On the invention of local anæsthesia by refrigeration / letter from Dr. James Arnott.

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[Extracted from the *Medical Times and Gazette* for March 30th, 1867, with Annotations.]

ON THE INVENTION OF LOCAL ANÆSTHESIA BY REFRIGERATION.

LETTER FROM DR. JAMES ARNOTT.

[To the Editor of the Medical Times and Gazette.]

SIR,—In the *Times* of Thursday se'nnight a report appeared of the proceedings at a meeting of the Society for the Prevention of Cruelty to Animals, where Dr. B. Richardson read a paper "On Local Anæsthesia in Surgical Operations," in which, it is stated, "he narrated briefly the line of inquiry which led him to the invention"; and on Tuesday last an advertisement was inserted in the same newspaper respecting the presentation of a testimonial to Dr. Richardson, "chiefly for his successful invention of a method of producing, without danger, local insensibility to pain in operations" implying that there is only one successful method, and that this is Dr. Richardson's invention. I consider these statements to be most erroneous, and I request the opportunity from you, as editor of the journal in which Dr. Richardson published his first paper on local anæsthesia by cold, and in which many of my own papers respecting it have also appeared, of submitting the question to the Profession.

As it is well known to the Profession that the discovery was made by me, many years ago, of the possibility of freezing the animal tissues without injuring them, and that this process might be used beneficially both as an anæsthetic in operations and as a remedy of disease, I need not at present do more than allude to it. What it is more necessary to elucidate is the great difference which exists between the invention of the principle of any new therapeutic proceeding, and the invention of any other means of carrying this principle into effect than those which have been originally proposed. All that Dr. Richardson can claim has relation merely to the

latter. The principle of his method of anæsthesia is precisely that which I discovered in 1848.

He endeavours, indeed, in his first communication (*Medical Times and Gazette*, February 3 last), to make it appear that there is a difference of principle between the two methods. After relating his unsuccessful attempt some years ago to produce anæsthesia by what he called voltaic narcotism, "I came," he says, "to the conclusion that Dr. James Arnott's plan of using extreme cold was the first true step in the progress of discovery, and that if it could be made easier of application, and at the same time could be combined with the use of a narcotic fluid, an important advance in therapeutics would necessarily follow." The supposed second true step, I presume, and the step intended to reach the goal, was (as far as concerns the principle) this combination of a narcotic agency with intense cold, and it was represented to be a very great advance; for by the narcotic fluid (ether) there was to be produced "the deepest anæsthesia," giving the power of amputating a limb painlessly. No wonder that a method of such pretensions should have been so well received by the Profession; but, unfortunately, it was soon discovered that the second "step" was an utter delusion. Dr. Richardson is not the only one who has fallen into this error. The same idea of a combined agency had been maintained by M. Richet the original proposer of anæsthesia by vaporised ether, and who also upon the strength of it, claimed the invention of a principle in some respects new.

As the principle, then, is precisely the same, whether the intense cold is artificially produced by the rapid solution of ice and various salts, or by the rapid evaporation of volatile fluids, upon what ground is the invention claimed for Dr. Richardson, and a large sum of money sought as a reward from the Profession and the public? Granting, for argument's sake, that the means which he employs for carrying out the principle are more convenient than those originally proposed, and that they were first proposed by him, that constitutes no sufficient reason, if the means originally proposed are adequate for their purpose. If great things may be compared with small, the history of vaccination will furnish an illustration of this point. The discovery that the having been affected with cow-pox is a preventive of small-pox is a very different thing from the devising of means of introducing the virus of cow-pox, and Jenner's means have doubtless been altered and perhaps improved in later times. Very recently a French Physician proposed a new instrument for the purpose. But were a modifier of these means to call the alterations made by him the invention of a new and successful method of preventing small pox, his pretensions would only excite ridicule.

It must be admitted, however, that if a new method devised for carrying out any important principle be decidedly superior to that originally proposed—if one is very difficult to practice, and the other, while equally effectual, is very easy—considerable credit is due to the deviser. How is it in the case in question? It is an erroneous statement, although it has served its purpose,

that the application of the cold produced by the rapid solution of ice and various salts is a difficult process. Nothing can be more easy than to dip a bit of ice into common salt and press it gently on the skin, and yet this is sufficient to freeze it in less than the quarter of a minute. In some cases, indeed, a frigorific mixture cannot be properly applied without a cup or vessel of peculiar shape to contain it, which unquestionably involves more trouble than the projection of ether; but in several situations the part cannot be congealed by ether, and it is necessary to use either a freezing mixture, or a metallic ball or oval which has been cooled to the requisite degree by immersion in it. When deep, extensive and long-continued congelation is required, or when it has to be used when inflammation is present either in operations or in the treatment of disease, congelation by a freezing mixture, which can be combined with pressure and applied to any extent of surface, is the only measure which will fulfil the purpose. Simplicity and facility of application are doubtless valuable properties, but efficiency must not be sacrificed to ease.

