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NOTES ON PEPSIN.

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MUCH has been said about physicians' prescriptions being inaccurately compounded, and much fault found with the incompetency of apothecaries and their assistants—frequently, no doubt, without injustice to either party. As much fault may be found with some of the preparations, because of their impurities, and often of the positive inertness of what should be the active principle on which the efficacy of the drug depends. Amongst these preparations, pepsin is particularly alluded to.

The market is flooded with pepsin, of German, French, English, and American manufacture, its elixirs, wines, and troches—elixirs *per se*, and in alleged combination with bismuth, iron, strychnia, &c.—in fact, so elegantly, and apparently therapeutically combined, as to please the eye, taste and judgment of the physician, and by its promised combination, to appear to him as the very thing he wants in his daily practice. All is not gold that glitters, nor is all pepsin that is called so, nor do all its preparations contain the promised principle.

Curiosity at first induced me to examine a sample of Boudalt's pepsin, and getting a negative result, I still more carefully tried three other samples of the same make, and found all inert; I then tried Velpeau's, and with the same result, and then various samples of American preparation, but not one could I find that was in any way a solvent of coagulated albumen. I next tried some elixirs, and not one of these would produce the required result; then some of the wines, and with the same lack of success.

After these experiments, I came to the conclusion that pepsin, as sold in the shops, was a fraud, that physicians were defrauded of their success, and the patient of his health and his means, through the worthlessness of the drug supplied.

But on looking over a review of the new Pharmacopœia, in the JOURNAL of March 13th, I noticed the mention of the pepsin made by Sheffer, of Louisville, and being anxious, if possible, to find a reliable article, I wrote to him for a sample, and received by mail two descriptions—"saccharated pepsin," and "concentrated pepsin." I at once experimented with them, and obtained most excellent results.

VOL. LXXXVIII. No. 21

With one grain of the concentrated article, I obtained the solution of one hundred grains of coagulated albumen, and with five grains of the saccharated, sixty grains of albumen—in each case using one ounce of water and six drops of muriatic acid. With one grain, also, of the former, I procured the solution of 137 grains of raw, lean beef.

These results are, I think, eminently satisfactory, and prove, at least, that there is one reliable article to be had—of home manufacture, also, instead of heavily dutied foreign goods, and which so frequently are considered the only reliable preparations.

I have been sceptical as to the therapeutic value of the so-called pepsin wines and elixirs, and my experiments have proved that, at all events, the pepsin used in their manufacture is not of any use, for there is no solvent action exerted whatever on coagulated albumen by any one of them that I have yet tried.

This may be the proper place to describe the method I adopt for detecting the presence of what I will call active pepsin.

It is well known that the presence of albumen in diabetic urine is an obstacle to the detection of the sugar, and that a fine mauve or purple color is produced on the addition of either Trommer's or Fehling's test solution. It occurred to me that this reaction would just come in for my purpose. Take coagulated albumen, and put into the pepsin wine, or elixir; submit to one hundred degrees Fahr. in a water bath, and if there is any of the active peptic principle present, solution of so much of the coagulum will ensue, and the albumen reduced to an allotropic condition, which, when added to the test solution, would produce the purple color.

On trying the experiment, no such thing occurred; but, on making a simple solution of pepsin in water, adding acid and albumen, and digesting for half an hour, I procured a solution which, on addition of the cupreous test, at once yielded the splendid color; repeated trials have yielded the same results. Flesh, or cheese, may be used in place of albumen, as all that is required is to obtain a peptone, and the reaction will show it; hence proving that pepsin must have been present in order to its production.

I have repeatedly dissolved a perfectly good pepsin in sherry wine, added chlorhydric acid and albumen, and submitted to a water bath and then tested for peptone; but in no case have I been able to detect it. From this fact, I conclude that pepsin wines are useless, as far as the drug is concerned, and that the presence of the alcohol, or the small proportion of tannin, or both, destroys its catalytic action. Tannin, in solution, does the same thing. So, also, do most metallic salts; and here I would quote from Dr. T. King Chambers "On the Indigestions," page 94:—"But for the time named, I advise its being given alone, and the action not interfered with in general by other medicines. *Many will really prevent its chemical effect, and all will confuse one's judgment of the advantage gained.*"

But, if to a peptone already formed, either wine, alcohol, tannin,

or a metallic salt is added, the addition of the test will at once exhibit its reaction, though tannin, if in large quantity, somewhat masks it. From this, it appears that the failure of the test to indicate pepsin in wine or dilute alcohol, or in the presence of tannin, or a metallic salt, does not result from the action being masked, but from the fact of the pepsin (under the conditions) being inert.

If, then, these elegant pseudo-pharmaceutical preparations will not accomplish the conversion of a protein substance into a peptone in the test tube, there is, I think, small likelihood of their doing so in the disabled stomach. It is true the other ingredients may be of service, but often it is the pepsin that the physician is prescribing the compound for, and frequently the one thing the patient most requires; so that, if the above conclusions are true, then, indeed, are physicians laboring in the dark, and their patients done out of their health and their money.

Since writing the above, I have tried samples of Dr. Hawley's preparation, and find that his wine and glycerole of pepsin are good articles, but the wine does not appear to be a pure sherry, and the proportion of alcohol so small as not to interfere with the functions of the principle. They both will dissolve albumen, but not so actively as Sheffer's.

I give these few facts for what they are worth, and hope at least they may be of some service to the profession, especially to those who have not the time for making the investigations they otherwise would do, and to those who may have been disappointed in not obtaining results they may have been led reasonably to expect.

PARALYSIS DURING VARIOLA.

By S. G. WEBBER, M.D.

THE article by Dr. Goss in the number of the JOURNAL for May 8th, recalls to my mind a communication by Westphal, which I had lately read on this subject. Paralysis may occur after variola as after other febrile diseases, typhus, typhoid, yellow fever, and especially it is seen after diphtheria. In most cases, except after diphtheria, paralysis is seen most frequently after the severer forms of the disease, and seems dependent upon the severity. It occurs generally towards the close of the disease, or during convalescence. That it may occur near the beginning is, however, recognized by Trousseau; and Graves refers the causation of the post-febrile paralysis to the initial backache. Both these writers consider that there is congestion or inflammation of the cord.

Trousseau is most clear in the statement of these views. He says: "This backache is not, as has been supposed, a muscular pain, it depends upon an affection of the spinal cord; here is the proof: in rather a large number of cases—and last year within a few days I