History of modern anæsthetics : a second letter to Dr. Jacob Bigelow / by Sir J.Y. Simpson.

HISTORY

OF

MODERN ANÆSTHETICS,

A SECOND LETTER TO

Dr. JACOB BIGELOW

BY

Sir J. Y. SIMPSON, Bart.

My Dear Sir—A few months ago I saw in an American general newspaper the gratuitous attack upon me which you had published in the *Boston Medical Journal*, but of which you had forgotten to send me either an intimation or a copy—doubtless from accident and not from intentional discourtesy. Towards the beginning of the present year, I sent, in reply to your groundless accusation, an answer in the form of a letter to yourself; and subsequently I received from you a written note in which you stated you were "not disposed to pursue the subject further." In consequence, I dismissed the matter entirely from my mind; and I deeply regret, both for your own sake and for the peace and character of our honourable profession, that you have not adhered to your resolution. For I have just received a slip of printed statement, unaccompanied by one word of writing, but drawn up in the form of another letter from you to me, in which you continue the subject in terms perhaps still more bitter and personal than before. On first perusing it, my impression was that it was too querulous in tone and temper to deserve an answer. I then thought of sending back a reply to you, stating, simply and briefly, that the new imputations in it were, one and all of them, without a shadow of foundation in fact, and even more worthless than those in your first. But, on reperusing it, it struck me that you were considering yourself a representative and champion of the Boston School of Medicine, and that it might be well, once for all, to answer you as such even more fully and perfectly than I had done; and thus state, in my opinion, the great things which the Boston School had done—and had not done—in the cause of anæsthetics; and how mistakes and errors might possibly have originated on the subject, between your city and other places, which I trusted could be fully removed. Hence pardon me addressing to you the following observations.

1.—TENDENCY TO CONFUSION FROM THE DISCOVERY OF CHLOROFORM RAPIDLY FOLLOWING THAT OF SULPHURIC ETHER.

From some communications which I have lately received from America, I find that your observations have stirred up there, in some minds, the idea that I have held up the introduction of chloroform as an anæsthetic in Edinburgh to be antecedent, in point of time, to the introduction of sulphuric ether in Boston. I feel sure that you and I will mutually agree that never anything so wild or extravagant was hinted or suggested by either of us. The first case of an anæsthetic operation under sulphuric ether occurred at Boston on the 30th September 1846. The first case of an anæsthetic operation under chloroform occurred at Edinburgh on the 15th November 1847. During the intervening thirteen months, I had worked much with sulphuric ether in midwifery, etc.; and some of our surgeons, here and elsewhere, had used it more or less extensively; but it was not by any means adopted by all.

At the same time, you must allow me to remark that the ideas on the subject in your own mind, which have excited you to write, have, it appears to me, become chiefly bewildered and confused in consequence of one thing—namely, of the rapidity with which chloroform thus followed as an anæsthetic after the discovery of sulphuric ether; and in consequence also of the relative practical adaptability and superiority of the former in many respects, leading speedily to its general substitution in Europe, Asia, Australia, etc., for the latter.

In the *Dispensatory of the United States of America*, Drs. Wood and Bache, when speaking of the use of sulphuric ether for inhalation in medicine, observe: "Many years ago [1796, etc.] its use in this way was proposed by Drs. Beddoes, Pearson, and Thornton, in England, as a remedy in certain diseases of the lungs. As early as 1805, Dr. Warren of Boston employed ethereal inhalation to relieve the distress attending the last stage of pulmonary inflammation. About the year 1812, in Philadelphia, at a time when nitrous oxide was the subject of popular lectures, the vapour of ether was frequently breathed from a bladder for experiment or diversion, and its effects in producing a transient intoxication analogous to that caused by the nitrous oxide were observed." Now, if in Boston in 1805, or in Philadelphia in 1812, the inhalation of sulphuric ether had been tried to a sufficient depth for its anæsthetic effects to be discovered in dentistry and surgery,—while the superior anæsthetic powers and higher practical properties of chloroform in midwifery as well as in surgery remained undetected till 1848,—then all this storm of mist and obscurity, which has been attempted within the last few months to be stirred up on the matter, would have been an entire failure, or indeed an entire impossibility. For while the glory of first discovering the induction of surgical anæsthesia by the vapour of sulphuric ether would have been, as it undoubtedly is, American in its birth-place and origin, a Lord Provost of Edinburgh in 1869—or 40 or 50 years afterwards—knowing and looking to the fact that chloroform in Scotland and in other parts of Europe, etc., had for the previous 20 years, if not entirely, yet nearly entirely, superseded the use of sulphuric ether,

and by its general adoption diffused greatly and everywhere the practice of anæsthesia—might surely, without vindictive challenges and recrimination on your part, have ventured to speak of "the discovery and application of chloroform to the assuagement of human suffering" as "the greatest of all discoveries in modern times in connection with medicine."*

