calculated to afford temporary relief. I believe, however, that the success of the operation depends very considerably upon its early performance; and I am satisfied that many simple cases of pleuritic accumulation have been converted into very formidable ones by long continued irritation and pressure. With the hope of rendering this operation some what less formidable, I have had an instrument made (see illustration) that provides against the entrance of air by means of a tap, and that will enable the operator to draw off the fluid gradually, at intervals, and by a very small wound.

"The more slowly the fluid is evacuated, the less chance there will be of the admission of air, and of the renewed effusion of fluid." (*Physician's Vade Mecum*, 7th edit., page 543.) It is adapted for the evacuation of serum, and especially for those cases of true empyema in which, by removing a portion of pus, the remainder is disposed of by absorption; for, notwithstanding the smallness of the cannula, pus can readily be drawn through it by means of the syringe that screws into it. By the way, an interesting case occurred at the Westminster Hospital some years ago, in which there was manifest bulging with increased measurement of the chest, yet absorption took place, and the patient recovered, after the evacuation of an ounce of pus.

For the following reasons I consider that, whenever absorbent treatment seems steadily resisted, the operation should be at once performed.

1. The effusion, when thin and small in quantity, can be evacuated by a very small cannula.

2. Provided adhesions do not exist, or exist to only a small extent, or perhaps laterally, the success may be considerable and the danger slight, in proof of which I may mention that there is a case recorded of a man who, in one of the London hospitals, was tapped five or six times, and in the intervals pursued arduous labour.

3. Cases are often met with of true empyema in persons of a scrofulous habit, or who have been debilitated by mercury or previous disease, and are not likely to be benefited by delay.

4. Patients bear the operation better in the earlier stages of the disease.

In conclusion, I may remark that, after examining various recorded cases, I am satisfied that the success of paracentesis thoracis, though not hitherto as great as could have been wished, is much greater than is generally supposed; and, while I confess to having met with cases that ought to have succeeded but did not, I am happy to add that I have met with several others that must have surpassed all expectation.

"And that which is can never not have been;
Facts are solid as the pyramids."

For some recent cases and interesting observations on this subject, see *BRITISH MEDICAL JOURNAL* for 1866, vol. i, pages 95 and 644, and vol. ii, page 671; for 1867, vol. i, page 535, and vol. ii, page 80; also *Lancet* for 1866, vol. ii, page 693, and for 1867, vol. i, page 599.

The operation is performed in the usual way, as proposed by Trousseau, and the instrument is made in various sizes.

Fig. 4.—Trocar, cannula, and tap, with syringe screwed into cannula.

EMBOLISM OF THE MESENTERIC ARTERIES.—The symptoms, Dr. Moos says, are: 1. The manifestation of some source of embolism, as endocarditis, etc. 2. The occurrence of profuse, and frequently exhaustive hæmorrhage from the intestines, which may be explained either by active lesion of the intestinal mucous membrane itself, or by an obstruction in the portal circulation. 3. Rapid and considerable lowering of the temperature of the body. 4. Abdominal pains, which may be very severe and similar in character to those of colic. 5. The abdomen becomes tense and tympanitic, and symptoms of peritoneal effusion are presented. 6. Symptoms of embolism in other arteries frequently precede or come on at the same time with the special symptoms just described. 7. The existence of large blood cysts between the layers of the mesenteric may sometimes be made by external examination of the abdomen.—*Virchow's Archiv*, bd. 41, 1867.

ON DRUGS: THEIR ACTIONS AND THEIR DOSES.*

By HENRY WM. FULLER, M.D. Cantab., F.R.C.P.,

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IN proposing "Drugs: their Actions and their Doses", as the subject for discussion this evening, I had three objects principally in view: the first, to trace the causes of the distrust of medicine so prevalent in the present day; the second, to call attention to our utter want of precise knowledge respecting the action of drugs, and of the doses in which they may be safely administered, and to the wide differences in practice consequent thereupon; the third, to suggest that the Harveian Society should appoint a committee to undertake the investigation of therapeutical subjects.

