Correspondence

Race Suicide for Social Parasites.

DAVENPORT, IOWA, Jan. 7, 1908.

To the Editor:—In Dr. W. T. Belfield's recent Chicago address on the above subject, an abstract of which appeared in *THE JOURNAL*, January 4, he is credited with views which, it appears to me, should not pass unchallenged by the medical profession. His advice, if generally sanctioned by reputable physicians, would tend strongly to race suicide not confined to the parasitic classes, and would leave the spread of the venereal diseases unchecked. It can not have been carefully considered by *THE JOURNAL*, I think, or it would hardly have been published without comment.

That it is both the right and the duty of the state to restrict the breeding of confirmed criminals and other defectives in every practicable way is not questioned; and, as I see it, castration for such would be a practicable and wise provision both for the safety of the state and for the physical and moral advancement of the people. But when it is proposed, by cutting the vas, to put a stop to procreation while purposely leaving to the brute his impelling desire, his potency and "pleasure," it is hard to conceive of a course of unwisdom more blind. Rape and the spread of the venereal diseases would remain unchecked, and adultery, including miscegenation, and all forms of consenting vice, would be encouraged, the fear of telltale progeny being removed.

Strong as are these objections to Dr. Belfield's dangerous proposition—and they are sustained by his own statement that in Indiana, where the process has been legalized, hundreds of criminals have been sterilized at their own request—an even stronger one is suggested by his further statement that, within his own personal knowledge, married men have chosen this means of prevention as preferable to criminal abortion. He seems not to perceive that it is no less culpable for the physician to lend his aid to the one measure than to the other, or to realize the encouragement to race suicide in this all too convenient means of sterilization without loss of power.

When, too, the husband and the illicit lover are told that they can play fast and loose with their vasa deferentia, cutting off and on the testicular secretion by an operation "less serious than the extraction of a tooth," it would be hard to conceive of a recommendation fraught with more far-reaching evil and disaster. I trust *THE JOURNAL*, as the exponent of the profession, will present this measure in its true light.

C. H. PRESTON.

[COMMENT:—The above letter was referred to Dr. Belfield and we print below his reply.—ED.]

"The worthy critic laboriously constructs formidable demons of straw, and then demolishes them with great fury and success. By similar reasoning he can prove that arsenic and strychnin should never be prescribed, because they have often been used in perpetration of murder; that labor should never be induced to save the life of mother and child, because criminal abortion is so common. In short, he condemns the intelligent, lawful and beneficial use of sterilization, because it can be prostituted to harmful and objectionable abuse."

Death Following Injection of Diphtheria Antitoxin.

WAKE FOREST, N. C., Jan. 9, 1908.

To the Editor:—The sudden death following an injection of diphtheria antitoxin, reported by Dr. Gillette, Cuba, N. Y., in *THE JOURNAL*, January 4, page 40, is worthy of general notice. The case in question was one of bronchial asthma and the antitoxin was administered for its relief. Since several other fatal cases similar to this one have come to light it would appear possible that there is a connection between this disease and a grave susceptibility to diphtheria antitoxin.

Cases of sudden death following small injections of diphtheria antitoxin have been recorded—though death from this cause is a very rare occurrence. Certain of these cases have undoubtedly been due to the status lymphaticus. Osler reports two such cases in his "Practice" (Ed. 1901, p. 827). One of the individuals was the son of Professor Langerhans of Berlin and excited considerable attention at the time.

Park, in his Harvey Society lecture, Jan. 20, 1906, mentions several fatal cases in which it was probable that the status lymphaticus existed. He also mentions other cases in which death seemed imminent, but the patients were saved by the employment of emergency measures. These patients described sensations very similar to those which occurred in Dr. Gillette's case.

In regard to Dr. Gillette's tentative explanation that the status lymphaticus was present in his case, it appears that that explanation would be improbable for two reasons. First, on account of the age of his patient. The condition obtains almost exclusively in children and young adults. Second, in the autopsy record there is no mention of hyperplasia of the lymph tissue of the body, which anatomic feature is usually marked in this condition.

I have the temerity to suggest a connection at least between the asthmatic condition and the possible action of the antitoxin. The respiratory center in chronic asthmatics is in a constant state of pronounced irritability due to the accumulation of carbon dioxide in the blood at frequent intervals. It is thus in a condition to be easily acted on by further irritants, one of which we may presume is diphtheria antitoxin. It is well known that excessive irritation leads to depression which in case of the respiratory center would lead to severe or fatal results. This hypothesis would explain the symptom of dyspnea of which the patient suddenly complained just before death. Further, if this be true, it should lead to extreme care in the administration of diphtheria antitoxin to all patients in whom there is an accumulation of carbon dioxide in the blood.

LEWIS M. GAINES, M.D.

The "Hyoscin-Morphin-Cactin" Anesthesia.

REPLY OF THE ABBOTT ALKALOIDAL COMPANY.

CHICAGO, Jan. 6, 1908.

To the Editor:—In your issue of December 21 you devoted some four pages to the discussion of the following subject: "The 'Hyoscin-Morphin-Cactin' Anesthesia. An Interesting Example of the Subordination of Science to Commercialism."