An illustration, however, may show my meaning better than an abstract statement. We have now at present in practice various means of abolishing the pain attendant upon surgical operations, as nitrous oxide gas, sulphuric ether, chloroform, etc.; and the olden surgeons had others. We have various means also of arresting the hæmorrhage attendant upon these operations, as cauterisation, torsion, deligation, acupressure, etc. These hæmostatic means all arrest hæmorrhage by closing up, in one way or other, the open mouths of the cut vessels. They get at one and the same end by three or four different means; but because these means have been suggested at three or four different and distant times, any one displacing the former does not of necessity require to be apologised for and denounced, as you seem rather to think ought to be the fact in the case of anæsthetics. Or take another illustration:—The greatest thought ever perhaps broached in practical medicine, was the suggestion in relation to small-pox,—and to probably other fatal diseases, destined to occur only once in life,—that their severity and fatality might be averted, if, instead of the contagious poison producing them being allowed to enter in limitless quantities into the body by respiration, it could be inserted in very small and definite quantities by inoculation through the skin. Hence small-pox inoculation, and the wonderful protection obtained by it against the fatality of small-pox;—an idea brought from Asia and Turkey, and acted on in England in the beginning of the last century. Ere, however, the century was closed, a new variety of matter was proposed to be inoculated by Dr. Jenner, and proved infinitely a greater success. The material used by the old Asiatic and Turkish inoculators was small-pox matter taken directly from pustules on the bodies of human beings who were infected with small-pox. The material used by Jenner was small-pox matter taken from the pustules produced on the udders and nipples of cows who were infected with small-pox poison. That vaccination was thus a modification of small-pox inoculation, has never, however, been allowed to detract one iota, I believe, from the merit of the great pathological and practical revolution produced by Dr. Jenner. And the two discoveries—or two prophylactics against small-pox—the Asiatic and English variolous and vaccine inoculation—have never clashed and been entangled together: for they were in our own country upwards of half-a-century or more separate from each other in the date of their introduction and discovery. Neither, I think, would the relative merits of the two anæsthetics, the American and the English, sulphuric ether and chloroform, have been commixed in the manner in which they have been confused by you and others, had their discoveries been separated by upwards of half-a-century also.

* See the report of the Lord Provost's speech as given in the *Scotsman* of 27th October 1869.

2.—EARLIEST ANÆSTHETIC OPERATIONS IN AMERICA, AND THEIR CONNECTION WITH HARTFORD AND BOSTON.

From ancient times anæsthesia in surgery has been attempted by various agents or anæsthetics; but till latterly with very uncertain or equivocal effects. At the present time three kinds of anæsthetics are principally and specially used in practice, viz.—

1. *Nitrous oxide gas*, now, I believe, employed extensively in dental surgery, etc.,* since it was reintroduced a few years ago by Dr. Evans, of Paris; but originally suggested by Sir Humphrey Davy in 1800, and practically and successfully employed by Dr. Horace Wells in Hartford, in 1844.
2. *Sulphuric ether*, first used by Dr. Morton, at Boston, in 1846.
3. *Chloroform*, first employed in Edinburgh in 1847.

There have been latterly used, also, from time to time, various minor anæsthetic agents, but none of them, I believe, to any great practical extent; though in all likelihood some will yet be discovered of types superior to any we as yet know. In my former letter to you, and on different other occasions, I have, with other writers, shown that the ancient surgeons—Mediæval, Roman, and Greek—were long employed in the search after surgical anæsthetics, and so far succeeded, by making their patients inhale the fumes of narcotic vegetable extracts, drink solutions of them, etc. etc. Apparently afraid that the history and uses of these olden surgical anæsthetics would detract from the merit of the Medical School of Boston in the discovery of the anæsthetic properties of sulphuric ether, you bitterly denounce in your letter to me the study and consideration of them. Rest assured that no wishes or declamations, either on your part or mine, will wash out or obliterate that or any other points of the past history of surgery. "I did not desire," you exclaim, "to provoke this mediæval history." But was not your *sole* cause of complaint against me this—that in speaking to the Town Council of Edinburgh one or two sentences regarding chloroform, I omitted—most erroneously in your opinion—to refer to, or speak of, the past history of anæsthetics, say for a quarter or half a century backward? "Your prolix mediæval history," you again querulously complain, "is simply irrelevant, and its application illogical." It is in no degree illogical; but I believe that it would have been quite irrelevant if brought before the Town Council of my native city. My letter to you, as you further again bitterly observe, is "occupied with a cloud of antiquarian dust, of which the only apparent result is to obscure the truth and create a confusion in the mind of the readers, in the midst of which chloroform may be advantageously introduced." Surely, my dear sir, this undignified and calumnious sentence is unworthy alike of the heart and of the pen of Dr. Jacob Bigelow, and requires no answer from me.

But, dismissing the history of the olden forms of anæsthetics, let me direct your attention for a moment to an episode in their more modern

* See, for example, papers in the last numbers of the London *Lancet*, by Mr. Fox, "On the use of Nitrous Oxide as an Anæsthetic in Surgery."

history connected with Boston and its Medical School. You properly claim for yourselves true and vast merit from the discovery and application of sulphuric ether in dentistry and surgery. Indeed, you almost seem to me to insinuate in your letter that the medical world should have been ever afterwards contented to use sulphuric ether, and it alone. For you now argue and hold that sulphuric ether (see your last letter) formed a "*discovery of wonderful perfection at its very outset.*" I think, however, Dr. Channing (pp. 322 and 337) alludes to you yourself using chloroform in some midwifery cases; and early in the practice of etherisation in midwifery, I found that no busy obstetric practitioner could extensively employ sulphuric ether without inevitably carrying about with him, and upon his clothes, an odour so disagreeable to many other patients and other houses, as to make his presence there ought but desirable. Other Boston surgeons have tried, at least, other anæsthetics besides sulphuric ether, as if they did not look upon it in the way of "wonderful *perfection*," as you do. I have read also of your accomplished son, Dr. Henry Bigelow, excising the mamma after he had placed the patient under the anæsthetic influence, not of sulphuric ether but of nitrous oxide gas (*Official Documents*, p. 323).

