liquid.
" 5. No lateral displacement of the heart.	5. Lateral displacement considerable.
" 6. Cavernous râle large.	6. Cavernous râle absent.
" 7. Sound of fluctuation absent or indistinct.	7. The reverse.
" 8. Pectoriloquism often present.	8. Pectoriloquism absent.*

* Diseases of the Chest, p. 408.

On comparing these two extracts, it will be evident to our readers that all the valuable diagnostic points have been insisted on much more fully by Dr. Stokes than by our author, and we cannot but think that he would have done his readers a greater service by extracting the whole passage we have just quoted, than by the introduction of portions of it. Dr. Hughes next alludes to the diagnosis between empyema and phthisis, in which we find nothing worth quoting; he is evidently quite ignorant of the forms of empyema, to which Dr. Green and Dr. Robert Mac Donnell have directed the attention of the Profession, otherwise he could not have written that he has

“Known many cases, very many cases, in which empyema has been mistaken for phthisis. But this has arisen, *not from any real difficulty in the diagnosis of the two complaints, and when the physical signs have been consulted, but simply from their not having been efficiently employed.*”—page 61.

We beg to assure Dr. H. that there are cases which do present very great difficulties, even where physical signs have been “*efficiently employed*,” and by physicians equally skilful as he is, in availing themselves of the assistance of those signs. In support of his view Dr. H. gives the following case :

“An intelligent practitioner, and a very fair auscultator, formerly a pupil of Guy’s Hospital, sent a patient from the country town in which he resided, for my opinion as to the condition of the chest, as the individual had many, or most of the general symptoms of phthisis, though he was unable to detect any physical indication of the existence of that disease in the apex of either lung. I stripped the patient; the complaint appeared evident at a glance; auscultation and percussion completely confirmed the hasty conclusions derived from simple inspection. I sent him back to his medical attendant, with a note requesting him to inspect and examine the whole chest, but without stating my own opinion. He immediately wrote in reply that there could be no doubt of the presence of empyema, which would consistently explain the whole course of symptoms, and that the clear indications of that disease had not been previously discovered, simply because the probability of their existence had not been contemplated, and therefore had not been investigated.”—page 62.

We leave our readers to form their own opinion of the *intelligence* of this practitioner, but we cannot agree with Dr. H. that he was “a very fair auscultator,” although “formerly a pupil of Guy’s Hospital;” nor can we bring ourselves to believe that he knew anything at all about the science, otherwise he would not have sent his patient “from the country town in which he resided” up to London, for Dr. H.’s “opinion as to the condition of the chest,” without having himself previously examined every part, particularly as he had failed to find evi-

dence of phthisis at the apex of either lung. This simple case of empyema, examined by a careless or ignorant practitioner, is adduced to shew how easily the disease might be mistaken for phthisis!

Dr. Hughes next discusses the diagnosis between empyema and malignant disease of the lung or pleura, and adds, that

“Neither Dr. Townsend nor Dr. Stokes enter upon the diagnosis between these two complaints; and though I have recently perused, for the third time, the valuable essay of the latter gentleman on malignant diseases of the lung, in the twenty-first volume of the Dublin Journal, I am unable to collect therefrom anything which induces me to alter the tenor of the following observations in the sixth volume of Guy’s Hospital Reports.

“‘It must, however, be added, that the disease sometimes, as in the second case herein related, very accurately resembles empyema; that the history of the case, and the physical signs are on such occasions insufficient for the purpose of distinguishing the two complaints; and that the diagnosis, if at all practicable, must be deduced from the general symptoms, the peculiar character of the expectoration, the obstruction to the flow of blood, through the superficial veins of the affected side, and the appearance of malignant tumours in other parts of the body.’”—page 65.

Here, again, we find Dr. Hughes appropriating to himself the discoveries of others, and we see with what self-complacency he puts forward the ground of diagnosis between malignant disease of the lung and empyema, coolly asserting that “neither Dr. Townsend nor Dr. Stokes entered upon the diagnosis between these two complaints.” This last announcement, we confess, took us completely aback, particularly as we had so often perused the points of diagnosis in Dr. Stokes’s book, to which Dr. H. alludes, and we now quote them that our readers may compare what Dr. Stokes published in 1837, with that put forward by Dr. Hughes in 1844. After describing an interesting case of cancerous disease of the lung, Dr. S. points out the particulars in which it differed from ordinary phthisis and pneumonia, and then goes on to detail the circumstances in which it differed from empyema:

“Lastly,” he says, “it was at one time supposed that the case was empyema, but with this the signs were also irreconcilable; the side was contracted, the intercostal spaces unaffected, the vibration of the voice was not extinguished; position made no difference in the signs; the heart was in its natural situation, and the liver was not displaced until a short time before death. The dulness and absence of respiration, if proceeding from empyema, would point out the greatest possible effusion, yet the remaining phenomena were inconsistent with this condition.”

“But other unusual circumstances existed, namely, *the varicose*

state of the subcutaneous veins, the dysphagia, the predominance of œdema on the right side, the apparent enlargement of the liver, and the growth of these external tumours, which were, doubtless, of the same nature as the internal lesion."

Again, in his paper on "Cancer of the Lung and Mediastinum," in vol. xxi of this Journal (and which Dr. H. says he has read three times over, without finding anything which could enable him to distinguish between that disease and empyema), we find Dr. Stokes speaking more explicitly on the point:

"That the following symptoms are important as indicative of this disease: pain of a continued kind; a varicose state of the veins of the neck, thorax, and abdomen; œdema of one extremity; rapid formation of external tumours of a cancerous character; expectoration similar in appearance to currant jelly; resistance of symptoms to ordinary treatment."

"That though none of the physical signs of this disease are, separately considered, peculiar to it, yet that their combinations and modes of succession are not seen in any other affection of the lung."