Questions of minor importance in this enquiry are—When and by whom was anæsthesia by evaporating ether first practised? It was first practised in Paris in 1854, and about six years after the invention of local anæsthesia by cold. M. Richet dropped the ether on the part, and quickened its evaporation by a bellows. Soon afterwards, M. Guérard projected the ether on the part from a syringe of peculiar construction. Then followed the invention of the pulverization of fluids, or their conversion into spray, by Dr. Sales-Girons, the principle of which was, as a matter of course, soon adapted to the construction of instruments for anæsthesia by those who had been employing ether in preference to freezing mixtures. At a discussion on the subject in the Surgical Society of Paris (reported in the *Gazette des Hôpitaux* of March 24 last), it was mentioned by M. Desormeaux that he had employed such an instrument years before, and by M. Velpeau that his pupils at the Hôpital de la Charité had used the more simple expedient of the perfumer's spray-producing tube. Dr. Richardson's instrument is constructed on the same principle, and has been followed by other instruments in France and elsewhere. But the method of producing anæsthesia by freezing mixtures, which M. Velpeau calls in the discussion referred to "*un moyen d' anesthêsie locale très simple et très efficace*," has generally been preferred by French Surgeons.

In concluding this letter, I cannot but express the regret I feel at being compelled either to publish the above statement or to suffer myself not only to be deprived of the credit of an invention which I have so long endeavoured to bring to perfection, but to incur the risk of being charged with making pretensions to a discovery which belongs to another. The public, addressed in the manner they have been through the newspapers, and with no other knowledge of the subject, will not distinguish between the principle of an invention and the means of carrying it into effect; but though I have not the opportunity of explaining the difference through the same channel, I place the task of doing so

with confidence in the hands of the Profession. I am well aware that though a testimonial is not always a reliable indication of an opinion, being often subscribed to from various motives, there is much force in names and numbers; yet if all the circumstances I have stated were known, I do not think that the testimonial referred to would have much weight. It would be interesting to know how many of the subscribers were aware that the principle of the "successful method" mentioned in the advertisement is identical with that which I invented eighteen years ago, and that the means of carrying it out lately introduced into England (inferior, in all important respects, to those which I had employed) is merely a slight modification of a subsequent French device.

I am, &c. JAMES ARNOTT.

8, St. Stephen's-crescent, Westbourne-park, March 23.

As the advertisement respecting a testimonial complained of in the above letter was so little in unison with the facts of the case, I naturally expected that some explanation would be immediately offered by the person who had drawn it up and inserted it in the *Times*. Instead of this, my letter was answered a fortnight afterwards by Dr. Richardson, but without any reference to the advertisement. My immediate rejoinder having been refused insertion in the Journal, not, I think, with the highest sense of justice, I am obliged to notice certain statements in his reply in the present informal way.

Whoever contributes to the stock of useful knowledge in medicine, must lay his account for finding that the difficulty of making a discovery or invention is nothing in comparison with the trouble of retaining the credit of it. Either his originality is disputed from its being found that something in ancient times bears a resemblance to his invention, or, if this be of a mechanical description, it is altered, and, in its modified form, is announced, and too often received, as a thing altogether new. About sixteen months ago, I published a summary of several of the therapeutical inventions which I had made in the course of my professional life, for the purpose, partly, of explaining certain points respecting them which had been misunderstood, and partly of recovering those to which others had attached their names. I hoped that the trouble of this publication would be repaid, if not by regaining what was lost, at least by the security it would afford of preserving what remained. But I was mistaken in this supposition. It appears only to have furnished further evidence of the defenceless condition of rights of this description. After an undisputed possession for so many years of the credit of having introduced the medical use of intense cold or congelation, I flattered myself that this possession was secure. I had little idea of what the advertisement in the *Times* has revealed,—that on the ground of a slight modification of the means of producing congelation, a claim would be founded for

the invention. Dr. Richardson, himself, does not go so far as this in his reply to my letter. He acknowledges his mistake in supposing he had discovered a new principle of local anæsthesia; but he greatly exaggerates the value of the method he employs for carrying into practice the principle which I had discovered eighteen years before.

The "natural" discovery, of which he speaks, that cold is the destroyer of sensation, was alluded to in my first communication on local anæsthesia in the *Lancet* for September 9th, 1848. It is there stated that "we have all had experience of this anæsthetic in frosty weather;" but the dread of gangrene and inflammation from intense cold (a dread which the high authority of John Hunter confirmed) prevented its benumbing effect from being rendered useful in medicine, until my experiments and obvservations had shewn the perfect safety of applying a degree of cold even greater than that which will immediately freeze the part. It is in shewing this, and in ascertaining that the organic change produced by intense cold, instead of impeding, promotes the healing of the operation wound, and that it is powerfully remedial in several important diseases, that my discoveries respecting it principally consist. The most efficient way of applying congelation, and combining it with pressure, has also occupied much of my attention, as I have been anxious to extend its use to the deepest operations and most deeply seated diseases.*

In the reply of Dr. Richardson there is an imposing air of liberality displayed by what he calls his "unbounded recognition of my labours" on Local Anæsthesia in his "papers and addresses." But this recognition was not required by medical societies, most of the members of which, it may be presumed, were already acquainted with one or more of my numerous writings on the subject; and on other occasions it does not appear to have been always made. There is no sign of such recognition in the Report of the Meeting of the Society for the Prevention of Cruelty to Animals, above adverted to, nor in the fact that a Medical Journal over which he appears to have an "unbounded" influence, has in its constant notices of Local Anæsthesia during the greater part of last year, carefully ignored these labours ever since Dr. Richardson's announcement that he had discovered a new way of producing it.

