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THE WORKS

OF

SIR JAMES Y. SIMPSON, BART.

VOLUME II.

ANÆSTHESIA, HOSPITALISM, HERMAPHRODITISM,

AND A

PROPOSAL TO STAMP OUT SMALL-POX AND OTHER CONTAGIOUS DISEASES.

BY

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A.B

PREFACE.

It would have been presumptuous in me, not being a medical man, to attempt to edit this volume of my father's works, had it not been evident that a careful selection and arrangement of material was nearly all that was required of me.

The Papers on Anæsthesia contain most of my father's writings on the subject, those papers and parts of papers only being omitted which would have been repetitions of others which appear in the volume. Many of the articles were published in his *Obstetric Memoirs and Contributions*, edited in 1855-6 by Drs. Priestley and Storer. Others have been written since. I have tried to give them in a logical rather than in a chronological sequence, putting, when necessary, at the head of each the date of its original production. Although a consecutive work, written in later years on the subject by my father, would have given more prominence to some points and less to others than these papers do, yet it is hoped that they will be found at least to touch upon most of the topics that would have come under discussion in a more complete treatise on the subject.

The Papers on Hospitalism are similarly arranged, and contain all that their author lived to write in vindication of his opinions on the subject.

References to the journals, etc., in which such of the Papers as have already been in print originally appeared, will be found at the end.

W. G. SIMPSON.

EDINBURGH, *October* 1871.

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PART I.

HISTORY OF ANÆSTHESIA.

CHAPTER I.

ANCIENT HISTORY OF ANÆSTHESIA.

THE idea of saving by some artificial means the human body from the pains and tortures inflicted by the knife of the surgeon is by no means a thought first broached or first acted upon in recent times. For the production of anæsthesia a variety of measures had been suggested and employed long before the introduction of sulphuric ether and chloroform.

We may consider these means under several heads, according as the insensibility to pain was to be effected either—

1. By the inhalation of gases or vapours.
2. By mechanical means.
3. By the internal administration of drugs.

1. *Anæsthesia by the inhalation of vapours or gases.*—Under this head I may mention the following suggestions:—In 1795 Dr. Pearson proposed the inhalation of sulphuric ether (and he appears to have been the first to do so), but apparently only for the purpose of relieving the cough in asthma, etc., and not for producing general anæsthesia. In 1816 Nysten introduced a special apparatus for its inhalation. In 1800 Sir H. Davy recommended the employment of nitrous oxide gas for the relief of pain in the extraction of teeth and other minor surgical operations, and he also described the effects it had on himself when he inhaled it to relieve local pain. Thus, he says:—"The power of the immediate operation of the gas in removing intense physical pain, I had a very good opportunity of ascertaining. In cutting one of the unlucky teeth called dentes sapientiæ, I experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of

repose and of consistent action. On the day when the inflammation was most troublesome I breathed three large doses of nitrous oxide. The pain always diminished after the first four or five inspirations, the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind, however, returned, the state of organ returned with it; and I once imagined that the pain was more severe after the experiment than before."[1]

Sir H. Davy recommended its employment in surgery in the following words:—"As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place."[2]

Dr. Hickman also, in 1828, suggested the inhalation of carbonic acid, as a means of producing insensibility in surgical operations.

There were other agents used for this purpose in more ancient times, which, although they produced their effects by inhalation of their vapours, I may describe more conveniently under the third of these divisions.

2. *Anæsthesia by mechanical means.*—In 1784 Dr. Moore proposed to bring about local anæsthesia of limbs during surgical operations, by compressing the nerves supplying them. He tried this method in a case in Dr. Hunter's practice, applying a tourniquet for an hour before the operation, to compress the nerves, with, however, only partial success. Malgaigne also tried the tourniquet, but with no better result, for he found that the instruments used for the compression of the nerves themselves produced considerable pain. This mode had been suggested, I may observe, by Ambrose Parè long before Moore's time. It was supposed at one time that compression of the carotid arteries, and consequent diminution of the supply of blood to the brain, would be successful in producing anæsthesia; and this plan was even put into practice by Valverdi, Hoffman, and others, in the sixteenth and seventeenth centuries. Morgagni mentions that this practice was followed in the dissecting-room on animals to induce anæsthesia, and he also suggests its use in the human subject. There is not the least doubt that it will induce stupor or coma, and that, moreover, with extreme rapidity. This is too frequently proved in cases of garrotting, of which an instance is on record where stupor was successfully produced, and robbery committed, between the time of the victim ringing his door-bell and the servant answering the summons. This method was again lately

[1] *Chemical and Philosophical Experiments*, p. 464. [2] *Idem*, p. 566.

proposed for employment in surgery some years ago by Professor Fleming.

3. *Anæsthesia by internal administration of drugs.*—In various authors, both of ancient and modern times, we read of drugs used to induce anæsthesia, but we can seldom make out exactly what were the substances employed.

In the *Medical Gazette*, vol. xli. p. 515, Dr. Sylvester quotes a passage from a German work by Meissner, called *Skizzen*, published in 1782, to the following effect :—

"Augustus, King of Poland and Elector of Saxony, suffered from a wound in his foot, which threatened to mortify. The Court medical men were opposed to the operation of amputation, but during sleep, induced by a certain potion surreptitiously administered, his favourite surgeon, Weiss, a pupil of Petit of Paris, cut off the decaying parts. The royal patient was disturbed by the proceeding, and inquired what was being done, but on receiving a soothing answer he again fell asleep, and did not discover till the following morning, after his usual examination, that the operation of amputation had really been performed."

Opium has been suggested at various times, but the dose which would be required to produce anæsthesia would be so large that no one could safely administer it. We know, however, that formerly two substances were held in great repute by patients about to undergo surgical operations, and also by criminals condemned to suffer torture. Those drugs were Indian hemp (*Cannabis sativa*, var. *Indica*) and mandragora (*Atropa mandragora*, but now called *Mandragora officinalis*). It will be interesting, if not advantageous, to consider the history of each of these separately, and somewhat in detail.

Indian Hemp.—Sir Joseph Banks alludes to the preparation and use of this drug in various parts of the east from Morocco to China, in which countries it goes under the names of bang, churrus, and gunjah. He mentions that, when any preparation of this plant is swallowed, the mind is immediately filled by a delicious succession of pleasant ideas, which banish all cares and produce a perfect indifference to pain. This state is, however, soon followed by sleep. The drug is more especially used by criminals doomed to suffer torture, and it seems to be what is taken by the Hindoo Suttee to give her powers of endurance. Some years ago an old Chinese manuscript was laid before the French Academy by M. Julien, in which the use of Indian hemp for annulling the pain in cauterisation and surgical operations is mentioned as having been had recourse to

by a practitioner by name Hoa-tho, as early as the third century, that is about 1500 years ago.

In this manuscript, which is called Koukin-i-ting, we find the following passage :—" He (Hoa-tho) gave to the patient a preparation of hemp (Ma-yo), and at the end of some instants he became as insensible as if he had been drunk, or deprived of life ;" and a little farther on it is remarked that, " after a certain number of days the patient found himself re-established, without having experienced the slightest pain during the operation."

We can trace the use of Indian hemp still farther back, for some divines maintain that the draught offered to our Saviour on the cross was some preparation of Indian hemp.[1]

A preparation of the same drug is probably referred to by the prophet Amos, 700 years before Christ, as the " wine of the condemned."[2]

The effect of the inhalation of the vapour of burning hemp is mentioned by Herodotus, who also tells us of its use by the Massagetæ and by the Scythians to produce excitement. Thus, talking of the Massagetæ, he says " that they have discovered other trees that produce fruit of a peculiar kind, which the inhabitants, when they meet together in companies, and have lit a fire, throw on the fire as they sit round in a circle ; and that, by inhaling the fumes of the burning fruit that has been thrown on, they become intoxicated by the odour, just as the Greeks do by wine ; and that the more fruit is thrown on the more intoxicated they become, until they rise up to dance, and betake themselves to singing."[3]

Again, when describing another tribe of the Scythians, he informs us that " they have a sort of hemp growing in this country, very like flax, except in thickness and height ; in this respect the hemp is far superior ;"[4] and a little farther on he informs us that, " when therefore the Scythians have taken some seed of this hemp, they creep under the cloths, and then put the seed on the red-hot stones ; but this being put on, smokes, and produces such a steam that no Grecian vapour-bath would surpass it. The Scythians, transported with the vapour, shout aloud ; and this serves them instead of washing, for they never bathe the body in water."

Homer also gives us an account of the effect on Ulysses and his companions of drinking some preparation, which he called Nepenthe, and the phenomena he describes are so like those produced

[1] Mark xv. 36. [2] Amos ii. 8. [3] Cary's *Herodotus*, i. 202.
[4] Cary's *Herodotus*, iv. 74.

by hemp that we may conclude it was of this substance they made use. He tells us that the Jove-descended Helen planned other things, and that "she straightway cast a drug into the wine, from whence they were drinking, that frees men from grief and from anger, and causes oblivion of all ills. Whoever should drink down this, when it is mixed in a cup, would not shed a tear down his cheeks for a whole day, not even if both his mother and father should die, nor if they should slay with the steel a brother or a beloved son before him, and he should behold it with his eyes. Such cunning and excellent drugs the daughter of Jove possessed, which Polydamnia, the wife of Thon, gave her, an Egyptian."[1]

Pliny also mentions this drug (Indian hemp) in his *Natural History*,[2] but he does not speak of its inhalation, but merely remarks that it was used for making ropes.

Mandragora.—This plant was most extensively used by Hugo of Lucca in the twelfth century, and the preparation employed by him is described to us by his son and successor, Theodoric, who died in 1298, under the name of the Spongia somnifera, that is the somniferous ball, or sponge, or apple. In his *Chirurgial*, which he published on entering the church, and in which he made known most of the chemical preparations and rules of the art of surgery which had been followed by Hugo, who had, however, kept them all secret, we find that this somniferous sponge was prepared according to the following prescription:—"Take of opium, of the juice of the unripe mulberry, of hyoscyamus, of the juice of hemlock, of the juice of the leaves of mandragora, of the juice of the woody ivy, of the juice of the forest mulberry, of the seeds of lettuce, of the seeds of the dock which has large round apples, and of the water-hemlock—each an ounce; mix all these in a brazen vessel, and then place in it a new sponge; let the whole boil, as long as the sun lasts on the dog-days, until the sponge consumes it all, and it has boiled away in it."[3]

He then goes on to describe the way in which this sponge is to be used—"As oft as there shall be need of it, place this sponge in hot water for an hour, and let it be applied to the nostrils of him who is to be operated on, until he has fallen asleep, and so let the surgery be performed."

He then tells us how to rouse the patient after the operation is over. "This being finished, in order to awaken him, apply another

[1] Homer by Buckley, *Odyssey*, iv. 219. [2] *Hist. Nat.* xix. 36.
[3] *Chirurgial*, iv. 8.

sponge, dipped in vinegar, frequently to the nose, or throw the juice of the root of fenugrek into the nostrils ; shortly he awakens."

This preparation was, however, gradually abandoned, perhaps from its occasionally causing, as we are told by Aretæus, convulsions and coma, and in some cases even death. In the works of M. Chamappe, published in 1534, we learn that this somniferous sponge was still in use in his time ; but it must have been very nearly abandoned then, for towards the close of that century Parè tells us that it was a practice "used formerly" by operators.

But we find many allusions made to the anæsthetic effect of mandragora by ancient authors, among which I may mention the following :—

Dioscorides refers to the effects of the drug in several passages thus :—

"Some persons boil the root in wine down to a third part, and preserve the decoction, of which they administer a cyathus in want of sleep and severe pains of any part, and also before operations with the knife or the actual cautery, that they may not be felt."[1]

"There is prepared also, besides the decoction, a wine from the bark of the root, three minæ being thrown into a cask of sweet wine ; and of this three cyathi are given to those who are to be cut or cauterised as aforesaid ; for, being thrown into a deep sleep, they do not perceive pain."[2]

Again, when speaking of another kind of mandragora, which he calls morion, he says—

"They relate that a drachm of it being taken as a draught, or eaten in a cake or other food, causes infatuation, and takes away the use of the reason. The person sleeps without sense, in the attitude in which he ate it, for three or four hours afterwards. Medical men also use it when they have to resort to cutting or burning."[3]

Pliny, speaking of mandragora, says, "It has the power of causing sleep in those who take it. The dose is half a cyathus. It is taken against serpents, and before cuttings and puncturings, lest they be felt. For these purposes it is sufficient for some persons to have sought sleep from the smell."[4] Apuleius, after telling us that there are two plants of mandragora, one male and the other female, which have the power of even causing death, remarks—

"If any one eat it he will immediately die, unless he be treated with butter and honey, and vomit quickly. Further, if any one is

[1] Dioscorides, iv. 76. [2] *Ibid.* [3] *Ibid.* [4] *Hist. Nat.* xxv. 94.

to have a member mutilated, burned, or sawn, let him drink half an ounce with wine, and let him sleep till the member is cut away without any pain or sensation."[1]

Aurelianus talks of the "wandering of the mind" which occurs "in those who have drunk mandragora or altercus."[2] Baptista Porta, in his *Natural Magic*,[3] which was published in 1608, speaks of the account given of the drug by Dioscorides, who "says that those men who have drunk mandragora will sleep, all their senses being entirely lost, for three or four hours after they have taken it, and that medical men use this plant when they wish to cut or burn anybody." A little further on he informs us how to prepare a sleeping apple "with opium, mandragora, juice of the water-hemlock, seeds of hyoscyamus, and to these musk is added to impart an agreeable odour, collected into a mass as large as one can hold in his fist; this, he adds, "when repeatedly smelt, softens down and binds the eyes in sleep." Subsequently he states that "it is possible to extract from several soporific plants a quintessence, which is to be shut up in well covered leaden vessels, lest the drug should evaporate. When it is to be used, the lid is to be removed and the medicament held to the nostrils, when its vapour will be drawn in by the breath and attack the citadel of the senses, so that the patient will be sunk in the deepest sleep, not to be shook off without much labour. After sleep no headache remains, nor any suspicion of art. Such things," he adds, "are clear to a wise physician, to a wicked one obscure."

Columella also mentions mandragora, but does not say much about its use.[4] Demosthenes mentions anæsthesia.[5] The effects of mandragora are also alluded to by Galen, who concludes by remarking that those who are thus affected quickly lose the sense and motion of the whole body.[6]

Innumerable passages occur among more modern authors, from which we learn that deep sleep was brought on by anæsthetising draughts, but in most of those which I shall bring forward their nature and compositions are not told us.

In *Le Procès Criminel* by Claude Lebrun de la Rochette, we have the following remark as to the employment of such drugs by criminals about to suffer torture:—"As to their artifices not to feel the pain of the rack, I saw in the first year of my reception at the

[1] *Medicaminibus Herbarum*, cap. 129. [2] *De morbis acutis*, I. 4.
[3] *Natural Magic*, viii. i. [4] Book x. 17. [5] Demosthenes, by Reiske, 564-12.
[6] Galen, vii. 207.

bar of Beaujolais, which was in the year 1588, that one of four thieves, who were prisoners,—the chief named Grand François, a man of gigantic stature, was put to the rack, fell asleep, and the toes were torn from both his feet, without his manifesting any signs of pain. One of his companions observed that he had eaten soap which stupified the nerves. The remedy to the artifice is to give wine, which being brought and drunk, he then said he was dead, and without any further torture, freely confessed an infinite number of murders and robberies, to atone for which he and his companions were broken in the wheel by sentence of Master Thomassot, Provost of the Mareschals in Beaujolais."[1]

Shakespeare, in the tragedy of "Romeo and Juliet," says—

"Within the infant rind of this weak flower
Poison hath residence, and med'cine power:
For this, being smelt, with that part cheers each part;
Being tasted, slays all senses with the heart."[2]

And a little farther on he says—

"Take thou this phial, being then in bed,
And this distilled liquor drink thou off:
When, presently, through all thy veins shall run
A cold and drowsy humour; for no pulse
Shall keep his native progress, but surcease.
No warmth, no breath, shall testify thou liv'st;
The roses in thy lips and cheeks shall fade
To paly ashes; thy eyes' windows fall,
Like death, when he shuts up the day of life;
Each part deprived of supple government,
Shall stiff, and stark, and cold, appear like death;
And in this borrow'd likeness of shrunk death
Thou shalt continue two-and-forty hours,
And then awake as from a pleasant sleep."[3]

So also in "Cymbeline" we find the following lines:—

"Those she has
Will stupify and dull the sense awhile:
Which first, perchance, she'll prove on cats and dogs;
Then afterward up higher; but there is
No danger in what show of death it makes,
More than the locking up the spirits a time,
To be more fresh, reviving."[4]

In the works of Dio Bartas the following passage occurs:—

"As the surgeon who desires to remove some incurable limb, before he approaches the knife to the diseased part, sends the patient

[1] Book ii. p. 144.
[2] Act ii. Scene 3.
[3] Act iv. Scene 1.
[4] Act i. Scene 6.

to sleep by an iced beverage, then without any pain, guided by use and art, to save the entire man, cuts off a part."

Boccaccio, in his *Decameron*, published in 1461, says—

"It occurred that the surgeon had in hand a patient, one of whose limbs was diseased, and knowing whence the evil proceeded, told the man's friends that if a rotten bone in the leg was not removed, it would be necessary to cut off the limb or the patient would die; but by removing the bone it might be cured; however, he would not undertake the operation unless the patient were given up as dead. To this the family agreed. The surgeon, thinking that if the patient were not sent to sleep he would be unable to bear the pain, and would not permit the operation, deferred performing it till the following evening; and distilled in the morning a water of a certain composition of his own, which, when the patient had drunk of it, would keep him asleep as long as the operation might last."[2]

Jacques Yver of Poitou remarks, in his "Printemps"—

"And if I am so delighted (pardon me if I cannot lie) that, like a patient set to sleep with mandragora, for the purpose of cutting off a limb, I do not feel my disease."[3]

The following extracts are from Guillaume Bouchet's work entitled *Les Sérées*, published in 1554:—"Others have written that if you take some marble from Grand Cairo, called by the ancients *memphitis*, reduce it to powder, and apply it as a liniment with vinegar, and lubricate with it the part to be cut off or cauterised, the patient will not feel much pain or inconvenience" (p. 39). "But if they have taken certain beverages, replied some one, what can the judge do? For Albert affirms that the stone denominated memphitis, when pulverised and mixed with water and butter, and taken by the malefactor who is to be tortured, causes him to feel no pain. Cardow also says that a person anointed with opium, celandine, saffron, and the marrow and fat of a man, with oil of lizards, or if he drinks wine in which the seeds of portulæa marina has been steeped for a weeke, that will prevent him from feeling any pain, and consequently from confessing anything. However, according to some, De Marsilis, who believed in these charms, and employed counter-charms, is worthy of ridicule only; they say it is proved that all the recipe for persons tortured, in order that they may feel no pain, is nothing more than soap dissolved in clean water, which

[1] See *Journal of Practical Medicine and Surgery*, 1861. Eng. Edn. p. 139.
[2] *Idem.* [3] *Idem*, pp. 139 and 140.

is given to them to drink, and has the property of lulling the senses. I should deem it still better, says another—if Pliny tells the truth—that they gave in wine an herb named archimenides, to criminals suspected of any offence ; which, being drunk, sets to sleep, and in their sleep the confession of the fact may be drawn from them much better than by any other kind of torture and rack" (pp. 63 and 64). "And I think, added he, they could give me a potion which would prevent me from hearing the thunder, since the Turks administer to those they are about to castrate, cauterise, or amputate, a composition which makes them sleep so soundly that they suffer no pain" (p. 113).

I shall close my remarks on the ancient history of anæsthetics in surgery, by quoting the following passage from Middleton's tragedy of "Women, beware Women," in which he alludes to the practice of setting patients to sleep before performing operations on them :—

> "I'll imitate the pities of old surgeons
> To this lost limb—who, ere they show their art,
> Cast one asleep, then cut the diseased part."

The History of Anæsthetics in Midwifery.—The ancients appear also to have attempted to relieve the pain attendant upon parturition by anæsthetising agents, as we may learn from various Greek writers. Such a practice is mentioned by Plautus in his *Ophelion*, and I may also quote the following passage. Theocritus says—

"For then the daughter of Antigone, weighed down with throes, called out for Lucina, the friend of women in travail, and she with kind favour stood by her, and in sooth poured down her whole limbs an insensibility to pain, and so a lively boy, like to his father, was born."[1] That author calls the insensibility to pain nodynia (νωδυνια), which is a better word than anæsthesia ; and I have often regretted not adopting it, rather than the latter. The composition of the various narcotic draughts in use for this purpose is not, however, recorded.

In Jocelyn's Life of St. Kentigern or St. Mungo of Glasgow,[2] which is contained in the *Vitæ Antiquæ Sanctorum Scotiæ*, we read that Thenu, the saint's mother, was impregnated without her knowledge, under the influence of some anæsthetic potion. In consequence of this she was condemned to be cast down from the top of a high hill called Dunpelder, which was accordingly done, but, wonderful to say, she received no hurt. Not satisfied with this apparently divine interference, her judges then sentenced her to be

[1] Idyl xviii. [2] Cap. i. *et. seq.*

cast adrift in a small boat on the Firth of Forth, but she was safely floated across to Fife, and her boat stranded near a place called Collotious (Culross), where she was received and taken care of by St. Servanus, who taught near there, and in due time she was safely delivered of a son, the afterwards famous St. Kentigern, the first Christian in Scotland.

In the trials of the sixteenth century we find many cases in which witches were prosecuted for attempting to abolish the pains of labour by charms and other means. One method that was practised was to hold a sword before the patient, who was directed to look at it steadily, in the same way that Latina is said to have held a palm branch, and brought forth Apollo without suffering; an attempt at mesmerism in reality. Another way employed was to hang the husband up in the next room by his feet, till the labour was accomplished. Such a plan would not meet with much approbation and encouragement now. The celebrated case of the Countess de St. Geran is another instance of narcotic draughts being administered to alleviate the pains of labour. After she had been nine hours in labour, the midwife gave her a mixture which kept her in an anæsthetic state till the following morning, by which time she was safely delivered of a son.

CHAPTER II.

MODERN HISTORY OF ANÆSTHESIA.[1]

Edinburgh, January 1870.

DEAR SIR—There has been sent to me from America a Chicago newspaper, containing a letter of yours which is alleged to have been published in a late number of the *Boston Medical and Surgical Journal*. In this letter you speak of the bestowal upon me, some months ago, by my fellow-townsmen, of the rank of an honorary burgess of Edinburgh; and you comment, in terms of bitterness, upon the subject, and upon what I said—or rather upon what I did not say—on that occasion. I feel assured that if you or any one else had felt as nervous and timid as I did on rising to address the

[1] Letter to Dr. Jacob Bigelow, Boston, in answer to the following.—Dr. Bigelow says—"But many persons will think it a mistake in the adoption of a foreign discovery to ignore the source whence he derived it. Sir James Simpson, in a long and eloquent reply, while he complacently accepts the crown of borrowed plumes thus tendered to him, makes not the slightest allusion to the country from which they were plucked, in which country anæsthetic inhalation, with more agents than one, was established, vindicated, and successfully practised, long before it was heard of in Edinburgh or any other part of Europe.

"It is not wonderful that, in the designs of Providence, medicinal agents should exist capable of averting pain by the suspension of sensibility; but the wonder is that, after mankind had borne pain ever since the creation of their race, any person should be found of sufficient courage and strength of conviction to put through the untried and formidable experiments necessary to decide whether life could continue under the inhalation of a scarcely respirable vapour, carried to such an extent as to destroy sensibility and produce apparent death. That man was not Sir James Y. Simpson. The history of anæsthetic inhalation is well known. It began in this country, and was first used in the extraction of teeth, and afterwards in capital operations at the Mass. General Hospital, and in obstetrical practice. The attention of the civilised world was immediately drawn to the great American discovery. Every known variety of ethers, and of compounds containing the elements of ethers, together with volatile substances, gases, and vapours, were at once submitted to the test of experiment. It is possible that better agents than those now in use will hereafter be discovered, but for the last twenty years the anæsthetic practice seems to have settled mainly on two agents—viz., sulphuric ether, with which the discovery was made, and which has thus far shown itself to be the most safe and manageable, and chloroform, which is more portable and agreeable in its odour, but which experience has shown to be more frequently attended with danger in its use." [ED.]

public meeting which witnessed the presentation, you would not be astonished at anything I did allude to, or did not allude to; or that I failed in adverting to numerous matters to which I might and ought to have adverted.

The *gravamen* of your charge is this:—In his extempore address to me on the occasion in question, the Lord Provost thought fit to allude to some of my professional investigations, and specially to those bearing on Anæsthetics, Acupressure, and Hospitalism. He spoke of the application of *chloroform* to the assuagement of human suffering as among the 'greatest medical discoveries in modern times.' In replying on the spur of the moment to this, among other remarks I stated simply, in a sentence, the amount to which chloroform was now used for anæsthetic purposes, by adverting to the great extent to which it was manufactured by one single firm at the present day. I might, if there had been time, have added evidence of the extent to which it has superseded all previous anæsthetics, by stating the amount of its manufacture by other firms here and elsewhere. But I had many other subjects to advert to besides chloroform, and only a few short minutes within which I was expected to include them all. According, however, to your views, I am very deeply blamable for not taking up a subject which the Lord Provost did not allude to—viz. the history of anæsthesia. You hold that I should have entered, to a greater or less extent, into some historical notice of anæsthetic agents. The history of them has always taken me a full hour in my University lectures; and in these lectures I have year after year paid heartily every due compliment to the most important part borne in the consummation of the practical application of anæsthetics by America, particularly by the cities of Hartford and Boston, and specially by the energy and genius of Dr. Morton. Surely, however, it would have been sadly out of place, on such an occasion, and before such an audience, to have shown that, before I discovered the application of chloroform to anæsthetic purposes, numerous other agents had been previously suggested and used for the same object—as sulphuric ether by Drs. Jackson, Morton, and Marcy; as carbonic acid by Dr. Hickman, in imitation of the experiments performed for ages on the poor dogs at the Grotto del Cane; and as nitrous oxide (an agent extensively employed as a dentist's anæsthetic at the present hour), and first proposed for 'destroying physical pains' during 'surgical operations' by Sir Humphry Davy. Or should I, in your opinion, have even gone still farther back in therapeutic history, and described what,

doubtless, as a former lecturer on Materia Medica, you are well acquainted with—namely, the other soporific vapours and measures employed by different olden surgeons in Greek, Roman, and mediæval times, with the view of rendering their operations painless to the patient? In that way I could have easily shown that the idea of making a patient anæsthetic before subjecting his body to the knife or cautery was a kind of knowledge familiar even to non-professional writers of mediæval and of later times, and that some theological writers—like Origen, for example, in the third century[1]—allude to the artificial production of anæsthesia in surgery as a well-known practice; while in reference to Scotland I might have cited Abbot Bower, who lived and wrote about the year 1400, within ten miles of Edinburgh, as telling us by what means anæsthesia in surgery was accustomed to be effected in those days, and what they gave to patients—'secandi, ut possent sine dolore secari;' or I might have adduced the monk Jocelyn as alluding, with circumstantial details, to an alleged instance of it in Scottish hagiology, as early as the sixth century. All this, and much more, might have been mentioned; but all this would have been in my opinion—though not apparently in your opinion—totally misplaced and grievously out of order; just as any historical disquisition on the previous employment of the ligature, torsion, etc., in hemorrhage, would have been when I briefly alluded to acupressure.

In the way of a climax, you terminate one of the paragraphs in your letter with the statement that I was not the 'first man' to inhale a vapour to such an extent as to destroy sensibility. Most certainly I was not; and certainly I never was foolish enough to claim to be so. In the course of my investigations I have, however, experimented upon myself with various vapours, the innocuous or the poisonous effects of which upon the body were previously altogether unknown and unascertained; and I have sometimes suffered in consequence. As a Professor of Therapeutics, you must surely be well aware that the first experiment of breathing a vapour to such an extent as to destroy sensibility was made neither in America nor in our own days. Without adverting to the acknowledged fact that it was accomplished with the vapours driven off from hypnotic

[1] In the proceedings of one of the councils of the English Church held at Exeter in the year 1287, the statement of Origen is cited—"Respondit Origenes et dicit—'Quando volunt medici incidere aliquos, vel urere, dant eis bibere aliquem potum, qui facit eos profunde dormire, ita quod amentes fiant, et sic non sentiant.'"—(See Wilkin's *Concilia Magnæ Britanniæ et Hiberniæ*, vol. ii. p. 162.)

vegetable extracts by the older surgeons, from Hugo de Lucca and Theodoric downwards, let me remind you that Sir Humphry Davy boldly—and notwithstanding he had witnessed occasional deaths in animals from it—made the experiment to which you advert many times upon himself in the last year of the last century with nitrous oxide, and further found that headache and other pains disappeared under its influence.

About forty years ago, Faraday in this country, and Godman in America, showed, as the result of their observation and experience, that the effects of the inhalation of the vapour of sulphuric ether were quite similar on the nervous system to those produced by the inhalation of the vapour of nitrous oxide gas—a truth subsequently proved by many pupils in many chemical and other schools in your country, as well as in mine,[1] by their inhalation of ether. Your remarks, as far as I understand them, imply that it is your belief that Dr. Morton was the 'first man' of 'sufficient courage' to breathe 'a vapour' so as to produce a state of anæsthesia. But you must know, as well as I do, from the official documents laid before the Senate of the United States, that this is doubtful even as regards the course of matters in America; for it appears in these documents (1st), that Dr. Jackson avers that he breathed with this effect sulphuric ether earlier than Dr. Morton; (2dly), that before Dr. Morton made the ultimate experiment upon himself in 1846, he made it first upon others, and particularly upon his pupil Mr. Spears; and (3dly), that two years previously (or in 1844), Dr. Marcy of Hartford in Connecticut had successfully excised a tumour from a man who had been rendered anæsthetic for the purpose by the vapour of sulphuric ether; whilst at that same early date, in the same city, Dr. Horace Wells had extracted teeth from a dozen or more patients rendered insensible by inhaling nitrous oxide gas[2] according to Davy's suggestion.

[1] I have elsewhere shown, in treating of the therapeutic history of sulphuric ether, that its employment by inhalation—first pointed out by Dr. Pearson in 1796—in asthma, etc., is mentioned by many writers on Materia Medica before 1846, as by Duncan, Murray, Brande, Christison, Thomson, Pereira, Nysten, Barbier, Wendt, Vogt, Sundelin, etc., in Europe: and its powers of producing effects like intoxication, or like the influence of nitrous oxide gas, are mentioned in America by Godman (1822), Samuel Jackson (1833), Wood and Bache (1834), etc.—(See *Anæsthesia, or the Employment of Chloroform and Ether*, p. 190. Philadelphia, 1849.)

[2] When, however, in December 1844, Dr. Horace Wells conceived the idea that the inhalation of nitrous oxide gas would render dentistry anæsthetic, he was evidently unaware of the previous suggestion of Sir Humphry Davy. The idea

There has lately been raised in Boston a monument in commemoration of it being the birth-place of anæsthesia in dentistry and surgery in 1846. But have the erectors of this monument cut upon it the names of either of your fellow-citizens, Dr. Morton or Dr. Jackson, as the first investigators, or the names of Warren and Heyward, as the first Boston hospital surgeons who operated upon patients under the influence of sulphuric ether? Or have they generously inscribed upon its sides any allusions to the fact that two years previously anæsthetics had been inhaled successfully in dentistry and surgery in the city of Hartford? I have been informed that there does not yet appear upon the monument the name of a single American chemist, dentist, or surgeon. Is it so? You have the monument. Have you not had the man or men?

You commence the concluding paragraph of your letter by averring that anæsthetic inhalation 'began' (to use your own words) 'in this country [America], and was first used in the extraction of teeth, and afterwards in capital operations in the Mass. General Hospital, and in obstetrical practice.' Your words so far affirm that anæsthetic inhalations, besides being first employed in America in dentistry and surgery, were in your country also first used in 'obstetrical practice.' You must excuse my saying that this last assertion is unaccountably incorrect. The use of anæsthetic inhalations in obstetrical practice was begun and extensively followed out in Edinburgh, weeks or even months before it was tried in Boston or in America. The first case of midwifery in which sulphuric ether was adopted as an anæsthetic occurred here under my care on January 19, 1847, and was soon afterwards reported in the journals of the day. On March 1, 1847, was published by me, in the *Edinburgh Medical Journal*, an essay on the subject, containing a series of obstetrical cases, and a longish discussion of the question of the applicability of anæsthetics to midwifery. It was not, however,

occurred in consequence of a Mr. Cooley, at a public lecture and exhibition of "laughing gas" at Hartford, striking and injuring his limb against the benches without suffering pain. On the subsequent day, to test the truth of the idea, Dr. Wells himself breathed deeply and fully the gas, and had a molar tooth extracted from his own mouth by Dr. Riggs without pain. *This was the first anæsthetic operation in America.* Thus, in that country, the idea itself of producing artificial anæsthesia by inhalation, and the reduction of that idea to actual practice, occurred at Hartford on December 10 and 11, 1844. The first anæsthetic operation at Boston—viz. the extraction of a tooth from a man named Frost—did not occur till 30th September 1846, or nearly two years subsequently.—(See *Official Documents*, Appendix, pp. 91, 95, etc.)

according to the published evidence of your townsman, Dr. Channing, till April 7, that the first case of the employment of anæsthetics in midwifery occurred in America, and the second did not take place till May 5.—(See Dr. Channing's *Treatise on Etherification in Childbirth*, p. 26.) But before the date of these two cases the practice had been fully established in Edinburgh and elsewhere.

Perhaps you and I, as parties implicated, are not adequate judges as to whether your statement on this point is candid and creditable, or utterly the reverse. But I willingly leave the decision of this to the feelings and verdict of an honourable profession.

You think me greatly blamable because—in the way of omission—I did not advert to the previous application of sulphuric ether in America as an anæsthetic when the employment of chloroform was referred to. I think, on the contrary, that you are infinitely more blamable, because, without the slightest reason or ground—and in the way not of omission but of deliberate commission—you have in this letter of yours attempted to appropriate for your city and country what indubitably belongs to my city and country—namely, the credit of the first introduction and establishment of anæsthetic inhalation in obstetrical practice.

I have the honour to be, yours truly,

J. Y. Simpson.

Note.—The official documents on the modern history of anæsthesia in America, referred to in the preceding letter, are contained in a thick, but, I believe, unpublished volume of seven or eight hundred pages, printed at the expense of Dr. Morton, and entitled "Statements supported by Evidence of Wm. T. G. Morton on his Claim to the Discovery of the Anæsthetic Properties of Ether, submitted to the Honourable the Select Committee appointed by the Senate of the United States:" Washington, 1853. There is included a "Report to the House of Representatives of the United States of America, vindicating the Rights of Charles T. Jackson to the Discovery of the Anæsthetic Effects of Ether Vapours, and disproving the Claims of W. T. G. Morton to that Discovery." Those interested in the question will find the whole volume full of curious and important matter in the way of memoirs, documents, affidavits, examinations of witnesses, speeches, etc.

CHAPTER III.

MODERN HISTORY OF ANÆSTHESIA—*Continued.*[1]

Edinburgh, April 1870.

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 farther." 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

[1] Second Letter to Dr. Jacob Bigelow of Boston, in answer to the following:—

Sir James Y. Simpson. BOSTON, *Feb.* 27, 1870.

DEAR SIR—In the *Edinburgh Daily Review*, October 27, 1869, is contained some account of an ovation given in Edinburgh to Sir James Y. Simpson in recognition of certain material advantages enuring to that place from his residence in the city, and of his "*discovery*," in the words of the Lord Provost, "*the greatest of all discoveries in modern times, the application of chloroform to the assuagement of human suffering.*"

As this appeared to confound what may well be spoken of as one of the greatest discoveries of modern times, with the later and more objectionable use of chloroform to imitate its result, I endeavoured to define the respective claims of America and Scotland, in an explanatory article, subsequently published in the *Boston Medical and Surgical Journal*, from which the following is a brief extract:—

"It is not wonderful that in the designs of Providence medicinal agents should exist, capable of averting pain by the suspension of sensibility. But the wonder is that after mankind has borne pain ever since the creation of their race any person should be found of sufficient courage and strength of conviction to put

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

through the untried and formidable experiments necessary to decide whether life could continue under the inhalation of a scarcely respirable vapour, carried to such an extent as to destroy sensibility and produce apparent death. That man was not Sir James Y. Simpson."

My remarks were replied to with some asperity by yourself, in a letter sent to me, which I supposed to be of a private nature, and therefore sent you the following response, not then knowing that its publication in print had been already ordered by yourself:

Sir James Y. Simpson, Bart. BOSTON, U. S. A., *Jan.* 29, 1870.

MY DEAR SIR—I acknowledge the receipt of your letter of the 3d inst., and have also received from a friend a copy of the *London Medical Times*, both expressing disapprobation of remarks published by me in the *Boston Medical and Surgical Journal* for November 25, 1869. I at first had a question whether I should not publish a part of your letter, with comments, but being unwilling to commit an act of discourtesy towards one for whom, in common with the rest of the medical world, I entertain an exalted respect, I lost sight of the matter.

I am not disposed to pursue the subject farther, now that it seems all parties are so nearly agreed as to the facts of the discovery. The erroneous impression seems to have been made by the Lord Provost of your city, whose seemingly exclusive assumption, remaining uncorrected by any one, shut out the American hemisphere from the credit of the discovery.

Poor Morton is dead, and is to have a monument. He was not a man of much cultivation or science. But, like the pioneers who have penetrated the Arctic regions and the deserts of Africa, he had a hardihood and tenacity of purpose which carried him where more cautious and perhaps better instructed men had failed to advance. As far as we know, he is the *only man*, without whom anæsthetic inhalation might have remained unknown to the present day.

I have requested my publishers to forward to you, through Messrs. Sampson, Low, & Co., a copy of my work called *Modern Inquiries*, which I shall feel flattered if you will do me the honour to accept.—Very respectfully yours,

JACOB BIGELOW.

But as your letter to me may, by its publication, tend to mislead the unprofessional part of the community, it becomes necessary to extend my reply, which I now publish as final; and before so doing, I have read your letter somewhat more carefully than at first seemed necessary.

Let me say at the outset, that you misstate my words when you represent me as saying that you were "not the first man to inhale a vapour to such an extent as to destroy sensibility." This was not what I said. What I did say of this "untried and formidable experiment" is reprinted above. Of course no man could experiment decisively on himself. No man, when etherised, could operate in any way on himself, nor could he, adequately, test on himself the degree of insensibility which surgeons require in their operations.

For more than twenty years sulphuric ether has been extensively used in the country of its discovery. It has proved itself to be one of the safest and most manageable of medicinal agents, and when administered to others may be fearlessly urged to the extent of profound insensibility, and to this extent it is always

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

carried by humane and intelligent surgeons. It is only chloroform which has piled up its hundreds of cases of disaster and death.

To judge from the array of rules and precautions laid down in the *British Medical Journal* of December 4, 1869, and copied as a curiosity in the *Boston Medical and Surgical Journal* of February 17, 1870, almost a little science has grown out of the nominal and mostly fruitless precautions, which are brought out in showy opposition to possible, and too often probable, deaths from the effects of chloroform.

You correctly quote the words of the Lord Provost, and I am quite ready to make due allowance for any "nervousness and timidity" which prevented you from disclaiming the honour of being one of the greatest medical discoverers of modern times, because you first applied chloroform to produce anæsthesia. But I felt it a duty, in behalf of my own country, to correct the statement of the Lord Provost, who obviously confounded the modern discovery of a *sure and safe anæsthesia* which belongs to Boston, with the subsequent application of chloroform to the production of anæsthesia in Edinburgh ; and was the more impelled to do this because the same mistake not unfrequently occurs through the ignorance or inadvertence of English writers ; and especially because there seemed to be also a tendency, in some of your own writings, to foster and encourage this mistake.

Here was the great medical discovery of the nineteenth century, perhaps the greatest since the creation of the world ; which dated from experiments made in Boston, in Tremont Street, and at the Hospital. It furnished to the world a *complete and optional exemption*, during several hours at a time, *from the severest pain ;* an anæsthesia *sure to occur* when desired, in every case without fail, and which *was free from danger* (which cannot be said of chloroform) ; an anæsthesia unlike any previous one, at once *certain, complete, and innocuous.* Such was this discovery of wonderful perfection at its very outset. No European surgeon then pretended to have heard of such a discovery before, and your own English friends were among the first to recognise its magnitude. It was revealed to the world, not gradually, but flashed upon it abruptly : and in less than three months afterwards, "in grateful and unhesitating recognition of it, the entire civilised world simultaneously rose up to hail it with acclamatory welcome." This was in 1846.

We may now turn for your own recognition of this boon to the human race, to the *Encyclopædia Britannica*, 1854, article "Anæsthesia," and read *See Chloroform.* Under the latter title, we find an elaborate article of many pages, containing as one of its titles, the "History of Anæsthetics," and the following paragraph offered as this *History.*

"The vapour of chloroform was first proposed by Dr. Simpson as an anæsthetic agent in surgery and midwifery in 1847. 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 oxyde gas, *had been long previously known*," *etc. etc. etc.*, and so on back

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

through that history of the middle ages which you have substantially reproduced in the letter you have sent me.

This, from an article signed J. Y. S., is offered by you as a "History of Anæsthetics"—a history of the great modern discovery of a sure and safe anæsthesia by ether, completed and perfected years before this was written. No "nervous and timid" condition dictated the deliberate sentences in this "History of Anæsthetics" by Dr. J. Y. Simpson. The excuse that they were uttered "on the spur of the moment" will hardly serve to cover this inversion of historical order in favour of the self-exaltation of the writer. This was not a time when there were only "a few short minutes." The ample pages of the *Encyclopædia* afforded you abundant opportunity for doing liberal justice to the great American discovery, had you so intended.

Your possible reply that this was an article on chloroform, not ether, will hardly avail one to whom the whole subject of anæsthesia by inhalation was intrusted in a great literary and standard work, and who availed himself of this opportunity chiefly to place himself conspicuously in the foreground.

The letter you have done me the honour to address to me, is open to criticism of the same general character. A considerable part of it 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 readers, in the midst of which, chloroform may be advantageously introduced. Your logic confounds in the same category both beneficent and detrimental agencies, the etherisation practised in America, and the cruel spectacle of the asphyxiated dogs in the Grotto del Cane. No one will disagree with you that it would have been "sadly out of place on such an occasion and with such an audience," to have entered into the details of "soporific vapours and measures employed by different olden surgeons in Greek, Roman, and mediæval times, with the view of rendering their operations painless to the patient." In this way you say you "might easily have shown that the idea of making a patient anæsthetic," was a kind of knowledge (what knowledge was there?) familiar even to non-professional writers of mediæval and of later times, and that "some theological writers, like Origen for example, in the third century, allude to the artificial production of anæsthesia in surgery, as a well-known practice." You also quote the "Abbot Bower," and the "Monk Jocelyn," who alludes to an instance of it in the hagiology of Scotland, etc., and so on down to the times and discoveries of Sir Humphry Davy, about seventy years ago.

I did not desire to provoke this mediæval history; I only wished that you might have explained what the Lord Provost evidently did not understand, the difference between the modern discovery of anæsthesia and the less important use of chloroform.

Everybody knew that people could be made drunk with alcohol, or narcotised with opium, or asphyxiated and poisoned with noxious gases, or made insensible with nitrous oxyde. But these expedients were all abandoned even by their strong partisans as *uncertain* or *unsafe*. No educated surgeon would have recommended one of them in a case of responsibility, as efficient or free from danger. But with ether came "the triple and demonstrated discovery, not of a probable and untrustworthy, but of an inevitable, complete, and safe anæsthesia."

Your prolix mediæval history is simply irrelevant, and its application illogical.

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.

A man who would believe your deductions from it, would also believe that baloons and gas-lights were known to the ancients because the classical writers believed in wax wings and subterranean fires. Sir Humphry Davy 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.

The great discovery having been made of a secure, perfect, and always attainable anæsthesia, the substitution for ether of chloroform, preferable in odour and bulk, but so far more dangerous that no life insurance company would take the risks as equal, is a matter of much less important detail.

And so of the further application of the newly discovered anæsthesia to the different forms of pain. You object to my statement which you quote, that anæsthetic inhalalation "began in America, and was first used in the extraction of teeth, and afterwards in capital operations in the Mass. General Hospital, and in obstetrical practice;" the last three words of which, afford you the text for several paragraphs of animated invective with which you close your letter. Had I attached the same importance which you do to your agency in this detail of obstetric application, I should probably have given it more prominence and careful attention. I do not now question that you were the first to use ether in labour, but who first introduced anæsthetics in obstetrical practice is a matter of limited importance. Soon after the great discovery of etherisation was made, the pages of medical journals, and the meetings of medical societies, were crowded with reports of its application to most of the ills and difficulties which flesh is heir to—colic and convulsions, dislocations, hernia and parturition, neuralgia, gout, gravel, and gall-stones—each heralded by its respective claimant as an original discovery. The several glories of these *ex-post-facto* benefactors of man, may perhaps pass into oblivion, unless rescued by some future Council of Edinburgh. The world is more interested about the origin of great discoveries than the question who afterwards suggested their various applications.

Finally you allude to the monument erected in Boston by a public-spirited individual, and which, among others, bears the following inscription:—"To commemorate the discovery that the inhaling of ether causes insensibility to pain; first proved to the world at the Massachusetts General Hospital, in Boston, October, A.D. 1846." You inquire why no individual names were inscribed upon it. I reply, because it was intended only to commemorate the city of Boston as the birth-place of the discovery; perhaps in prophetic view of some effort to confuse the history and relations of the whole subject, and then to connect some other name and place with this discovery.

Mankind are not apt to forget their benefactors, nor even those who stand in the place of benefactors. They cheerfully unite in ovations and festivities given to distinguished men by "their friends and fellow-citizens." But the suffering and now exempted world, will not forget the poor dentist, who, amid poverty, privation, and discouragement, matured, revealed, and established the most beneficent discovery which has blessed humanity since the primeval days of paradise. —Your obedient servant, JACOB BIGELOW. [ED.]

I. 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 birthplace and origin, a Lord Provost of Edinburgh in 1869—or forty or fifty years afterwards—knowing and looking to the fact that chloroform in Scotland and in other parts of Europe, etc., had for the previous twenty 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."[1]

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 hemorrhage attendant upon these operations, as cauterisation, torsion, deligation, acupressure, etc. These hemostatic means all arrest hemorrhage 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

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

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.

II. 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.,[1] since it was reintroduced a few years ago by Dr. Evans of Paris; but originally suggested by Sir Humphry 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.

[1] See, for example, papers in the *Lancet* for 2d and 9th April 1870, by Mr. Fox, "On the use of Nitrous Oxide Gas as an Anæsthetic in Surgery."

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 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 aught 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.[1]

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,[2] 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

[1] *Official Documents*, p. 323.

[2] 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.)

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.[1]

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,[2] 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 footnote) 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.[3]

Elated with his discovery, he in a week or two procceded 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

[1] 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 Humphry Davy is given.

[2] 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.

[3] 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.

gentleman being a former pupil and partner of his own, and destined to be the future discoverer of anæsthesia by sulphuric ether.[1]

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," contemptuously writes Dr. Charles T. Jackson, "for his pretensions, and left Boston. No one ever believed in his story."[2] 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."[3] In these experiments, Dr. Wells, as I have said, used the nitrous oxide gas which in 1800 Sir Humphry Davy had in England found capable in his own person of 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 Humphry 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 Humphry 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

[1] 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.

[2] *Official Documents*, p. 472. [3] *Ibid.* p. 47.

the remarkable fact that Sir Humphry 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 Humphry 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 Massachusetts 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 Humphry 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 the primeval days of paradise?"[1] 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,[2] but Dr. Marcy thought nitrous oxide the safer and pleasanter of the two, and also more easy to inhale.[3] In the essay in which your son first describes the inhalation of sulphuric ether in surgery, he points out its similarity

[1] 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 Humphry 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.)

[2] *Official Documents*, pp. 26, 27, and 43; Appendix, pp. 87, 111.

[3] 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.

to nitrous oxide.[1] "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.

III. 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 Humphry 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

[1] In the volume of *Official Documents*, p. 372, there is one case entered, suggestive of the applicability of artifitial 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."

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 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, hemorrhage, 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*."[1] This was published in Novem-

[1] 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.

ber. 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 midwifery? 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."[1]

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 applicacion 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,"[2] 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

[1] *Official Documents*, pp. 180-182.

[2] 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.)

acknowledges that I was the first to introduce anæsthetics 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 Humphry 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 ?

IV. 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.[1]

[1] 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.

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." In that article, after defining chloroform and stating its composition, modes of preparation, physical, chemical, and physiological properties, I have 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 about 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

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.)

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.[1] *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 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

[1] 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).

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*[1] (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." 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 anæthesia 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,[2] while he avoided witnessing for results.

[1] I have seen this epitome repeatedly cited, at pretty full length, in American essays on anæsthetics—sometimes with, sometimes without, acknowledgment.

[2] *Official Documents* pp. 352 and 446.

If we try to put into a summarised form the data[1] which we have been discussing regarding the introduction of anæsthesia in America and 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 Humphry 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

[1] 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 Massachusetts 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 Massachusetts 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 Massachusetts General Hospital" (Channing *On Etherisation*, 1848, p. 26).

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.

PART II.

DEFENCES OF ANÆSTHESIA.

CHAPTER I.

ANSWER TO THE RELIGIOUS OBJECTIONS ADVANCED AGAINST THE EMPLOYMENT OF ANÆSTHETIC AGENTS IN MIDWIFERY AND SURGERY.

"For every creature of God is good, and nothing to be refused, if it be received with thanksgiving."—1st TIMOTHY iv. 4.

"Therefore to him that knoweth to do good, and doeth it not, to him it is sin."—JAMES iv. 17.

Edinburgh, December 1847.

ALONG with many of my professional brethren in Scotland, and perhaps elsewhere, I have, during the last few months, often heard patients and others strongly object to the superinduction of anæsthesia in labour, by the inhalation of ether or chloroform, on the assumed ground that an immunity from pain during parturition was contrary to religion and the express commands of Scripture. Not a few medical men have, I know, joined in this same objection;[1] and have refused to relieve their patients from the agonies of child-birth on the allegation that they believed that their employment of suitable anæsthetic means for such a purpose would be unscriptural and irreligious. And I am informed, that in another medical school, my conduct in introducing and advocating the superinduction of anæsthesia in labour has been publicly denounced *ex cathedra* as an attempt to contravene the arrangements and decrees of Providence,

[1] "Pain during operations is, in the majority of cases, even desirable; its prevention or annihilation is, for the most part, hazardous to the patient. In the lying-in chamber, nothing is more true than this: pain is the mother's safety, its absence her destruction. Yet, there are those bold enough to administer the vapour of ether, even at this critical juncture, forgetting it has been *ordered*, that 'in sorrow shall she bring forth.'"—On the "Injurious (?) Effects of the Inhalation of Ether," in *Edinburgh Medical and Surgical Journal* for July 1847, p. 258.

hence reprehensible and heretical in its character, and anxiously to be avoided and eschewed by all properly principled students and practitioners. I have been favoured with various earnest private communications to the same effect. Probably, therefore, I may be excused if I attempt, however imperfectly, to point out what I conscientiously conceive to be the errors and fallacies of those who thus believe that the practice in question ought in any degree to be opposed and rejected on religious grounds.

It is almost unnecessary to begin with premising, that those who object to the superinduction of anæsthesia in parturition upon religious grounds, found their objections principally on the words of the primeval curse which God pronounced after the temptation and fall of our first parents. Few or none, however, of those who have most zealously urged the existence of this curse as a reason against the employment of anæsthetic means in obstetric practice, have, I believe, made themselves at all intimate with the words and tenor of the curse itself. I shall therefore, in the first place, quote the words of it in full from the third chapter of Genesis, interpolating in Roman letters the Hebrew originals of those two nouns which are the more immediate subjects of doubt and difference of opinion.

GENESIS, chap. iii. v. 14.—"And the Lord God said unto the serpent, Because thou hast done this, thou art cursed above all cattle, and above every beast of the field; upon thy belly shalt thou go, and dust shalt thou eat all the days of thy life:

15. "And I will put enmity between thee and the woman, and between thy seed and her seed; it shall bruise thy head, and thou shalt bruise his heel.

16. "Unto the woman he said, I will greatly multiply thy sorrow (*'itztzabhon*) and thy conception: in sorrow (*'etzebh*) thou shalt bring forth children; and thy desire shall be to thy husband, and he shall rule over thee.

17. "And unto Adam he said, Because thou hast hearkened unto the voice of thy wife, and hast eaten of the tree, of which I commanded thee, saying, Thou shalt not eat of it: cursed is the ground for thy sake; in sorrow (*'itztzabhon*) shalt thou eat of it all the days of thy life;

18. "Thorns also and thistles shall it bring forth to thee; and thou shalt eat the herb of the field.

19. "In the sweat of thy face shalt thou eat bread, till thou return unto the ground; for out of it wast thou taken: for dust thou art, and unto dust shalt thou return."

In the form of a few separate observations, I will now add the remarks and answers which I wish to make. And I would begin by observing, that—

1. The primeval curse is triple. It contains a judgment, First, upon the serpent (verses 14, 15); Secondly, upon the woman (v.

16); and, Thirdly, upon the ground for the sake of the man (vv. 17-19).—With the first of these three curses—that on the serpent—and its apparent permanence (Isaiah lxv. 25), our present inquiry has nothing to do. It is enough for me to remark, that the second and third curses—on the woman and on the ground—are evidently, from different parts of the Holy Word, not immutable. God himself, on more than one occasion, promises the removal of them, and in general conjunctly, to the Israelites, provided they would keep their covenants and obey his laws. See, for example, Deuteronomy vii. 13, "I will bless the fruit of thy womb, and the fruit of thy land," etc.; xxviii. 4, "Blessed shall be the fruit of thy body, and the fruit of thy ground," etc. See also chap. xxviii. 11, etc. In Isaiah (xxviii. 23-29), man's culture by the plough, etc., of the ground cursed by God, is said to come from the providence of God himself. "For his God doth instruct him to discretion, and doth teach him" (v. 26); and, "This also cometh forth from the Lord of hosts, which is wonderful in counsel and excellent in working" (v. 29).

