TABLE III.—*Amounts of Chloroform (calculated from Chloride) found by Distillation of Small Animals (Rats) Killed by Inhalation of Chloroform Vapour.*

	Duration.	Weight of Animal.	Weight of $CHCl_3$ Found.	Lethal Quotient, Ratio of $CHCl_3$ to Body Weight.	Remarks
Experiment 21 ...	6½ hours	175 grams	0.0164 gram	0.000094	
,, 22 ...	6 ,,	191 ,,	0.0180 ,,	0.000102	First distillation.
	7 ,,		0.0015 ,,		Second distillation of same animal.
,, 23 ...	15½ ,,	157 ,,	0.0000 ,,	—	Check experiment; no $CHCl_3$ introduced.
	15 ,,		0.0000 ,,	—	Same result on second distillation.
,, 24 ...	17½ ,,	168 ,,	0.0157 ,,	0.000093	First distillation.
	16 ,,		0.0000 ,,		Second distillation of same animal.
,, 25 ...	16½ ,,	174 ,,	0.0170 ,,	0.000098	
,, 26 ...	22 ,,	210 ,,	0.0250 ,,	0.000119	

The mean lethal quotient of 5 experiments is 0.000101, or very nearly 1 part of chloroform by weight to 10000 parts of animal by weight.

The results recorded in the preceding summary are defective mainly as regards Table I, which should contain a more extensive and accurate elementary justification of the method. The data of this table were, however, obtained at the outset of the investigation, when experimental error was great. Table II is more satisfactory, the experimental error is greatly diminished; and in those instances where an unduly large deficit has occurred, the source of error has been clearly due to some very recognisable and therefore avoidable accident. These "bad" results might properly have been omitted from the summary, but have been left undisturbed on account of the warnings they convey. Table III, containing the latest series of experiments, in which various improvements of detail suggested by previous experiments had been introduced, is probably the most accurate, although obviously there is here no measure of inaccuracy possible by comparison between amount of $CHCl_3$ taken and amount found.

The preliminary conclusion from this last series—to be confirmed or modified by further experiment—is that the weight of chloroform recoverable from the body of a small animal killed by the inhalation of chloroform amounts to 1 part in 10,000 of the body weight in the case of a small animal (rat), a quotient which is about twice that estimated by Snow as being lethal to the human subject.

THEORIES OF INHERITANCE.

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ALL the characters of a living being may be grouped under one or other of two headings—either they are *inborn* or else they are *acquired*. Inborn characters may be defined as those which take origin in the germ cell. Thus arms, legs, eyes, ears, etc., are all inborn. They arise because the germ cell is so constituted that it tends under fit conditions of shelter and nutrition to proliferate into an organism having arms, legs, eyes, ears, etc. An acquired character (technically termed "modification") is an alteration impressed on an inborn character by influences acting on that character *after* it has developed from the germ. Thus a hand is inborn, but if it be altered in any way, as by use or injury, the alteration is an acquirement. It will thus be seen that the difference between the inborn and the acquired is essentially one of *origin*. Inborn traits take origin in the germ, acquired characters in cell descendants of the germ. It is necessary to dwell on this point. Endless confusion has arisen through the ambiguous use of the of the term "acquired." For instance, in medical literature, the term is frequently used as synonymous with "new," and every new character is then called an acquirement. Thus a sixth digit on its first appearance in a family is often called one; but a sixth digit, since it results from a germinal peculiarity, is no more an acquirement than a man's head is. It could only be an acquirement were it implanted on an already developing hand by outside influences.

VARIATIONS.

Among the higher animals at least offspring invariably differ inherently from their parents—that is, they are born different from what their parents were born.[1] They make a different start in life. These congenital differences are technically termed "*variations*." Thus a sixth digit on its first appearance in a family is a variation. It follows, since a variation takes origin in the germ, that it is not an acquirement, but an inborn trait. It arises because the germ from which the offspring sprang is different from the germ whence the parent sprang. Formerly the term "variation" was used in a less restricted sense, being applied to acquirements, as well as to congenital differences. But in the interests of scientific precision of late years the more restricted meaning has generally been adopted.

[1] I use the word born to avoid circumlocution. But of course if an embryo or fœtus (as distinguished from a germ) acquired a character (say a disease), the change is as much an acquirement as if acquired after birth.

ALLEGED TRANSMISSION OF ACQUIREMENTS.