The recognition by Dr. Richardson of my labours has probably been always accompanied by the same depreciation of the means I have employed for carrying out the principle of local anæsthesia, which is expressed in his letter, and which pervades the notices of the subject in the journal alluded to. The writer of one of these (*British Medical Journal*, 10th March, 1866) while he condemns all other plans of producing congelation as "perfect

* The discovery of the uses of intense cold in therapeutics has been followed by its employment in physiological investigations. The fact mentioned by Dr. Weir Mitchell, in his interesting paper on this subject in the *American Journal of Medical Science*, for January last, that stupor or deep sleep is caused by its application to the head of a bird, I ascertained at a very early period of my enquiries.

failures," extols that of ether spray as being "in many respects a more important discovery than ether and chloroform," and "a great national blessing." When writing this passage, he must have been well aware that congelation produced by freezing mixtures had been in general use amongst Surgeons for many years; and he might have learned from reports in other Medical Journals that ether spray had failed in cases where inflammation was present, and when employed in situations where its evaporation was obstructed, or where extensive and rapid incision was required.*

With respect to Dr. Richardson's observations on the origin of the use of ether spray, I have nothing to add to my statement on this point in the *Lancet* of the 19th May, last year, and in the above letter. He states in his first communication on Local Anæsthesia, that he took the idea of his instrument from seeing the recent French invention of the spray-producing tube used by perfumers, and I have remarked in my paper in the *Lancet* that the obvious applicability of spray-producing instruments for anæsthetic purposes seems to have occurred to several Surgeons independently, of whom no doubt, Dr. Richardson was one. He is mistaken, however, in supposing that the French, although they do not value it highly, do not claim priority in devising this mode of producing anæsthetic congelation. "*L'anesthesie locale*," says the Editor of the *Revue Medicale*, (April 15, 1866) when speaking of Dr. Richardson's spray-producer, "*est partie de Paris, instrument, idée et pratique, et qui nous revient par Londres.*"

Various methods of applying intense cold have already been employed, and other methods will doubtless be hereafter devised for particular exigencies in practice. Many of these have their peculiar advantages. The different freezing mixtures are by far the most extensively applicable; the rapid evaporation of volatile fluids, which can be effected by various kinds of mechanism, is the most handy or convenient. If confined to the slighter operations and forms of disease, the latter method will constitute a valuable addition to our means, as, from the difficulty of always procuring ice, it will be employed where, without such a resource, either chloroform would be used, or the operation would be performed without anæsthesia. Ice, indeed, can be easily and economically made when it cannot be procured in its natural state; and there are powerful freezing mixtures of which ice forms no component part, but these would not on ordinary occasions be so convenient as ether. The danger is that, owing to this great convenience, it may be employed in unsuitable cases, and, by its frequent failure, may bring the principle of Local Anæsthesia into disrepute.

* Amongst other alleged advantages of freezing by ether spray, Dr. Richardson states that "slough is altogether prevented as a sequence." When sloughing has been produced by a freezing mixture, it has been in consequence of mismanagement. The *Lancet* of to-day (May 25th) reports the occurrence of sloughing from ether spray at the Middlesex Hospital, where there was no mismanagement. This difference in the results is easily explained.

This observation is especially applicable to congelation in the treatment of disease, which is, perhaps, its most valuable use. It has proved an excellent remedy in cancer, by suspending the vitality of the growth for a short time at intervals, or by destroying the growth quickly by a longer continued application. But such a suspension of vitality can only be effected by the intense and enduring cold produced by freezing mixtures. To talk of the evaporation of ether "removing morbid growths out of the reach of the knife," as has been done in the *British Medical Journal* (page 445, 1866) is absurd. In my work "On the painless extirpation of Cancerous Growths," I mention cases where the tumour was kept under a cold of 12° below Zero of Farenheit for hours. Inflammation is another morbid condition for which, when attacking the joints or other accessible parts, the congelation produced by freezing mixtures is a powerful and prompt remedy. In a tract published ten years ago, I state that "the fact of an adequate degree and continuance of cold so altering the functions of the vessels and nerves of the part subjected to it, as not only to arrest inflammation instantly, but to render the part incapable of this morbid affection for some time afterwards, constitutes, from the extensive applicability of the remedial principle, one of the most important medical truths which have yet come to light" ("On the expediency of instituting an Academy of Medicine, &c.," 1857.) The remedial efficacy of congelation thus produced in rheumatic, neuralgic, and ~~spinal~~ Cancerous affections forms the subject of several separate publications, of which the substance is given in my "Contributions to practical Medicine and Surgery," published at the beginning of last year.