The most striking thing on the first page of your presentation is the reproduction of two paragraphs, without context, from an article of mine appearing in the *Chicago Medical Recorder*, in which I call attention to the favor which the remedy is winning in the field of obstetrics, because it has reduced the pain and consequently the fear of childbirth. I can not, of course, read beneath the troubled surface of your mind as to just what impression you intended to convey by this truly sensational presentation. I assume, however, that you wished to question the truth of my statements. It may be of interest to know that the opinion concerning the sociologic value of hypodermic anesthesia and its effect on race suicide did not originate with me, but was first expressed concerning scopolamin-morphin and most strongly voiced by Holt, writing from Gauss' clinic at the University of Freiburg. It does correctly express, however, the sentiments of hundreds of physicians who have used H.M.C. (Abbott), as shown in their letters to me, an exhibit of any reasonable number of which we will gladly show you.

I wish first of all to deny *absolutely* the statement made in *THE JOURNAL* article concerning hyoscin-morphin-cactin anesthesia, that "everything connected with its promotion has conveyed the impression that this method of anesthesia is entirely new," or as otherwise and elsewhere stated in the same article: "It is evident that it has been the intention of the manufacturers to convey the impression that this method of anesthesia originated with them." Nothing can be farther from the truth. As a matter of fact there have appeared in our literature numerous references to the use of scopolamin and morphin for anesthetic purposes. The *American Journal of Clinical Medicine* has contained fully as many abstracts of the European literature on this general subject as any other medical journal—more, we believe, than has your own, which has been so utterly closed to anything favoring or fairly discussing the American side of the movement while wide open to the wildest, most prejudiced criticism.

The longest contributed article on hypodermic anesthesia ever published in *Clinical Medicine* was on "Scopolamin-Mor-

phin in Obstetrics," which gave a detailed account of Gauss' work in the University Women's Clinic at Freiburg, published in May, 1907, this article being written by Dr. William Holt, an American, at that time studying at Freiburg. What we have claimed—and we still maintain it—is, that our hyoscin combination is superior to the scopolamin combination which was used by early investigators, with unfortunate results in some cases, said gruesome statistics being erroneously held to apply to improved conditions as they now exist. *Priority of suggestion of hypodermic anesthesia we have never claimed.*

In one sense, however, hyoscin-morphin-cactin anesthesia certainly is new. Scopolamin-morphin anesthesia, in spite of its eight years of history, was "new" to the doctors of America when we first manifested an interest in it. There had been a number of deaths reported, mainly abroad. It was discredited by the journals, and only a few American surgeons, perhaps a dozen or two, had had the courage to try it. We took it up, sought to discover its defects and remove its dangers—to convert it, if possible, from a merely experimental procedure into a reasonably safe and satisfactory aid to the work of surgeon and obstetrician, as well as to the internist in colics, spasms, etc., and in all those efforts we have succeeded, if sales to some 20,000 American physicians and surgeons and thousands of ringing testimonials are worth anything.

More than two pages of THE JOURNAL article are devoted to an attempt to prove the identity of hyoscin and scopolamin. Our advertising literature is extensively quoted and every effort made to show that our expressed preference for hyoscin to scopolamin is an evidence of purposeful deceit or of ignorance. We shall not repeat these quotations, though they are garbled, and in one instance at least a word is omitted from the body of a sentence written by me which *completely changes its meaning*—this without any mark to show that such omission has been made. It makes quite a difference in your favor to say: "It's the synergistic which produces the result," instead of "It's the synergistic whole," (referring to the published formula) our claim and text.

We do not purpose to enter with detail into a controversy on this point, which, as we shall endeavor to point out a little later, is not the main and centrally important point at issue. But it is noteworthy that THE JOURNAL admits that "the German literature, until recent years [very recently, in fact.—W. C. A.], furnished an abundance of material from which to quote to prove that the alkaloid as made from hyoscyamus is different from that made from scopola," and that "there have been echoes of the old controversy until recently." This does not quite accord with the statement made in the next sentence, that the identity of the two substances "was settled long ago." We might even quote the following statement by Prof. John Uri Lloyd (*Eclectic Medical Gleaner*, January, 1907): "In our opinion, one has no more the right to dispense scopolamin for hyoscin than to label hyoscyamus as belladonna."

The chemical identity of pure hyoscin and scopolamin no one disputes. For argument's sake let us pass for the present—admit, if you prefer—the pharmacologic identity of the two substances—provided we can be sure that in both cases we are dealing with a substance of absolute purity. If when pure they are therapeutically identical, we are content; satisfactory therapeutic result and the safety of the clinician is our only desire.

Your article says: "The pharmacopeia of a nation is the standard according to which drugs are manufactured and by which they are judged. In all countries these standards are recognized by law; they are the highest authority." (Italics mine.) Let us examine the German and the United States Pharmacopeias, which are quoted extensively to prove the identity of hyoscin and scopolamin and see if they are a sufficient and safe authority for us to depend on. Pharmacopeias are guides by which the pharmacist is supposed to serve the doctor, provided the doctor chooses to be so served, the right of choice and specification being vested in him.

I know I am antagonized on this point by you, but the fact remains, as voiced by Hatcher in your issue of January 4, who, in his excellent paper on "The United States Pharmacopeia,"

says: "It is obviously not the province of pharmacists to decide for physicians what remedies they are to use."

While pharmacopeias are officially accepted as "authorities" no reasonable man has ever asserted that they are flawless. Indeed, no sooner has a pharmacopeia appeared than it has ever been made the immediate object for attack. This has been the case in Germany even more than in the United States. This criticism of "official statements" is true to-day concerning scopolamin.