In your two late articles you have carefully eschewed all reference to this last special anæsthetic nitrous oxide gas, in despite of its being now largely and successfully employed in Paris, London, and elsewhere in tooth-extraction. I wish, on the contrary, to recall your attention particularly to it. For let me here again put you in mind that the first anæsthetic operation under sulphuric ether at Boston occurred on the 20th September 1846, when Dr. Morton drew a tooth from the head of Eben Frost, who had been previously placed under the influence of the anæsthetic vapour. Nearly two years previously, however, or on the 11th December 1844, the same anæsthetic operation was as successfully performed at Hartford, the anæsthetic inhaled being not sulphuric ether but nitrous oxide gas, and the patient being Dr. Wells himself,* to whose mind the idea had suggested itself on the night previously, that a person under a deep dose of nitrous oxide might not feel, when in that state, the pain of tooth-drawing and other operations, because he had seen Mr., now Colonel, Cooley wound his limbs severely against the benches without feeling any suffering from these injuries.†

* The account which Dr. Riggs has given, in his official examination in 1852, of this first anæsthetic operation in America is sufficiently graphic. Messrs. Cooley, Wells, Collin, etc., were present. Dr. Riggs says: "A few minutes after I went in, and, after conversation, Dr. Wells took a seat in the operating chair; I examined the tooth to be extracted with a glass, as I usually do; Wells took a bag of gas from Mr. Colton, and sat with it in his lap, and I stood by his side; Wells then breathed the gas until he was *much* affected by it; his head chopped back, I put my hand to his chin; he opened his mouth and I extracted the tooth; his mouth still remained open some time; I held up the tooth in the instrument, that the others might see it; they standing, partially behind the screen, were looking on. Dr. Wells soon recovered from the influence of the gas, so as to know what he was about, discharged the blood from his mouth, swung his hand, and said, 'A NEW ERA IN TOOTH-PULLING.' He said it did not hurt him at all. We were all much elated, and conversed about it for an hour after."—(See Appendix, Dr. Colton's *Statements*, p. 95.)

† This occurrence took place at a public exhibition of, and lecture on, laughing gas, at Hartford, by Mr. Colton. The advertisement for Mr. Colton's lecture, published in the *Daily Times* of December 10, 1844, has been republished in the *Daily Journal* for February of the present year. "The entertainment," says the advertisement, "is scientific to those who make it scientific." For a full account of the effects produced by the gas upon some of the most distinguished men in Europe, Mr. Colton refers to Hooper's *Medical Dictionary*, where an abstract of the experiments of Sir Humphrey Davy is given.

A short and adequate experience of a dozen or more cases soon satisfied Dr. Horace Wells and others that teeth could in this way be extracted without pain,* however much trouble there might be in preparing and applying the gas with the imperfect means then in existence. His affidavits of its success (see foot-note) are unchallengeable. His friend Dr. Riggs drew six teeth from one patient, at one sitting, without any suffering whatever. During this time also he seems to have discovered the great point which we now know to be so essential in the successful exhibition of nitrous oxide—namely, that it should be breathed as pure as possible, and without any mixture of atmospheric air.†

Elated with his discovery, he in a week or two proceeded to Boston, in order to lay it before the medical faculty there, and show its effects. He first made it known there—according to his own account—to Drs. Warren, Heyward, Jackson, and Morton, the last gentleman being a former pupil and partner of his own, and destined to be the future discoverer of anæsthesia by sulphuric ether.‡

A case of amputation was about to be performed by Dr. Heyward in the Massachusetts Hospital, but was put off for some days. After Dr. Wells had addressed Dr. Warren's class on the subject, it was proposed that the anæsthetic should be tried in a case of tooth-extraction. "Accordingly," writes Dr. Wells, "a large number of students, with several physicians, met to see the operation performed—one of their number to be a patient. Unfortunately," he continues, "for the experiment, the bag was withdrawn much too soon, and he was but partially under its influence when the tooth was extracted. He testified that he experienced some pain, but not as much as usually attends the operation." The audience pronounced it a humbug affair and an imposition, and Dr. Wells was hissed away, left Boston, and gave up, for a time, his profession with disgust and vexation. "He was laughed at," contemptously writes Dr. Charles T. Jackson, "for his pretensions, and left Boston. No one ever believed in his story" (*Official Documents*, p. 472). According to the statement of Dr. Morton, his friend and former partner (who accompanied him), "the spectators laughed and hissed; the meeting broke up, and we were looked upon as having made ourselves very ridiculous" (See *Official Documents*, p. 47). In these experiments, Dr. Wells, as I have said, used the nitrous oxide gas which in 1800 Sir Humphrey Davy had in England found capable in his own person of

* In his pamphlet, and elsewhere, Dr. Wells brings forward sworn affidavits, from different patients of the anæsthetic effects of the nitrous oxide gas. Thus, for example, Mr. Burleigh states that, after having had an opportunity of witnessing its effects on several persons, he himself breathed it, and he adds, "two carious teeth were extracted from my lower jaw without the least suffering on my part, though, ordinarily, owing to the firmness with which my teeth are fixed in my jaw, I suffer extreme pain from their extraction." "Dr. Wells," states Mr. Goodrich, "was most successful in extracting for me a large, firmly-set, bicuspid tooth, without the slightest sensation of pain. I also witnessed, soon after, a repetition of the same process by Dr. Wells upon several individuals, accompanied in every instance with perfect success," etc.

† See Dr. Morton's volume of *Official Documents*, p. 29, etc. "The less atmospheric air is admitted into the lungs, with any gas or vapour, the better—the more satisfactory will be the result of the operation." Dr. Morton, unaware of the rules for breathing nitrous oxide, denounces this observation of Dr. Wells as "inconsistent with fact." "This agent"—nitrous oxide gas—"*never*," stoutly avers Dr. Morton, "was, nor can it ever be of any value (Official volume, p. 12). Dr. Morton's first chapter in this volume against nitrous oxide reads now, I fear, as only an exhibition of jealousy and ignorance; and Professor Jackson's letter against Dr. Wells (see p. 472 of the same volume) is still more painful and inexcusable in its tone and character.