These passages, from Dr. Stokes's writings, show, beyond doubt, that he had, long before the appearance of Dr. Hughes's essay, pointed out the grounds of diagnosis; and we only wonder how Dr. Hughes could, after the third perusal of Dr. Stokes's paper, assert that he had not entered upon the diagnosis between the two diseases. In one part of his paper, Dr. H. states, that "there are few members of the Profession for whose opinions I have a greater respect, and I may perhaps add, from whose writings I have derived more information than those of Dr. Stokes," we have only to hope that, in future, he will acknowledge with candour the source from which he derives that information.

These remarks have been suggested by an attentive perusal of Dr. Hughes's various essays, all of which are strongly tainted with the same fault we have pointed out, viz., putting forward, as his own, the discoveries of others, and which, we trust, will be remedied in his future lucubrations.

Mr. Cock prefers opening the chest "below the scapula, between either the seventh and eighth, or eighth and ninth ribs, and at a point distant from one to three inches from the angles of the bones." At page 70, he says:

"I will make a few remarks on the size of the instrument which I employ for paracentesis thoracis; for I consider this to be a matter of considerable importance as regards the facility of introduction, and the diminished liability to injury, as well as the successful result of the operation. The trochar and canula which I have found best adapted for general use is about one-twelfth of an inch in diameter, and about two inches in length, exclusive of the handle. In some cases, where there exists great œdema of the subcutaneous tissue, a

longer instrument might be required to penetrate the pleural cavity, and I have in more than one instance found it necessary to pit the skin by pressure with the finger, before the canula could be pushed far enough to reach the fluid. I prefer a circular to an oval instrument, as the former is more easily introduced and does less injury to the intercostal muscles, whose fibres are perpendicular to the long diameter of the oval canula. I am aware that I use a much smaller instrument than is usually employed in paracentesis, but I think it has many advantages; its introduction is easy, and attended with so little friction, that the operator feels his way before him, and is immediately conscious when he has entered the cavity of the chest. It gives but very slight pain, as it does little more than separate, instead of lacerating the tissue through which it passes; and it is calculated to elude the nerves and vessels, whose immunity from injury no skill or care of the operator may otherwise be enabled to secure on every occasion. On its withdrawal, the opening which it has made becomes immediately and permanently closed, thus at once restoring the integrity of the cavity which has been entered. It is adapted to all ages, from the infant to the adult, and can hardly fail to find its way between the ribs, however narrow the intercostal space may be from age or formation, and however nearly the bones may have become approximated during the progress of the disease."

Mr. Cock next alludes to the narrowing of the intercostal spaces, which, he very correctly observes, is noticed in the advanced periods of empyema; but he is wrong in attributing this change to a mere mechanical approximation of the bones to each other, as it has been shown at the Pathological Society of Dublin, to be owing to an actual enlargement of the ribs, the tendency of which is to destroy the intercostal space, and thus bring the ribs into closer proximity. Mr. Cock, however, deserves credit for having observed the fact, though unaware of its true explanation.

Another advantage claimed by Mr. Cock for his mode of operating is,

"That it ensures a slow and gradual evacuation of the fluid, and enables us to avoid the admission of air, both of which are, in my opinion, matters of great importance in the operation, more especially where the effusion is of recent occurrence, and when we may, therefore, reasonably suppose that the lungs have not become permanently collapsed by the pressure."

He next discusses the point, whether or not admission of air into the cavity of the pleura, during the operation, is attended with danger; he is decidedly of opinion that it is, but as we intend introducing the interesting debate on this subject, which took place lately at the *Medico-Chirurgical Society*, we shall pass over Mr. Cock's excellent remarks, merely stating, that he has observed on some occasions, after the operation of tapping

for *hydrothorax*, that a change has taken place in the character of the fluid which either had been left in the cavity or had become subsequently secreted; that when, after the lapse of a short period, paracentesis has again been performed, the fluid had lost the limpid transparency of pure serum, and become turbid and discoloured, approaching more in its characters to the fluid of empyema.

Dr. Hughes and Mr. Cock next give a table of twenty cases of empyema, in which the operation was performed, in some with perfect success, in others with relief. Out of the twenty cases there were six in which phthisis complicated the disease, in two of which the patients were labouring under the affection at the time of the operation, and it was only proposed as a palliative measure; in the other four cases the disease ensued after the operation, or, at least, was not detected at the time it was performed.

There were seven cases of perfect recovery, in five of which there was no other disease; in one there was secondary syphilis, in the other a "tumour of the abdomen." There were also three cases of *partial* recovery, one of which was complicated with an old pneumonia, another with ascites, diarrhœa, and phthisis, and the third was accompanied by enlarged liver* and ascites.

From these facts it appears, that when the empyema is uncomplicated with structural disease of the lungs or other organs, that the operation, so far from being dangerous, has, in the hands of Mr. Cock, been extremely successful; and we have no doubt whatever that this success is in a great measure due to the mode of operation proposed, and the instruments employed by him, which we have reason to know are those now generally adopted by most of our Dublin surgeons, and with very remarkable success as compared with the old mode of tapping the chest. We must now conclude our notice of this very useful paper, and regret that we felt it our duty to speak of Dr. Hughes's portion of it, as our sense of duty dictated.

We are unable to give our readers a more full account of Dr. Hamilton Roe's paper on *Paracentesis Thoracis* than is contained in the *Lancet* for May 4th, 1844, from which we extract the following notice of it, and the discussion that followed the reading of Dr. Roe's and Dr. T. Thompson's communications, from which our readers will ascertain the opinions of some of the leading members of the Profession in London, with respect to the operation:

* The case is given in detail in the essay; the empyema was situated in the *left* pleura, and was evidently one of those cases to which Dr. Robert Mac Donnell has directed the attention of the Profession, and to which we shall return farther on.

