fill up the bowel with milk, or meat, or those things that constantly irritate the bowel. With regard to medication, I do not care if it is only water, pure water, and given abundantly outside or inside. I prefer it inside. I do not believe in Brand. I do not believe in being chained down to Brand's method, or Woodbridge's method, or anybody else's method.

Dr. ANDREWS of Pennsylvania—I should like to counteract the influences of the statement made by Dr. McCormick that opium should not be used in hemorrhage of the bowel in typhoid fever. I trust the medical profession will not lose sight of that leading principle in surgery as well as in medicine, that the first requirement in all cases of hemorrhage is absolute rest of the part. It is just as necessary to arrest peristalsis where hemorrhage occurs, or seriously threatens, as it is to quiet the heart in hemoptysis.

Dr. J. E. WOODBRIDGE of Cleveland, Ohio—The gentleman who read the paper did not follow my advice, or did not quote me correctly in every instance. I certainly never said that one dose would cure a patient. One gentleman says that reports of cases are of no value. How, then, are you to establish any theory in medicine? If in one epidemic they have a certain percentage of deaths, and in the following epidemic they have no deaths by a different method of treatment, is that not some evidence that the different method is better than the one first employed, especially if the duration of the disease is greatly shortened? If in the hospital in Massachusetts they had a very large death rate, they changed the method of treatment and had no deaths, and the same physician who applied the treatment in the hospital also applied it in private practice and had no deaths, quite a large number of cases having been treated, is not that some evidence that the one method is better than others, especially when all his brother practitioners using former methods had the same death rate as in previous years? Such instances could be multiplied indefinitely. The cases are numerous.

I have used Professor Osler's name, and I fear he has misunderstood the manner in which I have used it. I wish to say that I used it in the most complimentary way. I took him as the representative of the best thought of the medical profession of the United States and of the world when I quoted him. I have been accused of saying many harsh things, but I want to say that I never said a harsh thing about any member of the medical profession until I was driven to it by harsh duty. Leading members have continued to write me and ask about my management of typhoid fever. I would like to read one letter from a gentleman of wide experience, who said he proposed to write me about my views and apply the method as closely as his intelligence would permit, and if he found that I had misrepresented facts, if he found I did not give him a correct statement he would denounce me before every medical society in the land. He writes me that since commencing the treatment of typhoid fever by this method he has employed it exclusively and he would not now use any other. All of the patients recovered; none had sequelæ. In one the temperature went two or three degrees higher within two days after beginning the treatment, but it then came down and the patient made an early recovery. That is only one letter out of perhaps fifteen hundred which I have received from various physicians. If I am wrong, there are a great many physicians in the United States who are wrong. The gentleman whose letter I have quoted has charge of a hospital through which thousands of cases pass annually; he knows typhoid fever when he sees it, and he would have denounced me if he had not secured the results which I had promised.

As to one remark of Dr. Osler—I beg his pardon—that cases of typhoid with constipated bowels did better than those with diarrhea. The diarrhea of typhoid is evidence of poison, and if the patients are treated by the antiseptic system there can be no diarrhea, but every single case will be constipated. If antiseptics are properly used it is not possible to have diarrhea, or to get along without using saline laxatives in addition to the little tablets.

Dr. OSLER—Could you not have diarrhea from fermentation?

Dr. WOODBRIDGE—In the antisepticised alimentary canal you can not have diarrhea. You will have constipation in spite of all you can do, and you will have to fight that from the beginning to the end of the disease. I can give these tablets and get up passages next day; I may get them the day following, but not on the fifth, sixth, seventh or eighth day. You can not give enough of the tablets for such an effect. In one hospital it was said they were given ten at a dose, yet it was necessary to give Hunyadi water. Those are only a few facts.

Dr. ———— I have looked into the Brand system, and have been much pleased with the results. It is applicable to the hospitals in our large cities. Now we need something which can be applied by the country practitioner in houses where

there is no bath tub. If we can get just as good results from the Woodbridge method, or the McCormick method, let us have both of them tried. I feel that the professors in our great hospitals would do a very great service if they would put these several methods on trial side by side in our great hospitals, and announce the result for the benefit of the general practitioner. It is a notorious fact that physicians are so prejudiced against the antiseptic treatment that they are afraid to give it a trial.