‡ See the appendix to Dr. Morton's volume of *Official Documents*, pp. 11, 14, 15, etc.; and Dr. Wells's pamphlet about the history of the discovery of the application of nitrous oxide gas, p. 6, etc.

removing intense physical pain, and which he consequently suggested as "capable of destroying physical pain during surgical operations in which no great effusion of blood takes place." In your late letter to me, however, you maintain that "Sir Humphrey Davy," to quote your own extraordinary words, "*must* be exonerated from all practical knowledge of anæsthetic inhalation, otherwise he is chargeable with all the tortures of amputation and lithotomy which have taken place since he made the discovery and concealed it." You have used, I know, the same wild and irrelevant argument against Dr. Jackson which you here use against Sir Humphrey Davy, forgetting that their profession was that of chemists and not of surgeons. Have you really any hope or expectation that, either in Boston or elsewhere, such a violent observation as I have just now quoted, will blot out and erase in some cabalistic way the remarkable fact that Sir Humphrey Davy, seventy years ago, relieved intense physical pain in his own person by breathing nitrous oxide gas, or that he suggested that it might be used as an anæsthetic in some surgical operations, and—published the suggestion?

But now mark what subsequently occurs. An American dentist works out to its practical results the suggestion published in England half-a-century before by Sir Humphrey Davy, and which you seem to wish to efface from anæsthetic records, and he travels a long distance to place the important result before the Medical School at Boston, and some surgeons of the Massachussetts Hospital. There is a slip in the single experiment allowed him. He is spurned and hooted away. In doing this the Medical School of Boston thus delays the whole subject of artificial surgical anæsthesia for a couple of years. Was not the Medical School of Boston then, in your violent language, "chargeable with the continuance of operative tortures" for that period, much more than Sir Humphrey Davy? Did not your school stamp out—and thus prevent for two years more—the "most beneficent discovery," to use again your own grandiloquent words, "which has blessed humanity since primeval days of paradise?"* I am using here not my language and logic, but yours.

It is perhaps here unnecessary to add that there is sufficient evidence that Drs. Wells, Marcy, and Goodrich debated the question together whether sulphuric ether would not be an agent preferable to nitrous oxide in these experiments (see *Official Documents*, pp. 26, 27, and 43, Appendix, pp. 87, 111, 114); but Dr. Marcy thought nitrous oxide the safer and pleasanter of the two, and also more easy to inhale.† In the Essay in which your son first describes the inhalation of sulphuric ether in surgery, he points out its similarity to

* Of course, unaware of the comparative perfection to which Dr. Horace Wells's method of inducing brief surgical anæsthesia might yet be brought, the Select Committee of the House of Representatives of the United States, in 1852, report so far against the practical utility and success of Dr. H. Wells's claims; but they add, "He had the merit of attempting to carry out practically the idea suggested by Sir Humphrey Davy of rendering (by the influence of nitrous oxide gas) a patient insensible to pain in a surgical operation. He has also (they add) undoubtedly the merit of having contributed something in directing the mind of Dr. Morton to the subject." (See *Official Documents*, pp. 13 and 16.)

† It is unnecessary to enter here into the discussion whether Dr. Marcy of Hartford, in 1844 or 1845, removed, without pain, a tumour about the size of a walnut from the head of a young man who was at the time anæsthetised by the vapour of sulphuric ether (see volume of *Official Documents*, p. 27, and Appendix, p. 132, etc.) If so, he forestalled the use of sulphuric ether at Boston as an anæsthetic in surgical operations. But his experiment was so far sterile, as the employment of sulphuric ether in surgery did not spread from that point.

nitrous oxide.* "Ether inhaled in vapour," he says, "is well known to produce symptoms similar to those produced by the nitrous oxide" (see Paper read before Boston Society of Medical Improvement, 3d November 1846). Dr. Horace Wells had the idea suggested to his mind one day (10th December 1844) that the inhalation of nitrous oxide gas would prove an anæsthetic in tooth-drawing, and he had it proved and verified the next day (11th December) on his own person. But the step from *using* nitrous oxide gas to using sulphuric ether vapour was slower and yet greater and more momentous in its results. Dr. Morton, who, as his friend and old partner, assisted at Dr. Wells's experiment at Boston, no doubt knew all the results obtained at Hartford, where he twice visited Dr. Wells after 1844 ; and he evidently, betimes, got the idea or speculation into his mind that sulphuric ether might prove successful. From a different line of observation, Professor Charles Jackson was led to the entertainment of the same speculation. Assisted, apparently, by one or two hints from Dr. Jackson regarding the pure quality of the ether, or, possibly, its easiest mode of exhibition, Dr. Morton verified the speculation on the 30th September 1846, by operating on Eben Frost, and fixed that date as an era in science.

3.—ETHERISATION, OR ANÆSTHESIA, IN MIDWIFERY.