It is a fundamental assumption of every theory of heredity that variations (not acquirements) are transmissible to offspring, and through them to remoter descendants. Thus a sixth digit after its first appearance tends to be transmitted to posterity. The fact is nowhere disputed; and up to this point all theories of heredity march together. But they differ in the causes they assign to variations; that is, in the reason by which they seek to explain the fact that children at birth differ from their parents at birth. At present we need not enter into an elaborate exposition of the many theories that have been formulated in past times to account for variations. It will be sufficient if we note the theory which finds most favour among medical men and the rest of the general public. This theory, authentically as old as the patriarch Jacob, is certainly of even vaster antiquity. Probably it dates from the time when men first began to consider the relation of child to parent; that is, from the time when men began to reason. According to it, parental acquirements (that is, changes in the parental soma) tend *so* to affect the associated germs that identical variations are reproduced in the offspring which spring from the germs. Thus it supposes that, if a man strengthens his arms by exercise, his germs will be so affected that his offspring will tend to have arms stronger than they would otherwise have had. In that case the child is born different from what the parent was born, since it has inborn the peculiarity the parent only acquired. According to this—the Lamarckian—theory, then, the parents' acquirements tend to be transmuted into variations in the child, andt hus to become transmissible to the child's offspring and toremoter descendants. It is, in fact, the theory that supposes that parental requirements are transmissible.

Before we proceed to test the truth of this alleged transmission of acquired characters it is necessary to note one fact of vital importance. Even if it be true that acquirements are transmissible, yet it is abundantly clear that variations in offspring cannot all be attributed to the transmission of parental acquirements. Thus, since no man ever acquires a sixth digit, since a sixth digit is always inborn from the first, that structure on its appearance in an infant can never be attributed to the transmission of an acquirement. Again, the individuals of a litter of puppies not only vary from their parents but they differ among themselves. Some of them are bigger or stronger, smoother or rougher than the others. The endless differences cannot be attributed to the transmission of parental acquirements, for the acquirements if transmitted would produce the same variations in all the puppies. Clearly, then, variations do arise in offspring quite irrespective of the transmission of acquirements. How they arise

does not at present concern us. The essential fact is that they do arise, no matter how.

THE CHARACTERS ACQUIRED BY MAN.

We now reach the kernel of our inquiry. The characters acquired by every human being, for example, are millions in number. In fact, the adult man is structurally a mass of acquirements, reared on a comparatively insignificant basis of inborn traits. His body changes from infancy to old age mainly in response to influences acting on him from the environment—that is, it changes by the endless superimposition of one acquirement on another. His limbs grow in response to use and exercise, for an infant limb does not develop when rendered useless by disease or accident. His brain and most of the structures of his trunk grow in response to the same strain. It may be said that after birth almost nothing except his hair, teeth, and the full development of his congenital organs comes to him except by way of acquirement—that is, except in response to stimulation from without. Mentally he is even more a creature of acquirement than he is physically. At birth his mind is a blank. Later it has an enormous range. Again, almost nothing enters him save as an acquirement. For example, every single word of his language, every idea he possesses is a separate acquirement. The whole contents of his memory, in fact, and all that flows from memory, are acquirements.

The human body is compounded of billions of cells, but a future individual springs from only one of them, a germ. The other cells—the somatic cells—afford shelter and nutrition to the germs, but there is not a tittle of evidence that they do more. They are specialised for their separate functions, just as the germ is specialised for reproduction. There is nothing to show that they are concerned in reproduction, any more than that the germs are concerned in the secretion of saliva or bile.

Now let us see what the current medical belief in the transmission of acquirements involves. It involves the supposition that, of all the millions of acquirements, every one tends to influence each germ cell in such a special manner that offspring tend to reproduce as inborn characters—variations in this case—the particular traits the parent acquired. A change in the great toe is supposed to affect the germs in one way, a change in the thumb in another, a change in the lung in a third, a change in the mind in the fourth, and so on *ad infinitum*. In each instance the child is supposed to reproduce the trait the parent acquired.

ACQUIRED CHARACTERS NOT TRANSMISSIBLE.

It is unbelievable that this can be true. What is the machinery by which this magical process is carried out? We know of none. On the face of it, therefore, the transmission of acquirements appears wildly incredible. We are entitled to reject all belief in it unless the clearest and most conclusive evidence be furnished. Has such evidence been furnished? The simple fact is that, though during the past twenty years the plant and animal kingdoms have been ransacked, no single instance of the transmission of an acquirement has yet been proved. In every instance—and the instances have been hundreds if not thousands in number—when transmission has been alleged, the case has broken down on investigation.