Dr. MCCORMICK—It would appear to be a piece of presumption on my part, being but a country doctor, to read a paper here that would in any way lead this Association to believe that the college professors and the makers of text-books were not teaching the proper teaching. But, somewhere early in my education as a physician, I conceived the idea that the cure of disease was the ultimate object and aim of the physician, and on that basis I have practiced medicine ever since. So far as the hemorrhage of typhoid fever is concerned, I wish to say to my friend, Professor Andrews of the Medico-Chirurgical College, that no stronger impulse to abandon opium was ever made upon my mind in case of hemorrhage in typhoid fever, than when I was called in consultation by a young man who had just left his college to see a woman who had had hemorrhage and had lost considerable blood. The doctor gave this woman a hypodermic injection of a quarter of a grain of opium. At the end of fifteen hours she had another hemorrhage and he gave her another injection. After this I saw her in consultation. She was swollen like a drum. I said I thought I would not have given her the second dose. He replied that he was never taught anything else. That woman died, and I am as firmly convinced as I have existence that she would not have died if she had not received that injection of morphia. If one single principle has been demonstrated in medicine it is that opium and morphia are no longer the agents with which to treat inflammation in the abdominal cavity. The surgeons long since taught us that we must stop giving opium in these diseases, and if we must stop giving opium in surgical diseases, why not more so in medical diseases? I believed the college professor was infallible; I believed the text books were infallible. But when my patients died under the treatment they told me to give them I made up my mind something was wrong.

Dr. OSLER—As to college professors and authors of text-books, they are just as fallible as the rest, just. But they have eyes and ears, and some of them common sense, and those who have common sense when they have a case of typhoid fever which is bleeding will give opium and morphia.

SUMMARY OF VIEWS EXPRESSED AT THE DISCUSSION ON SERUM DIAGNOSIS AT THE MEETING OF THE AMERICAN MEDICAL ASSOCIATION AT PHILADELPHIA.

The committee appointed by the Chairman of the Section on Practice of Medicine make the following report:

1. In selecting the material used in making the test the choice between: *a*, serum, *b*, dried blood, *c*, fluid blood, and *d*, blister fluid, will depend largely upon whether the object be scientific research, clinical diagnosis in hospital or private practice, or public laboratory diagnosis where the samples have to be sent some distance.

2. In spite of considerable variation in technique, there has been a remarkable uniformity in the results obtained by those taking part in the discussion, and their average of about 95 per cent. of successes agrees with the general average of the cases, nearly four thousand, thus far recorded in medical literature.

3. Each of several methods of technique advocated may thus give good results in the hands of those thoroughly familiar with the details found necessary in each case and the sources of error to be avoided, success depending rather on being perfectly familiar with one method than on the particular one selected.

4. For routine diagnostic work even the very simplest methods may give good practical results, but for recording scientific observations those methods which are accurately quantitative should be selected. This is especially necessary in reporting exceptional cases at variance with the general results recorded or where the observations are made the basis of generalizations.

5. A complete reaction should comprise both characteristic clumping and total arrest of motion occurring within a definite time limit. For practical diagnostic work a dilution of 1 to 10, with a fifteen minute time limit, is convenient. In any doubtful case the dilution should be carried as far as 1 to 50 or perhaps 1 to 60, and a reaction not obtainable at that point should

not be regarded as perfectly conclusive. For these higher dilutions the time limit should be extended to two hours.

6. Intensity of reaction in a given serum should be estimated by determining the degree to which it may be diluted without losing its power of giving a decided reaction, both as to agglutination and loss of motion.

7. The intensity of reaction shown by the same serum is influenced by the age, condition and virulence of the test culture and by the composition and reaction of the culture medium. For purposes of comparison the sensitiveness of the test culture should be taken into consideration.

8. The evidence so far recorded establishes that the reaction may be delayed or occasionally may not be obtained in cases of genuine typhoid infection; and also that it may be exceptionally present in non-typhoid cases, though not in an intense degree.

9. In investigating exceptional and contradictory results the following circumstances have to be considered: *a.* The uncertainty of clinical diagnosis. *b.* The absence of bacteriologic or other confirmatory methods of diagnosis during life, giving decisive negative results. *c.* The possibility of overlooking typhoid infection even postmortem, in the absence of characteristic intestinal lesions where a very thorough bacteriologic examination has not been carried out.

10. The modifying influences mentioned above suffice to explain the divergencies existing in the reports of different observers. Without being absolutely infallible the typhoid reaction appears to afford as accurate diagnostic results as can be obtained by any of the bacteriologic methods at our disposal for the diagnosis of other diseases. It must certainly be regarded as the most constant and reliable sign of typhoid fever, if not an absolute test.

N. B. The above summary, while expressing the general consensus of opinion brought out during the discussion on serum diagnosis before the Section on Practice of Medicine of the AMERICAN MEDICAL ASSOCIATION, does not claim to represent exactly the individual views of any one of those who took part.

W. H. WELCH,	E. B. BLOCK,
WYATT JOHNSTON,	H. M. BIGGS,
J. H. MUSSER,	N. S. DAVIS, JR.,
R. C. CABOT,	M. W. RICHARDSON,
S. S. KNEASS,	J. B. HERRICK,
A. C. ABBOTT,	A. R. GUERARD,
J. M. SWAN,	A. P. OHLMACHER,
	Committee.