The first operations under anæsthetic inhalations in America occurred, therefore, as we have seen in the last section, at Hartford, and not at Boston. In Hartford it was effected by an anæsthetic gas, long before suggested by Sir Humphrey Davy. But at Boston you at first retarded, for a time, the whole progress of anæsthesia, by rejecting the evidence of it offered you by Dr. Horace Wells. For, to quote the words of Dr. Riggs, "there (in your school) he met with a reception so cold that, after a single imperfect trial of the gas, amidst the sneers of those around him, he left Boston in disgust, and sick at heart at the unfair disposition manifested towards him." Besides erring in this direction you must permit me to add that in my opinion some of the Boston physicians have also erred in quite an opposite direction. For, after once making the discovery of the superinduction of anæsthesia by sulphuric ether, you seem inclined to hold that the subsequent merit of everything connected with etherisation belongs to Boston, and to Boston exclusively.

The object of your first article on the present subject was to show that, because when I received the burghership of Edinburgh, I omitted to allude to the previous use of anæsthetics at Hartford and Boston, I was therefore deliberately guilty of trying to appropriate what belonged to my American brethren. In my reply to you, whilst showing that I was entirely guiltless of any such appropriation in thought or in word, I pointed out that strangely enough, you yourself were in the same article openly and flagrantly guilty of the unprofessional misdemeanour of which you accused

* In the volume of *Official Documents*, p. 372, there is one case entered, suggestive of the applicability of artificial anæsthesia from breathing sulphuric ether quite similar to Colonel Cooley's experience with regard to nitrous oxide. "A young gentleman, ten years since, who is now a physician, was inhaling ether for amusement, as was the custom at Harvard College. He took enough to make him so insensible that he fell upon the floor. In falling he cut his head badly. On recovering he was *unaware* that he had injured himself at all."

me ; for, in claiming, as you there did, for Boston the introduction of anæsthetic inhalations in obstetrical practice, you attempted to annex and appropriate to your country what most indubitably belonged to mine.

In your last letter you begrudgingly state to me, "I do not *now* question that you were the first to use ether in labour ;" and then you superciliously add, "but who first introduced anæsthetics in obstetrical practice is a matter of limited importance." According to the testimony, however, of our late mutual friend, Sir John Forbes, the application of anæsthetics to midwifery involved many more difficult and delicate problems than its mere application to dentistry and surgery. New rules required to be established for its use—the time during which it could be given ascertained—its effects upon the action of the uterus, upon the state of the child, and upon the parturient and puerperal state of the mother, etc., all required to be accurately studied. Would it increase or diminish the tendency to convulsions, hæmorrhage, and various other complications ? Moral and religious questions also came to be involved, and required to be duly answered. The Boston patent for the use of sulphuric ether taken out by Drs. Morton and Jackson, did not, I believe, include its employment in midwifery ; and your son, Dr. Henry Bigelow, weeks after its use was first begun, deemed it only "adapted to operations which were *brief* in their duration, whatever be their severity. Of these the two most striking perhaps are *amputations* and the extraction of *teeth*." * This was published in November. When I saw Mr. Liston in London, during the following Christmas holidays, he expressed to me the opinion that the new anæsthetic would be of special use to him,—who was so swift an operator,—as he thought, like Dr. Bigelow, it could only be used for a brief time. I went back, however, from this London visit to Edinburgh, bent on testing its applicability to midwifery, and found that it could be safely used for hours, etc.

But is its application to midwifery of "limited importance," as now in the fervour of disputation you seem anxious to affirm ? Your words in your first article regarding the commencement of anæsthetics in Boston are these :—That anæsthetic inhalation "began in this country, and was *first* used in the extraction of teeth, and afterwards [2] in capital operations in the Massachusetts General Hospital ; and [3] in obstetrical practice." You adduce thus three kinds of practice in which it was used in Boston—namely (1) dentistry ; (2) surgery ; and (3) midwifery. You have omitted medicine, probably because you well knew the employment of the inhalation of sulphuric ether had been introduced (as we have seen in a previous part of this letter) into medical practice by Dr. Pearson half-a-century before. Holding, as you now affect to declare, that the use of anæsthetics in obstetrical practice is a matter of limited importance, upon what ground, may I venture to ask, did you, only two or three months ago, in your first attack, adduce its application to midwifery as one of its three chief applications ? Further, among these three chief applications, may I ask you, in all honour and honesty, is its use not,—even in your opinion,—a matter of infinitely less importance in dentistry than in mid-

* See the paper which he read five or six weeks after the introduction of sulphuric ether before the Boston Society of Medical Improvement, as cited in Brook's Essay on the Vapour of Sulphuric Ether, page 30.

wifery? Of the relative value of any new practice, such as artificial anæsthesia, we are bound to judge by its utility, not in any specialised practice, as that of a surgical hospital, but in the general practice of the general practitioner. Now most general practitioners have 20 or 30 cases at least of labour in which they may employ anæsthetics for every one case of surgery in which its use could be adopted by them; surgical cases being rare, and obstetrical cases common in general practice. You practise, and have, I believe, all along practised, chiefly as a physician, and are hence, perhaps, no good judge in the matter; but let me extract for you from the volume of Official Documents published by your townsman Dr. Morton, the opinions of one or two general practitioners on the subject. "It is," says Dr. Appleton, "in obstetric practice that I have most frequently used these most valuable agents, and I regard their usefulness in this relation as among *the most* valuable results of their discovery." "In private practice," writes Dr. Ellis, "its *most* happy and beneficial effect is in obstetrics . . . . its benefit, if in no other, in this class of cases alone *is the greatest discovery* in any age of the world for the relief of suffering humanity" (*see* vol. of Official Statements, pp. 180, 182).

You profess to deem it a "matter of limited importance" who first introduced anæsthetics into midwifery. Perhaps it is so. But you will excuse me adding that at the time of the first application of anæsthesia to obstetrical practice—amidst the hundreds and thousands of practitioners who were then engaged in midwifery in the old and the new world—I happened to be the first who took up the subject and worked out most of the problems connected with it. Any one of these hundreds and thousands might certainly have done the same, but did not do it.