2. Those who, from the terms of the first curse, argue against the superinduction of anæsthesia in labour, aver that we are bound to take and act upon the words of the curse *literally*, "I will greatly multiply thy sorrow and thy conception;" or, as Gesenius and other Hebrew authorities state, that, being a case of Hendiadys, it may be *more* correctly rendered, "I will greatly multiply the sorrow *of* thy conception;[1] in sorrow thou shalt bring forth children." If, however, we are bound to take *this* part of the curse literally, and act accordingly, then we are bound to take and act also upon *all* other parts of the curse literally. If it is sinful to try to counteract the effects of this part of it, referring to child-bearing women, it is sinful to try to counteract the other parts of it, regarding the state of the ground, and the judgment upon man. The agriculturist, in pulling up "the thorns and thistles," which the earth was doomed to bear, so far tries to counteract that part of the primary doom; and yet is never looked upon as erring and sinning in doing so. Or grant, as I have heard argued, that he may be entitled to pull up "the thorns and thistles," because the curse further implies that he was doomed to till the ground,—still he was doomed to till it by "the sweat of his face." Now if, I repeat, the whole curse is, as is averred, to be understood and acted on literally, then man must be equally erring and sinning, when, as now, instead of his

[1] "Augebo tibi *graviditatis* molestias."—Dathe's *Pentateuchus*, p. 38.

own sweat and personal exertions, he employs the horse and the ox —water and steam power—sowing, reaping, thrashing, and grinding machines, etc., to do this work for him, and elaborate the "bread" which he eats. The ever active intellect which God has bestowed upon man, has urged him on to the discovery of these and similar inventions. But if the first curse must be read and acted on literally, it has so far urged him on to these improper acts by which he thus saves himself from the effects of that curse. Nay, more; if some physicians hold that they feel conscientiously constrained not to relieve the agonies of a woman in childbirth, because it was ordained that she should bring forth in sorrow, then they ought to feel conscientiously constrained, on the very same grounds, not to use their professional skill and art to prevent man from dying; for at the same time it was decreed, by the same authority, with the same force, that man should be subject to death,—"dust thou art, and unto dust shalt thou return." If, on the other hand, it be allowed that it is justifiable in the physician to try to counteract the effects of one part of the curse, and justifiable in the agriculturist to try to counteract the effects of another part, it is surely equally justifiable in the accoucheur to try to counteract the effects of a third part of it. But if, on the contrary, it is unjustifiable for him to follow out this object of his profession, it is equally unjustifiable for the physician and agriculturist to follow out the corresponding objects of their professions. Are those who maintain the uncanonical character of using human means to contravene the pains of childbirth ready, then, to maintain that we should not use human means to contravene the tendency to death, or to increase the fertility and produce of the ground except by personal labour, and the actual "sweat" of the brow? To be consistent, they must of necessity maintain this strange and irrational view of man, and of the duties and destinies which God has appointed for man. Or, otherwise, they must own that if it is right and meet in us to exert the human intellect so as to ameliorate the condition of man from the results of the fall, it is equally right and meet in us to employ the same means to ameliorate the condition of woman from the results of the same cause.

3. But does the word sorrow (in "sorrow thou shalt bring forth children") really mean physical and bodily *pain*, as is taken for granted by those who maintain the improper and irreligious character of any means used to assuage and annul the sufferings of childbirth? Now, the word "sorrow" occurs three several times in two

consecutive verses of the curse (verses 16 and 17). The corresponding word, or rather words, in the original Hebrew, as I have already shown when citing the terms of the curse, are, *'etzebh*, and *'itztzabhon*. These nouns are both synonymous in meaning and origin, although longer and shorter in form (like labour, laboriousness—pain, painfulness—in our own language). All philologists agree that they are derived from the same root—viz. the verb *'atzabh*. The true and primitive meaning of a derivative word in the Hebrew, as in other languages, is generally the best attained by considering the signification of the root from which it is derived. The meaning of the verb *'atzabh* (the root of these nouns) is given as follows, by Professor Gesenius, the highest authority, I believe, I could quote on such a point. In his Lexicon he enters "*'atzabh* 1. To *labour*, to *form*, to *fashion*. The original idea (says he) is perhaps that of cutting, whether wood or stones. 2. To *toil with pain*, to *suffer*, to be *grieved;* used also of the mind" (Tregelles' Translation of Gesenius' *Hebrew and Chaldee Lexicon*, p. DCXLVI). Of the disputed nouns, the noun *'etzebh* ("in sorrow—*'etzebh*—thou shalt bring forth children") is nearest in form, and hence in meaning, to the original verb-root *'atzabh*—and, I believe, no scholar would deem it erroneous to affix to it the same simple original signification "*labour*," "*toil*," without deeming it requisite to believe that it at all farther necessarily imports that the implied labour and effort must essentially be to such an excess as actually to amount to the supervention of pain and agony. In fact, the Hebrew word for *labour* (in the sense of work or toil) is exactly like the English word *labour*, used also to import the act of parturition. Certainly, the greatest characteristic of human parturition as compared with parturition in the lower animals, is the enormous amount of muscular action and effort (labour) provided for, and usually required for its consummation. The erect position (*vultus ad sidera erectus*) of the human body, renders a series of peculiar mechanical arrangements and obstructions necessary in the human pelvis, etc., for the prevention of abortion and premature labour, and for the well-being of the mother during pregnancy. But these same mechanical adaptations and arrangements (such as the angle at which the pelvis is set to the spine,—the great difference in the axis of the pelvic brim, cavity, and outlet—the rigidity of the soft structures, etc.) render also, at last, the ultimate expulsion of the infant in labour, a far more difficult and more prolonged process than in the quadruped, for instance, with its horizontal body. To overcome these

greater mechanical obstacles, the human mother is provided with a uterus immensely more muscular and energetic than that of any of the lower animals. The uterus of woman is many times stronger and more powerful than the uterus, for example, of the cow. In other words, I repeat, the great characteristic of human parturition is the vastly greater amount of muscular effort, toil, or labour required for its accomplishment.[1] The state of anæsthesia does not withdraw or abolish that muscular effort, toil, or labour; for if so, it would then stop, and arrest entirely, the act of parturition itself. But it removes the physical pain and agony otherwise attendant on these muscular contractions and efforts. It leaves the labour itself (*'etzebh*) entire. And in relation to the idea that the Hebrew noun in the text truly signifies muscular *toil* and effort, and not physical *pain* and maternal agony, it is further highly important to remark, that in the very next verse (verse 17)—viz. in the first part of the curse on man—the analogous Hebrew noun (*'itztzabhon*), which we translate by "sorrow," does *not* in any degree mean or imply mortal suffering or pain, but toil and labour. "In sorrow thou shalt eat of it (the ground) all the days of thy life." Indeed, the very same noun (*'itztzabhon*), when it occurs with the same meaning, and in relation to the same curse two chapters onwards—Genesis v. 29—is, in our version, rendered by the word "toil," and not "sorrow." "And he called his name Noah (rest or comfort), saying, This same shall comfort us concerning our work or toil (*'itztzabhon*) of our hands, because of the ground which our Lord hath cursed."

The word "sorrow" is a term at once simple and striking, but, at the same time, very comprehensive in its signification; and used under various specific meanings in our authorised English version of the Bible. In the Old Testament, above twenty different terms or nouns in the original Hebrew text are translated by the single

[1] In some of the black tribes of the human race, the muscular efforts and exertions of the uterus seem to be accompanied with comparatively little or no physical pain—there is labour *without* suffering. But the black woman was cursed as well as the white; and surely it cannot be irreligious to *reduce* the sufferings of the civilised female to the degree and amount which nature has left them existing in the uncivilised female of our race. There are abundance of "maternal sorrows" connected with children and child-bearing in civilised women, quite independently of the actual agonies of parturition. My friend Dr. Churchill of Dublin, some years ago, published a large octavo volume on the affections *peculiar* to the pregnant and puerperal states, without at all including those observable *during* labour.

term or noun "sorrow" in the English text.[1] And perhaps it may not be considered irrelevant, if I remark, that the identical Hebrew noun *'etzebh*, translated "sorrows" in the 16th verse ("in sorrow—*'etzebh*—thou shalt bring forth children"), recurs in six, and I believe only in six, other passages in the Old Testament; and in not one of these does it certainly imply physical pain. In two of these six places it is rendered, in our English version, by the very word "labour," in the signification of toil or work—viz. in Prov. xiv. 23, "In all labour (*'etzebh*) there is profit;" and Prov. v. 10, "Lest thy labours[2] (*'etzebh*) be in the house of a stranger." In one passage it is translated "anger,"[3] Prov. xv. 1—"Grievous words stir up anger (*'etzebh*)." In another passage in which it occurs, in Prov. x. 22, it is rendered sorrow, but still in the sense of toil and work—"The blessing of the Lord, it maketh rich, and he addeth no sorrow (*'etzebh*)[4] with it." In Psalm cxxvii. 2, it is also, in our English version, translated "sorrows"—"It is in vain for you to rise up early, to sit up late, to eat the bread of sorrows (*'atzabhim*, the plural of *'etzebh*)."[5] And, lastly, in Jeremiah xxii. 28, the same noun is translated "idol" (a thing made, worked, or fashioned)—"Is this man Coriah a despised, broken idol (*'etzebh*)?"

The context, I repeat, in these six biblical passages in which the noun *'etzebh* recurs, shows that in *them* the word is not, in any respect, employed to designate the *sensation* of pain which accompanies the act of parturition in the human female. And it is surely not an unfair or illegitimate deduction, to infer that in the only *one* remaining, or seventh instance in which the word occurs in the Bible—viz. in Genesis iii. 16—it would be used in the sense in which it is generally elsewhere used—of effort, toil, or labour—and not in a new sense, in which it is nowhere else used—of the *feeling* or perception of excruciating suffering, or bodily anguish.

4. But that the preceding deduction is sound and just, admits of additional, and still stronger, corroborative evidence. In various passages in the Bible, the proverbial agony and pain of a woman in travail is brought in—and particularly in the inspired language of the Prophets—as a striking and beautiful simile, to mark the

[1] See a list of these various Hebrew words which the translators of the English Bible have rendered by the word "sorrow," in *The Englishman's Hebrew and Chaldee Concordance of the Old Testament*, p. 1639.

[2] "Labours," *i.e.* "things done with toil."—Gesenius.

[3] "A word pronounced with anger—a bitter, sharp word."—Gesenius.

[4] That is, no "heavy and toilsome labour."—Gesenius.

[5] "Bread obtained by toilsome labours."—Gesenius.

greatest possible degree of anguish and suffering. In not one of these passages, in which the pure pain and super-sensitive suffering of the parturient mother are thus referred to, is the word in Genesis iii. 16—viz. the word *'etzebh*—employed to designate this feeling of pain and suffering. Two other and totally different Hebrew nouns are used for this purpose in the passages to which I allude. These two nouns are *hhil* and *hhebhel*. They mark and designate the sensations of agony accompanying parturition, as contradistinguished from the muscular efforts (or labour) (*'etzebh*) in which the physiological part of the process of the expulsion of the child essentially consists. To illustrate the particular signification thus attached to the words *hhil* and *hhebhel*, as contradistinguished from *'etzebh*, I will cite the passages in which the two former nouns are used. In the following instances, the noun *hhil* is translated "pain," "pangs," etc.:—Psalm xlviii. 6, "Fear took hold upon them there, and pain as of a woman in travail." Jeremiah vi. 24, "Anguish hath taken hold of us, and pain as of a woman in travail." Jeremiah xxii. 23, "When pangs come upon thee, the pain as of a woman in travail." See also Jeremiah, l. 43. Micah, iv. 9, "Now why dost thou cry out aloud? is there no king in thee? is thy counsellor perished? for pangs have taken thee as a woman in travail." In the following instances, the noun *hhebhel* occurs in the original Hebrew with the same meaning attached to it:—Isaiah xiii. 8, "Pangs and sorrow shall take hold of them; they shall be in pain as a woman that travaileth." Isaiah xxvi. 17, "Like as a woman with child, that draweth near the time of her delivery, is in pain and crieth out in her pangs." See also Isaiah lxvi. 7; Jeremiah xiii. 21, and xlix. 23. Hosea xiii. 13, "The sorrows of a travailing woman shall come upon thee."

From what I have stated under the two preceding heads, we are then, I believe, justly entitled to infer that the Hebrew term which, in our English translation of the primeval curse, is rendered "sorrow" (Genesis iii. 16), principally signifies the severe muscular *efforts* and *struggles* of which parturition—and more particularly human parturition—essentially consists; and does not specially signify the *feelings* or *sensations* of pain to which these muscular efforts or contractions give rise. And, 2. On the other hand, the *feelings* or *sensations* of excruciating pain accompanying the process of parturition, are designated throughout the Bible by two Hebrew words which are entirely and essentially different from

that term which is translated "sorrow," in the oft-repeated expression—"in sorrow thou shalt bring forth children."

5. But even if—contrary to what, I think, the whole philological consideration of the very terms and words of the Bible shows to be the case—we were to admit that woman was, as the results of the primal curse, adjudged to the miseries of pure physical pain and agony in parturition, still, certainly under the Christian dispensation, the moral *necessity* of undergoing such anguish has ceased and terminated. Those who believe otherwise, must believe, in contradiction to the whole spirit and whole testimony of revealed truth, that the death and sacrifice of Christ was not, as it is everywhere declared to be, an all-sufficient sacrifice for all the sins and crimes of man. Christ, the "man of sorrows," who "hath given himself up for us an offering and a sacrifice to God," "surely hath borne our griefs and carried our sorrows;" for God "saw the travail of his soul, and was satisfied." And He himself told and impressed on his disciples, that his mission was to introduce "mercy, and not sacrifice." (See Matthew ix. 13; xii. 7; also Hos. vi. 6.) At the end of his commentary upon the curse in the third chapter of Genesis, the sound aud excellent Matthew Henry, in his own quaint, pithy, and zealous style, justly observes, "How admirably the satisfaction our Lord Jesus Christ made by his death and sufferings, answered the sentence here passed upon our first parents. 1. Did *travailing pains* come in with sin? We read of the 'travail of Christ's soul,' Isa. liii. 11: and the pains of death he was held by, are called ὠδῖναι, Acts ii. 24—the 'pains of a woman in travail.' 2. Did *subjection* come in with sin? Christ was 'made under the law,' Gal. iv. 4. 3. Did the *curse* come in with sin? Christ was made a 'curse for us;' died a 'cursed death,' Gal. iii. 13. 4. Did *thorns* come in with sin? He was crowned with 'thorns' for us. 5. Did *sweat* come in with sin? He sweat for us, 'as it had been great drops of blood.' 6. Did *sorrow* come in with sin? He was 'a man of sorrows;' his soul was in agony 'exceeding sorrowful.' 7. Did *death* come in with sin? He became 'obedient unto death.' Thus is the plaister as wide as the wound. Blessed be God for Jesus Christ."[1]

6. It may not be out of place to remind those who oppose the employment of anæsthetic means in labour on supposed religious grounds, that on the very same grounds many discoveries in science and art—even in the medical art—have been opposed upon their

[1] *Exposition of the Books of Moses*, p. 19.

first proposition; and yet, *now* that their first introduction is over, and the opinions and practices they inculcate are established, no one would be deemed exactly rational who would turn against the present or future *continuance* of their employment any such improper weapon. I might adduce many instances, but one may suffice for all. When small-pox inoculation was introduced towards the commencement of the last century, the Rev. Messrs. Delafaye and Massey published sermons against the practice as indefensible, on religious as well as medical grounds.[1] Inoculation was declared a "diabolical operation," and a discovery sent into the world by the Powers of Evil. And, again, when Dr. Jenner introduced vaccination instead of small-pox inoculation, towards the commencement of the present century, theological reasons again were not wanting for calling in question the orthodoxy of this other new practice. "Small-pox," argued Dr. Rowley, "is a visitation from God, and originates in man, but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"[2] "The projects of these vaccinators seem," it was affirmed, "to bid bold defiance to heaven itself, even to the will of God."[3] "Providence," reasoned another author, "never intended that the vaccine disease should affect the human race, else *why* had it not, before this time, visited the inhabitants of the globe. "The law of God,"

[1] See Delafaye's sermon on "Inoculation; an Indefensible Practice."—Massey's "Sermon against the Dangerous and Sinful Practice of Inoculation." In his admirable *Account of the Inoculation of Small-pox in Scotland* (1765), Dr. Monro (*primus*) states, "The first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a heinous crime."—P. 5. "Clergymen," observes Dr. Baron, in his *Life of Jenner*, vol. i. p. 231, "preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism." "These things," he adds, "would scarcely obtain credence, were it not that similar arguments and assertions have been employed against vaccination itself."

[2] Blair's *Vaccine Contest*, p. 84.

[3] Rowley on *Cow-pock Inoculation;* with the Modes of treating the Beastly new Diseases produced by it, p. 9.

he continues, "prohibits the practice; the law of man and the law of nature loudly exclaim against it."[1]

Such historical facts and efforts, and the results in which they have invariably terminated, are surely sufficient to make men cautious and hesitating against always recklessly calling up again the same religious, or supposed religious, arguments under the same circumstances.[2] Views and arguments of this description against every new practice intended to increase the well-being and happiness of mankind, certainly are greatly more calculated to inflict damage than benefit upon the interests of true religion.

Probably I may here be excused adding, that my friend Professor Miller informs me, that when reluctantly consenting to write the elaborate article on Etherisation, which he afterwards penned for the *North British Review* (No. for May 1847), he stated to the late Dr. Chalmers, who solicited him to undertake the task, that if he "wrote the medical, Dr. Chalmers should himself write the theological part." Dr. Chalmers at once professed that he did not see

[1] Dr. Squirrell's Preface to the second edition of his *Observations on Cow-pox*, and the dreadful consequences of this new Disease, p. iv.

[2] Perhaps, in the history of misplaced religious arguments against all novel opinions and practices, none in the retrospect may appear stranger than one that has been repeatedly mentioned to me during the few past months. Formerly, among my countrymen, most agricultural operations were performed, as commanded in the primeval curse, by personal exertion, and the "sweat of the face." Corn, in this way, was winnowed from the chaff by tossing it repeatedly up into the air, upon broad shovels, in order that any accidental currents which were present might carry off the lighter part. At last, however, about a century ago, "fanners," or machinery made for the production of *artificial* currents to effect the same purpose, were invented and introduced into different parts of the country. Some of the more rigid sects of Dissenters loudly declaimed against the employment of any such machinery. "Winds (they argued) were raised by God alone, and it was irreligious in man to attempt to raise wind for the aforesaid purpose for himself, and by efforts of his own." Mr. Gilfillan, the well-known Scottish poet, has furnished me with evidence of one clergyman debarring from the communion of the Lord's Supper those members of his flock who thus irreverently used the "Devil's wind" (as it was termed). And such sentences, I believe, were not uncommon almost within the memory of some aged members of the present generation. Sir Walter Scott, in his *Old Mortality*, introduces honest Mause Headrigg as charging the Lady Margaret Bellenden and the authorities at Tillietudlem with abetting this reprehensible practice. "And since your leddyship is pleased to speak o' parting wi' us, I am free to tell you a piece o' my mind in another article. Your leddyship and the steward hae been pleased to propose that my son Cuddie suld work in the barn wi' a new-fangled machine for dighting the corn frae the chaff, thus impiously thwarting the will of Divine Providence, by raising wind for your leddyship's ain particular use by human art, instead of soliciting it by prayer, or waiting patiently for whatever dispensation of wind Providence was pleased to send upon the sheeling hill." (Chap. vii.)

any theological part pertaining to it. Mr. Miller then explained to him, that some had been urging objections against the use of anæsthesia in midwifery, on the ground of it so far improperly enabling woman to avoid one part of the primeval curse. At last, when Mr. Miller was enabled to convince him that he was in earnest in saying that such ground *had* been taken, Dr. Chalmers thought quietly for a minute or two, and then added, that if some "small theologians" really took such an improper view of the subject, he would certainly advise Mr. Miller not to "heed them" in his article. Dr. Chalmers's mind was not one that could take up or harbour the extraordinary idea, that, under the Christian dispensation, the God of Mercy should wish for, and delight in, the sacrifice of woman's screams and sufferings in childbirth. Perhaps he thought also, as I have heard other clergymen state, that if God has beneficently vouchsafed to us a means of mitigating the agonies of childbirth, it is His evident intention that we should employ these means. The very fact that we have the power by human measures to relieve the maternal sufferings, is in itself a sufficient criterion that God would rather that these sufferings be relieved and removed. If He had willed and desired them not to be averted, it would not be possible for man to avert them. For while it is our duty to avoid all misery and suffering that is avoidable, it would certainly be impossible for us to eschew any that God had permanently and irreversibly decreed should not be eschewed.

7. I have heard objections urged against the state of anæsthesia as a counteraction to pain in surgery and midwifery, on other and different grounds from any I have yet noticed—viz. that in superinducing a temporary absence of *corporeal* sensibility, we also superinduce, at the same time, a temporary absence of *mental* consciousness. And it is argued, that, as medical men, we are not entitled to put the activity and consciousness of the mind of any patient in abeyance, for the mere purpose of saving that patient from any bodily pain or agony. Some medical men even have gravely pressed this argument. But if there were any propriety in it, why, then, these same medical men could never have been justified in doing what they have one and all of them done perhaps hundreds of times—viz. exhibit by the mouth, opium and other narcotics and hypnotics to their patients, to mitigate pain and superinduce anæsthesia and sleep. There is no greater impropriety or sin in producing sleep and freedom from pain by exhibiting a medicine by the mouth than by exhibiting it by the lungs. There is *less* impropriety in the latter

practice than in the former, even according to the very doctrine of these opponents. For narcotic or anæsthetic agents which are swallowed, are far more prolonged in their "insensibilising" action upon both the mind and body than those that are inhaled. The questionable character of the practice (supposing it for a moment to be questionable), must be much less when the effect is short and evanescent, as with ether and chloroform when respired, than when it is long and protracted, as with opium, morphia, henbane, etc., when swallowed. The proper anæsthetic state is one physiologically and psychically analogous to natural deep sleep. It is an artificial deep sleep. Those who object and urge that we should never follow ourselves, or induce others to follow, the practice of voluntarily surrendering up our mental consciousness for a time, in order to avoid any corporeal torture or agony that we should otherwise endure during that time, forget how often and how long they and others are in the habit of voluntarily surrendering up their mental consciousness in common sleep, far, far beyond the time required merely for the refreshment and renovation of the system. Many thus *daily* surrender their minds and reason up for unnecessary hours to the state of unconsciousness existing in common or natural sleep, without any object except the reprehensible indulgence of sloth and indolence; and then they turn round, and declaim against others having induced upon them, at some *rare* and extraordinary time, the unconsciousness of artificial sleep, when there is a great and laudable object in view—viz. the avoidance of excruciating corporeal suffering, and the saving of human life by saving the human system from the shock and dangers accompanying that suffering. Besides, those who urge, on a kind of religious ground, that an artificial or anæsthetic state of unconsciousness should not be induced merely to save frail humanity from the miseries and tortures of bodily pain, forget that we have the greatest of all examples set before us for following out this very principle of practice. I allude to that most singular description of the preliminaries and details of the first surgical operation ever performed on man, which is contained in Genesis ii. 21:—"And the Lord God caused a deep sleep to fall upon Adam; and he slept; and he took one of his ribs, and closed up the flesh instead thereof." In this remarkable verse the whole process of a surgical operation is briefly detailed. But the passage is principally striking, as affording evidence of our Creator himself using means to save poor human nature from the unnecessary endurance of physical pain. "It ought to be noted (observes Calvin in his commentary

on this verse), that Adam was sunk into a profound sleep, in order that he might feel no pain."[1] In his collected commentaries on the same verse, Pool quotes different authorities for the same opinion, that this deep sleep was induced upon Adam, in order that "he might not feel pain from the removal of the rib."[2] And the *profundity* of the sleep, as expressed in the Hebrew, is also worthy of note. For the noun "*tardemah*," translated in our version "deep sleep,"[3] signifies, according to all the best Hebrew scholars, the deepest form of induced slumber. In the early and very literal Greek translation which Aquilla made of the Bible, he renders, in this passage, the Hebrew word *tardemah* by the expressive Greek term καταφοξα, a term which Hippocrates, Galen, Ætius, and other Greek physicians used as implying that state of insensibility and total unconsciousness which in modern medical language we express by "coma" and "lethargy."[4] Gesenius renders *tardemah* by the Latin word "sopor," the Hebrew term for common sleep being *shenah*. In the Vulgate it is translated "sopor" (*immisit Deus soporem in Adam*). In the quotation which I have given from Calvin, that great authority renders the term *tardemah* by the expression profound "sopor" (*profundo sopore*); and Pool quotes different authorities to show that the Hebrew word does signify "sopor" of a profound kind, "*notat profundum soporem.*"[5]

[1] "Notandum, Adam profundo sopore fuisse demersum, ut nihil doloris sentiret."—*Johannis Calvini in Librum Geneseos Commentarius*, Hengstenberg's edition, p. 36.

[2] "Ne ablationis costæ dolorem sentiret."—*Poli Synopsis Criticorum aliorumque Scripturæ Interpretum*, vol. i. p. 29. See also the same opinion expressed in Rosenmuller's *Scholia Vetus in Testamentum*, vol. i. p. 106, "Adamo, somno sopito, ne dolorem sentiret:" and in the English Commentaries of Bishop Patrick, p. 14, "Whereby he was made less sensible of the pain which otherwise he would have felt in the opening his side;" and of Drs. D'Oyly and Mant, "Adam was thus less sensible of bodily pain;" etc. etc.

[3] "In Luther's German Bible, an exactly corresponding expression, "*tiefen schlaf*" is used. In Dathe's valued Latin version of the Pentateuch, a similar translation is given, "*Deus gravem Adamo soporem immisit*," p. 27.

[4] "Cataphora (from καταφερω, to sink or fall down), a term used by some authors to designate a state of coma, and by others an unusually profound sleep." —Hooper's *Medical Dictionary*.

[5] See his *Synopsis Criticorum et Scripturæ Interpretum*, p. 29.

CHAPTER II.

SAME SUBJECT CONTINUED, IN A LETTER TO DR. PROTHEROE SMITH, OF LONDON.[1]

Edinburgh, July 1848.

MY DEAR SIR—According to promise, I sit down to write you a few hurried notes on the subject of the avowed religious objections to the adoption of Anæsthesia in Human Parturition.

I regret to hear from you that, in London, the progress of Anæsthetic Midwifery is impeded by any groundless allegations as to its unscriptural character; and I can sincerely sympathise with you in your exertions to annihilate these scruples. Here, in Edinburgh, I never now meet with any objections on this point, for the religious, like the other forms of opposition to chloroform, have ceased among us.

But in Edinburgh matters were very different at first. I found many patients with strong religious scruples on the propriety of the practice. Some consulted their clergymen. One day, on meeting the Rev. Dr. H——, he stopped me to say that he was just returning from absolving a patient's conscience on the subject, for she had taken chloroform during labour, and so avoided suffering, but she had felt unhappy ever since, under the idea that she had done something very wrong and very sinful. A few among the clergy themselves, for a time, joined in the cry against the new practice. I have just looked up a letter which a clergyman wrote to a medical friend, in which he declares that chloroform is (I quote his own words) "a decoy of Satan, apparently offering itself to bless woman: but, in the end," he continues, "it will harden society, and rob God of the deep earnest cries which arise in time of trouble for help." And you are aware how earnestly some medical men attempted to preach, and, as you state, still preach against it on religious grounds. The medical friend who sent me the note from which I have quoted, himself read a wild and fanatical paper before the Medical Society

[1] See Appendix to Dr. Protheroe Smith's pamphlet on the *Scriptural Authority for the Mitigation of the Pains of Labour.* S. Highley, London, 1848.

of —— on the subject; and, I am told, it met with no small favour from the Society. I have enclosed a copy of this paper for your perusal. Some lecturers on midwifery, in London and Dublin, publicly adopted the same line of opposition and argument.

With the view of meeting, if possible, these strange and extraordinary objections, I wrote, in December last, a pamphlet on the subject of the so-called Religious Reasons against the employment of Anæsthetic Agents in Midwifery and Surgery. After its publication, I received a variety of written and verbal communications from some of the best theologians and most esteemed clergymen here and elsewhere, and of all churches—Presbyterian, Independent, Episcopalian, etc.—approving of the views which I had taken. I have letters of the same kind from some men of high rank in your church; and a note in approval was brought to me, emanating from one of your most exalted and most esteemed episcopal dignitaries.

The pamphlet itself, however, was no doubt imperfect. It was principally written during a day's confinement to my room when convalescing from the prevailing influenza. I do not know what views you intend to take in your forthcoming publication, but there are some points on which, if I had had time, I would perhaps have more insisted on in mine; and, if you will bear with me, I will briefly state them.

1. In the whole inquiry, nothing appeared to me more satisfactory or striking than the philosophic precision of the language of the Bible upon the point; and I did not sufficiently insist upon this, as an evidence of the fact that the primal curse on woman did *not* refer to the pure physical sufferings and agonies of parturition. Each so-called labour-pain consists, as you well know, of two distinct and separate elements; viz. *first*, of contraction of the uterus and other assistant muscles; and, *secondly*, of sensations of pain, more or less agonising, accompanying these contractions, and directly resulting from them. Now, I have been often struck, as you must have been, in chloroform labours, with the fact that, in the anæsthetic state, not only does the uterus contract powerfully, but the abdominal muscles often do so also, and even the face of the patient will sometimes betoken strong expulsive muscular action, while all accompanying suffering is quite annulled. We abrogate the second element of the so-called labour-pain, without destroying the first. We leave intact the expulsive muscular efforts, but remove the sense and feeling of pain accompanying these efforts. It is only of late that

these two elements or constituents of labour-pains have been recognised and studied by the profession as *two* separate objects. But it is surely, as I have above stated, worthy of remark and wonder, that the language of the Bible is, on this as on other points, strictly and scientifically correct, and long ago made, with perfect precision, the very distinction which we are now-a-days only recognising. For the Hebrew noun, *'etzebh*, distinctly signifies the muscular contraction or effort, and the nouns *hhil* and *hhebhel* as distinctly signify the sensations of pain accompanying these efforts; and you are aware, as I have elsewhere fully shown, it is not the latter but the former of these nouns that is used in the language of the primary curse:—"In sorrow (*'etzebh*) shalt thou bring forth." Now, I repeat the efforts or muscular contractions (the *'etzebh* of the curse) are, as I have just stated, left in their full and complete integrity under the state of anæsthesia; while the pangs or sufferings (or *hhil*), against which the language of the curse does *not* bear, are alone annulled and abrogated.

2. Some of your London medical divines, however, argue, I hear, that *'etzebh must* mean pain, and that, as meaning such, the curse *must* be taken *literally;* and hence that woman *must* be allowed to go on suffering. In the pamphlet referred to, I have attempted to answer this by showing that then we of the sex of Adam must adhere literally, also, to the words of the curse, so far as they apply to us, and hence must earn our bread by the "sweat of our face," and by that only. Nay, the very physicians who thus insist on reading and acting upon this and other texts literally—and literally only—forget, I fear, that, according to their own doctrines, in practising physic, they are really and truly practising a profession of sin and iniquity, in so far as man in the primeval curse was doomed to die, and yet they daily and hourly persist in attempting to make him live. An esteemed clerical friend, in writing to me on the matter, stated that he was afraid *his* cloth was perhaps even more sinful than ours, if this outrageous view were true; for the introduction of sin was the consequence of the fall, and the church, in labouring to banish and abrogate that effect—in trying to turn mankind from sin—were actually trying to cancel the greatest and most undoubted effects of the first curse upon the human race.

3. But the accoucheurs and surgeons among you who object to the use of chloroform, on the ground that it goes, in their opinion, against the object and end of the primeval curse upon woman, strangely forget that the whole science and whole art and practice

of midwifery is, in its essence and object, one continuous effort to mitigate and remove the effects of that curse. By warm baths, aperients, regulated diet, etc., they attempt to destroy the intensity of the approaching pains and penalties of childbirth ;—during labour, they use counter-pressure on the back, to relieve the intense pains there ; they use unguents, perineal support, venesection, etc. etc., to ease the pains and insure the safety of the mother. By these means they succeeded partially, in times past, in mitigating the sufferings and effects of parturition, and thought they committed no sin. But a means is discovered by which the sufferings of the mother may be relieved far more effectually ; and then they immediately denounce this higher amount of relief as a high sin. Gaining your end, according to their religious views, imperfectly, was no sin—gaining your end more fully and perfectly is, they argue, an undiluted and unmitigated piece of iniquity. To relieve our patients, however, by our interference, a little, and a little only, is assuredly, in a moral and religious point of view, just as sinful as if we succeeded in affording them complete relief from suffering. The *principle* of interference is not altered by the *degree* of relief afforded being more or less, greater or smaller. "For whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." If, on religious grounds, your obstetric friends object to relieving entirely a woman of her worst pains, now that they have the means of doing so, they must, on the very same grounds, refuse to relieve her imperfectly and partially of these or any other pains and sorrows connected with parturition ; they must, or at least ought to abstain, in fact, from all obstetric practices whatsoever ; they should, in short, give up their present profession as a profession of sin—and "in the sweat of their face" eat bread. I can see no other possible alternative for them, provided, that is to say, they choose to reduce actually their theory into practice. If, on the other hand, they think it not sinful to relieve their female patients, to a small amount, from the alleged sufferings entailed upon them by the first curse, then surely it is not sinful in them to relieve their patients from their sufferings to a far greater amount, now that they have the power of doing so—nay, is it not sinful in them obstinately to withhold that relief? For, "to him that knoweth to do good, and doeth it not, to him it is *sin*."

These remarks apply to medical practitioners. And if any of your female patients hold the same groundless doctrine—a doctrine far more in accordance with the blindness and fatalism of Mahom-

medanism, than with the spirit and genius of Christianity—if they hold that it is improper, for scriptural reasons, to abrogate the pains and sufferings of childbirth, then such mothers cannot conscientiously content themselves with rejecting merely the use of chloroform in annulling the pangs of parturition ; they must reject all kinds of medical assistance in their hour of travail ; they must give up, indeed, all assistance whatever. If the supposed pains and perils of the primeval curse are to be submitted to, on the ground that they are divinely appointed and unavoidable ordeals—then they must be submitted to in all their unmitigated power and plenitude ; no doctor must sinfully dare to stay the ebbing stream of life, if a fatal flooding suddenly supervene during labour ; no nurse must venture, as heretofore, to relieve and mitigate the agonies of the shrieking mother by counter-pressure to her back, etc., "for whosoever shall keep the whole law, and yet offend in one point, he (she) is guilty of all."

4. Those who object to the adoption of anæsthesia in midwifery, on religious grounds, entirely forget that, if God had really willed the pains of labour to be irremovable, no possible device of man could ever have removed them. I have elsewhere attempted to state this argument, but it is so much better and more clearly given in a letter in my possession, from the pen of a clergyman, Dr. ———, who is acknowledged to be one of the ripest biblical scholars, and most profound theological critics and writers of the present day, that I will cite his letter at length to you. I received it a few days after the publication of the pamphlet I have referred to.

"I have just finished the perusal of your pamphlet, entitled *Answer to the Religious Objections advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery ;* and I cannot refrain from expressing to you, though almost a stranger to you, the gratification which I have derived from it I think your argument irrefragable, both as respects the question of philology, and as respects the moral question ; and, as a theologian, I feel very grateful to you for so ably wiping away the reproach from the Bible, of discouraging any attempt to mitigate the sufferings of mankind. I am very sure the word of God, the revelation of his love and grace to man, has no such aspect ; and that it is only injudicious and ignorant zeal that leads any of its professed disciples to speak as if it had.

"The objection which you so ably expose is not, as you observe,

novel—though now, for the first time, adduced in the special application of it to the relieving of the pains of childbirth. I remember when many pious people had great scruples about endeavouring to emancipate the negroes, on the ground that they were the descendants of Ham, on whom the curse of perpetual slavery had been pronounced. I should not be surprised, in the course of the debates upon the emancipation of the Jews, to find some members pleading, as some have pleaded in former times, that to give a Jew a legitimation in any commonwealth, is a plain contravention of the will and word of God concerning that people.

"It has strongly appeared to me, for many years, that there are two principles laid down in Scripture, a due regard to which would preserve good people from those hasty applications of Scripture predictions—whether minatory or otherwise. The one is the Apostle Peter's course for the understanding of predictions, that no prophecy is self-interpreting (ἰδιας ἐπιλυσεως)," from which it follows, that in the case of a prediction threatening, we are to get at its meaning not from the words themselves in which it is couched as from those in connection with the events or circumstances by which the Almighty and Allwise fulfils his own declarations. The other is the obvious truth that God's blessing and God's curse no one can reverse; so that if any class enjoying God's blessing meet with pains, or any class exposed to his curse enjoy relief or advantage, the first inference is, that the pain was not excluded by the blessing, nor the benefit by the curse. Applying these principles to the case you have so ably discussed, I came speedily to the conclusion that, as you could not by chloroform, or anything else, set aside God's curse, and as the primary threatening is, like all predictions, to be interpreted by events in God's providence, the mere fact that by the adhibition of that agent you could relieve women from the agonies of childbirth, was to me proof sufficient that these mere agonies were not designed to form any essential part of that curse. The justice of this conclusion *a priori* your pamphlet amply substantiates by inductive reasoning."

5. The employment of anæsthesia in obstetric and medical practice is in strict consonance with the whole glorious spirit and beneficent arrangements of the Christian dispensation—for all our greatest divines are agreed, I believe, on one point, viz., that this dispensation, in the application of its principles and precepts, is intended and calculated not only to regenerate and advance our moral condition, but more and more to ameliorate the physical

sufferings and state of mankind. Witness, for example, the mighty power and resistless influence by which it has gradually acted, and is acting, through the development of its rules and doctrines, in the extermination from this earth of the curse of human slavery.

6. Some thoughtlessly argue that the employment of anæsthetic means, and the abrogation of pain in labour, must be irreligious, because it is "unnatural." They seem to think that it looks as if we fancied that nature, or rather that the God of Nature, had made the function of parturition in some repects imperfect or improper in its mechanism. These same individuals strangely forget that they themselves do not think it "unnatural" to assist and supplement other physiological functions of the body. They wear clothes to assist the protecting influence of the skin, and do not think that "unnatural." They use cookery and condiments to aid the functions of mastication and digestion. Is this because they think that nature has left the functions of mastication and digestion imperfect in their formation or mechanism? They constantly ride in coaches, etc. Is the function of progression imperfect in man? "How unnatural," exclaimed an Irish lady to me lately, "how unnatural it is for you doctors in Edinburgh to take away the pains of your patients when in labour." "How unnatural," said I, "is it for you to have swam over from Ireland to Scotland against wind and tide in a steamboat." Many habits and practices—in fact, almost all the habits and practices of civilised life are really and fundamentally as "unnatural" as assisting the function of parturition by inducing anæsthesia during it. But we do not look upon them as such, simply because they are already passed into acknowledged and universal adoption. Those who lived at the time when each separate infringement and improvement took place, could perhaps tell a story of doubt and opposition not unlike that which we can now do with regard to anæsthetic midwifery. And those who have taken up this ground of opposition, in the present as in other cases, always seem, for the time being, to forget that it is God who has endowed man with mental powers calculated gradually to enable him to extend his knowledge and improve his earthly condition, and that this extension and this improvement are so far evidently allowed and willed by God himself.

7. An additional argument is suggested in a letter lying before me, from a gentleman of high name in the literary world, to his son, who was a pupil of mine during the last winter. To understand its applicability, however, let me premise one or two words.

I have elsewhere given an extensive and carefully collected series of statistical returns from various British hospitals, showing the operations of surgery were much less fatal in their results when patients were operated on under the condition of anæsthesia, and consequently without any attendant suffering, than when, formerly, they were submitted to all the horrors and agonies of the surgeon's knife in their usual waking and sensitive state.[1] Thus, I found that, while, before the introduction of anæsthesia, in every 100 cases of amputation of the thigh performed in our hospitals, from 40 to 45 of the patients died ; the same amputation, when performed upon anæsthetised patients, did not prove fatal to more than 25 in the 100 cases ; or in other words, out of every 100 such operations, the previous induction of anæsthesia was the means of preserving 15 or 20 human lives. So much are all our surgeons here impressed with the fact that the state of anæsthesia saves their patients alike from pain, and from the subsequent hazards and dangers of pain, that I believe not one among them would deem himself justified in submitting a fellow-being to the tortures of the operating table, without the previous employment of chloroform. And, I believe, you are aware that we also use it here, in Edinburgh, constantly in midwifery—its omission being the exception, and a rare exception, to the general rule of its employment. By thus shielding our patients against the more severe portion of the pains of parturition, we not only save them from much immediate suffering, but we save their constitutions also from the effects and consequences of that suffering ; and, as a general rule, they assuredly make both more rapid and more perfect recoveries. I most sincerely believe, that in thus cancelling the pains of labour, we also, to a great extent, cancel the perils of it ; for all our highest authorities in pathology admit that pain, when either great in excess, or great in duration, is in itself, and by itself, deleterious and destructive ; and the mortality accompanying parturition is regulated principally by the law of the length and degree of the patient's struggles and sufferings. In the Dublin Lying-in Hospital, when under Dr. Collins's able care, out of all the women, 7050 in number, who were delivered within *two* hours from the commencement of labour, 22 died ; or *one in every* 320. In 452 of his cases, the labour was prolonged above *twenty* hours, and out of these 452 mothers, 42 died ; or *one in every* 11 ; a difference enormous in amount, and one strongly calculated to force us all to

[1] See Part ii. chap. 6.

think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

Now the writer of the letter to which I have alluded is the author of one of the most eloquent essays in the English language, on the holy character and genuineness of the Bible. He is not a physician, though deeply read in medical, as in all other forms of knowledge; and, aware of the dangers and destructive properties of severe pain, when unmitigated and unrestrained, he reasons thus: "If pain, when carried—as in parturition—to the stage which we call agony, or intense struggle amongst the vital functions, brings with it some danger to life, as I presume no one can deny *must* be the case, then it will follow, that, knowingly to reject a means of mitigating, or wholly cancelling, the attendant suffering and its dangers (now that such a means has been discovered), travels, in my opinion, on the road towards suicide. If I am right in believing that danger to life lies in this direction, then, clearly, the act of rejecting the remedy against it, being wilful, lies in a suicidal direction. It is even worse than an ordinary movement in that direction, because it affects to make God an accomplice through the Scriptures in this suicidal movement, nay, the primal instigator to it, by means of a supposed curse interdicting the use of any means whatever, though revealed by Himself, for annulling that curse." The same argument which is here brought against the wilful rejection of anæsthetic measures by the patient, necessarily applies with the same spirit, but with some changes in the terms, against the wilful rejection of the same means by the medical attendant.

But I must be done; for I fear I have exhausted your patience as well as my own time. Let me merely add, that I am sure you deeply regret and grieve with me that the interests of genuine religion should ever and anon be endangered and damaged by weak but well-meaning men believing and urging that this or that new improvement in medical knowledge, or in general science, is against the words or spirit of Scripture. We may always rest fully and perfectly assured that whatever is true in point of fact, or humane and merciful in point of practice, will find no condemnation in the Word of God.

With many apologies for the unexpected length to which these remarks have extended,—Believe me, my dear Sir, very faithfully yours,

J. Y. Simpson.

CHAPTER III.

MERE OPINIONS AND PREJUDGMENTS NOT SUFFICIENT TO SETTLE THE QUESTION OF THE PROPRIETY OR IMPROPRIETY OF ANÆSTHETIC AGENTS : ILLUSTRATION FROM THE HISTORY OF VACCINATION.

"The multiplied experiments to prevent pain in surgical operations, which bear so delightful a testimony to the humanity of their authors, will certainly, in the course of time, be crowned with success."—*Marx's Akesios—Letter to Herman Boerhaave.*

Edinburgh, September 1847.

During the latter half of the last century, 30,000 individuals were computed to die annually of small-pox in England.[1] From the official returns of the Registrar-General, it appears that in England and Wales the number that perish annually of this same disease at the present time is reduced to less than 10,000.[2] In England alone, therefore, the absolute mortality from small-pox is less by twenty

[1] Dr. Gregory observes, "The total deaths by small-pox throughout England were estimated at about 45,000 annually."—*Cyclopædia of Medicine*, vol. iv. p. 402. Dr. Haygarth calculated the annual number of deaths from small-pox to amount to 38,000 in 8,000,000 of inhabitants.—See the data of his computation in his *Sketch of a Plan to exterminate Small-pox*, 1793, p. 144. In making the various computations regarding vaccination in the text, I have, in order to avoid the possibility of error, kept all the calculations considerably below the ascertained data.

[2] During the five years from 1838 to 1842 inclusive, there died, on an average, 8893 individuals yearly of small-pox. In 1842, only 2715 died. —See *Sixth Annual Report of the Registrar-General*, p. 514. Formerly, 1 in about every 250 of the general population died annually of small-pox ; now, only 1 in about every 1700. In England, the registration of every birth and every death is properly enforced by law. If the registration of the vaccination of each child were enforced as rigorously as the registration of its birth, much disease, and many thousand human lives would thus undoubtedly be saved annually in Great Britain. Surely it is a subject well worthy of the attention of a benevolent legislature. We see the good effects of such interference in other European states. For, whilst in England (the native country of Jenner), still 1 in every 1700 inhabitants dies annually of small-pox ; in Austria, 1 in 4800 dies of this disease ; in France, 1 in 11,000 ; and in Sweden, only 1 in 27,000. On the great extent of the number of individuals in society who remain unvaccinated, see some excellent remarks by Dr. Stark, in the *Edinburgh Medical and Surgical Journal*, No. 161.

thousand a-year than it was half-a-century ago. If a similar rate of reduction in the number of deaths from small-pox holds good—as we have every reason to believe is the case—in the other kingdoms of Europe, then, out of the 220 millions of people that inhabit this quarter of the globe, 400,000 or 500,000 fewer die of small-pox, than, with a similar population, would have died from this malady fifty years ago. In other words, according to this rate of computation, there are now preserved from death by small-pox in England, during the currency of a single half-century, a number of lives greater in amount than the whole existing population of Wales. There are preserved in Europe, during the same period, a number of lives greater in amount than the whole existing population of Great Britain.

For this mighty triumph of medicine over one of the most loathed and dreaded forms of human disease and death, science stands indebted to the inestimable discovery of Dr. Jenner;[1] and every medical man is ready to allow, at the present day, that his discovery is not less remarkable in consequence of its gigantic results and amazing success, than in consequence of the singular simplicity and safety of the means with which that success is obtained. For no one now dreams of ever expecting any deleterious or dangerous consequences to ensue from vaccination; and, indeed, the performance of it has been mainly or entirely conducted, in some districts, by non-professional individuals—by the priest as well as by the physician—by the nurse as well as by the surgeon.

Yet at the time of Dr. Jenner's first public announcement of vaccination in 1798, and for many years subsequently, the proposal of substituting vaccine for variolous inoculation was encountered by various members of the profession, with incredulity and ridicule, and direct and determined opposition. The measure by which he taught medical science to save annually from death thousands of human

[1] In answer to those who have affected to doubt entirely the utility of physic and physicians, medical science may proudly point to the results of vaccination. During the long European wars connected with and following the French Revolution, it has been calculated that five or six millions of human lives were lost. In Europe, vaccination has already preserved from death a greater number of human beings than were sacrificed during the course of these wars. The lancet of Jenner has saved far more human lives than the sword of Napoleon destroyed. On these devastating European wars England lavished millions of money, and freely bestowed honours, peerages, and heavy annual pensions upon the soldiers who were most successful in fighting her battles and destroying their fellow-men; she grudgingly rewarded Jenner with thirty thousand pounds for saving thirty thousand of her subjects annually.

lives in England, and hundreds of thousands throughout Europe, was, on its first introduction, bitterly denounced and decried in different quarters, its effects doubted, and its own safety and propriety strongly and strenuously called in question.

Dr. Squirrell earnestly and publicly supplicated his Majesty George the Third to suppress "the destructive practice of vaccine inoculation throughout his dominions."[1] "It ought," observed Professor Monro of Edinburgh, "to be prohibited by Act of Parliament."[2] "The College of Physicians have," exclaimed Dr. Moseley, "a duty to perform, and I trust this business will not escape them."[3] Others, despairing of interference on the part of the King, Parliament, or Colleges, appealed to the people themselves. "It would," said Dr. Brown, "undoubtedly be downright madness to imagine *they* will condescend to encourage it."[4] The Anti-Vaccinarian Society called upon the public "to second their efforts in supporting the cause of humanity against cow-pox injuries," and besought their aid to suppress "the cruel despotic tyranny of forcing cow-pox misery on the innocent babes of the poor—a gross violation of religion, morality, law, and humanity."[5]

Frightful, and even fatal consequences were boldly averred to be the direct and immediate results of vaccination. Deaths from cow-pox inoculation were published in the mortality bills of London.[6] "I have," alleged Dr. Moseley, physician to the Chelsea Hospital, "seen children die of the cow-pox without losing the sense of torment even in the article of death."[7] Dr. Rowley, physician to the St. Marylebone Infirmary, professed to publish true accounts of fifty-nine deaths from "cruel vaccination;" and added, that "when humanity reflects" on these and (to use his own words) "a great heap of victims diseased for life, and likely to transmit to posterity, for ages, beastly chronic diseases,[8] it is enough to freeze the soul with horror." And "it is," he exclaims, "the duty of honourable men in the medical profession to alarm mankind of the impending danger of vaccination; to warn society of the multifarious evils that await them in the form of this mild catholicon,

[1] *Observations on the pernicious consequence of Cow-Pox Inoculation.* 2d ed. London, 1606, p. vi. [2] *Edinburgh Medical and Surgical Journal*, vol. xv. p. 64.

[3] *A Treatise on the Lues Bovilla.* 2d edition. London, 1805, p. xiv.

[4] *A Letter in Reply to the Surgeons of the Vaccine Institution.* Edinburgh, 1809, p. 96. [5] See their address of 1806 in Blair's *Vaccine Contest*, p. 56.

[6] Mr Blair's Pamphlet, p. 95. [7] Treatise, p. 95.

[8] *Cow-Pock Inoculation;* with the Modes of Treating the Beastly new Diseases produced by Cow-Pock. 2d edition, 1805, p. 128.

of a sweetened potion that carries fatal poison in all its destructive particles."[1] He elsewhere eloquently declaims against "affectionate parents being robbed of their serenity, and the minds of tender mothers being wrung with eternal suspense," "whilst a few projectors or visionists are pursuing their deleterious projects on human victims," and perpetrating a "dangerous innovation which so many fatal facts illustrate."[2]

Mr. Lipscomb urgently maintained, in an essay on small-pox inoculation, published in 1805, that cow-pox, the "new scourge industriously dispersed to afflict the children of men," is "sometimes fatal of itself, and that the diseases introduced or brought into action by it may be also sometimes fatal, and can never be completely guarded against."[3] One author had seen "numerous instances" of vaccination producing eruptions, remaining "for months and even years afterwards, undermining the constitution, and very frequently terminating in phagedenic or corroding ulcers." And he had likewise witnessed coughs, dyspnœa, hectic marasmus, tedious and difficult to eradicate, etc., result from cow-pox. "Shocking reflection," he adds, "to a humane mind, that a poison should thus be introduced into the human constitution without the plea of necessity, or the support of reason and experience."[4] "Several children," observes Dr. Moseley, "have died from diseases brought on by the cow-pox where no ulcerations had appeared, and others have lost their nails and ends of their fingers, several months after the inoculation."[5] "My accounts from the country are," he continues, "full of dismal histories of ulcerated arms and mortifications."[6] "Blindness," it was averred, "lameness, and deformity, had been the result of employing the vaccine in innumerable instances, and its fatal venom had removed many an infant untimely from the world."[7]

Nay, it was strenuously maintained and believed, that not only were various old maladies, peculiar to man, thus excited into action by the "cow-pox poison," but that different new diseases peculiar to the cow were sometimes communicated to the human constitution by vaccination. "Various beastly diseases," writes Dr. Rowley,

[1] *Cow-Pock Inoculation;* with the Modes of Treating the Beastly new Diseases produced by Cow-Pock. 2d edition, 1805, p. 14. [2] *Ibid.* p. 128.

[3] *Inoculation for the Small-pox vindicated*, etc., 1805, p. 40.

[4] *Observations addressed to the Public on the Cow-pox*, pointing out the dreadful consequences of this new Disease, so recently and rashly introduced into the Human Constitution. By R. Squirrel, M.D., 1805, pp. 16, 17.

[5] *Treatise on the Lues Bovilla*, p. 118. [6] *Ibid.* p. 92.

[7] Moore's *History of Vaccination*, p. 39.

"common to cattle, have appeared among the human species since the introduction of cow-pox—cow-pox mange, cow-pox abscess, cow-pox ulcer, cow-pox gangrene, cow-pox mortification, and enormous hideous swellings of the face, resembling the countenance of an ox, with the eyes distorted, and eyelids forced out of their true situation; diseased joints, etc."[1]

This was published in 1806, eight years after Dr. Jenner's first essay on vaccination appeared. During the year subsequent to the first public announcement of his discovery, Dr. Moseley suggested the possibility of the "bestial humour" of cow-pox producing "a brutal fever, exciting incongruous impressions on the brain;" and "who knows," says he, "but that the human character may undergo strange mutations from quadrupedan sympathy, and that some modern Pasiphaë may rival the fables of old?" Some, after vaccination, were actually supposed to "cough like cows," and "bellow like bulls."[2] And one anti-vaccinist ingeniously suggested that if cow-pox were known to have existed in a family, this fact might debar the members of it from the chances of matrimony. For[3] "it would," he remarks, "be no letter of recommendation, and it would be cruel for the world to know who had laboured under the cow-pox mange, evil, ulcer, or any other beastly disease; it might infallibly injure their fortune in life, particularly in matrimonial alliances. Who would marry into any family, at the risk of their offspring having filthy beastly diseases?"