Scepticism as to the curative value of drugs is the besetting fault of the profession at the present time. It pervades all classes; but is most common amongst those whose mental training ought to have preserved them from being so ensnared. In part, doubtless, it has arisen as a natural reaction from the blind and overweening confidence in drugs which obtained at the commencement of the present century; but this alone will not explain the change which has taken place. Brandy and beef-tea, as all-efficient remedies in pneumonia; and mint-water, as a specific for acute rheumatism, are simply exponents of a wide-spread superstition to which many causes have contributed. The rapid strides which have been made of late years in pathology, and in our means of detecting and diagnosing disease in the living body, have tended greatly towards this unfortunate result. Carried away by his enthusiasm in tracing the ravages of disease, the student has too often neglected to investigate the indications for treatment, or to pay much heed to remedies. Medicines, consequently, are prescribed which, either in their nature or mode of administration, are ill adapted to the exigencies of the case; failure necessarily follows their use, and scepticism as to their curative value is the logical result.

In another class of minds, the same distrust is brought about by other means. Our knowledge on many subjects is so precise, that a similar precision is sought for and expected in regard to the operation of medicine; consequently, each remedy is administered alone, and definite effects invariably follow its administration; and where any uncertainty or failure in this respect is observed, the conclusion is drawn that the remedy is not trustworthy, and, if useless, is probably mischievous. Constitutional peculiarities are ignored; the necessity for regulating the secretions and for strictly adapting the dose of the medicine to the exigencies of the case is overlooked; and the bald fact that the remedy cannot be trusted to produce a definite effect, under all circumstances, however dissimilar, is cited as conclusive evidence, that it possesses little virtue as a curative agent.

Again, we know little respecting the natural course of many diseases, and consequently we are in the dark as to what may be effected by the *vis medicatrix nature*: but observing, as we often do, that patients recover under various, and the most opposite, modes of treatment, and even when no medicine is administered, we are naturally inclined to ask whether drugs are of any use, and whether they may not be harmful? The question is a philosophical one, and deserves attention; but it is the reverse of philosophy to return an answer without careful investigation, and to ignore or endeavour to explain away all facts which serve to show that remedies are useful. It does not follow, because an object can be attained by different means, that therefore those means did not contribute to its attainment. Modern observation is directly opposed to any such supposition. Who can doubt that, when the nutrition of any part is interfered with, and so-called inflammation is set up, the morbid action may be modified or controlled by measures apparently the most opposite? Nay, more; who would hesitate to affirm that it is their very contrariety of action which enables remedies manifestly so dissimilar to compass the same object? Local depletion, for instance, by unloading the vessels, oftentimes tends to the restoration of the circulation; heat and cold, stimulants and sedatives—each in their turn, and under various circumstances, contribute to the same result. And, if this be admitted, what ground is there for doubting the efficacy of a remedy simply because another drug or another mode of treatment, most opposite in its nature, apparently effects the same object? What are we to think of the contest which has so fiercely raged between the respective advocates of the purgative and the astringent treatments of cholera—each side straining facts to suit their own views, and each unwilling to admit even a modicum of truth in the statements of their opponents? For my own part, I can readily conceive that each method

* Read before the Harveian Society, February 6th, 1868.

of treatment may at times be serviceable; that the plan which proves most efficacious when the cholera-germ has found its way into the alimentary canal, may be less so when the poison has found entrance into the system through the pulmonary mucous surface; and I should be disposed to urge a careful and discriminating inquiry into the class of cases in which each method of treatment proves most efficacious, rather than, as is often done, decry the efficacy of all remedies and all methods of treatment, simply because, though differing utterly in their nature, they are nevertheless followed by similar results when adopted indiscriminately in all cases.