Let me here add that I would not have dwelt thus long upon the application of anæsthetics to midwifery, did I not feel compelled to add that other of your medical townsmen have seemed quietly on this head to have tried, like you also, to appropriate to Boston what belongs to Edinburgh. Thus Dr. A. Gould, in his official deposition in 1852, speaks of "the first administration of sulphuric ether in obstetrics by Dr. N. C. Keep,"* of Boston, as "a similar step in the discovery" as anæsthesia in dentistry and surgery. In my former letter, however, to you, I showed you that Dr. Keep's case, here alluded to by Dr. Gould, did not occur in Boston till weeks and months after the practice of anæsthesia in obstetrics had been fully studied and established in Edinburgh. Besides, I find now, on looking over the large volume published on Etherisation in Midwifery, by my friend, and your townsman, Dr. Channing, that—avoiding all allusion to the midwifery cases reported, and the papers published upon etherisation in midwifery, in Edinburgh, weeks previously to the occurrence of Dr. Keep's case in Boston—he speaks also of Dr. Keep's as if it were the first instance in which ether had been employed in obstetrics. But this, perhaps, is merely an omission, as in a holograph inscription of his volume to me, Dr. Channing duly acknowledges that I was the first to introduce anæs-

* Dr. Keep's case happens to be described by Dr. Channing in such words as to *leave it equivocal* whether or not it was the first obstetric instance in which sulphuric ether was given in Boston or America only, or in the world. Dr. Channing's account of it is as follows:—"The anæsthetic power of sulphuric ether, when inhaled, was first used in childbirth in this city, in a case of natural labour, on the 7th April 1847, by Dr. N. C. Keep, and was successful" (*Etherisation in Childbirth*, by Channing, p. 26).

thetics into obstetrical practice ; and, in the dedication of his work, he speaks, let me add, of midwifery as being a department which has derived "*special* and *vast* benefit" from the application of anæsthetics.

According to your reasoning (?) in the case of Sir Humphrey Davy, that great philosopher "must be exonerated from all practical knowledge of anæsthetic inhalation ; otherwise he is chargeable with all the tortures of amputation and lithotomy" which have taken place from his time till the end of 1846. According to the same ratiocination, were not you and the other accoucheurs of Boston chargeable with all the tortures of childbirth and parturition borne by the female population of that city for months onwards after October 1846 ; or, till the knowledge of the mode of relieving them from these tortures was sent out from Edinburgh—seeing the use of ether in labour was a matter of limited importance, and could and should have been at once discovered and applied in your city, and not in Europe?

4.—ALLEGED NEGLECT OF AMERICAN CLAIMS IN WRITING A SKETCH OF THE HISTORY OF ANÆSTHETICS.

The chief or only subject of your attack upon me in your first article, was the frivolous allegation that, when last year I received the honorary burgess-ship of Edinburgh, and when I had to speak on various and different topics, I omitted to do justice to your city and to America, by omitting to talk of the discovery of sulphuric ether as an anæsthetic when I was called upon to answer an observation or two of the Lord Provost's on chloroform.*

In your last letter, following out the same jealous strain of complaint, you argue that, besides the alleged omission in an *impromptu* speech, I was guilty, in an article which I had calmly written upon chloroform in the *Encyclopædia Britannica*, of not doing "justice to the great American discovery." The article in question was printed in a volume of the *Encyclopædia* for 1854, and has been republished in a second volume of my works, collected and edited by Drs. Priestley and Storrer. In that article, after defining chloroform and stating its composition, modes of preparation, physical, chemical, and physiological properties, I have

* Lest there be any mistake regarding the grounds or supposed grounds of all the war which you have tried to stir up against me, let me here cite in full the Lord Provost's remarks on chloroform, and my reply to them. The Lord Provost, let me state, was one of the most intelligent and intellectual men of the age, William Chambers, Esq., the well-known author and publisher. His address to me in presenting the burgess ticket was spoken extempore, and I find that his words on chloroform are somewhat differently reported in our three morning journals. The version most favourable for you is the one you select—the *Daily Review*—and is as follows:—"I will not dwell on what you have accomplished in medical science. I will only allude to your discovery—the greatest of all discoveries in modern times—of the application of chloroform in the assuagement of human suffering. That was a great gift to mankind at large, and it well befits us, the Corporation of Edinburgh, to mark our sense of the great act of beneficence on your part by this small compliment." His Lordship subsequently alluded to my writings on Acupressure, Hospitalism, etc. etc.

With regard to the observations on Chloroform, I replied in the two following sentences:—"You adverted to the discovery of anæsthetic effects of chloroform. Perhaps you will allow me to state that there are various manufactories of it in Great Britain, and that a single one of these, located in Edinburgh, makes as many as eight thousand doses a-day, or between two million and three million of doses every year—evidence to what a great extent the practice is now carried of wrapping men, women, and children in a painless sleep during some of the most trying moments and hours of human existence ; and especially when our frail brother-man is laid upon the operating table, and subjected to the tortures of the surgeon's knives and scalpels, his saws and his cauteries."—(See *Journal of the Gynæcological Society of Boston*, *No.* 6, p. 370).

described at length the various therapeutic uses to which it, and consequently any other similar anæsthetic, could be applied in surgery, in midwifery, in medicine, and in medical jurisprudence ; and ultimately I have occupied the last three columns of the article by a brief historical sketch of the various anæsthetic agents which have been used previously to the introduction of chloroform. And this historical sketch is the special object of your new attack.