Nor were theological reasons, of course, wanting for calling in question the orthodoxy of vaccination, as of other new discoveries and practices."[4] "Small-pox," argues Dr. Rowley, "is a visitation

[1] *Cow-pox Inoculation*, p. 105. See prefixed to the work the coloured portrait "of a cow-poxed, ox-faced boy," with two scrofulous abscesses, which were at one time alleged to indicate sprouting horns!—"This boy," observed Dr. Rowley, in a clinical lecture on the case, "is gradually losing the human lineaments, and his countenance is transmuting into the visage of a cow." (Moore's *History*, p. 46.) He further wrote—"A great number of new complaints, the diseases of beasts, filthy in their very nature and appearance, in the face, eyes, ears, with blindness and deafness, spreading their baneful influence over the whole body, have been not unfrequently the consequence evidently of cow-pox inoculation; either originating from the grease in horses, or the natural diseases of cows," p. 12.

[2] Mr. Ring, in his treatise on cow-pox, mentions "a lady who complained that since her daughter was inoculated, she coughs like a cow, and has grown hairy all over her body; and Mr. Blair was told, on a late excursion into the country, that the inoculation of the cow-pox was discontinued there, because those who had been inoculated in that manner bellowed like bulls!"—Blair's *Vaccine Contest*, p. 69.

[3] Introduction to Rowley's pamphlet, p. vii.

[4] As, for example, small-pox inoculation: see a "Sermon against the danger-

from God, and originates in man; but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the Gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"[1] "The projects of these vaccinators seem," it was affirmed, "to bid bold defiance to heaven itself, even to the will of God."[2] "Providence," reasoned another author, "never intended that the vaccine disease should affect the human race, else why had it not, before this time, visited the inhabitants of the globe. The law of God," he continues, "prohibits the practice; the law of man and the law of nature loudly exclaim against it."[3]

In short, vaccination was opposed and denounced on a variety of grounds. It was alleged to be occasionally fatal in its consequences; to be liable to excite various diseased actions and predispositions; to produce diseases new to the human constitution; to "be impious, unthinking, profane, and irrational;" to be an innovation, neither "established on the basis of reason, nor supported by the foundation of truth."[4] "The vaccine," exclaimed one enemy to cow-pox inoculation, "was the damnedest thing ever proposed; he wished the inventors were all hanged, and he would give his vote for its being done."[5] And strong pictures were hung up to the public eye of the miseries it would infallibly lead to in case of

ous and sinful practice of Inoculation," preached at St. Andrew's, Holborn, in 1722, by Edmund Massey, M.A. He urges various theological arguments against the "diabolical operation" of inoculation, and at last maintains that, even if it were medically successful, it was not to be courted, for he believes, if mankind should thus "happen to become more healthy, it is a great chance but they would be less righteous."—P. 26. In his admirable "Account of the Inoculation of Small-Pox in Scotland (1765)," Dr. Monro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a heinous crime."—P. 5. "Clergymen," observes Dr. Baron, in his *Life of Jenner*, vol. i. p. 231, "preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things," he adds, "would scarcely obtain credence, were it not that similar arguments and assertions have been employed against vaccination itself."

[1] Rowley's Pamphlet, p. 9.
[2] *Vaccine Contest*, p. 84.
[3] Preface to the second edition of Dr. Squirrell's *Observations*, p. 4.
[4] Blair's *Essay*, p. 83; and Lipscomb's Pamphlet, p. 28.
[5] Moore's *Reply to the Anti-Vaccinists*, 1806, p. 14.

the recurrence of epidemic small-pox. "In many families,"[1] writes an author whom I have already quoted, "there will be none to attend the sick, nurses will quit their patients for their own safety, and servants fly from their masters' houses to shun the pestilence. Then we shall experience an horrid scene of public and private calamity—brought on by a medical experiment, embraced without due consideration, extended by a rash transgression over the bounds of reason; and, after the fullest conviction of its inutility, obstinately continued, by the most degrading relapse of philosophy that ever disgraced a civilised world."

Such were the chief forms of opposition and argument that were stoutly and vigorously urged against vaccination during the earlier years of its progress. They are the same by which many of the happiest and greatest improvements in our profession have each in turn been assailed at their first promulgation. From time to time in the march of medicine and other allied sciences, some earnest and expanded mind conceives and elaborates a great and novel thought, destined in its practical application to ameliorate the condition and promote the happiness of mankind. But hitherto, almost as often as the human intellect has been thus permitted to obtain a new light, or strike out a new discovery, human prejudices and passions have instantly sprung up to deny its truth, or doubt its utility, and thus its first advances are never welcomed as the approach of a friend to humanity and science, but contested and battled as if it were the attack of an enemy.[2] Practical medicine, in its past career, is full of instances illustrative of this remark. Witness the history of the immense and now almost forgotten difficulties accompanying the first introduction of mercury, antimony, and cinchona-

[1] Preface to *Treatise on Lues Bovilla*, p. 23.

[2] This remark holds true, for instance, with regard to small-pox inoculation, etc. etc. Lord Wharncliffe, in his edition of the letters and works of Lady Mary Wortley Montagu, after giving the history of her ladyship's introducing the practice of small-pox inoculation into England from the East, observes :—"What an arduous, what a fearful, and, we may add, what a thankless enterprise it was, nobody is now in the least aware. Those who have heard her applauded for it ever since they were born, may naturally conclude, that when once the experiment had been made and had proved successful, she could have nothing to do but to sit down triumphant, and receive the thanks and blessings of her countrymen. But it was far otherwise. Lady Mary protested that in the four or five years immediately succeeding her arrival at home, she seldom passed a day without repenting of her patriotic undertaking; and she vowed that she never would have attempted it if she had foreseen the vexation, the persecution, and even the obloquy it brought upon her. The clamours raised against the practice, and of course against her, were beyond belief. The faculty all rose in arms to a man,

bark, into medical practice; or the stern obstinacy with which the ligature of arteries after amputation was long, long rejected, and cauteries and caustics preferred; or the professional and religious prejudices which the propriety of saving human life by inducing premature labour has encountered up to within the last few years. Further, every proposed improvement seems to be met with the same invariable array of objections and arguments. The discovery may be new, but the grounds of opposition to it are not new—they are merely the old forms of doubt, and difficulty, and prejudice, used on former occasions, recalled and reproduced anew. Thus, not only in their leading principles and spirit, but in most even of their minute details, identically the same arguments that forty or fifty years ago were urged against the propriety and safety of vaccination, or a hundred years ago against small-pox inoculation,[1] have, within the last few months, been again invoked and used against the employment of etherisation. Time has amply proved how futile and inapplicable these arguments were as directed against vaccination. In truth, those forms of reasoning and opposition against the employment of cow-pox that, some forty or fifty years ago, appeared to many members of the profession to be perfectly conclusive and insuperable, now read and appear to us at the present day as in the highest degree illogical and absurd. History has been compared to a mirror, in which we may study the faults of our predecessors, with the view of avoiding the same errors ourselves. The history of cow-pox is certainly calculated to teach us this one lesson, that in relation to the truth of any novel doctrine or practice, such as vaccination or etherisation, adverse opinions and prejudgments are, however strongly entertained, or however strongly expressed, not in

foretelling failure and the most disastrous consequences; the clergy descanted from their pulpits on the impiety of thus seeking to take events out of the hand of Providence; the common people were taught to hoot at her as an unnatural mother who had risked the lives of her own children.

"We now read in grave medical biography, that the discovery was instantly hailed, and the method adopted, by the principal members of that profession. Very likely they left this recorded; for whenever an invention or a project—and the same may be said of persons—has made its way so well by itself as to establish a certain reputation, most people are sure to find out that they always patronised it from the beginning; and a happy gift of forgetfulness enables many to believe their own assertion."—*Letters and Works of Lady Mary Wortley Montagu.* Edited by her great-grandson Lord Wharncliffe, vol. i. p. 55.

[1] "The very same objections," writes Mr. Moore in 1805, "accompanied with the same species of proof, were adduced against it (small-pox inoculation), as are now brought against vaccination."—See his *Reply to the Anti-Vaccinists*, p. 70.

themselves adequate, as some, at the present time, would seem to believe, to decide the whole matter in dispute, either in one direction or another.[1] And the moral is obvious—that while minds anxious to promote new and probable inquiries should not be intimidated and deterred from their pursuit by such prejudgments on the part of others, those who are, on the contrary, anxious to suppress them, should not venture to base their opposition upon mere impressions and mere opinions only. The ultimate decision upon such investigations ever comes to be founded, not upon preconceived beliefs or hasty deductions, but upon the careful examination and evidence of a sufficient body of accurate and well-ascertained facts. During the last six months, etherisation has been used to a considerable extent in British surgery; and, at the present time, we

[1] I have been told that any comparison between the progress of vaccination and etherisation cannot be true in one respect, that vaccination was at once and generally received. The quotations in the text show the contrary; and many of the strongest adverse opinions which I have quoted were published in 1805-6, seven or eight years after Dr. Jenner published his first admirable essay on the subject in 1798. After Dr. Jenner published this essay, he went to London, and resided there for nearly three months; but during this time, "with all his efforts and those of his friends, he was unable in the metropolis to procure one person on whom he could exhibit the vaccine disease. Not one individual would submit to it. After Jenner left London, Mr. Cline made the first experiment in London with cow-pox, by inoculating it as a counter-irritant on the hip of a patient affected with morbus coxarius."—(Baron's *Life of Jenner*, vol. i. p. 150.) Jenner first tried artificial vaccination in May 1876. In March 1800, Mr. (afterwards Sir Matthew) Tierney wrote him from Edinburgh, where he was then a student, stating that "Dr. Gregory, the professor of physic, knew very little about it, and, of course did not encourage it. Mr. Anderson, a surgeon at Leith, is the only person here who has tried it."—(*Ib.* p. 376.) As late as 1805, the popular opinion in London was much shaken with regard to the propriety aud safety of vaccination. "The influence," says Mr. Blair, "of false rumours and distorted facts operated so strongly in the district of Bloomsbury and St. Giles, as to preclude even a single person from applying for vaccination at that dispensary."—(Pamphlet, p. vi.) Dr. Moseley boasts (p. 13 of his Treatise, second edition), that at that date, 1805, the middle and inferior classes in London had "renounced the delusion," and would not "expose their children to cow-pox." Instances of more marked popular hostility against it were not wanting in the early history of cow-pox. Mr. Gooch states, that the first people he vaccinated in Hadleigh, Suffolk, "were absolutely pelted and drove into their houses if they appeared out."—(Baron's *Life*, vol. i. p. 382.) In the town of St——n, Kincardineshire, a surgeon was lately used in a similar manner, for venturing to etherise a patient for extraction of a tooth. But still etherisation has made more progress in months than vaccination affected in years; and already, within a few short months, a knowledge of it has spread over almost the civilised world. Within these few days, I received a letter of consultation from a lady, asking some directions for the use of etherisation at her approaching confinement, in October next. The letter was from the Far West, and dated "Mount Morris, Illinois, United States."

are perhaps in a condition to turn and look back upon this past experience with it, in order to endeavour to form, from the existing facts and cases, a proper judgment upon its merits or demerits, and especially in order, if possible, to obtain some satisfactory light upon that all-important question in relation to its employment—viz. whether its adoption increases or diminishes the usual mortality consequent upon surgical operations.

CHAPTER IV.

THE PREVENTION OF PAIN IN SURGICAL OPERATIONS ALLEGED TO BE UNNECESSARY AND IMPROPER.

Edinburgh, September 1847.

THERE is one strange episode which has been frequently reacted in the early history of several of those practical improvements, which we all now proudly refer to, and rejoice over, as among the greatest and most undoubted advances made in the past march of medical science. It is this. Some striking discovery happens to be made, or some great improvement suggested. Its worth and importance, betimes, are acknowledged by different members of the profession. Others, however, full of doubts and difficulties, conjure up to their own minds, and to the minds of their brethren, all the usual forms of objection to the new view that has been propounded; and the more ardent among these opponents always fix, and insist among other points, upon this special and singular ground of objection, that the disease or evil proposed to be remedied is comparatively and truly less obnoxious in its character than was in general previously imagined, that its removal is, consequently, not a matter of much moment, and that the new and artificial measure now suggested for that purpose is, probably, in its action and effects on mankind, really more dangerous and deleterious than the very state or malady which it was proposed to remove by it.

Take, for instance, as an example of the remark, the strong opposition offered first to variolous inoculation, and afterwards to vaccine inoculation.

It has been repeatedly calculated in regard to Europe, that before the introduction of variolous inoculation, small-pox regularly decimated the human race, one out of every ten deaths that occurred being a death from small-pox; and further, the disease was reputed fatal to at least one out of every six or eight individuals attacked by it. Inoculation was introduced, and its effects were so marked, that of those who submitted to the disease in this

form, about one only in every 300 or 400 seems to have perished.[1] Vaccination was discovered, and this preventive affection was found to prove rarely or never *per se* dangerous or fatal to life. But science, in substituting—first, inoculated for natural small-pox; and secondly, cow-pox for inoculated small-pox, was anxiously combated by the double argument, that the old evil was not so great an evil as it was usually represented to be, and that perhaps it was in reality safer and better than the new measure proposed as a substitute for it.

Thus, a hundred and twenty years ago, among the various pamphlets published with the view of contesting and opposing the propriety of the newly introduced Variolous Inoculation, Mr. Howgrave, whose Essay on the subject is one of the best of the period, maintains, that "the small-pox in the natural way very rarely affects life where the habit of body and constitution are good;"[2] "that the hazard of inoculation is not only not considerably less, but considerably greater than that of the natural small-pox;"[3] "that this "new and strange method,"[4] "more frequently produces accidents than the natural way,"[5] that it is "not only unsafe but uncertain,"[6] and that, "all persons who will suffer reason to determine their opinion, must be convinced that their (the inoculation) method has no degree of safety in it."[7] Hence we can scarcely wonder when he "affirms that the best method is to trust Providence, and not allow mortal man to inflict diseases upon us at his pleasure."[8] Other contemporary writers against small-pox inoculation, maintained, upon the same grounds, the same views of the impropriety of the practice.[9]

Again, forty or fifty years ago, in opposition to the proposal of Dr. Jenner to introduce vaccination, or, in other words, to substi-

[1] In reference to the mortality of natural and of inoculated small-pox, Dr. Gregory, Physician to the Small-Pox Hospital of London, observes, "It is commonly stated that one-fourth of those who are attacked by small-pox in the natural way perish."—*Cyclopædia of Medicine*, vol. iii. p. 742. And "The average number of deaths (from inoculated small-pox) at the Inoculation Hospital, was only three in a thousand (or about 1 in 330). In the wards appropriated to the casual diseases the deaths were, and continue to be, 3 in 10," or more than 1 in 4.—*Ibid.* vol. iv. p. 749.

[2] *Reasons against the Inoculation of the Small-pox.* London, 1724, p. 67.

[3] *Ibid.* p. 72. [4] P. 8. [5] P. 61. [6] P. 5.

[7] *Ibid.* p. 28. [8] *Ibid.* p. 63.

[9] See, for instance, the Rev. Mr. Delafaye's Sermon on *Inoculation, an Indecensible Practice*, pp. 22, 23; or *A Letter in Reply*, by N. Bolaine, Surgeon, p. 17. Sparham's *Reasons against the Practice of Inoculating the Small-pox*, p. 27. Dr. Wagstaffe's *Letter* to Dr. Friend, showing the danger and uncertainty of Inoculating the Small-pox, pp. 48, 49, etc.

tute the inoculation of cow-pox for the inoculation of small-pox, the same objections to the practice were reproduced. Dr. Moseley stated,[1] for example, several years after vaccination was introduced, that "the mischiefs of the natural small-pox, we all know, are great enough, but they may be prevented by inoculation;" that, "instead of 1 in 300 dying of the small-pox from inoculation, there need not be one in 300,000;" "that the inoculated cow-pox is not a milder or safer disease than the inoculated small-pox;" and that "the necessity of an immediate restoration of small-pox inoculation must strike every person interested in the welfare of society, and the happiness of his fellow-creatures. Cow-pox (averred Mr. Drew and Mr. Forrester) was a "*far more* severe disease than small-pox."[2]

"Out of many thousands, nay millions" (so wrote Dr. Rowley in 1805[3]), "it has been fully proved scarcely any one died from small-pox inoculation;" "it was safe, mild, and certain; therefore, cow-pox inoculation as a substitute was absolutely unnecessary;" besides, the substitute itself "stands condemned by the experience of veterans in the profession;" "disagreeable events (eruptions, abscesses, gangrenes, chronic diseases, etc.) have in great numbers followed, and can be traced to arise from cow-pox inoculation," while small-pox inoculation was free from them; and "fifty-nine have died, and a great number of victims become diseased for life," in consequence of vaccination; in fact "the senses," says Dr. Rowley, "are appalled, and the pen is tired of recording its dreadful disasters."—(P. 61.)[4]

But, in our own day, exactly the same line of argumentation

[1] *Treatise on the Lues Bovilla*, 2d edit. 1805, pp. xv. xx. xxii. 110, etc.

[2] See Lipscomb's Essay, p. 7, and Lipscomb's own similar opinion, p. 39.

[3] On *Cow-pox Inoculation*, pp. 4, 31, 100, 128, etc.

[4] See the works also of other anti-vaccinists for the same line of argument. Thus, observes Dr. Squirrell, the small-pox inoculation, "whatever impediment it might have met with at its introduction, owing, at that time, to a want of experience in the practitioners, is now a mild and harmless disease," and hence "there is no necessity to forsake its practice," for that of vaccination, with "all its difficulties, ambiguities, and malignant effects." "The cow-pox (he argues more at length) produces malignant effects, vitiates the blood, and other juices, and is tedious as well as difficult to cure; the small-pox inoculation produces no ill consequences whatever. The cow-pox produces very ill health in children; the small-pox inoculation improves the health and constitution, and carries off many complaints, which were very uncomfortable both to the parents and children. The cow-pox matter is taken from an animal diseased, and is of a specific scrofulous kind, as is proved by its effects; the small-pox matter is taken from a healthy subject, and produces no disease whatever, but the one for which it was intended." —*Observations on the Cow-pox*, pp. 24, 49, 55, etc.

that was thus zealously directed against the adoption of artificial variolation and vaccination, at *their* first introduction into practice, is now as zealously directed against the adoption of etherisation. For precisely in the same way some minds, averse to the employment of ether-inhalation, anxiously argue that there is not only no call for its employment in surgery, but that its employment, and the result which it produces—namely, immunity from the physical sufferings inflicted by the knife of the surgeon—is probably a direct and positive evil.

The common opinion of mankind and of the profession in regard to the pain attendant upon surgical operations, seems, till of late, to have been unanimous and unchallenged. The human agony and torture following the surgeon's knife have hitherto been borne with and submitted to, merely because, while they seemed absolutely necessary for the preservation of health and life, they were considered at the same time absolutely unavoidable. It is true that differences in the mental and physical constitutions of different men, enable them to encounter the surgical mangling and mutilation of their limbs and bodies, with different degrees of equanimity and hardihood. And under special morbid states of the nervous system —in the way both of great excitement and great overstraining and collapse—flesh and bone have sometimes been extensively cut and divided without the common accompanying feelings and common manifestations of acute suffering. But the every-day experience of mankind in every age shows how greatly and truly the reverse of this is usually the fact. And all past human testimony on this point goes fully to corroborate the truth of the sentiment which England's great epic poet puts into the mouth of the wounded Nisroch, on the first occasion in which wounds were ever inflicted, and living beings first "knew pain":—

> "But Pain is perfect misery, the worst
> Of evils, and excessive, overturns
> All patience.

Nor have we any proper test, either of the fortitude with which it will be borne, or of the amount of pain actually endured in individual cases. For whilst the degree of outward manifestation of suffering, shown by the patient on the rack of the operating table, affords no perfect evidence of his actual feelings, the greatest torture being incapable in some of contorting a muscle, or eliciting a groan, and the slightest scratch forcing screams and cries from others; neither, on the other hand, is the degree of equanimity and endur-

ance with which the agony of a surgical operation is submitted to, any true and direct criterion of the natural moral strength and mental courage of the patient. Sometimes the sailor and soldier, who would not feel one moment's dread in facing, under the most desperate circumstances, the sword and fire of the enemy, will quail before the very thought of having his living flesh deliberately cut and mutilated by the cold steel of the surgeon.[1] And thus, the mere fear and horror of encountering the pain and agony of a surgical operation, will sometimes, by unnerving the strongest mind and boldest heart, bring on, as a consequence, such a depressed state of the system, as certainly by no means places the patient in a state favourable for securing a happy and successful result. "An extraordinary excitement of mind," observes Mr. Travers, "such as is produced by dread, or by the screwing up of the system for the endurance of painful operations, when it is already much depressed and enervated by continued suffering, or apprehension of it, sometimes proves suddenly fatal."[2]

In the greater operations of surgery, the employment of etherisation is not only, thus far, a great blessing to the patient, but it is a great boon also to the practitioner. It is a benefit to the operator, as well as to those operated upon. For, whilst it relieves the one from all the feelings of agonising pain, it relieves the other from the feelings of inflicting such pain upon a fellow-creature. Humanity,

[1] The following illustrative anecdote refers to Lord D——, assuredly one of the bravest admirals in the English service, and a man of all others whom his country has long looked upon as the very personification of valour and courage. The injury alluded to was received in cutting off the Spanish frigate A——, one of the most daring feats attempted in modern warfare. I extract the account from an article on Etherisation in the *North British Review*, excellent in matter, in manner, and in feeling, and written by one or the most distinguished surgical practitioners and authors of the present day. "We remember," says the reviewer, "the case of a gallant admiral—one of the bravest hearts that ever beat, in a service where men of every grade are, to a proverb, dauntless—who, in the opening of his distinguished career, had been engaged in cutting out an enemy's frigate. From the gun-boat he climbed up the ship's steep side, and, foremost of his crew, had reached the bulwarks, when receiving a stunning blow, he fell backwards into his boat again, striking his back violently on the tholpin. Many years afterwards a tumour had grown on the injured part, and at length the admiral—grey, and bent in years, found it advisable that this growth should be removed. The man that never feared death in its most appalling form, while in the discharge of duty, now shrank from the surgeon's knife; the removal, contemplated with a feeling almost akin to fear, was long deferred; and at length, half stupified by opium though he was, a most unsteady patient did he prove during the operation."—*North British Review* for May 1847, p. 169.

[2] Travers on *Constitutional Irritation*, vol. i. p. 22.

in the fullest sense of the term, is the great object of the healing art, and the aim alike of the surgeon as of the physician. Hitherto the professional duty of the surgeon has compelled him to inflict present suffering upon his patients, with a prospective view to their own ultimate benefit and advantage. And surely there is no one, however much inured to the sight and shriek of pain, who would not rejoice to be able to obtain these advantages for his patients, without compelling them to pass through so severe an ordeal as the tortures of the operating table. If, as some few operators themselves have indiscreetly boasted, their hearts have become so hardened by education and custom as not to be affected by the sufferings of those submitted to their knives, it is still pleasing and refreshing to know that this change in the human feelings, and this artificial violation of one of the first laws in human nature—namely, sympathy with the sufferings of others—is by no means necessary to make a man a perfect surgeon.

Perhaps no surgeon ever operated, either more frequently or more successfully, than the celebrated Cheselden. In St. Thomas's Hospital alone, and exclusively of his practice elsewhere, he performed lithotomy upwards of two hundred times, and with results that have never yet been surpassed by any other operator. But he adds, in language bearing all the simple impressiveness of truth, "If I have any reputation in this way, I have earned it dearly; for no one ever endured more anxiety and sickness before an operation; yet, from the time I began to operate, all uneasiness ceased; and, if I have had better success than some others, I do not impute it to more knowledge, but to the happiness of a mind that was never ruffled or disconcerted, and a hand that never trembled during any operation."[1] It was under this great master that John Hunter received his first lessons in surgery; and the biographer of Hunter tells us that, to quote his words, "Cheselden's manners were exceedingly kind and gentle, and, notwithstanding the extensive practice he had enjoyed, he always, before an operation, *felt sick at the thoughts of the pain he was about to inflict;* though, during its performance, his coolness and presence of mind never forsook him. Such feelings," adds Mr. Ottley, "are in a less marked degree, perhaps, far more commonly experienced than is generally supposed, by the very best surgeons, previously to undertaking operations of importance."[2] And, no doubt, it is the desire to diminish the

[1] Cheselden's *Anatomy of the Human Body*, 5th edition, p. 333.

[2] Ottley's *Life of John Hunter*, in Mr. Palmer's edition of his works, vol. i. p. 9.

actual amount of pain endured by patients, by curtailing the actual duration of it, that has led many of our best surgeons, with praiseworthy earnestness, to attempt to invent new and more rapid modes for performing particular operations,—a field in which no small degree of success has consequently been met with.

A new era, however, arrives in chirurgical science, and a measure is, at last, brought to light, through the influence of which surgeons may perform operations, and patients submit to them, even when of a prolonged nature, without the necessity of pain. It is found that the excruciating tortures and writhings, and shrieks of patients on the operating table, may be saved; and yet the required operations be as well and perfectly executed as before. Scarcely, however, is this glad and glorious discovery announced and acted upon, than another new, and, if possible, still stranger discovery, is broached and anxiously promulgated; namely, that in cutting the living flesh of man, the surgeon's knife does not, after all, produce any very remarkable or very important amount of pain, and that immunity from this pain during operations would be, perhaps, an evil rather than a good to humanity—a calamity rather than a blessing.

At a meeting of the South London Medical Society, held in April last, Dr. Gull read a paper on the injurious effects of ether-inhalation, and ended his communication with queries as to the "*desirability* of removing pain," etc.[1] Mr. Bransby Cooper, surgeon to Guy's Hospital, afterwards affirmed it as his opinion, "that pain was a premonitory condition, no doubt fitting parts, the subject of lesion, to reparatory action, and, therefore, he (Mr. Cooper) should feel averse to the prevention of it."[2] "Pain," argues Mr. Nunn, surgeon to the Colchester and Essex Hospital, in some observations against ether-inhalation,—"pain (toothache?) is, doubtless, our great safeguard under ordinary circumstances; but for it we should be hourly falling into danger; and I am," he continues, "inclined to believe that pain should be considered as a healthy indication, and as an essential concomitant with surgical operations, and that it is amply compensated by the effects it produces on the system, as the natural incentive to reparative action."[3] Arguing in a similar but still more bitter strain against etherisation, Dr. Pickford affirms that "pain during operations is, in the majority of cases, even desirable; and its prevention or annihilation is, for the most part, hazardous

[1] See Report of the Meeting in the *London Medical Gazette* for April 30, 1847, p. 777.

[2] *Ibid.*

[3] *London Medical Gazette* for March 5, p. 415.

to the patient."[1] Upon one of the first communications being given in to the French Academy of Sciences upon etherisation, M. Magendie, the distinguished physiologist, maintained[2] that "pain has always its usefulness;" he doubted if there was a true advantage "in suppressing pain, by rendering patients insensible, during an operation;" and argued that "it was a trivial matter to suffer (*c'est peu de chose de souffrir*), and a discovery whose object was to prevent pain was of a slight (*mediocre*) interest only."

It would be as idle as it would be useless to confute by mere argument the preceding allegations, in regard to the supposed necessity and advantages of pain in surgical operations. For, in fact, the whole question amounts to this:—Mankind are perfectly agreed, that the cutting and mutilation of the living human body *is* painful, however loudly surgeons may preach to the contrary; and medical men have hitherto assented, without one contradictory voice, to the self-evident aphorism of Galen, that pain is useless to the pained (*dolor dolentibus inutilis est*). If we find then, as we do now, a few men entertaining and expressing opinions on these points so very different from the general ideas and general experience of mankind, these opinions can scarcely be looked upon as aught else than indications of a strange degree of eccentricity of thought upon one special subject. And if the same line of thought that they apply to pain were extended, as it should be if true in principle, to other diseases or effects of disease, the untenable and irrational character of it would lead to conclusions that might, perhaps, surprise and startle even those minds that at present employ it against etherisation. For if physical pain and agony be a blessing and benefit, and not an evil and a calamity to be eschewed, then all other morbid symptoms and affections should equally, upon the very same grounds, be included under the same category—their presence endured and courted, and their removal forbidden and avoided; diseases and death are parts of the great economy and general police of nature, and the labours of the physician and surgeon to counteract their agency, should be denounced and decried as unnecessary and improper.

Let us view the subject, however, calmly, and as a question in pathology. And that pain is, *per se*, and especially when in excess, a condition which is not only trying and difficult for any exertion

[1] On the Injurious Effects of the Inhalation of Ether, in the *Edinburgh Medical and Surgical Journal* for July 1847, p. 258.

[2] *Gazette Médicale de Paris*, 6th Feb. 1847, pp. 112, 113.

of human fortitude to bear, but at the same time directly injurious and damaging in its action upon the constitution, and sometimes very fatal in its effects, is a practical truth that many of our best surgeons and soundest pathologists have long amply acknowledged and attested, and that, too, without any prejudgment in regard to it, or, at all events, without any view to such an extraordinary proposition as the now alleged impolicy and impropriety of abolishing it. Speaking of the pains of wounds, the great father of French surgery, Ambrose Paré, pithily tells us, that it "ought to be assuaged; because," adds he, "nothing so much dejects the powers of the patient (*prosterne et abatte les vertus du malade*)."[1] "Mere pain," observed the late distinguished Dr. Gooch, "*can* destroy life."[2] "Pain," according to Mr. Travers, "when amounting to a certain degree of intensity and duration, is of itself destructive."[3] "Pain in excess," he again remarks, "exhausts the principle of life; so that either its continuance without intermission, or the superaddition of the slightest shock subsequent to its endurance for a certain period, is fatal. In operations protracted by unforeseen difficulties, as in cases of lithotomy, in which the stone is of such magnitude as to require crushing, the patient has begun to die upon the table. The same happens in parturition, etc., protracted by mechanical impediment, etc."[4] Again, "Bodily exhaustion from continued pain, spasm, and other causes, not unfrequently proves suddenly fatal. I have often observed a sudden deliquium at the conclusion of an operation for recent injury, arising evidently from the exhaustion of the nervous system in the effort to support it."[5] "Great sensibility or excessive pain attendant on an injury (or operation) has," remarks

[1] *Paré's Works*, Johnston's Translation, p. 329.

[2] Merriman's *Synopsis of Midwifery*, 1826, p. 239.

[3] An inquiry concerning that disturbed state of the Vital Functions, usually denominated *Constitutional Irritation*, 1826, p. 65.

[4] Travers on *Constitutional Irritation*, vol. i. 76. The statistical data published by Dr. Collins in his excellent Report of the Dublin Lying-in Hospital, proves how true Mr. Travers's remark holds as a general principle with regard to the effect of pain in protracted parturition. In fact, the maternal mortality attendant upon parturition regularly increases in a ratio progressive with the increased duration of the woman's sufferings. Thus, according to calculations which I have made from Dr. Collins's data, while in the women whose sufferings were terminated within two hours, only one in about 320 of the mothers died; where the labour varied in duration from two to six hours, one in 145 died; in those in whom it continued from seven to twelve hours, one in 80 died; when it endured from twelve to thirty-six hours, one in 23 died; and out of those whose sufferings were prolonged beyond thirty-six hours, one in every 6 perished.

[5] Travers, p. 24.

Professor Burns, "two effects. First, it exhausts both the system and the part: and, secondly, it acts as an exciting cause of inflammation, a disease apt enough to take place from the injury itself."[1] "We have many facts," observes Professor Alison, "to prove that various violent and overpowering sensations, intense pain, etc., when acting in the utmost intensity, affect the circulating system just as a concussion does, and sometimes with fatal effect—especially when they take place in a state of unusual weakness or exhaustion."[2] Dr. Ranking has lately published a striking instance of the fatally depressing effects of extreme pain. It occurred in a case in which a ligature was applied for the cure of an erectile tumour of the breast. "The patient," he relates, "a healthy female, bore the initiatory steps of the operation without a murmur, without failure of pulse, and without change of countenance. The instant the ligature was tightened, which it was with the full force of two surgeons, she gave a yell of agony, the pulse became imperceptible, the countenance became ghastly pale, and in eighteen hours she was a corpse."[3]

But it is, I believe, needless to accumulate surperfluous proof on a point on which the observations and feelings of the profession and of mankind are sufficiently agreed—namely, that bodily pain, particularly such excess of pain as, with all its concomitant fears and sickening horrors, accompanies the larger operations in surgery, is, with very few, if indeed any exceptions, morally and physically a mighty and unqualified evil. And, surely, any means by which its abolition could possibly be accomplished, with perfect security and safety, deserves to be joyfully and gratefully welcomed by medical science, as one of the most inestimable boons which man could confer upon his suffering fellow-mortals. Few now deny, and no doubt ere long fewer and fewer will venture to doubt, that with a generality as certain and constant, or indeed more so, than we see exemplified in the action of most other therapeutic agents upon the human constitution, etherisation *does* possess the desired and entire effect of abolishing and annulling the pain following the scalpel and saw of the surgeon. But, as I have already stated, a question of the most important moment remains behind. Many surgeons, who

[1] The *Principles of Surgery*, by John Burns, M.D., vol. i. p. 502.

[2] *Outlines of Pathology and Practice of Medicine*, p. 13. See also pp. 203 and 317, on peritonitis proving directly fatal, "by virtue, probably, of the intense and peculiar sensation (of pain) it excites, acting as a powerful sedative on the heart." On pain as a cause of exhaustion of nervous power, see also Dr. Holland's *Medical Notes and Reflections*, p. 618.

[3] *Abstract of the Medical Sciences*, by Dr. Ranking, vol. v. 1847, p. 383.

cannot gainsay the indisputable effects of ether-inhalation in cancelling the pains of surgical operations, still ardently maintain that this admitted amount of present good can only be purchased and obtained by the patient, at the hazard or certainty of a greater and disproportionate amount of future evil. In other words, they allege and maintain that the condition of etherisation is one which, from its marked powerful effects upon the economy, must produce, in some cases, immediately dangerous results, which must affect the system, so as to impede and interfere with the condition of wounds, and the recovery of patients from them, and hence that it will render operations more perilous in their consequences, and more fatal in their ultimate results.

The correctness or incorrectness of this last feasible and assuredly most formidable objection to etherisation in surgery, is a problem that no mere reasoning or mere opinion could ever certainly and satisfactorily solve. The evidence of simple prejudgment and argument could never perfectly settle it, however plausible and ingenious the grounds of the prejudgment and argument might be. It is one of those allegations, the accuracy or inaccuracy of which is a matter than can be fully and finally determined by one method only—namely, by an appeal to the evidence of facts, and to the evidence of facts alone. For the purpose of assisting in the decision of this question, I have, through the great kindness of my professional brethren, collected the results of above three hundred amputations of the thigh, leg, arm, and forearm, performed within the last six months upon patients in an etherised state, in the civil hospitals of England, Scotland, Ireland, and France. The statistical analysis of these three hundred amputations with ether, and the comparison of *their* results with the results of various similar collections of the same amputations without ether, in the same and in other similar hospitals, will, I believe, enable us to arrive at some more definite ideas and deductions than we are yet in possession of, in regard to the debated question of the danger or safety of etherisation in the operations of surgery.

CHAPTER V.

PAIN IN OPERATIONS FROM A PATIENT'S POINT OF VIEW.[1]

Edinburgh, 1855.

MY DEAR Dr. SIMPSON—I have recently read, with mingled sadness and surprise, the declarations of some surgeons that anæsthetics are needless luxuries, and that unendurable agony is the best of tonics. Those surgeons, I think, can scarcely have been patients of their brother surgeons, and jest at scars only because they never felt a wound; but if they remain enemies of anæsthetics after what you have written, I despair of convincing them of their utility. My present object in writing is not to supplement your arguments in favour of the administration of anæsthetics to those who are about to undergo surgical operations; but, as one who knows from personal experience what operations were to the patient before ether or chloroform was employed anæsthetically, I am anxious to state certain reasons in justification of their use, which only those who suffered without their help are in a condition to urge.

Several years ago, I was required to prepare, on very short warning, for the loss of a limb by amputation. A painful disease, which for a time had seemed likely to yield to the remedies employed, suddenly became greatly aggravated, and I was informed by two surgeons of the highest skill, who were consulted on my case, that I must choose between death and the sacrifice of a limb, and that my choice must be promptly made, for my strength was fast sinking under pain, sleeplessness, and exhaustion.

I at once agreed to submit to the operation, but asked a week to prepare for it, not with the slightest expectation that the disease would take a favourable turn in the interval, or that the anticipated horrors of the operation would become less appalling by reflection upon them, but simply because it was so probable that the operation would be followed by a fatal issue, that I wished to prepare for

[1] Written by an esteemed professional colleague who holds a distinguished place in British science and literature, and who, before the days of anæsthetics, was himself the subject of a severe surgical operation.—J. Y. S.

death and what lies beyond it, whilst my faculties were clear and my emotions were comparatively undisturbed, for I knew well that if the operation were speedily followed by death, I should be in a condition, during the interval, in the last degree unfavourable to making preparation for the great change.

The week, so slow, and yet so swift in its passage, at length came to an end, and the morning of the operation arrived. There were no anæsthetics in those days, and I took no preparative stimulant or anodyne of any kind, unless two cups of tea, which with a fragment of toast formed my breakfast, be considered such.

The operation was a more tedious one than some which involve much greater mutilation. It necessitated cruel cutting through inflamed and morbidly sensitive parts, and could not be despatched by a few swift strokes of the knife. I do not suppose that it was more painful than the majority of severe surgical operations are, but I am not, I believe, mistaken in thinking that it was not less painful, and this is all that I wish to contend for.

Of the agony it occasioned, I will say nothing. Suffering so great as I underwent cannot be expressed in words, and thus fortunately cannot be recalled. The particular pangs are now forgotten; but the black whirlwind of emotion, the horror of great darkness, and the sense of desertion by God and man, bordering close upon despair, which swept through my mind and overwhelmed my heart, I can never forget, however gladly I would do so. Only the wish to save others some of my sufferings, makes me deliberately recall and confess the anguish and humiliation of such a personal experience; nor can I find language more sober or familiar than that I have used to express feelings which, happily for us all, are too rare as matters of general experience to have been shaped into household words.

From all this anguish I should of course have been saved had I been rendered insensible by ether or chloroform, or otherwise, before submitting to the operation. On that point, however, I do not dwell, because it needs no proof, and the testimony of the thousands who have been spared such experiences by the employment of chloroform, is at hand to satisfy all who are *not* determined not to be satisfied.

But there are other modes in which anæsthetics may serve a patient than by rendering him insensible at the period of his undergoing a surgical operation, and it is to these modes of service, which may not strike even the most humane and thoughtful surgeon,

and cannot be matters of experience except to patients who have not taken anæsthetics, that I seek mainly to refer in this letter.

I am not gifted with physical courage. Physical courage I understand to signify that consciousness of a power to endure bodily agony, which accompanies a certain temperament. Its possessors know from the first instinctively, and by and by learn from experience, that a blow, a cut, a burn, an attack of toothache, or the like infliction of injury, or onset of pain, can be endured by them, though unwelcome, up to an extent of considerable severity, without excessively incommoding them or exhausting their patience. From severe injuries and dangerous diseases such persons recover, fortified by the assurance that they can bear without flinching what would make others complain loudly, and they are not afraid to anticipate suffering, believing that they will be able to bear it. This estimable virtue is possessed more largely by men than by women, and by savage than by civilised men, and may or may not be accompanied by moral courage.

I belong, on the other hand, to that large class, including most women, to whom cutting, bruising, burning, or any similar physical injury, even to a small extent, is a source of suffering never willingly endured, and always anticipated with more or less of apprehension. Pain in itself has nothing tonic or bracing in its effects upon such. In its relation to the body, it is a sheer and unmitigated evil, and every fresh attack of suffering only furnishes a fresh proof of the sensitiveness possessed to pain, and increases the apprehension with which its attacks are awaited.

When I, accordingly, made up my mind to submit to the operation proposed to me, it was with the fullest conviction that the pain it would occasion would far exceed my power of patient tolerance, and I prepared for it, simply as for a dreadful necessity from which there was no escape. I awoke each morning from troubled sleep to reconsider the whole reasons for and against submitting to the surgeons, and by a painful effort reached again the determination not to draw back from my first resolution. From all this distracting mental struggle, which reacted very injuriously on my bodily constitution, I should have been exempted, had I been able to look forward to the administration of chloroform. A far greater amount of internal composure and serenity would then have been mine, and this mental peacefulness would have been a powerful aid towards sustaining my strength, and fitting me to bear the shock of the operation.

Again, I concealed from the relatives who were about my sickbed what awaited me, knowing that an announcement of the impending operation would occasion them the greatest grief, and fearing that the expression of that grief would utterly shake my resolution. On the very morning of the operation, I performed my toilet with peculiar pains and care, with a view to disarm their apprehensions on hearing that the surgeons were to pay me a visit that day; and I had at least the satisfaction of afterwards learning that the *ruse* was successful. But I need scarcely say that the mental tension occasioned by this reserve, and the continued effort to play a part, was a prejudicial exertion, and kept my faculties injuriously on the strain. Could I have told my friends that the operation would be painless, we should have conferred about it, and they and I would have been saved much distress.

Further; during the operation, in spite of the pain it occasioned, my senses were preternaturally acute, as I have been told they generally are in patients in such circumstances. I watched all that the surgeons did with a fascinated intensity. I still recall with unwelcome vividness the spreading out of the instruments; the twisting of the tourniquet; the first incision; the fingering of the sawed bone; the sponge pressed on the flap; the tying of the blood vessels; the stitching of the skin; and the bloody dismembered limb lying on the floor.

Those are not pleasant remembrances. For a long time they haunted me, and even now they are easily resuscitated; and though they cannot bring back the suffering attending the events which gave them a place in my memory, they can occasion a suffering of their own, and be the cause of a disquiet which favours neither mental nor bodily health. From memories of this kind, those subjects of operations who receive chloroform are of course free; and could I, even now, by some Lethean draught erase the remembrances I speak of, I would drink it, for they are easily brought back, and they are never welcome.

How far my experiences agree with those of others who have undergone similar operations I do not know, but except that I may have a more active and roving fancy or imagination than some of my fellow-sufferers, I cannot doubt that my experiences are not singular.

That the dread of pain keeps many a patient from submitting to operations which would save life, is notorious; but the dread of a particular mode of inflicting pain is a more dissuasive motive with

many than the dread of the pain so inflicted. Hundreds every day endure the great torture of toothache, rather than the small torture of the extraction of the tooth. Women, in particular, suffer prolonged agonies for months, rather than submit to a fraction of the same amount of pain at a surgeon's hand, because, as produced by him, it takes the form of an incision with a sharp knife; and a red-hot iron is held in such horror by most persons, that, rather than be touched by it, though the pain it occasions is but momentary, they will endure the application of chemical caustics which occasion torture for hours.

Anæsthetics render all such persons as great a service by rendering them insensible to the accompaniments of an operation, as by rendering them insensible to its pain. It is true that if they felt no pain, they might be as calm and even curious spectators of the dismembering of themselves as in dreams all men are, of what in waking life would be the most agonising realities. But it is not less true, that sufferings equal to those of the severest operations are experienced by patients, in the course of acute or aggravated maladies, without being followed by the crushing effect of the operations which they rival in power to occasion agony; and surely this is not to be wondered at. Before the days of anæsthetics, a patient preparing for an operation was like a condemned criminal preparing for execution. He counted the days till the appointed day came. He counted the hours of that day till the appointed hour came. He listened for the echo on the street of the surgeon's carriage. He watched for his pull at the door-bell; for his foot on the stair; for his step in the room; for the production of his dreaded instruments; for his few grave words, and his last preparations before beginning. And then he surrendered his liberty, and revolting at the necessity, submitted to be held or bound, and helplessly gave himself up to the cruel knife. The excitement, disquiet, and exhaustion thus occasioned, could not but greatly aggravate the evil effects of the operation, which fell upon a physical frame predisposed to magnify, not to repel, its severity. To make a patient incognisant of the surgeon's proceedings, and unable to recall the details of an operation, is assuredly to save him from much present and much future self-torture, and to give to him thereby a much greater likelihood of recovery.

Further; the horror with which attached relatives regard the prospect of operations on those very dear to them—a horror far surpassing that with which they would, in many cases, hear of such

operations awaiting themselves—leads them often to dissuade their friends from submitting to surgical interference. The issue in too many cases is, that the poor patient listens, though but half-convinced, to their arguments; tries doctor after doctor, and remedy after remedy, only to be compelled in the end, after weeks or months of prolonged suffering, to submit to the operation. The prospects of recovery, however, in such cases, are too often immensely lessened by the physical exhaustion and enfeebled general health which have resulted from the delay. The knowledge, on the other hand, that a mother, a sister, a wife, or a child, will be carried unconsciously through a severe operation, cannot but rob it of half its horrors in the eyes of friends, and will make them often the allies rather than the opponents of the surgeon, and keep them from showing the false kindness to their relatives, of dissuading them from submitting to the only treatment which promises a cure.

The sum you will perceive of what I have been urging, is, that the *unconsciousness* of the patient secured by anæsthetics is scarcely less important than the *painlessness* with which they permit injuries to be inflicted on him. To steep his senses in forgetfulness, and throw the whole intellectual machine out of action, when, if allowed to work, it only moves with a rapidity and irregularity which threaten its integrity, and permanently injure it, is to do him a service, second only to that of saving him from suffering. And to make it impossible for him to recall a scene of horror, and torture himself by going over and over all its incidents again and again, is also to do him a signal service. Nor need more be said concerning the service done to his friends.

I plead therefore for the administration of anæsthetics on the grounds enumerated. I fear you may think my confessions exaggerated, but I can most honestly declare that they are not. When I first heard that anæsthetics had been discovered, I could not and would not believe it. I have since thanked God many a time, that He has put it into your heart, and into that of other wise and humane men, to devise so simple and so safe a way of lessening pain.

As for the fear entertained by some, that the moral good which accrues from suffering, and is intended by the Ruler of all to be secured by it, will be lost if agony is evaded by sufferers having recourse to anæsthetics, we may surely leave that to the disposal of Him who does all things well. The best answer to such complaints

I have heard, was that given by an excellent old lady to another, who was doubting whether any of the daughters of Eve were at liberty to lessen by anæsthetics the pangs of child-bearing: "You need not be afraid," said the wiser lady, "that there will not be enough of suffering in the world."

I think not; but may you be honoured still further to reduce its sum.—Yours most truly,

AN OLD PATIENT.

CHAPTER VI.

DOES ANÆSTHESIA INCREASE OR DECREASE THE MORTALITY ATTENDANT UPON SURGICAL OPERATIONS?

"Why dost thou whet thy knife so earnestly?
. . . . . Shylock must be merciful.
On what compulsion *must* I? Tell me that."
SHAKESPEARE'S *Merchant of Venice.*

Edinburgh, April 1848.

SHORTLY after anæsthesia began to be employed in surgery, its alleged beneficial or baneful effects were keenly discussed among the members of the profession; and principally, or entirely, upon the results of individual or isolated cases. Some eagerly and stoutly doubted, *in toto*, the possibility of making operations painless; and many who admitted its possibility, denied altogether its propriety, on the alleged ground of its increasing the general subsequent dangers of the patient, inducing a variety of alleged morbid states and lesions, and adding, on the whole, to the fatality of operative surgery.

Amidst the many conflicting and contradictory assertions that were uttered on these points, I became convinced that there was only one method of arriving at the truth—viz. by instituting a statistical investigation, upon as large a scale as possible, into the results of the practice, and thus ascertaining whether, out of an extensive series of operations performed with and without anæsthesia, the mortality was greater or was less when the patients were operated on in a narcotised and anæsthetic state, than when they were operated on in a waking and æsthetic state.

The first difficulty to be encountered in such an inquiry was the difficulty of obtaining a proper field and standard for the proposed comparison. But *first* of all, it was evident that the comparison, whatever it might be, could only be properly instituted between patients operated on in public hospitals, with and without anæsthesia. For we had nowhere published, nor did it seem possible to obtain, any adequate comparative returns of the results of operations

from the surgical practice of private practitioners. Besides, hospital returns were preferable in this respect, that there existed on the whole, everywhere, undoubtedly a far greater uniformity between the hygienic and other collateral circumstances of patients operated on in hospital than in private practice. *Secondly*, however, it was further evident, that in seeking and fixing upon a criterion by which we could compare the statistical results of surgical operations formerly performed without anæsthesia, with those now performed upon anæsthetised patients, it was improper and impossible to institute the comparison between *all* operations and reports of operations in hospitals; for the severity and danger of the operations performed in, and reported from, different hospitals, differed immensely in their nature, and consequently in their results. In order, therefore, to obtain the primary requisite for a correct statistical inquiry—of having data of a *similar* kind and character for the proposed testing and comparison—it was necessary to select and contrast the result of some one operation without ether, with the results of the same one operation with ether. With this view I selected the larger amputations of the limbs as the fittest field on which to conduct the proposed investigation; and I restricted myself to hospital amputations of the thigh, leg, arm, and forearm, on account of their being everywhere performed in almost the same manner, for the same causes, under the same circumstances, and on the same class of subjects; and because there already existed extensive published researches, by Phillips, Lawrie, and Malgaigne, into their absolute mortality, when performed under ordinary circumstances and without anæsthesia, to aid us in satisfactorily determining the nature of the results of the new practice of operating upon patients in an anæsthetic state.

Having thus fixed upon the mode of inquiry, I proceeded to apply for returns from all the surgical hospitals of Great Britain and Ireland that I could hear of, as likely to have employed anæsthesia in amputations. And I feel it quite impossible to return thanks, in any adequate terms, for the very great politeness and kindness with which my inquiries were answered on all hands.[1] In some hospitals

[1] In my letter of application, I stated, that "the effects, whether favourable or unfavourable, of anæsthesia upon the ultimate recoveries of patients from surgical operations is still a matter of much doubt and uncertainty. We have as yet had no proper collection of data to ascertain whether the mortality of operations has been increased or not by patients being placed under the influence of ether at the period of their performance. In order to determine as far as possible this important point, I have been induced to undertake the statistical investiga-

anæsthesia had not been tried, and I was consequently furnished with no data; in others in which it was used, my correspondents were quite at issue about its propriety; many were doubtful; some expressed themselves strongly against it, and others strongly for it. But I was principally anxious to obtain the total results, believing that *they* would decide the question far more certainly than any individual experience or individual opinion could. In Table No. I. (see pp. 96 and 97), these results are given in a detailed form, with the names of the gentlemen who kindy reported each return to me.[1]

tion of the results of the larger amputations in cases where anæsthesia was employed at the time of operation. Amputations have been selected for this purpose in preference to other operations, because they are, under all common circumstances, nearly and everywhere alike, and because the general average mortality accompanying most of the greater amputations is already known, from the inquiries of Phillips, Lawrie, and others, and thus a ready standard of comparison is afforded us. You would, therefore, oblige me by filling up the following table with any results, however few in number, of amputations in which ether was used in your hospital. I especially wish to know *all* the deaths as well as all the recoveries in these operations; and by thus collating, on the whole, a large body of statistical data, I hope to be able to arrive at the same general results."

COPY OF FORM OF TABLE SENT.—"RESULTS OF AMPUTATIONS PERFORMED UPON PATIENTS IN AN ETHERISED STATE IN THE ——— HOSPITAL."

Seat of Amputation.	Primary or for Injury.		Secondary or for Disease.	
	Total No. of Cases.	Total No. of Deaths.	Total No. of Cases.	Total No. of Deaths.
Amputation of Thigh . .				
Amputation of Leg . . .				
Amputation of Arm . .				
Amputation of Forearm .				
Total . . .				

It may be proper to remark that, in answer to the returns, I had the results of twenty-four amputations of the forearm sent me, which are not included in the subsequent remarks in the text. Out of these twenty-four amputations ten were primary, with one death, and fourteen secondary, with two deaths. I have omitted them in the text, in consequence of finding that Mr. Phillips, in his standard of amputations, confines his returns to those of the thigh, leg, and arm, and does not include those of the forearm.

[1] In No. 49 of the Table, the name of the hospital is not mentioned, as my correspondent unfortunately omitted to date his return. The Paris hospital returns of twenty-two cases (No. 40) are distributed according to the standard of Malgaigne; Dr. Burguières, in a note to me, having stated that he was unable to give the exact number of these amputations which were respectively primary and secondary.