It matters nothing that a belief in transmission is almost universal among medical men. Medical men as a body have not studied the subject. It is true that they ought to have done so. Heredity is a part of physiology—an essentially medical science—its most important part. Nevertheless, it has been studied almost exclusively by zoologists and botanists, whose interest in the question has been merely abstract, and whose fund of data has been incomparably inferior to that in the possession of medical men, the students in health and disease of man, the best known of all living beings.

Medical men know—or rather should know, for the fact is constantly ignored—that man passes from infancy to old age almost solely by the accumulation of thousands upon tens of thousands of acquired traits. Were acquired characters transmissible, the child of an old man ought to be clearly distinguishable from the child of one 40 or 50 years younger. In fact, the child of the aged should be born aged. But no medical man is able to distinguish the child of an aged couple from the offspring of a boy or girl who have barely reached puberty.

We are told that though local modifications, which affect this or that organ merely, may not be transmissible yet widerspread and deeper-seated acquirements are transmissible. Hæmophilia is given as a case in point. But hæmophilia is never an acquirement. It appears as a variation, an inborn trait, from the beginning.

Syphilis is another instance. But syphilis never occurs in the absence of the specific virus. A foreign body passes from parent to child, and it would be as reasonable to speak of a bullet, which, after piercing the mother, lodged in the child, as an instance of the transmission of an acquirement. The parent acquires syphilis and the child in turn acquires it. It is never inborn in either, it is never a variation, and as we have seen the transmission of an acquirement implies the reproduction of it as a variation by the child. A distinction is sometimes drawn between hereditary syphilis and "syphilitic heredity." In the latter case, any abnormality in the child of a parent who has suffered from, and even recovered from, syphilis is attributed to the parental disease. But people who have not had syphilis occasionally have feeble children. There is such a thing as a confusion of *post* with *propter hoc*.

Gout is a third example. It is admitted on all hands that parents, who have become gouty under fit conditions of ease and high living, tend to have children who are liable to develop gout under like conditions, just as big or dark men tend to have big or dark children. Thereupon it is assumed that parental high living is a cause of filial goutiness. *Post hoc* is again confused with *propter hoc;* diathesis with disease. The diathesis, the inborn tendency to acquire the disease under certain conditions, is transmissible; but there is no evidence that parental high living increases it in the child. On the contrary, there is evidence that the children of poor Irish peasants who have never had gout are as liable to it when placed under easy circumstances as the scions of the British aristocracy. Were gout very common and fatal, races that had most been affected by it would, following the rule of other common and fatal diseases, be the most resistant to it, the least liable to contract it.[2]

Long experience of certain zymotic diseases has endowed various races with superior resisting powers, which in each case are specific. We are told to attribute this to the transmission of acquired immunity. But in no case has there been an evolution of greater resisting power than in the case of tuberculosis. Experience of tuberculosis does not confer immunity or increased resisting power on the individual. It weakens, rather than strengthens, against subsequent attacks. If then acquired characters were transmissible, a race that had long been afflicted by tuberculosis should be weaker, not stronger, against the disease. The contrary is the case. The British, for example, who have suffered for thousands of years, are infinitely more resistant to tuberculosis than Polynesians, whose ancestry had no experience of it. Again the mortality caused by chicken-pox is practically non-existent, but one attack confers immunity against subsequent attacks. In this case races that have longest been afflicted suffer as severely, but not more severely, than races to which it has been newly introduced. Clearly then in every case the evolution of resisting power has been due to the weeding out of the unfit, to the constant and prolonged elimination by each lethal disease of individuals weak against it, not to the transmission of any acquired character. It is needless to multiply instances here. But presently it will be necessary to return to the subject.

The fact that each individual is derived from a single cell, the fertilised ovum, enables us to formulate a theory of heredity which denies the transmission of acquired traits. The fact is undisputed; the deduction reasonable, and supported by a vast mass of evidence. But attempts have been made to go deeper, to formulate theories as to how parental characters are transmitted to the offspring. Several so-called working hypotheses have been put forward.

DARWIN.

Darwin supposed that each cell of the multicellular organism sent off portions (which he called gemmules) of itself to each

[2] *Vide infra.*

germ cell, which thereafter, on being fertilised, was thus enabled to proliferate into a being resembling the parent organism. To understand the full beauty of this theory it must be remembered that the germ cells of a man, for instance, are in millions, and his somatic cells in billions. To compute the number of the gemmules we must multiply the millions of the germ cells by the billions of the somatic cells. This theory was quite seriously discussed by biologists for a number of years. It is very wonderful, but by far the most wonderful thing about it is the fact that of all men Darwin should have been its author.