Again, it often happens that drugs are given for the relief of disease over which they exercise little or no influence; or appropriate drugs are given when the system is in a condition ill fitted for their reception; or they are given in doses inadequate to restrain the violence of the attack. In either, or in all these cases, they necessarily fail to exercise a corrective influence; and the physician, ignoring his own want of judgment and practical skill, too often charges his failure on the remedies. But experience, no less than sound common sense, would demur to the correctness of his conclusions. Of what use is quinine in ague when given in minute doses, while the bowels are torpid, and the liver or other internal organs congested? Of what service is colchicum in a paroxysm of gout when the kidneys have undergone granular degeneration, and the patient is pallid, weakly, and exhausted? Of what benefit are alkalies in the treatment of acute rheumatism, when administered in doses which fail to render the urine alkaline? Absolutely none. And yet it will scarcely be contended that, when properly handled, these drugs do not exert an extraordinary influence over the disorders for which they are respectively administered.

Another source of the prevalent distrust of drugs, is the faulty information respecting their operation derived from experiments on animals. To take mercury as an illustration; we are gravely told that calomel does not exercise any influence over the secretion of the liver or over the absorbents; that the fact has been ascertained by careful experiments which do not admit of doubt; that the drug has been administered to rabbits, dogs, and other animals, and that the result of an examination of their viscera, while the drug was in operation, has proved the absence of its reputed influence. Let anybody who is bilious take a dose of calomel, and then speak his convictions as to the value of such experiments. They may, and unfortunately they do, serve to mislead many persons who have a hankering after precise knowledge in regard to the operation of medicines, and are willing to shut their eyes to all clinical experience which tells against their supposed facts. But, except in corroboration of knowledge gleaned at the bedside of the patient, such experiments are simply valueless, and must necessarily tend to create erroneous impressions respecting the value and action of drugs. The same may be said of similar experiments on man in a state of health. They do not afford trustworthy evidence as to the action of remedies in disease. Who could judge from the influence of quinine on a healthy person, that it would possess the power of curing ague? who would imagine from the operation of colchicum, that it would serve to check a paroxysm of gout? and who would conjecture that iodide of potassium would remove syphilitic nodes? Yet to many persons the absence of any physiological action has proved a source of doubt and perplexity, and has led not a few to disbelieve the evidence of their senses, and to mistrust the curative influence of remedies.

The great stress which has been laid on the value of inductive reasoning in the study of our profession, has proved another and a very common stumbling-block. In the study of chemistry, and many of the allied sciences, the system of induction, strictly carried out, has led to eminently satisfactory results. But it is otherwise in respect to therapeutics, the highest and most important of our studies—the end and object of our professional existence. Empiricism is here the very keystone of success—a cautious and well considered empiricism, but an empiricism nevertheless. There is no room here for inductive reasoning, except on a very limited scale. Given any drug which the *Pharmacopœia* contains, and no inductive reasoning could ascertain its action. By no other means than by mere empiricism—by carefully conducted experiment—is it possible to discover the fact that quinine will check ague; that colocynth will stimulate the action of the bowels; and that the inhalation of chloroform will confer immunity from pain. View the question as we will, we cannot avoid the admission that our knowledge of the action of drugs can be enlarged by experiment only; and the sooner the fact is recognised, and acted upon, the better will it be for us and for our patients. It affords no ground for a distrust in drugs, but it necessitates a careful investigation into their action. The reports of cases in our weekly journals show how seldom this is attempted, and how little its importance is understood. Our periodical literature teems with the record of cases, which are almost wearisome from the minuteness with which their details are worked out, while the all-important question of treatment is dismissed in half a dozen

words. After a column of close print, devoted to the history, and diagnosis of the case, we are summarily informed, after the manner of Dr. Hunter of consumption-cure notoriety, that “the usual treatment was adopted,” or “the quinine treatment was employed,” or “the alkaline method was had recourse to”; or, if a different set of words is made use of, we are told that “colchicum was given,” or “mercury was employed,” or “tartar emetic was administered”; as if it mattered not what dose of any remedy was given, nor how often it was repeated, nor whether it was deemed necessary to assist its action by stimulants, or in any way to regulate the secretions. Seldom is any mention made of the effect produced by the remedies, or of the rules laid down respecting diet, exercise, and the like; but we are simply told that “the patient gradually improved”, or, possibly, that “he sank”, in which case we are favoured with the minutest details of the *post mortem* examination.