No. I.—Table showing, in detail, the number of Individual 302 Patients

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.			
			Primary.		Secondary.	
			Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary	Dr. Keith and Dr. Macintosh	...	...	2	1
2	Bedford General Infirmary	Mr. Hurst	...	...	3	0
3	Birmingham General Infirmary	Mr. Amphlett	...	...	1	0
4	Birmingham Queen's Hospital	Dr. Wright	...	...	1	0
5	Bristol, General Hospital	Dr. Lansdowne and Mr. Mason	...	...	3	0
6	Bristol Infirmary	Mr. Morgan	...	...	1	1
7	Belfast Hospital	Mr. Moore	1	1	1	1
8	Cumberland Infirmary	Mr. Page	...	...	2	1
9	Chester Infirmary	Mr. Harrison	...	...	...	...
10	Cork South Infirmary	Dr. Tanner	...	...	3	0
11	Dundee Infirmary	Dr. Monro	...	...	...	...
12	Dumfries Infirmary	Mr. Borthwick	1	1	...	...
13	Derbyshire General Infirmary	Mr. Fox	...	...	1	0
14	Devon and Exeter Hospital	Mr. James	...	...	2	0
15	Dublin Mercer Hospital	Dr. Jamieson	...	...	1	1
16	Dublin Richmond Surg. Hosp.	Dr. Hamilton	...	...	3	1
17	Edinburgh Infirmary	Mr. Miller	1	1	2	0
18	Edinburgh Infirmary	Dr. Duncan	2	2	4	1
19	Elgin Infirmary	Dr. Paul	...	...	...	...
20	Glasgow Hospital	Dr. Orr and Dr. Steel	...	...	8	4
21	Hereford Infirmary	Mr. Wandby	...	...	3	0
22	Hull General Infirmary	Mr. Craven	1	0	1	1
23	Hants, Royal South Infirmary	Dr. Bullar	...	...	...	...
24	Ipswich Hospital	Dr. Durrant	...	...	3	0
25	Leeds Infirmary	Mr. Hey	...	...	...	...
26	London Hospital	Mr. Curling	1	0	4	0
27	Lond., St. Bartholomew's Hos.	Mr. Haig	2	1	6	3
28	London, Univ. College Hosp.	Mr. Liston and Mr. Cadge	2	1	5	0
29	London, King's College Hosp.	Mr. Fergusson	...	..	1	0
30	London, St. George's Hospital	Dr. Snow	...	...	12	2
31	London, Middlesex Hospital	Mr. Shaw	...	...	5	0
32	London, Charing Cross Hosp.	Mr. Avery	1	0	2	1
33	London, Westminster Hospital	Dr. Bird	...	...	4	0
34	Leicester Infirmary	Mr. Paget	2	0	...	...
35	Liverpool Northern Hospital	Dr. Bainbrigge	...	...	3	2
36	Liverpool Southern Hospital	Mr. Morris	...	...	1	0
37	Manchester Royal Infirmary	Dr. Reid	...	..	2	1
38	Newcastle Infirmary	Mr. Greenhow	...	...	...	...
39	Nottingham General Hospital	Mr. Wright	...	...	3	0
40	Paris Hospitals	Dr. Burguières	3	2	7	2
41	Perth Infirmary	Dr. M'Farlane	4	2	1	0
42	Sussex County Hospital	Mr. Parson	1	0	3	0
43	Staffordshire, North Infirmary	Mr. Turner	...	...	2	0
44	Sheffield General Infirmary	Mr. Jackson	...	...	3	0
45	Salisbury Infirmary	Mr. Young	...	...	4	2
46	Stockport Infirmary	Mr. Rayner	...	...	...	...
47	Winchester County Hospital	Mr. Wickham	...	...	3	0
48	Worcester Infirmary	Mr. Sheppard	1	1	4	0
49	... ...	Mr. Stocker	1	0	1	0
	Total		24	12	121	25

AMPUTATIONS AND THEIR RESULTS, IN DIFFERENT HOSPITALS, UPON UNDER ETHERISATION.

AMPUTATION OF LEG.				AMPUTATION OF ARM.				TOTAL CASES.			
Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.	
Cases.	Deaths.	Cases	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
...	...	5	0	...	...	...	...	...	...	7	1
...	...	...	...	...	...	1	0	...	...	4	0
2	0	1	0	...	...	2	0	2	0	4	0
1	0	3	0	...	...	...	...	1	0	4	0
...	...	3	0	...	...	...	...	...	...	6	0
...	...	...	...	...	...	...	...	...	...	1	1
...	...	1	0	...	...	1	0	1	1	3	1
...	...	2	0	1	0	...	...	1	0	4	1
...	...	1	0	...	...	...	...	...	...	1	0
1	0	2	1	1	0	...	...	2	0	5	1
...	...	1	0	1	0	...	...	1	0	1	0
...	...	...	...	...	...	...	...	1	1	...	...
1	0	...	...	...	...	...	...	1	0	1	0
...	...	1	0	...	...	...	...	...	...	3	0
...	...	...	...	...	...	...	...	...	...	1	1
...	...	...	...	...	...	1	1	...	...	4	2
...	...	1	1	...	...	...	...	1	1	3	1
1	0	3	0	...	...	...	...	3	2	7	1
...	...	...	...	...	...	1	0	...	...	1	0
5	1	5	2	2	0	4	3	7	1	17	9
...	...	...	...	...	...	...	...	...	...	3	0
3	1	2	0	1	0	1	0	5	1	4	1
...	...	2	0	1	0	1	1	1	0	3	1
...	...	...	...	...	...	...	...	...	...	3	0
1	0	1	0	...	...	...	...	1	0	1	0
3	2	1	0	...	...	...	...	4	2	5	0
2	1	4	1	2	1	1	0	6	3	11	4
...	...	5	1	...	...	1	1	2	1	11	2
...	...	1	0	...	...	...	...	...	...	2	0
...	...	7	0	...	...	1	0	...	...	20	3
1	0	1	1	...	...	1	0	1	0	7	1
...	...	...	...	...	...	1	0	1	0	3	1
...	...	1	0	...	...	1	0	...	...	6	0
...	...	1	0	...	...	...	...	2	0	1	0
...	...	4	3	...	...	2	1	...	...	9	6
1	0	...	...	1	0	1	0	2	0	2	0
...	...	2	1	...	...	...	...	...	...	4	2
2	2	...	...	1	0	...	...	3	2	...	...
1	0	1	0	1	1	...	...	2	1	4	0
3	1	5	2	2	1	2	1	8	4	14	5
...	...	...	...	...	...	...	...	4	2	1	0
3	1	1	0	1	0	...	...	5	1	4	0
...	...	2	0	...	...	...	...	...	...	4	0
...	...	2	0	...	...	2	0	...	...	7	0
...	...	3	0	...	...	...	...	...	...	7	2
...	...	2	0	...	...	...	...	...	...	2	0
...	...	...	...	...	...	2	0	0	...	5	0
...	...	4	0	2	1	...	...	3	2	8	0
1	0	...	...	...	...	...	...	2	0	1	0
32	9	81	13	17	4	27	8	73	25	229	47

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM, WITHOUT ANÆSTHESIA.

Before attempting to determine whether the results in these anæsthetic amputations (Table No. I.) are, or are not favourable to the adoption of Anæsthesia in Surgery, let me in the first place state the results of the previous investigations that have been published by Phillips, Lawrie, and Malgaigne, relating to the mortality of these same amputations, when the same operations were performed without anæsthesia. In the year 1837, Mr. Benjamin Phillips brought before the Royal Medico-Chirurgical Society of London a communication [1] on the results of amputation of the thigh, leg, and arm, in different countries. From the collection of cases which he laid before the society, Mr. Phillips concluded that the general mortality of these larger amputations amounted to 23 deaths in the 100 operations. The correctness, however, of his conclusions was called in question by the publishing committee of the society, on the idea that the alleged mortality was too great, and he was recommended to investigate the subject more fully before proceeding to publish his observations. Farther inquiry served only to satisfy him that his previous results were understated rather than overstated.

Subsequently, in 1844, Mr. Phillips published a table of a still more extensive series of cases.[2] This collection, however, includes the results of private as well as of hospital practice. "They are," says Mr. Phillips, "the whole, so far as I know, of the cases of amputation recorded in the periodical literature of this and other countries during the present century. I by no means," Mr. Phillips adds, "think that the results furnished by such data will fairly represent the mortality. I believe it will be *understated*, because successful cases are more likely than unsuccessful ones to find their way into print."

The Table (No. II.) extracted from Mr. Phillips's second paper, shows in a summary way the results which he obtained from these sources.

[1] Observations on the Results of Amputation in different Countries. *Medical Gazette*, vol. xxii. 1837-38, p. 457.

[2] *Medical Gazette*, vol. xxxiii. 1843-44, p. 804.

No. II.—Table of the Mortality of 1369 Cases of Amputation of the Thigh, Leg, and Arm.

Seat of Amputation.	Primary.			Secondary.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh . . .	245	176	72	415	87	21
Leg . . .	204	88	43	231	61	27
Arm . . .	164	49	29	110	26	24
Total . .	613	313	51	756	174	23

In the year 1840, Dr. Lawrie of Glasgow published an excellent paper [1] on the results of amputations, with tables showing the rate of mortality from amputation in the Glasgow Hospital, from the period of its foundation in 1794 down to 1839. Dr. Lawrie's inquiries yielded an average mortality greater than that of Mr. Phillips, being as high as 36 per cent. The following table, made from data in Mr. Lawrie's paper, contains the results of amputation of the thigh, leg, and arm, in the Glasgow Hospital:—

No. III.—Table of the Mortality of 242 Amputations of the Thigh, Leg, and Arm, in the Glasgow Hospital, from 1794 to 1839.

Seat of Amputation.	Primary.			Secondary.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh . . .	35	27	77	92	19	20
Leg . . .	27	18	66	35	12	34
Arm . . .	36	18	50	17	3	17
Total . .	98	63	64	144	34	23

In 1842 a valuable series of papers on the statistics of amputations was published by Professor Malgaigne in the *Archives Générales de Médecine*, his data being derived from the reports of the Parisian hospitals. In these papers, Malgaigne enters largely upon the subject of the mortality of amputations. The following table, compiled from data in his returns,[2] exhibits a mortality still higher than that of the Glasgow Hospital.

[1] On the Results of Amputations. *Medical Gazette*, vol. xxvii. 1841, p. 394.

[2] *Archives Générales de Médecine*, vol. lviii. 1842, p. 40.

No. IV.—Table of the Mortality of 484 Amputations of the Thigh, Leg, and Arm, in the Parisian Hospitals, from 1836 to 1841.

Seat of Amputation.	Primary.			Secondary.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh . . .	48	34	70	153	92	60
Leg . . .	80	51	63	112	55	49
Arm . . .	30	17	56	61	24	39
Total . .	158	102	64	326	171	52

These three tables of large collections of cases by Phillips, Lawrie, and Malgaigne, may be properly considered as giving a correct idea of the general mortality of these amputations in hospital practice, and may be used with justice as subjects of comparison with any series of cases similar to them in the whole series of circumstances, except that one whose influence upon the results is to be decided. After, however, I began to collect the results and mortality of the same amputations upon patients in an anæsthetised state from various British and other hospitals, it was objected to the inquiry that it would be unsatisfactory in two respects—viz., that the amputations compared were possibly performed in different classes of hospitals, and at dates so different that I did not consider in my investigation the changes and improvements which might possibly have been introduced into the very methods of operating.

In order, then, at once to enlarge the basis of data for comparison, and to obtain a series of cases still more exactly similar to the collection of anæsthetic amputations which I was making, I procured from various British hospitals, through the kindness of different correspondents, and from published data, returns of the latest amputations that had been performed in them immediately previous to the introduction of anæsthesia. These returns are given in detail on page 101. All of the operations have been performed within the eight years, from 1839 to 1846 inclusive. By having this collection of cases as an additional standard, I hoped to avoid all cavil on the ground of any supposed difference in the time, and other collateral circumstances, in which the compared operations were performed.

No. V.—Table showing, in detail, the results of 618 Amputations, in 30 different British Hospitals, immediately before the introduction of Anæsthesia.

No.	Name of Hospital.	Date of the Observations.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
				Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary	1841-1846	Dr. Macintosh	5	3	7	2	5	2	15	4	5	0	5	0	15	5	27	6
2	Bristol, St. Peter's Hospital	1844-1846	Dr. Rogers	..	..	3	0	..	..	..	..	..	..	..	..	..	..	3	0
3	Bristol Infirmary	1845-1846	Mr. Morgan	1	0	17	2	2	0	12	1	3	0	1	0	6	0	30	3
4	Bristol General Hospital	1846	Mr. Mason	..	..	2	1	..	..	..	..	..	..	..	..	..	..	2	1
5	Berks Royal Hospital	1840-1845	Mr May	7	5	5	0	3	0	..	..	3	0	1	0	13	5	6	0
6	Cork Northern Infirmary	1845-1846	Mr. Bullen	1	0	2	1	..	..	3	0	..	..	..	..	1	0	5	1
7	Cumberland Infirmary	1845-1846	Mr. Page	..	..	4	1	..	..	2	0	2	0	..	..	2	0	6	1
8	Dumfries Infirmary	1842-1846	Mr. Borthwick	..	..	6	1	..	..	2	0	..	..	1	1	..	..	9	2
9	Derbyshire General Infirmary	1845-1846	Mr Fox	..	..	5	2	..	..	6	0	..	..	..	..	..	..	11	2
10	Dundee Infirmary	1844-1846	Dr. Monro	1	0	2	0	1	1	4	0	3	0	..	..	5	1	6	0
11	Edinburgh Royal Infirmary	1840-1845	Dr. Peacock	8	8	35	13	4	3	12	2	5	4	2	1	17	15	49	16
12	Elgin Infirmary	1844-1846	Dr. Paul	..	..	8	2	1	1	1	0	..	..	..	..	1	1	9	2
13	Glasgow Hospital	1840-1844	Dr. King	11	10	14	6	14	5	6	1	7	4	..	..	32	19	20	7
14	Glasgow Hospital	1844-1846	Dr. Orr	10	3	22	8	17	5	13	4	12	3	11	5	39	11	46	17
15	Hull General Infirmary	1846	Mr. Craven	..	..	2	0	3	0	..	..	2	0	..	..	5	0	2	0
16	Inverness Infirmary	1845-1846	Dr. Manford	1	1	..	..	..	..	..	..	1	0	..	..	2	1	..	..
17	Ipswich Infirmary	1845-1846	Dr. Durrant	..	..	6	0	1	0	..	..	3	0	..	..	4	0	6	0
18	London St. Bartholomew's Hos.	1846	Mr. Haig	2	1	7	2	3	0	4	1	3	0	3	0	8	1	14	3
19	London University College Hos.	1841-1846	Mr. Cadge	5	3	16	7	1	1	15	2	1	0	7	1	7	4	38	10
20	Leicester Infirmary	1845-1846	Mr. Paget	2	0	3	1	1	0	4	0	3	0	2	0	6	0	9	1
21	Liverpool Northern Hospital	1845-1846	Dr. Bainbrigge	5	3	8	4	4	1	6	2	2	0	..	..	11	4	14	6
22	Liverpool Southern Hospital	1846	Mr. Morris	1	1	..	..	2	1	..	..	..	..	1	0	3	2	1	0
23	Newcastle Infirmary	1840-1846	Mr. Benson	7	3	10	2	8	2	10	3	10	2	2	0	25	7	22	5
24	Perth Hospital	1847	Dr. M'Farlane	..	.	13	5	..	..	7	2	..	..	2	1	..	..	22	8
25	Sussex County Hospital	1844-1846	Mr. Parson	2	2	2	2	2	1	3	0	3	1	1	0	7	4	6	2
26	Salisbury Infirmary	1845-1846	Mr. Young	2	0	3	0	3	2	3	1	1	0	1	0	6	2	7	1
27	Stockport Infirmary	1845-1846	Mr. Rayner	2	2	2	0	..	..	1	0	2	0	..	..	4	2	3	0
28	Sheffield Infirmary	1845-1846	Mr. Jackson	..	..	1	0	3	0	..	..	..	..	..	..	3	0	1	0
29	Worcester Infirmary	1846	Mr. Sheppard	..	..	5	0	1	0	3	0	4	2	1	0	5	2	9	0
30	York County Hospital	1845-1846	Mr. Hey	..	..	1	0	1	1	3	0	2	1	1	1	3	2	5	1
	Total			73	45	211	62	80	26	135	23	77	17	42	10	230	88	388	95

The data in the preceding Table, No. V. (p. 101), when condensed into the tabular form, afford the results in the following Table, No. VI.—

No. VI.—Table of the Mortality of 618 Amputations of the Thigh, Leg, and Arm, without Anæsthesia, performed during the last few years in 30 British Hospitals.

Seat of Amputation.	Primary.			Secondary.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh . . .	73	45	63	211	62	29
Leg . . .	80	26	32	135	23	17
Arm . . .	77	17	22	42	10	24
Total . .	230	88	38	388	95	24

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM, UPON PATIENTS IN AN ANÆSTHETIC STATE.

In the preceding lengthened Table, No. I. (pp. 96, 97), I have given from forty-nine different hospitals the detailed reports of 302 amputations of the thigh, leg, and arm. When these 302 amputations are reduced into a tabular form, similar to those which I have used for stating the data of similar amputations without anæsthesia, they present the following results:—

No. VII.—Table of the Mortality of 302 Amputations of the Thigh, Leg, and Arm, under Anæsthesia.

Seat of Amputation.	Primary.			Secondary.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh . . .	24	12	50	121	25	20
Leg . . .	32	9	28	81	13	16
Arm . . .	17	4	23	27	8	29
Total . .	73	25	34	229	46	20

I shall now proceed to contrast these results with the results of the same operations in the same class of hospitals, and when performed upon patients not in an anæsthetic state.

Before doing so, however, let me observe in passing, that the data I have adduced in Tables Nos. I. and V. (pp. 96-97, 101) have

been objected to on the ground that they are collected from too many different hospitals, and too many different sources. But, on the contrary, I believe all our highest statistical authorities will hold that this very circumstance renders them more, instead of less, trustworthy. Professor Chomel of Paris, after pointing out the first requisite for a successful statistical comparison of therapeutic or other results—viz. a sufficient similarity between the number of collated cases—adds, as the second condition, "that the data be numerous, collected at different times, in different places, and, if possible, by several observers. It is easily seen," he adds, "that the results of a number of facts too limited, collected in a short space of time, in a single place, and by a single observer, however exact as regards that individual series of data, may yet be very different from, or even the reverse of, conclusions drawn from a larger series, and one collected under various circumstances." [1]

COMPARISON OF THE MORTALITY FOLLOWING THE LARGER AMPUTATIONS OF THE LIMBS — 1. WITHOUT, AND 2. WITH ANÆSTHESIA.

The major amputations of the limbs, including those of the thigh, leg, and arm, are generally fatal in hospital practice in the proportion of about 1 in every 2 or 3 operated upon. In the Parisian hospitals, the fatality, according to Malgaigne, amounts to upwards of 1 in 2. In Glasgow, it is $2\frac{1}{2}$. In British hospitals, I found that under these amputations 1 in $3\frac{1}{2}$ died. The same operations, performed in the same hospitals, and upon the same class of patients, in an anæsthetic state, present a mortality of 23 in 100, or 1 in 4 only. The following Table shows the amount of the individual cases, and the percentage of deaths in different collections, with the corresponding proportion of deaths in those operated on in an anæsthetic state.

No. VIII.—Table of the Mortality of Amputation of the Thigh, Leg, and Arm.

Reporter	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Parisian Hospitals—Malgaigne .	484	273	57 in 100
Glasgow Hospital—Lawrie . .	242	97	40 in 100
General Collection—Phillips . .	1369	487	35 in 100
British Hospitals—Simpson . .	618	183	29 in 100
Upon Patients in an Etherised State	302	71	23 in 100

[1] *Bulletin de l'Acad. Roy. de Médecine.* Séance du Mai 2, 1837.

The evidence which the preceding table affords in favour of the greater safety of amputation with anæsthesia than without it, is sufficiently strong and striking. While 23 in 100 died under the amputations named, when the operations were performed upon patients in an anæsthetic state, 29 in every 100 died under the same amputations in the same hospitals when the patients were not anæsthetised ;—in the Glasgow hospital as many as 40 in 100 died ; and in Paris, as many as 57 per cent. In other words, out of every 100 persons submitted to amputations of the thigh, leg, or arm, the lives of six were, by the employment of anæsthesia, saved above the average number of the same operations in British hospitals ;—17 lives in each 100 were saved, if we take the Glasgow returns as a standard of comparison ; the average mortality was, under anæsthesia, less by 34 in every 100 cases than that which was found by Malgaigne to accompany the same operations in the Parisian hospitals.

But probably, to most minds, this comparison would be rendered more clear and simple, if we took not a class of operations, but a single operation as a standard and medium of comparison. For this purpose let us select amputation of the thigh as the *individual* operation regarding which we possess the largest series of observations.[1]

COMPARISON OF THE MORTALITY FOLLOWING AMPUTATIONS OF THE THIGH—1. WITHOUT, AND 2. WITH ANÆSTHESIA.

There are few or none of the operations deemed justifiable in surgery, that are more fearfully fatal in their results than amputation of the thigh. "The stern evidence," says Mr. Syme, "of hospi-

[1] One objection may be urged against the comparison of the results of a single operation, with or without anæsthesia, that I am now about to institute, on the ground, viz. that the number of cases (145) is too limited to afford a result that is perfectly decisive. I am perfectly willing to admit the justness of this remark in a statistical point of view, and to hold this part (and indeed the whole of the present inquiry) as, so far, the commencement and nucleus merely of a more full and lengthened investigation by other hands. At the same time I have, during the course of the inquiry, had the conviction impressed upon me, that future results will more and more confirm those that I have here stated in the text, and be still more in favour of etherisation ; for no small number of the operations reported to me were, in the first periods of the new practice, doubtlessly performed upon patients in whom the anæsthesia was by no means entire and complete, in consequence of imperfection in the forms of apparatus, in their management, in the dose given, etc. ; and I believe that, as the profession becomes more accomplished and certain in the use of such measures, the resulting effects will become proportionally happier and more favourable.

tal statistics shows, that the average frequency of death is not less than from 60 to 70 per cent,"[1] or above one in every two operated on die. Out of 987 cases of amputation of the thigh collated by Mr. Phillips, 435 proved fatal, or 44 in every 100 were lost.[2] "On referring," observes Mr. Curling, "to a table of amputations in the hospitals of London, performed from 1837 to 1843, collected with care by a private society to which I have the honour of belonging (the Medical Society of Observations), I find 134 cases of amputation of the thigh and leg, of which 55 were fatal, giving a mortality of 41 per cent."[3] Out of 201 amputations of the thigh performed in the Parisian hospitals, and reported by Malgaigne, 126 ended fatally. In the Edinburgh Infirmary 21 died out of 43. Dr. Lawrie found the mortality attendant upon this operation in the Glasgow hospital to amount to 46 deaths in 127 cases. In the collection of cases from thirty different British hospitals, which I have published in table No. V. (p. 101), 284 cases of amputation of the thigh are reported; 107 out of these 284 operations proved fatal. On the contrary, I have collated 145 cases in which the same operation has been performed during the past year in British hospitals, upon patients in an anæsthetic state. Out of these 145 cases of amputation of the thigh, only 37 proved fatal. Or, in other words, the fatality was not greater than one in every four operated on when the patients were previously anæsthetised. It was as high as one in every two or three operated upon when the patients were not previously anæsthetised. The following table presents these results in a more clear form :—

No. IX.—Table of the Mortality of Amputation of the Thigh.

Name of Reporter.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Parisian Hospitals—Malgaigne . .	201	126	62 in 100
Edinburgh Hospital—Peacock . .	43	21	49 in 100
General Collection—Phillips . . .	987	435	44 in 100
Glasgow Hospital—Lawrie . . .	127	46	36 in 100
British Hospitals—Simpson . . .	284	107	38 in 100
Upon Patients in an Anæsthetised State	145	37	25 in 100

The preceding figures speak in a language much more emphatic

[1] *Monthly Journal* for May 1845, p. 337.
[2] *Medical Gazette* for 1844, p. 805.
[3] Address to the Hunterian Society of London, 1848, p. 31.

than any mere words that I could employ in favour of anæsthesia, not only as a means of preserving surgical patients from pain, but as a means also of preserving them from death. Between even the lowest mortality in the table without anæsthesia, 36 in 100, and the rate of mortality with it, 25 in 100, there is the difference of 11 per cent. That is to say, according to this standard, out of every 100 patients submitted to amputation of the thigh without anæsthesia, 11 more would die from the operation than if the same 100 patients were submitted to the same operation in a state of anæsthesia. And if the condition of anæsthesia effects thus a saving of 11 lives in every 100 amputations of the thigh, then out of every 1000 such operations the lives of 110 patients would be preserved by the use of antipathic means.

If we compare these results with the standard of Mr. Phillips, the contrast is still more startling. Out of 987 amputations of the thigh collected by him, 435 proved fatal, or 44 in the 100. Out of 145 amputations of the thigh under anæsthesia, 37 proved fatal, or 25 in 100. According to this comparison, the number of persons saved from death in amputation of the thigh by the patients being rendered anæsthetic during the operation, amounts to 19 lives in every 100 operations performed.

In conclusion, led me add, that when anæsthesia first began to be employed in surgical operations, it was eagerly argued that its adoption produced a greater tendency to primary and secondary hemorrhage, to imperfect union of the wounds, to pneumonia, etc. If my space had permitted, it was my intention to show, from the analyses of the three hundred cases of amputation reported to me, that these various allegations were foundationless and imaginary[1]—that such consequences were not so frequent after amputations with anæsthesia as after amputations previously performed without it—that as the casualties were reduced in number, so were also the attendant accidents and complications.[2] But I believe such proof to be at the present day superfluous, as few or none now maintain such opinions. When writing to me as early as in June last on this subject, the late lamented Mr. Liston stated what all the subsequent experience of our ablest surgeons here and in London has

[1] Some of my correspondents, who expressed the strongest opinions in regard to the reality of these supposed evil consequences, have, I know, now abandoned such opinions as utterly untenable.

[2] In my communication to the Medico-Chirurgical Society in June last, I went over this ground at some length.

confirmed. "The ether," says he, "produces no bad effect, as far as I can see. There is no change in the blood, nor in the vessels or muscles. The recoveries are, at least, quite as good as before it was employed." An excellent surgical pathologist (Mr. Curling, surgeon to the London Hospital)[1] has more recently afforded still stronger testimony to the same effect. "I have carefully watched," says he, "the progress of cases, after operations of various kinds performed upon patients in a state of anæsthesia, and I can with confidence declare that, so far as my present experience has reached, the constitutional symptoms have been milder, and the cases have proceeded more satisfactorily, than after operations in which no means had been taken to prevent pain. Several of my surgical friends can fully confirm this statement."

I have also avoided entering into the theoretical question—How does anæsthesia render severe operations less fatal and dangerous in their consequences? I have already shown[2] that the endurance of severe pain is in itself depressing and destructive; and apparently the anæsthetic state saves the patient from this suffering and its effects, as well as saves him, in some degree, from the shock of the operation and its consequences. When writing, in 1839, on the subject of pain and shock, and on certain states connected with or produced by wounds or injuries, Professor Burns of Glasgow offered some remarks bearing directly on the present subject, and which are more valuable as they were written without any theory, and without any prospect of such a state as he speaks of being capable of being artificially induced. I shall quote them in his own words:—"The mere lopping off of the member by the abridgment of the quantity of living body, the instant loss of so large a portion which was formerly acting along with the system, is productive of serious evil to it from the sympathy which universally prevails. But *if the nervous system becomes in part torpid, so as to prevent this sympathy*, or to be incapable of maintaining it, the loss of a member, or what is, in one respect, the same, the loss of its connection with the system, and its failure in power and action and sensibility, may not have the same bad effect."[3]

[1] Address to the Hunterian Society of London, 1848, p. 23.

[2] See *ante*, p. 83, etc.

[3] *Principles of Surgery*, vol. i. p. 493.

CHAPTER VII.

ANSWER TO OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.

Edinburgh, October 1848.

OBJECTIONS of various kinds, religious, moral, and medical, have been zealously brought against the practice of anæsthesia in midwifery.

I have already attempted[1] to answer the supposed religious objections that were at first so very strongly urged in various quarters against the practice, on the supposed ground of the permanence of the primeval curse; and I have shown that the disputed word "sorrow," *'etzebh* ("in sorrow thou shalt bring forth children"), does not in the original Hebrew really signify the sensations of pain, but the muscular efforts and contractions connected with childbirth. Besides, if this were not the fact, and it was the duty of man to give effect to the curse, instead of struggling to ameliorate and resist its penalties and influences, then the whole art of physic would require to be abandoned entirely, for, in the primeval curse, man was doomed to die; and yet is not the great leading aim and object of the physician a continuous attempt to preserve him in life? All forms of obstetric assistance would require also to be rejected, for the whole art and science of midwifery is one undivided effort to abate and ameliorate the effects of the curse; and to attain that object imperfectly, as heretofore, by venesection, baths, by counter-pressure to the back, and other minor practices, is as sinful as to attain it more perfectly now by anæsthetics, inasmuch as the principle of interference is not altered by the degree of relief given; "for whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." In short, if there is any evidence of feelings of impiety and irreligion in the whole question, it is surely on the side of those persons who suppose that pain is permanently ordained in the primal curse as an accompaniment of human parturition; and

[1] See *ante*, Part II. Chaps. I. and II.; see also a pamphlet by Dr. Protheroe Smith, entitled, *Scriptural Authority for the Mitigation of the Pains of Labour.*

yet that by anæsthetics, man, the creature, has discovered a power by which he can alter and subvert an immutable decree of God, the Creator.

The principal moral "objection," as it has been termed, against the employment of anæsthesia in midwifery, amounts to the often-repeated allegation, that it is "unnatural." "Parturition," it is avowed, is a "natural function," the pain attendant upon it is a "physiological pain"—(Dr. Meigs);[1] and it is argued that it is impossible "to intermeddle with a natural function;" and to use anæsthetics is a piece of "unnecessary interference with the providentially arranged process of healthy labour"—(Dr. Ashwell[2]). The above is, perhaps, the most general and approved of all the objections entertained and urged at this moment against the practice of anæsthesia in midwifery. But it certainly is a very untenable objection; for, if it were urged against any of our similar interferences with the other physiological functions of the body, every one of which is as "providentially arranged" as the function of parturition, then the present state of society would require to be altogether changed and revolutionised. For the fact is, that almost all the habits and practices of civilised life are as "unnatural," and as direct interferences with our various "providentially arranged" functions, as the exhibition of anæsthetics during labour. Progression upon our own two lower extremities is a "providentially arranged" function, a "natural process;" and yet we "unnaturally" supplement and assist it by constantly riding on horseback and in carriages, etc. The "physiological process" of walking is apt to produce pain and injury of the uncovered foot of man, and we "unnaturally" use boots and shoes to bind the foot, and add to the protecting power of the cutaneous and other structures of the sole. Mastication and digestion are "natural processes;" but we daily intermeddle with and attempt to aid them by the arts of cookery and dietetics; and so on with regard to other functions.

To annul the pain of labour by anæsthetics, is, argues Dr. Meigs, "a questionable attempt to abrogate one of the general conditions of man." Riding and railway travelling abrogate one of the general conditions of man (progression), and are constantly leading to accidents and deaths. Should we never travel therefore except on foot? Disease and death itself form one of the most "general

[1] *Philadelphia Medical Examiner*, March 1848, p. 152.
[2] *Lancet* for March 11, 1848, p. 291.

conditions of man,"—and medicine is a "questionable attempt to abrogate them." Should medicine therefore be abandoned?

In a note now lying before me, an eminent London divine urges the following objections against anæsthesia, either in midwifery or surgery; and I notice it here, because it is an objection which I have often heard repeated. He writes:—"The question with me is not the alleviation of pain, but the destruction of consciousness. I should hesitate greatly to take a step which destroys consciousness." Now, certainly, our consciousness is destroyed in natural sleep as much as in the anæsthetic sleep. I have little doubt that the distinguished writer whom I have quoted, has, many a day, perhaps during almost every day for a long lifetime, voluntarily given up and destroyed his own consciousness in sleep, for an hour or two longer each morning than the necessities of his system required. Putting these many hours together, he has, perhaps now, from first to last, unnecessarily, but voluntarily, surrendered up his mental consciousness for periods, that, if added together, would count up weeks, and months, and perhaps years. He has done so too, merely for the reprehensible indulgence of indolence; and yet he insists upon his fellow-creatures not surrendering up *their* consciousness for a short time, on rare and extraordinary occasions, when the object is the far more legitimate one of the avoidance of unnecessary physical pains, and the securing life and health by saving the system from the endurance of these pains. If we may sleep, and thus indulge in the destruction of consciousness to avoid and cure corporeal fatigue, surely we may do the same to avoid and cure corporeal agony.

Dr. Merriman[1] opposes the employment of anæsthesia in natural labour, on the ground of "the great superiority of allowing nature to conduct the whole process of the birth." But the practice of anæsthesia does, in reality, allow "nature to conduct the *whole* process of the birth;" it merely abstracts that intensity of pain and suffering which accompanies the act of labour in the civilised woman —a "disadvantage inseparable" from civilisation, to employ Dr. Merriman's own expression, and which is not an essential part of the process of parturition, according to his own doctrine; for, as he himself states, "in the earliest ages of the world, and in savage nations at present, childbirth appears to have been, in almost every instance, *easily* accomplished; the mother suffers *little*." And in this state of natural anæsthesia, the convalescence of the mother is

[1] *Arguments against the Indiscriminate Employment of Anæsthetic Agents in Midwifery.* London, 1848.

consequently unusually rapid; for, again to quote Dr. Merriman's words, she almost "at once resumes her ordinary occupations." Dr. Merriman afterwards, in speaking of the use of chloroform, decries its propriety in any except "instrumental or very tedious labours,"—arguing that we should not interfere unless where the labour is morbid, for (to use his own words), "the duty of the physician is to *imitate* nature as far as possible, and watch her methods of acting." But surely the physician strictly imitates nature in her most natural state, according to Dr. Merriman's own premises, when, during labour, he induces by art that state of anæsthesia, which, in Dr. Merriman's opinion, *originally* pertains to parturition in the human mother. The female in an uncivilised state more truly shows us the true method and types of nature, than the female in a civilised state. Besides, are we not called upon to relieve the woman, when we can, of her sufferings, as an act of common professional duty and common professional humanity? In law and in morals, we judge greatly of actions by their intent. No accoucheur would intentionally inflict upon a patient the agonies of labour by a deliberate act of *commission* on his part. Is an accoucheur properly justified in intentionally refusing to save a patient from the agonies of labour, by a deliberate act of *omission* on his part? When a child, at birth, is intentionally destroyed by the loss of blood, it does not matter, in the eye of the law, whether the death has been produced by voluntarily *omitting* to tie the umbilical vessels, or voluntarily *opening* other vessels.

Up to within the last few months, and till the power of annulling pain by the inhalation of ether was discovered, severe bodily pain, such as we witness in surgery and midwifery, was universally regarded by the profession as possessing an evil and morbid effect. Some of the opponents of anæsthesia have taken up a novel and different view; and, as a medical argument against the practice of anæsthesia in midwifery, it has been particularly averred that a labour pain is a "desirable, salutary, and conservative manifestation of life force"—(Dr. Meigs). Parturient "pain is," says Dr. Copland,[1] "often salutary as respects its effects; neither its violence nor its continuance is productive of injury to the constitution," etc. No opinion, I believe, could be more erroneous. I have already shown, from the evidence of extensive statistical returns, that some of the graver operations of surgery are now much less fatal in their results when patients are operated on under the condition of anæsthesia,

[1] *Dictionary of Practical Medicine*, vol. iii. p. 484.

and consequently without any attendant pain, than the same operations were formerly, when patients were submitted to all the agonies of the surgeon's knife in their usual waking and sensitive state. The prevention of the pain in surgical operations is, in other words, one means of preventing danger and death to those operated on; the saving of human suffering implies the saving of human life. And what holds good in relation to pain in surgery, holds good in relation to midwifery.[1] Pain, whenever it is great in degree or great in

[1] Custom and prejudice, and, perhaps, the idea of its inevitable necessity, make both the profession and our patients look upon the amount and intensity of pain encountered in common cases of natural labour, as far less worthy of consideration than in reality it is. Viewed apart, and in an isolated light, the degree of actual pain usually endured during common labour is as great, if not greater, than that attendant upon most surgical operations. I allude particularly to the excessive pain and anguish, which in nine out of ten cases accompany the passage of the child's head through the outlet of the pelvis and external parts. Speaking of common or natural labour in its last stages, Dr. Merriman observes, the pulse gradually "increases in quickness and force; the skin grows hot; the face becomes intensely red; drops of sweat stand upon the forehead; and a perspiration, sometimes profuse, breaks out all over the body; frequently violent tremblings accompany the last pain, and at the moment that the head passes into the world, the extremity of suffering seems to be beyond endurance."—(*Synopsis of Parturition*, p. 15.) Or, take the picture of the suffering of the mother in the last stage of natural labour, as portrayed by the most faithful of living observers—Professor Naegele of Heidelberg.—"The pains," he observes, "of this stage are still more severe, painful, and enduring; return after a short interval, and take a far greater effect upon the patient, than those of the previous stage. Their severity increases so much the more from the additional suffering arising from the continually increasing distension of the external parts. They convulse the whole frame, and have hence been called the *dolores conquassantes*. The bearing down becomes more continued, and there is not unfrequently vomiting. The patient quivers and trembles all over. Her face is flushed, and, with the rest of the body, is bathed in perspiration. Her looks are staring and wild; her features alter so much that they can scarcely be recognised. Her impatience rises to its maximum with loud crying and wailing, and frequently expressions which, even with sensible, high-principled women, border close upon insanity. Everything denotes the violent manner in which both body and mind are affected."—(*Lehrbuch der Geburtshülfe*, p. 104. See *Brit. and For. Medical Review*, vol. xix. p. 64.)

I have stated that the question which I have been repeatedly asked is this—Shall we ever be "justified" in using the vapour of ether to assuage the pains of natural labour? Now, if experience betimes goes fully to prove to us the safety with which anæsthesia may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured as shown in the descriptions of Merriman, Naegele, and others—Dr. Rigby, in his *System of Midwifery*, p. 103, observes, "This is the moment of greatest pain, and the patient is quite wild and frantic with suffering; it approaches to a species of insanity," etc. etc.—I believe that the question will require to be quite changed in its character. For, instead of determining in relation to it whether we shall be "justified" in using

duration, is in itself deleterious; and by shielding our patients, by anæsthetic measures, against the more severe portion of the pains of parturition, we not only preserve them from the agony of their more immediate sufferings, but we preserve their constitutions also from the effects and consequences of these sufferings. And the evidence which I have adduced in the preceding pages tends to prove, that when thus freed from the endurance of pain by artificial anæsthesia, they assuredly, as a general rule, make both more rapid and more perfect recoveries than when such means are not used; just as woman in a savage state, and where she enjoys a kind of natural anæsthesia during labour, recovers more easily and rapidly from the shock of labour than the civilised female. In short, in cancelling the pains of parturition by anæsthesia, we also, I believe, to a great extent, cancel the perils of the process; for the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital, when under Dr. Collins's able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, 22 died; *or* 1 *in every* 320. In 452 of his cases, the labour was prolonged above twenty hours; and of these 452, 42 died; *or* 1 *in every* 11—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

The last, and certainly the principal objection against anæsthesia in midwifery, is the supposed danger accompanying the exhibition of anæsthetic agents. In the earliest paper which I published on the subject of chloroform, I pointed out this circumstance in mentioning various cautions in the use of it. When we consider the immense extent to which it has already been employed in all quarters of the world, in medicine, surgery, and midwifery—the little care

this agent under the circumstances named, it will become, on the other hand, necessary to determine whether on any grounds, moral or medical, a professional man could deem himself "justified" in withholding, and *not* using any such safe means, as we at present presuppose this to be, provided he had the power by it of assuaging the pangs and anguish of the last stage of natural labour, and thus counteracting what Velpeau describes as "those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable" (*Traité des Accouchemens*, vol. i. p. 449), which accompany the termination of natural parturition in the human mother.

sometimes observed in its use—and the deleterious and dangerous articles with which it is sometimes mixed, the wonder is that so few alleged accidents have happened from its employment. By saving a vast amount of human suffering, it has already, I believe, been the means of saving no small amount of human life; and it is assuredly improper to argue, as some have done, that the mere chance of its disagreeing with some rare and special constitution, now and then, is any valid reason for refusing its use for the abatement and abrogation of human suffering. If there were any soundness in the reasoning, a thousand things beside would require to be abandoned. Railways, steamboats, stage-coaches, etc., when used as substitutes for the natural and physiological function of human progression, are ever and anon attended with accidents to limb and life. But surely no one would, from this, maintain that these means of conveyance should, in consequence, be abandoned. Many persons are annually drowned in bathing—Should bathing, therefore, be prohibited, and this powerful means of maintaining and restoring health be entirely forsaken? According to the Registrar's official returns, a great number of lives[1] are lost yearly in England by the improper medical use of opium—Should the use of opium, therefore, be given up? Patients sometimes sink under the depressing action of antimony, calomel, etc.—Should these valuable drugs, therefore, be banished from the Pharmacopœia? Many a patient has perished in consequence of venesection—Should this operation be expunged from the art of surgery? From mistakes and errors, etc., in diagnosis and practice, medicine and surgery are sometimes the unhappy means of destroying instead of saving life—Should these arts, consequently, be interdicted? Works on medical subjects have sometimes led both patients and practitioners into serious and fatal errors—Should no medical works, therefore, be allowed to be printed? Long ago Raynalde, in sending forth the FIRST work on midwifery ever published in the English language, seems to have foreseen that, against the utility of publishing any book or books on midwifery, the same argument would be used as we have found in our own day used against the

[1] In 1840, out of every 1,000,000 living in England and Wales, 24 were poisoned by opium, and 22 by other medicines improperly given to children below the age of five years alone.—See *Seventh Annual Report*, p. 82. See Taylor on *Poisons*, p. 187, etc., for the great numbers destroyed in England by opium, etc., improperly given.

application of anæsthesia to midwifery; and he has answered the argument in a style so earnest and apposite, that I shall quote this reply, by anticipation as it were, in his own words, and in an abridged form :—

"Loe!—such is the lyght judgement of them, the which in every thyng, wherof may ensue both good and evyll, have alwayes theyr eyes wakyng and firmely affixed and directed uppon the evyll, pyckyng and choosyng out the worst of every matter, omittyng and leavyng to speake of the best, as the thyng whiche were nothyng to theyr purpose. If every thyng in this worlde shoulde be wayed and passed uppon after this sort, then shoulde we be fayne to condempne and banishe those thynges farre from us, whiche are at this tyme accompted and taken for the most necessarie, worthy, and of greatest price or estimation. For to be short, there is nothyng under Heaven so good, but that it may be perverted and turned to an evyll use, by them that be evyll and naught themselves, and so abuse it; ne is there any thyng so absolute and perfecte, but by the occasion of the abuse therof, at one tyme or other, may and doth ensue great daunger and damage to mankynde. Fyre and water be two right necessary elementtes to the use of man, without the whiche we coulde not lyve: yet by the meanes of them many a miserable deede hath been done and perpetrated. By fyre hath ben consumed and devoured whole Cities and Countries. By water, swalowed and drowned infinite men, shippes, yea, and whole regions. Agayne," he continues, "meate and drynke, to the moderate users therof, doth minister and maynteyne lyfe: And contrary, to the unmeasurable and unsaciate gourmauntes and gluttons, it hath full many thousand times brought surfeetes, sickenesse, and at the laste, death." * * * * But he argues, "shoulde men, for the avoydyng al these foresayde inconveniences, and for the reasons abovesayde, condempne and bannysh fyre and water, forsake theyr meate and drynke? * * * * No, it were but madnesse once to thinke it. Therefore I say, the judgment of that eye can never be eqall and indifferent, whiche hath more respecte and regarde alwayes to the displeasures and hurtes possible to happen (only through the misuse of a thyng) than to the emolumentes and profites dayly and commonlye lyke to ensue to the well users of the same. That that of it selfe is good, is never to be disalowed for the sake of them that do abuse it. For * * * * to them that be good them selfe, every thyng turneth to good, whatever it be, is

to them a sufficient matter and occasion therein to seeke the glory of God, and the onely profyte of theyr even Christen. And contrary, suche as be of an yll disposition, in every thyng (be it never so good and salutarie), picketh out matter of maynteynaunce to theyr lewdnesse, turning matters of sadnesse and discretion to foolyshe and pyvyshe pratyng contention."[1]

[1] Prologue to Raynalde's *Birth of Mankinde*, Edition of 1565, p. 9.

CHAPTER VIII.

ANSWER TO THE OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY, ADDUCED BY PROFESSOR MEIGS OF PHILADELPHIA.

Edinburgh, August 1848.

MY DEAR SIR—A few days ago, I saw your excellent epistle to me on the use of anæsthesia in midwifery, extracted, in an abridged form, from the *Philadelphia Medical Examiner* of March last, into the *London Medical Gazette* and *Lancet*. It reminded me, that amid other avocations and work, I had hitherto indolently omitted to answer the objections contained in your able and kind letter. And I feel that I am the more to blame for this neglect—on one account—namely, that as in your own country, so also in ours, there are few or no living obstetricians, whose opinions and name carry, and deservedly carry, more weight with them than yours. Be so good, then, as bear with me now for a few minutes, while I endeavour to state in what respects I am inclined to demur to your arguments against anæsthetic midwifery.

On reperusing, as I have just done, your esteemed letter, it appears to me that in it you ground your opposition to the adoption of anæsthesia in midwifery upon four or five different arguments, although you do not specialise them. I shall notice each of these arguments separately. You have not placed them in any particular order. I shall begin first with the one which you placed last.

1. *You object to anæsthesia in deliveries requiring "chirurgical intervention," and especially in forceps operations, on the ground that the sensations of the patient afford us our best aid for the introduction of the instrument.*

In order to introduce the forceps with the greatest safety to the mother, you state that—to quote your own words—"the best guide of the accoucheur is the reply of the patient to his interrogatory, 'Does it hurt you?' The patient's reply, 'Yes' or 'No,' are," you observe, "worth a thousand dogmas and precepts. I cannot, there-

fore," you continue, "deem myself justified in casting away my safest and most trustworthy diagnosis, for the questionable equivalent of ten minutes' exemption from pain, which, even in this case, is a physiological pain."

In answer to this novel objection, you will excuse me when I say, for I say it most conscientiously, that I think every man who ventures to use the forceps, in any midwifery case, ought to know the anatomy of the parts implicated, a thousand-fold better than you here presuppose. You would have the accoucheur guide his instrument, not so much by his own anatomical knowledge, as by the feelings and sensations of his patient. In this, as in other points, relative to any novel question in practice, we can often, it appears to me, best perceive the soundness or unsoundness of our views upon it, by considering and contrasting them with our established views on other analogous questions, regarding which the opinions of the profession have been long ago fixed and determined. Now, what would the surgical world, at this time of day, think of an operator, who, in making a ligature of a large artery, such as the humeral, placed his chance of discriminating the attendant nerve from the blood-vessel which he wished to tie, by appealing, not to his own anatomical knowledge, but to the feelings of his patient, as he touched the suspected structures. "Does it hurt you?—Yes, or No." Would not our surgical brethren denounce and decry the capabilities of any man who, in operating, required to have recourse to such imperfect and incompetent means for his anatomical direction and diagnosis? Would it be right and moral in a surgeon to deny to his patients the advantages of anæsthesia, in order that their sensations and sufferings should make up for his want of anatomical and operative knowledge?

But in saying this, do not, I pray you, for one moment suppose that I fancy that the argument which you adduce betrays any want whatever of the highest degree of operative skill on your part. Nothing could be farther from my thoughts. And, to confess the truth, I do sincerely believe that you yourself, while using the forceps, do not require to have recourse to any such rude rule as you here propound—and that, in fact, the rule itself, and the objection to anæsthesia in operative midwifery which it contains, is an *afterthought* on your part, which has only sprung up since the practice of anæsthesia was proposed. For, in looking over the excellent precepts which you have given, relative to the use of the forceps, in the valuable work on Midwifery which you published a few years

ago—viz. the *Philadelphia Practice of Midwifery*—I find no trace, or mention whatever of such a rule as you have quoted above in your letter to me. If that rule really formed, as you now state, the "safest and most trustworthy" guide in the operation, you would certainly have at least noticed it, or alluded to it in some way. In the precepts which you laid down in your work, you would assuredly not have forgot that one rule which, you say, is worth a "thousand other dogmas and precepts." And it would, I think, have been only the more incumbent upon you to have mentioned it, seeing that all other authors omit the notice of it.

I feel assured that when you come to re-consider, "dispassionately," your opinions regarding the non-employment of anæsthesia in operative midwifery, you will alter these opinions. And when you come to employ anæsthesia in actual practice, in cases in which the forceps are used, you will find that, instead of impeding the application of instruments, the anæsthetic state very greatly facilitates it. It enables you to guide the forceps far more safely to their destination, because it enables you, without any pain to the patient, to introduce your fingers for this purpose far more deeply between the head and maternal structures than you could do if the patient were awake, and in her usual sensitive state. You yourself state, in your published work on Midwifery, that care should be "taken to direct the point [of the forceps] by the two fingers, as far as they can reach" (p. 300). "If," you again observe, "any difficulty occurs in getting the second blade forward enough, the two left fingers that are guiding it will serve to guide it edgeways into the proper position." Now, the state of anæsthesia, I repeat, gives you (as I have several times found) the power of fulfilling these and other most important rules, to an extent that never can be attained without it; and I am sure you will find them worth any "thousand dogmas and precepts" derivable from the mere sensations of the patient.

Besides, these sensations, or rather the expression of them, would constantly betray you if you *did* place any dependence upon them. Under the same amount of pain, scarcely any two women would give you exactly the same expression of suffering. What one woman would loudly complain of, another would declare to be nought.

Before interfering instrumentally with the forceps, the labour has generally been allowed to endure for twenty or thirty long hours. After a poor patient has undergone such a protracted ordeal of pain

and suffering, her mind is not, I fear, in general in a very fit state to guide the operator by her sensations or directions.

At page 302 of your published work on midwifery, you state that when the forceps are used, the patient's mind is naturally wound up to a state of great anxiety; "it is strained," you observe, "to the highest tension, by the mere thought that she is under the operation." Now, putting entirely out of view, for the moment, the propriety of our saving our patients the increased corporeal agony attendant upon instrumental delivery, is it not, let me ask, our right and our duty, as medical men, to save her, as we can do, from this trying state of mental anxiety at the time of operating? In most cases, she will have been suffering and struggling on for many hours previously. Why, then, thus needlessly and greatly intensify both her mental anxieties and physical sufferings at the time of our instrumental interference, when her strength, alike of mind and body, is perhaps little calculated to bear any increase of suffering; and, above all, when the resources of our art furnish us with simple and certain means of saving her from the unnecessary endurance of the one state and of the other?

But, in instrumental delivery, besides greatly facilitating the application of the forceps, and relieving the patient from enduring the pains of the operation, and that "highest tension" of mind which is present during it, the state of anæsthesia saves her, I believe also, in a great measure, from the effects of the shock of the operation, and thus gives her a better chance of recovery. If we omit it, we omit, I believe, not only a means of saving her from the sufferings attendant upon the operation, but a means of saving her from some of the dangers attendant upon it. When first publishing on the subject of anæsthesia in midwifery, in February 1847, I offered one or two observations on this point, which subsequent surgical statistics have amply fulfilled. In allusion to some cases of operative delivery, which I recorded, I observed:—"The cases I have detailed sufficiently show its value and safety in cases of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtless be. But, in modifying and obliterating the state of conscious pain, the nervous shock otherwise liable to be produced by such pain—particularly whenever it is extreme, and intensely waited for and endured—is saved to the constitution, and thus an escape gained from many evil consequences that are apt to follow in its train."

The observations which I have hitherto made refer entirely to your opinion of anæsthesia in instrumental delivery. But—

2. *You object to anæsthesia in natural labours, because you hold that the pain of natural labour should not be annulled, and that it is calculated to promote the safety of the mother.*

You regard, you say, "the pain of a natural labour as a state not by all possible means and always to be eschewed and obviated," —"a labour pain being," you declare, "a most desirable, salutary, and conservative manifestation of life-force."

In the above expressions you make no distinction between the two separate and distinct elements of which a so-called labour pain consists—viz., 1. The contractions of the uterus, and 2. The sensations of pain resulting from these contractions. If you apply the language I have quoted to the first of these elements, the uterine contractions (which contractions are not annulled by anæsthetics), I decidedly and entirely agree with you. If you apply it, however, to the sensations of pain produced by the uterine contractions (which sensations are annulled by anæsthetics), I most decidedly and entirely dissent from your opinion.

In your work on midwifery, you make, correctly, the important distinction to which I refer. You state (p. 148) that "the word [labour] is highly expressive of the violent and painful struggles and efforts of the woman." You add that "the essential element of labour is the contraction of the muscular fibres of the womb." And, at page 303, in speaking of the strength of these uterine contractions, you observe, "Let it be well borne in mind that the expulsive powers of the womb are enormously great." In more than one place in your work, you allude to the intensity of the sensations of pain, "the pangs and agonies of travail," as you term them (p. 155); and at page 153 you speak of the "painful sensations" of the mother, in the last part of labour, as so great in degree, "as to be absolutely indescribable and comparable to no other pain." In your still later work on Female Diseases, speaking of these pains—the pains of parturition—you observe, "Men cannot suffer the same pains as women. What," you continue, "do you call the pains of parturition? There is no name for them but Agony."

The muscular contractions of the uterus form, you say, the "essential element" of labour. In that opinion you and I are at one, and further, I quite agree that this cannot safely be "eschewed

and obviated" in natural labour; nor are they "eschewed and obviated" under the proper use of chloroform.

But the pain, the second element, is a non-essential in the process. It is non-essential, because—1. Labour, that is the uterine contractions, are occasionally, though very rarely, in the course of practice, seen to accomplish the full expulsion of the child with little or no pain; 2. In whole tribes of the human race, as in some of the black tribes, comparatively little or no pain seems to be endured, if we may believe various authorities; and 3. Hundreds of women have, during the last year, been delivered with perfect safety, but without any pain, while placed under the influence of anæsthetic agents.

I hold the pain to be *non-essential*, and I protest against the truth of your opinion that "the pain of a natural labour is a state not by all possible means to be eschewed and obviated." On the contrary, I maintain that we omit and forego a mighty part of our professional duties whenever we forget the axiom of Bacon, that "it is the office of a physician not only to restore health, but to mitigate pain and dolours." And if, as medical men, we are called upon to mitigate and remove pain of any degree in our fellow-beings, we are surely called upon to mitigate and remove those "pangs and agonies of travail," as you term them, which in degree are, in your own language, "absolutely indescribable and comparable to no other pain,"—"pains for which there is no other name but Agony."

