

## Correspondence

### SERUM DIAGNOSIS OF SYPHILIS

*To the Editor:*—May I point out some fallacies in Dr. Noguchi's answer to my criticism of his proposed new method of performing the Wassermann test in Army and Navy laboratories?

It is true that the presence of native antisheep amboceptors in human serum may cause errors in the Wassermann test when performed with a sheep hemolytic system. His statement, however, that this factor is uncontrollable is not correct. As a matter of fact I am quite certain that most careful workers who use the sheep hemolytic system remove the native antisheep amboceptors from serum before testing it.

Dr. Noguchi frankly admits that with his method a moderately positive serum containing an excess of native complement may give a weaker reaction than would a serum with an average amount of complement. Then by what process of reasoning can he reach the conclusion that a weakly positive serum may not give a negative reaction under the same conditions?

Dr. Noguchi claims that there is no great variation in the complement content of human serums. This is certainly not in accord with the teachings of most authorities on this subject, and is not in accord with my own experiences. During the past two years I have made a large number of native complement determinations on serums which were to be tested in the raw state, and have found the variations quite considerable. A detailed report of this work is now in the course of preparation and will soon be published. It will be sufficient here to point out the fact that some of the procedures advised by Dr. Noguchi for his new test themselves betray a recognition of the existence of this wide complement variation. After the addition of corpuscles and amboceptor to his tests he incubates for one-half hour. At the end of this time some of his controls are completely, and some only partially, hemolyzed; as a matter of fact, a few may have been hemolyzed for from ten to fifteen minutes. Those serums which do not show complete hemolysis in the controls at the end of the half hour period receive another unit of amboceptor and are incubated for another half hour. Those serums which even then do not show a completed reaction are retested with negative human serum as complement. We here recognize variations in complement content ranging all the way from an amount sufficient to bring about complete hemolysis in fifteen minutes with one unit of amboceptor to an amount that will not complete hemolysis in one hour with two units of amboceptor. That these differences represent considerable variations in complement quantities will be readily appreciated by any one who has ever done a complement titration.

As an example of the importance that authorities place on the need of uniformity in complement strength, I may quote C. F. Craig (Observations on the Noguchi Modification of the Wassermann Complement Fixation Test in the Diagnosis of Lues in the Military Service, *Jour. Exper. Med.*, **12**, 726. This refers, of course, to the old Noguchi test). Craig adopted the Noguchi test but used inactivated serum, because he feared that with noninactivated serums a certain percentage of false positives would be obtained. He insists on the careful titration of complement and says, "A whole series of tests may be rendered valueless by a slight loss of strength in the complement."

Dr. Noguchi surely deserves the thanks of all laboratory workers for having devised the technic for preparing acetone-insoluble lipoids for antigenic purposes. I am ready to admit that a good antigen of this kind is usually more highly antigenic and less anticomplementary than one obtained by any other method. That it is anticomplementary, however, is evidenced by the fact that we make anticomplementary titrations on it. No matter what fraction of the anticomplementary unit we accept as our antigen dose, it always carries with it a corresponding fraction of anticomplementary qualities. As I have pointed out in my previous communication, the anticomplementary activity of antigens varies for different serums. Now then, given a serum with the minimum com-

plement content, or one that in Dr. Noguchi's method requires twice the amboceptor dose and from two to four times the hemolyzing time that a maximum complement serum would, and which is unduly sensitive to the anticomplementary qualities of the antigen, it is readily conceivable that a false positive reaction would result.

Dr. Noguchi fails to see any objection to his procedure of adding a second dose of amboceptor to serums whose control tubes are not hemolyzed at the end of the half hour period. I would again refer him to the paper by Ottenberg mentioned in my previous communication, where he will find discussed the fallacy of the assumption that complement deficiency can be safely compensated for by amboceptor excess, and which, I am sure, will convince him that such a procedure is not a safe one.

After all has been said, the fact remains that Dr. Noguchi's proposed new test ignores a complement variation the maximum of which is represented by a hemolyzing time of fifteen minutes, or possibly less, with one unit of amboceptor and the minimum by a hemolyzing time of one hour with two units of amboceptor. If this is permissible, then all our conceptions of complement fixation tests must be revised. Then can a chemist also properly make determinations with sodium hydroxid solutions ranging all the way from a tenth normal to a fiftieth normal, and make all his calculations as though he had been working with a twentieth normal.

Dr. Noguchi's old test is a model of scientific accuracy compared with his present method. And yet Kolmer says, "A positive Wassermann reaction, however, is better evidence of the presence of syphilis than a positive Noguchi reaction, because of the possibility of false complement fixation occurring in the latter when active serums are used." With a good acetone-insoluble lipoid antigen he finds these false reactions to be about 2 per cent. McFarland says, "Unfortunately, it [the Noguchi test] seems to have the demerit of occasionally finding the reaction in negative cases, which is a serious defect." Kaplan says, "All things being equal, the method is fairly reliable, and in my hands gave a positive error of 2.5 per cent." These quotations represent the general opinion of authorities. If Noguchi's old method has a positive error of about 2 per cent., his new method surely has a much larger positive error and also, in addition, a negative error.