“The author commenced his paper by alluding to the very generally-received opinion of the uselessness of paracentesis thoracis as a curative measure in the treatment of pleuritic effusions, as founded upon the supposed facts, that re-accumulation of fluid necessarily takes place after its withdrawal by the operation; that the frequent repetition of tapping subsequently demanded in those cases in which life had been prolonged by it, was only sufficient for the alleviation of certain urgent symptoms; and, lastly, that recorded experience is completely opposed to the employment of the operation. His own experience had induced the author to depart from these opinions, and a careful examination of the cases recorded by various writers during the period of thirty years, had supported him in the correctness of the results at which he had arrived. Of thirty-nine cases recorded in British medical journals, between the years 1812 and 1842 (a period selected as that in which an acquaintance with auscultation had rendered the diagnosis more accurate), in all of which paracentesis had been had recourse to, he had found that eleven only died. Twenty-four cases had occurred to himself, and the chief object of the author was to prove, from the results he had obtained, that the operation was as free from danger as any other performed upon the human body; that most of the evil consequences supposed to attend it are far more imaginary than real; that it is commonly successful when employed at an early stage of either empyema or inflammatory hydrothorax, and the common cause of failure is to be found in the late period at which alone it is alone regarded as admissible.

“The author next proceeds to notice at length the objections of certain writers to the treatment of empyema by operation, and opposes the opinion that the ultimate removal of pleuritic effusion must depend upon the action of the absorbents, and observes, ‘that the proper function of the absorbents is to carry off the ordinary healthy secretion, but not a diseased one; that this power may be sufficient to take up the quantity usually secreted, and yet wholly unequal to take up many pints added to it;’ and that it is, at least, very probable that the action of the absorbents becomes paralysed, partly by over-distention of the membrane in which they are situated, and partly by the general prostration of strength produced by the great suffering and the disturbance which the other functions undergo from the pressure upon vital organs. These views were supported by a case of inflammatory hydrothorax, in which the withdrawal of a small quantity of fluid from the chest was sufficient to lessen the mechanical pressure upon the absorbent vessels, and thus to enable them to reassume their healthy action, and by which the great bulk of the fluid was subsequently removed.

“With reference to the supposed dangers of the operation, the author had been unable to find one case on record in which mischievous results had occurred; in his own practice no precaution had been employed to prevent the admission of air into the pleural cavity, but no bad results had followed; and even in one case (the only example of the kind) in which the air failed to be absorbed with the rapidity commonly observed, it had been readily withdrawn by means

of a syringe, the wound made in tapping the chest having been healed; but, although the accidental admission of air at the time of the operation was never productive of bad effects, yet its continued entrance in those cases in which a fistulous opening had been made into the pleura, had very commonly been followed by mischievous results; the author, therefore, was strongly in favour of the complete removal of the fluid, and the immediate closing of the aperture. Although it was difficult to determine what length of time might, without danger, be allowed, for the exhibition of internal remedies, yet, from his own experience, the author was induced to believe that in the general class of cases three weeks is the longest period that can with safety be permitted to elapse before the withdrawal of the fluid. In none of his own cases had complete restoration of the lung resulted after it had been subjected to the pressure of pleuritic effusion for six weeks. It was important to understand rightly the exact state understood by the term 'cured empyema,' much of the difference of opinion relative to the propriety of tapping the chest depending upon the varied manner in which such expression is employed. Thus, in the posthumous essay of Dr. Hope, a long line of cases of empyema are recorded, in which the continued action of mercury was followed by 'cure.' In the essay in question, however, it is not stated that in any one case the lung had been restored to its healthy condition. By the early employment of paracentesis, those changes in the pulmonary tissue by which its expansibility is destroyed are prevented, and thus, not only is the removal of the pleuritic effusion effected, but the lung also restored to the full performance of its function.

"The author next describes, at length, the morbid changes produced in the pleura by the long continuance of purulent collections, more particularly alluding to the manner in which the pleural sac becomes ultimately obliterated in chronic cases of empyema, in which the effusion has been very slowly removed, and at a later period. As one of the secondary effects he had also observed, that tubercular disease of the opposite lung not unfrequently occurs in old cases of empyema. Amongst the several changes produced in the lung during the existence of pleuritic effusion, the most important were those by which it was rendered subsequently incapable of expansion. In old cases of hydrothorax, the author had observed such effects to result from condensation of the pulmonary tissue, which was often so complete as to prevent even partial expansion by the artificial introduction of air. In the cases in which purulent secretion had resulted, it had been frequently found that the pleura had become wholly altered in character; in some cases adherent to the parietes of the chest, in others greatly thickened and contracted, and so binding down and compressing the lung as to render its restoration impossible.

"Twenty-four cases had occurred to the author, which had been treated by paracentesis thoracis; of these eighteen recovered, and six died. Nine of them were cases of purulent effusion, of which eight recovered, and one died. Thirteen were cases of inflammatory

hydrothorax, of which nine recovered, and four died. One was a case of hydrothorax dependent upon cardiac disease, in which relief was afforded by the operation, and the remaining case was one of pneumothorax, having a fatal termination.

“In conclusion, the author remarked on the comparative value of certain physical signs, alluding to one of them, namely, the distention of the intercostals, as a differential sign, serving to mark the character rather than the quantity of the contained fluid. He had repeatedly found that retraction of the intercostals had existed with a very large amount of pleuritic effusion; and, on the other hand, that very distinct bulging of those spaces had been present with a very small quantity of fluid. In the former class of cases the fluid had been serous, in the latter purulent, and he was disposed to adopt the opinion of Dr. Stokes, and of some of the older writers, who believe that the projection of the intercostals depends upon the purulent character of the effusion. The inability for patients affected with pleuritic effusion to lie upon the healthy side had not been observed to accord with the opinion commonly expressed in systematic works, but that the contrary rule obtained in nearly one-half of the cases. It was somewhat remarkable that the disease more commonly existed on the left side of the chest, and of the twenty-four cases adduced by the author the ratio had been as two and a half to one.”