In your practice, you, like other medical men, constantly use measures to mitigate and relieve the pains of headache, of colic, of sciatica, of pleurodyne, of gout, rheumatism, and all the other innumerable "dolours" that flesh is heir to. Like other physicians, you deem it, I doubt not, your duty to wield the powers of your art, in order to free those that submit themselves to your medical care, from these and from other similar sufferings. But if it is right for you to relieve and remove these pains, why is it not right for you also to relieve and remove the pains accompanying the act of parturition? I cannot see on what principle of philosophy, or morality, or humanity, a physician should consider it his duty to alleviate and abolish, when possible, the many minor pains to which his patients are subject, and yet should consider it improper to alleviate and abolish, when possible, pains of so aggravated a character, that, in your own language, they are "absolutely indescribable and comparable to no other pains,"—"pains for which there is no other name but Agony."

3. *You object to anæsthesia in natural labour, because you deem the pain of natural labour a "physiological pain."*

"The sensation of pain in labour is," you observe, "a physiological relative of the power or force," and "to be in natural labour is the culminating point of the female somatic forces."

Now, for the reasons that I have already stated, I entirely doubt if we should look upon the severe sensations of pain endured by our patients as truly "physiological," for, as I have just stated, they are *not* essential to the mechanism and completion of the process in the white races of mankind, and they are absent, to a great degree, in the black. The severity of them could, I think, be easily proved to be the result of civilisation, and, as I believe, of that increased size of the infantile head which results from civilisation. Parturition is always physiological in its object, but not in some of the phenomena and peculiarities which attend upon it in civilised life.

But, waiving this point, or the discussion of it, let me state, that even if I allowed all the intense pains of parturition to be "physiological pains," I cannot conceive that to be any adequate reason for us not relieving women from the endurance of them. Because nature has fashioned any particular physiological function in any particular manner, that, I opine, is no reason why the science and art of civilised life should not, when possible, alter and amend its workings. If it were improper for us, for instance, to intermeddle with the functions of the hair of the head, or of the skin generally, then all hats and other coverings for the scalp, all clothings and coverings for the body, should be at once abandoned and unconditionally condemned. If it were improper for us to alter and amend the functions of the eye, then all optical glasses, the telescope, the microscope, etc., must be thrown aside. And, indeed, not later than the seventeenth century it was held and argued so in England. For, in his history of the first beginning of the Royal Society of London, Sprat tells us that it was generally believed that this "new experimental philosophy, namely, the philosophical papers laid before the Society, was subversive of the Christian faith," and many, he adds, mortally hated the newly-invented optical glasses, the telescope, and the microscope, as *atheistical inventions*, which perverted our organs of sight, and made everything appear in a new and false light (D'Israeli). You argue as if we should not use means to eschew the pains of parturition *because* that pain is physiological. When Columbus first discovered your mighty American continent,

a large portion of the inhabitants were unprovided with any kind of dress or covering. "To most of them," says Robertson, "nature had not even suggested any idea of impropriety in being altogether uncovered." And I do think that men living in such a state, could, against the fashion of dressing, use with far greater propriety and consistency than you or me, your own argument against anæsthetics in labour. Chloroform and ether should not be used in labour, you argue, because the pain against which they protect us is natural and physiological. No kinds of clothing or dress should be used, the original Americans might have equally argued, because the cold or heat against which they protect us is natural and physiological.

I have a letter lying before me on the subject of anæsthetics in midwifery, by a very highly and very justly esteemed teacher of midwifery in Dublin. "I do not," he writes, "believe that any one in Dublin has as yet used ether in midwifery; the feeling is very strong against its use in ordinary cases, and merely to avert the ordinary amount of pain which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural labour; and in this feeling I heartily and entirely concur."

The argument thus used, and so very well expressed by my Irish correspondent, is one which has been often adduced and repeated during the course of the past year. Some minds at first gave immense weight and importance to it. For my own part, I must confess that I never could view it as possessing any great force. Look at it as applied to any other practice which happens to be sufficiently old and established; and then we shall see it in its true import. Supposing, for example, it referred to the *first* introduction of carriages into use; it would then read thus:—"I do not believe that any one in Dublin has as yet used a carriage in locomotion; the feeling here is very strong against its use in ordinary progression, and merely to avert the ordinary amount of fatigue which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural walking; and in this feeling I heartily and entirely concur."

Nay, this frequently-repeated argument against new innovations becomes not only, I think, ridiculous, but really almost irreverent, when we look far backward into the march of civilisation, and apply it to any practices that are so very long established as to be very antiquated, and with which, therefore, the human mind has been long and intimately familiarised. Some one, but who I cannot pretend to say, no doubt first introduced the practice of wearing hats or bonnets, or some covering for the head. Supposing this

practice, however, stoutly resisted, as doubtlessly it was at first, then the argument of my Dublin friend against this innovation would read somewhat as follows :—" I do not believe that any one in Dublin has as yet used a hat to protect his head ; the feeling here is very strong against its use in ordinary weather, and merely to avert the ordinary amount of wetting and cold which the Almighty has seen fit—and most wisely we cannot doubt—to allot to mankind ; and in this feeling I heartily and entirely concur." Some day a canal will, in all probability, be made through the Isthmus of Panama. It has, you are well aware, long been proposed to cut one ; and there and thus unite the Atlantic and Pacific Oceans. When it was proposed in the sixteenth century, a priest of the name of Acosta brought forward the following reason against it. " I am," said he, writing in 1588, " of opinion that human power should not be allowed to cut through the strong and impenetrable bounds which God has put between the two oceans, of mountains and iron rocks, which can stand the fury of the raging seas. And, if it were possible, it would appear to me very just, that we should fear the vengeance of Heaven for attempting to improve that which the Creator in his almighty will and providence has ordained from the creation of the world." The arguments which are here brought forward by the earnest Spanish priest, against man meddling with and altering the impediments to navigation caused by the natural mechanism of the Isthmus of Panama, are essentially the same as those lately brought forward against men meddling with and altering the agonies caused by the natural mechanism of parturition in the civilised woman. We can all, perhaps, at this time of day, see through and smile at the character of the old priest's argument with regard to the supposed impropriety of changing and cancelling, if possible, the natural obstruction produced by any isthmus. Some years after this, perhaps, our descendants will equally see through and smile at the analogous modern argument in regard to the supposed impropriety of changing and cancelling, when possible, the physical suffering produced by a physiological function.

The truth is, all the tendencies of man, in a civilised state of society, are to intermeddle with and change, and, as he conceives, improve, the action of almost every function in the body. And each such improvement has, at the time of its introduction, been, like the practice of anæsthesia, very duly denounced as improper, impious, etc. etc. I might refer to numerous such cases. Let me cite only one example. The human fingers are admirably constructed

by our Creator for the function of seizing and lifting objects. The late Sir Charles Bell wrote a whole octavo volume—a Bridgewater Treatise—on the mechanism of the human hand, as beautifully adapted for this and other functions. In the reign of the earlier Stuarts, forks were introduced from the Continent to assist our hands in the act or function of seizing and lifting the divided portions of meat, etc., that we wished to eat. But this was a very sad and uncalled-for innovation upon the old and established physiological functions of the human fingers; and, at the time, it was as loudly opposed and decried as the modern employment of anæsthetics in aiding the physiological function of human parturition. D'Israeli tells us that the use of forks was so much reprobated in some quarters, that some uncleanly preachers denounced it "as an insult on Providence not to touch our meat with our fingers." Nature herself has provided us with fingers of flesh and bone and nerve, and consequently, is it not unnatural and impious in man to attempt, in his human pride and arrogance, to substitute for these, artificial metallic fingers of silver and steel?

I repeat—all our tendencies and workings, in the present state of civilisation, are attempts to intermeddle with, and change and improve, the action of almost every function in the economy. And assuredly, if we use means in regard to the function of parturition, with the view of ameliorating and abolishing the unnecessary, but, as you call them, "absolutely indescribable" pains that attend upon it, we should be doing nothing more than what you and I and all of us are ever doing in relation to most of the other natural or physiological functions of our own bodies.

Let me illustrate this last remark by one more example, for, as I have already said, it is only in this way that we can properly judge of the soundness or unsoundness of our views of novel points in theory or practice. You are well aware that the act of parturition has been often familiarly compared, as the late Professor Hamilton expressed it, "to the toils of a journey," and like it divided into stages. "The sufferings of the mothers," says he, "have been in most languages compared to those of travellers." Now let us for a moment continue this natural simile between the function of parturition and the function of progression. You maintain that "labour is the culminating point of the female somatic forces." One of the most illustrious Presidents of your great American Republic—Thomas Jefferson—makes in his memoirs a remark of precisely the same import regarding walking or progres-

sion. He describes the act of walking, but not exactly in the same words, as the kind of "culminating point of the human somatic forces."[1]

Few, or none, perhaps, will question the abstract truth of Jefferson's observations on this point. But, because walking or progression is a "physiological" function, and the practice of it is reputed salutary, would this be, with you, a proper and sufficient reason for never setting aside or superseding in any way this "physiological" state, in the same way as you insist, on the same grounds, that the physiological pain of labour should not be set aside or superseded. Because progression is a natural condition, would this be any adequate reason for your medical advisers adopting your own arguments against anæsthesia in midwifery, and insisting upon this, that, the next time you travelled from your own city of Philadelphia to the cities of Baltimore or New York, you should walk the distance on foot instead of travelling it by railway or other conveyance? What opinion would you form of the judgment of any medical adviser to whom you entrusted your own health, if, on going next time to the New York or Baltimore railway station, he should gravely and solemnly repeat to you, as his patient, what you tell your midwifery patients, and, in your own language, advise you to try to accomplish the intended journey on foot, as (to quote your own words) "a desirable, salutary, and conservative manifestation of life-force"? And yet this would really be nothing more than making your *argumentum ad fœminam* an *argumentum ad hominem.*

You state, in a passage which I have already quoted, that even the agony accompanying instrumental delivery by the forceps is a "physiological pain." I do not, I confess, see why the suffering attending the use of the forceps, when the head is impeded by any cause of obstruction, should be regarded as a "physiological pain," any more than the suffering attending the use of the catheter, in obstruction from the prostate gland or other morbid conditions of the urethra, should be regarded as a "physiological pain." They are

[1] Since writing the above, I have turned up Jefferson's Memoirs to get his own words. "Walking," says the American President, "is the best possible exercise; habituate yourself to walk very far. The Europeans," he continues, "value themselves on having subdued the *horse* to the uses of man; but I doubt whether we have not lost more than we have gained by the use of this animal. No one has occasioned so much (as the horse) the degeneracy of the human body. Our Indians go on foot nearly as far in a day, for a long day, as an enfeebled white does on his horse; and he (the Indian), will tire the best horses."—*Memoirs*, vol. i. p. 287.

both operations intended to remove the natural contents of the respective viscera, when their operative removal becomes necessary.

But let us waive this point, and return again to the analogy between the functions of progression and parturition. Suppose you plead with your medical adviser that, instead of insisting on your going on foot, they should allow you *for once* to take advantage of artificial assistance, and proceed on your journey from Philadelphia to Baltimore or New York by railway, because you were unable to walk the distance in consequence of being incapacitated by a rheumatic knee, or a sprained ankle, or an inflamed or blistered toe, and they replied to you that you should not care for this, but still proceed and suffer, because the pain you might thus suffer was (to use again your own language) still only a "physiological pain." Would that argument be any adequate philosophic consolation under the endurance of your suffering? or would you not laugh at the logic of your medical adviser, and take your seat in the railway in spite of his doctrine? And I have a fancy that betimes, in midwifery, patients *will* learn to adopt exactly the same line of practice under the analogous circumstances, and think and act too exactly in the same way.

4. *You object to anæsthesia in labour, because the mother, in escaping by it from the "pangs and agonies of labour," may, in a few rare cases, be thus made to encounter danger to her own life.*

"Should I," you observe, "exhibit the remedy for pain to a thousand patients in labour, merely to prevent the physiological pain, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the life or the death of one in a thousand in a questionable attempt to abrogate one of the general conditions of man?" Let me add that I have seen this argument of yours already repeated from your letter, and strongly insisted upon by the opponents of anæsthesia in this country.

And, indeed, in a new practice, such as that of anæsthesia, and with which the mind is yet not at all familiarised, the above forms one of that kind of apparently strong statements, which it is impossible to answer directly, or, indeed, by any other way than by taking, as I have already said, a corresponding illustration and simile from some other matter with which the mind is already familiarised. Let us for a moment longer

then, adhere to the familiar comparison which I have already taken up, under the last head, between the physiological function of human parturition, and the physiological function of human progression. Suppose, then, that you and I were standing at the Philadelphia station on the first day of the opening of the railway to Baltimore or New York. I wish the passengers to Baltimore or New York, or the shorter and intermediate stations, to proceed thither by railway; but you argue with them, like President Jefferson, that "progression is the culminating point of the human somatic forces," and that "walking is a desirable, salutary, and conservative manifestation of life-force," and that progression being a "physiological function," and fatigue a physiological pain, they ought to proceed on foot. I say "No." Place yourself in a railway carriage, and thus eschew and obviate all the great fatigue and useless over-exertion of foot-travelling. Then comes that answer and argument of yours which I have quoted, and which runs as follows. "But should I exhibit, sir, the remedy for fatigue (*a railway carriage*) to a thousand travellers, merely to prevent the physiological exertion and fatigue of walking, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the death of one in a thousand, in a questionable attempt to abrogate one of the general conditions of man—viz. his power of progression by walking?"

I shall not stop to inquire whether, among our supposed lady passengers or patients (uninured, as most of them are, either to long pain or long walking), more than one in a thousand would not be worn out and destroyed by taking the journey on foot. A less proportion, I believe, would be found to be ultimately destroyed by the perils and dangers of the journey by railway than by the exertion and fatigue of the journey on foot, and the walk would shake and damage, both temporarily and permanently, many more constitutions than the railway carriage. I have a firm conviction that, on the great scale, there would be found a more absolute saving both of human life and of human health by adopting the means invented by art than the means provided by nature. And I most firmly believe that yet a similar difference will be found to hold good between the two corresponding practices of allowing women to pass through labour afflicted with all their usual physiological "pangs and agonies," and carrying them through that process without their being subjected to the endurance of these pangs and agonies.

But I proceed to remark, that if your supposed theory with regard to the function of parturition were carried out in regard to the other functions of the human body, it would produce a vast and mighty revolution in the practices of civilised life. Follow it out, for instance, with regard to any one of them, as, for example, with regard to the one we have already spoken of, viz. progression, and see what would be the results. Ever and anon our newspapers contain paragraphs, telling us of one or more human lives being lost by collisions on railways, explosions of steamboats, upsettings of stage-coaches, etc. Consequently, according to your doctrine, the featherless biped, pedestrian man, should no longer, when travelling, fly in railway cars, ply in steamboats, ride in coaches, etc., for these are evidently all so many questionable attempts to abrogate what you call "one of the general conditions of man, viz. his original pedestrianism."

In the great government and police of nature, disease and death are among the most certain "general conditions of man." If your theory were true, the practice of medicine itself should, I fear, be at once and summarily abandoned, for, perhaps, in your own language, it is, at best, a questionable attempt to abrogate one of the general conditions of man, and I am sure you will agree with me, that in this "questionable attempt" human lives are often lost from the mistakes, or the passiveness, or the want of knowledge and skill, on the part of the physician. In England and Wales, in 1840, there were, according to the returns of the Registrar-General, above 100 persons publicly and officially reported as having died from the effects of one drug alone, opium. But would this be any reason, or any ground of reason, for abandoning in medicine the use of opium, perhaps, in itself, the most valuable of all the remedies in our pharmacopœia? Would this be any adequate argument for refusing to relieve, by a dose of opium, the next appropriate case of pain that you are called to? Or because chloroform or ether, in a very rare case, now and again produces deleterious or even fatal consequences, should we refuse, in a thousand other persons, to mitigate and annul their agonies by its use?

In your esteemed letter to me, you quote some remarks from the celebrated old work, Raynalde's *Birth of Mankinde*, the first book on Midwifery printed in English. Look at the Prologue to the work. It is excellent in reference to the very matter we are discussing—viz. whether the rare accidents, from abuse or otherwise, to which any good gift may occasionally subject those who use it, should be a reason for repudiating the general use of that gift. "There is not

anything," says Raynalde, "so absolute and perfecte, but by the occasion of the abuse thereof at one tyme or other, may and doth ensue greate daunger and damage to mankynde." He instances fire and water, "two right necessary elementtes to the use of man, without the whiche he could not lyve," yet sometimes "by fyre hath ben consumed and devoured whole cities and countreys. By water swalowed and drowned infinite men, shippes, yea and whole regions. Agayne," he continues, "meate and drynke, to the moderate users thereof, doth minister and maynteyn life; and contrary, to the unmeasureable and unsatiate gourmauntes and gluttons, it hath full many thousand tymes brought surfettes, sickenesse, and at the laste, death. . . . . But," he argues, "shoulde men, for the avoydyng al these foresade inconveniences, and for the reasons abovesayde, condempne and bannysh fyre and water, forsake theyr meate and drynke? No, *it were but madnesse once to thinke it.*"

Before passing from these, your supposed dangers of anæsthetics, let me add two remarks—1st, I do believe that if improperly and incautiously given, and in some rare idiosyncrasies, ether and chloroform may prove injurious or even fatal—just as opium, calomel, antimony, and every other strong remedy and powerful drug, will occasionally do. Drinking cold water itself will sometimes produce death. "It is well known," says Dr. Taylor, in his excellent work on Medical Jurisprudence, "it is well known that there are MANY cases on record, in which cold water, swallowed in large quantity, and in an excited state of the system, has led to the destruction of life"—(p. 8). Should we, therefore, never allay our thirst with cold water? What would the disciples of Father Matthew say to this? But, 2dly, You and others have very unnecessary and aggravated fears about the dangers of ether and chloroform, and in the course of experience you will find these fears to be, in a great measure, perfectly ideal and imaginary. But the same fears have, in the first instance, been conjured up against almost all other innovations in medicine, and in the common luxuries of life. Revert again to our old simile regarding travelling. Cavendish, the Secretary to Cardinal Wolsey, tells us, in his life of that prelate, that when the Cardinal was banished from London to York by his master—that regal Robespierre, Henry VIII.—*many* of the Cardinal's servants refused to go such an enormous journey—"for they were," says Cavendish, "loath to abandon their native country, their parents, wives and children." The journey, which can *now* be accomplished in six hours, was considered *then* a perfect banishment.

We travel now between London and Edinburgh (some four hundred miles) in twelve or fourteen hours. A century ago the stage-coach took twelve or fourteen days. And in his life of Lord Loughborough, Lord John Campbell tells us that when he (the biographer) first travelled from Edinburgh to London, in the mail-coach, the time was reduced to three nights and two days; "but," he adds, "this new and swift travelling from the Scotch to the English capital was wonderful, and I was gravely advised," adds Lord John, "to stop a day at York, as several passengers who had gone through without stopping had died of apoplexy from the rapidity of the motion."—*Lives of the Lord Chancellors.*

Be assured that many of the cases of apoplexy, etc. etc., alleged to arise from ether and chloroform, have as veritable an etiology as this apoplexy from rapid locomotion; and that a few years hence they will stand in the same light in which we now look back upon the apoplexy from travelling ten miles an hour. And as to the supposed great moral and physical evils and injuries arising from the use of ether and chloroform, they will by and by, I believe, sound much in the same way as the supposed great moral and physical evils and injuries arising from using hackney coaches, which were seriously described by Taylor, the water poet, two or three centuries ago, when these coaches were first introduced. In his diatribe against hackney coaches, Taylor warned his fellow-creatures to avoid them, otherwise, to quote his own words, "they would find their bodies tossed, tumbled, rumbled, and jumbled without mercy." "The coach," says he, "is a close hypocrite; for it hath a cover for knavery; they (the passengers) are carried back to back in it like people surprised by pirates; and, moreover, it maketh men imitate sea-crabs in being drawn sideways," and altogether "it is a dangerous carriage for the commonwealth." Then he proceeds to call them "hell-carts," etc., and vents upon them a great deal of other abuse, very much of the same kind and character as that lavished against anæsthetics in our own day.

In the course of your remarks, you imply, I think, though you nowhere explicitly state, another objection to anæsthetics in midwifery, viz.—

5. *You object to anæsthesia in labour, because you do not consider that the mother encounters danger to her health or life from the endurance of the pains.*

"I have been accustomed," you observe, "to look upon the sen-

sation of pain in labour as a physiological relative of the power of force, and notwithstanding I have seen so many women in the throes of labour, I have always regarded a labour pain as a most desirable, salutary, and conservative manifestation of life-force."

If you hold, as your language appears to me to imply, that the sensation of pain, even when, as in labour, the degree of the pain is "absolutely indescribable," has no morbid or deleterious influence upon those who endure it, then I most decidedly disagree with you. On the contrary, I sincerely believe that the human constitution is so constituted that it cannot endure pain, particularly when that pain is long in duration, or severe in degree, without being more or less affected and injured by it. I know of many medical and obstetric authors, from the time of Ambrose Paré down to the time of Travers, Gooch, Alison, Burns, etc., who have stated and explained the common and hitherto unchallenged opinion of our profession in all ages, that pain was, in itself, deleterious and destructive, causing depression of the heart, syncope, and even, when in excess, sometimes producing speedy and sudden death. But, till the late discovery in your own country of the possibility of annulling the pains of surgical operations by the inhalation of ether, I know of no writer in medicine, in surgery, or in midwifery, who held that pain, when "absolutely indescribable" in degree, was a matter of no importance in regard to the life or health of the sufferer, and should not be relieved even when we had the complete power of relieving it.

If the mere pain of the labour were, as you state, a "desirable, salutary, and conservative manifestation of life-force," its long continuance, the very length of it, would insure, more certainly, the health and safety of the patient, than its shortness. Anything "salutary and conservative" to the constitution, should manifestly be safe in proportion to the length, and dangerous in proportion to the shortness, of the duration. But as far as regards the life and health of the mother, the pain of labour is perfectly the reverse of all this. It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labour, more subsequently died than when their pain did not exceed two hours; of those that were eight hours in labour, more subsequently died than of those that were four hours ill; of those that were twelve hours in suffering, more died than of those that were eight: and so on in a regular

progression. The longer this supposed "salutary and conservative manifestation of life-force," as you term it, the greater became the mortality; so that, in the long run, the maternal mortality was fifty-fold greater among the women that were above thirty-six hours ill, than among those who were only two hours in labour; one in every six of the former dying in childbed, and only one out of every three hundred and twenty of the latter.

Some time ago, I published a long series of statistics, tending to show, that out of a large collection of cases of the same operation, performed with and without anæsthesia, those who were operated on under anæsthesia, and consequently without the usual suffering, recovered in a much larger proportion than those who had the same operation performed without anæsthesia, and whose constitutions were subjected to the endurance of the usual pains and agonies of the surgeon's knife.[1]

The same result holds good, I believe, in midwifery as in surgery. Save the maternal constitution, either by natural or artificial anæsthesia, from the endurance of the pains connected with parturition, and you will enhance both the chances of her recovery, and the facility of it. Among your Red Indian and other uncivilised tribes, the parturient female does not suffer the same amount of pain during labour, as the female of the white race; and in consequence of this escape, they recover far more rapidly from the effects of parturition; nor are fatalities at all common among them. So easy is the convalescence among uncivilised tribes, that Strabo, Marco Polo, and other historians and travellers, tell us of whole communities in which the husband immediately went to bed for a number of days, upon the birth of a child, and the wife watched and nursed him. "They that write the history of America," says Guillemeau, "tell of the women in that country, that, as soon as they be delivered, they presently rise up, and lay their husbands in their room, who are used and attended like women in childbed."

Among the patients who have been delivered in Scotland under anæsthesia, the rapidity of the stage of convalescence has, as a general rule, been increased in a degree that seems often to surprise the patient herself, as much as her escape from the labour pains themselves. Many of my obstetric brethren have remarked this circumstance to me. In fact, on awaking after delivery, the patient does not encounter and endure the usual feelings of exhaustion and fatigue. Some have declared to me that they have felt as if they

[1] See *ante*, Part II. Chap. VI.

had awoke from a refreshing sleep. And when we consider the capabilities for the enduring of suffering and exertion among the class of patients in civilised life upon whom you and I attend, perhaps the propriety for employing anæsthesia during labour may appear more evident. Unaccustomed by their mode of life to much pain and fatigue, patients in the higher ranks of life are not fitted to endure either of them with the same power or the same impunity as the uncivilised mother, or even as females in the lower and hardier grades of civilised society; and hence there is the greater propriety and necessity in the physician employing all the means of his art, so as to save them, as far as possible, from their sufferings. To illustrate the point, let us revert again to our old comparison between the physiological functions of progression and parturition. Let us compare, for a moment, our ideas of the effects of fatigue from walking and of pain from parturition upon the female constitution; and surely the comparison is not an unfair one for your views, as far as the severity of the effects of the two influences, physical fatigue and physical pain, are concerned, for surely the effects of pain, of "absolutely indescribable" pain, should be greater upon the constitution than mere muscular fatigue. Suppose, then, that our patients, at the end of the ninth month of pregnancy, had to walk on foot a continuous journey of one, two, three, six, or a dozen or more hours' duration—that is, of five, ten, twenty, or thirty miles, or upwards—instead of passing through a continuous journey of recurring labour pains of the same duration, the pains gradually becoming stronger, and latterly becoming "absolutely indescribable, and comparable with no other pains"—what would be the result with, say, one hundred ladies of the upper classes of society? Some of them might be little or not at all affected by the journey; others, weak perhaps when they began, would suffer more or less severely from it. Not a few would be inclined, sooner or later, to stop, and beseech you, if you were the medical attendant upon them, to save them from farther exertion and fatigue, by allowing them to be carried or coached the required distance. In answer to their solicitations, would you console them by telling them that, after all, progression was a "conservative manifestation" of life-force, and free from danger, or would you take the other view, and give them means of travelling the required distance by carriage or rail? I am sure you would have recourse not to the former but to the latter, for you would fear and dread the effects of fatigue upon the fragile constitutions of your lady patients. And I repeat, that certainly

the effects of the endurance of pain are as great, if not greater, upon the constitution, than the effects of the endurance of fatigue. But if you would allow your patients to ride the supposed journey, instead of unnecessarily forcing and compelling them to walk it on foot, equally, I think, should you allow them to escape what you term the "pangs and agonies of travail," by saving them by chloroform, or other anæsthetic agents, during their travail, from all the unnecessary endurance of these pangs and agonies.

You state, "I have not yielded to several solicitations, as to the exhibition [of chloroform], addressed to me by my patients in labour." If, when driving out into the country, you perchance meet one of your fair patients, a few miles from Philadelphia, walking homeward, but so tired and way-worn that every five or ten minutes she stopped and groaned for fatigue, "absolutely indescribable, and comparable to no other fatigue," I am sure you would consider yourself bound, on the principles of common humanity, not to withstand her "solicitations" to be driven home in your carriage, and thus relieved of her present anxieties and suffering. And I cannot see why, if you do this (and who would not do it?), to relieve a patient from the mere effects of fatigue, you could refuse to relieve the same lady when in "the pangs and agonies of travail," from the endurance of pains which are, in your own words, absolutely "indescribable, and comparable to no other pains."

"Perhaps," you observe, "I am cruel in taking so dispassionate a view of the subject." Of course, it would ill become me to pass any such judgment upon you; but I feel this, that you and I, and other teachers of midwifery, are placed, in reference to this question, in a position far more fearfully responsible than ordinary medical practitioners. The ordinary obstetric practitioner has little or no power, except over the relief or the perpetuation (according as he may choose it) of the sufferings of his own immediate patients. But you and I, as obstetrical teachers, may, through our pupils, have the power of relieving or of continuing the sufferings of whole communities. If, perchance, you persist for some years longer in your present opinion, it will have the effect of inflicting a large amount of what I conscientiously believe and know to be altogether unnecessary agony and suffering upon thousands of our fellow-beings. If you review and alter your opinions (which I earnestly hope you will do), and make yourself sufficiently acquainted with the peculiarities in the mode of action and mode of exhibition of chloroform during labour, a

vast proportion of human suffering may, even within the next few years, be saved by your happy instrumentality and influence.

Feeling, as I do deeply, the great responsibility, in this respect, of your situation and of mine, I trust you will kindly pardon and excuse me, if anywhere in the preceding pages I may have appeared to defend my views with too much earnestness. If I had to rewrite or revise the observations, I would perhaps have stated them more accurately; but I must send them as they are. And along with them I beg to send, also, the most sincere esteem and reiterated respects of, my dear sir, yours very faithfully,

J. Y. Simpson.

CHAPTER IX.

REPLY TO OTHER MINOR OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.

Edinburgh, February 1849.

An objection to the employment of chloroform in midwifery, on the ground of alleged indecencies committed by patients whilst under its influence, has been raised by Mr. Gream.[1] This is by no means a new objection. At the time when various of our now common articles of food and medicine were first introduced into use, these innovations were frequently opposed and decried on exactly the same ground. As a general rule, this was an argument always resorted to by weak and impure minds, when they could find no stronger arguments against any proposed innovation in our customs or habits. For instance, at the present day, no one imagines or argues that the eating of potatoes is liable to cause in those who eat them any indecency of word or action. And yet that was one of the principal arguments maintained by the unscrupulous against the use of potatoes for long after their introduction. In his "Traditions," Mr. Chambers states that, even as late as the last century, there was a prejudice against the potato for this, among other reasons—"That it was a provocative to incontinence." A hundred years after this our successors in the profession will probably feel as much surprised at the idea of the use of chloroform exciting improper words and actions, as we are all now at the old idea of the use of potatoes being attended with the same alleged consequences. I have now used upwards of 2000 oz. of chloroform, and have never witnessed any such effects, and never expect to witness them. In a circular letter of Mr. Gream to the English practitioners, to which attention has been directed by Dr. Cumming,[2] Mr. Gream did not ask for any beneficial effects from chloroform that his correspondents might have seen; he asked only for the bad effects; he wanted to see only one side of the picture. And, doubtless, he would be

[1] See *Remarks on the Employment of Anæsthetic Agents in Midwifery*, by G. T. Gream, Churchills, 1848.

[2] See *Proceedings of Edinburgh Obstetrical Society*, February 14, 1849.

gratified by some of the reports given him ; for I believe from what I occasionally hear from some parts of England, that in many districts and towns there the use of chloroform is still greatly misunderstood ; a bad and dangerous article is often used under the name of chloroform ; the article imperfectly and improperly exhibited ; and no proper care and precaution used in its exhibition. But still the profession would take any report from Mr. Gream on the subject, *cum grano salis permagno.* In a former amusing pamphlet on the subject, Mr. Gream proceeded so far as to alter the official report in a case of death from chloroform at Aberdeen, in order to prove to his readers that those who used chloroform may have morbid adhesions of the heart to the pericardium. In the report given in to the Law or Government authorities in this case, it was stated that there existed "old adhesions" between the heart and percardium which required to be forcibly torn asunder with the finger. Mr. Gream pretended to quote this report verbatim, and in inverted commas ; but he carefully omitted the word "old," etc., for he wished to prove to the ladies who read his pamphlet that these pericardial adhesions were quite recent, and the *effects* of the chloroform ! Our grandchildren will, no doubt, wonder at such pathological ratiocination, even with the omission of the word "old ;" but at the present day it is calculated to teach us this circumstance, that one who could tamper with an official and public document, for the purpose mentioned, will have little hesitation in following the same course with any private documents intrusted to him.

In Edinburgh, chloroform has now (1849) been exhibited, I believe, to 40,000 or 50,000 persons, without a single accident or deleterious result, traceable to its use. Perhaps as many doses of opium, or antimony, or calomel, or Epsom salts even, or any other potent medicine, would not have been followed by results equally innocuous. Some two or three hundred die in England and Wales every year of the effects of opium and other medicines, given in improper doses, or in particular idiosyncrasies ; and when the proper use of chloroform comes to be better understood, it will, I believe, stand less high in that list than most other medicines used ; while, besides saving pain, it will ultimately be found to save human life to no small extent in surgery and midwifery. One means by which its proper mode of employment will become more widely spread in a few years is that our medical students, who daily see its use, and the rules for using it practised in the hospitals of Edinburgh and London, will leave their studies, and com-

mence practice with all the knowledge necessary for its employment, and without any of the fears naturally shown by those who are unacquainted with its phenomena. It has been often repeated that in Harvey's time no physician who was above forty years of age would believe in the doctrine of the circulation ; and new medical theories and practices were very seldom adopted by men beyond that term. Some beyond this age of conversion have argued and reasoned on the subject in the most extraordinary way. For instance, an old but excellent accoucheur, Dr. James Reid,[1] in a late discussion on the subject of anæsthesia, at the Westminster Society, stated that he had used chloroform in three midwifery cases, and that it had *not* relieved the pains of labour. Of course Dr. Reid's argument merely and simply showed one thing, that he was ignorant of the mode of giving it, for no one here or elsewhere, who knows anything of the matter, has met with such results. And the most strange results were often ascribed to it. In fact, if *anything* whatever happened to the mother or child for months, it mattered not what, it is by some of our English friends directly ascribed to chloroform. It produces all manner of medical, and some surgical diseases. If the child has a dislocation or fracture, the cause is the chloroform. The same gentleman, Dr. Reid, some time ago mentioned, at the same Society, the case of a child dead-born, in the practice of Dr. Smith, after a very long labour (forty or fifty hours), and where the mother was chloroformed. The child died in consequence of dislocation or fracture of the lower parts of the parietal bone against some projecting point or other in the interior of the maternal pelvis. But the death, Dr. Reid argued, was from chloroform ; and as the immediate cause of death was the above, hence chloroform could produce dislocation or fracture, and a hundred deleterious results besides. I have heard chloroform accused, on equally logical grounds, of being the cause of a monstrosity on the part of the fœtus, when it happened to be given at the birth of the malformed child. Certainly not many years will elapse ere the profession must see the weakness and folly of this mode of argument. But, as it stands, no newly-introduced practice has ever, I believe, made so great and steady progress in so short a time as anæsthetic midwifery ; and of its ultimate and entire success and adoption everywhere, no obstetrician in Edinburgh, where all witness and practise it, has any kind of doubt.

[1] *Lancet*, January 27, 1849.

CHAPTER X.

ALLEGED DIFFICULTIES IN THE SUPERINDUCTION OF ANÆSTHESIA.

Edinburgh, October 1848.

I HAVE been occasionally told of cases, in which it was supposed that it was impossible to produce the anæsthetic effect of chloroform. In my own practice (and I have now used chloroform in many hundred cases), I have never yet met with a single instance in which a person was proof against its full influence.

It has been sometimes averred, in the English journals, that on attempting to use chloroform, jactitation, incoherent talking and delirium, spasms, etc. etc., have supervened, *instead* of a state of quiet anæsthesia. These are symptoms which do occasionally come on in the *first* or exciting stage of its action, more especially if strict quietude is not enjoined; and, though they are apt to terrify the beginner, they are in reality no more serious in their effects and character, than some of the equally frightful symptoms sometimes seen in hysteria. They are an evidence, however, of one of two things—either that the vapour is being given too slowly, or that it is given in two small quantity—in an exciting, in fact, instead of a soporific dose; and the simple remedy, as every one properly experienced in its action knows, is at once to increase the dose in order to pass the patient as speedily as possible into the *second* or full narcotic stage.

Chloroform, it is alleged, sometimes gives rise to much coughing, and pulmonary irritation. Certainly not so, if the chloroform is of good quality, and its vapour is not at first approached in too strong and concentrated a form to the face of the patient. After some experience, it will be found that it can be given so as seldom or never to induce even coughing. Some time ago, a well-known physician, in a large city of the south of England, wrote me, that he and his townsmen had found it too dangerously irritant a substance to breathe, and that he had seen it produce cough, bronchitis, phthisis, etc. The answer was simple; it never produced any such

effects in Edinburgh practice. And I believe that the explanation was equally simple; he and his townsmen had experimented with an imperfect and impure article. A few days ago one of the principal druggists in Edinburgh showed Dr. Christison, Dr. Douglas Maclagan, and myself, a bottle of chloroform of high specific gravity, viz. 1·490, which he had just received from a very large manufacturing chemical house in London. It was impossible to breathe it without feeling great irritation in the throat and chest. It emitted fumes that at once reddened litmus paper; and which, on examination, proved to be muriatic acid. Is it wonderful that bronchitis, coughs, and more serious disasters, should have followed the inhalation of such an improper and dangerous article?

Dr. Letheby has shown that some kinds of chloroform in the market, besides containing muriatic acid, are also mixed with aldehyde, hydrochloric ether, hypochlorous acid, etc.[1]

[1] See *Medical Gazette* for June 16, 1848, p. 1038. The presence of some of these deleterious agents has been supposed to be an inevitable and speedy effect of the spontaneous decomposition of very pure chloroform. But I find that some of the article, manufactured several months ago, in its purest form, by Duncan, Flockhart, and Company, of this city, has undergone no kind of change, even though long exposed to the sun. Messrs. Smith have also shown me the same, in regard to their chloroform. I have reason to know, that the dangerous article alluded to in the text as containing a quantity of muriatic acid, has been extensively sold to the profession, throughout Scotland and England, at a price two or three shillings per pound cheaper than is charged for the pure chloroform manufactured by other houses; and, probably, its very cheapness has led to its extensive use. The following is the formula by which chloroform is prepared by Messrs. Duncan, Flockhart, and Company, of Edinburgh, whose article I have always found of the most superior quality:—4 pounds of chloride of lime, and 12 pounds of water, are first well mixed together, and then 12 ounces of spirit added. Heat is then applied to the still (which ought not to be more than a third full), but as soon as the upper part of the still becomes warm the heat is withdrawn, and the action allowed to go on of itself. In a short time the distillation commences, and whenever it begins to go on slowly, the heat is again applied. The fluid which passes over separates into two layers, the lower of which is chloroform. This, after having been separated from the weak spirit forming the upper layer, is mixed with half its measure of strong sulphuric acid, added gradually. The mixture, when cool, is poured into a leaden retort, and distilled from as much carbonate of baryta by weight, as there is of sulphuric acid by measure. The product should be allowed to stand over quicklime for a day or two, and repeatedly shaken, and then redistilled from the lime. The specific gravity of the resulting chloroform is generally 1·496 or 1·497.

CHAPTER XI.

REMARKS ON THE OCCASIONAL FATAL RESULT OF THE ADMINISTRATION OF CHLOROFORM, WITH CASES OF SUDDEN DEATH DURING SURGICAL OPERATIONS WITHOUT ANÆSTHETICS.

Edinburgh, June 9th, 1852.

In the last number of the *Medical Times and Gazette*, Dr. Snow asks me to state the cases of death from chloroform which have occurred in Scotland.

He mentions two cases, both at or near Glasgow; one under the operation of extracting a toe-nail, the other while sounding for stone. I do not know any particulars whatever as to the former case. The common current account of the latter is given in the last number of the *Monthly Medical Journal*, page 554, under some observations on chloroform, to which I beg to refer your correspondent.

In addition to these two cases, I have heard only of one more instance of a fatal result from the employment of chloroform in surgery, and that also near Glasgow. In this instance, chloroform was given by the practitioner for tooth-extraction; but, I am sorry to add, none of the parties present were at the time in a condition to give any very satisfactory evidence.

These are the only cases, so far as I know, of death in Scotland from chloroform, among the many thousand cases in which it has now been exhibited in connection with surgical proceedings! and I believe, that if any others had occurred, I should have been certain to have heard of them.

Before chloroform was introduced, sulphuric ether was reported to have produced the death of one patient in Scotland, who had amputation of the thigh performed, in consequence of a severe railway injury. One of the surgeons, however, afterwards assured me, that in the dark hovel in which the operation was performed, it was found impossible to secure the vessels sufficiently quickly with the ligatures to prevent the collapsed patient dying from hemorrhage after the leg was removed; but the ether was a better apology than the hemorrhage for the man's sudden sinking.

I know of one medical patient who died south of the Tweed, when using, or shortly after using, chloroform, to procure sleep in *delirium tremens*. But, as Dr. Snow is well aware, death often enough occurs suddenly in that disease; and the last thing done is always apt to be blamed for the result. A medical friend of mine had, some time ago, a patient suffering under *delirium tremens*. Opiates, etc., had all failed to produce quietude, or induce sleep. At last, the practitioner who was watching by his patient, went home to his own house, which was in the immediate neighbourhood, to obtain some chloroform for the purpose of using it as a hypnotic. During the few minutes of his absence in search of the chloroform-bottle, his patient died.

A somewhat similar coincidence happened at the very first introduction of chloroform here. After discovering the anæsthetic effects of chloroform, I was, of course, anxious to get it tried in a surgical operation. The first surgical cases in which it was used were operated upon in the Royal Infirmary here, on 15th November 1847. Two days previously an operation took place in the Infirmary, at which I could not be present, to test the power of chloroform; and, so far, fortunately so; for the man was operated upon for hernia, without any anæsthetic, and suddenly died after the first incision was made through the skin, and with the operation uncompleted. I know of another case in Edinburgh where death instantaneously followed the use of an abscess-lancet, without chloroform; the practitioner, in fact, deeming the case too slight to require any anæsthetic.

While, since the anæsthetic effects of chloroform were known, we have thus had two patients dying in Edinburgh on the operating-table, who did not use chloroform, we have had no death under the same circumstances in the vast number who have now taken chloroform here for surgical operations and other purposes.

Some time ago I was informed of an instance in which a practitioner urged a patient to use chloroform for the purpose of allowing a tooth to be removed that had worn her out with pain. She postponed it for a few hours; and, in the meantime, went to bed to procure, if possible, some rest. On going to her room an hour or two subsequently, she was found dead. In this, as in other similar instances, the reputation of chloroform had a narrow escape.

In the different discussions that have taken place, in this country and on the Continent, regarding chloroform as the supposed cause of death in various surgical patients, it appears to me, that it has very

generally been forgotten that patients have ever and anon died during, or immediately after, operations, long before the time of the use of any anæsthetics. But, when such cases occur now, and anæsthetics do happen to be employed in them, the latter are always naturally, though perhaps not always justly, blamed. I have already alluded to two such cases which have happened in Edinburgh since the time chloroform was known here; and, doubtless, if chloroform had been employed in these cases, it would have been taxed with the fatal result. Shortly before the first of these cases, my friend Dr. Robertson had shaved the groin of a patient, and was about to proceed to perform the operation for hernia, when the patient fainted, and died before any incision was made. I have been told of various cases by other surgeons, where the patient died on the operating-table before the days of ether and chloroform, and where the result now would be considered by many as the palpable and indisputable effect of any anæsthetic the patient might chance to use.

In making this remark, I do not, of course, by any means wish to argue, that chloroform may not, and has not proved fatal when used in surgical operations. Nothing could be farther from my thoughts or intentions.

The druggists of Edinburgh have sold, I believe, during the last four or five years, as much chloroform to the medical practitioners and to the public of this city, as would produce anæsthesia in one or two hundred thousand separate instances; and, indeed, most of the practitioners here use it daily for diagnostic and other purposes, surgical, obstetrical, and medical.

I am sure you will doubt with me, whether an equal amount of full doses of antimony, or of opium, or even of Epsom salts, would not have been followed by a greater number of deaths occurring, perhaps more slowly, but, probably, also more surely. And, on the contrary, this agent, while, like all other medicinal agents, proving injurious now and again in a rare exceptional case, has, I rejoice to think, been already the means of saving, during the last few years, a vast amount of human suffering, and by that means no small amount also of human life.

I am not aware of any death in Scotland or elsewhere from the use of chloroform in midwifery, out of the many thousand cases in which it has now been employed in the old and new world. Nor, indeed, does the obstetric patient run anything like the risk of the surgical patient; for, in midwifery, though the anæsthetic is re-

quired to be given for a far longer period, it does not require to be given so deeply as in surgery.

Since November 1847, I have only attended twelve cases of labour in which chloroform was not used during delivery. In all my other cases I have employed it; and none of those patients, I venture to say, who have used it, would again choose to suffer the unnecessary pains attendant upon labour without it. Most of my obstetric brethren employ it as frequently as I do. After once beginning its use at an obstetric case, I generally leave its exhibition to be continued by the nurse, or by any intelligent friend of the patient who may be in the room. Some of our midwives use it in the cases which they themselves attend. Two weeks ago one of them told me that she had now employed it herself in her own practice in above fifty cases, with nothing but the happiest results, and without meeting with any circumstances to give her the slightest feeling of uneasiness in its employment.

Chloroform is manufactured to a large extent by three or four chemical establishments in Edinburgh; and as its high price in England has been, I believe, one great cause of its slow introduction into general practice among you, let me state, that with us it is usually sold wholesale to apothecaries at six or seven shillings per pound weight; and it is retailed to medical men at eightpence, and to the general public at one shilling the ounce per weight. It ought to be as cheap in the south.

I am not aware that in Scotland, where chloroform is employed so very extensively and so very successfully by medical men and others, it is ever exhibited by any one except on a handkerchief, towel, or the like; no kind of formal apparatus is used. Doubtless, one principal point consists in diluting its vapour sufficiently freely with air, and this can always be readily accomplished when a handkerchief is employed. Some American and English practitioners have proposed to render the vapour of chloroform less strong by diluting it when used with the vapour of sulphuric ether, or of alcohol, as in the so-called chloric ether. But alcohol, etc., often leave headache and excitement, which chloroform does not. And it is surely a thousand-fold better to dilute it with the vapour of "common atmospheric air," than with any other diluting medium.

Perhaps I should add, that twice or thrice chloroform has been used in Scotland by medical men and others to commit suicide; but, of course, Dr. Snow's inquiry does not extend to these cases of its deliberate abuse. He alludes to one of those cases in which a

druggist at Aberdeen destroyed himself by "breathing chloroform for amusement when no one was present;" and he proceeds to speak of a fatal accident "from this proceeding" as a "matter of course." I am not aware of any other similar case in Scotland.

Edinburgh, 1855.

The exhibition of chloroform, as of every other potent drug used in medicine, is liable to be attended with danger and death, provided it be given in too large or in too long-continued doses. Like most other valued medicinal agents, it is powerful for evil as well as for good. But its occasional disagreement with, or deleterious influence upon, one in 10,000 or 20,000 patients, is no sound argument against other patients benefiting from its employment. It has been calculated, from the returns of the Registrar-General, that every year in England and Wales alone some 300 or 400 human beings are poisoned with opium; but certainly no one would argue that this is any reason why opium, the most valuable remedy in our pharmacopœia, should not be given to other human beings in proper doses and in proper cases. Patients have often sunk under the depressing effects of calomel, antimony, digitalis, etc.; but such accidents, while they teach us very strong lessons of caution, form no reason why these most useful drugs should be banished from the pharmacopœia. Many persons are annually drowned in bathing; but no reasonable man would argue from such unfortunate occurrences that this powerful means of maintaining and restoring health be therefore abandoned and forsaken. Deaths certainly ever and anon occur in patients subjected to the influence of chloroform, but assuredly only very rarely indeed when a pure drug and all proper precautions are used. Perhaps the exhibition of any other potent medicinal agent in the materia medica, exhibited in equally full doses to as many hundreds of thousands of patients as have now inhaled full doses of chloroform, would have been followed by more accidents and deaths than have been witnessed in the use of this anæsthetic agent. When we consider the immense extent to which chloroform has already been employed in all quarters of the world, in medicine, in surgery, and in midwifery, the frequent great impurity of the drug, and the little care which has sometimes been observed in its use, the wonder perhaps really is, that so few accidents have happened from its employment. And as a counterbalance to these accidents, we know from statistical evidence the fact, that in the absolute it has been a great means,

not only of saving human suffering, but also of saving human life, by diminishing in a marked ratio the danger and fatality attendant upon surgical operations and diseased states. Thus, let us take amputation of the thigh as an example. Out of 987 cases of this operation, collected by Mr. Phillips, 435 proved fatal, or 44 in every 100 died. But out of 144 amputations of the thigh performed upon patients in an anæsthetic state, only 27 proved fatal, or 25 in 100 died. According to this computation the number of persons saved from death in amputation of the thigh by the patients being anæsthetised during the operation, amounts to 19 lives in every 100 operations performed, or to 190 lives out of every 1000 such operations.

All the patients that die under the hand of the operator when chloroform is used, do not necessarily die from the effects of the chloroform upon the constitution. In several of the recorded cases the dose given was far too small to have had any such fatal effect. Before the time that anæsthetics came to be used in surgery, deaths on the operation-table ever and anon occurred. Such cases have been recorded by Brodie, Cooper, Home, Travers, etc. etc., but they excited no marked share of professional attention, as they were generally supposed to be accidents against which no caution could be of any use. Of late years, and since chloroform has been employed, they have usually been directly and at once ascribed to the deleterious action of the chloroform.

Edinburgh, February 1870.

A few weeks ago, Dr. Brotherston, of Alloa, sent into Edinburgh a patient who was the subject of an ovarian tumour. She brought a note from him asking if I thought the case a fit one for ovariotomy. I wrote back that it seemed to me to be so. The patient was married, about twenty-two years of age, thin and emaciated, and I thought the tumour, which was as large as the pregnant uterus at the sixth or seventh month, felt more solid than multilocular ovarian tumours of this size usually do; yet it seemed free from adhesions.

Dr. Brotherston took the patient into the small village Hospital at Alloa, and urgently requested me to be present when he operated. Accordingly, on the morning of February 5, I went up to him. Drs. Duncanson and Wilson of Alloa were also to be present, but Dr. Duncanson did not arrive till after the patient was laid upon the table and the operation begun. With the view of allowing Dr.

Wilson to give full assistance to Dr. Brotherston at the operation, I chloroformed the patient. In doing so I placed a single layer of towel over the nose and mouth, leaving the eyes exposed, and dropped the chloroform upon the towel. When Dr. Brotherston made his first cutaneous incision, the patient moved so much that he stopped for a brief time till I put the patient more deeply under the effects of the anæsthetic. The tumour was rapidly reached, and was then attempted to be diminished in size by tapping, but only a comparatively small quantity of fluid escaped. Dr. Brotherston then extended the opening upwards for an inch or more above the umbilicus, and was introducing and using his hand with the view of turning out the ovarian mass, when the patient vomited suddenly and profusely. Immediately the eyes opened, the pupils were preternaturally dilated, the face looked pallid, and the respiration, which had never been affected by the chloroform so as to have the least noise or stertor in it, seemed arrested. Instantly artificial respiration was set on foot, and the tongue pulled forward. Deep spontaneous respiration then occurred several times in succession, and I deemed at the moment that the patient was hence out of danger; but a second collapse occurred, which terminated in death, all means of resuscitation proving unavailing.

On a *post-mortem* examination of the body, ordered by the legal authorities, no diseased conditions could be found in the head, chest, or elsewhere. The ovarian tumour was free from any peritoneal adhesions. On examining its structure, Dr. Pettigrew, the esteemed Pathologist of the Royal Infirmary of Edinburgh, found it to be cancerous in its character.

Remarks.—In the first paper which I published on chloroform, December 1847,[1] I stated that this drug, if given in too great or too long-continued doses, "would doubtless produce serious consequences, and even death;" and at the same time I expressed the hope that "its great potency would be one great safeguard against its abuse." Since that period I have exhibited it myself, or been present when it was exhibited, in several thousands of instances, but have not seen its employment terminate in death before the occurrence of the preceding unhappy case.

According to all the experimental and clinical observations which have been made, chloroform appears capable of destroying life in two ways—namely, (1) by asphyxia, and (2) by syncope. Death by asphyxia can generally, if not always, be averted by at once arrest-

[1] See *Edinburgh Monthly Journal* for December 1847. p. 415.

ing the inhalation of the drug whenever the breathing becomes noisy or stertorous—states which, as already mentioned, never occurred with the preceding patient. Death by syncope, or sudden stoppage of the action of the heart, is, doubtless, far less under control, and has apparently formed the principal cause of the fatal issues in almost all the cases in which patients have perished when under the use of chloroform. Perhaps fewer cases of syncope actually do occur under operations since the introduction of anæsthetics, because the nervous and sensory systems of the patients are so far obtunded by their employment that a patient is, I believe, in greater jeopardy, if syncope do happen, when he is under the influence of chloroform than when he is not under it, because the irritability and action of the heart are diminished by the free use of it, as shown by the lowering and slowing of the pulse. Yet when syncope does occur in chloroformed persons, artificial respiration and its accompaniments usually are sufficient to rally and restore the patient. When the preceding case was described by me at a late meeting of the Edinburgh Obstetrical Society, Dr. Gordon and Dr. Angus Macdonald each mentioned an instance in which sudden fainting took place, with pallor of the face, open eyes, and very dilated pupils, at the first commencement of the incisions in two slight operations—the one for the removal of a small tumour, the other for the incision of a carbuncle, and in neither case had the patient taken any unusual dose of chloroform. They both recovered under artificial respiration. Various analogous cases have been recorded.

But are all such cases of syncope that take place during operations, and which end or do not end in death, the result of the action of the chloroform which happens to be used at the time? The question is one which has never, perhaps, sufficiently attracted the attention of the profession; for doubtless it is true that, antecedently to the introduction of anæsthetics, patients sometimes died from syncope upon the operating-table, both immediately before and after the operation was commenced, and under conditions and circumstances which in modern times, when anæsthetics are almost universally employed in operations, would be not unnaturally described and regarded as deaths from chloroform. Formerly, such sudden deaths under surgical operations do not seem to have been looked upon as matters of moment, because, in fact, no special pathological or practical interest was attached to their occurrence. They were simply regarded as inevitable accidents, and are usually only incidentally alluded to, when alluded to at all by surgical authors,

provided they illustrate some special observation or opinion on the part of the writer. Thus, as showing how "violence alone without the loss of blood may often produce immediate fatal effects," John Hunter makes the following observation:—

CASE I.—"I have seen," writes Mr. Hunter, "a man thrown into such convulsions from the operation of the hydrocele being performed upon him, that I began to despair of his recovery." "I have known," he adds, "a man die immediately of castration." (See his works, vol. iii. p. 431.)