While there may be little discussion over the identity of absolutely pure hyoscin with absolutely pure scopolamin, it has been pointed out—and especially in Germany—that the official (pharmacopeial) tests for purity are entirely inadequate. In the United States Pharmacopeia the tests for hyoscin hydrobromid (or scopolamin hydrobromid) are, from a practical viewpoint, for identity only; as regards impurities they are worse than weak, and you ought to know this to be a fact.

Indeed, the only tests of this character distinctly given are for carbonizable impurities (sugar, gum, etc.) and morphin, the grossest of adulterants! There is no mention of the optical rotation tests; no suggestion of the possibility of the presence of the really common and as regards the second often exceedingly dangerous impurities, atroscin and apotropon. Indeed, a sample of scopolamin which apparently perfectly satisfied pharmacopeial requirements might be loaded with these substances and be correspondingly dangerous.

The same is true of the German Pharmacopeia; and in Germany much more has been said on this subject than in America. Says Kobert (*Riedel's Berichte*, November, 1904). "The commercial preparation (scopolamin) are obviously a mixture of active scopolamin with some other base. Hence, there should have been introduced into the latest German Pharmacopeia tests that would assure the purity of the active scopolamin hydrobromid. This requirement was aimed to be fulfilled by demanding about 12 per cent. water. But this criterium is not worth very much. About optical activity the book unfortunately says nothing, and about the melting point, not the right thing, so that a *slightly hydrous atroscin might even be preferred to the true active scopolamin*. It is not strange that many practitioners have not been and are not giving their patients the active, but a nearly inactive (optically inactive) article." A powerful article, thus unknown, may, therefore, conceal any degree of danger.

The impurities above mentioned, and which as we have shown, have been common in scopolamin are, in the main, atroscin and apotropon. Neither of these is readily detectable by purely chemical tests, the optical rotation being depended on to determine their presence. Pure scopolamin and pure hyoscin are levo-rotatory; atroscin and apotropon are optically inactive—do not cause deviation of light rays. Atroscin is not toxic; apotropon is toxic and it is now believed that the deleterious side-effects occurring when impure scopolamin is given are due to the presence of this very toxic alkaloid. Its exclusion, therefore, as we have claimed, is a matter of the utmost importance, especially when the remedy is to be given to its maximum effect, as in the induction of hyoscin-morphin anesthesia—hypnotic narcosis.

Says Kobert (*Riedel's Berichte*, November, 1904): "Since the presence of inactive scopolamin (atroscin) is without deleterious influence, there necessarily must be contained in the scopolamin of commerce some other substances, optically inactive, but pharmacologically very active and very toxic." Again he says: "Scopolamin in general, or at least that of particular factories, is a most questionable preparation, the use of which not only can not be recommended with a clear conscience, but it must be warned against." These deleterious and dangerous effects he ascribes to the presence of apotropon, saying: "The presence of this base, therefore, in the scopolamin of commerce would in man give rise to precisely the same side-effects as observed by Lewin and Guillary (local irritation, psychic excitation, spasms, delirium, dizziness, cyanosis). Hence I venture to contend that all commercial specimens of scopolamin hydrobromid which have produced (and are so doing to this day) the above deleterious effects were contaminated with this base."

This is practically the same conclusion reached by other German investigators, and men like Gauss, who have used scopolamin with the greatest success have insisted most strongly on the necessity of having a drug of the utmost purity—one entirely free from this toxic base, apoaotropin. And it is a striking fact that the men who have had the highest success with scopolamin and have become enthusiastic in its use (and are now largely using pure, hyoscyamus-derived hyoscin) are those who have insisted on the use of something better than commercial scopolamin; while those who have condemned it are men like Hocheisen, who have been careless in this most important matter, a carelessness little short of culpable, which our chemical critic, oblivious to therapeutic result, would commend.

In striking contrast with Kobert's broadly scientific insistency for purity is the position of certain members of the Council, and the pronouncement in *THE JOURNAL*, in which the most sweeping statements have been made concerning the danger of scopolamin (likewise hyoscin) while not a word has been said concerning variations in the quality of scopolamin, and the possibility that impurities in the drug (our contention) are a factor in the early reported mortality and present bad side-effects. Apoaotropin is ignored and rotation tests apparently undreamed of. Why? The writers of the attacks on us have leaned heavily on the "authority" of the Pharmacopeia as all-sufficient—in this case a feeble if not "a broken reed."

A word concerning the source of apoaotropin: This substance seemed to be formed in part in the process of manufacture: in part to preëxist in the solanaceous plants. Whether it is equally abundant in scopolin and hyoscyamus we have not as yet sufficient data to determine accurately, but there are certain facts which at least point towards its greater frequency in the former, and its presence or absence should always be and is determined by the conscientious, profession-protecting pharmacist by proper tests of each individual specimen.