Weismann.

Subsequently, after the transmission of acquired characters had been denied, Weismann formulated his theory of the continuity of the germ plasm. He supposes that some of the germ plasm of the fertilised ovum is separated off and handed on more or less unchanged to form the germ cells of the offspring. He adds amazing complications in the way of ids and idants, biophors, etc., the bearers of heredity. If his hypothesis be true, it must follow as a corollary—*but as a mere corollary*—that acquired traits are not transmissible. But a theory that only inborn characters are transmissible is one thing; a theory as to how inborn characters (to the exclusion of acquired characters) are transmitted is quite another thing. The former theory rests on the solid ground of well-ascertained fact—on the cell theory. The other is absolutely unsupported by evidence. It may or may not be true. There is not a shred of evidence one way or the other. No one has seen, or, at least, no one can recognise the germ plasm, much less an id or an idant. It is a remarkable fact that a great many people have assumed that the proof of the doctrine of the transmissibility of acquired traits depends on the proof of Weismann's hypothesis of the continuity of the germ plasm.

Adami.

Weismann had at least the excuse that he built on a foundation of fact—offspring do arise from a single cell, and no instance of the transmission of an acquirement is known. The latest theory has not this excuse. Its author, Professor Adami, pours contempt on Weismann's scholastic subtleties, and then proceeds to formulate a hypothesis entirely similar in kind. He puts forward a chemical theory of inheritance. But the chemistry of inheritance is, if possible, even more a matter of speculation, of pure guess-work, than are ids, idants, and biophors. No doubt, as Weismann says, the germ plasm is the bearer of heredity, and no doubt, as Professor Adami says, it has its chemistry. But there our knowledge, and even our power of making legitimate inferences, ends. If we attempt to go forward we enter into the regions of the unknown and probably the quite unknowable. Consider a man. Consider the vast complexity of his body, and, above all, of his mind (or its physical concomitant the brain). Consider that he springs from a microscopic speck of protoplasm, the fertilised ovum. Think of the enormous complexity and mystery of the processes, vital or chemical, which transform that speck into a child, an adult, an aged human being. Remember that the fertilised ovum of an elephant or a mouse is indistinguishable in all essential particulars from that of a man. Think of all this, and think also of our futile microscopes, our infantile chemical analyses, and some idea will be gathered of the vanity of attempting to pry into the how of the inheritance either of inborn or of acquired traits. The infinitely small is as difficult as the infinitely great. With our present knowledge it were as wise to attempt to solve the mystery of the universe as to seek to solve the mystery of inheritance. All we can do is to found ourselves on verifiable evidence, and by the light of it say that such-and-such traits are not inherited, and that such-and-such traits are inherited. How they are inherited is, as I say, quite another problem.

It is unnecessary therefore to discuss Professor Adami's chemical theory of inheritance in detail. In support of it he offers absolutely no evidence, but only some illustrations drawn from chemistry, or rather from some chemical "working hypotheses." Like Weismann he may or may not be right; indeed, both he and Weismann may be right, though owing to the obscurity and complexity of the subject, and the absolute lack of data, it is infinitely more probable both are wrong; but apart from speculations about things which are unknown, Professor Adami falls into manifest error about things which are positively known. He adopts the fallacy that the theory of the non-transmissibility of acquired traits depends on Weismann's transcendental speculations concerning the continuity of the germ plasm. With that we need not deal. It is probable that Professor Weismann deserves all and more than Professor Adami says of him. He makes an even worse error when he confuses the inborn with the acquired, with the odd result that his theory is perfectly compatible with a belief in the non-transmissibility of acquired characters. In fact, contrary to the author's intentions, it is like Weismann's theory—really a speculation as to how inborn characters are inherited.

Professor Adami remarks that characters acquired by unicellular organisms are transmissible, and, after giving various examples, declares that "the argument that phenomena observed in unicellular organisms cannot be applied to multicellular organisms is, to say the least, severely strained." Again, after arguing that toxins circulating in the parent's blood must affect the germs, he continues, "Here Weismann would make the somewhat subtle distinction that we are not dealing with the direct transmission of acquired parental defects; that the toxins produce these results not by acting on the body cells, but by direct action on the germ cells; that the inheritance is blastogenic, not somatogenic. This is a sorry and almost Jesuitical play upon words. Let us grant that they are of blastogenic origin; they are nevertheless of individual acquirement."