In the first volume of his work on *Constitutional Irritation*, Mr. Travers cites several cases of sudden death in surgical patients, to illustrate different pathological principles to which he points. For example, he gives the following:—

CASE II.—A robust middle-aged man, the subject of a moderate-sized aneurism in the femoral artery, was received into the London Hospital, and readily assented to the usual operation for that disease. On entering the theatre, however, he fainted, and had some wine and water given to him. The operation was then proceeded with; the artery was exposed, and the ligature applied, but not tightened. During the operation it was observed that no pulsation could be felt in the tumour, and on examining the patient more minutely it was found that he was quite dead. On dissection, both sides of the heart were found empty, and the lungs turgid with blood. No other particular appearance was observable.

CASE III.—"I saw," again writes Mr. Travers, "a man who was the subject of strangulated hernia. He expired suddenly on the table during the steps preliminary to the operation, which, from the state of the symptoms and of the bowel, as ascertained by examination after death, might be said to afford the fairest prospect of relief."

CASE IV.—A man, who had been bitten in the finger by a cat, and in whom symptoms resembling those of hydrophobia had been present for twelve hours, submitted to the excision of the bitten part, and died in three minutes.

CASE V.—A brewer's servant, a man of middle age and robust frame, suffered much agony for several days from a thecal abscess, occasioned by a splinter of wood penetrating beneath the nail of the thumb. A few seconds after the matter was discharged by a deep incision he raised himself by a convulsive effort from his bed, and instantly expired. (See Travers on *Constitutional Irritation*, vol. i. p. 25.)

These several cases are, as I have said, stated by Mr. Travers, each to illustrate some special pathological principle; but they do not necessarily include all the cases of sudden death upon the operating-table which he may have seen or heard of, and which were not calculated, in his opinion, to point to any special surgical fact. In former years, in talking with surgeons who had seen many operations before the time of anæsthetics, I heard of various instances in which patients had been removed dead from the operating-table. It is, perhaps, impossible now to collect adequate data to fix the

probable frequency with which such accidents formerly happened. We may, perhaps, fairly infer, however, that they were not very rare, for, when attending specially to this question in the first six or eight years of anæsthesia in surgery, the following cases occurred in Edinburgh and its neighbourhood.

Case VI.—Shortly before the introduction of anæsthetics into surgical practice in 1846, my late friend, Dr. John Argyll Robertson, lecturer on surgery, was asked to see a case of strangulated inguinal hernia in the practice of Dr. Thomson. The gentleman was removed from bed, and placed upon a table to facilitate the required operation. Dr. Robertson, before proceeding to use his scalpel, was employed in removing the hairs from the groin with a razor, when the patient suddenly complained of faintness, gasped, and died.

Case VII.—After discovering the anæsthetic effects of chloroform in November 1847, I tested it in a case of tooth-pulling, but required to wait eight days before I had an opportunity of using it in the hospital or elsewhere in any surgical operations. A few days, however, after its discovery, a hernia, which had been strangulated for a few hours, was brought into the Infirmary, and Professor Miller thought it a case demanding operative interference, and one on which chloroform should be tried. But I could not be found in time for the purpose of giving it, and the patient was operated on without any anæsthetic. Professor Miller had only proceeded the length of dividing the skin, when the patient fainted, and died with the operation unfinished. If the chloroform had happened to be used, and this fatal syncope had occurred while the patient was under its action, the whole career of the new anæsthetic would have been at once arrested.

Case VIII.—Within a year or two afterwards, I saw, at my own house, a child with a large abscess in the neck, and I wrote to Dr. Pattison, the medical attendant, to the effect that I thought it was sufficiently advanced to be opened. Next day, when about to open the collection of pus, the mother suggested that the child should first have some chloroform exhibited to it. Dr. Pattison had none with him, explained that the walls of the abscess were so thin that the operation would not give much pain, and put his lancet into the purulent swelling. The child immediately fainted, and died, without any hemorrhage or any other complication to account for the fatal syncope.

Case IX.—In a case belonging to Dr. Gilchrist, of Leith, I saw a surgeon try to treat a sacculated aneurism at the root of the neck by a small quantity of hemostatic injection. As the operation involved no cutting, no chloroform was used. The patient's respiration speedily became stertorous, and he died in a few minutes.

Case X.—In 1853, Dr. Richard Mackenzie was called to see a patient who had shortly before fallen and fractured the radius. After examining the case, Dr. Mackenzie felt his pocket for his chloroform-bottle, but found he had it not with him. Anxious not to lose time by sending for it, he forthwith adjusted the displaced ends of the bone, and applied splints and a bandage. A few minutes after leaving the patient's house, Dr. Mackenzie was suddenly recalled, and found the patient dead.

If, in the preceding cases, chloroform had happened to be employed, the fatal results would naturally by most minds have been attributed to the anæsthetic, and not to the operation or the condition

of mind or body connected with the operation. Such cases, however, teach us at least that caution is required in our reasoning and inferences, seeing death may occur, and has occurred, in operations without chloroform, and with phenomena quite similar to those ascribed to the action of chloroform. Most of the stronger drugs in the pharmacopœia—as opium, elaterium, antimony, mercury, etc.—are, proportionally to the number of cases in which they are used in full doses, as fatal as, or more fatal than, chloroform, but they are not so sudden, and hence not so terribly appalling in their dangerous and fatal effects. The number, for example, of lives lost yearly by the poisonous effects of opium, etc., is much greater than that lost by chloroform.[1] At our different drug-manufactories in Edinburgh we have upwards of two million doses of chloroform manufactured annually, yet how rarely does a fatal result follow its use! Is there any other common or potent drug which could be given in full doses in two millions of instances per annum with greater impunity?

[1] In 1840, out of every 1,000,000 living in England and Wales 24 were poisoned by opium and 22 by other medicines improperly given to children below the age of five years.—See *Seventh Annual Report of the Registrar-General*, p. 82. In England and Wales, in the five years from 1863-67 there were poisoned by preparations of opium 632 individuals; by salts of lead, 242; by overdoses of medicine, 52; by strychnine, 41, etc. There were drowned during the same period, while bathing, 707 persons, while sliding or skating, 116; burned to death by clothes taking fire, 2194; killed by falls in walking, 194; suffocated by bedclothes, 2332 children; suffocated by overlaying, 682; died from "navel hemorrhage," 572, etc.—See *Thirtieth Report of the Registrar-General*, pp. 176-8.

PART III.

THE NATURE AND POWER OF VARIOUS ANÆSTHETIC AGENTS.

CHAPTER I.

GENERAL CONSIDERATIONS.

What are Anæsthetics?—The word Anæsthetics is employed to designate a series of vapours or gases, which, when inhaled, produce, with more or less rapidity, the anæsthesia or insensibility to pain, which is required for the practice of midwifery and surgery.

They vary greatly in external characters and in chemical composition, as the following table will show :—

TABLE OF THE CHIEF ANÆSTHETISING AGENTS.

SUBSTANCES.	Chemical Formula.	State at common Temperature.	Specific Gravity of Liquid.	Specific Gravity of Vapour or Gas.	Boiling Point.
Nitrous Oxide . .	N_2O	gas	...	1·525	
Chloroform . .	$CHCl_3$	liquid	1·497	4·2	142°
Sulphuric Ether .	$C_4H_{10}O$	liquid	·7365	2·586	94·8°
Nitric Ether . .	$C_2H_5NO_3$	liquid	1·112	...	185°
Nitrous Ether . .	$C_2H_5NO_2$	liquid	·947	2·627	61°
Alcohol . . .	C_2H_6O	liquid	·815	1·6133	173°
Aldehyde . .	C_2H_4O	liquid	·8009	1·532	67·8°
Dutch Liquid . .	$C_2H_4Cl_2$	liquid	1·280	3·450	184°
Bisulphide of Carbon .	CS_2	liquid	1·272		
Amylene . . .	C_5H_{10}	liquid	·659	2·45	102°
Hydride of Amyl .	C_5H_{12}	liquid	·638	2·5	86°
Chloride of Amyl .	$C_5H_{11}Cl$	liquid	...	...	215°
Benzina Benzoli .	C_6H_6	liquid	·85	2·77	177°
Propyl . . .	C_6H_{14}				

Various theories have been advanced at different times, trying to attribute the power of producing the peculiar action manifested by this class of bodies to one or more of their constituent elements. The one which has met with most favour is that in which this power is said to be resident in the carbon, from which, it is maintained, all these agents derive their anæsthetising virtues, more or less modified by the other elements with which the carbon is associated. There is one insuperable objection, however, to this view, for the experiments of Davy and Wells have shown that nitrous oxide gas is an anæsthetic of considerable power, and yet it contains no carbon at all.

So also with all the other theories, none of which are successful in tracing the peculiar action of this class of bodies to any particular element.

How do Anæsthetics produce their peculiar effect?—On this point we must at once admit our complete ignorance, for no one has yet satisfactorily demonstrated the manner in which they do so.

Some suppose that they poison the blood, as occurs in uræmia, and that so the coma is induced. This is not probable, however, for we could hardly suppose that the effects would come on so rapidly, or pass away so quickly, if such were the case. Dr. Murphy holds the following view. He thinks that the chloroform, not being soluble in the blood, travels through the circulation unchanged. This free chloroform, he says, exerts a great affinity for oxygen, so as to prevent the usual affinities taking place. Carbonic acid is not, therefore, formed in the same proportion, so that carbon is not sufficiently removed from the tissues, and accordingly he supposes the anæsthetic influence is kept up. He supports this view by the fact, that intense cold acts as an anæsthetic, and that this is owing to cold diminishing the amount of carbonic acid.

The rapidity of action of anæsthetics is to be explained by the mode in which they are introduced into the system ; for it is a well-known fact that medicines act much more rapidly if inhaled in the form of vapour or gas, than if introduced into the stomach, or taken in any other way. The vapour is at once applied to the whole surface of the pulmonary mucous membrane (which is very large, being calculated by Dr. Reid to have fifty times the superficies of the cutaneous surface), immediately beneath which an infinite number of minute capillaries are distributed, so that the vapour almost instantaneously gets entrance into the blood. It is then hurried through the circulation, and, in the space of one or two minutes,

comes in contact with the lining membrane of the entire vascular system, which is many hundred times greater in extent than the cutaneous surface.

Under those circumstances, can we wonder that its action, in whatever way it may subsequently be brought about, should be so rapid? A great revolution may occur some day in medicine, so that it may become a more general practice to administer remedies by the way of inhalation, and thus insure a far more rapid induction of their physiological effects. What an advantage it would be, for instance, if we were able to produce mercurialism in a few hours, instead of requiring at least two or three days to affect it!

The rapid disappearance of the effects of the chloroform is, however, quite as remarkable as the rapidity with which they are produced. Sulphuric ether and most other anæsthetics are easily known to pass off, in a great measure at least, by the lungs, as the breath of the patient smells of them for some days afterwards.

This is not observed so easily with chloroform, but it, too, probably passes off in the same manner. Dr. Snow says that the chloroform cannot be detected in the breath by the sense of smell, after the lungs have been emptied by two or three expirations of the vapour they contained when the inhalation was stopped. He has detected it, however, in the breath by chemical tests, even after the return of consciousness. The chloroform may pass off by the kidney also, but, if so, it is only in very minute quantity.

CHAPTER II.

CHLOROFORM.

"I esteem it the office of a physician, not only to restore health, but to mitigate pain and dolours."—BACON.

Edinburgh, November 1847.

FROM the time at which I first saw ether-inhalation successfully practised in January last, I have had the conviction impressed upon my mind, that we should ultimately find that other therapeutic agents were capable of being introduced with equal rapidity and success into the system, through the same extensive and powerful channel of pulmonary absorption. In some observations, which I wrote and published in March last, relative to the inhalation of sulphuric ether in midwifery, I stated that, in several obstetric cases, I had used ergot of rye in this way, along with ether.[1]

With various professional friends, more conversant with chemistry than I am, I have, since that time, taken opportunities of talking over the idea which I entertained of the probable existence or discovery of new therapeutic agents, capable of being introduced into the system by respiration, and the possibility of producing for inhalation vaporisable or volatile preparations of some of our more active and old-established medicines: and I have had, during the summer and autumn, ethereal tinctures, etc., of several potent drugs, manufactured for me, for experiment, by Messrs. Duncan, Flockhart, and Company, the excellent chemists and druggists of this city.

Latterly, in order to avoid, if possible, some of the inconveniences and objections pertaining to sulphuric ether—(particularly its disagreeable and very persistent smell, its occasional tendency to irritation of the bronchi during its first inspirations, and the large quantity of it occasionally required to be used, more especially in protracted cases of labour)—I have tried upon myself and others the inhalation of other different volatile fluids, with the hope that some one of them might be found to possess the advantages of ether

[1] See Part V. Chap. I.

without its disadvantages. For this purpose, I selected for experiment, and have inhaled, several chemical liquids of a more fragrant or agreeable odour, such as the chloride of hydrocarbon (or Dutch liquid), acetone, nitrate of oxide of ethyle (nitric ether), benzin, the vapour of iodoform, etc.[1] I have found, however, one infinitely more efficacious than any of the others, viz., Chloroform, or the Perchloride of Formyle, and I am enabled to speak most confidently of its superior anæsthetic properties, having now tried it upon upwards of thirty individuals. The liquid I have used has been manufactured for me by Mr. Hunter, in the laboratory of Messrs. Duncan, Flockhart, and Company.

As an inhaled anæsthetic agent, chloroform possesses over sulphuric ether the following advantages :—

1. A greatly less quantity of chloroform than of ether is requisite to produce the anæsthetic effect ; usually from a hundred to a hundred and twenty drops of chloroform only being sufficient ; and with some patients much less. I have seen a strong person rendered completely insensible by six or seven inspirations of thirty drops of the liquid.

2. Its action is much more rapid and complete, and generally more persistent. I have almost always seen from ten to twenty full inspirations suffice. Hence the time of the surgeon is saved ; and that preliminary stage of excitement, which pertains to all narcotising agents, being curtailed, or indeed practically abolished, the patient has not the same degree of tendency to exhilaration and talking.[2]

[1] In talking over, with different chemists, what fluids might be sufficiently volatile to be respirable, and hence deserving of being experimented upon, Mr. Waldie first named to me the perchloride of formyle, as worthy, among others, of a trial ;—Dr. Gregory suggested a trial of the chloride of hydrocarbon, etc. I have been deeply indebted to Dr. Gregory and Dr. Anderson, for their kindness in furnishing me with the requisite chemical agents for these experiments ;—and also to my assistants, Dr. Keith and Dr. Duncan, for the great and hearty zeal with which they have constantly aided me in conducting the inquiry.

[2] In practice I have found that any such tendency, even with ether, is avoided by—1st, giving the patient from the first a large and overwhelming dose of the vapour, and 2dly, by keeping him perfectly quiet and still, and preventing all noise and talking around him. I have elsewhere insisted on the importance of these points. (See the numbers of the *Monthly Journal of Medical Science* for March 1847, p. 726, and for September, p. 154.) In the paper last referred to, I took occasion, when discussing the conditions requisite for insuring successful etherisation, to observe, "*First*, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherisation, and during his recovery from it. All talking and all

3. Most of those who know from previous experience the sensations produced by ether-inhalation, and who have subsequently breathed chloroform, have strongly declared the inhalation and influence of chloroform to be far more agreeable and pleasant than those of ether.

4. I believe that, considering the small quantity requisite, as compared with ether, the use of chloroform will be less expensive than that of ether; more especially, as there is every prospect that the means of forming it may be simplified and cheapened.

5. Its perfume is not unpleasant, but the reverse; and the odour of it does not remain for any length of time, obstinately attached to the clothes of the attendant—or exhaling in a disagreeable form from the lungs of the patient, as so generally happens with sulphuric ether.

6. Being required in much less quantity, it is much more portable and transmissible than sulphuric ether.

7. No special kind of inhaler or instrument is necessary for its exhibition. A little of the liquid diffused upon the interior of a hollow-shaped sponge, or a pocket-handkerchief, or a piece of linen or paper, and held over the mouth and nostrils, so as to be fully inhaled, generally suffices in about a minute or two to produce the desired effect.[1]

questioning should be strictly prohibited. In this way any tendency to excitement is eschewed, and the proper effect of the ether-inhalation more speedily and certainly induced. And, *Secondly*, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapour as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect; * * * * a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotising dose of the vapour. Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule;—not the principle of etherisation, but the mode of putting it in practice being altogether to blame. But, *Thirdly*, whatever means or mode of etherisation is adopted, the most important of the conditions required for procuring a satisfactory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife, until the patient is under the *full* influence of the ether vapour, and *thoroughly and indubitably soporised by it.*" In fulfilling all these indications, the employment of chloroform evidently offers great and decided advantages, in facility and efficiency, over the employment of ether.

[1] When used for surgical purposes, perhaps it will be found to be most easily given upon a handkerchief, gathered up into a cup-like form in the hand of the exhibitor, and with the open end of the cup placed over the nose and mouth of

It is perhaps not unworthy of remark, that when Soubeiran, Liebig, and Dumas, engaged a few years back in these inquiries and experiments by which the formation and composition of chloroform was first discovered, their sole and only object was the investigation of a point in philosophical chemistry. They laboured for the pure love and extension of knowledge. They had no idea that the substance to which they called the attention of their chemical brethren could or would be turned to any *practical* purpose, or that it possessed any physiological or therapeutic effects upon the animal economy. I mention this to show that the *cui bono* argument against philosophical investigations, on the ground that there may be at first no apparent practical benefit to be derived from them, has been amply refuted in this, as it has been in many other instances. For I feel assured, that the use of chloroform will soon entirely supersede the use of ether; and, from the facility and rapidity of its exhibition, it will be employed as an anæsthetic agent in many cases, and under many circumstances, in which ether would never have been had recourse to. Here then we have a substance which, in the first instance, was merely interesting as a matter of scientific curiosity and research, becoming rapidly an object of intense importance, as an agent by which human suffering and agony may be annulled and abolished, under some of the most trying circumstances in which human nature is ever placed.

Edinburgh, December 1847.

At the first winter meeting of the Edinburgh Medico-Chirurgical Society, 10th November 1847, I directed the attention of the members to a new respirable anæsthetic agent which I had discovered a short time previously—viz., Chloroform, Chloroformyle, or Perchloride of Formyle. In this chapter I shall state briefly some of the principal facts pertaining to its history, composition, effects, etc.

Chemical History and Composition.—Chloroform was discovered at nearly the same time by Soubeiran (1831), and Liebig (1832). Its chemical composition was first ascertained by Dumas and Peligot

the patient. For the first inspiration or two, it should be held at the distance of half-an-inch or so from the face, and then more and more closely applied to it. To insure a rapid and perfect anæsthetic effect—more especially where the operation is to be severe—one or two teaspoonfuls of the chloroform should be at once placed upon the hollow of the handkerchief, and immediately held to the face of the patient. Generally a snoring sleep speedily supervenes; and when it does so, it is a perfect test of the superinduction of complete insensibility. But a patient may be quite anæsthetic without this symptom supervening.

(1835). It consists of 2 atoms of carbon, 1 of hydrogen, and 3 of chlorine; or, to express it otherwise, of 1 atom of formyle, and 3 of chlorine. Hence its chemical formula is C_2HCl; or $FoCl$.[1]

Modes of Preparation.—It may be obtained by various processes. 1. By the distillation of a mixture of diluted spirit, pyroxylic or wood spirit, or acetone, and chloride of lime (bleaching powder); or, 2. By making milk of lime, or an aqueous solution of caustic alkali, act upon chloral; 3. By leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, etc.

Physical and Chemical Properties.—It is a clear limpid liquid, as heavy as 1·480; not inflammable; very volatile; and boils at 141°. It has a fragrant, fruit-like odour; and a sweet saccharine taste.

Therapeutic History.—It has been used internally. Guillot employed it in asthma, diluted with water one hundred times (1844). My friend, Dr. Formby of Liverpool, told me, about two years ago, that he used it often in a diluted form as a diffusible stimulant; and I have, since that period, frequently prescribed it instead of valerian, camphor, etc.[2] But I am not aware that any person has used chloroform by inhalation, or discovered its remarkable anæsthetic properties, till the date of my own experiments.

Physiological Effects.—After the first two or three full inspirations, a feeling of warmth and excitation, radiating from the chest to the extremities; followed by whirring noises in the ears; a sensation of vibratory thrilling and benumbing throughout the body; with, betimes, rapid loss of sensation and of motion, and at last of consciousness. Often before total unconsciousness supervenes, the patient, guided by instinct rather than by volition and reason, makes an effort to get rid of the inhaling vapour and handkerchief, as if it interfered with free respiration. This temporary effort must be resisted by the exhibitor. During the full anæsthetic sleep produced by chloroform, sometimes no mental action goes on, or at least is remembered; in many others, the mind is active as in dreams. The respiration is usually at first soporose; the pupil sometimes natural, in others slightly contracted, in others, dilated The pulse is usually quickened ten or twenty beats at first, but afterwards falls to its normal rate, and if the vapour is exhibited

[1] $CHCl_3$, by present notation. [Ed.]

[2] Since first publishing on the subject of chloroform, Dr. Glover of Newcastle has pointed out to me, that, in an essay on Bromine, in the 152d number of the *Edinburgh Medical and Surgical Journal*, he mentions having poisoned several animals with chloroform, by injecting it into their blood-vessels, stomach, and the cavity of the peritoneum, and has investigated its physiological mode of action.

very long in very powerful doses, it comes down more and more below the natural standard; muscles of voluntary motion in general relaxed; more rarely cataleptic; still more rarely clonically contracted, as happens also occasionally with ether.

In small doses, given slowly, its effects are exhilarating, and exactly like those generally following the inhalation of nitrous oxide gas. Of course, when exhibited in this way, the patient is in a state of excitement quite unfit for a surgical operation. When given for surgical operations, it should be exhibited rapidly in large doses, and the patient sent over into a deep soporose or stertorous sleep *before* the incisions are begun.[1]

Uses in Surgery.—1. To relax the muscles in reducing dislocations, etc.; 2. To avert the sufferings attendant on deep probing, and other painful but necessary modes of diagnostic examination and dressing; and 3, and principally, To annul the pain of operations by the caustic, ligature, or knife.

Uses in Midwifery.—To diminish and annul the physical pains attendant on labour, and more especially those which accompany the passage of the child's head through the pelvic cavity and outlet —(the second stage of Denman).

Uses in Medicine.—1. As an *antispasmodic;* as in asthma, laryngismus, tetanus, and other spasmodic diseases, etc.,[2] I have used

[1] I believe all the reputed failures and misadventures are attributable to two causes—viz., 1. Using an impure and imperfect variety of chloroform; and 2. Not giving it in sufficiently large and rapid doses.

[2] In various trials at Morningside, at which Dr. Christison, Dr. Skae, and Dr. Wingett were present, Dr. S. had sent violent patients over into a soporose sleep in a minute or less. As to what its therapeutic effects, if any, might be in insanity, we had, he believed, no power yet of judging. It could not be expected to be of any marked service—at least in any short time—in such chronic cases, as it had been tried. But it had this effect: the patients could be kept asleep under it for a long series of hours. In this way, it had already apparently cured some cases of delirium tremens, and, he believed, also of puerperal insanity, and might yet be found useful in other forms of acute mania. At all events, it was a means of restraining a furious maniac; as powerful, and, perhaps, it would be found far more safe than a strait-jacket or the grasp of a number of keepers. Once set a patient over (and that was the work of a minute), and a nurse with a pocket-handkerchief and some chloroform might keep him under perfect and complete restraint. Nor need he add how useful the same means might be in enabling a riotous and resisting patient to be removed to an asylum, or from one place to another. It would be tedious to discuss all the other diseases in which it had already been tried. Dr. S. was most anxious to observe its effects in acute local inflammations, but he had, as yet, few opportunities of doing so. He mentioned a case he had seen with Dr. Dease (apparently an extra-uterine conception bursting into the abdomen), and where the accompanying severe abdominal pain, and its

successfully the inhalation of ether to arrest the paroxysms of hooping-cough, dysmenorrhœa, colic, and the pains attendant on the passage of biliary calculi. In a case of the most severe, at the same time painful, spasmodic twisting and convulsions of the extremities attending a second attack of chorea, I allowed the patient ether-inhalation; and sometimes she lay under its influence for hours, with relief while its action lasted, but generally without sleep. Latterly the chloroform has both relieved the spasms and their attendant pain, and procured sleep. 2. As an *anodyne* or *narcotic*. In neuralgia, I have seen chloroform stop the fit at once; in two other cases the pain remained absent only while the chloroform acted. A patient suffering under severe delirium tremens had remained awake for about seventy hours; a half-ounce of laudanum, given at a single dose, failed to produce rest; ten hours afterwards, the inhalation of chloroform was immediately followed by several hours of critical sleep. What cases of insanity would it benefit? I have exhibited it in full doses in some cases of dementia, combined with excitement and wakefulness. They were all asleep in about a minute,—and remained so for some time. In nothing does chloroform differ from ether more than in its soporific effects—when given in full doses, and continued for some time. 3. In small doses as a *diffusible stimulant;* to arrest the first commencement of ague, ephemera, etc.; in hysteria, etc. Perhaps it may be used by inhalation in small quantities when the stomach will not bear wine or other stimulants; in severe vomiting, fevers, etc. I have seen its inhalation at once dispel a sick headache. 4. As a *contra-stimulant*[1] in inflammatory diseases, especially those of a painful character?? Acute rheumatism, etc.??

Cautions.—The liquid used should be sufficiently strong. Its proper sp. gr. is (as I have said) 1·480. I have seen some specimens perfectly unfit for use from their weakness; others perfectly unfit from their want of purity, for although of the proper specific gravity, they contained free chlorine, irritating the throat, and making the patient cough. It is certainly far too powerful an agent to be

dreadfully depressing effects, were kept at bay by the anæsthetic and contra-stimulant effects of the chloroform. He mentioned a case of extremely severe *cholera*, where, after all things had failed, and the patient was apparently sinking, the inhalation of chloroform had induced sleep—dispelled the spasms and vomiting—and restored the patient.—(From the Discussion at the Medico-Chirurgical Society of Edinburgh, on December 15, 1847. See *Monthly Journal of Medical Science* for January 1848.)

[1] See also p. 189.

intrusted to nurses or unprofessional individuals. I have given it, up to this date, to above eighty persons, without the slightest bad result of any kind whatever in any one of them. The power, however, which we have with it, of bringing down the pulse, etc., shows that, if exhibited in *too strong* a dose, given *uninterruptedly* for too great a length of time, it would doubtless produce serious consequences, and even death. But, certainly, all its *full* anæsthetic and other influences may be perfectly obtained without allowing it to produce such depression as would be in any degree dangerous. I have, in obstetric cases, kept patients under its influence for several consecutive hours, without any resulting harm of any kind to either mother or child,—renewing its application from time to time after the first full dose. Like many other agents, it may be powerful for evil as well as for good. I believe its great potency will be one great safeguard against its abuse.

Its influence upon the blood, etc., the counter-indications to its use, etc. etc., remain still to be ascertained.

CHAPTER III.

CHLORIDE OF HYDROCARBON, NITRATE OF ETHYLE, BENZIN, ALDEHYDE, AND BISULPHURET OF CARBON.

Edinburgh, April 1848.

During the last few months two or three different substances have been brought forward as anæsthetic agents; but our medical journals have afforded little or no detailed notice of their effects. The few following notes, however imperfect, may not therefore be uninteresting; more particularly as they are the result of direct experiments upon myself and others with the agents in question. In most of these experiments I had the kind and able assistance of Dr. Keith and Dr. Duncan.

When first publishing, in November 1847, upon the anæsthetic properties of chloroform, I stated that "in making a variety of experiments upon the inhalation of different volatile chemical liquids, I have, in addition to perchloride of formyle, breathed chloride of hydrocarbon, acetone, nitrate of oxide of ethyle, benzin, the vapour of iodoform, etc. I may probably," I added, "take another opportunity of describing the result."[1]

Three of the substances which I named in the preceding list, produce, when inhaled, a state of anæsthetic insensibility — viz. chloride of hydrocarbon, nitrate of oxide of ethyle, and benzin.

CHLORIDE OF HYDROCARBON.

Chloride of Hydrocarbon, or *Dutch liquid*, as it is often termed, in consequence of it being first discovered by the Dutch chemists of the last century, is one of the various fluids to which the name of *Chloric Ether* was for some time given.

When equal parts of olefiant gas and chlorine are mixed together, the two gases rapidly disappear, and produce a colourless oily liquid, of a peculiar sweetish taste and ethereal odour. Its specific gravity

[1] See *Lancet* for 20th November 1847, p. 549.

is 1·247. It boils at 148°. It is composed of four atoms of carbon, four of hydrogen, and two of chlorine. Hence its formula is $C_4H_4Cl_2$.[1]

When its vapour is inhaled, the chloride of hydrocarbon causes so great irritation of the throat, that few can persevere in breathing it for such a length of time as to induce anæsthesia. I have latterly, however, seen it inhaled perseveringly until this state, with all its usual phenomena, followed, and without excitement of the pulse, or subsequent headache. When I myself attempted to inhale the chloride of hydrocarbon, it produced an extreme degree of acrid irritation in the throat, which did not disappear entirely for many hours afterwards.

NITRATE OF ETHYLE.

When two parts of alcohol, and one part of pure nitric acid, are distilled together, with the addition of a small quantity of urea, *Nitrate of Ethyle*, or, more properly, *Nitrate of Oxide of Ethyle*, is produced. It is a transparent colourless liquid, with a sweet taste, and very agreeable odour. Its specific gravity is 1·112; it boils at 185°. It is a compound of four proportions of carbon, five of hydrogen, six of oxygen, and one of nitrogen; and its formula is (C_4H_5), O, NO_5, or AcO, NO_5.[2]

Nitrate of ethyle is easy and pleasant to inhale, and possesses very rapid and powerful anæsthetic properties. A small quantity, such as fifty or sixty drops, when sprinkled on a handkerchief and inhaled, produces insensibility after a few inspirations. But during the brief period which elapses before the state of complete anæsthesia is induced, the sensations of noise and fulness in the head are in general excessive; and much headache and giddiness have usually followed its employment, and persisted for some time.

BENZIN.

Benzin or *Benzole* was first discovered by Faraday, as one of the products in his experiments on compressing oil-gas, and was designated by him *Bicarburet of Hydrogen*. Mitscherlich afterwards obtained it by distilling, at a high temperature, benzoic acid with an excess of slaked lime.

It is a clear colourless liquid, of a peculiar ethereal odour; with

[1] $C_2H_4Cl_2$ by present notation. [Ed.]
[2] C_2H_5 O, NO_2 ,, ,, [Ed.]

a specific gravity of 0·85; and boils at 186°. Its formula is $C_{12}H_6$.[1]

In my experiments with benzin I found it capable of producing anæsthesia; but the ringing and noises in the head accompanying and following its inhalation were so excessive, and almost intolerable in the case of myself and others, as to seem to us to render its practical applications impossible, even had there been no other objections to its use. Latterly, Dr. Snow has tried its employment upon some patients for tooth-drawing; and in one instance of amputation. In this last case it produced convulsive tremors.[2]

ALDEHYDE.

Aldehyde, or *Hydrate of Oxide of Acetyle*, was first noticed by Dœbereiner in distilling together sulphuric acid, alcohol, and peroxide of manganese; but it was left for Liebig to fix and determine everything about its chemical nature. It is a colourless limpid liquid, of specific gravity 0·791. It is very volatile, boiling at 72°. It spontaneously changes when long kept, and is converted into two substances, a solid and a fluid, metaldehyde and elaldehyde. Liebig found it to be composed of four atoms of carbon, four atoms of hydrogen, and two of oxygen; and its formula is C_4H_2O.[3]

Professor Poggiale of Paris has lately made some experiments with dogs on the inhalation of the vapour of aldehyde, and from these has concluded that its anæsthetic effects will be found more prompt and energetic than those of sulphuric ether or chloroform. It certainly possesses, like some of the preceding agents, well-marked anæsthetic properties; but it assuredly will never come into use, as very few will be found capable of inhaling a sufficient dose of its vapour. In fact, out of five of us that attempted to inhale aldehyde, very carefully prepared and purified, four were driven to suspend the respiration of it in consequence of the coughing and insufferable feelings of dyspnœa which it immediately induced. The sensations of difficult respiration and constriction in the chest which the vapour produced, resembled precisely those of a severe fit of spasmodic asthma. In the fifth case, the experimentalist, after perseveringly breathing the aldehyde for a minute or two, became entirely insensible; the state of anæsthesia lasted for two or three minutes; during it, the pulse became excessively small and feeble. On recovering, the bronchial constriction and coughing, which had disappeared as the

[1] C_6H_6 by present notation. [Ed.] [2] *Lancet* for 12th February 1848, p. 130.
[3] C_2H_4O by present notation. [Ed.]

anæsthesia was induced, returned immediately, and were annoying for some time after.

BISULPHURET OF CARBON.

Bisulphuret of Carbon, or *Alcohol of Sulphur* (as it was at first termed), was accidentally discovered in 1796 by Lampadius, when experimenting on iron pyrites. Different opinions of its composition were held by different chemists; but Berzelius and Marcet, in 1813, at last fully confirmed the previous idea of Clement and Desormes, Vauquelin, etc., that it consisted only of sulphur and carbon. It is composed of two atoms of the former to one of the latter; consequently its formula is CS_2.

The most easy method of procuring it is by transmitting the vapour of sulphur over fragments of charcoal heated to redness in a closed porcelain or iron tube. The resulting bisulphuret of carbon, when purified by distillation, is a clear colourless liquid, of a pungent taste. Its specific gravity is 1·272. It is very volatile, boiling at 108°.

It has been stated in various literary journals, that bisulphuret of carbon has lately been used as an anæsthetic agent at Christiania; but no particulars regarding its employment in Norway have, so far as I know, been yet published.

I have breathed the vapour of bisulphuret of carbon, and exhibited it to about twenty other individuals, and it is certainly a very rapid and powerful anæsthetic. One or two stated that they found it even more pleasant than chloroform; but in several it produced depressing and disagreeable visions, and was followed for some hours by headache and giddiness, even when given only in small doses. In one instance I exhibited it, with Mr. Miller's permission, to a patient, from whom he removed a tumour of the mamma. It very speedily produced a full anæsthetic effect; but it was difficult to regulate it during the operation. The patient was restless in the latter part of it, but felt nothing. Like several others when under it, her eyes remained wide open. After the operation she was extremely sick, with much and long-continued headache; and, for fifty or sixty hours subsequently, her pulse was high and rapid, without rigor or symptoms of fever.

I tried its effects in a case of midwifery, in presence of Dr. Weir, Dr. Duncan, Mr. Norris, and a number of the pupils of the Maternity Hospital. It was employed at intervals during three-quarters of an

hour. The patient was easily brought under its influence, a few inspirations sufficing for that purpose; but it was found altogether impossible to produce by it the kind of continuous sleep attending the use of chloroform. Its action was so strong, that when given, as a pain threatened or commenced, it immediately affected the power of the uterine contractions, so as often to suspend them; and yet its effects were so transient that the state of anæsthesia had generally passed off within a minute or two afterwards. The patient anxiously asked for it at the commencement of each pain. During its use she was occasionally sick, and vomited several times. Latterly her respiration became rapid, and her pulse rose extremely high. I then changed the inhalation for chloroform, and, under it, the patient slept quietly on for twenty minutes, when the child was born. During these twenty minutes there was no more sickness or vomiting, and the pulse gradually sunk down to its natural standard. A few minutes after the child was expelled, and while the mother still slept, her pulse was counted at 80. Next day the mother and infant were both well, and she has made a good recovery.

While these experiments prove the strong anæsthetic properties of bisulphuret of carbon, they at the same time show its disadvantages. I have not alluded to another strong drawback upon its use, viz. its very unpleasant odour. "It has," says Dr. Gregory, "a peculiarly offensive smell of putrid cabbage."[1] By dissolving various essential oils in the bisulphuret, I tried to overcome this disagreeable defect, but without much success.

None of the five anæsthetics which I have mentioned in the present communication are, I believe, comparable with chloroform or sulphuric ether, either in their manageableness or in their effects. And the after-consequences which all of them tend to leave, are too severe and too frequent to admit of their introduction into practice. They are more interesting physiologically than therapeutically.

[1] *Outlines of Chemistry*, p. 130.

CHAPTER IV.

ANÆSTHETIC AND SEDATIVE PROPERTIES OF BICHLORIDE OF CARBON, OR CHLOROCARBON.

Edinburgh, December 1865.

LATELY I have inhaled and used a liquid the vapour of which seems to me to approach nearer in its quality and effects to chloroform than any other anæsthetic agent. The fluid I refer to is one of the chlorides of carbon.

In describing the products of the action of chlorine on one of the anæthetic fluids mentioned above—namely, chloride of hydrocarbon, or Dutch liquid—Mr. Fownes states (see his *Manual of Chemistry*, seventh edition, p. 445) that three or four chlorides of carbon can be artificially made from Dutch liquid by the abstraction of successive portions of hydrogen and its replacement by equivalent quantities of chlorine. He enumerates as belonging to this series—1, Sesquichloride or perchloride of carbon (C_4Cl_6); 2, Protochloride of carbon (C_4Cl_4); 3, Subchloride of carbon (C_4Cl_2); and 4, Bichloride of carbon (C_2Cl_4).[1]

The last of these compounds—the bichloride of carbon—is the new anæsthetic which forms the special subject of the present observations. It was first, I believe, discovered by M. Regnault, in 1839. It has already received various appellations from various chemists, as perchloroformene, perchlorinated chloride of methyl, dichloride of carbon, carbonic chloride, tetrachloride of carbon, superchloride of carbon, perchloruretted hydrochloric ether, and perchloruretted formene (see Gmelin's *Handbook of Chemistry*, vol. vii., p. 355, and Watt's *Dictionary of Chemistry*, vol. i. p. 765).

If it becomes, as I believe it will, for some medicinal purposes, an article of the Materia Medica, it will require to have a pharmaceutical name appended to it, and perhaps the designation of perchloroformene, or the shorter term chlorocarbon, may prove sufficiently distinctive. In its chemical constitution, bichloride of carbon, or chlorocarbon, is analogous to chloroform; with this difference, that

[1] 1, C_2Cl_6; 2, C_2Cl_4; 3, C_2Cl_2; 4, CCl_4, by present notation. [ED.]

the single atom of hydrogen existing in chloroform is replaced in chlorocarbon by an atom of chlorine, for the relative chemical constitution of these two bodies may be stated as follows:—

Chloroform = C HCl
Chlorocarbon = C_2ClCl_3 [1]

The chlorocarbon can be made from chloroform by the action of chlorine upon that liquid; and Geuther has shown that the process may be also reversed, and chloroform produced from chlorocarbon, by treating it in an appropriate vessel with zinc and dilute sulphuric acid, and thus exposing it to the action of nascent hydrogen. The most common way hitherto adopted of forming bichloride of carbon consists in passing the vapour of bisulphide or bisulphuret of carbon together with chlorine through a red-hot tube either made of porcelain or containing within it fragments of porcelain. There result from this process chloride of sulphur and bichloride of carbon, the latter being easily separated from the former by the action of potash.

The bichloride of carbon, or chlorocarbon, is a transparent, colourless fluid, having an ethereal and sweetish odour, not unlike chloroform. Its specific gravity is great, being as high as 1·56, whilst chloroform is 1·49. It boils at 170° Fahrenheit, the boiling point of chloroform being 141°. The density of its vapour is 5·33, that of chloroform being 4·2.

Besides trying the anæsthetic effects of bichloride of carbon upon myself and others, I have used it in one or two cases of midwifery and surgery. Its primary effects are very analogous to those of chloroform, but it takes a longer time to produce the same degree of anæsthesia, and generally a longer time to recover from it. Some experiments with it upon mice and rabbits have shown this—two corresponding animals in these experiments being simultaneously exposed, under exactly similar circumstances, to the same doses of chloroform and chlorocarbon. But the depressing influence of chlorocarbon upon the heart is greater than that of chloroform; and consequently, I believe it to be far more dangerous to employ as a general anæsthetic agent. In a case of midwifery in which it was exhibited by my friend and assistant, Dr. Black, and myself, for above an hour, with the usual anæsthetic effects, the pulse latterly became extremely feeble and weak. In another case in which it was exhibited by Dr. Black, the patient, who had taken chloroform

[1] Chloroform $CHCl_3$; Chlorocarbon $CClCl_3$, by present notation. [Ed.]

several times before, was unaware that the new anæsthetic was different from the old; her pulse continued steady and firm, although she is the subject of valvular disease of the heart. The surgical operations in which I have used chlorocarbon have been, the closure of a vesico-vaginal fistula, the division of the cervix uteri, the enlargement of the orifice of the vagina, and the application of potassa fusa to a large flat nævus upon the chest of a young infant. In all of these cases it answered quite well as an anæsthetic. The child did not waken up for more than an hour and a half after the employment of the caustic, which was used so as to produce a large slough. Its pulse was rapid and weak during the greatest degree of anæsthetic sleep. One of the mice exposed to its influence, and which was removed from the tumbler where the experiment upon it was made as soon as the animal fell over, breathed imperfectly for some time after being laid on the table, and then died.

Chlorocarbon, when applied externally to the skin, acts much less as a stimulant and irritant than chloroform, and will hence, I believe, in all likelihood by found of use as a local anæsthetic in the composition of sedative liniments.

In two cases of severe hysteralgia I have injected air loaded with the vapours of chlorocarbon into the vagina. The simplest apparatus for this purpose consists of a common enema syringe, with the nozzle introduced into the vagina, and the other extremity of the apparatus placed an inch or more down into the interior of a four-ounce phial, containing a small quantity—as an ounce or so—of the fluid whose vapour it is wished to inject through the syringe. Both patients were at once temporarily relieved from the pain. The first patient told me her relief at the first application of the anæsthetic vapour was so long that she slept during the following night far more soundly than she had done for weeks previously.

The injection of the vapour of chlorocarbon into the rectum does not prove so irritating as the vapour of chloroform. In one case it removed speedily pains in the abdomen and back.

Chloroform vapour applied by sprinkling a few drops on the hand, and held near the eye, is one of the very best and most sedative collyria in some forms of conjunctivitis, ulcerations of the cornea, with photophobia, etc. I have not yet tried the vapour of chlorocarbon, but perhaps it may answer still better, as less irritant, and almost as strongly sedative.

I have found ten or twenty drops injected subcutaneously by Dr. Wood's syringe repeatedly relieve local pains of the walls of the

chest, abdomen, etc., without being followed by the distressing nausea so frequently the result of the hypodermic injection of preparations of opium and morphia.

Internally I have only hitherto tried it in small doses in gastrodynia, where it has the same effect as swallowing a capsule of chloroform.

The specimen of chlorocarbon which I have used was made by Mr. Ransford, who sent it down to Messrs. Duncan, Flockhart, and Company, of Edinburgh, under the idea that, by a chemical substitution, it might be converted into chloroform, and make a cheap medium for the manufacture of the latter drug. And perhaps I may be permitted here to remark that the quantity of chloroform used is now becoming very great, and possibly might be rendered greater if it could be produced at a still cheaper rate. We have two or three manufactories for chloroform in this city. The chief of these manufactories for it—that of Messrs. Duncan, Flockhart, and Company—now make upwards of 7000 doses of chloroform every day, counting two drachms as a full dose; they thus send out nearly 2,500,000 doses a-year. Are every two million and a-half full doses which are used of opium, antimony, aloes, Epsom salts, etc., attended with as little danger and as few ultimate deaths as these annual 2,500,000 doses of chloroform?

PART IV.

APPLICATIONS OF ANÆSTHESIA IN SURGERY AND MEDICINE.

CHAPTER I.

CONDITIONS FOR INSURING SUCCESSFUL ANÆSTHESIA IN SURGERY, CAUTIONS, ETC.

Edinburgh, September 1847.

To produce the full and perfect effects of etherisation, it is necessary to conduct the process in conformity with certain conditions. These conditions it is not the object of the present communication to consider. But I will take the liberty of mentioning two or three leading points, the importance of which, in relation to the attainment of complete success, has become strongly impressed upon my own mind by a somewhat extensive experience in etherisation during the last few months.

First, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherisation, and during his recovery from it.[1] All talking and all questioning should be strictly prohibited.

[1] The area of an hospital operation theatre is hence, perhaps, not the most favourable place for securing all the advantages of etherisation, or rather for shunning all its disadvantages. Lately, in a case in which Professor Miller performed partial amputation of the foot, in the Royal Infirmary, I etherised the boy who was the subject of it, in his bed in the wards. He was carried in this state up stairs to the operating theatre—the amputation performed—and the patient brought back again to his bed before he was allowed to awake. He was thus, at one and at the same time, entirely spared the moral shock and pain of being transported and carried in before a formidable collection of surgeons and students, and saved from the physical sufferings attendant upon the amputation itself; for he was perfectly unconscious of aught that had occurred, and, when he awoke, he was not aware that he had been operated upon, or had even left his bed. While being carried from the ward to the operating room, the sponge with which he was etherised was kept fixed over his face with a couple of common elastic letter bands. In our surgical hospitals, if a ward immediately adjoining the operating theatre were set aside for operation cases, it would in this way facilitate the process of etherisation, and insure more certain and perfect results from it.

In this way any tendency to excitement is eschewed, and the proper effect of the ether-inhalation more speedily and certainly induced. And, *secondly*, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapour as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect. Latterly, I have found that for surgical purposes, and when it is not necessary to keep up the etherisation above five or ten minutes, by far the best and most perfect inhaler is formed by a large sponge of the common hollow conical shape, perforated artificially with a pretty large aperture at the apex, and placed over the face like a mask, so as to include both the mouth and nose in its concave base. At first, it should be held at a little distance from the face, and afterwards gradually advanced to it, in order to avoid exhibiting the vapour in too powerful and irritating a form for the first few inspirations. Its interior should, immediately before using it, be fully and freely saturated with ether—a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotising dose of the vapour.[1] Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule; not the principle of etherisation, but the mode of putting it in practice being altogether to blame. But, *thirdly*, whatever means or mode of etherisation is adopted, the most important of the conditions required for procuring a satisfactory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife until the patient is under the full influence of the ether-vapour, and thoroughly and indubitably soporised by it.

In the operating theatres of the Paris hospitals, the most triumph-

[1] When a prolonged effect is required, as in midwifery cases, an instrument is necessary—were it for no other reason than the saving of ether, and the prevention of its diffusion through the apartment. Within the last few days I have seen a pamphlet dated Boston, May 30, 1847, in which it is stated that for three months previously, all apparatus had been laid aside, and the sponge alone used for etherisation, by Dr. Morton of that city—the gentleman to whom, I believe, the profession and mankind are really and truly indebted for first introducing into practice the production of insensibility, by ether-inhalation, with the object of annihilating pain in surgical operations.—See *Some Account of the Letheon*, by Edward Warren, p. 87.

ant successes, in the original trials with ether, were obtained by M. Velpeau, who differed from his fellows in one all-important particular only—namely, in the forbearance with which he waited for the complete insensibility of his patients before venturing to take his operating knife into his hand. Few men have had more practice in etherising than Dr. Snow of London, who has been in the habit, for some time past, of thus assisting in their operations some of the first surgeons in the metropolis. Speaking of the stage of etherisation required, he draws a proper distinction between two degrees of this state—the first, and slighter, in which the patient moves, and winces, and seems to feel pain at the moment, but without afterwards remembering it; and the second and deeper state in which there is no evidence whatever of pain being felt, far less remembered. And he adds, "In full four-fifths of the cases in which he had administered the ether, there was not the least flinch or groan during the cutting of the surgeon's knife. He considered cases of this kind the only truly successful ones, and believed that with proper care every case might be of this nature. When the patient exhibited signs of pain, although he might have no knowledge or recollection of it afterwards, the ether was only partially successful."[1] As a proof that such deep states of etherisation are not accompanied with danger, I may mention here, though in the way of anticipation, that out of 39 surgical operations, "nearly all serious ones," in which Dr. Snow has exhibited ether at St. George's Hospital, 2 only of the patients died—namely, 2 on whom amputation of the thigh was performed, after they had been previously reduced to an "extremely weak and emaciated"[2] state. Now, 2 deaths in 39 hospital operations, or 1 in 18, would certainly be regarded as a very satisfactory and favourable result under almost any circumstances, and either with or without ether. Dr. Peacock, in his official reports of the Edinburgh Infirmary for 1842 and 1843, has published[3] two tables showing the results of the "principal operations" in that institution, from amputation and lithotomy down to the operation for harelip. The tables include 150 cases in all; and 32 of the 150 patients operated upon died, or about 1 in every 5. Excluding 57 cases of

[1] *Medical Gazette*, February 26, 1847; and Braithwaite's *Retrospect of Medicine*, vol. xv. 1847, p. 409. [2] *Lancet* for May 29, 1847, p. 553.

[3] *Statistical Tables of the Royal Infirmary of Edinburgh for* 1842, p. xix.; and for 1843, p. xviii. In the reports of other years the operations are unfortunately not tabulated on the same plan, and do not show the mortality dependent upon them.

"extirpation of tumours in various parts," 32 in the 93 individuals on whom other operations were performed, or nearly 1 in every 3 died.

Edinburgh, 15th December 1847.

To produce the complete anæsthetic and soporific effects of the chloroform, some conditions are necessary to be attended to. Without attending to these conditions, you will have failures. 1. The chloroform vapour must always be exhibited as rapidly, and in as full strength as possible, if you desire to have its first or exhilarating stage practically done away with, and excluded; and you effect this by giving the vapour so powerfully and speedily as to apathise the patient at once. If you act otherwise, and give it in small or slow doses, you excite and rouse the patient in the same way as if nitrous oxide gas were exhibited. 2. In order that the patient be thus brought as speedily as possible under its full influence, the vapour should be allowed to pass into the air-tubes by both the mouth and nostrils—and hence all compression of the nostrils, etc., is to be avoided. 3. The vapour of chloroform is about four times heavier than atmospheric air. And hence, if the patient is placed on his back during its exhibition, it will, by its mere gravitation, force itself in larger quantities into the air-passages than if he were erect or seated. As to the best instrument for exhibiting the chloroform with these indications, the simple handkerchief is far preferable to every means yet adopted. It is infinitely preferable to any instrument I have yet seen, some of which merely exhibit it by the mouth, and not by the nostrils, in small and imperfect, instead of full and complete doses; and with instruments so constructed, there is no doubt whatever that failures and exciting effects would ever and anon occur. Besides, inhaling instruments frighten patients, whilst the handkerchief does not; and mental excitement of all kinds, from whispering and talking around the patient, is to be strictly avoided, if possible. As to the quantity required to be applied to the handkerchief, it has been stated, that the average dose of a fluid drachm was generally sufficient to affect an adult; but I have latterly seldom measured the quantity used. We must judge by its *effects*, more than its quantity. The operator, gathering his handkerchief into a cup-like shape in his hand, should wet *freely* the bottom of the cup (so to speak), and if the patient is not affected in a minute or so, he should add a little more. It evaporates rapidly; and you must not wet your handkerchief, and then delay for a minute or more in applying it. It must be applied imme-

diately. Not unfrequently, when the patient is just becoming insensible, he will withdraw his face, or forcibly push aside the handkerchief. If you *then* fail to re-apply it to his face, and keep it there, you will be liable to leave him merely excited. But probably two or three inhalations more will *now* render him quite insensible. The simplest test of its full and perfect effect, is some noise or stertor in the respiration. Cease it as soon as this is fully set in. But re-apply it, of course, from time to time, if it is wished to keep up its effects.

Dr. Bennett has spoken of the stertor, or some other symptom being "serious." Now, this and other terms are, it is believed, calculated to excite unnecessary fear. "Serious" was a relative and conventional term, constantly liable to be altered by increased knowledge and experience. Twenty years ago, travelling at the rate of thirty miles an hour would have been reckoned a very serious matter. Now-a-days every one knew it was not so. The tyro looks at first upon the symptoms of an aggravated attack of hysteria as very serious. The physician of more experience knows they are not so. The stertorous breathing, the spasms, and almost convulsive symptoms, etc., sometimes produced by chloroform, may appear serious to those who have had little experience in the use of this agent. But every one who has seen much of its effects, knows that there is no danger following, nor is there inconvenience left, after such a show of serious symptoms.

The strength and purity of the chloroform employed are essential elements of success. Professor Gregory has examined about a dozen specimens which he had procured from various shops, here and in Glasgow. Several of them were by no means of the standard strength. A medical friend two days ago asked me if I had ever failed obtaining the effects of chloroform upon any person. I replied, never in any one case. My friend added that he had, the night previously, been unable to influence a parturient patient, though he had given her an ounce. On inquiry, I found he had used chloroform from a shop where, according to Dr. Gregory's researches, it was sold under a specific gravity of 1·200, instead of being 1·480.

Edinburgh, February 1856.

During the last few months, since an untaxed "methylated spirit" has come to be employed for pharmaceutical purposes, chloroform has been extensively manufactured from methylated instead of common spirit; and apparently of equally good quality. In conse-

quence of the comparative cheapness of methylated spirit, chloroform is thus further reduced about one-third in price. See its price in 1852 (p. 146); and the remark or prediction (p. 159) made, at its first introduction into practice, in 1848, when chloroform cost four shillings per ounce.

Instead of using a folded towel or handkerchief, or still more complex instrument for exhibiting chloroform, it will be found, that when its application is required in small and repeated quantities, the concavity of the hand of the patient or attendant is generally the simplest and safest, and certainly the readiest and the least formidable instrument. When inhaling it, for example, to allay coughs, bronchitis, pneumonia, laryngismus, fits of pertussis, etc., it is sufficient, in many cases, to contract the semiflexed hand into a cup form—to pour a few drops of chloroform into the concavity of the palm — and immediately hold the hand thus prepared over the mouth, the patient at the same time breathing fully and deeply. The heat of the human hand speedily evaporates the liquid. In the same way, in irritable and scrofulous ophthalmia, etc., holding over the affected eye the concave hand, wetted with a few drops of chloroform, forms often the best collyrium to the diseased organ, and is a practice which in general speedily removes the distressing supersensibility to light attendant upon some forms of ulcerative corneitis, etc. The adoption of the same simple means in painful fissures and ulcers of the nipple, in irritable cutaneous ulcers and eruptions, etc., is too evident to require specification.