There is unquestionable evidence enough of the necessity for the surgeon or obstetrician who desires to secure safe and satisfactory results, using a preparation of hyoscin (or scopolamin) which is free from apoaotropin. The question naturally arises: "Are the preparations of this remedy offered to the profession satisfactorily?" Since the Abbott Alkaloidal Company has been probably the largest purchaser and dispenser of this alkaloid for the last two years our opportunities for investigation have been pretty good. Regarding scopolamin (from scopolin) we find that there is the widest variation as to quality. Samples have been obtainable giving optical rotation tests varying all the way from -2 to -23 . We have in our possession at the present time a sample of scopolamin, made by one of the great German chemical houses, giving a test of -9 . Some months ago we had another testing -7 , this test being verified by Mariner and Hoskins of this city. Within the past week we have obtained another in the open market of this city (also the product of one of the great German houses) giving only -9 . All of these samples, however, would apparently satisfy the pharmacopeial requirements as to purity—likewise our critic, and raise the very mischief at the clinical end. This is the "stuff" the doctor doesn't want.

We have not found the same degree of variation as regards hyoscin from hyoscyamus. Within the last two years we have used a very large quantity of this alkaloid. The lowest degree of optical rotation we have found in true hyoscin was -16 , the highest -22 , the average being in the neighborhood of -20 . In other words the hyoscin obtainable by us has been of remarkably uniform quality—this in striking contrast with the exceedingly variable quality of the scopolamin on the American market during the same period.

You insinuate that we ourselves may not use hyoscin from *hyoscyamus*, and, to bolster up this covert but perfectly obvious slur, you quote from letters received from the representatives of prominent German manufacturing houses. It is true that it would be exceedingly difficult to prove by any chemical test the difference between the hyoscin from *hyoscyamus* and the scopolamin from scopolin; and it is also true that we have not rested our confidence on any such tests. But we are none the less assured that we have not been deceived.

Hyoscin is not made in America. We have bought it (the genuine, subject to all tests for purity) very largely, as *THE JOURNAL* correctly assumes, from Merck & Company, their label, under which we have bought it, our critic to the contrary, being as follows (reproduction from photograph):

POISON

Hyoscin Hydrobrom., Merck
From Hyoscyamus—Cryst.

Merck & Co.

New York.

When we first put out this anesthetic tablet we had no difficulty in obtaining the true alkaloid, labeled "from hyoscyamus." Appreciating from its prompt acceptance by the profession what the demand would be we promptly took all we could get. Later the New York representatives of the firm found difficulty in furnishing us with this product in the quantities desired (which is natural enough when it is remembered that the proportion of hyoscin in hyoscyamus is very small, and the true alkaloid, therefore, correspondingly more expensive to produce). We insisted, and Feb. 14, 1907, wrote them as follows:

We desire to know, positively and definitely, from you if the hyoscin hydrobromid that we are getting from you is true to its label—if it is made direct from hyoscyamus and not from scopolin. Answer positively and by wire with confirmation and details by letter. As to the claimed chemical identity of the two substances we are familiar and we do not want the scopolin product. Kindly advise us, and promptly.

In reply to this letter we received the assurance that their sales of hyoscin to us were "from hyoscyamus," though in the same letter they wrote us "we can not promise to keep you supplied with hyoscin from hyoscyamus. Later lots may be made from one of the other sources according to the exigencies of the market on raw material." Now we either did receive true hyoscin from Merck & Company or they deliberately deceived and misled us as to its source, fraudulently labeling their product "from hyoscyamus," under the U. S. guarantee No. 7—which we do not for one minute believe.

Our orders for hyoscin from Merck & Company practically exhausted their supply of this (hyoscyamus-derived) alkaloid, and in the summer of 1907, while they would give us all they could get, they notified us of their inability to supply us longer. We confess that this "stumped" us, and, fearing that we might be compelled, though yet well in stock, to resort to scopolamin, we at that time purchased a supply of the best we could find in the market (optical rotation -23). This supply, however, we are glad to say, is as yet practically intact in our hands, a small amount only having been used for experimental purposes. In November, 1907, a special representative was sent abroad by the Abbott Alkaloidal Company and was able to find a sufficient supply of hyoscin "from hyoscyamus," added to what we are able to obtain from other sources, to supply even our rapidly increasing needs for some time to come.

Now, some may ask, Why not use and recommend high-rotation scopolamin instead of hyoscin? It is probable that if the best quality of scopolamin were always forthcoming this would prove reasonably satisfactory, perhaps quite so. But I think I have shown that a scopolamin even sufficiently pure to meet pharmaceutical requirements, likely to be dispensed by retail druggists and made into tablets by many purely commercial manufacturers, runs great danger of being really impure and dangerous, from the unsuspected presence of apoaotropin. Furthermore, we have found true hyoscin very uniform in quality and the results obtained from its use have been so good that we do not feel justified in making a change; and finally, in spite of "authorities," we remain of the opinion that true hyoscin and scopolamin, or "hyoscin from scopolin" as obtainable on the market, are not of identical quality and action. Our results with true hyoscin have more than justified our confidence in it and our preparation of "hyoscin, morphin and cactin compound" containing it has been endorsed by the profession in no uncertain way.

We drop the question of "identity" here. At this time when we are contending for truth and right as against misrepres-

sentation and fraud, we object to the foisting of hyoscin from scopolia or other mydriatic sources onto the profession. If it is scopolamin say so and let us use it as such if we desire. To do otherwise and to sustain the practice is both to perpetuate and to countenance, aye, to uphold a fraud. Give us a scopolamin equal to and identical with true hyoscin from hyoscyamus, such as we are using, call it scopolamin, not hyoscin, and the fight is off. Purity of drug to produce best therapeutic result is our only contention.

