lead among their molecular constituents, and partly by the loss of nervous power, in the way mentioned by Heubel, an uncertain intermittent contraction of the enfeebled fibres will in all probability replace that "sustained state of gentle contraction" which produces their "tone" in health, and consequently a general tremor of the affected muscle will quickly ensue.

MECHANISM OF EXPULSION OF THE PLACENTA.*

By WILLIAM LYON, A.M., M.R.C.S.Eng., Peter-Culter, by Aberdeen.

HAVING had my attention directed to the natural mechanism of the expulsion of the placenta by reading Dr. Matthews Duncan's paper, I was anxious to know what could be learned of the subject from books. I was both disappointed and astonished at the unsatisfactory results of my search, and to find that the subject was not even alluded to in the directions given by midwifery authors for the management of the placenta. I have been long satisfied that Nature observes a particular form in the expulsion of the placenta, from careful observation of the manner in which she conducts the ordinary process.

In most cases of detached placenta, where undue interference has not been practised, we find, on examination, that its lower part is lying in the cavity of the pelvis, and its upper more or less above the brim. When the uterus contracts, the placenta is folded on itself, with sometimes the uterine and sometimes the foetal surface in contact, and the folds always longitudinal or in the direction of the length of the passages. The doubled mass is then expelled with its two edges foremost, and the part which first passes the os vaginae is the point where the two edges meet in forming the double.

I have so often observed this process, that I believe that every placenta, when separated from the uterus, will be expelled in this form, provided that Nature is allowed to work alone, and no pretended assistance is given by pulling the cord or otherwise. We cannot help admiring the mechanism displayed in this process as much as we admire that of natural labour; and we must at the same time be convinced of the propriety of withholding all interference, except what may contribute to uterine contraction—the only admissible moving power. The adaptation of the mass to the shape of the passages, and its consequent facility of egress, will be more apparent if we contrast the case with one in which the protrusion is effected by pulling the cord. Under such circumstances, the placenta is brought through the passages in the form most certain to offer resistance, and is protruded like a plug. The irritation caused by this treatment may raise a pain, which may assist in its expulsion; but it is remarkable how much force is often required to effect that purpose, if the placenta be of large size. Cases may occur, however, which may seem to contradict my statement. We may sometimes feel the placenta advancing in the form of a smooth ball, by purely uterine effort; and may be unable to detect the edge of the mass; but, on particular examination, we find that the insertion of the cord is up in the cavity of the uterus, and that the membranes are still adhering to the edge of the placenta. The situation of the insertion of the cord proves that the presenting part must be the edge of the placenta; and, if we wish for further proof, it will be obtained by tearing the membranes with our finger-nail, and converting the case into an ordinary natural one. If the expulsion take place before we can accomplish that, as is sometimes the case when the membranes are strong, we can take the most prominent part between our finger and thumb, and retain our hold till it is completely protruded, when we will be quite sure. We find also, in cases in which the whole mass is brought into the pelvis by traction of the cord, that it will be expelled, if left to uterine contraction, in the regular way, and not otherwise; whereas, if the traction be continued till the completion of the process, the part which is first protruded is that into which the cord is inserted. Under the same circumstances, also, if it be necessary to help the pains, we effect our purpose most easily by reaching the edge of the placenta with our finger, and using a little pressure on it, so as to form a natural folding. It will then be in the most favourable position for being expelled, whether it be left to the action of the uterus, or assisted by pulling the cord.

We must look on this subject as something more than mere matter of curiosity, because it affects the management of the placenta—a very important part of labour. The temptation to pull the cord, both when there are no pains and when they are not sufficiently strong, is the cause of much rough treatment; and such conduct can only be defended

on the ground that it tends to ensure contraction of the uterus. But surely such a state is more certain and less painful when induced by the natural effort, or by manipulation of the uterus in the abdomen.

ON ANÆSTHETICS.