What a reflection it would be on science if considerably over 2 per cent. of all those of our Army and Navy who will be subjected to complement fixation tests were to be falsely stigmatized with syphilis. Surely these men now face enough hazards without adding this one. I am still firmly convinced that to use the test proposed by Dr. Noguchi in Army and Navy laboratories for diagnostic purposes would be a great injustice to our soldiers and sailors.

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### BÁRÁNY CHAIR TESTS AND FLYING ABILITY

*To the Editor:*—In an article on "Bárány Chair Tests and Flying Ability" (*THE JOURNAL*, April 13, 1918, p. 1064), we presented certain data concerning the Bárány chair tests of 100 naval aviators, and found an absence of correlation between the tests and the flying ability of the men. While, of course, the Bárány tests have in no way been intended to predict the flying ability of applicants for aviation, nevertheless, men who have failed to respond normally to these tests have in most instances, it is understood, been rejected, and rejected solely because of that failure. In other words, the tests have been used to predict inability to fly. If abnormal reactions constitute sufficient grounds for rejecting applicants, then perhaps it may be well to see what actually successful fliers do in a Bárány chair, and what, if any influence flying has on their reactions.

Table 1 shows the results of the nystagmus times of fifty aviators, all of whom were examined twice by the same examiner. The first examination was given before the man's flight instruction began, and the second after he had qualified. The interval between these two examinations varied from

forty-six to eighty-eight days. The tables were compiled and submitted by Asst. Surg. C. J. Brown, U. S. Navy, at the Naval Air Station, Pensacola, Fla.

TABLE 1.—NYSTAGMUS TIMES OF FIFTY AVIATORS

Name	Nystagmus Time
1. J. E. . . . .	20 25
After 60 days . . . . .	25 25
2. F. B. . . . .	19 19
After 47 days . . . . .	8 2
3. E. B. . . . .	23 21
After 81 days . . . . .	19 19
4. W. A. B. . . . .	26 21
After 51 days . . . . .	21 18
5. J. S. B. . . . .	27 25
After 84 days . . . . .	15 12
6. J. S. B. . . . .	20 11
After 71 days . . . . .	10 5
7. K. H. C. . . . .	22 19
After 60 days . . . . .	20 17
8. D. D. . . . .	21 21
After 67 days . . . . .	22 24
9. S. S. H. . . . .	24 27
After 47 days . . . . .	11 13
10. A. L. H. . . . .	19 17
After 46 days . . . . .	28 22
11. L. B. H. . . . .	20 19
After 55 days . . . . .	22 19
12. S. C. K. . . . .	22 19
After 67 days . . . . .	14 20
13. K. B. K. . . . .	12 11
After 51 days . . . . .	14 14
14. A. J. L. . . . .	24 25
After 66 days . . . . .	25 27
15. R. L. F. T. . . . .	20 19
After 63 days . . . . .	10 13
16. S. P. W. . . . .	30 26
After 84 days . . . . .	18 21
17. H. M. W. . . . .	20 14
After 62 days . . . . .	17 17
18. W. M. W. . . . .	18 17
After 69 days . . . . .	18 17
19. S. A. . . . .	21 20
After 65 days . . . . .	13 15
20. R. L. A. . . . .	18 22
After 70 days . . . . .	15 16
21. G. T. B. . . . .	19 20
After 68 days . . . . .	15 15
22. T. M. B. . . . .	12 13
After 65 days . . . . .	13 14
23. E. D. B. . . . .	16 21
After 82 days . . . . .	15 19
24. J. F. B. . . . .	28 24
After 85 days . . . . .	12 15
25. E. J. B. . . . .	25 26
After 72 days . . . . .	23 20
26. S. T. C. . . . .	19 17
After 58 days . . . . .	20 20
27. H. R. D. . . . .	28 30
After 85 days . . . . .	38 30
28. J. A. E. . . . .	22 20
After 79 days . . . . .	24 21
29. L. A. H. . . . .	27 22
After 56 days . . . . .	21 19
30. R. H. . . . .	8 11
After 61 days . . . . .	10 12
31. G. S. H. . . . .	24 22
After 84 days . . . . .	12 14
32. C. F. K. . . . .	19 24
After 67 days . . . . .	19 17
33. L. H. L. . . . .	17 15
After 66 days . . . . .	14 10
34. R. R. L. . . . .	25 22
After 67 days . . . . .	20 18
35. T. N. M. . . . .	28 24
After 71 days . . . . .	20 21
36. F. R. M. . . . .	24 20
37. J. C. N. . . . .	10 13
After 51 days . . . . .	14 13
38. G. T. O. . . . .	18 22
After 85 days . . . . .	32 30
39. W. L. P. . . . .	17 22
After 47 days . . . . .	19 33
40. H. M. P. . . . .	22 23
After 84 days . . . . .	20 22
41. G. L. R. . . . .	18 17
After 86 days . . . . .	21 20
42. W. T. S. . . . .	23 17
After 52 days . . . . .	16 18
43. C. M. S. . . . .	22 25
After 69 days . . . . .	16 20
44. J. F. S. . . . .	24 21
After 88 days . . . . .	13 15
45. W. W. T. . . . .	11 10
After 52 days . . . . .	12 8
46. J. H. T. . . . .	25 24
After 69 days . . . . .	22 19
47. I. B. T. . . . .	10 19
After 78 days . . . . .	10 19
48. V. F. V. . . . .	17 19
After 85 days . . . . .	13 15
49. A. K. W. . . . .	20 22
After 51 days . . . . .	17 18
50. C. H. W. . . . .	17 15
After 67 days . . . . .	0 7
Summary	Nystagmus
Total days . . . . .	3,383
Number of aviators . . . . .	50
Average Number . . . . .	67.6
Least Number of days . . . . .	46
Greatest number of days . . . . .	88
	Normal in both examinations 26
	Normal first, abnormal second 13
	Abnormal first, normal second 2
	Abnormal both examinations. 9
	Abnormal in one or both. . . . . 24