And now a few words concerning cactin—or cactus. This is not a “new” substance. Cactus has been before the profession for forty years. The eclectic literature is full of it, the first studies I have found concerning it dating back to 1864. I might quote extensively the literature on the subject, but it is hardly necessary. The fact is that it is hardly possible to pick up an eclectic publication without seeing something concerning it. The regular text-books have also taken it up, took it up years and years ago, and praise of its action may be found in the works of Hare, Potter, Shoemaker, Butler, Solis-Cohen and others. Thousands of physicians, of all schools, have testified to its therapeutic activity—to its unmistakable, and unquestionable value in properly selected cases. Preparations from cactus are made by many houses.

Cactin, you insist, is a “secret” “nostrum”—because identifying chemical tests were not available—and because only we made it. You question the truth of our statements concerning it; apparently even questioned its source. At a time when split-hair technicalities were not insisted on as now, and on what we then considered good authority, we did years ago list cactin as a “glucosid,” to the tests for which it in a large measure reacts; when this technical error (theoretically true, but not entirely so) was called to our attention we corrected it in our lists and literature as promptly as possible, and so announced through the columns of *Clinical Medicine*.

We certainly endeavored to put ourselves right just as soon as the point was raised, although the truth was, and is, as we then believed and still believe, though seeking “light,” that the glucosidal principle predominates in cactin.

When we were carrying cactin as a glucosid in our list you were still carrying the advertisement of antikamnia. Your Pharisaism concerning past “sins,” therefore, becomes the more nauseating. [The Antikamnia advertisement has been refused by THE JOURNAL since 1895; cactin was called a glucosid in the 1906 price-list of the Abbott Alkaloidal Company.—Ed.]

Finally, in an effort to satisfy, we called and still call cactin a concentration, which is, we believe, as correctly descriptive of the preparation as any term available. The term is used by practically all drug manufacturers: John Wyeth & Co., Sharp & Dohme, and others, including Parke, Davis & Company, in whose catalogue an excellent definition may be found.

Now, as we have repeatedly said, there is nothing secret about cactin, and it is in no sense a “nostrum.” We have freely given to interested doctors coming to our laboratory any information available concerning it; and have invited members of the Council to come and investigate for themselves. They will not come. We will do more.

We have carried cactin in our list for more than ten years. We first made it from the excellent eclectic liquid preparations available, buying them in the open market and concentrating them ourselves. I tried the products of various houses and was best satisfied with that put out by the William S. Merrell Chemical Company, of Cincinnati. I therefore entered into a trade arrangement with them, they agreeing to collect for us large, definite, contracted-for-in-advance quantities of true cactus (there are hundreds of varieties), for which their facilities were unexcelled, and also to prepare, from our specifications, a special concentrated liquid preparation, from which certain known-to-be inert portions of the plant were excluded, this liquid being cask-stored in their laboratories in adequate quantity and shipped to me as required, when final concentration is made in our own plant.

All the details of the method employed by the Merrells I do not know myself, hence could not and can not possibly give them to others. On that point the house writes me as follows:

So far as the process of manufacture is concerned, that brings up an entirely different question, and one which we were obliged to

answer the Council with regard to our normal tinctures and green drug fluid extracts: that our process of manufacture and extraction was the outgrowth of nearly 75 years of labor, study and experience, and it was not reasonable for them to ask us to publish, for the benefit of our competitors, information which it has cost us so much to obtain.

When this highly concentrated liquid cactus (five times fluid extract strength) is received by us from Merrell, it is slowly concentrated at a very low temperature and with extreme care, so that the activity of the drug can not possibly be impaired, the resultant retaining all the active ingredients of the green drug. The points to be remembered are: 1, that cactin is no more a “secret” remedy than is cactus itself; 2, the therapeutic action of cactin is believed to be identical with that of cactus; 3, that it is in no sense a “secret nostrum,” and that any statement to this effect is unfair, unjust and untrue.

Considerable chemical study of cactus has been made in my laboratory, and we have determined the presence of a glucosid, although I am not as yet assured that this is the active or at least the only active medicinal principle contained therein. Mr. R. W. Proctor, chemist for the Merrell Chemical Company, assures us that they have determined the presence of both an alkaloid and a glucosid, but so far we have not been able to obtain from any single principle the desirable and definite results obtainable from the concentration of proximate principles we market as “cactin.” There is room here for work by our erudite critic. Why not get busy?

It will be noted our critic says the word “cactin” should designate a glucosid or some other active principle ending in “in,” with “e” final it should designate a glucosid or a resinoid, and may (and does in this instance) designate a concentration or other as yet indefinite proximate principle, but the unfortunate omission of “e” final in many alkaloids, as well as other definite principles, adopted by THE JOURNAL and some others, has so broken into and broken up efforts at exactness and consistency that one must now know the principle and all about it before he can know what is meant by the names used in your pages.

I must dissent from the theoretical conclusion that because cactin is devoid of a digitalis-like, strychnin-like, or of any toxic action for that matter, it is therefore therapeutically worthless. Clinical reports from real workers in the field are far too numerous and too uniformly favorable to the preparation to be dismissed by unnecessarily inconclusive laboratory experiments, that would deny to the doctor his ability, even his right to judge. Pharmacologic investigations of products of this class are still in their infancy. Practically nothing satisfactory to the clinician has been done along this line. As regards cactus, no manufacturer, so far as I know, has initiated such experiments. My failure to experiment along this line is therefore neither strange nor unusual. Like others, I have had confidence in the clinical results reported by hundreds of competent and distinguished physicians, and in my own experience.