“In the summer of 1843, Dr. Thompson visited, with Mr. Roberts, of Great Coram-street, a little boy, between five and six years of age, who had suffered for two months from febrile symptoms. On examining the chest conclusive indications were observed of purulent effusion in the right side, and the operation of paracentesis thoracis was performed on the 27th of June. The puncture was made through the fourth intercostal space, and fourteen ounces of pus were withdrawn, various precautions being adopted to prevent, as much as possible, the access of air. On the 30th of June the operation was again repeated, and about a pint of matter withdrawn. The relief obtained, although very considerable, proved only temporary; notwithstanding the use of appropriate remedies the accumulation was renewed, and on the 10th of July the operation was performed for the third time, twenty ounces of pus being removed. After the performance of the fourth operation, on the 21st of July, when twenty-two ounces of thick, but not foetid matter was removed, the boy improved in strength, and the excess in girth of the right side of the chest over the left was materially reduced. On July 28th the puncture, which had been for some days perfectly healed, opened spontaneously, and within twenty-four hours, gave exit to about four ounces of pus. After three days the discharge ceased, but above the seat of the two last punctures a swelling was formed about two inches in length, at the posterior part of which an aperture, discharging matter, appeared. On the 16th of August both openings were discharging; the anterior spontaneously, the posterior when pressed. The local symptoms gradually became more favourable and the general health improved, so that in September the boy was able to walk

out. The right side of the chest contracted, and in the month of November was an inch and a half less than the left in circumference. A partial healing of the aperture being followed by some aggravation of the symptoms, it was determined to attempt the gradual emptying of the sac and approximation of its sides. This object was successfully obtained by means of plugs made of sponge, firmly tied with pack-thread, and saturated with wax; matter, which was to the last inoffensive, was thus repeatedly removed from the cavity. The opening healed about the end of January, and the boy has since remained perfectly well.

"The author remarks that, although *serous effusion* into the pleural sac is frequently removed with little assistance from medical treatment, yet that, in cases of considerable purulent effusion, there is little hope of relief without an operation, which, in most instances, should be performed early, without losing time and hazarding strength by the use of mercury and diuretics. He argues that puncture of the thorax involves no circumstance of peculiar hazard, provided suitable precautions be adopted. Amongst these precautions he urges the expediency of repeating the operation, in preference to removing a large quantity of pus at once, and especially insists on the conclusive evidence which recorded cases afford, that the practice of leaving a canula in the wound is highly detrimental, in consequence of the increase of pleural inflammation and decomposition of the enclosed matter, owing to the long-continued contact of atmospheric air.

"Dr. C. J. B. WILLIAMS inquired the average age of the patients operated on by Dr. Roe. The success of the operation depended so much upon age, that he was desirous of information on that point.

"Dr. ROE replied that few of his patients were under twenty, and that some of them were above thirty.

"Dr. WILLIAMS rose, and said he considered that it would be scarcely right to discuss the paper before the Society without being acquainted more fully with its contents, than the abstracts read by the secretary had enabled him to be. He would observe, however, that the result of Dr. Roe's cases was to him altogether a matter of surprise, as the success was far beyond any thing he had seen in his own practice, or recorded in that of others. If this success were confirmed by subsequent experience, the prospect would be most encouraging. He should not discuss the contents of the paper further, but should refer to several points in connexion with the operation of paracentesis; and first, as to the propriety of adopting measures for the exclusion of air from the pleural cavity during the proceeding. Now, several years since, he had performed the operation in a number of cases, and the result had been invariably fatal. This had been the case also with Dupuytren and many other surgeons, and the consequence had been, that the operation had fallen into disrepute, and opinion was uniformly against its employment. In all the cases alluded to, no attempt whatever was made to exclude the external air, and the canula was sometimes left in the wound. The evil effects were evidently the result of the entrance of air into the chest.

He concluded this from the following facts:—The immediate effect of the operation in all the cases was a very decided relief to all the symptoms; and in no case of pure uncomplicated empyema was the fluid first drawn off in any way foetid; and in addition to the immediate relief, the patients seemed as if they would do well. In three or four days, however, symptoms of irritative fever came on; the pulse became quicker; the patient was affected with night sweats; became cachectic; and died. Now, it was invariably found that the discharge from the wound, whether of pus or air, became more and more foetid as these symptoms progressed; and, indeed, one condition seemed to bear so complete a relation to the other, that he had no doubt that the cause of both was the same, and that this was the entrance of the air. He had observed this sympathy, particularly in adults, the exceptions to it being in children, in whom, when the discharge and air from the wound were foetid, the constitution did not always appear to suffer in the same degree. It seemed, indeed, that there was greater power in the child than in the adult, to resist the noxious influence of decomposed pus. Fully impressed with the truth of these remarks, in all subsequent operations he had adopted measures for the exclusion of air, and in none of these did the foetid condition of pus come on, nor did any irritative fever occur. He would now inquire whether the operation was an expedient one? and reply to that question by saying, that it was not so frequently necessary as had been supposed; and he thought that by-and-by, most practitioners would agree with Dr. Stokes, that it ought not to be resorted to except in cases in which there was pus in the chest. It might be difficult in some cases to decide whether the effusion were purulent or not previous to the exploration of the chest, or of the performance of the operation; but generally, he thought, the hectic fever, the frequent pulse, and other constitutional symptoms, might decide the question. In such cases, then, when the symptoms were aggravated, when the dyspnœa was urgent, and life itself threatened, he should resort to the operation. When these urgent symptoms, however, did not obtain, and there was reason to think the fluid to be serous, he should be inclined to wait for the effect of remedies. He spoke thus from experience, for within the last few years he had seen many cases (in which, years before, he should have advised an operation), where the oppression of breathing and constitutional disturbance had given way under the use of tonics and slight stimulants. He related the case of a lady, in whom there was effusion into the pleura, accompanied by much constitutional debility. Antiphlogistic remedies, with mercurials, were employed, without benefit, and she seemed to be sinking from the affection. He was called in. It was evident that the system required support; tonics and slight stimulants were accordingly administered, and from that moment the constitution began to rally, and she eventually got quite well. We did not, he thought, sufficiently consider the fact, that effusion often depended on the low state of the circulation, and that as the strength increased, the effusion diminished. Two objects were sought to be obtained in the operation of paracentesis, the removal

