

occurrence, even in the midst of an epidemic of something else, should not be forgotten, however.

To be sure, there are in this disease meningeal symptoms, but nothing that a simple meningitis caused by some unknown organism could not explain. The fact that in every acute case organisms have been obtained shows that there are bacterial toxins to produce degeneration of the cord and to inflame the cord membranes. Is it not a logical and scientific hypothesis that in this disease the simple meningitis and the poliomyelitis may be caused by the same thing, and that sometimes the infection of the meninges may predominate and at other times the infection of the gray substance of the cord? The mortality is too high for poliomyelitis of the commoner text-books. The percentage of recoveries without atrophy is not according to text-book description. But could we not find organisms differing from all those in the past epidemics if not in kind, in characteristics, which would produce an anterior poliomyelitis and a meningitis? Is it not natural to suppose that in most cases, on account of the blood-supply and the proximity of the membranes to the cord substance, the two infections go hand in hand and that, while poliomyelitis may leave the predominating lesion usually, at any time it would be possible for the meningeal symptoms to appear uppermost?

I wish to thank the doctors of York County who have kindly cooperated in studying this epidemic.

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### JONNESCO'S ANALGESIA METHOD

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By reference to Jonnesco's article,<sup>1</sup> "Remarks on General Spinal Analgesia," it appears that he first advocated this method before the International Society of Surgery, in Brussels, September, 1908, when he reported fourteen cases "by my new method . . ." (addition of strychnin to stovain). At the German Society of Surgery, in Berlin, April, 1909, Professor Bier (Berlin) declared that the method must be rejected; and Professor Rehn (Frankfort), at the same meeting, stated that animal experiments demonstrated considerable danger in injections higher than the lumbar region.

In explanation of this hostility, Jonnesco says that, as he expected, the method would be considered ". . . too novel and too hardy to be accepted without opposition . . ." and he adjures his critics not to be condemnatory on *a priori* grounds, predicting that ". . . my new method of general spinal analgesia will in a short time be universally accepted. . . ." He adds that since October, 1908, he has used this method exclusively in his private and public operations, and he claims to have convinced Professor Schauta (Vienna) of its merits by personal demonstration.

The article then goes on to explain the method of preparing the strychnin sulphate and stovain, with the proper dosage according to the age and general physique of the patient. He advocates only two spinal zones for injection; namely, a "superior" (or "high") between the first and second dorsal vertebrae, and an "inferior" (or "low") between the twelfth dorsal and first lumbar

vertebrae. The former, he claims, is efficient in producing analgesia of the upper part of the body, including the face, head and neck; while the latter permits operative manipulation on all parts approximately below the waistline, and is especially helpful in laparotomies because it produces what he terms "abdominal silence."

### SOME STATISTICS

His total number of personal cases by the "new method" is 412; of these, 117 were high and 295 low injections.

After-effects were notably absent, he says; nausea being noted in 2.25 per cent.; vomiting ("a single effortless ejection") in 1.25 per cent.; sweating in 2 per cent.; headaches in 6.25 per cent.; transitory retention of urine in 4.5 per cent.; in no case did postoperative temperature reach 104°. He never saw any paralysis.

The duration of analgesia in the high injections is about forty-five minutes; in the low, from an hour and a half to two hours. A second injection is given if the operation is not completed within the above periods. He relates cases in which at first he failed to produce the desired effect because he did not enter the arachnoid space in the mid-line; or because the patient made an involuntary movement at the instant the needle was felt, thus deflecting the latter. He, however, has never failed to produce analgesia, although in some cases many attempts are necessary (see below).

This article is illustrated by four large photographs, two of them showing Jonnesco performing his high and low injection, respectively.

During his visit to this country, and up to Dec. 29, 1909, Jonnesco says that he has effected twenty-three spinal analgesias. Eleven of these were in New York hospitals; five in Philadelphia; three in Chicago, and three at Rochester, Minn. (State Hospital). Seven of these were superior dorsal and sixteen low dorsal injections. Commenting on these cases in a letter to the *New York Medical Journal*, Jonnesco says he regrets that newspaper reports make it appear that he claimed to have discovered medullary narcosis; on the contrary, he disclaims any intent to rob Dr. J. Leonard Corning of this city of that honor. Neither does he claim to have discovered stovain or strychnin; but he does assert that his method of combining these drugs is new and original with him, as is his introduction of them into the two spinal zones already mentioned.

### ANALYSIS OF AN UNFAVORABLE CASE

In this letter he explains the unfavorable outcome of the high injection given in Case 3 at the Post-Graduate Hospital (osteoma of left frontal region) on the theory that the patient was an epileptic and had a bad cardiac lesion; adding that ". . . Immediately after the operation began, he [the patient] had an epileptic fit which lasted throughout the whole operation. This should be enough to explain a momentary cessation of the respiration, but the heart never ceased beating and a few moments of artificial respiration were enough to bring him around again. The delirium which appeared after the operation is simply an epileptic delirium, and I cannot understand how anyone can think it was produced by the anesthetic. . . ."

This statement is in marked contrast to the report of the case as given<sup>2</sup> by Dr. Aspinwall Judd, who assisted Dr. Judd makes no mention of an epileptic fit which Dr. Robert T. Morris at the operation, and certainly it does not accord with my observations of the occurrence.