Right here I want to say that you fail to inform your readers that the experiments of Professor Matthews of Chicago University (whose report you say you have in hand ready to publish) were commenced at my initiation and made at my expense. I met Professor Matthews, after arranging for an interview by letter, and it was agreed that he was to make tests of various preparations of cactus, and of cactin alone and in various combinations. I freely gave him permission to publish his studies, he suggesting at the time the *American Journal of Physiology or Science* as proper media. Does this look as though I was afraid of publicity?

As material I supplied him with samples of cactin, both in liquid form and trituration, and also samples of the fluid preparations of cactus of two standard houses. A few days later he telephoned me, “Doctor, you have a remarkable preparation”—telling me some very interesting and flattering things about its action, which, he said, were revealed in his first experiments. That was early in November. November 13 I got a letter reporting negative results, but without details; December 16 I received one more letter (in reply to one of my own) with more negative results—each letter less than a sheet of writing paper, with still no details, no report on anything but cactin—nothing about the other cactus preparations.

That's the last I heard from him. At the time his second letter was received, his work, for which I paid, must have been in the hands of the editor of THE JOURNAL of the Association, who, he must have known, was attacking me and my product. Although I paid for these studies, although they were taken up at my request in my desire to learn all possible concerning cactin, I never received a detailed report of his experiments; have had no opportunity to discuss them with him. Instead of helping me to make my product better, if possible, his secret work, if he did any, is given freely as a further club to my enemies. Is this honorable? Is it worthy of a great university?

Language somehow seems inadequate to express my opinion of a scientific "worker" of this character. I hope I may be excused if I feel some doubts as to the value of reports from such a source.

Meanwhile, I am having careful studies made of this product, both pharmacologic and clinical, by men whose standing and attainments cannot be questioned, broad in scope, and I believe trustworthy in character. I regret that the rapid growth of my business, the limited room as yet available, and the small number of my scientific helpers have made it impossible to do full work of this character in my own laboratory. I shall get to it in time. But in the meantime I do not propose to be stampeded into undue haste by your unfriendly attitude. I have confidence in cactin, and before I get through I propose to know all there is to know about it; to prove that I am right or to acknowledge that I am wrong. I can afford to bide my time—and the multitude of friends who have confidence in my integrity will stand by me, will wait with me till the problem can be worked out. Cactin is one criticized, questioned, product among over six hundred against which it is not dared to raise a hand.

Hyoscin, Morphin and Cactin Compound ("H-M-C, Abbott"), is a good preparation; explain it as you will, it is impossible to "go behind the returns," to deny results—for no preparation offered to the profession within recent years has received such uniform tributes of praise from those who have used it as has this. I have on file letters testifying to its worth from hundreds, yes thousands of members of the American Medical Association, men of the highest standing, who know that I am telling the absolute, unadulterated truth! It is non-secret. The trademark, to which you object, is one of brand only, to designate our preparation. Every manufacturer is free to put up a hyoscin, morphin and cactin compound, leaving off my name and trademark—and indeed, several are already offering such a product. My special claim rests, primarily, on the superior quality of my product, which my brand-trademark designates.

THE ABBOTT ALKALOIDAL CO.,

By W. C. Abbott, President.

[COMMENT.—We have omitted from the above communication considerable matter that refers to the Council on Pharmacy and Chemistry, as it is entirely beside the question, since our article was not inspired by, or submitted to, the Council. We have also eliminated remarks regarding the motives prompting our exposure, as they, too, are irrelevant. In spite of this, we feel that we owe an apology to our readers for printing as much of this "reply," with all the irrelevant matter it contains, as we have done. It is never satisfactory to discuss purely scientific questions with those who are financially interested in the subject-matter under discussion; their solution depends on scientific evidence and not on vehement protestations, assertions of individual integrity or personal reflections on others. What our readers are interested in is the answer of the company to the facts, the direct accusations in our article, which were mainly:

1. That the Abbott Alkaloidal Company's anesthetic contains nothing new in any scientific sense.

2. That there is no difference between hyoscin-morphin-

cactin anesthesia and scopolamin-morphin anesthesia, for the reasons that "hyoscin" and "scopolamin" are merely synonymous terms for the same alkaloid.

3. That "cactin" is a nostrum and is practically inert.

In its reply, the Abbott Alkaloidal Company seems to have been at great pains to obscure these, the main questions at issue, by irrelevant matter and ambiguous verbiage. The predominant impression created by reading this reply is one of "it is and it isn't"—"now you see it and now you don't"—mostly you don't. Nevertheless, we shall attempt to analyze what is said.

1. *The Abbott Alkaloidal anesthesia contains nothing new in any scientific sense.*—This is met by the assertion that this claim was never made; nevertheless, we read again, "our hyoscin combination is superior to the scopolamin combination"—presumably through the substitution of hyoscin and the addition of cactin. The words "new" and "our" occur repeatedly in the literature. No one ever accused the company of having invented "hypodermic anesthesia" in general.

2. *Hyoscin and scopolamin are merely synonymous terms for the same alkaloid.*—The reply to this statement is most interesting from a psychologic standpoint:

We quoted the pharmacopeias as final and legal authorities for the use of these terms. This meets with the reply that the purity tests of the pharmacopeias are not what they ought to be—"do you see it?"