of the liquid, and the prevention of the ingress of air into the chest ; the air irritated and kept up the effusion, and, he believed, by pressure, prevented the expansion of the lung. With the view of preventing this accident, he had practised, and recommended the employment of pressure on the parietes of the chest, which pressure should be continued as long as the liquid was allowed to flow. By this means alone, it was easy to prevent the entrance of air into the pleura ; but other measures had also been employed to obtain the same result. Thus, it had been recommended that the patient should be placed in a bath during the performance of the operation, so that if anything did enter the cavity of the chest, it should be water. This plan was said to have been successfully employed in Berlin. Another plan was, the employment of an instrument with a valve, which, while it prevented the entrance of air, allowed of the exit of fluid ; but this instrument was liable to be blocked up by the matter. Again, it had been advised that the intestine of a rabbit, or small animal, should be attached to the canula, that the intestine should be placed in water, so that the air would be kept out, and the fluid allowed to ooze out. This plan had the advantage of allowing the wound to be kept open for a long time, and of preventing the necessity of a second operation. The case, however, by Dr. Theophilus Thompson, read that night, proved the value of repeated operations over that of emptying the chest at one operation.

“ Dr. MAYO took occasion to comment on a mixture of calomel with sulphate of quinine, as used by Dr. Thompson, which he considered to be questionable, as one of the compounds might interfere with the other.

“ Dr. CURSHAM made an inquiry respecting the diagnostic differences in regard to increase of size in the chest, in cases of serous and purulent effusion.

“ Dr. THEOPHILUS THOMPSON, in answer to the last question, replied, that he had repeatedly seen the bulging out of the chest, both in cases of serous and purulent effusion, and that he did not consider it a diagnostic mark between the two kinds of fluid. The frequency with which serous effusion was removed by the agency of medicine would make him careful of resorting to operative means for its evacuation. He related the case of a gentleman in whom, after mercurial and other antiphlogistic means had been resorted to, the effusion had been got rid of by the decoction of winter green ; and the case of a boy, in whom calomel appeared to do harm, but who was cured by the employment of spirits of sweet nitre, with demulcent medicines. The objections raised by Dr. Williams had reference rather to the mismanagement than to the use of the operation. These objections, however, shewed the importance of employing the precautions which he had alluded to. He vindicated the combination of remedies which he had employed in his own case, and remarked that the combination of tonics with remedies like calomel was often beneficial.

“ Mr. BENJAMIN PHILIPS spoke from the result of his own experience, which extended over sixteen or seventeen cases, two of which only were purulent, the others being serous. He had seen, in several

of the latter cases, the bulging out of the chest, and he, therefore, did not consider this sign as at all conclusive evidence of the effusion being purulent. It was desirable, if possible, to determine whether the effused fluid were purulent or serous, for, if purulent, the entrance of air might be injurious. When he had performed the operation in cases of hydrothorax, he had frequently observed the air to completely expand the chest, which became as resonant as a drum; but no evil results had followed, and the air had disappeared in a few hours, but by what means he did not pretend to say. His experience with respect to the operation was less favourable than that of Dr. Roe, as in not one case in which he had operated had the patient lived for six months afterwards. In all the cases, immediate relief followed the operation; in many, the relief endured for weeks, but the fluid again accumulated, and tapping was again required. In some cases, in which, however, there was no evidence of inflammation of the pleura, the fluid from the second tapping contained more albumen than that from the first.

“ Mr. HEWET was surprised at the result of Dr. Roe’s cases. He had seen several instances of effusion into the pleura which had been operated upon, and they had all died. The only case which had recovered was one in which nature had effected an external opening for the effused fluid. In cases of hydrothorax there was occasionally such a coating of lymph over the lung, that it could not expand, even though the fluid were entirely removed. In these cases, the exploring needle would fail in detecting the real nature of the case. He related an instance in which a man died of dyspnoea two days after the operation of paracentesis. It was found that the lung was coated with such a quantity of lymph, that it could not be inflated until several incisions had been made into the false membrane.

“ Dr. KINGSTON observed, that Dr. Roe’s results could not be regarded as *opposed* to those of others, as they related to cases differently circumstanced. In those instances of pleuritic effusion, in which paracentesis thoracis had been hitherto performed, it had not in general been resorted to until the constitution had been completely worn out by the long continuance of the disease; whereas the point which Dr. Roe’s twenty-four cases tended to establish was, that although hardly ever successful, when postponed to this late period, it would succeed in the great majority of the cases in which it was required, if performed at a period when the constitution was better able to struggle with the disease, and bear up under the effects of the operation.

“ Mr. LLOYD briefly alluded to three cases in which the effusion, though supposed to be in the pleura, was really external to that membrane. The fluid was evacuated, and the patients did well for a time, but afterwards died of other diseases. He believed that tapping, either in empyema or hydrothorax, never permanently removed the cause of the disease, and that the patients soon perished, either of that or some other disease.