By WILLIAM ALEXANDER, M.D., F.R.C.P.,
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THE general subject of anæsthetics, to which the JOURNAL prominently calls the special attention of the profession at this moment, is second to none other in its importance and practical value; and, having had frequent opportunities of witnessing the relative merits of nearly all these agents, and having introduced the hydrate of chloral from Berlin, I have to offer a few reflections as my mite of contribution to the appeal of the editor.

During 1847 and the two or three following years, we used ether inhalation at the Infirmary here, and privately pretty generally, without any ill result. We found it very apt to occasion cough and a sense of suffocation, which might probably have been obviated had the patient previously practised with the mouth-piece, and had an anhydrous preparation been employed. Total oblivion was with difficulty obtained, and the cerebro-spinal reflex action never utterly lost. The volatile liquid used had about 85 per cent. of ether, with a gravity of 0.750, and was given in half-ounce quantities every two or three minutes until the desired effect was induced. Its stimulating, intoxicating property was often very conspicuous, as evinced by very profane ejaculations. As with chloroform, I look to the respiration rather than the pulse, and see that the heaving of the chest is properly performed, voluntary suspension of that function being commonly attempted whilst consciousness remains. No doubt the pulse must be carefully watched, since paralysis of the cardiac ganglia and pneumogastric nerve is unquestionably the customary cause of death.

For etherisation, Morgan's inhaler, used at Mercer's Hospital, would probably answer better than that of my late friend, Dr. Snow, as it altogether obviates the dilution of the vapour by atmospheric air, and thus earlier secures the object desired; but the conical towel and sponge, or even folds of lint, supply a ready method and answer the purpose. An empty stomach is desirable, though after-sickness very rarely follows, and the somnolent effect soon vanishes.

So far as is yet ascertained, the percentage of accidents is very greatly in favour of ether. Dr. Jones, who was a great authority on this matter at St. George's Hospital, feels assured that, efficiently administered, ether will soon supplant chloroform as the anæsthetic in general use. It should not be forgotten, however, that casualties have always occurred occasionally, and are incident to operative manipulation arising either from shock or syncope, or from idiosyncrasy of constitution. "If a sufficient amount of chloroform be given to produce narcotism," Dr. Jones observes, "there is sufficient in the system to cause death in these cases"; and he illustrates the axiom by reference to a case in which but five per cent. of it was inhaled—the amount prescribed as being within the limit of safety—and yet the patient died suddenly without any warning, by lividity of countenance or other symptom. Unlike such instances, the signals of danger usually come on gradually, and may, fortunately, be arrested. When full narcotism is accomplished and consciousness abolished by chloroform, it seems very desirable, if the effect be desired to be kept up, to change the anæsthetic to ether or nitrous oxide. Mr. S. Charles Smith of this town informs me that this practice is already adopted at most of the London ophthalmic hospitals.

In the selection of one of the two great agents, I should be much governed by the precise nature of the case; as, for example, traumatic injury with hæmorrhage, or a morbid condition of the heart or other viscera. Wherever alcohol would be indicated I should give the preference to ether, its special action being stimulant. Complete stupor by the protoxide of nitrogen is difficult and ephemeral, though safe, and is accompanied by a great mitigation of local pain under the knife. It is rather a curious circumstance, as mentioned by Mr. Woodhouse Braine, that the lowest known temperature is produced on the combined mixture of two anæsthetics—the bisulphide of carbon and compressed liquid nitrous oxide; viz., 220 deg. below zero of Fahrenheit.

On a review of the question so hastily and imperfectly expressed, my impression is, that the late ventilation of the subject has greatly added to the store of our knowledge, and restored ether to its pristine position. It has given us the option of one or other of these agents, or a combination of them in any threatened cardiac failure. Profound narcotism is, no doubt, more easily induced by chloroform, were it unattended by danger. On Tuesday last I administered a drachm and a

* Read before the Deeside Medical Association.

half on lint to a youth whilst under lateral lithotomy, performed by Mr. Jubb of this place. Complete somnolence continued until after he had been put to bed, and was not followed by any untoward after-symptom. Chloroform is more pleasant in such quantity to take than ether, though in that respect surpassed, perhaps, by the nitrous oxide gas. As observed by Mr. Couper, however, the time relatively consumed in the administration, and the disagreeable perfume of the vapour of ether, are unimportant, if regarded as safer, and to be free from nausea and other sequelæ.