Now as to the most important question: Is the "scopolamin" identical with "hyoscin"? the company has this to say:

"The chemical identity of pure hyoscin and pure scopolamin no one disputes." So this point is conceded with truly remarkable candor. The company is somewhat less clear as to the pharmacologic identity, but does not seem to feel quite safe in challenging, so admits it "for argument's sake." The heavy ordnance is reserved for the "therapeutic identity" in the same paragraph as follows:

"If when pure they are therapeutically identical, we are content; satisfactory therapeutic results and the safety of the clinician is our only desire." Do you see it? If not, peruse the following:

"The men who have had the highest success with scopolamin and have become enthusiastic in its use . . . are those who have insisted on the use of something better than commercial scopolamin." Then they are therapeutically identical? But wait. . . .

"It is probable that if the best quality of scopolamin were always forthcoming, this would prove reasonably satisfactory, perhaps quite so." Then it is conceded that they are identical? Not so fast!

"Finally, in spite of 'authorities,' we remain of the opinion that true hyoscin and scopolamin, as obtainable on the market, are not of identical quality and action." (Italics ours.) Do you see it? When you read this sentence without emphasizing the italics, it sounds like a bold Abbottesque statement that, therapeutically, they are not identical, whatever the facts may show. Then if you read it, emphasizing the italics, it—but oh, fudge! What's the use! *Mir wird bei alledem so dumm, als ging mir ein Mühlrad in Kopf herum!* This is enough; especially since we feel quite safe in leaving to our readers the question whether two substances, conceded to be chemically and pharmacologically identical, can be judged to be therapeutically different on the strength of this kind of medieval logic.

If the company's only or main reason for preferring hyoscin to scopolamin is that the former is apt to be more pure, let it say so frankly, and the subject will become one for intelligent investigation, were it worth while. At present we know nothing about this purely commercial question, except what the Abbott Alkaloidal Company tell us.² This is interesting enough, but what it tends to show is, not that hyoscin should

1. Incidentally, with all that has been said by the Abbott Alkaloidal Company, especially in its journal, the *American Journal of Clinical Medicine*, regarding the treatment it has received from the Council on Pharmacy and Chemistry, it might be stated that the company has submitted to the Council just two products—Nuclein and Neuro-Lecithin—and after a large amount of correspondence between the Council and the company, the Council has finally been able to approve only one of these—Neuro-Lecithin.

2. The fact that a year ago it was possible to find on the market the alkaloid as made from hyoscyamus is not strange, nor does it alter the facts. It is quite possible that this supply had been on hand one, two or even more years, for a few grains would last a long time when there was as little call for it as was the case before the artificial demand was created in this country by advertising. Possibly the manufacturers would still be glad to make the alkaloid from hyoscyamus on a special order; and it would not be necessary to send a man to Europe to place such an order.

be preferred, but that only the pure alkaloid should be used; if the associated apotropon or atropin is really as dangerous as is made out, then the commercial hyoscin could not be safely used; and, according to the company's own figures, it has obtained commercially an even purer scopolamin than hyoscin.

One more thought! This company has been dogmatically asserting that hyoscin is safer than scopolamin. This has been the keynote, the central thought, in the advertising literature. What evidence has it on which to base such assertions, or to assume that there have been fewer deaths from the use of its H-M-C than from the recognized scopolamin-morphin combination? What are the mortality statistics? How many deaths have occurred from the use of the tablets? And especially, what has been the infant mortality from the use of so much morphin in obstetrics? We have seen no statistics published on the subject.

Passing now to the last point:

3. "*Cactin*" is a nostrum and is practically inert.—Abbott replies, "There is nothing secret about cactin." Then he proceeds to describe it as the evaporated residue from a liquid preparation of cactus, from which certain inert portions of the plant are excluded, by a process which is so much a secret that he does not even know it himself! "Do you see it?" What class of constituents is extracted—whether those soluble in water or alcohol or ether or gasoline; what is the relation of the strength of this product to the crude drug—these very interesting data, all of the greatest importance to the scientific prescriber, are veiled in mystery—but not in secrecy!

Finally, as to the action of "*cactin*."

As we understand it, the company now acknowledges that it has no reliable evidence regarding the activity of "*cactin*," and is "having careful studies made of this product, both pharmacologically and clinically, by men whose standing and attainments can not be questioned."

Would it not have been better for the company to have had these investigations made, and have found out whether or not this was a valuable product before advertising it in such an extravagant manner as it has been doing for so many years? Would it not be advisable, also, for this company to await the results of this investigation before repeating these extravagant claims?

After carefully reading the communication from the Abbott Alkaloidal Company, we see no reason for changing the statements made four weeks ago, and reiterate the conclusions then reached.—Ed.]

Queries and Minor Notes

ANONYMOUS COMMUNICATIONS will not be noticed. Queries for this column must be accompanied by the writer's name and address, but the request of the writer not to publish name or address will be faithfully observed.

LEGAL MEANING OF VACCINATION.

BOSTON, Jan. 9, 1908.

To the Editor:—In THE JOURNAL, Dec. 7, 1907, page 1936, Dr. Bradford A. Booth, of Pittsburgh, asks: "What is the legal meaning of vaccination? (If possible cite decision giving such definition.)"