“ Mr. ARNOTT had seen the bulging out of the intercostal spaces as frequently in cases of serous as purulent effusion into the chest. His experience proved that in cases of serous effusion, the entrance

of the air into the chest did no harm, as it was soon absorbed, and gave rise to no bad symptom. When the effusion was purulent, he should not allow more than twelve or fourteen ounces of effused fluid to escape at one time, and he should employ pressure over the parietes of the chest, as recommended by Dr. Williams, to prevent the ingress of air; he thought, however, that when the quantity of fluid removed did not exceed that already mentioned, there could be little fear of air getting into the pleural cavity. He vindicated the combination of quinine with mercury, in practice, as often most serviceable.

"Dr. WEBSTER passed an eulogium on the paper of Dr. Roe, which shewed the advantage of resorting to the operation of paracentesis early.

"Dr. HAMILTON ROE, in reply to the observations of the previous speakers, observed, that the first objection urged against the employment of paracentesis, by Dr. Williams, was that of the admission of air into the pleural sac. In the course of the paper he had distinctly stated, that although the admission of air into the pleura, during the operation, was neither injurious in itself, nor mischievous in its subsequent effects, yet that its continued entrance, as in the case of a fistulous opening, had not unfrequently been productive of bad effects. It had been one of the objects of his paper to demonstrate the advantages of operating in the manner practised in the cases he had brought forward, namely, that of withdrawing at once the entire collection of fluid, and immediately afterwards closing the aperture made by the trocar. In cases in which a fistulous opening had been made, either by retaining the canula in the wound, or otherwise permitting the continued ingress of air, unsuccessful results had followed, in the majority of cases to be attributed to the physical changes produced in the pleuritic secretion, by the contact of the air; in many examples of hydrothorax, treated by making a fistulous opening, the effused fluid, before serous, had become purulent, and a severe form of disease was thus induced. A good illustration of this fact had been referred to both by Dr. Theophilus Thompson and himself, in the case which had occurred to Dr. Stroud. The strongest evidence against the old opinion quoted by Dr. Williams, was to be found in the cases related in the paper, in every one of which air entered freely during the time of the operation, yet did no unpleasant effects ensue. He had frequently found the pleural sac immediately afterwards filled with air, producing all the physical signs of pneumothorax, but that a few hours had sufficed for its removal by absorption. He had been surprised at the observation made by Dr. Williams, to the effect that air, admitted into the pleural cavity, was capable of exerting the same amount of pressure on the lung as the fluid previously contained. Experience completely disproved the correctness of the remark, and the observations that had just been made by Mr. Arnot and Mr. B. Philips strictly coincided with the results obtained by himself, that the air admitted during the operation had commonly been removed within a few hours. With regard to the fatal termination of the cases that had occurred under the observation of Dr. Williams, there could be little doubt that their unsuccessful issue

was not in any way dependent upon the mere admission of air into the chest, but that the cause was to be sought for in the period at which the operation had been had recourse to. It had long been the opinion of authors that paracentesis could only be regarded as a *dernier ressort*; and if the cases alluded to by Dr. Williams had been treated in accordance with such views, it was not remarkable that no recoveries should have taken place. In all cases in which the operation had been long deferred, the lung had become so much compressed and atrophied, and its investing pleura so thickened, contracted, and bound down by morbid adhesions, that its future expansion could not take place, and though the collection of fluid might be removed, the lung would be wholly lost as an organ of respiration. If, however, the practice he had advocated were adopted; if the operation were had recourse to at the only period at which it was really of value as a curative means; if it were employed to remove a disease rather than to protract it, very different and far more gratifying results would ensue. Dr. Williams had also stated that the majority of cases of hydrothorax were to be cured by tonics, and that, therefore, the operation of paracentesis was unnecessary. He (Dr. Roe) dissented not only from the assumed fact, but also from the inference deduced. He did not deny that many patients got rid of pleuritic effusions by the use of tonics, after depletory measures had been too actively employed, and that such persons did recover without the operation; but the fact was not less true, that a large number of patients died annually from hydrothorax, even after the employment of all those remedies commonly relied upon as useful in the treatment of this disease. In few, if any, of the twenty-four cases had the operation been employed until other remedies had been unavailingly had recourse to. In reply to the prophetic remark of Dr. Williams, that the operation of paracentesis would cease to be employed in the treatment of hydrothorax, he was fully convinced that it was only required to regard the operation in its true character, as a curative measure, to demonstrate the incorrectness of the opinions which had served to surround this operation by unreal dangers, and to shew, by the record of cases, that it could be almost an exception unsuccessfully (when early) employed. He believed that it would then meet with general adoption, and a very few cases would be met with in which it would be found inadequate to the complete removal of the disease. In the course of his paper, he had especially directed attention to the greater value of paracentesis as a remedial measure, inasmuch as, by its timely employment, not only was the effusion itself removed, but it also relieved the lung from the pressure exercised by the surrounding fluid, by which its proper structure was ultimately destroyed, and its subsequent expansion prevented.

“DR. P. M. STEWART, in relation to the combination of calomel with quinine, remarked that Dr. Mackenzie, of Glasgow, was in the habit of employing this mixture in cases of scrofulous diseases of the eye. The state of the constitution, in some cases of accumulation in the pleura, might be such as to render the medicine in question a very desirable and valuable one.

“ Mr. Stanley, had operated on five cases of fluid in the chest; of these, three were collections of pus, two of serum. Three ended favourably, two fatally. With reference to the point at which paracentesis might be most properly and safely performed, it had been asserted by high authorities that the puncture should not be made lower than between the fourth and fifth ribs; and Laennec had recorded two cases in which the puncture was as low down only as between the sixth and seventh ribs; in one of these the diaphragm was wounded, and in the other the trocar passed into the kidney, and the patient died from extravasation of blood into the abdomen. Notwithstanding these results, however, he (Mr. Stanley) had, in two of his cases, made the puncture between the eighth and ninth ribs; the proceeding was successful, and no bad consequences followed.”