With the bichloride of methylene, except as spray, we have had no experience here, and it would seem to possess no immunity from danger in its use.

ALCOHOL AS A CAUSE OF BRIGHT'S DISEASE; AND DR. DICKINSON'S STATISTICS.

By W. ROBERTS, M.D., F.R.C.P.,

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In a previous paper, I pointed out that Dr. Dickinson's attempt to prove from the Registrar-General's Reports that renal disease was not causally connected with intemperance, broke down on a closer investigation—that, in fact, by the same rule it might be proved that cirrhosis of the liver was unconnected with intemperance, which is a manifest fallacy. In his recent rejoinder to my criticism, he rests his argument mainly on the *post mortem* statistics of St. George's Hospital for a period of thirty years, which he has collected and compared with admirable industry. First, he compares the state of the kidneys in fifty-two persons who died of delirium tremens with that in fifty-two persons who died from accident and who were presumably of sober habits. Secondly, he compares the kidneys and urine in a hundred and forty-nine persons who followed alcoholic trades with those of a hundred and forty-nine persons who followed other occupations. He finds in both series of cases that the total of renal disease was about as great in the presumably sober as in the presumably intemperate.

This conclusion is so opposed to the general opinion, and the statistics contain on the face of them some things so improbable, that I am sure Dr. Dickinson will not think me captious if I subject them to a somewhat searching criticism. I believe I shall be able to show that they only cover a part of the ground occupied by alcohol as a producer of disease; and that, in so far as they relate to the kidneys, they are misleading.

Before going further, it is desirable to define the exact scope of the controversy. It is not contended that alcohol has any important share in the production of the acute forms of Bright's disease, or of those chronic forms which begin acutely either as a result of scarlet fever or other acute disease, or of a sudden impression of cold, and which are associated with the large, smooth, white, or mottled kidney; but it is contended that alcohol is one of the most important factors in the production of those types of Bright's disease which are chronic from the beginning, and which are associated with the granular contracting or cirrhotic kidney.*

The first thing to be remarked about these statistics is, that they contain no mention of gouty lesions; yet it cannot be denied that one of the commonest morbid effects of alcohol is gout, and that one of the not uncommon results of gout is a certain form of Bright's disease. Dr. Dickinson has felt this difficulty, and endeavours to turn it by the allegation that the "gouty kidney" generally occurs in saturnine gout. But this can hardly apply to the better classes, among whom the gouty kidney is common enough. Further, Dr. Garrod doubts if lead alone, without the help of alcohol, can produce gout.

In the next place, cases of chronic Bright's disease (non-gouty) with the symptoms of granular contracting kidney, are frequently actually met with in practice, in which no definite causal antecedent can be discovered, except the abuse of alcohol. These are generally middle-aged or elderly persons, most of whom have not been intemperate in the ordinary sense of the word, but who have been in the constant habit, for many years, of taking every day alcoholic drinks to some excess. Three such cases are at the present moment under my observation, all belonging to the easy classes, and about whom it could never perhaps be said that they had been palpably the "worse for liquor." It is also remarkable how often intemperate habits are mentioned among the antecedents of the cases of chronic Bright's disease recorded by authors—especially in this country. Of fifty-one cases related by Bright, Christi-

son, George Johnson, Frerichs, and Grainger Stewart, no fewer than twenty-seven are reported as persons of intemperate habits. Dr. Dickinson thinks it a sufficient answer to this point, to say that intemperate habits are so common in this country, that this is a mere coincidence. But let him take another chronic disease, which also prevails largely in middle life and advancing years, namely, cancer, and he will find that clinical observers have not found any such coincidence. I believe that the chief reasons why these remoter effects of alcohol on the kidneys are not indicated on Dr. Dickinson's statistics are, first, that these statistics apply to comparatively young persons, who have drank hard and killed themselves early, in whom, therefore, the later and slower effects of more moderate abuse of alcohol are not developed; secondly, that the granular kidney of chronic Bright's disease has not been sufficiently distinguished, by morbid anatomists, from granulation of the kidney induced by other morbid states.