From this table it will be seen that at the first examination thirty-nine of the fifty had normal reactions, eleven abnormal reactions, if we consider twenty-six seconds as the average duration of nystagmus in normal individuals and sixteen seconds and thirty-two seconds the lower and upper limits of normal nystagmus time. At the second examination twenty-eight were normal and twenty-two abnormal, thirteen aviators dropping into the abnormal class after flying, and nine giving abnormal reactions in both examinations. The abnormal reaction in all but one instance consisted of a nystagmus time of less than sixteen seconds. In 56 per cent. of the men, nystagmus time was very positively and definitely decreased following a period of flying. In THE JOURNAL, May 25, 1918, Lewis and Pike deny the actuality of the decrease in nystagmus time after numerous flights. They refer to mumps and syphilis as the probable causes in any cases coming to our knowledge in which nystagmus time has been diminished. We have been unable to substantiate either disease as the cause in any of the cases reported.

We are further informed by Assistant Surgeon Brown in his paper read before the Medical Society at Pensacola that in an examination of 250 qualified aviators, while their falling tests were normal in all instances, in 66 per cent. of the cases an abnormal past-pointing test was obtained. In an examination of 402 men who have qualified as fliers or are likely to qualify in the near future, Dr. Brown found but ninety-seven men with normal Bárány reactions.

Much the same sort of results are obtained in examining individuals whom we know have excellent senses of balance and equilibrium, such as whirling dancers and trapeze performers. This fact is also denied by Lewis and Pike in their article referred to above. We have examined only a very small number of these individuals, six in all, but in all of the six cases obtained abnormal reactions. They are given in Table 2.

TABLE 2.

	Nystagmus		Past-Pointing				Falling	
	R-LN	L-RN	To R	To L	To R	To L	R.	L.
E. H., whirling dancer	18	17	6	1	1	1	None	None
Mrs. E. H., whirling dancer	12	13	1	1	1	1	None	None
C. M., circus trapeze performer	19	19	3	2	1	2	None	None
E. S., circus trapeze performer	19	21	1	1	1	1	To right	To left
F. M., circus trapeze performer	12	17	2	1	1	2	None	None
O. J. S., circus trapeze performer	38	10	1	1	1	1	None	None

It is interesting to note that in five of these individuals there was no falling after whirling, in five the past-pointing was markedly abnormal, and in three, the nystagmus times were below normal, in two normal, although both below twenty-six seconds, and in one "mixed," that is, left nystagmus above normal and right nystagmus below.

We believe, therefore, that not only are we justified in our conclusion that abnormal Bárány reactions do not indicate inability to fly, but also that there is no correlation whatever between equilibrium tests as established in the Bárány chair and actual flying ability, and that flying and other practices requiring an acute sense of balance, if indulged in over a considerable period of time, have a very definite effect on the Bárány reactions. To what this effect is due, we are unable to say.

Prof. Raymond Dodge of Wesleyan University, who has for the past year been doing elaborate experimental work along these lines, says:

"The grounds of my scientific conviction that the nystagmus test does not indicate fitness to fly are both practical and theoretical. It seems incomprehensible to a psychologist that these fallacious sensory data, which are really worse than useless to a flier, should have been selected as a test of his ability to fly. It almost seems like a joke of the enemy physiologists to hinder our air service. I believe that it should be abolished as a test, since, while the results are scientifically interesting, they have no practical value in indicating fitness to fly."

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