We have not been able to procure the treatise of Dr. KRAUSE,* and must content ourselves with quoting from a notice of it in the *London and Edinburgh Monthly Journal*, and we are the more anxious to do so, as our cotemporary has extracted from the work strong confirmations of some views advanced by Dr. Mac Donnell, published in the March Number of this Journal.

“ Under the anatomical department,” says the Reviewer, “ he gives some details regarding the nature of the effused fluid, which he divides into three species, the purulent, sero-purulent, and sanguineous. It is difficult, he says, to state with accuracy the numerical proportions of these three varieties; but throwing together single cases, collected by other authors, along with those observed by himself, he finds the first to be of much the most frequent occurrence, and also the least dangerous, whilst, on the other hand, the sero-sanguineous exhibits the most unfavourable results; thus, in 162 cases the effusion was purulent in 100, and the cures were 50; sero-purulent in 41, the cures being 18; and sero-sanguineous in 20, with cures 2. In that part of the work devoted to the causes, we have another table, shewing the relative proportion of its occurrence on both sides of the chest, and also the relative proportion in both sexes, and the periods of life, a summary of which we subjoin. Of 137 cases, 96 were men, 18 women, and 23 children. In both sexes the disease occurred more frequently in the young. Of the 96 men, 51 had not attained the age of 30; of the 18 women, 10 were under the same age. The greatest number of cures occurred in the young. Of the 51 young men, 25 were cured, and of the 10 young women, 5. But the most favourable result of all occurred among children; of the 23 children, 16 were cured. Out of the 137 patients, 81 were affected in the left side, and 56 in the right. Of the 54 cures, on the other hand, 30 were in individuals affected in the left side, and 24 in those affected in the right.”

* Das Empyem und seine Heilung auf Medizinischen und Operativem Wege nach eigener Beobachtung dargestellt.—Von DR. ALBERT KRAUSE, Danzig.

“In the interesting Paper of Dr. Mac Donnell, which appeared in our [*London and Edinburgh Monthly Journal*] last Number, it will have been observed by many of our readers that the able author states, that enlargement of the liver very often accompanies empyema, not only when it is seated in the right side, but also when it occurs in the left cavity of the chest. This effect he ascribes to a ‘congestion or engorgement of the liver, analogous to what takes place in cases of morbus cordis, and diseases of the lungs, attended with imperfect aëration of the blood.’ He also remarks, ‘that it has been mentioned by many writers, in their accounts of the appearances noticed at the autopsies in cases of empyema, though unaware of its connexion with the subject.’ In the work before us Dr. Krause remarks, that disease of the liver is but a rare attendant upon empyema, as he has only witnessed it in two of the numerous cases he has seen; in one it was prodigiously enlarged, and studded over with tubercles; in the other case hydatids were found in the viscus. On looking over Dr. Krause’s cases, however, we find, that though in many he makes no mention of the state of the liver at all, merely concluding with the general remark, that the abdominal organs were healthy, yet in those in which he does notice its condition, we find that out of 14 cases where the effusion was in the *left* side, in 4 it is stated to have been congested, in 2 much congested, and in 3 large and congested; of the remaining 5 it is stated to have been hard in 2 cases, large and hard in 1, and softened in 1. In those in which the effusion occurred in the *right* side, out of 6 cases, it was large and congested in 2, much congested in 1, large but bloodless in 1, small in 1, and sound in 1.”

So that out of *twenty* cases of empyema (fourteen of the left and six of the right side) the liver was congested in different degrees in twelve cases.

From the fact of this congestion of the liver having been so frequently observed after death, there can be no doubt that its presence would have been detected in many of the successful cases, had the attention of Dr. Krause been directed to the subject; but as it is, they fully corroborate the statements advanced by Dr. Mac Donnell, and “this condition of the liver must now be considered as constituting an additional feature in the diagnosis and pathology of empyema.” Dr. Krause gives an interesting case of encephaloid disease of the lung, which was mistaken for empyema, which we shall lay before our readers in his own words:

“Nisten Nadolski, aged 40, of a slender make, was admitted into hospital on the 27th February. Up to that time she had always enjoyed good health; had never suffered from chest complaints; and was seized with pneumonia of the right side, which ran its usual course. At the end of four weeks the patient was, by her own urgent desire, dismissed, and although she had still some difficulty of breathing, with purulent sputa, and there was still dulness on percussion at the base of the right side, and want of respiratory murmur. She continued to

improve at home, the cough disappeared, and she gained strength. At the end of four weeks, difficulty of breathing again occurred, the expectoration returned, she became hoarse, and complained of pain during respiration. She was again admitted on the 27th May. There was now considerable emaciation, and pretty smart fever. The respiration was hurried, and the right side a little enlarged. A firm tumour was observed under the right arm-pit, but somewhat anterior, which rested on the ribs, had a natural colour, and was very slightly painful. A vein about the size of a quill spread over it, running down from the arm-pit: the patient had first remarked it fourteen days previously. The glands in the axilla were free from swelling; respiration was heard at the summit and base of the lung, but was wholly absent at the middle portion; percussion was dull over the whole right side, and normal only under the clavicles; the expectoration was considerable, of a dirty green, with a penetrating odour. The state of the patient much the same for some days; she lay continually on her back, and slept little during the night. The tumour continued to increase in size, became livid, and fluctuation was felt. It was opened by means of a bistoury. No pus, however, appeared, but a bloody-coloured serum, and the incision was immediately filled with a grumous, homogeneous mass. There was considerable hæmorrhage from the wound, but this was controlled by cold applications; the strength continued to sink, and she died on the 6th of June.