In answer to Dr. Booth, you said: "We know of no legal decision giving the meaning of vaccination."

In "*Commonwealth v. Pear*," and "*Commonwealth v. Jacobson*," (183 Mass. 242), the Supreme Judicial Court of Massachusetts decided: "A defendant charged with violation of a requirement of the board of health of a city or town that all its inhabitants shall be vaccinated, can not introduce evidence to show what a vaccination consists of, this being a matter of common knowledge; or of the grounds of his refusal to comply with the requirements, this involving a matter of opinion, and also being immaterial; or of the alleged injuries or dangerous effects of vaccination, the legislature being the judges of what the welfare of the people demands."

CHARLES HARRINGTON.

PHENOLPHTHALEIN.

AUGUSTA, ME., Dec. 12, 1907.

To the Editor:—1. Can you tell me how phenolphthalein produces catharsis? 2. Is it a safe drug? 3. Do the combinations with menthol, salicylic acid and soap work better than phenolphthalein alone?

S. J. BEACH.

1. Our available knowledge respecting phenolphthalein was given

in THE JOURNAL, March 30, 1907, p. 1133; January 5, pages 64 and 70; April 20, 1907, p. 1351. Its physiologic action has not so far been experimentally determined. 2. No untoward results seem to be reported from physiologic dosage. 3. We think that clinical experience has so far been insufficient to warrant a definite reply to this question.

WATER AS A THERAPEUTIC AGENT.

DEVON, PA., Jan. 3, 1908.

To the Editor:—Your most excellent article on "Internal Use of Water as a Therapeutic Agent," in THE JOURNAL, Nov. 30, 1907, p. 1854, has been read with much interest and profit, and I thank you for it. Your list of qualifications regulating its use is very complete. One must think that had pure water been rather freely used before the conditions which forbid and also those which call for its use had developed, the conditions would probably never exist. One may question whether even in inflammation of the kidneys, weakness of the heart muscle, and insufficiency of gastric secretions, a reasonable use of pure water would not benefit by its solution, and consequent diminution of waste lodged in tissues—either muscular or nervous. Gentle exercise after ingestion of water aids its efficiency. Your suggestion of distilled water is good, yet I remember that J. M. Da Costa in discussing this point once claimed that he was curiously and unaccountably disappointed in the use of distilled water.

WILLIAM M. CAPP.

GERMAN MEDICO-THEOLOGICAL JOURNAL.

CHICAGO, Dec. 30, 1907.

To the Editor:—Some time ago there was an item in THE JOURNAL relative to the establishment in Germany of a journal to discuss borderland questions about religion and medicine. As I can not find it now, I ask you to let me know in what manner it was published or the name of the journal.

DANIEL S. HAGER, M.D.

ANSWER.—The *Zeitschrift für Religionspsychologie* is published by K. Marhold, Halle, Germany. As stated in THE JOURNAL, June 1, 1907, the aim of the new journal is to discuss the borderland questions of theology and medicine, to develop a healthy religious life—the "psychagogics of practical theology," and to study the development of the religious life under various influences. The list of contents of the first number included "Biblical Religious Psychology," "Religious Conscientiousness" and "Obsessions and Religious Practices." The editors are a clergyman and a physician.

BRUSH TREATMENT OF DIABETES?

A PENNSYLVANIA SUBSCRIBER asks where he can find reference to the Brush treatment of diabetes.

THE VON RECKLINGHAUSEN TEST OF HEART FUNCTION.

NEW YORK, Dec. 30, 1907.

To the Editor: Has the v. Recklinghausen method for testing heart function been reviewed in THE JOURNAL at any time? If not, will you give the technic?

N. B. F.

ANSWER.—The method of von Recklinghausen was published in 1901 in *Archiv. f. exper. Path. u. Pharm.*, vols. lv and lvi. It depends on the proposition that the product of the frequency of the pulse by the amplitude of the blood pressure is equal to the amount of blood expelled by the heart in a second divided by the distensibility of the circulatory system or the relative increase in contents of the blood vessels. These relations must be found by determining the systolic and diastolic blood pressure and their difference (the pulse pressure) and also the rate of the pulse. The method appears to be defective because it gives no reliable method of determining the distensibility of the circulatory system; as this is unknown, it is impossible to calculate from it the second's volume, which is the measure of the heart's work. It can not be determined with certainty from the average blood pressure, as has been proposed, because the distensibility of some of the vessels may be increased, while the average blood pressure is maintained at a high level by contraction of other areas of the circulatory system. Some investigations indicate, however, that the distensibility of the circulatory system can not vary greatly, so that the product of the pulse pressure by the pulse frequency may be taken as practically a measure of the heart's activity, especially when it is desired to compare the functional power of the heart at different periods in the same person.

TREATMENT OF LEUCOPLAKIA.

KANSAS CITY, KAN., Jan. 2, 1908.

To the Editor:—Will you kindly publish the most satisfactory treatment for syphilitic leucoplakia of the fauces. In a case I now have it covers the entire tonsils, uvula, back of pharynx and tongue. My medical literature is silent on this subject, and so far I have been unable to stop the progress of the disease.

WM. H. WALKER.

ANSWER.—In most cases, leucoplakia occurs in syphilitics, but this is not by any means always true. Even when occurring in syphilitics, however, it is not a manifestation of active syphilis, even tertiary, but is a parasyphilitic affection and does not yield