The mean age of the cases tabulated by Dr. Dickinson varied from 36.8 to 40.6 years; but the mean age of the victims of the granular kidney of Bright's disease is, according to the same authority, 50.2 years. Here is a difference of age of nearly a decade, which may fairly be assumed to have an important influence. But there is something more than the difference of age. These statistics do not really test the point at issue; they apply, on the alcoholic side, to what one may call the acute effects of alcohol to persons who, in ordinary parlance, would be called intemperate. In such persons are produced cirrhosis of the liver, alcoholic phthisis, early fatty changes, and delirium tremens. But there is another very considerable class of persons who ostensibly rank among the temperate—persons who never get drunk, who run little risk of cirrhosis or alcoholic phthisis, and none at all of delirium tremens. These persons imbibe daily a few glasses of spirits or wine, or a few pots of porter, more than is good for them. This goes on steadily for a long course of years, apparently without ill effects; but at length they become gouty, or the urine becomes albuminous, or their arteries become fatty ten years earlier than they would otherwise. Are these effects less real because they are produced more slowly? There has, I believe, been too little attention paid to the effects of these long continued small excesses of alcoholic drinking, which do not, as hard drinking often does, rob a man of three-fourths or one half his life, but which curtail his life by some ten or fifteen years.

Another source of error in these statistics is the excessive amount of renal disease credited to presumably healthy persons. Dr. Dickinson would have us believe that one in every seven presumably healthy persons, of the average age of forty-one, is affected with granular kidneys. If this is to be taken to mean that one-seventh of our presumably healthy population are the subjects of chronic Bright's disease—even in the incipient stage—the statement must involve some notable fallacy. Chronic Bright's disease is not easily robbed of its victim. If this statement were true, or anything like true, Bright's disease ought to rank enormously high in our mortuary returns. But I find that all the types of renal disease only account for about one per cent. of the total mortality. But this point may be tested in another and more satisfactory manner—namely, by the examination of the urine of a number of presumably healthy persons. If the urine be free from albumen, and of normal quantity and specific gravity, it may be assumed—with only a very slight risk of error—that the producer of that urine is free from Bright's disease. I would suggest for this purpose the examination of the urine of the patients admitted into a general hospital for fractures and other injuries, which do not compromise the general health. I have made a small beginning of an investigation of this sort, but under less stringent conditions. My clinical clerks were instructed to examine the urine of all patients in the surgical wards of the Manchester Infirmary above the age of twenty years. Vesical cases were excluded, as were also patients who were moribund, or labouring under severe general ailment. Forty-two cases answered to these conditions; their average age was forty-two: twenty of them were cases of fractures and other injuries, seven had joint-disease, seven had ulcers, and the remainder had cancerous tumours or some trifling ailment. Of these, only two had albuminous urine; one was a man of intemperate habits, suffering from strumous disease of the knee-joint; the other was a syphilitic youth, with ulcer of the leg. All the accident cases—which were really the only cases fairly comparable with Dr. Dickinson's cases of death from accident—had not a trace of albumen in the urine; so that, instead of one in seven exhibiting signs of renal disease, there was only one in twenty-one, and this in spite of the inclusion of struma and syphilis. These numbers are far too few to found a safe conclusion on, but they indicate very clearly the necessity for caution in accepting purely *post mortem* data on the condition of the kidneys. Dr. Dickinson is inclined to reproach me with testing pathological observations by clinical experience. I would in my turn venture to say that morbid anatomy, without the control of clinical observation, may sometimes lead very

* I put out of view the "lardaceous" or "waxy" cases, because of their limited number, and their more obvious connection with chronic suppuration, joint-disease, and syphilis.