Now, in all soberness and earnestness of purpose, I will venture to suggest that, when the action of drugs is so completely ignored, it is not surprising that the public should lose faith in our ability to relieve their sufferings, and, declining to take the medicine in which they see that we have no confidence, should betake themselves, as an alternative, to the homœopath. It is impossible to read most of the published reports of cases, without feeling a conviction that the reporter had no belief in the efficacy of medicine, and indeed had two objects only in view; viz., firstly, to prove himself apt at diagnosis, and, secondly, to fortify his diagnosis by the result of *post mortem* examination. No one whose aim was to add to our stock of knowledge respecting the means for the relief of human suffering, could so ignore the question of treatment. Strongly as I would denounce the indiscriminate administration of medicine where no indications for treatment exist—a practice which even now is far too prevalent—and greatly as I rejoice at the abandonment of a system which rendered drug-giving a necessity to the very existence of a large section of our profession, I would emphatically protest against the monstrous doctrine, which, though not openly avowed, is yet tacitly adopted and carried into practice by many members of our profession, viz., that drugs can do little for the cure of disease. The practical result of this modern scepticism is a widely spread homœopathy under the guise of scientific medicine. A few cases, which have come within my knowledge, will serve to illustrate this significant fact. An unfortunate patient afflicted with ague was treated with grain-doses of quinine, until, after many weeks of weary suffering, he applied to a physician who had more confidence in remedies, and got rid of his ailment in a few days under the use of five-grain doses of the same alkaloid, aided by the action of aperients. A sufferer from secondary syphilitic eruption was treated for nearly three months with two-grain doses of iodide of potassium three times a-day, until, frightened by the progress of the malady, he applied to a less sceptical or more practical physician, and was speedily cured by fifteen-grain doses of the same drug. A patient was seized with rheumatic fever, and was ordered fifteen grains of carbonate of potash in a state of effervescence every four hours, and was thus allowed to linger on until he became a victim to inflammation of the heart, which such doses are utterly inadequate to prevent. If this be scientific treatment, commend me to the quacks; but if, as I believe, it is simply quackery under the guise of scientific medicine, the sooner we purge ourselves of the reproach the better. If we have not sufficient data for an opinion as to the value of remedies, let us not ignore the question of treatment, but apply ourselves to obtain the information we require. Let us endeavour, as far as possible, to ascertain the natural course of each disease, and then note and carefully record the effect of remedies administered in definite doses, and in given combinations, and in conjunction with certain methods of diet. The result, I venture to predict, will be an increase of confidence in the means at our disposal for the relief of human suffering; but if not, let us openly avow our mistake, and admit like honest men that drugs are of no avail as curative agents, and that the homœopaths have just grounds for their vilifications of the so-called legitimate treatment of disease.

This leads me, sir, to suggest that this Society, which has already done good service to the profession and the public, by appointing committees to investigate other questions of general interest, should nominate a committee to investigate various questions connected with therapeutics. The newly formed Clinical Society at present shows no disposition to do so; and it is a subject in which the Harveian Society may well take the lead. I do not mean to imply that any committee, however constituted, can investigate and arrive at a definite conclusion on all questions relating to therapeutics, for such an investigation would be of infinite extent, and must necessarily be coeval with the existence of our profession. But at present we are absolutely without data for the administration of medicine; the profession professes no authoritative record of a single therapeutic fact; the traditions of our forefathers are handed down to us orally or in the works of individuals; and we have these, together with our own experience to guide us—but nothing more. There are no public records—no authoritative statements as to the influence of any method of