In giving, in my lectures and otherwise, a history of anæsthetics, I have sometimes traced them from the earliest known periods downwards to the present day ; but more frequently I have followed the *inverse* order, because I have found it more instructive and interesting—viz. that of tracing them gradually backwards from their most recent to their most ancient form. I have followed this last method in the said article in the *Encyclopædia Britannica*, and have hence first mentioned chloroform as then the most recent anæsthetic in the two following lines :—" The vapour of chloroform was first proposed by Dr. Simpson as an anæsthetic agent in 1847."

I then, after these two lines, give above twenty lines to sulphuric ether, beginning thus :—" For a year previous the vapour of sulphuric ether had been used to a considerable extent both in America and Europe, for the purpose of inducing insensibility to pain in surgical operations. It was first practically adopted for this purpose in 1846 by Dr. Morton, a dentist at Boston, in America. Subsequently Dr. Charles T. Jackson of that city claimed the right of having suggested to Dr. Morton sulphuric ether as an agent capable of producing insensibility to pain. But the power of producing by the vapour of sulphuric ether an insensibility exactly like that produced by the inhalation of nitrous oxide gas, had been long previously known," and so on through its history.* *Thirdly*, I allude to carbonic acid as suggested by Dr. Hickman in 1828 ; *fourthly*, to nitrous oxide gas as hinted at by Davy in 1800 ; *fifthly*, to compression of the nerves as used by Dr. Moore in 1784 ; *sixthly*, to compression of the carotids as suggested by Valverdi and others in the sixteenth century ; *seventhly*, to the fumes and extracts of mandragora, Indian hemp, and other soporific drugs, as practised by mediæval and ancient Roman and Greek surgeons.

Now comes your strong and strange accusation or accusations. For first you hold, as far as I understand you, that the article was written for my " self-exaltation," or to quote your own words, " in favour of the self-exaltation of the writer." Of any such object I know and feel myself to have been utterly guiltless, either in this or any other of my writings. In the whole course of this long encyclopædic article upon chloroform, if my object had been " self-exaltation," I might not unjustly have connected my name with several of the original suggestions and practices stated in the article ; but I have mentioned my name only once, and that in the brief

* You underscore the expression used " to a considerable extent," probably with a view of indicating that that is doubtful ; but such, I believe, was the fact here and elsewhere in the first year of etherisation. In the *Edinburgh Medical Journal* for September 1847, I find it stated by me (p. 153) that, " during the last six months etherisation has been *used to a considerable extent* in British surgery." The Editor of the same journal, in his December number—chloroform having been introduced in the interval—observes, " In Edinburgh it (chloroform) has been used publicly by all the surgeons of the Royal Infirmary [they had not all used ether], and its employment in midwifery practice is almost universal. Ether," he adds, " has almost been abandoned" (p. 456).

historical sentence already quoted, and when (I appeal to yourself or any honourable man) such mention was utterly unavoidable for the sake of simple historical accuracy, all such history inevitably involving an enumeration of names.

But then comes your other accusation, that in enumerating the different methods of producing anæsthesia I have adduced chloroform *first*, sulphuric ether *second*, carbonic acid *third*, nitrous oxide *fourth*, etc., to "cover"—to use your own reprehensible words—"this inversion of historical order in favour of the self-exaltation of the writer . . . who availed himself of this opportunity principally to place himself conspicuously in the foreground."

Believe me, I feel difficulty in commenting upon these criticisms of yours; they are essentially so groundless and absurd; and I know them in my own heart to be so utterly untrue. If an American or English schoolboy were asked to give a *retrograde* chronological list of the Presidents of the United States, or the Sovereigns of England, from the present time to the commencement of this century, would he not begin with General Grant and Queen Victoria? According to your logic, however, that would imply "self-exaltation" on the part of the pupil; and to avoid this he ought to commence with the Presidents Johnson and Lincoln, or King William the Fourth. But would not such a strange historical obliquity and misstatement, if unhappily indulged in, bring down condign punishment and contempt on the disciple? And is there not occasionally truth in the saying that "sages sometimes do as foolish things as schoolboys?"

If I had the same history to re-write to-day, I do not know that I would or could write it in any different terms, except by pointing out more distinctly Dr. Wells's claims, and also Dr. Jackson's. And pray in what terms would or could you advise me that it should have been written, or should be written now? Ought I to have broken out into some high-flown sentence or sentences regarding the history of the anæsthetic effects of sulphuric ether, when I spoke secondly of that anæsthetic? Would it not, let me ask you, have been more natural—for me at least—to have done so in speaking of the history of the anæsthetic effects of chloroform, instead of dismissing it in the two brief lines I have already quoted; seeing, especially, that I knew that it was employed in hundreds or even thousands of instances for every five or ten in which sulphuric ether was used?

I have, I find, printed another short epitome of the history of anæsthetics, but I am not sure that it will please you better. In a paper on *Etherisation in Surgery*, published in September 1847—the first of a series on the subject—I take occasion to speak of Dr. Morton of Boston as "the gentleman to whom I believe the profession and mankind are really and truly indebted for first reducing into practice the production of insensibility by ether-inhalation, with the object of annihilating pain in surgical operations"—language stronger, I think, than I have seen in most American essays on the subject. And at the meeting of the Edinburgh Medico-Chirurgical Society, on November 10th of the same year, I laid before them a paper termed "Historical Researches regarding the Superinduction of Insensibility to Pain in Surgical Operations; and Announcement of a new Anæsthetic Agent." This communication on the