Now, as we have seen, the words acquired and acquirement are technical biological terms having very precise and definite meanings. They are applied to the alterations of the soma. We have seen, moreover, that when an acquired character is thought to be transmitted, the parental germs are supposed to be so altered that the character the parent acquired is reproduced as an *inborn* trait by the offspring. Alterations of germ, therefore, result in *inborn* changes. It follows, since unicellular organisms have no soma, and, since each one is a germ cell, that alterations of them are inborn characters, and for that reason are transmissible. In this case the very cell that is modified transmits its modification to its own descendants and to every one of them. In the case of a multicellular organism, one set of cells (the somatic cells) acquire the modifications, but quite another set (the germ cells) are supposed to transmit the modifications to some of their very remote cell descendants. Professor Adami might with as much reason complain that since Brown is able to transmit his traits to his own offspring, we severely strain the argument when we decline to admit that he can transmit them to the offspring of Jones and Robinson also.

"Indirect Transmission."

He seeks to prove that parental ill-health or toxins circulating in the parents' blood tend to enfeeble offspring subsequently born. He calls this "indirect transmission." But when an acquired character is said to be transmitted, a peculiarity similar to the acquirement is supposed to be reproduced by the offspring, and this (according to the Lamarckian doctrine) for the reason that acquirements tend to be transmitted. But if a parent's acquirement so affects the germ that something quite different is reproduced, then that clearly is no case of transmission. Thus if a man has some disease (say phthisis), and his child in consequence is merely enfeebled, that would not constitute a transmission. To constitute a transmission the child should reproduce the lesions of the parent. Similarly if a toxin circulating in the parent's blood enfeebled both the parent and the offspring again we should have no transmission, not even if by a singular coincidence the enfeeblement were identical in type in parent and child. It must be remembered that no one has asserted that the germs of multicellular organisms are incapable of alteration. It has only been asserted that modifications of the parental soma do not tend so to affect the associated germs that similar peculiarities are reproduced in the offspring. When, therefore, Professor Adami speaks of the transmission of acquirements by unicellular organisms, or of "indirect inheritance" among multicellular organisms he is speaking in reality of the transmission of alterations of the germs (variations, new inborn traits), not of acquirements (modifications of the soma). And then, as I say, his theory,

like Weismann's, is an attempt to explain how these inborn traits are transmitted.

EFFECTS OF PARENTAL DISEASE.

Had, then, Professor Adami proved that parental diseases, etc., affect offspring subsequently born in some way other than by causing them to reproduce parental acquirements, he would still have been very far from proving his case. As a fact he has not even proved the little that in this instance he sought. His assumption is apparently reasonable. We have abundant evidence that the germs of unicellular organisms are capable of being altered by environmental influences, and it seems only reasonable to expect the germs of multicellular organisms are equally capable of alteration (for example, by toxins circulating in the blood). Nevertheless the evidence is still to seek. Indeed it seems probable that the germs of multicellular organisms are much less capable of modifications than lower types. For did environmental influences (toxins circulating in the blood) so injure germs as to enfeeble offspring, a race that used alcohol for instance, or was affected by malaria would by the accumulation of the injury grow more and more enfeebled generation after generation till by the accumulation of injury it would tend towards extinction. I am not aware that South Europeans who have used alcohol for thousands of years are more degenerate than the lowest savages, the Terra del Fuegians, for instance, who have never used it. Again were Professor Adami's reasoning correct races that have long suffered privation and hardship should be degenerate whereas races that have lived in ease and plenty should be the reverse. If anything the contrary is the case.

THEORIES OF EVOLUTION.

All theories of heredity are in essence theories of evolution. If the Lamarckian doctrine be true, if acquirements are transmissible then all agencies which beneficially affect the individual, good and plentiful food, sunlight, fresh air, exercise and so forth must also benefit the race, must during the lapse of generations become causes of evolution, whereas all agencies which injuriously affect the individual must equally injure the race, must be causes of degeneration. On the other hand if acquirements are not transmissible then agencies which benefit the individual cannot be causes of evolution, which must be attributed wholly to injurious agencies that by weeding the unfittest leave the propagation of the race to the fittest. The two doctrines are thus fundamentally and violently opposed. It follows, by watching the course of racial change, that we are able to decide which of the two doctrines is true. Let us then turn to Nature. Does she furnish a single instance of racial change due to the transmission of acquirements? Not one. It is true instances by the hundred have been alleged, but all without exception have broken down on investigation. Does she furnish instances of racial change due to the weeding out of the unfittest, of the accentuation of variations by selection (natural or artificial)? She furnishes them without number. The effects of disease selection in rendering races resistant to disease, and the effects of artificial selection in evolving our domesticated animals and cultivated plants are alone decisive.