“ When the skin was dissected off the tumour, an encephaloid mass of the size of the fist was remarked; it lay upon the third and fourth ribs, and was in part covered by the pectoralis major and serratus magnus. It could not be separated from the ribs, as it protruded through the intercostal spaces, and had its root within the thorax. On the sternum being removed, a large mass was perceived, filling almost the entire of the right side of the chest. After being separated from its attachment to the parietes, it was removed; on an incision being made into the lung, the middle portion of it was found to be completely supplanted by an encephaloid mass, which had the consistence as well as the appearance of brain, and was traversed here and there by fasciculæ of vessels. The apex of the lung for about the breadth of a hand was in good condition, but compressed. Several small tumours, varying in size from a nut to that of a hen's egg, were situated in the lower lobes; some rested on the ribs, and on removing them, the former were found to be deprived of their periosteum, but not carious; the cellular texture of the bone was sound. There were no traces of similar tumours in any of the other cavities. The left lung was somewhat cedematous, but otherwise healthy, as were also the heart and liver. The kidneys and the intestines shewed nothing abnormal. The glands of the chest were removed along with the tumour, and could not be found. Those of the neck and opposite side were quite unchanged.”

We regret we have not room to insert two cases of empyema, in one of which the matter escaped through a hole in the dia-

presented the appearance of a psoas abscess, and was opened. The boy, however, soon died, and then the true nature of the disease was discovered.

In the second case the matter perforated the diaphragm, and burst into the transverse portion of the colon by two apertures, through which faeces passed upwards into the right pleural cavity, which had been the seat of the disease.

“When an operation becomes necessary, Dr. Krause is decidedly of opinion, that an early period of the disease is not the best, but that it is much more likely to prove beneficial after the tendency to inflammation is somewhat diminished.”

These views are (as our readers may perceive) in direct variance with those of Dr. Hamilton Roe, the success of whose operations affords, in our mind, strong evidence of the soundness of his opinions.

We shall conclude this notice with an extract from one of the Clinical Lectures of Schönlein, published in the *Lancet* for April 28th, 1844.

“I read a few days ago,” he says, ‘in the Austrian Medical Annals (Oesterreichische Medicinische, Jahrbücher, 1841, Januar. Juli), Skoda’s report on the cases of empyema that were treated in his ward for diseases of the chest, and must confess that the facts he makes known, with characteristic openness and sincerity, for he frequently performed the operation, do not tempt us to follow his example. Almost two-thirds of the cases he treated ended unfortunately, for the operation was very soon followed by symptoms of returning pleurisy. Here, in this case, where we are menaced with a complication of delirium tremens, the indications with regard to the treatment are charged, but I confess that the matter is not quite clear to me, and that I am not inclined to come to a precipitate decision, for the following reasons; first, because the apparition of delirium tremens merely threatens as yet, and has not actually taken place: secondly, because, if delirium tremens should really supervene, the operation might have a still worse result, for we know that wounds during the existence of this malady very easily assume a putrid character, and that every operation has an inflammable reaction upon the brain.

* * * “In the account of Skoda and Schuh, which I have cited, a form of effusion into the cavities of the pleura is mentioned, which cannot be distinguished during life, at least as yet, but which, when the operation has been undertaken, not only makes it extremely difficult, but inevitably leads to a fatal termination, and furnishes another reason why I must declare myself an opponent to the operation; this is the case where coagulable lymph swims in the effused liquid. I, for the first time, saw such a case in Würzburg, in an elderly man, on whom Textor, at my request, undertook the operation. After more than a hundred of these flocci had already been evacuated, the flow of the liquid was suddenly stopped. Textor examined the wound with the finger, and thought that it had pene-

trated into the pericardium, on feeling a mass similar to the *cor villosum*, but it was only a convolution of plastic flocci, which, on being pushed back, allowed free egress to the liquid. In consequence of the irritation produced by the remaining flocci on the pleura, violent inflammation was brought on, which terminated fatally. Skoda and Schuh have also cited several similar cases, and undertaken the operation when there was no urgent necessity from the violence of the symptoms; their termination was unfavourable, and on dissection the cavity of the pleura was found filled with coagulable lymph. As yet we have no means of distinguishing the quality of effused liquid, in each individual case during life; it is only to be found out by autopsy. We have, therefore, another reason for not undertaking the operation precipitately. 'But,' it might be objected, 'by stimulating the secretions the flocci which remain after the more fluid part has been absorbed, will produce the same mechanical irritation as after paracentesis.' To this I answer, that experience, it is true, can prove nothing in this question, but that analogy and induction furnish sufficient grounds for invalidating this objection. After paracentesis the emptying of the sac is rapid, and the coagula of albumen and fibrine come into immediate contact with the surface of the pleura, after the evacuation of the water; but we know that the more rapid contact of a surface with a foreign body is, the more violent the reaction becomes, and again, that when it takes place slowly, the reaction proportionally decreases, and sometimes even does not occur at all, or in other words, the irritation gradually wears itself away; this is a universal pathological truth. But we have still another answer to this objection, for in the same way that coagula of blood (for instance, in apoplexy of the brain) are gradually absorbed by the surrounding fluid, so the absorption of the coagula of albumen and fibrine can also gradually take place in this case, and this is a further reason for preferring, to immediate evacuation, the slower process of absorption, by stimulating the secretions."

We have now laid before our readers an analysis of all that has been written within the last few months on this interesting subject. Dr. Hughes and Mr. Cock are strong advocates for the performance of the operation, but do not confine it to any particular stage of the disease. Dr. Williams and Schönlein are opposed to it, but their objections appear only applicable to the advanced stages of empyema. Dr. Hamilton Roe operates early in the disease, and with marked success; and though Skoda, of Vienna, has been cited as averse to paracentesis in *Empyema*, we know that he advocates the operation in the early stage of inflammatory hydrothorax, for we ourselves have seen him perform it on a patient who had been admitted into his wards only a few hours previous to the clinical visit, and who had been but a couple of days ill. In this instance he resorted to paracentesis immediately he had satisfied himself of