1. Brit. Med. Jour., Nov. 13, 1909, p. 1396.

2. New York Med. Jour., Dec. 25, 1909.

Dr. Judd makes no mention of an epileptic fit which began at the time of and lasted throughout the operation, nor did I witness any such incident. Respecting the "momentary cessation of respiration" it appears from Dr. Judd's report that the operation lasted twelve minutes and that respiration thereafter was very seriously interfered with, and that it required heroic treatment for twelve minutes before respiration was re-established. Those who observed the patient after he left the operating room would hardly agree with Jonnesco in terming the case one of "momentary cessation of respiration"; nor would he have any supporter in his statement that "... a few moments of artificial respiration was enough to bring him around again ... ." This patient had a wild delirium an hour after, requiring a strait jacket for two hours, the delirium recurring at intervals until the following morning. Jonnesco says that this was an epileptic delirium and "... cannot understand how anyone can think it was produced by the anesthetic ... ."

The consensus of opinion respecting this case was that the high injection was exceedingly dangerous, and that the analgesia was far from complete and was responsible for the almost fatal collapse of the patient.

#### A HISTORICAL SUMMARY

Spinal anesthesia was demonstrated by Corning of New York in 1885, but it was several years later before the method was tested surgically, and it was then abandoned as unsafe and uncertain not only as to the immediate but also to the later effects. Since then, especially with the advent of some widely heralded new local anesthetic, medullary narcosis for a time again becomes prominent; but, despite the advocacy of many surgeons in many lands, the method has never gained a strong or lasting following. Jonnesco is the most recent sponsor for this more than twenty-year-old "discovery," declaring that the addition of strychnin to stovain, novocain, the various eucains, alypin, tropococain, etc., robs these local anesthetics of their dangers and makes high or low invasion of the cord safe and reliable.

I witnessed the operations performed at the Post-Graduate Hospital of this city under spinal analgesia induced by Jonnesco, and also visited Chicago and Rochester, Minn., soon after the demonstrations were given in those cities. From personal observation and from conversations with surgeons of my own and the above-mentioned places a consensus of opinion was obtained that seems unanimous both as to the method and the means of inducing this form of anesthesia.

#### THE DANGERS OF HIGH ANALGESIA

Observers of Jonnesco's method condemn high spinal analgesia without exception, because of the known dangers of invasion of that section of the cord, even though a small needle and 1 c.c. of sterile solution are the only foreign bodies introduced. That this opinion is well founded seems proved by recalling that of the seven cases of high spinal analgesia demonstrated during Jonnesco's visit, four resulted unfavorably. One of these was the Post-Graduate case above mentioned; another at New York (superficial chest operation) caused so much pain that the patient asked for chloroform; in a third case Jonnesco was unable to obtain cerebrospinal fluid after repeated trials, but did succeed in withdrawing blood; ether was finally given. In a fourth case reported by Dr. Edward Martin of Philadelphia (amputation of breast) it appears that the patient "... narrowly escaped death. ... Later ether had to be given to control the pain. ... ." Incidentally, Dr. Martin

states that there has been one death and one case of partial paralysis in his city following spinal analgesia by imitators of Jonnesco.

#### THE DANGERS OF LOW ANALGESIA

Respecting low spinal puncture there appears to be some division of opinion as between those who would not advocate it under any circumstances and those who might employ it in the event of some contraindication to inhalation or local anesthesia.

#### THE DANGERS IN GENERAL SUMMED UP

In general, the cogent factors warranting hostile criticism of the method are:

1. Danger of interference with a highly organized section of the nervous system, considering the possibility of (a) puncture of blood vessels, leading to (b) spinal hemorrhage and areas of (c) spinal sclerosis or (d) syringomyelia later.
2. Uncertainty of reaching the arachnoid space and hence failure of analgesia.
3. Psychic shock incident to operations where patients are conscious and appreciative of the sights and sounds occasioned by the occurrence.
4. The advantages do not outweigh the dangers, known and unknown, in a yet insufficiently tried radical departure from older methods.

#### SPINAL ANALGESIA HAS A LIMITED FIELD

It is apparently the general opinion that spinal analgesia will always have a limited application that is best represented by emergency or battlefield operations; and that it will not be employed except for marked contraindications to either inhalation or local anesthesia.

We have had the opportunity of witnessing the demonstrations of an expert, but we were unconvinced that even with his considerable experience and enthusiastic advocacy we are warranted in recommending this method, even though so attractively and widely again offered to a willing but skeptical profession.

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### BISMUTH SUBNITRATE GAUZE FOR USE IN THE NOSE

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After submucous resection of the nasal septum, after inferior turbinectomies, etc., I have been using small strips of bismuth subnitrate gauze, prepared at the time of using by rubbing powdered bismuth subnitrate on gauze bandages, two inches wide and cut to the proper length. These strips were folded to a width of about one-half inch and the packing accomplished by placing one strip of gauze on the other.

Since Dr. Wiener's paper appeared<sup>1</sup> I have been using the bismuth gauze prepared according to his formula. The only objection I have found to Dr. Wiener's gauze is that, being packed in a crumpled condition in a glass tube, it cannot be laid out smoothly for use in the nose, when cut in small pieces.

In order to overcome this difficulty, I have had the gauze put up in a package of a different style, which is far more convenient for my use. The gauze is cut into small pieces, two and one-half inches square, after being immersed in the bismuth emulsion and dried. About fifty of these squares are packed in onion-skin paper, the ends of the paper being so folded that they can be easily lifted

1. Wiener, Solomon: The Use of Bismuth Gauze in Gynecologic Work, THE JOURNAL A. M. A., Oct. 23, 1909, III, 1397.