Moreover this line of argument furnishes conclusive proof that agencies that affect the parent do not as a rule in any way affect the offspring subsequently born; otherwise in this case also beneficial agencies would lead to evolution, injurious agencies to racial degeneration. As we see the contrary is the fact. Professor Adami quotes Paul to prove that plumbism in the parent almost invariably results in the death of offspring subsequently born. I am not prepared to impeach Paul's results off hand. It is possible that lead specially poisons germ cells just as strychnine poisons nerve cells. But in that case it is a very remarkable poison. It does not destroy the delicate sperms; it merely destroys the offspring which arise from the sperms many months, even many years after. In other words plumbism does not destroy the cells which are exposed to its influence, but only their very remote cell-descendants when long removed from its direct influence. Considering the number of mare's nests of this description that reinvestigation has exposed it is probably wise to suspend judgment until this particular case has been reinvestigated. In any case one swallow does not make a summer. If ever deleterious agencies acting on the multicellular parent do affect the germs, the instances in which they do are evidently so rare as not to affect the general question.

THE OBJECT OF THE ESSAY.

The main object of this essay is an endeavour to place heredity on what I hope is a scientific basis for medical readers. Heredity ought to be a science. Already we have excellent data on which to found very important conclusions. The fact that offspring take origin, not from the whole of the parent's body, but only from the microscopic germs renders the transmission of acquirements exceedingly improbable. Doubt is converted into certainty by the fact that though all high organisms acquire millions of traits in no case has the transmission of an acquirement been proved. This line of argument is extremely simple and absolutely clear and conclusive. But it has been almost quite ignored. One may wade through volumes devoted to the subject and get no hint of it. Instead we are treated to theories of *how* characters are transmitted—to treatises on pangenesis, physiological units, the continuity of the germ plasm, and so forth. We are led through a fog of vague conjectures into the darkness of the unknown. "What might be a science is converted into a tumbling ground for whimsies." Let us keep to the facts we know. We shall not then be able to explain how a man transmits a head to his offspring, nor why a man has for offspring another man and not a dog or a tree, nor indeed why he has offspring at all. But we shall be able to formulate certain "laws" from the facts of our common experience. We shall be able to say that while inborn traits are transmissible, acquirements are not transmissible, and having done that we shall have stated a truth of enormous importance to all men, but to none more than to medical men.

DIRECT INTRODUCTION OF PURGATIVES INTO THE LARGE INTESTINE IN CASES OF OPERATION FOR SEPTIC PERITONITIS.

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I WISH to draw the attention of the profession to a method which I believe is of great utility in the surgery of septic peritonitis—the direct introduction of purgatives into the intestines at the time of operation. It is not too much to say that in many of these cases the patient's life hangs on the possibility of overcoming the paralytic obstruction and the free evacuation of gas and fæces. The worse the case the more difficult is this to bring about, since the patient vomits everything he takes by the mouth.

I have hitherto only used this method in cases of perforative appendicitis, and here the performance of the injection is very simple. The nozzle of a small syringe—the hydrocele-injecting syringe is a convenient form—is introduced into the "stump" of the appendix and the solution directly thrown into the cæcum. Three drachms of magnesium sulphate, with ten drops of tincture of nux vomica, and a drachm of glycerine in an ounce of water is the formula I have generally employed. Two hours afterwards a turpentine enema is given, and the result has been excellent.

I have employed this method in 5 bad cases of septic peritonitis associated with perforative appendicitis. In every case the results have surprised me. And though the number is too small for a pronouncement as to the establishing intra-cæcal purgatives as a definite line of treatment, yet the cases are sufficiently striking to justify me in urging a trial of it upon my professional brethren.

It is obvious that in other cases the solution could be easily and safely thrown into the colon by means of a hypodermic syringe obliquely introduced. Further experience may elicit better purgatives than magnesia. The amount of magnesium sulphate is difficult to estimate. In one very bad case I introduced four drachms, and I think the dose should be proportional to the age of the patient and the extent of the peritonitis.