NEUROLOGIC PROGRESS IN AMERICA.

Presented to the Section on Neurology and Medical Jurisprudence, at the Forty-eighth Annual Meeting of the American Medical Association, at Philadelphia, Pa., June 1-4, 1897.

BY C. H. HUGHES, M.D.

ST. LOUIS.

America has contributed much to the creation of modern neurology, especially to neuriatry or clinical neurology. By the term neurology I include normal neurology and abnormal neurology, which I term neuriatry, and psychology and its opposite morbid aspects psychiatry. In this great historic city one of its most honored names in medical and political history, Dr. Benjamin Rush, was the pioneer psychiatrist and neuriatrist¹ (if you will permit the coinage of this word) of the century. The consideration of mental aberration received much attention at his hands. It was here that insanity first had a hospital set apart to its victims, and inebriety was treated as a disease under the name oinomania. This great physician and statesman made a study of alcoholism, its causes and consequences and boldly proclaimed it the drink disease in advance of all the world, just as Ephraim McDowell in Kentucky had boldly cut into the abdominal cavity and made the first successful laparotomies, thus opening the way to the greatest of the present life-saving triumphs of modern surgery, as he had also opened the way for that great declaration of

principles that made this a free independent and invincible nation.²

No record of American medical progress, no tribute to American medical glory is complete without Pennsylvania, and no literary monument commemorative of the the profession's grandeur and honor could stand whose pedestal is not placed in this city of medical greatness and brotherly love. He not only took a seat in the Continental Congress and made a preliminary draft of and signed the final Declaration of Independence, but he laid the foundation, with his eminent colleagues, broad and deep for the first, and still living and vigorous, American medical college. But he did more in neuriatry than I have mentioned. He recognized the neuropathic diathesis which he called the "phrenitic predisposition," "a union of diffusive morbid irritability involving the nerves and that part of the brain which is the seat of the mind," and he notes the disposition and capacity of certain lunatics to simulate sanity for a purpose. He also notes how the incipient delusions of the insane are mistaken for the cause and relates a case of delusional mental infidelity in a woman "of great medico-legal significance" (*op. cit.*, p. 44). He recognized and named a phase of affective or moral insanity before the term had been invented in what he called the lying disease.

Rush's definition of illusion, "a waking dream," is the briefest on record, and his amplification of it, as "a false perception, in the waking state, from a morbid affection of the brain," is perhaps the best on record. His presentations of the morbid aspects of love, grief, joy, fear, anger, reverie, the moral faculties and, in fact, of all the passions and of the morbid state of the sexual appetite, are unique and advanced for his day; described in 1793 a sexual pervert who answered the doctor's question as to desire and excess thus: "*Dixit per annos tres, quinque vices, se coitum fecisse in horis viginti quatuor, et semper semine injecto,*" antedating the records of Arnold, Krafft-Ebing and others on this subject.³

He was familiar with sexual tabes dorsalis, hypochondriasis, etc., and from 1804 to 1807 records four cases of madness due to onanism. He took the psycho-somatic view of insanity, such as the best alienists hold today, saying that "did cases of madness reside exclusively in the mind, a sound state of the brain ought to occur after nearly every death from that disease," whereas he knew of "but two instances upon record of the brain being found free from morbid appearances in persons who have died of madness."

Here is sound modern psycho-pathology and a correct psycho-somatic conception of the nature and definition of insanity such as we hold today. He denied that insanity was "an ideal disease" and asserted that "epilepsy was a bodily affection."

He had a correct conception of aphasia as a brain trouble, though he neither knew its precise seat nor its name as we call it today. Although he believed too much in venesection, as we believe too little, his

² Though not germane it is a source of professional pride to note the historic fact as a "member of the provisional conference of Pennsylvania and chairman of the committee to which was referred the great question whether it had become expedient for Congress to declare independence," he made a "report which was adopted and sent to the Congress the same day." This was a "most animating document, most probably written by Rush, as he was chairman of the committee and ever ready with his pen." The whole committee consisted of himself and Col. James Smith, and "included all that has been so much praised in the declaration attributed to Thomas Jefferson, of which it might appear to have been the protocol" (Stone's "Eminent American Physicians and Surgeons," p. 437). Rush also went into the Continental Congress, knowing he was to vote for independence, for he was elected to fill a vacancy created by the resignation of a member who declined to remain and vote for separation.

³ Vide p. 348, "Diseases of the Mind," Philadelphia, 1812.

¹ I certainly think the time has come for the invention and use of this term. A term I have employed for many years in my lectures, explaining its derivation and necessity.