treatment, or as to the action of any remedy in the *Pharmacopæia*; and neither the College of Physicians, nor any of our medical societies, has made the slightest effort to obtain such a record. Here, then, is an opportunity for the Harveian Society to step in, and do a real service to the profession. The committee might test at the bedside the accuracy of the statements made by individuals respecting the so-called natural course of disease, and endeavour to ascertain, and impartially record, the amount of assistance which may fairly be expected from the *vis medicatrix nature*. They might investigate the action of certain drugs, the doses in which they might be safely administered, and the conditions which appear to modify their action; they might note the action of given remedies, in definite doses, or in certain combinations, in modifying the course of certain disorders, and might verify the various statements which have been made respecting their remedial powers. Observations of this kind, even though limited to a few drugs and very few diseases, if carefully made and briefly recorded, would serve as a model for further investigations, and as a basis for a work, which within a few years, would be considered as needful to the medical practitioner as the *Pharmacopæia* is at the present time.

In another manner, too, I believe, a therapeutical committee would prove most useful. They would collect information about, and bring to the test of bedside experience, the reputed action of remedies not commonly employed, and the application of well known remedies, or unusual doses of remedies, to the relief of disorders for which they are not commonly prescribed. There are numerous practitioners throughout the country, who have acquired a great local reputation for the cure of certain forms of complaint; and it has come to my knowledge in several instances, that their reputation has been well deserved, and has been gained by the employment of remedies not commonly in use for the cure of those disorders, or by the administration of combinations or doses of medicines which are not usually prescribed. This sort of information rarely finds its way into print, or, if it does, attracts little attention. It is handed down from father to son, or from master to apprentice, and so, perhaps, ultimately becomes known to the profession; but much of it is necessarily lost, and will be so to a far greater degree now that the system of preliminary education is being changed. This information the committee might readily collect and submit to the proof of bedside experience.

So again, with regard to the doses of medicines and to their administration for the relief of certain disorders: no person who has not made inquiries on the subject can have an idea of the wide discrepancy which exists, between the practice of different medical men, in relation to the doses in which remedies are administered. Ignorance respecting drugs and their actions is so general, that in the absence of any authoritative statement on the subject, the vaguest and most groundless fears are entertained respecting their action; one physician, for instance, is in the habit of prescribing forty-minim or drachm doses of tincture of cantharides, but never ventures on more than ten or fifteen-minim doses of tincture of digitalis; whilst another, who habitually orders thirty or forty minims of tincture of digitalis, and in certain instances gives drachm or drachm-and-a-half doses, is not aware that tincture of cantharides is ever given in doses exceeding twenty minims, and himself has never prescribed more than ten or fifteen minims. And so with regard to numberless other remedies. We have no public or authoritative statement respecting the doses in which any medicine can be given with impunity, nor respecting the disorders in which it proves useful; and therefore most practitioners do as they have been taught, and oftentimes have no knowledge as to its administration in any other doses. But if the smaller doses suffice to produce the characteristic effect of the medicine, it can scarcely be doubted that the larger doses must be pernicious; whereas, on the contrary, if the larger doses are really needed, the smaller doses must obviously be inefficient. In either case the patient suffers: in the one, from being unnecessarily drugged; in the other, from losing valuable time, at the risk, perhaps, of his life, in taking doses which are inadequate to relieve his suffering. This extreme contrariety of practice, which is a standing reproach to our profession, might be removed through the investigation of a committee, and the establishment of something like a definite knowledge respecting the doses of medicines. The Harveian Society would thus obtain the honour of having inaugurated an inquiry which would not have been unworthy of the Royal College of Physicians.

MISCONDUCT OF ALLEGED MEDICAL STUDENTS.—At the Middlesex Sessions, lately, George Richmond, John Hind, Robert Foss, and Alfred Swinborne, medical students, pleaded "Guilty" to assaulting Police-constable Cave, 35 C, and Allwood, 186 C, in the execution of their duty. Mr. Cooper prosecuted; Mr. F. H. Lewis defended. The Assistant-Judge said he should require time to consider what course ought to be taken, and postponed sentence until next sessions.

RESEARCHES ON THE ACTION OF THE HEART.*

By GEORGE PATON, M.D., Bowmanville, Canada West.