Edinburgh, 14*th November* 1860.

For some time past I have administered chloroform in a manner somewhat different from that in which it was formerly used; and I believe that by the new method the patient is more rapidly anæsthetised, whilst a great saving is effected in the amount of the drug employed. The difference of the two modes consists in this, that according to the old plan the fluid is poured upon a cloth folded into several layers, and the hand of the administrator has to be kept between the cloth and the patient's face in order to secure the due access and admixture of air; while in following out the new method, *one* single layer of a towel or handkerchief is laid over the patient's nose and mouth, care being taken not to cover the eyes, and on this single fold the chloroform is poured, drop by drop, until complete anæsthesia is induced. There is little or none of the drug lost by evaporation when it is administered in this manner, for the patient

inhales it at the moment when it is poured on the cloth, and inhales it mixed with a sufficient quantity of air, which is easily inspired through a single layer of an ordinary napkin. Dr. Moir has long been in the habit of administering chloroform in this way, and I believe that this manner of using chloroform will add to the safety of its employment. I have often feared lest the lives of patients should be sacrificed by the careless manner in which, in particular, students and young practitioners sometimes employ the damp folded cloth over the patient's face without admitting a sufficient supply of air; and no doubt many of the deaths attributed to chloroform are due only to the improper administration of it, and are consequently no more chargeable on the drug itself than are the many deaths resulting from overdoses of opium, etc. etc. But the dangers from carelessness and improper administration would be diminished were there never placed over the patient's nose and mouth more than one single layer of cloth, moistened with a few drops of fluid. The first patient to whom I administered it in this manner had been chloroformed several times previously, and had never gone to sleep till an ounce and a half or two ounces of the fluid had been used; but when administered drop by drop on a single layer of a thin towel one drachm sufficed to induce the most profound sleep. It has thus all the advantages that have ever been claimed for the complicated apparatus which some medical men are still in the habit of using. There is only one precaution to be attended to in employing chloroform in this manner, viz.—care must be taken to anoint the lips and nose of the patient beforehand with oil or ointment, to prevent the skin being injured by the contact of the fluid with the patient's face, resulting from the close application of the wetted towel.

Edinburgh, 1855.

In the way of caution in the employment of chloroform, the points that demand the principal attention are the following:—1. The drug employed should be as pure as possible, and free from those various deleterious ingredients that are sometimes found mixed up with it, and which are liable to produce cough, headache, etc. 2. In its exhibition there ought always to be allowed to enter with the vapour of chloroform a free intermixture of atmospheric air, the fingers of the exhibitor being for this purpose always kept placed at one side between the face of the patient and the chloroformed towel or handkerchief; and 3. Its action should always be suspended, and the handkerchief or instrument containing it instantly

removed, whenever snoring and stertor supervene in the respiration, or when the pulse becomes languid, and falls much below the natural standard; or when the face and lips greatly alter in their colour either to pallor or lividity.

When in any case too powerful and large a dose of chloroform is given, the means of recovery which ought to be pursued are chiefly the following:—1. The instant removal of the chloroform handkerchief or instrument, and of everything containing the liquid, from the neighbourhood of the patient. 2. The supine position. 3. The free access of pure air to his face. 4. If necessary, the performance and continuance of artificial respiration by alternate compression and relaxation of the walls of the chest, or other means, taking special care at the same time to pull forward the tongue in the first instance, provided it has fallen backwards on the top of the windpipe. Some authorities have recommended the use of galvanism if an apparatus be at hand, the inhalation of oxygen or ammonia, inversion of the body, etc. No liquid should be poured into the mouth of the patient till he is able to swallow.

It is a very important question whether all should be subjected to the influence of anæsthetics, or whether there are any diseased states of the system which contra-indicate the employment of those agents.

The following are the chief diseases which have been alleged to afford instances of such.

Among pulmonary diseases phthisis with hæmoptysis was thought at one time to forbid the employment of anæsthetics, but it has since been proved, that, so far from being injurious, chloroform is highly beneficial, and is now used pretty extensively to allay the distressing cough which accompanies that fatal disease.

Pneumonia was also held to forbid the induction of anæsthesia; nay, it was even supposed that chloroform had often the effect of producing pneumonia. But now, on the contrary, that agent is used to a great extent on the Continent in the treatment of that affection, forming often the only remedy employed.

Among nervous diseases epilepsy may be mentioned as, according to some, contra-indicating the exhibition of chloroform.

Most practitioners were in the habit of withholding that drug if a patient was affected with epilepsy; and many even still do so. It is maintained by some that chloroform is most valuable in helping

us to diagnose between true and feigned epilepsy, as in the first it invariably causes a fit, while in the latter it merely produces its usual effects. Dr. Snow tells us, however, that this is an incorrect statement, for though it does very frequently bring on a fit, in some cases of undoubted epileptic patients chloroform may be pushed to its highest effects without the occurrence of one. Even though an attack be induced, it will be much less severe than usual, and will soon cease if the inhalation be gone on with, so that this affection is no true contra-indication.

Hysteria is situated very like epilepsy, because, though chloroform may bring on a paroxysm, that will soon subside if the inhalation be continued.

In photophobia arising from scrofulous ulcerations, and also in some cases of conjunctivitis where the little patient is quite unable to turn his eyes to the light, if a few drops of chloroform be placed on the hand and held close to the eye, the vapour will act as a sedative, and the child will be able to open its eyes, so that a proper examination can be made. I have even in some cases been successful in curing the disease by repeating the application of the vapour several times during the day.

Among cardiac diseases pericarditic adhesions do not, as once supposed, prohibit the employment of anæsthesia.

Valvular diseases are those which are most generally considered and insisted on as contra-indicating the use of chloroform. But this is really not the case, for that agent may be employed without the least danger in any valvular disease, except perhaps disease of the mitral valve. This is the only affection where I have the least hesitation in administering chloroform. There is perhaps no necessity for this dread after all. Dr. Snow thinks that there is no single disease that contra-indicates the employment of anæsthetics, and concludes "that this agent [chloroform] when carefully administered, causes less disturbance of the heart and circulation, than does severe pain."

CHAPTER II.

CASES ILLUSTRATIVE OF THE USE AND EFFECTS OF CHLOROFORM IN SURGERY.

Edinburgh, November 1847.

I HAVE, through the great kindness of Professor Miller and Dr. Duncan, had an opportunity of trying the effects of the inhalation of chloroform, in three cases of operation in the Royal Infirmary of Edinburgh. A great collection of professional gentlemen and students witnessed the results, and among the number was Professor Dumas of Paris, the chemist who first ascertained and established the chemical composition of chloroform. He happened to be passing through Edinburgh, engaged, along with Dr. Milne Edwards who accompanied him, in an official investigation for the French government, and was in no small degree rejoiced to witness the wonderful physiological effects of a substance with whose chemical history his own name was so intimately connected.

I append notes obligingly furnished to me by Professor Miller and Dr. Duncan, of the three cases of operation. The first two cases were operated on by Professor Miller; the third by Dr. Duncan. In applying the chloroform in the first case, I used a pocket-handkerchief as the inhaling instrument; in the last two I employed a hollow sponge.

CASE I.—"A boy, four or five years old, with necrosis of one of the bones of the forearm. Could speak nothing but Gaelic. No means, consequently, of explaining to him what he was required to do. On holding a handkerchief, on which some chloroform had been sprinkled, to his face, he became frightened, and wrestled to be away. He was held gently, however, by Dr. Simpson, and obliged to inhale. After a few inspirations he ceased to cry or move, and fell into a sound snoring sleep. A deep incision was now made down to the diseased bone, and, by the use of the forceps, nearly the whole of the radius, in the state of sequestrum, was extracted. During this operation, and the subsequent examination of the wound by the finger, not the slightest evidence of the suffering of pain was given. He still slept on soundly, and was carried back to his ward in that state. Half-an-hour afterwards, he was found in bed, like a child newly awakened from a refreshing sleep, with a clear merry eye, and placid expression of countenance, wholly unlike what is found to obtain after ordinary etherisation.

On being questioned by a Gaelic interpreter, who was found among the students, he stated that he had never felt any pain, and that he felt none now. On being shown his wounded arm, he looked much surprised, but neither cried nor expressed the slightest alarm."

CASE II.—"A soldier who had an opening in the cheek—the result of exfoliation of the jaw—was next made to inhale. At first he showed some signs of moving his hands too freely; but soon also fell into a state of sleep and snoring. A free incision was made across the lower jaw, and from this the dense adhering integuments were freely dissected all round, so as to raise the soft parts of the cheek. The edges of the opening were then made raw, and the whole line of incision was brought together by several points of suture. This patient had previously undergone two minor operations of a somewhat similar kind; both of them had proved unsuccessful, and he bore them very ill—proving unusually unsteady, and complaining bitterly of severe pain. On the present occasion, he did not wince or moan in the slightest degree; and, on the return of consciousness, said that he had felt nothing. His first act, when apparently about half-awake, was suddenly to clutch up the sponge with which the chloroform was used, and re-adjust it to his mouth, obviously implying that he had found the inhalation from it anything but a disagreeable duty.

"This case was further interesting as being one of those operations in the region of the mouth, in which it has been deemed impossible to use ether—and certainly it would have been impossible to have performed the operation with any complicated inhaling apparatus applied to the mouth of the patient."

CASE III.—"A young man, of about twenty-two years of age, having necrosis of the first phalanx of the great toe, and ulceration of the integuments, the consequence of injury. The ulcerated surface was exceedingly tender to the touch—so much so, that he winced whenever the finger was brought near to it; and the slightest pressure made him cry out. After the removal of the dressings, which caused some pain and fretting, the inhalation was commenced, and the patient almost immediately[1] became insensible, and lay perfectly still, while the diseased mass was being removed by amputation of the toe through the middle of the second phalanx. The inhalation was now stopped. The edges of the wound were then brought together with three stitches, and the wound dressed. The patient shortly afterwards awoke, looked round him, and gratefully declared his entire and perfect freedom from all pain and uneasiness during the operation."

The whole quantity of chloroform used in these three operations did not exceed half-an-ounce—and, as Professor Miller afterwards observed to the students that were present, if ether had been used, several ounces of it would have been requisite to produce the same amount of anæsthetic effect

The following case occurred also to-day, to Mr. Miller, in private practice. The notes of it and the subsequent remark are in his own words.

CASE IV.—"A young lady wished to have a tumour (encysted) dissected out rom beneath the angle of the jaw. The chloroform was used in small quantity

[1] Dr. Christison, who was watching the result, informs me that this patient was affected in half-a-minute.

(about a drachm) sprinkled upon a piece of operation sponge. In considerably less than a minute she was sound asleep, sitting easily in a chair, with her eyes shut, and with her ordinary expression of countenance. The tumour was extirpated, and a stitch inserted, without any pain having been either shown or felt. Her sensations throughout, as she subsequently stated, had been of the most pleasing nature; and her manageableness during the operation was as perfect as if she had been a wax doll or a lay figure."

"No sickness, vomiting, headache, salivation, uneasiness of chest, in any of the cases. Once or twice a tickling cough took place in the first breathings."

I have, up to this date, exhibited the chloroform to about fifty individuals. In not a single instance has the slightest bad result of any kind whatever occurred from its employment.

CHAPTER III.

CHLOROFORM IN INFANTILE CONVULSIONS, AND OTHER SPASMODIC DISEASES, IN PNEUMONIA, AND IN PERITONITIS.

Edinburgh, January 1852.

"THERE are," observes Dr. Churchill,[1] "few diseases of infants and children which are more formidable or more fatal than convulsions." The great number of deaths from convulsions, especially in infancy, which appears in all our published Mortality Returns, so far bears out the justness of Dr. Churchill's remark. During the five years from 1838 to 1842 included, there occurred, according to the Registrar-General's official returns, 127,276 deaths from convulsions in England and Wales.[2] Of these deaths, amounting to about 25,000 annually, almost all are among children below five years of age; and the greatest proportion of cases and deaths takes place among infants during the first year, or rather during the first months or weeks of life.[3]

Without entering into the question of the nature of the different types or forms of convulsions observable in early life, I shall content myself, at present, with referring to the general opinion of pathologists, that by far the greatest proportion of infantile convulsive attacks are sympathetic or functional merely;—a predisposition to the disease being laid by an undue excitability, or super-polarity of the cerebro-spinal, or rather "true spinal" or reflex system of Dr. Marshall Hall; and the immediate exciting cause of the affection being usually traceable to some morbific irritation of a distant excitant surface or part, as the stomach, bowels, teeth, etc. Hence when the disease proves fatal under this form, no organic lesions are

[1] *Diseases of Children*, p. 97.

[2] *Seventh Annual Report of the Registrar-General*, p. 63.

[3] "The frequency of convulsions has, in my practice, appeared most considerable in the first month of life; from this period the disease becomes gradually rarer up to the fifth month, and then again more common up to the period when the incisor teeth make their appearance. After this age, the disease again becomes rare."—See observations of Dr. Schœpf Merei, formerly Professor of the Diseases of Children in the University of Pesth, in *Monthly Journal* for 1850, p. 566.

usually detected. "Dissections," says Dr. Merei, "have incontestably established, that in the great majority of cases of infantile convulsions terminating fatally, there is no cerebral or spinal inflammation, nor even evidence of active vascular congestion."[1]

Consequently, in cases of infantile convulsions, particularly when of a sympathetic, reflex, or eccentric type, after removing all the traceable exciting sources of irritation, and diminishing any excess of vascular action in the nervous centres, physicians have generally proceeded to combat the disease, if it still persisted, with medicinal agents that tended to reduce the super-irritability of the excito-motory system, or otherwise to restore it to its proper and healthy standard of action. To fulfil this indication, preparations of zinc, iron, etc., have been used in the more chronic cases; and in the more acute or sub-acute cases, antispasmodics of very different kinds, as opium, hyoscyamus, musk, etc., have been generally employed. In the following instance, after all the ordinary means of treatment failed, chloroform was used as an antispasmodic with the most marked and satisfactory effect.

Case.—The Viscountess ——— was confined on the 7th October. The child, a boy, kept quite well till the 17th of the same month, when it was observed by its nurse, two or three times during the day, to have twitchings in the muscles of the face; but they were not so severe as to attract any very special attention. During the two following days these convulsive twitchings were repeated with rather greater frequency; the hands were observed to be clenched during them, and the thumbs were turned inwards.

On Monday the 20th, the convulsions became far more violent in their character, were more prolonged in their duration, and were repeated with much greater frequency. They continued with little change, and no abatement in their intensity or frequency, for the next fourteen days. Sometimes they affected the right side of the body much more severely than the left. In the meantime, Dr. Scott and I tried a great variety of means for their relief; but all in vain. The bowels were well acted upon with mercurials, magnesia, etc.; and every separate function attempted to be brought as near as possible to the standard of health. A new wet nurse was procured, lest the milk might perchance have been proving, as it sometimes does, the source of irritation. The child was placed in a larger and better ventilated room. Ice and iced-water were occasionally applied to the scalp. At one time, when the fits became unusually prolonged, and were not only accompanied, but followed for a time, by much congestion in the vessels of the scalp and face, and an elevated state of the anterior fontanelle, two leeches were applied. Liniments of different kinds were used along the spine. Musk, with alkalies, was given perseveringly for several days, as an antispasmodic; and small doses of opium, turpentine enemata, etc., were exhibited with the same view.

[1] *Monthly Journal* for 1850, p. 566. See also Rilliet et Barthez's excellent work (*Maladies des Enfans*), vol. ii. p. 281; North's *Practical Observations on Convulsions of Infants*, p. 45; Bouchut's *Manuel Pratique des Maladies des Nouveaux-Nés*, p. 387; etc. etc.

All these and other means, however, proved entirely futile. As I have already stated, it was on Monday the 20th October that the fits first assumed a severe character, and they continued without any amelioration for about fourteen days from that period, recurring sometimes as frequently as ten or twelve times in an hour. At last the child, who had hitherto wonderfully maintained his strength and power of suction, began to show symptoms of debility and sinking; and during the fifteenth and sixteenth days of the attack the fits became still more violent, and more distressing in their character. They were now accompanied with moans and screams that were very painful to listen to; symptoms of laryngismus and dyspnœa supervened towards the termination of each fit; and in the intervals the respiration, as well as the pulse, continued much quickened.

During these two last days of the disease, the exhaustion became so great, the dyspnœa in the intervals so distressing, and the fits so very violent and constant (seventeen being counted in one hour), that Dr. Scott and I gave up all hopes of the possible survival of the infant. We had exhausted all the usual means of relief. Ultimately, but much more with the view of abating the screaming, laryngismus, and other distressing symptoms under which the little patient was suffering, than with any great hope of permanent relief and cure, I placed the child, on the forenoon of the 5th November, for about an hour under the influence of the inhalation of chloroform. During this hour there was no recurrence of the fits; but in a short time after the withdrawal of the action of the anæsthetic, the convulsions recommenced with their old violence and frequency. The benefit, however, was sufficient to encourage a longer repetition of the remedy; and from four to eight o'clock in the afternoon of the same day, my assistant, Dr. Drummond, placed and kept the child again under the influence of chloroform—a few inhalations, from time to time, of a very small quantity of the drug sprinkled upon a handkerchief, and held before the face of the infant, being sufficient for this purpose. It was specially applied at any threatening of the recurrence of a fit, and during the four hours in question all convulsions were in this way repressed. When the child was allowed to waken up at eight o'clock, it took the breast greedily, and continued well for upwards of an hour, when the convulsions again began to recur. At last, about twelve o'clock P.M., it was again placed under the inhalation of chloroform, and kept more or less perfectly under its action for upwards of twenty-four continuous hours, with the exception of being allowed to awaken eight or ten times during that period for the purpose of suction and nourishment. During most of this period it was carefully watched by Dr. Drummond, and at last the nurse was entrusted with the duty of adding the few drops of chloroform to the handkerchief, and exhibiting them at any time the child was offering to awaken or become restless.

After this long continuation of the chloroform, the child, on being allowed to waken up, as usual drank greedily at the nipple, and immediately fell back into a quiet and apparently natural sleep. The chloroform and all other formal medication was in consequence discontinued; and from this time there was subsequently no recurrence whatever of the convulsions. In about ten days the child was removed with the family to the country. I have, within the last two days (December 18, 1851), seen the child as it was passing through Edinburgh. It was strong, plump, and well-grown for a child of ten weeks, and was, in fact, revelling in the best of health.

In exhibiting the chloroform to this infant, ten ounces of the drug were expended; but of course a very large proportion of this quantity was lost by evaporation, in consequence of the mode in which it was employed.

I have known the inhalation of chloroform similarly useful in other cases in arresting infantile convulsions; but I am not acquainted with any instance in which the patient was so young as in the above instance. In the adult also, especially in cases of puerperal convulsions, I have now repeatedly seen the inhalation of chloroform as signal and satisfactory in its antispasmodic power over the convulsive fits, as it was in the little patient whose case I have described. Tetanus and epilepsy have been temporarily arrested and controlled by it. And perhaps it will yet be found one of our most certain and beneficial therapeutic means in the functional forms of those different convulsive or spasmodic diseases that are produced either by an undue excitability of the true spinal system, or by distant morbid irritations acting through this—the excito-motory system. Such reflex convulsive or spasmodic affections are, as is well-known, particularly common in infancy and childhood. I have seen its use arrest laryngismus, colic, hiccup, etc.; and cases have been detailed to me of its occasional successful use in asthma, spasmodic urethral stricture, etc. But there is one common and too fatal spasmodic disease, almost confined to the period of childhood, in which I have seen anæsthetic inhalations successful in arresting and controlling the paroxysms, and where probably a more extended and persevering use in the employment of them would be found to be attended with beneficial effects. I allude to hooping-cough. I have known chloroform-inhalations greatly abate the irritability of the cough attendant upon phthisis, etc. But with others, I have scrupled to use chloroform-inhalations in hooping-cough, under the fear that they might possibly increase the great predisposition which exists in this affection to pneumonic inflammation, or aggravate that inflammation if it were already present. This *a priori* reason, however, against the use of chloroform-inhalations as an antispasmodic in hooping-cough, has been of late set aside by the observations and experience of different German physicians. In a paper, containing some remarks relative to the medical uses of chloroform, published December 1847,[1] in addition to its employment as an antispasmodic, anodyne, etc., I suggested the possibility of the drug acting as a contra-stimulant in some inflammatory diseases, and particularly in those of a painful kind. Latterly, we have had records published of its employment in upwards of 200 cases of pneumonia in German practice. Out of 193 cases of pneumonia treated with chloroform-inhalations by Wachern, Baumgärtner, Helbing, and Schmidt, 9 patients died, or

[1] See p. 163.

the mortality amounted to $4\frac{1}{4}$ per cent. Dr. Varrentrapp has given chloroform in 23 cases of pneumonia in the Frankfort Hospital. One of these 23 patients died.[1] The detailed results in the other 22 cases seem to have been sufficiently satisfactory.[2] At all events, the effects of the chloroform-inhalations upon the cough, expectoration, etc., and upon the general course of the disease, would appear to show that we need have no fear of deleterious effects from it, so far as regards the chance or existence of pulmonary inflammation; whatever advantages we may derive from it in relation to its prevention of that inflammatory state, by allaying the cough, keeping the lungs in a relative state of quietude, and abating or restraining the succession of characteristic spasmodic attacks. I speak, of course, of the more severe cases of pertussis; for the milder forms of it require care merely, rather than actual treatment.

Edinburgh, 29th July 1863.

Chloroform in Acute Inflammation.—Most of us have from time to time met with cases of peritonitis in which opium refused to sub-

[1] It is proper to add, that during the time that these 23 cases of pneumonia were admitted into the Frankfort Hospital, and treated in that institution by chloroform-inhalations, three other cases of the same disease presented themselves, where the patients, at the time of application, were already in a hopeless state. Chloroform was not tried with them.

[2] Out of these 23 cases of pneumonia reported by Varrentrapp, in *addition* to chloroform, the first was treated by venesection and antimony; a second case was bled; and two others, that were complicated with pleurisy, had calomel exhibited and blisters applied; the remainder were treated with chloroform alone, about sixty drops being placed upon a piece of cotton, the vapour inhaled for ten or fifteen minutes, and the dose repeated every two, three, or four hours. It was not given so rapidly or strongly as to produce unconsciousness. The patients were all adults; the mean period of the disease at their entrance into the hospital was the fourth day; and the chloroform-treatment was usually commenced on the following morning. The effects of the chloroform-inhalations seemed generally to be—1. The induction of perspiration, sometimes after the first inhalation, in no case later than the third or fourth. 2. Gradual diminution and ultimate disappearance of pain in the thorax or side. 3. Relief of the feeling of thoracic tightness. 4. Daily decrease of the frequency of respiration from thirty-seven per minute (the average on admission) down to the natural standard. 5. In all cases, without an exception, the cough was lessened by the inhalation, the intervals between the coughs shortening, the cough itself being less violent, and the expectoration looser; the sputa gradually losing their red tinge, and diminishing in quantity. 6. The pulse fell rapidly in frequency (down to eighty on an average on the fifth day of treatment), and the fever diminished gradually, in one case suddenly. 7. Good and comfortable sleep ensued on an average on the third or fourth day after the commencement of the chloroform-inhalations.—See Henle's *Zeitschrift für Rationelle Medicin*, and the *London Medical Times* for October 18, 1851.

due the symptoms, and was rejected, as well as the wine, brandy, and other medicines that were attempted to be administered. I have at present under my care the case of a lady who took an attack of peritonitis ten days ago. At first it seemed very slight; but on the fourth day she grew much worse, the pain becoming intense, and the pulse almost imperceptible. She took large and repeated doses of opium; but as the symptoms continued unabated, I began in the evening to make her inhale chloroform. It was with some difficulty that she was brought under its influence; but when she had once been fairly anæsthetised, the pulse gradually became stronger and steadier. All night she was kept asleep by this means, and on the following day the pulse was better, the pain easier, and the patient seemed comparatively out of danger. Until three days ago, she seemed to progress favourably, when symptoms of hepatitis showed themselves. These were subdued, and once more she began to recover. But the day before yesterday she again became collapsed. Chloroform was administered with the same salutary effect, and at the present time she is tolerably comfortable.

Chloroform is much used on the Continent as a remedial agent in pneumonia and some other acute inflammations; but as I am not aware of its having been used specially in peritonitis, the suggestion may prove helpful in some rare but very distressing cases of that disease.

PART V.

APPLICATIONS OF ANÆSTHESIA IN MIDWIFERY.

* * "Not poppy, nor mandragora,
Nor all the drowsy syrups of the world,
Shall ever medicine thee to such sweet sleep."

SHAKESPEARE.

CHAPTER I.

ON THE INHALATION OF SULPHURIC ETHER IN THE PRACTICE OF MIDWIFERY.

Edinburgh, March 1847.

ABUNDANT evidence has of late been adduced, and is daily accumulating, in proof of the inhalation of sulphuric ether being capable, in the generality of individuals, of producing a more or less perfect degree of insensibility to the pains of the most severe surgical operations. But whilst this agent has been used extensively,[1] and by numerous hands, in the practice of surgery, I am not aware that any one has hitherto ventured to test its applicability to the practice of midwifery. I am induced, therefore, to hope that the few following hurried and imperfect notes relative to its employment in obstetric cases, may not, at the present time, prove uninteresting to the profession.

Within the last month I have had opportunities of using the inhalation of ether in the operation of turning, in cases of the employment of the long and of the short forceps, as well as in several instances in which the labour was of a natural type, and consequently required no special form of artificial aid.

The first case in which I employed the ether vapour, occurred

[1] In a lecture delivered to the Royal College of Surgeons, Edinburgh, on 27th March 1868, Dr. James Simpson stated that Dr. Scott of Dumfries was the first in this country to make trial of sulphuric ether in surgery. [ED.]

on the 19th of January 1847.[1] The pelvis of the mother was greatly contracted in its conjugate diameter from the projection forwards and downwards of the promontory of the sacrum; the lumbar portion of the spine was distorted; and she walked very lamely. The present was her second confinement. Her first labour had been long and difficult; she began to suffer on a Monday, and after a protracted trial of the long forceps, was at last delivered by craniotomy late on the subsequent Thursday night. Even after the cranium had been fully broken down, a considerable time and much traction had been required to drag the diminished and mutilated head of the infant through the contracted brim of the pelvis; and she was long in recovering. Contrary to the urgent advice of her medical attendant, Mr. Figg, he was not made aware of her present or second pregnancy till she had arrived at nearly the end of the ninth month. It was thus too late to have recourse to the induction of premature labour, which had been strongly pressed upon her as the only means of saving her child, should she again fall in the family way. The pains of her second labour commenced in the forenoon of the 19th. I saw her, with Mr. Figg, at five o'clock in the afternoon, and again at seven. The os uteri was pretty well dilated, the liquor amnii not evacuated, the presenting head very high, mobile, and difficult to touch; and a pulsating loop of the umbilical cord was felt floating below it in the unruptured bag of membranes. From five to nine o'clock the pains seemed only to push the circle of the os uteri farther downwards, without increasing its dilatation, or making the head in any degree enter into the pelvic brim. Assisted by Dr. Zeigler, Dr. Keith, and Mr. Figg, I shortly after nine o'clock made the patient inhale the ether vapour. As she afterwards informed us, she almost immediately came under the anodyne influence of the ether; but in consequence of doubts upon this point, its use was continued for nearly twenty minutes before I proceeded to turn the infant, as I had previously predetermined to do. A knee was easily seized, and the child's extremities and trunk readily drawn down; but extreme exertion was required in order to extract the head. At length it passed the contracted brim, with the anterior part of its right parietal bone deeply indented by pressure against the projecting promontory of the sacrum, and the whole cranium flattened and compressed laterally. The infant gasped several times, but full respiration could not be established.

[1] See *Edin. Monthly Journal of Medical Science*, February 1847, p. 639, and communicated to Obstetric Society, Edinburgh, January 20, 1847.

The transverse or biparietal measurement of its head, at the site of the indentation, was, in its compressed state, not more than $2\frac{1}{2}$ inches. Hence we judged the conjugate diameter of the pelvic brim not to exceed this. The infant was large, and rather above the usual size. It weighed eight pounds. On afterwards examining the head and removing the scalp, no fracture could be found at the seat of indentation. The thin parietal bone had merely bent inwards.

On questioning the patient after her delivery, she declared that she was quite unconscious of pain during the whole period of the turning and extracting of the infant, or indeed from the first minute or two after she first commenced to breathe the ether. The inhalation was discontinued towards the latter part of the operation, and her first recollections on awaking were "hearing," but not "feeling," the head of the infant "jerk" from her (to use her own expressions), and subsequently she became more roused by the noise caused in the preparation of a bath for the child. She quickly regained full consciousness, and talked with gratitude and wonderment of her delivery, and her insensibility to the pains of it. Next day I found her very well in all respects. I looked in upon her on the 24th, the fifth day after delivery, and was astonished to find her up and dressed, and she informed me that on the previous day she had walked out of her room to visit her mother. Mr. Figg informs me that her further convalescence has been uninterruptedly good and rapid.

I have previously alluded to two cases of delivery by the forceps, in which the patients were under the action of ether at the time of the operation. The woman in the first of these cases was brought into the Royal Maternity Hospital, in strong labour, early on the morning of the 3d February. It was her second confinement. At her first accouchement, seven years before, she had been delivered by instruments, in Ireland, and had been informed by the attendant practitioner that artificial delivery would be similarly required at her future labours. I saw her between ten and eleven o'clock A.M. The os uteri was well dilated, the membranes ruptured, and the pains extremely strong and frequent; but the large head of the child seemed not to enter fully into the brim, and was little affected by the powerful uterine contractions under which the patient was suffering. By three o'clock her pulse had risen to above 125 beats a minute, and it appeared to the medical officers present that it would be improper to allow the ineffectual and exhausting efforts of the patient to be longer continued. She was then, at my request, brought under the influence of ether. Dr. Moir, with great

skill, applied the long forceps upon the head of the child. He subsequently was obliged to use strong traction during the pains that followed, and becoming temporarily fatigued with his efforts, I supplied his place. After the head fully passed the brim, the forceps were laid aside, and one or two uterine contractions finished the delivery. The child was large and strong, and cried vigorously soon after it was expelled. During the whole of this severe operation the patient appeared quiet and passive. The cries of her child speedily roused her from her anæsthetised state, and she subsequently assured Dr. Moir that she had felt comparatively little or no pain during the whole operation and delivery.

On the evening of the 12th February, I saw another forceps case, with my friend Dr. Graham Weir. The patient was advanced in life, and it was her first confinement. The waters had escaped early, and the anterior lip of the uterus had subsequently become forced down in a very swelled and œdematous state before the head of the infant. After this obstruction was overcome, the child's head speedily descended upon the floor of the pelvis; but it was there impeded in its farther progress by the narrow transverse diameter of the outlet. Under the compression of the converging tuberosities of the ischia, the bones of the fœtal cranium soon began to overlap; but at last, no farther progress being made, the patient becoming exhausted by a continuous labour of about twenty-four hours, and the soft parts being evidently well relaxed and prepared, Dr. Weir applied the short forceps, and extracted a living infant. For a considerable time before this operation was adopted, I exhibited the vapour of ether to the patient; under it she speedily became quite narcotised. Its action was kept up, and the pains appeared to be so strong as almost to warrant the idea that nature would yet be sufficient; but ultimately, instrumental delivery was, as I have already stated, had recourse to. The mother did not fully recover from her state of anæsthesia for ten or fifteen minutes after delivery, and then stated that she was quite unaware of anything that had been done, and of what had occurred. Dr. Weir informs me that this patient was up on the fourth day after delivery, and felt by that time so perfectly well, that she was with difficulty persuaded by the nurse to abstain from walking about the house as usual.

As far as they go, the preceding cases point out one important result:—in all of them, the uterine contractions continued as regular in their occurrence and duration after the state of anæsthesia had been induced, as before the inhalation was begun. The emotion of

fear has appeared to me to suspend, in one or two nervous patients, the recurrence of the first pains, after the apparatus was adjusted and its employment commenced, but this effect speedily passed off; and as yet I have seen no instance in which the pains were sensibly diminished in intensity or frequency after the ether had fairly begun to act. Indeed, in some cases they have appeared to me to have become increased as the consciousness of the patient became diminished. This has more particularly occurred with one or two patients, who breathed ether, combined with tincture of ergot, or containing a solution of its oil. A woman was brought into the Maternity Hospital on the 28th January, after being in labour for thirty or forty hours. It was her second child. Subsequently to her entering the hospital, at 7 P.M., scarcely any decided uterine contraction could be said to take place. The os uteri was well opened, but the head was still high in the pelvis; and when I saw her at 4 A.M. of the following morning, nine hours after her entrance into the hospital, little or no advance whatever had been made, and the case was becoming an anxious one. She was then made to inhale equal parts of sulphuric ether and tincture of ergot. In the course of a few minutes a series of extremely powerful uterine contractions supervened, and the child was born within a quarter of an hour of the commencement of inhalation. The mother subsequently declared that she recollected nothing at all of her delivery, except the removal of the after-birth. In this case, was the re-excitement of strong pains the result of the action of the sulphuric ether, or of the ergot, or of both? Or was it a simple but very strange coincidence? More facts than I yet possess are necessary to decide such a question; but I have seen some cases which lead me to believe that other therapeutic agents besides those I have named may be readily introduced into the system by means of pulmonary inhalation.[1]

[1] Dr. Richard Pearson, who, in 1795, was, I believe, the first person that recommended the inhalation of sulphuric ether as a therapeutic agent (see his *Account of the Nature and Properties of different kinds of Airs*, p. 24), suggested also the use of it impregnated with opium, squill, cicuta, etc.; and he speaks of the effect of "an emetic given in this manner." He employed the simple sulphuric ether vapour in some cases of phthisis, asthma, hooping-cough, croup, and catarrh, recommending it to be inhaled (after being rectified and washed), from a cup—through an inverted funnel—or, with children, by "wetting a handkerchief with it, and holding it near the nose and mouth." See *Medical Facts and Observations*, for 1797, vol. vii. p. 96. In the thirteenth volume of the *Dictionnaire des Sciences Médicales* (1816), p. 385, Nysten has described a particular apparatus, like some of our modern forms, for the inhalation of sulphuric ether. See also vol. xvii. p. 134. Vaporisable substances, when introduced into the system in

A more extensive and careful series of investigations than I have yet been able to institute, may perhaps show that in some constitutions, and under some circumstances or *degrees* of intensity, the process of etherisation may possibly interfere with the uterine contractility, particularly in the earlier stages of the labour.[1] At the same time, various analogies would lead us to expect that, as I have hitherto found, the action of the uterus would go on uninterruptedly, when the psychical influence of the mind and purely cerebral functions were suspended, as in the more complete states of anæsthesia. At all events, if we may judge from the analogous experiments of Vollkmann, Bidder, and Kölliker, upon the simple contractions and rhythmic reflex actions of the heart, intestines, etc., the motory nervous powers of the uterus belong to the ganglionic and to the spinal systems, and are not in any necessary dependence upon the brain or mind. Indeed, Ollivier and Nasse have published cases of perfect paraplegia, notwithstanding which the act of parturition in the human female proceeded regularly in its course, and without conscious pain. In the one case (Ollivier's), the cord was compressed and destroyed from the first to the fourth dorsal vertebra by a collection of acephalocysts;[2] and, in the other instance (Nasse's), complete paralysis had followed a fracture of the third and fourth cervical vertebræ.[3] Of course such lesions necessarily prevented the brain exerting any influence upon the uterus, or its contractions.

Long ago, in discussing this subject, Haller adduced the authority of Harvey, Smellie, Lamotte, etc., to prove that uterine contractions and labour may go on with the mother, "ignara, stupida et sopita, et immobili, et apoplectica, et epileptica, et convulsionibus agitata,[4] et ad summum debili."[5] Deneux mentions a fact still more in point,

this manner, probably pass undigested and unchanged into the circulation, and "seem (observes Wagner) to make their way into the blood through the unbroken vascular membrane [of the bronchial cells] with the *same* certainty and ease as when they are injected *directly* into the veins."—(*Elements of Physiology*, 1842, p. 443.) Will this not explain both the rapidity and intensity of their actions when thus used?

[1] See regarding this, p. 202, etc. [Ed.] [2] *Traite de la Moelle Epiniere*, p. 784.

[3] *Untersuchungen zur Physiologie*, etc. Dr. Cheyne reports a case of fatal hemorrhagic apoplexy and hemiplegia, in which, without any apparent pains, "the uterus (observes Dr. Kellie) appears as an involuntary muscle to have acted in the most perfect manner in expelling the fœtus and secundines," the day before death. The child was born alive.—*Cases of Apoplexy and Lethargy*, pp. 91 and 161.

[4] "During the continuance of puerperal convulsions, uterine action is *not* suspended, although no signs of pain are manifested by the woman, if she remain comatose."—Dr. F. Ramsbotham's *Obstetric Medicine* (1844), p. 455.

[5] *Elementa Physiologiæ*, tom. viii. p. 420.

because in it the analogy with the operation of anæsthetics is still stronger, or indeed identical. "A woman," says he,[1] "was brought to the Hôtel Dieu at Amiens in a comatose state, in consequence of her taking spirituous liquors since the commencement of labour. She was delivered in the natural manner in this state; the sleep continued for some time after delivery. The woman, on awaking, much surprised at finding her delivery completed, congratulated herself on having made so happy a discovery, and declared she would make use of it if she had again occasion."[2]

In obstetric, as in surgical practice, the degree of insensibility produced by anæsthesia, and its accompanying phenomena, differ much in different instances. In some, a state of total apathy and insensibility seems to be produced; others move about and complain more or less loudly during the uterine contractions, though afterwards, when restored to their state of common consciousness, they have no recollection of any suffering whatever, or, indeed, of anything that had occurred during the inhalation and action of the ether; others, again, remain quite aware and conscious of what is going on around them, and watch the recurrence of the uterine contractions, but feel indifferent to their effects, and not in any degree distressed by their presence; and in another class, again, the attendant suffering is merely more or less diminished and obtunded, without being perfectly cancelled and annulled.

On the evening of the 13th inst., in two cases that rapidly followed each other, I witnessed, in the above respect, two very different conditions induced by the use of the ether. The patients, each of whom had borne several children previously, were both placed under

[1] *Recueil Periodique de la Société de Médicine*, April 1818.

[2] The celebrated case of the Countess de St. Geran is sufficiently remarkable in relation to the present subject. See full and long details of it in Gayot's *Causes Célèbres*, tom. i. pp. 142 to 266. After the Countess had been nine hours in labour with her first child, the midwife in attendance exhibited to her a potion (*breuvage*), which rendered her insensible till the following morning. When the Countess then awoke to consciousness, she found herself bathed in blood, the abdominal tumour fallen, and all the signs of recent delivery present; but the child born during her state of insensibility had been removed, and its existence was even denied to her. It was years afterwards proved, to the satisfaction of the French law courts, that the Countess had been delivered of a male child, during an induced lethargic condition, and that the infant had been surreptitiously conveyed away to a distance, and brought up as the son of a poor man. The child's claims were, after much litigation, fully acknowledged; he was restored to his parents, and ultimately succeeded to his father's title. What nepenthean "breuvage" could possibly produce the alleged effect?

the influence of it just as the os uteri became fully opened, and in neither did the full expulsion of the infant through the pelvic passages require above twelve or fifteen minutes. My first patient, the wife of a clergyman, subsequently stated, that she knew all that was said and done about her, was aware of the pains being present, but felt no distress from any of them till the supervention of the last strong contraction, which drove the head out of the vulva, and the feeling then seemed to partake of the character of strong pressure, rather than of actual pain. Subsequently she told me she could only look back with regret to the apparently unnecessary suffering she had endured in the birth of her former infants. The second patient, a lady of a timid temperament, and very apprehensive about the result of her present confinement, was induced with difficulty to inhale the ether vapour; but it speedily affected her when once she did begin. In two or three minutes she pushed the apparatus from her mouth, talked excitedly to a female relative present, but was immediately induced to recommence the inhalation; and subsequently, according to her own statement, "wakened out of a dream, and unexpectedly found her child born." Like many others, she thought hours instead of minutes had elapsed from the commencement of the inhalation to the period of the complete restoration of consciousness. Making apparently an effort of memory, she afterwards inquired if she had not once awakened out of her dreamy state, and spoken some nonsense to her friend.

A careful collection of cautious and accurate observations will no doubt be required, before the inhalation of sulphuric ether is adopted to any great extent in the practice of midwifery. It will be necessary to ascertain its precise effects, both upon the action of the uterus, and of the assistant abdominal muscles; its influence, if any, upon the child; whether it gives a tendency to hemorrhage or other complications; the contra-indications peculiar to its use; the most certain modes of exhibiting it; the length of time it may be employed, etc.[1] In no case have I observed any harm whatever, to either mother or infant, follow upon its employment. And, on the other hand, I have the strongest assurance and conviction, that I have already seen no

[1] I have, during labour, kept patients under its influence for upwards of half-an-hour. In exhibiting it, the first, or exhilarating stage of its effects should be passed through as rapidly as possible, and the patient never allowed to be excited or irritated by the nurse or others. I have heard its use strenuously denounced, on the ground that its effects, though good and evanescent, are still of an intoxicating character. But on the same ground, the use of opium, etc. etc., in medicine, to relieve pain and procure sleep, should be equally reprobated and discarded.

small amount of maternal suffering and agony saved by its application. The cases I have detailed sufficiently show its value and safety in cases of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtlessly be. But in modifying and obliterating the state of conscious pain, the nervous *shock*,[1] otherwise liable to be produced by such pain—particularly whenever it is extreme, and intensely waited for and endured—is saved to the constitution, and thus an escape gained from many evil consequences that are too apt to follow in its train.[2]

Edinburgh, November 1847.

Perhaps I may be excused for adding, that since for the first time directing the attention of the medical profession to its great use and importance in natural and morbid parturition, I have employed it, with few and rare exceptions, in every case of labour that I have attended; and with the most delightful results. And I have no doubt whatever, that some years hence the practice will be general. Obstetricians may oppose it, but I believe our patients themselves will force the use of it upon the profession. I have never had the pleasure of watching over a series of better and more rapid recoveries; nor once witnessed any disagreeable result follow to either mother or child; whilst I have now seen an immense amount of maternal pain and agony saved by its employment. And I most conscientiously believe that the proud mission of the physician is distinctly twofold—namely, to alleviate human suffering, as well as preserve human life.

[1] On the extent of the nervous *shock* accompanying human parturition, see Dr. Hamilton's *Practical Observations*, p. 179, etc.; and Dr. Churchill's chapter on Convalescence after Labour, in his work on the *Diseases of Pregnancy and Childbed*, p. 240, etc.

[2] On what division or divisions of the nervous system does the nervous *shock* operate—the cerebral, spinal, or ganglionic? If on the former, it should be kept in abeyance by due anæsthesia. Some years ago, I saw Dr. J. Argyll Robertson, when he was Acting Surgeon at the Royal Infirmary, amputate, at the shoulder-joint, an arm sadly shattered an hour or so before by a railway injury. The man, at the time of receiving the injury, during the operation, and for several hours afterwards, was in a state of insensibility from deep intoxication; and at last wakened up, not knowing what had happened. His recovery was rapid and uninterrupted. Would it have been so if his nervous system had been sufficiently alive to the double shock of the operation and injury? Out of eighteen cases of primary amputation, performed during four years in the Edinburgh Hospital, and mentioned in Dr. Peacock's Report of the Institution (1843), this man and another patient were the only two out of the eighteen that survived.

CHAPTER II.

MODE OF EXHIBITING CHLOROFORM IN MIDWIFERY; DOSE, ETC.

Edinburgh, October 1848.

In the course of the preceding observations I have omitted making any remarks on the degree of artificial anæsthesia required in obstetric practice, with the exception of stating that when instrumental or operative interference is adopted, the anæsthetic state must be made adequately deep—so deep, that the patient must be rendered quite passive and apathetic. In fact, when induced for operative purposes in midwifery, the anæsthetic state should be as complete and profound as when it is induced for operative purposes in surgery. But, in common cases of parturition, the anæsthetic agent employed, whether chloroform or ether, does not, in general, require to be given in such large doses as in surgical practice. And in obstetric practice, the rules which I have usually followed in exhibiting the chloroform (the only agent, I believe, now used in Edinburgh and most other places), are those which I briefly stated when first writing on the subject in November last. "After the first full dose, a few inhalations, before or with each returning uterine contraction, are generally sufficient. The state of anæsthesia should be made more deep as the head is passing the perineum and vulva."[1]

Occasionally I have at first, and especially in the early stages of labour, given the chloroform in small doses only, so as to obtund or obliterate the sensations of pain, without altogether abrogating the state of consciousness. In many patients, this degree of anæsthesia, with the results stated—viz. the loss, in a great measure, of pain without the entire loss of consciousness—can be readily enough induced, and answers excellently well; but, as a general rule, it has appeared to me in some cases objectionable. For not unfrequently, small doses, such as produce this condition, are accompanied with excitement and talking; and sometimes patients have complained to me of this renewal of the chloroform in small doses with each pain, being accompanied each time with a renewal of the ringing in

[1] *Monthly Journal*, vol. for 1847-48, p. 417.

the ears, flashes of light, and other disagreeable sensations accompanying, in some persons, the primary effects of the inhalation. Besides, we are never thus sure that we are really saving the patient to the full extent by the means we are using. If, on the other hand, she happen to be thrown at once into a deeper state of anæsthesia, the chances of such inconveniences and drawbacks are avoided. Often, when the anæsthetic state is thus made deep from the first, the uterine contractions are arrested for a few minutes, but speedily return. In order to effect this, we take care that as soon as the patient is asleep—(and, in natural labour, we seldom or never require to push the inhalation so far as to affect the respiration, and produce noisy inspiration and snoring, as in surgery)—the chloroform should be withdrawn, and not reapplied again till the movements of the patient, or the state of the uterus, as felt through the abdominal walls, indicate a returning uterine contraction. A few inhalations given then, and repeated with each returning uterine contraction, keep the patient in a state of unconsciousness; and this condition may be easily maintained for hours, by administering in this way the chloroform vapour with each pain, and withdrawing it entirely during each interval. The practice is not to be expected to come upon medical men by intuition; for, like all other practices, some care and experience is necessary in order fully to acquire and apply it. And the *two* main difficulties which every beginner meets with are these—namely, to keep the patient in a state unconscious of pain, and yet not so deeply anæsthetised as to have the uterine action interrupted. For too deep a state of anæsthesia in general interferes with the force and frequency of the uterine contractions; while a lesser degree of the anæsthetic state leaves these contractions unaffected; and a still smaller dose often excites and increases them —the effects, in this respect, of chloroform upon the uterus, being similar to the effects of opium in different doses. But the influence of the inhaled agent passes off in a few minutes, differing in this respect from the more permanent influence of a drug when swallowed; and if, at any time, the anæsthetic effect is too deep, and the uterine action is in consequence impeded, all that is necessary is to abstain entirely from exhibiting the chloroform for a short time, till the parturient contractions have been allowed to come back to their proper degree of strength and frequency; and then the anæsthetic agency is to be sustained as before, by giving the vapour with every recurring pain, but in smaller doses, or for a shorter time during each pain, than was previously practised.

Anæsthetic vapours, when given in large doses, have less power of reining up the action of the uterus in the last than in the first part of labour. And as the sensations of pain become more agonising as the head is distending the perineum, and passing through the vulva, the anæsthetic state usually requires to be then rendered more deep and complete than in the early stages of the process; and in most patients this may be done without at all impeding the rapidity of the delivery. Indeed, in many women, this latter part of the process of parturition seems to be accelerated by the superinduction of anæsthesia; for the degree of relaxation of the muscular structures of the perineum and vaginal orifice, commonly resulting from it, usually more than compensates for any diminution of uterine action that may occur. If in any instance it proves otherwise, and the depth of the anæsthetic state interferes too much with the parturient contractions, the simple remedy is the one I have already mentioned—a diminution in the state of anæsthesia, so as to allow a return and increase of the expulsive efforts of the uterus.

The degree and depth of anæsthesia which different patients are capable of bearing without the irritability and contractions of the uterus being impeded, appears to differ greatly in different persons. In some, a very deep state will still leave the uterus almost or altogether unaffected; in others, its action is interrupted by a comparatively slight degree of the anæsthetic state. It is this variability which at first forms the principal difficulty to those commencing the use of chloroform in obstetric practice. But experience and care will soon enable any attentive observer to overcome this apparent obstacle, and to adapt the dose of the agent to the powers and capabilities of each different patient. I have never yet seen an instance, but I can conceive it possible, that in some rare exceptional cases and idiosyncrasies, the action of the ether or chloroform should, even in such small doses as merely produce unconsciousness to pain, interfere, especially in the first stage of labour, too much with the muscular action of the uterus, and require to be given up, at least till the labour be more advanced. But this would, of course, be no reason for not employing it on those other persons on whom it had no such influence; any more than because opium occasionally does not act as an hypnotic on particular patients, it should not be given with that indication to any other patients with the view of inducing sleep.

During the anæsthetic sleep which chloroform induces in natural labour, the patient usually lies perfectly quiet and passive in the

intervals between the pains, but moves more or less, and sometimes moans, as each uterine contraction begins to return. In the last stage she generally, with every recurring uterine contraction, makes the usual violent bearing-down muscular efforts, and the struggle can often be marked in the expressions of her face. The muscular action of the uterus and assistant muscles goes on, and yet she remains quite unconscious. The strictest quietude should always be observed and enforced around the patient, for noises and speaking, particularly soon after the chloroform is commenced, will sometimes excite and make her talk; and, if this happen, we may require to exhibit to her a deeper dose than would otherwise be at all necessary. One or two practitioners of midwifery in London have averred and repeated, over and over again, in our medical journals, and in pamphlets intended for non-medical readers, that obstetric patients, under the influence of chloroform, must be liable to talk and act grossly and obscenely. This objection to the practice of anæsthesia in midwifery has been repeated and gloated over by those who have propounded it, in a way which forms, apparently unconsciously on their own part, the severest self-inflicted censure upon the sensuality of their own thoughts. An impure mind, more especially in a professional man, may easily fancy and find impurities where none whatever exist; but he is not on that account entitled to imagine that his own lewd thoughts are typified in the thoughts or actions of his patients. In answer to the supposed objection itself, I have merely to observe, that I never once witnessed any trace of indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren, of whom I have inquired on the subject here, is to the same effect. In a paper on temporary delirium occurring in the course of labour, Dr. Montgomery several years ago described more marked instances of effects of this description, arising merely from "the extreme distress and pain," to which the mother was subjected in the dilatation of the os uteri, etc., during natural parturition, than were ever seen to arise from the influence of means used to abate and abrogate that "extreme distress and pain."

In administering chloroform in obstetric practice, I have always used the handkerchief, as the simplest and best apparatus. Sometimes, when the case is likely to be tedious, I have it folded and sewed into the form of a deep cup or cone. The chloroform is poured into the bottom of the cup, the open end of it held over the nose and mouth of the patient when the action of the vapour is

required; and, when its application is suspended, by closing the open end of the cone, the escape and loss of the vapour is prevented during the intervals. Such an arrangement saves the chloroform. But a handkerchief merely folded together and sprinkled with chloroform answers quite well; and in the intervals it may be compressed together in the hand, so as to prevent the escape of the chloroform. In *first* throwing the patient over into the anæsthetic sleep —(the point which requires the most management)—a handkerchief, thus presenting a large surface, is often much more serviceable than one folded into a cup shape; for the patient, when first coming under the influence of the chloroform, is apt to move her head from side to side; and, in order to keep up the constant inspiration of the vapour, she can be more easily followed by using a simple handkerchief, than by trying to keep any kind of apparatus applied to her mouth or face.

The quantity of chloroform used varies both according to the duration of the labour, and the susceptibility of the patient. Usually, when the handkerchief is used, about an ounce an hour is necessary—a small quantity being poured upon it from time to time. A less dose will suffice in some, and others require more. In one case lately, where the patient, in a first labour, was anæsthetised for two hours, I expended nearly six ounces, large doses being necessary to keep her in a sufficiently deep state of unconsciousness. The first quantity which I pour on usually amounts to three or four drachms; but I always judge by the *effects*, not by measuring the dose; and I pour on an additional quantity in a minute or so, if it be required. In holding the handkerchief towards the patient, I take care that plenty of atmospheric air is admitted, and seldom or never put it in contact with the face. At first, it is better to hold it at a considerable distance, in order to prevent any chance of irritation and coughing; and then gradually approach it. It is always to be remembered that the vapour of chloroform is nearly four times the specific gravity of atmospheric air; and if the patient is lying on her side, the handkerchief or pillow can be easily arranged so as to keep a larger supply of this heavy vapour opposite the mouth and nostrils. I have always held and managed the handkerchief myself in the first instance, and till the patient was asleep. Afterwards, I have generally trusted it to the husband or nurse, teaching them to apply it near the face when the pains supervened, and to fold up the handkerchief in the way mentioned, so as to preserve the chloroform during the intervals.

When exhibiting chloroform in obstetric practice, and in the way I have described, I have often been struck by the circumstance that its use is very rarely followed by sickness or vomiting. I do not remember having seen vomiting follow its exhibition during labour in more than four or five cases, and two of these, in the practice of Dr. Paterson and Dr. Cochrane, were instances in which I was called in to apply the forceps, and where the patients were placed for the operation in a state of anæsthesia as deep as that used in surgery. I have repeatedly seen it arrest the sickness and vomiting occasionally accompanying the first stage of labour.