history of anæsthetics, like that in the *Encyclopædia Britannica*, took up the subject in retrograde chronological order, beginning with sulphuric ether *first*, as chloroform was not known when it was drawn up a week or two previously. In the abstract of this historical paper, which appeared in the *Edinburgh Journal* (it was never published entire), I find that I traced out, at some length, the chemical and therapeutic history of sulphuric ether, and add as follows :—" Its power of producing, by inhalation, effects like intoxication, or like the influence of nitrous oxide gas, he (Dr. Simpson) showed to have been stated by various American authors, as by Professor Samuel Jackson (1833), Wood and Bache (1834), Miller (1846), before it was so fortunately adopted by Dr. Morton as an anæsthetic agent. His belief was, that Professor Charles Jackson improperly claimed the merit pertaining to its recent happy application to surgery, etc. Perhaps the idea of relieving patients from the pains of surgery by some such means, or rather, the restoration of that idea in recent times (for it was an old one), belonged justly to Horace Wells." —(See *Edinburgh Monthly Journal of Medical Science* for December 1847, p. 453.)* From the abstract of this paper it appears that I went chronologically backwards, through various old anæsthetic vapours and measures, to the use of the fumes of Indian hemp in the time of Herodotus. I then took up the last or second part of the paper, and showed the Society the newly-discovered anæsthetic, chloroform, and its effects.

At the time at which this paper was read, we had, with almost every mail from America, statements and counter-statements sent as to who was the rightful claimant for the discovery of anæsthesia with sulphuric ether ; and what was conceived to be true the one month, was apt to be upset the next. In none of these statements have I, I think, done sufficient justice to the claims of Professor Charles T. Jackson, for I now believe he had more merit in the discovery than formerly I felt inclined to attribute to him, since I have latterly looked over the large volume of *Official Documents* on the matter, presented to " the Select Committee appointed by the Senate of the United States." He held the idea that sulphuric ether vapour might anæsthetise a patient for an operation, though he had not reduced in any way that idea to practice, and at first seemed afraid of the possible results of Dr. Morton's experiments (*Official Documents*, pp. 352 and 446), while he avoided witnessing for results.

If we try to put into a summarised form the data† which we have been discussing regarding the introduction of anæsthesia in America and

* (See *Edinburgh Journal of Medical Science* for 1847, p. 451.) The epitome has been republished in a volume of mine on *Anæsthesia* (p. 190), printed at Philadelphia in 1849 ; but it is not republished in the collection of my writings edited by Drs. Priestley and Storrer. I have seen it repeatedly cited, at pretty full length, in American essays on anæsthetics—sometimes with, sometimes without, acknowledgment.

† You must kindly excuse me if some of the data are not strictly accurate in every point, as you know how difficult it is to make medical aphorisms quite correct ; for example, in the inscription which you represent as cut upon the monument, lately erected at Boston, to anæsthetics, namely, " To commemorate the discovery that the inhaling [the inhalation] of Ether causes insensibility to pain, first proved to the world at the Massachussetts General Hospital, in Boston, October, A.D. 1846 ;" there are, it appears to me, two errors. First, ether and sulphuric ether are two terms not at all synonymous, and still you have inserted the former for the latter. Secondly, it is not strictly true that the effects of sulphuric ether were "first proved to the world in operations at the Massachussetts General Hospital." For to cite the more correct statement of Dr. Channing :—" These operations were *first* performed in private practice, and immediately afterwards upon patients in the Massachussetts General Hospital " (Channing *On Etherisation*, 1848, p. 26).

this country, it appears to me that we might correctly state the whole matter as follows :—

1. That on the 11th December 1844, Dr. Wells had, at Hartford, by his own desire and suggestion, one of his upper molar teeth extracted without any pain, in consequence of his having deeply breathed nitrous oxide gas for the purpose, as suggested nearly half-a-century before by Sir Humphrey Davy.

2. That after having with others proved, in a limited series of cases, the anæsthetic powers of nitrous oxide gas, Dr. Wells proceeded to Boston to lay his discovery before the Medical School and Hospital there, but was unsuccessful in the single attempt which he made, in consequence of the gas-bag being removed too soon, and that he was hooted away by his audience, as if the whole matter were an imposition, and was totally discouraged.

3. That Dr. Wells's former pupil and partner, Dr. Morton of Boston, was present with Dr. Wells when he made his experiments there.

4. That on the 30th September 1846, Dr. Morton extracted a tooth without any pain, whilst the patient was breathing sulphuric ether, this fact and discovery of itself making a NEW ERA in anæsthetics and in surgery.

5. That within a few weeks the vapour of sulphuric ether was tried in a number of instances of surgical operations in Boston—Dr. Morton being generally the administrator ;—and ether vapour was established as a successful anæsthetic in dentistry and surgery.

6. That in January, and the subsequent spring months 1847, the application of sulphuric ether as an anæsthetic in midwifery was introduced, described in our medical journals, and fully established in Edinburgh, before any case with it was tried in Boston or America.

7. That on the 15th November 1847, the anæsthetic effects of chloroform were discovered in Edinburgh, and that it swiftly superseded in Scotland and elsewhere the use of sulphuric ether, and extended rapidly and greatly the practice of anæsthesia in surgery, midwifery, etc.

I am very sorry to have taken up so much of your time and my time with such a petty discussion as the present. It has extended to too great a length ; but I am a sad invalid just now, and quite unable to write with the force and brevity required. With many of our profession in America I have the honour of being personally acquainted, and regard their friendship so very highly that I shall not regret this attempt—my last perhaps—at professional writing as altogether useless on my part, if it tend to fix my name and memory duly in their love and esteem.

Yours very truly,

J. Y. SIMPSON.

To Dr. JACOB BIGELOW.

EDINBURGH, *April* 1870.