If the continued action and varied degrees of rapidity with which the ventricles dilate cannot be accounted for by mere muscular relaxation considered by itself, we might argue that, as the dilatation of the ventricles takes place at the same time that the auricles contract and project their blood into the ventricles, it must either depend on some inherent power possessed by the ventricles, or it must be produced by the impetus imparted by the blood from the auricles to the relaxing parietes of the ventricles, causing them more rapidly to expand and reach the point of distension, when contraction is produced.

The great difficulty which has presented itself to physiologists in investigating this subject is that, during the vigorous action of the heart, the diastole of the ventricles appears to take place with an energy that cannot be accounted for by the mere relaxation of the fibres—by their returning to a state of rest; whilst at the same time the auricles are considered the reservoirs or receptacles into which the blood enters on its return from the system, and before it is projected into the dilating ventricles, it being believed that the auricles contract but slightly. Hence Pechlin, Perrault, Bichat, Laennec, Carson, Cruveilhier, and several other physiologists who have examined the action of the heart in living animals whilst respiration was well maintained, observing the vigour with which the diastole is performed, considered that it depends on an actively dilating power possessed by the ventricles; and it undoubtedly bears the strongest resemblance to a movement of this character. In a paper which we published in the *Dublin Quarterly Medical Journal* for August 1866, we stated that the force with which the ventricles dilate affords every indication of a vital active movement; and that it must either depend on some inherent power possessed by the ventricles, or be produced by the force with which the blood is projected by the auricles into the ventricles, stimulating the parietes, and causing their fibres to extend and complete the dilatation. And, on more fully investigating this subject, we incline to the latter opinion. The whole of the auricles, it is to be observed, contract at this period, and not the mere appendices; and they contract with a force that exerts an influence on the dilating parietes of the ventricles. We have frequently seen, as formerly stated, the auricles contract with a power that caused the blood to open up the walls of the ventricles, the fibres distinctly relaxing till distension was produced and contraction commenced; and Dr. John Reid states that the systole of the auricles is performed with great rapidity when the action of the heart is still vigorous, and appears to be effected by the simultaneous contraction of all its fibres. According to the observations of Harvey, Lower, Senac, Haller, and others, the contractions of the auricles are performed with considerable force. In the experiments of Dr. Hope, Mr. Carlisle, M. Bouillaud, and the Dublin Committee for Investigating the Cause of the Sounds of the Heart, the contraction of the auricles appeared to be comparatively trifling, and was most apparent in the appendices. From my own experiments upon rabbits and dogs, I am convinced that the auricles contract considerably more when the movements of the heart are proceeding in a natural manner, than some of these last experiments would lead us to believe; and that this contraction is not confined to the appendix, but extends over the whole auricle. That, however, none of the muscular fibres of the auricles are passive, but exert a force proportionate to their strength, we have evidence both from experiment and from the effects of disease.

And, if we examine the action of the heart when it has been exposed in large warm-blooded animals, we perceive the auricles contract with vigour; and, as they project the blood into the ventricles, the ventricles expand with a force sufficient to separate the fingers that seize them. We denuded the heart of a calf after the animal had been rendered insensible by a blow upon the head, and artificial respiration was maintained. The heart beat at 62 pulsations per minute; and it was surprising with what energy the action was maintained. The contraction of the auricles was synchronous with the diastole of the ventricles, and the whole of the auricles contracted, and not the mere appendices. The diastole of the ventricles was performed with a force that opened up the hand grasping the parietes; and it appeared to be produced by the impetus of the blood from the auricles; for the parietes of the ventricles could be distinctly seen and felt gradually to expand, and with a proportionate vigour, as the blood from the auricle shot to the apex. The ventricles first dilated, and then contracted. During the diastole, the anterior parietes became slightly protuberant: during the systole, they

* Read before the Physiological Section at the Annual Meeting of the British Medical Association in Dublin, August 1867. [Concluded from p. 164.]