In addition, let me state that I have usually begun the employment of the chloroform when the os uteri was well dilated, or towards the termination of the first and the commencement of the second stage of the labour. But when the pains were severe I have commenced it earlier, and when the os uteri was still comparatively little dilated. There is, I believe, no limit as to the date of the labour at which we may give it.

The rules for the exhibition of chloroform in parturition may be summarised as follows:—

1. Begin the inhalation of chloroform when the patient complains of much pain. This is generally towards the end of the first stage.

2. Always inculcate perfect quietness around the patient, particularly when commencing to give the chloroform.

3. *Only give it during the pains, and withdraw it during the intervals.*

Exceptions.—Give a whiff of the chloroform also during the intervals when the pains are very severe, and the patient awakes complaining of them.

Give small doses, or only repeat them every second or third pain, when the chloroform affects the action of the heart and uterus. These cases are very rare.

4. When given during the first stage the anæsthesia need not be deep, unless the suffering be great or the symptoms of anæsthesia disagreeable.

5. As the second stage progresses, make the anæsthesia so complete as to destroy all sensibility.

6. Do not allow the urinary bladder to become over distended.

7. Do not restrain the patient in one position.

8. Be sure to remove the chloroform as soon as the child is born.

9. Do not awake the patient artificially.

CHAPTER III.

CASES ILLUSTRATIVE OF THE USE AND EFFECTS OF CHLOROFORM IN MIDWIFERY.

Edinburgh, November 1847.

I HAVE not yet had an opportunity of using chloroform in any capital surgical operation, but have exhibited it with perfect success in tooth-drawing,[1] opening abscesses, for annulling the pain of dysmenorrhœa, and of neuralgia, and in two or three cases where I was using deep, and otherwise very painful galvano-puncture for the treatment of ovarian dropsy, etc. I have employed it also in obstetric practice with entire success.

CASE I.—The lady to whom it was first exhibited during parturition had been previously delivered in the country by perforation of the head of the infant, after a labour of three days' duration. In this, her second confinement, pains supervened a fortnight before the full time. Three hours and a half after they commenced, and ere the first stage of the labour was completed, I placed her under the influence of the chloroform, by moistening, with half a teaspoonful of the liquid, a pocket-handkerchief, rolled up into a funnel shape, and with the broad or open end of the funnel placed over her mouth and nostrils. In consequence of the evaporation of the fluid, it was once more renewed in about ten or twelve minutes. The child was expelled in about twenty-five minutes after the inhalation was begun. The mother subsequently remained longer soporose than

[1] A young dentist who has himself had two teeth extracted lately—one under the influence of ether, and the other under the influence of chloroform—writes me the following statement of the results :—"About six months ago I had an upper molar tooth extracted whilst under the influence of ether, by Mr. Imlach. The inhalation was continued for several minutes before I presented the usual appearance of complete etherisation ; the tooth was then extracted ; and although I did not feel the least pain, yet I was conscious of the operation being performed, and was quite aware when the crash took place. Some days ago I required another molar extracted on account of toothache, and this operation was again performed by the same gentleman. I inhaled the vapour of chloroform, half a drachm being poured upon a handkerchief for that purpose, and held to my nose and mouth. Insensibility took place in a few seconds ; but I was so completely *dead* this time, that I was not in the very slightest degree aware of anything that took place. The subsequent stupifying effects of the chloroform went off more rapidly than those of the ether ; and I was perfectly well and able again for my work in a few minutes."

commonly happens after ether. The squalling of the child did not, as usual, rouse her; and some minutes elapsed after the placenta was expelled, and after the child was removed by the nurse into another room, before the patient awoke. She then turned round and observed to me that she had "enjoyed a very comfortable sleep, and indeed required it as she was so tired,[1] but would now be more able for the work before her." I evaded entering into conversation with her, believing, as I have already stated, that the most complete possible quietude forms one of the principal secrets for the successful employment of either ether or chloroform. In a little time she again remarked that she was afraid her "sleep had stopped the pains." Shortly afterwards, her infant was brought in by the nurse from the adjoining room, and it was a matter of no small difficulty to convince the astonished mother that the labour was entirely over, and that the child presented to her was really her "own living baby."

Edinburgh, December 1847.

Case II.—I exhibited it, with Mr. Carmichael, to a patient who had, at her preceding confinement, been in severe labour for twenty hours, followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body and bed clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born till she suddenly heard it squalling at its first toilet in the next room. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.

Case III.—Patient unmarried. A first labour. Twins. The first child presented by the pelvis, the second with the hand and head. The chloroform was exhibited when the os uteri was nearly fully dilated. The passages speedily became greatly relaxed, as has happened in other cases placed under its full influence; and in a few pains the first child was born, assisted by some traction. I broke the membranes of the second, pushed up the hand, and secured the more complete presentation of the head. Three pains expelled the child. The mother was then bound up; her clothes were changed, and she was lifted into another bed. During all this time she slept on soundly, and for a full hour afterwards; the chloroform acting in this, as in other cases of its prolonged employment, as a soporific. The patient recollected nothing from the time of the first inhalations; and was in no small degree distressed when not one—but two—living children were brought by the nurse to her. Dr. Christison accompanied me to this case.

Case IV.—Primipara of full habit. When the first examination was made, the passages were rigid, and the os uteri difficult to reach. Between six and seven hours after labour began, the patient, who was complaining much, was apathised with chloroform. In about two hours afterwards the os uteri was fully dilated, and in four hours and a half after the inhalation was begun, a large child was expelled. The placenta was removed, and the patient bound up and dressed before she was allowed to awake. This patient required an unusual quantity of chloroform; and Dr. Williamson, who remained beside her, states to me, in his notes of the case, "the handkerchief was moistened often in order to keep up the

[1] In consequence of extreme anxiety at the unfortunate result of her previous confinement, she had slept little or none for one or two nights preceding the commencement of her present accouchement.

soporific effect. On one occasion I allowed her to emerge from this state for a short time ; but on the accession of the first pain she called out so for the chloroform, that it was necessary to pacify her by giving her some immediately. In all, four ounces of chloroform were used." Like the others, she was quite unconscious of what had gone on during her anæsthetic state ; and awoke altogether unaware that her child was born.

Case V.—Second labour. This patient, after being several hours in labour, was brought to the Maternity Hospital. I saw her some time afterwards, and found the first stage protracted by the right side of the cervix uteri being thick, œdematous, and undilatable. The inhalation of chloroform was begun, and the first stage was terminated in about a couple of hours. Two or three pains drove the child through the pelvic canal, and completed the second stage. Fifteen minutes in all elapsed from the termination of the first to the termination of the third stage, or the expulsion of the placenta. The patient was dressed and removed into a dry bed, where she slept on for a short time before awaking, and being conscious of her delivery.

Case VI.—Second labour. The patient, a person of small form and delicate constitution, bore her first child prematurely, at the seventh month. After being six hours in labour, the os uteri was fully expanded, and the head well down in the pelvic cavity. For two hours subsequently it remained fixed in nearly the same position, and scarcely if at all advanced, although the pains were very distressing, and the patient becoming faint and exhausted. She entertained some mistaken religious feelings against ether or chloroform, which had made her object to the earlier use of the latter ; but I now placed her under its influence. She lay as usual like a person soundly asleep under it, and I was now able, without any suffering on her part, to increase the intensity and force of each recurring pain, by exciting the uterus and abdominal muscles through pressure on the lower part of the vagina and perineum. The child was expelled in about fifteen minutes after the inhalation was commenced. In a few minutes she awoke to ask if it was really possible that her child had been born ; and was overjoyed to be told that it was so. I had the conviction that in this case the forceps would in all probability have been ultimately required, perhaps hours subsequently, provided I had not been able to have interfered in the way mentioned. I might, it is true, have followed the same proceeding though the patient was not in an anæsthetic state, but I could not have done so without inflicting great misery and agony upon her, and meeting with great resistance.

Case VII.—A third labour. The patient had been twice before confined of dead premature children ; once of twins, under the care of Mr. Stone of London ; the second time of a single child, under my own charge. The liquor amnii began to escape about one o'clock A.M., but no pains followed for some time. I saw her between three and four, with the pains commencing, and the os uteri beginning to dilate. In two hours afterwards the first stage was well advanced, and, the pains becoming severe, she had the chloroform exhibited to her, and slept soundly under its influence. In twenty minutes the child was born, and cried very loudly without rousing the mother. In about twelve or fifteen minutes more she awoke, as the application of the binder was going on, and immediately demanded if her child was really born and alive, as she thought she had some recollection of hearing the nurse say so. She was rejoiced beyond measure on her son being brought in and presented to her.

Case VIII.—Fourth labour. The patient had borne three dead children prematurely, about the sixth and seventh months of utero-gestation. During her present pregnancy I placed her under strict rules and discipline ; and she used, from an early period, small doses of chlorate of potass several times a-day. She carried her children to the full time. Labour came on about one o'clock A.M. The membranes broke at eight A.M., when the os uteri was still very slightly open. It had made very little progress at ten o'clock, when Dr. Keith exhibited the chloroform to her. The pains continued very strong and regular, the passages relaxed, and at half-past eleven she was delivered of a large living child. The placenta came away immediately ; and she was bound up, and her soiled clothes removed, before she awoke. She remembered nothing whatever that had occurred after she began to inhale the chloroform till the period of her awaking.

The preceding instances afford, perhaps, a sufficient number of examples of the use of chloroform in natural labour. In these and in all others which I have seen, or that have been reported to me, the immediate effects of the chloroform have been delightful. The mothers, instead of crying and suffering under the strong agonies and throes of labour, have lain in a state of quiet, placid slumber, made more or less deep at the will of the medical attendant, and, if disturbed at all, disturbed only unconsciously from time to time by the recurring uterine contractions producing some reflex or automatic movements on the part of the patient—like those of a person moving under any irritation of the surface, or from the touch of another, though still in a state of sleep. Nor have the ultimate consequences and results been less happy. No difficulties have been met with in the third stage ; and the uterus has contracted perfectly after delivery. I never saw mothers recover more satisfactory or rapidly—or children that looked more viable. And the practice is not a great blessing to the patient merely ; it is a great boon also to the practitioner. For whilst it relieves the former from the dread and endurance of agony and pain, it both relieves the latter from the disagreeable necessity of witnessing such agony and pain in a fellow-creature, and imparts to him the proud power of being able to cancel and remove pangs and torture that would otherwise be inevitable. It transforms a work of physical anguish into one of painless muscular effort ; and changes into a scene of sleep and comparative repose, that anxious hour of female existence, which has ever been proverbially cited as the hour of the greatest of mortal suffering.

The effects of the superinduction of anæsthesia in parturition are, if possible, still more marked and beneficial in cases of morbid labour and operative delivery. In proof of its influence in this

respect, I shall cite some examples of its employment in cases of turning, of the application of the forceps, and of embryulsio.

Case IX.—Fourth labour. The mother deformed, and the conjugate diameter of the brim of the pelvis contracted from the projection inwards and forwards of the promontory of the sacrum. Her first child was delivered by embryulsio; the second by the long forceps; the third was small, and passed without artificial assistance. On the present occasion, after suffering slight pains during the whole night, labour set in with greater severity towards morning. After being in strong labour for some hours, she was seen first by Mr. Figg, and afterwards by Dr. Peddie, her ordinary medical attendant. I was called to her about four o'clock P.M. The pains were then enormously powerful and straining, imparting to the mind the dread of the uterus rupturing under their influence; but the head of the child was still altogether above the brim, and only an œdematous ridge of the scalp pressed through the superior and contracted pelvic opening. The passages had become heated, the mother's pulse raised, etc., and Dr. Peddie had tried two different pairs of long forceps. After I arrived, he applied with great skill another pair of long forceps which I had with me; but it was found impossible to move the head in the least degree forwards. The urgency and power of the uterine contractions, the immobility of the head upon the brim of a deformed pelvis, and the state of the patient and of the parts, all showed the necessity of relief being obtained by artificial delivery. In her first labour I had assisted Dr. Peddie in delivering her under similar circumstances by perforation of the head. But here the child's heart was heard distinctly with the stethoscope, and he at once agreed to my proposition, that I should try to deliver her by turning the infant—compressing and indenting the flexible skull of the fœtus, instead of perforating it, and thus affording, as I have for some time past taught and believed, some chance of life to the child, and more chance of safety to the mother. The patient was placed under the influence of chloroform still more deeply than when the forceps were used, in order, if possible, entirely to arrest the uterine contractions. I passed up my hand into the uterus, seized a knee, and easily turned the infant; but very great exertion and pulling was required to extract the child's head through the distorted brim. At last it passed, much compressed and elongated. The child was still-born, but, by applying the usual restorative means, it speedily began to breathe and cry. The child continues well, and the mother has made a rapid recovery.

Case X.—In the Maternity Hospital; first child. Labour began at ten P.M. (21st Nov.) I was desired to see her at six A.M. (22d). The os uteri was well dilated, but it was evident that the pelvic canal was contracted throughout, and the head was passing with unusual difficulty through the brim. The patient was complaining much of her sufferings. It was clear that it would be a very tedious and probably at last an instrumental case, and one therefore calculated to test the length of time during which chloroform might be used. She began to inhale it at a quarter past six A.M., and was kept under its influence till a quarter past seven P.M., the date of her delivery—thirteen hours in all. From the time it was begun to the time delivery was completed, her cries and complaints ceased, and she slept soundly on throughout the day. The bladder required to be emptied several times with the catheter. The head passed the os uteri at ten A.M.; and, during the day, gradually descended through the pelvis. At seven P.M. I at last deemed it proper to deliver her by the forceps; the head, which was now elongated and œdematous, having by that time rested for some hours against the contracted peivic outlet

with little or no evidence of advancement, the bones of the fœtal cranium overlapping each other; and the fœtal heart becoming less strong and distinct in its pulsations. A warm bath, irritation of the chest, etc., were necessary to excite full and perfect respiration in the infant. Whilst we were all busied with the infant, the mother lost some blood; but the placenta was immediately removed, and the uterus contracted perfectly. On afterwards measuring the quantity of blood lost, it was calculated to amount to 15 or 18 ounces. The mother's clothes were changed; she was bound up and removed to a dry bed before she awoke. She had at first no idea that the child was born, and was in no respect conscious of being delivered. In fact, she had been "sleeping," according to her own account, from the time she had begun the inhalation, and only thought she once or twice remembered or dreamed that she heard Dr. Williamson, the house-surgeon, speak near her. Dr. Beilby, Dr. Zeigler, etc., saw the case with me. The mother and child have continued perfectly well.

In this, as in other cases, I have watched and noted the effects of the chloroform upon the duration of the pains and of the intervals, the rate of the fœtal and maternal pulse, etc.

Case XI.—Patient with a deformed spine and contracted pelvic outlet. At her first confinement two different medical gentlemen had failed in effecting delivery by the forceps. At this, her second confinement, she placed herself under the care of Dr. Paterson of Leith. After being very long in labour, and the symptoms of the case becoming urgent, I saw her with Dr. Paterson. The head was low down in the pelvis; but it was placed in the right occipito-posterior position (the third of Naegele), and the forehead instead of the vertex was presenting, one orbit being easily felt behind the symphysis pubis. It had been lodged in nearly the same position for many hours. The fœtal heart was still distinct, but weak. I applied the forceps, turned the head round with them a quarter of a circle, into an occipito-anterior position (the second of Naegele); and, after being so adjusted, it still required considerable force to extract it. Before applying the forceps, the patient was sent into a state of deep anæsthesia by the inhalation of chloroform; and subsequently, when she awakened out of it, she was in no small degree surprised to find that she had really been delivered while she was sleeping and resting so soundly. The placenta separated, and the uterus contracted firmly. The child, which was large, lived for eight hours after delivery; but, despite of all the measures tried, full and perfect respiration was never established in it—apparently in consequence of some effusion or injury about the base of the brain. Unfortunately a post-mortem examination was not obtained. The mother has made an excellent recovery.

I quote the following instance of craniotomy under chloroform from a letter (dated 29th November) which I have received from my friend Professor Murphy of London. I give the case in Dr. Murphy's own words.

Case XII.—"I have tried the chloroform with great success in a case of distorted pelvis. It was the ovate deformity, the conjugate measurement being only two and a half inches; the head of the child could not enter the brim, and I was obliged to perforate. I got Dr. Snow to assist me in bringing her under the influence of chloroform. She made some resistance, and struggled a good deal at first, chiefly, I think, from apprehension that we were going to do something

very dreadful ; however, she soon began to inhale quietly, and gradually fell into a kind of dreamy sleep. I perforated the head, and laboured with the crotchet, sometimes with the craniotomy forceps, for three quarters of an hour before I could get the head through the brim. She was at length delivered ; the placenta was separated in about ten minutes, the bandage applied, soiled clothes removed, and she was made clean and comfortable as the midwives say. My patient was perfectly unconscious all this time, and did not awake for about a quarter of an hour after the operation ; she did so then quite quietly, and was greatly surprised to find that all her miseries were over. There was no hemorrhage ; but the uterus felt rather spongy and large. She is now recovering most favourably. I never had a case recover so far so well."

CHAPTER IV.

REPORT OF THE RESULTS OF ANÆSTHESIA FROM DIFFERENT OBSTETRIC HOSPITALS AND PRACTITIONERS.

Edinburgh, October 1848.

THE following account of the results of anæsthesia in the practice of the Maternity Hospital, Edinburgh, has been drawn up by Dr. Duncan and Mr. Norris, two gentlemen who have acted as resident house-surgeons in the institution, and upon whose power and accuracy of observation all who are acquainted with them will place implicit reliance :—

"Since the use of anæsthesia in labour became general in the Maternity Hospital, shortly after the discovery of chloroform, 95 women in all have been delivered in the house under its influence. Among these, 88 were natural, and 7 were morbid labours. In the 88 cases of natural delivery, only one of the mothers died, convulsions coming on five hours after delivery, and proving fatal after a continuance of six days. On a *post mortem* examination, the kidneys were found to have undergone, in some parts, the true stearoid degeneration.[1] Among the same 88 cases of natural labour, there were 5 dead-born children. In two of these cases, the birth was premature, being at the sixth month. In the third case, the mother had previously given birth to two dead infants. The fourth dead child had a very large hydrocephalic head. The proportion of still-born children was thus 1 in 17. In the Dublin Hospital the proportion of still-born children, as reported some years ago by Dr. Collins, was 1 in 15. The 7 instrumental cases were as follows :— One application of the short forceps, in an unsuccessful attempt to save the life of the child ; the mother recovered well ;—two cases in which the long forceps were applied ; one of the women, in whom the head was very long impacted in the pelvic brim, died from sloughing of the maternal passages ; the other made a good re-

[1] See a report of the case, *Monthly Journal* for September, p. 196.

covery; both the children were born alive;—four cases of version; one of the mothers died from rupture of the uterus, the others recovered quickly; three of the children were still-born, and in one of these three cases the cord was prolapsed.

"In addition to these 88 cases of anæsthetic delivery, there have been upwards of fifty women delivered in the house without chloroform. These have been chiefly very rapid labours, where the women have come into the hospital just in time to give birth to their infants, or where the house-surgeon has not been able to see them till very shortly before delivery. From the expense attending a large consumption of chloroform, it has always been an object to husband it as much as possible;[1] and therefore, in the hospital, it has not been given in cases where the mothers did not very severely complain of their sufferings, nor were harassed with feelings of anxiety and fear.

"On the whole, the results of anæsthetic midwifery, as observed by us in the hospital, have been perfectly satisfactory; and we can confidently state that the recoveries have been altogether more perfect and speedy than before. This has been remarked in so great a proportion of the cases, that there can be no doubt whatever of the truth of the observation. Besides the increased rapidity of recovery, we have noticed the almost entire absence of those uncomfortable feelings of fatigue, languor, and shivering, and of that shattered feeling which so frequently comes upon the mother immediately after an ordinary delivery. Instead of this, we have found the mother almost invariably awake from the anæsthetic sleep comparatively fresh, easy, and cheerful. Not unfrequently the anæsthetic has been found to change, without an intermission, into a natural sleep, which may continue for an hour or two.

"Further, there have been, since the introduction of chloroform into the practice of the hospital, far fewer than formerly of those violent attacks of rigors, ephemeral fevers or weeds, and abdominal pains, which are so common in most crowded hospitals, forming a class of cases which used formerly to cause much anxiety, and was a common cause of the mother's being detained in the hospital after

[1] Perhaps, in a short time, a benevolent government will allow chloroform to be made cheaper, by removing the very high duty on proof spirit when used for medical and chemical purposes (tinctures, etc.) At present that high duty is, in one respect, a direct tax upon the relief of human disease, and the mitigation of human pain; and a great obstacle to the progress of British organic chemistry. See also p. 173.

the usual fortnight allowed for recovery. In fact, since using chloroform, there have been scarcely any women detained in the house by these causes, and much less Dover's powder, calomel and opium, abdominal fomentations, etc., have been used.

"The women have been, invariably, found deeply grateful for the relief to their sufferings afforded by the anæsthetic influence of chloroform.—Yours, etc., J. M. DUNCAN. H. NORRIS."

At a meeting of the Edinburgh Medico-Chirurgical Society in June last, along with other practitioners, I gave in a report on the employment of chloroform in midwifery. At that time, and since, I have been favoured with written statements of the results, by various medical friends in Edinburgh, and in different parts of the country. I shall now give extracts from a variety of the letters which I have received, relative to this subject. Many more such communications might easily have been called up and adduced; but I have deemed it useless to multiply unnecessarily this kind of evidence. It will be observed that, with one exception (see the communication of Mr. Lansdowne), the following letters refer, like the preceding statements regarding the Maternity Hospital, to the use of chloroform alone.

The first statement which I give is from my assistant and friend, Dr. Keith.

"I have employed chloroform in every case of labour under my care since its introduction, with one exception; and also in almost every case to which I have been called in by other practitioners. In my own cases, amounting to about four-and-twenty, it has been given for a period varying from half-an-hour to eight hours. The quantity of chloroform consumed has been, on an average, about one ounce per hour; in a few cases double this quantity was found requisite. The anæsthesia has been in almost every case complete; that is, the patient on awakening has declared that, while under the influence of the chloroform, she was utterly unconscious of all pain. In most cases the patient has lain quiet even during the pains, the presence of which is then generally indicated by the breathing becoming more rapid and somewhat laboured. In other cases, there is suppressed moaning during the pains, or even, in some, loud manifestations of powerful straining and muscular exertion. I can state most positively that I have seen no serious symptom which could be traced to the chloroform, in any one case, either as affecting the mother or the child. Most of the mothers have made

uncommonly good recoveries. Those who have had children previously, have, almost without exception, stated to me that they felt very decidedly stronger after delivery than on former occasions. In two cases the recovery was rather slow; but this was owing to the patients having been in a very delicate state during pregnancy—and, in both instances, I considered the chloroform was of very great service, by saving their strength. All the mothers are now in their usual health.

"In no one of the twenty-four cases was the child still-born. In one case, labour was brought on at the end of the seventh month, owing to the brim of the pelvis being much contracted. The child was born alive, but died on the second day. All the other children are now alive. They have all been nursed by their own mothers, with one exception.

"I have had occasion to use the forceps seven times since the introduction of chloroform, and once to break up the child's head and extract by the crotchet. In all these cases the patient was first put into a deep anæsthetic state, and in most she lay perfectly still and apathetic during the operation. All the mothers have done well, except in the case of craniotomy, where the uterus had ruptured previous to the use of chloroform.—Yours, etc.

"G. S. KEITH."

From Dr. MOIR, Edinburgh.

"Since the beginning of December, I have, with a very few exceptions, used chloroform in the course of my midwifery practice; and I have not met with a single case where any unpleasant effects, either to mother or child, can be traced to its use.

"As far as my observation has gone, I think it will be found that, in some cases, the chloroform, if freely administered at an early period of the first stage, retards the pains a little, and in others also lessens their power; and when this does occur, the best remedy is either to intermit its use till the labour is further advanced, or to give it in smaller quantities and at longer intervals, so as not, at that stage, to induce complete unconsciousness. But, whether correct or not in this opinion, I am quite satisfied that the second stage is much accelerated, especially towards its termination, by the chloroform doing away with the resistance offered to the expulsion of the head by the muscles at the outlet of the pelvis—and this to such an extent that, in some *first* cases, there is a risk, unless very great care is taken, that the perineum be slightly lacerated, from the head

being so rapidly expelled as not to give time to the parts to yield so rapidly as they would otherwise do. But this is comparatively a very rare occurrence, and requires to be mentioned principally with the view of putting young practitioners on their guard against it, and of leading them to use the necessary means to prevent it.

"In exhibiting so powerful an agent as chloroform, I think it a point of importance to use as small a quantity as is compatible with the obtaining of its full anæsthetic effects; and as this seems to depend much upon the rapidity with which it is conveyed into the system, it seems a point worthy of consideration to ascertain the readiest means of so doing. Various instruments have been invented for this purpose, though they have been almost universally superseded by the use of the handkerchief, as recommended by you, and used either in the form of a hollow cone, applied again and again, after renewing the chloroform, over the nose and mouth of the patient; or simply folded up several times, and frequently having interposed between the folds a piece of wool or flannel, with the view of better retaining the chloroform. In both methods there is a considerable loss of chloroform, much of it being retained in the folds of the handkerchief, and much of it escaping without passing into the lungs, or else passing into them so slowly as not to produce the desired effect. I have, for some time, been in the habit of using a linen or white cotton handkerchief, folded only once, or, if very thin, folded twice; the point requiring attention being, that it should not be so thick as to offer any impediment whatever to free respiration when applied over the mouth and nostrils. Since using the chloroform in this way I have never failed in rapidly producing the anæsthesia, either in my own practice, or when accompanying some of my patients, who were several months advanced in pregnancy, to their dentists to have one or more teeth extracted. For administering the vapour to patients who are in the erect position, the chloroform should be poured on that part of the handkerchief placed on the palm of the operator; the edge of the little finger should then be applied close to the chin, and the hand gradually raised up towards the mouth, till the sensation of choking which generally accompanies the first inspirations has passed off, after which, the handerchief should be left on the face, and the hand removed; the patient then breathes freely through that part of the handkerchief wetted with the chloroform, and in general half-a-drachm is sufficient to produce anæsthesia.

In obstetric practice, I find it the most convenient plan to place

one end of the folded handkerchief under the left cheek of the patient, to pour a little chloroform on it, and then, taking hold of the loose end of the handkerchief, to bring it gradually near the mouth, till it can be left there without inconvenience, the patient breathing freely through it. And it is not necessary again to remove the handkerchief, but simply to pour on it occasionally a very few drops, whenever the patient begins to show symptoms of returning consciousness, or on the accession of a pain. The only precautions necessary are, to raise a small fold of the handkerchief from the skin when the chloroform is to be applied, so as not to blister the skin; and to drop the chloroform, not on that part of the handkerchief immediately over the mouth, but a little above it, so that the vapour, being heavy, may flow down towards the mouth or nostrils, and thus be, during inspiration, more readily received into the lungs. By adopting this plan, I have had the handkerchief applied for nearly two hours without removing it; and the quantity of chloroform I have used in single patients has been much less than other practitioners have used in cases of the same duration.—Yours, etc. JOHN MOIR."

From Dr. MALCOLM, Edinburgh.

"Since November last I have employed chloroform in above thirty cases of labour, and with the most satisfactory and delightful results. A majority of these were first labours. I have kept my patients under it for periods varying from half-an-hour to six hours, and have never found the slightest unpleasant effects result from its use. All the children have been born alive, and are at this moment in perfect health, with the exception of one that died when about a month old, of a sudden and severe attack of dysentery. All the mothers have made recoveries with rapidity and completeness, far above the average which I had previously observed in my practice. This has struck me as the more remarkable, seeing a large proportion of my patients were primiparous; and I can only attribute this result to the entire absence of suffering and shock to the nervous system which is effected by the use of chloroform. Although in a few cases my patients and their friends have at first objected to the use of anæsthesia to abolish pains which they considered "natural," yet every one has afterwards expressed to me sincere gratitude for saving them from their agonies; and I am sure not one who has experienced the beneficial effects of the practice will ever submit to these agonies again, now that they know that they are so totally

unnecessary, and can be so easily and safely abolished. I have repeatedly found the mothers of my patients object to anæsthesia, as if they grudged that their daughters should not experience the same sufferings as themselves—but I have uniformly found them afterwards as grateful as their daughters for the relief administered.

"Generally, I have employed about an ounce of chloroform per hour. I have never seen the uterine contractions arrested by its use, although I have no doubt a large dose would, when necessary, have that effect. I have seen no case of hemorrhage, or convulsions, or any other complication whatever. Let me add, that I cannot conceive on what principle the employment of chloroform in natural labour should be objected to, as long as it is our duty, and assuredly it is our duty, as physicians, to relieve and mitigate human suffering.—Yours, etc. R. B. MALCOLM."

From Dr. THOMSON, Edinburgh.

After stating the details of ten cases, Dr. T. remarks:—

"Among the middling classes I have met with more difficulty in using the chloroform than I had anticipated, as it has only been when the sufferings of the patient were very severe, or her friends had begun to dread the effects of prolonged continuous suffering on her constitution, that I could get my wishes carried into effect. That fatal Newcastle case, which was trumpeted a good deal in the newspapers, is still haunting their minds, and is very frequently urged by some timid friend when you propose the chloroform to relieve the sufferings of the patient.

"My experience of it has been, in all obstetric instances, analogous to your own; with one exception, I have had no difficulty in getting the patient under its influence; a minute or two was in general sufficient to lay the most restless or ungovernable patient quiet on her pillow.

"Not the slightest *post partum* hemorrhage has taken place in my ten cases, though in the two forceps cases, where it was given deeply, the uterus remained flabby for nearly thirty or forty minutes, and threw off the placenta with difficulty.

"Its relaxing effects are, I think, undeniable. In one case, the soft parts had resisted for a considerable time the descent of the head; they yielded very readily within an hour after the chloroform was begun. I have not had another instance of this kind lately; but, were I to meet with one, I feel confident it would yield

with much more facility under the chloroform than without it.—Yours, etc. ALEX. THOMSON."

From Mr. CARMICHAEL, Edinburgh.

"I have given the chloroform in twenty-six cases of midwifery, four of which were first labours; the others varied from the second to the eleventh pregnancy. The quantity given varied from two drachms to four oz., and the length of time during which it was exhibited from a few minutes to four hours. The preparation I have used has always been that of Messrs. Duncan, Flockhart, and Co., and I have never seen the slightest bad effects from it, either in midwifery or other medical cases, or in any cases where I have administered it for amusement, except occasional sickness where it was exhibited shortly after a meal.

"I have met with no case of flooding whatever. I have heard it alleged that it drives away the milk; but I have not found it so, as my patients have all been able to nurse, with the exception of one lady, who has not been able to suckle her child for the last three times.

"In all of these cases it was administered with the greatest ease and with perfect success, and in no case with any bad results.

"The recoveries have been certainly more than usually speedy. Indeed the only objection I have met with as to its use, has been on the part of the monthly nurses, who seem afraid that the new practice will curtail their attendance and pay.

"All the children were born alive, and are doing well. The only case in which the child proved the least refractory was a footling one.

"I have also found it most useful in cases of dysmenorrhœa, in spasmodic colic, and tic doloureux.

"I also gave it in a case, which you saw along with me, of most severe neuralgia of the uterus, with the most perfect success.

"In no case whatever have I seen any bad results of any kind arise from the use of the chloroform.—Yours, etc.

"W. S. CARMICHAEL."

From Dr. BURN, Edinburgh.

"I regret that I cannot give you the number of cases of labour in which I have exhibited the chloroform, but I may state that I

have given it repeatedly, and have not seen any bad consequences either to the mother or child result from its use.

"All the mothers made rapid recoveries, and the children did not appear to suffer from its use.

"I have given the chloroform in three or four cases of adherent placenta, where the uterus was firmly contracted, and had far less difficulty in extracting it than I have experienced in similar cases where the chloroform was not exhibited.—Yours, etc.

"J. BURN."

From Dr. PURDIE, Edinburgh.

"I have now used chloroform in seventeen cases, which I have noted, and in every instance with decided effect, not merely by lessening suffering, but, I am perfectly convinced, by the most careful observation, by shortening the duration of labour. The pains have never in my experience been interfered with, except by rendering them quicker, and far more effectual.

"There is one of the cases which I would wish to recall to your memory. The patient, thirty-eight years of age, was in her first labour, which commenced early on Wednesday morning, and went on well but slowly till the evening, when its progress ceased, although the pains continued regular and strong. The os uteri was well dilated, but the head made no progress, although there was no very evident cause. About one o'clock on Thursday morning I sent for you to deliver her with forceps. On your arrival, you thought that still there was hope of the labour being terminated naturally. The patient, who was suffering much, was then put under the influence of chloroform and ergot, while you waited patiently for any advancement, for nearly two hours, without effect. You then delivered with the long forceps, which cost you great exertion, from the head being impacted in the brim. The patient's position was changed, the placenta was extracted, she was bound up and laid in a comfortable and easy posture, in which state she continued to sleep soundly until she was awakened after the child was dressed, the crying of which surprised her, as she had not been conscious of what had taken place from the time she got the first dose of the chloroform after your arrival. This patient had an excellent recovery. I never saw a patient suffer less after labour, or recover more rapidly. I may just add, that there are few things vex me more with regard to patients, than to witness the sufferings of a childbed patient, who will not allow, from ignorance or prejudice,

the use of chloroform. Happily, however, such cases are very rare among us.—Yours, etc. W. PURDIE."

From Dr. FINLAY, Newhaven, near Edinburgh.

"I have used chloroform in a considerable number of cases of natural labour. It was with much reluctance that I first administered it, and only at the urgent entreaty of a patient who was enduring intense agony before the birth of a first child. It was completely successful. Her screams had been audible across the street. In a few minutes they ceased, and she fell asleep, while the uterus continued to act as powerfully as before. She was not aware that she had got her baby until a quarter of an hour after it was born. In five of the other cases the influence of the chloroform was as complete. In the other three cases *entire* insensibility was not induced, but the sufferings were greatly mitigated, and the relief was so evident to the patients, that, whenever the labour pain was approaching, they grasped with great eagerness the handkerchief on which the drug was sprinkled. Satisfactory as these cases have been, I have hitherto used the chloroform with considerable hesitation and caution, and only when it was asked for by the patient. But every trial has emboldened me to employ it with greater confidence on future occasions. In each of my cases the placenta was soon and easily detached; in none of them did hemorrhage occur; and they all made excellent recoveries.—Yours, etc. A. FINLAY."

From Dr. CUMMING, Edinburgh.

"I have now attended thirty-five cases of labour under chloroform, and it has been used in all with marked advantage. All the patients have made unusually good recoveries; and I have been very much impressed by the fact, which was remarked by the first patient submitted by me to chloroform, and repeated by all, that the convalescence was not accompanied by the crushed and dislocated feeling that they have experienced without it.

"I always begin by introducing the chloroform slowly and gradually into the lungs, allowing a large proportion of air to be inhaled along with it. In every instance it was administered, not with reference to the quantity given, but to the effect produced—this effect being complete unconsciousness during the pain; and thus administered, I have never seen any unpleasant or absurd consequences, nor anything to excite alarm or even uneasiness.

"Two of the cases had had large floodings in a previous labour; with the chloroform there was none. This, of course, I do not impute to the medicine; but it at least tends to prove that hemorrhage is neither a necessary nor a likely consequence, as many at first were disposed to imagine.

"All the children were born alive, and are so still. None of them as yet give the slightest indications of idiocy, either present or future; nor have I observed in any the temporary stupefaction immediately after birth, ascribed to the presence of chloroform in the apartment, that some have remarked.

"I am quite satisfied that, if properly given, it acts as a calmative; and I believe, from what has passed under my observation, that very many of what are called exceptional cases are not so in reality, but appear to be such from error in the mode of administration, and that further experience will amply demonstrate the truth of this.

"In short, I am, unfortunately for the appearance of veracity, compelled to say, that all my cases hitherto have been so successful, the recoveries so uniformly good, and the satisfaction on the part of the patient (I may add also my own) so great, that I am rapidly approaching to, if indeed I have not already arrived at, the conviction, that, *if there be any sin connected with chloroform, it is chargeable on those who refuse to administer it.*

"I may add, that not one of those patients who have already inhaled it will ever be denied it in any subsequent pregnancy, as they have repeatedly assured me; and certainly I shall not attempt to keep it from them, and that not more for their sake than my own.—Yours, etc. W. CUMMING."

If necessary, I might have adduced more evidence in favour of the anæsthetic effects of chloroform in midwifery practice, from Dr. Beilby, Dr. Zeigler, Dr. Weir, Dr. Young, Dr. Menzies, Dr. Gilchrist, Dr. Campbell, and other medical practitioners in Edinburgh, who have been using it.

In order to vary the kind of evidence, I shall next adduce extracts from various communications which I have received on the use of chloroform in midwifery, from medical correspondents in different parts of Scotland, England, and Ireland. It is needless, I believe, to attempt to arrange them in any special order; and I shall content myself, therefore, with beginning with the letters of those practitioners who live farthest north, and proceed southward.

The following is an extract from a letter written to me this summer:—

From Dr. GRIGOR of Nairn.

"Dr. Allan of Forres and myself would as soon think of going to an obstetric case without our chloroform phial, as we would of going to bleed a patient without a lancet. In this quarter, doctors are only called in when things are going wrong, or in extreme cases; so that, since your grand discovery, he and I have only used it in about twenty-four cases, in all which it came up to all you have written about it—no still-born children, mothers recovering well, fewer after-pains, etc. etc. One of my cases was a first child, the mother nearly forty-eight years of age, weakly in constitution, and of small formation. Had it not been for the chloroform, I do think she would have sunk.—Yours, etc. J. GRIGOR."

Dr. Dyce, lecturer on midwifery in Marischal College, Aberdeen, favoured me some time ago with the following interesting communication regarding the obstetric employment of chloroform:—

"I have reports from my friends Drs. Harvey, Pirrie, and Gilchrist, all of whom, I was aware, had been employing it. I may at once state that I consider it a most invaluable agent; that I have every confidence in its safety; that I recommend it almost on every occasion; that no evil consequences have ever attended its use; and that I have found its effects nearly alike in all. I have used it eleven times. The labours have, with one exception, been natural. The exception was a breech case, and a first child. Two of the eleven children were lost; one was putrid, and in the other (the breech case), though the funis did pulsate for some minutes, the child could not be recovered.

"Dr. Harvey has given me a brief account of four cases delivered under chloroform; all the children were born alive.

"Dr. Gilchrist at Woodside writes to me—'I have observed no evil results to the infants themselves from the use of chloroform.' He does not state the number of his cases; but I presume they have been numerous, from his remarking, that 'latterly I have not used this agent so generally as when it was novel, reserving it now, unless when urgently requested by the patient, for cases unusually painful, whether arising from excessive sensibility of the system, rigidity of the soft structures, or cases requiring manual assistance.'

"Dr. Pirrie has employed chloroform in fourteen cases. Ten of these were natural, two instrumental, and one a case of turning. He says, 'As to the children, they have all been born alive, and continue to go on satisfactorily.'

"I never use it early in labour; generally the second stage has come on, or at least the os uteri is tolerably well dilated. This appears to be the practice of my brethren here. I then keep the patient in a state of insensibility, more or less complete, during the future progress of the labour. Occasionally I have allowed them to come completely out of the anæsthetic state; but, on the recurrence of the pain, the patients invariably and urgently seek for the handkerchief. When I used chloroform, I imagined that the intenseness of the stupor induced did lessen the frequency and force of the pains, and even put a stop to the labour altogether; and I am still of opinion, that if the full effect is produced and kept up for any time, the pains will cease; but if a more moderate effect is produced, so that consciousness to a certain extent remains, my conviction is, that, instead of checking uterine action, its use enables the organ to act with more freedom; the mental influences are quickened; and, more than this, it has a decided power in relaxing the soft parts, and thus removing a very frequent cause of protracted labour. I had a very interesting case, illustrative of these remarks, in January last, in a lady in her fifth pregnancy, of a very irritable, anxious, and highly nervous temperament, who had, previously to labour, determined to use this wonderful agent. Her former labours had always been tedious, and very painful throughout every stage. This commenced, and had continued for a couple of hours before I saw her. She was complaining much of the severity of the pains, especially of her inability to move from one posture. Her anxiety and agitation were very considerable; her pulse was above 100, and her body was already wet with perspiration. On examination I found the parts rigid, and the os not larger than a shilling, while the rectum actually encroached upon the vagina from its loaded state. I determined, therefore, on emptying the gut by an enema before employing the chloroform, and mentioned this to my patient, and gave the nurse the necessary instructions. I very soon found that I had added not a little to her already excited state, as she was not prepared for delay, and would only, after some persuasion, consent to its exhibition, and only then on condition that immediately after she was to be allowed the 'stuff.' Her request was complied with, and I confess to you its effect on her general state,

from the moment she was under its influence, not only gratified myself, but pleased her friends, who complained much of her impatience and her imagined evils. A few inhalations sufficed to induce insensibility, before which she gave one or two hysterical laughs, and made some incoherent remark. She then became still and quiet, until a pain returned—the only indication of which was a gentle writhing of the body. Her pulse very soon fell to 80, at which it remained; her skin became cool; the parts rapidly relaxed; she moved, and allowed herself to be moved in any direction readily; the pains were certainly more regular and efficient than in any of her former labours; and in two hours and a half from the first inhalation, the child was born. Once only during this period, and that was after one hour had elapsed, was she perfectly sensible. This was permitted at the solicitation of the nurse and friends, who had not seen chloroform used before, and could not be convinced that so sudden a change could exist and the labour progress perfectly. They soon had evidence of this, by the patient screaming out in her former impatient tone, 'Pain, pain! where is the handkerchief?' From this time until delivery she was perfectly unconscious. The placenta came away naturally in ten minutes. The roller was put on, some of the soiled things were changed, and it was not for several minutes ere she was convinced that her trials were over. I need hardly say that she was gratified in the highest degree to find that she had escaped so much of her former sufferings, and expressed her deep thankfulness for so valuable a boon. The child was putrid. Her recovery was perfect, and more rapid than on any former occasion.

"In my practice I have met with no instance of chloroform failing in producing its usual effects in a very few minutes, where it was willingly and readily inhaled; in some nervous patients, a few seconds are sufficient for this purpose. In the case I have just given, I was surprised at the few inhalations she took; and Dr. Pirrie mentions that one of his patients was so highly susceptible of its effects, that, on making three or four inhalations, the state of anæsthesia became so complete that the labour was entirely suspended; he therefore, in this case, discontinued its use. On some occasions I had difficulty in persuading the patients to use it; hence a longer time elapsed. In one case, when persuasion failed, I watched an interval of drowsiness, and, in spite of herself, brought the woman fairly under its influence, in which state she remained for twenty minutes, when the child was born unconsciously. Its

cry aroused her, she turned herself hastily around with an inquiring look, and after a momentary gaze said, 'What is that? you have been giving me the stuff.'

"The quantity of liquid used has varied in my hands from ℥ss to ʒiiss. This last quantity was expended in the case of a young woman, of eighteen years of age, pregnant of her first child, and who was more or less under the influence of chloroform for five hours. As this was amongst my first cases, if not the very first, and the longest period in which I have employed this agent, perhaps you will excuse my giving it somewhat in detail, more especially as ergot was given along with it, which I am not aware that I had seen anywhere recommended. Jane ———, æt. eighteen, unmarried; first child, December 18, 1847. When seen at five P.M., the membranes were reported to have ruptured twelve or thirteen hours before; on examination, the os uteri was found dilated to the size of half-a-crown, and the breech was discovered as the presenting part; the pains were frequent, irregular, at times only effectual, but they had been increasingly severe during the after part of the day, and now were strong.

"About seven P.M., after some persuasion, she first inhaled the chloroform. She soon became insensible; it was then only presented to her at intervals as a pain recurred, a single inhalation being generally sufficient. She laughed and sang for a few minutes, but at length seemed perfectly unconscious, and we were only sensible that she had pain by her now and then rolling about, as is the case when labour-pains continue under convulsions. For an hour she was kept in this state; but thinking the pains were becoming more distant, and the progress of the labour slower, she was allowed to come out of this torpid state. Soon after ʒss. of powdered ergot was given, and repeated four times, at intervals of a quarter of an hour. The pains now became stronger and more frequent, and my patient, who was conscious of the powerful agency of the chloroform, and the advantages she had derived, insisted on its repetition. She had, in fact, secreted the handkerchief, and was now keeping it at her mouth. But, alas! its talismanic powers had vanished; she threw it rather angrily at the student, who at that moment sat by her bedside, desiring him to give her the bottle in its stead; she assured us that she had felt no pain until now, that she had been dreaming of her illness, which she believed was finished. From this time (nine o'clock), until her delivery, which took place between one and two o'clock the following morning, she was kept in a per-

fectly unconscious state, at times more so than at others, according to the severity of the pains. Towards the end of the labour, the throes were very severe, but during none of them did the patient complain of the slightest pain. The only indication, as I have already said, of apparent suffering, was a movement of the body, and occasionally a low moan. The child in this case did not breathe or cry; it was not, therefore, legally in life, though the funis pulsated for some minutes.

"I can scarcely think that the death ought to be attributed to the chloroform, or even to the ergot. The length of the labour, the nature of the presentation, and especially its being a first child, were almost against its being born alive. So that, with this exception, no injury has befallen the infant in the practice of any of us here who have employed chloroform.

"I have only further to say, that the recoveries in every case have been most satisfactory. They appeared even more rapid than under ordinary circumstances.—Yours, etc. R. DYCE."

From Mr. LAWRENCE, Montrose.

"I am sorry I cannot furnish much information as to the employment of chloroform in midwifery in this quarter. It has been very little used by myself or brethren, owing chiefly to a very general prejudice on the part of our parturient patients against it. The very last case I attended, the patient resolutely refused it, although suffering very severe pain! However, in one of those in which I administered it, and to which I had gone unprovided with the chloroform, my patient compelled me to send for it! In one case, in which a patient of mine had convulsions in her first labour, I exhibited chloroform at her second confinement with the usual success.

"I have seen no injurious consequences of any kind.—Yours, etc. SAMUEL LAWRENCE."

From Dr. STEELE, Montrose.

"I have used chloroform in midwifery practice on six separate occasions. In two of the cases, it was given to the extent of producing only a partial immunity from suffering; but with the effect of converting, especially in one to whom it was administered for four hours, what would have been a very severe into a very easy labour. In the other four, the effect was highly satisfactory; two

of the children being born without the consciousness on the part of the mothers, for some time after, of that event having taken place. One lady remarked, that she thought 'the march of intellect' had never taken a happier direction than when it led to the discovery of chloroform.

"All the children did well. One of them, however, was asphyxiated for nearly a quarter of an hour after its birth.

"From the little experience I have had of chloroform, any opinion of mine as to its merits is not entitled to much weight. It seemed certainly, in several of my cases, to prolong the interval between the pains, and thereby retard the termination of the labour; and I thought also that the uterine action was sometimes less effective when the patient was under the power of the medicine. —Yours, etc. GEORGE STEELE."

From Dr. PATON, Dundee.

"I have ascertained that chloroform has been employed in upwards of fifty cases up to this date (5th June 1848), several of them instrumental, and with the best effect; and no bad consequence has attended it either to the mothers or children. All the children have been born alive. In the cases in my own practice in which I have used it, it speedily induced the anæsthetic state, and appeared to accelerate the uterine action in some. The recoveries of the mothers were more favourable than in former instances where no such agent was employed. In all, the children were born alive, except one, where the child was *acephalous*.

"To explain the small number of cases in which it has been used here, I ought to mention that, in consequence of the deaths of two ladies in a respectable rank of life, from puerperal fever, when it was epidemic a few months ago—in both of which cases chloroform was used—the public, of course, attributed the unfortunate result to the new agent; and since then it has been difficult, and in many instances impossible, to overcome the prejudice against it. Of late, however, from the favourable opinion entertained of it by those who have had it administered, it is not so frequently objected to—Yours, etc. GEO. PATON."

Dr. Anderson, president of the Medico-Chirurgical Society of Glasgow, and one of the most experienced and esteemed physicians in that city, some time ago wrote me a letter, from which I make the following extract:—

"Not being connected with any obstetric institution, and having now little practice of this kind amongst the poor, I am sorry I cannot offer you any sufficient statistics on the use of chloroform; but I have had so much reason to be satisfied with its effect, that I almost always use it, and most of my patients have expressed themselves as strongly in its favour as you have done. I recollect only two exceptions, where, although the usual effects were produced, both patients complained that, previous to the full anæsthetic effects, their feelings, instead of being agreeable, were very much the reverse. One lady said she felt it like approaching insanity; and that she would rather endure the labour pains than be subjected to this again. In no case have I had reason to suppose that mother or child suffered injury. All the mothers have done well, and several of them have spontaneously remarked that they made better and quicker recoveries than after former accouchements. In this I coincide. Two children were still-born. One, in the sixth month, died during parturition, after long-continued discharge of the liquor amnii for many weeks before the induction of labour; the other, in the eighth month after hemorrhage from fatigue, followed by protracted labour; in this case a placental clot was found after delivery, so that the child's death was sufficiently accounted for.

"On the whole, my experience of chloroform in parturition is more favourable than in other cases, where I have several times seen it produce alarming spasmodic and other nervous symptoms, followed by sickness. These effects, I think, are most apt to occur in subjects who have had spinal irritation, or an excitable state of the nervous system, or who use the chloroform when exhausted, or after abstinence from nourishing food.—Yours, etc.

"A. D. ANDERSON."

The following communication was sent me in June last

By Mr. SPENCER, Douglas, Isle of Man.

"I forget how often I had used the chloroform when I last wrote you; but, since then, I have used it frequently, with invariable success so far as it is concerned. There have been no still-born children from its use, and all the children have continued to thrive well; none as yet have had a fit from teething—this I consider a great matter. We want some powerful proof here to bring it into general use. I cannot use it in half my cases, as I would not do it without leave from the patient. I had a case of puerperal mania the

other day, and did *not* use it in it. If I had used it, I should have been blamed. This is so small a place that everything one does is at once known. I have often operated under its use, with no ill effect.—Yours, etc. J. H. F. SPENCER."

From Mr. CEELY, Aylesbury.

"I have much pleasure in communicating to you the general results of my very limited use of anæsthetic agents in midwifery. I have used both ether and chloroform in numerous operations, small and great, in public and private practice, have been several times the subject of their influence, and have repeatedly employed them in hysteria, epilepsy, and other nervous and spasmodic affections; but my employment of them in midwifery has been more limited, and at present restricted chiefly to primiparous and difficult or tedious cases. I have used them in only six cases as yet.

"It has happened that most of my late obstetric cases have been so easy and so rapid as to supersede the use of chloroform. My own observations of its effects in midwifery would induce me to *urge* its use in all *severe, protracted*, or *operative* midwifery; and in any case of natural labour, with only the ordinary suffering, I would not hesitate to employ it, with the concurrence of the patient or friends, if I saw no contra-indication; because I believe that, in proper doses, with suitable management, it is harmless, and may be beneficial; but if the patient, under these circumstances, were averse, I should not, of course, press it. Neither do I think I should venture to employ it in natural and easy labour, where I suspected or knew that subsequent hemorrhage would arise, from flabbiness of fibre and flaccidity of the organ; because I think the *secale cornutum*, during the last hour or two of such labours, has in my hands been so beneficial, and seems more appropriate.

In all the cases in which I have employed ether or chloroform, the patients and their friends have been perfectly satisfied and gratified with the results, and abundantly thankful for the boon. In only one case has there been any sickness or syncope. In a town about twelve miles hence, it has also been successfully used by Mr. Knight of Brill, in several cases. In Bicester, I have heard of its use, but chiefly by younger medical men.

"I will only add that, in using the chloroform, I have, in some of the cases, now and then omitted to repeat the dose, to show the friends and attendants how differently the patient was situated; but

the latter has soon removed any hesitation of the friends by calling out hastily for that 'nice chloroform' again.—Yours, etc.

"ROBERT CEELY."

In a printed pamphlet[1] obligingly furnished me some time ago by Mr. Stallard, surgeon to the Leicester General Dispensary, that gentleman observes—

"I have exhibited the chloroform in upwards of thirty cases of midwifery during the present year, and they have included a greater number of severe cases than the general experience would support; this is accounted for by the fact that two of the worst were pauper patients, to whom the surgeon's attendance is required only in cases of unusual severity. One other formidable case also occurred to my father, and in it I was requested by him to use the chloroform. In this experience I have never once observed the least retardation of the parturient paroxysms, *so long as absolute insensibility was not induced;* and in several cases the pains were palpably increased. When a patient is suffering acute pain she does all in her power to suppress it, and the act of doing so is well known to retard the process of parturition; when, therefore, the pain is no longer felt, the effort to suppress it no longer ensues, and the accouchement is accelerated in a remarkable degree. But again, it has been argued that the pain of labour is desirable; and that it is wrong to interfere with this natural indication of what is going on. Now, with this assertion I am totally at variance; there is nothing so depressing to the powers of life; nothing so soon exhausts the already feeble body; nothing so effectually prevents the sufferer from aiding nature by her volitional effort, as pain; and, consequently, nothing so powerfully maintains the strength, or invigorates the system, as its annihilation. . . . In the thirty cases I have attended I have not had a single case of flooding, and two individuals had never been free from it on former occasions. This is, in my opinion, an additional recommendation for its exhibition; and in all those excitable persons in whom flooding is most likely to occur, the diminution of fear and excitement caused by its exhibition has a direct tendency to prevent the flooding. Nothing but a very extended experience can justify any conclusion upon this head; as far as my own experience goes, I am decidedly of opinion that chloroform exerts no perceptible influence upon the child."

[1] *Practical Observations on the Administration and Effects of Chloroform in Natural Labour*, p. 16, etc.

From Dr. PROTHEROE SMITH, London.

"I have records in my own practice and that of my friends of upwards of 125 cases of anæsthetic labour; and, with one exception, all have done well. In several thus treated, no hemorrhage has ensued, though in previous labours there was flooding. In nearly all, the getting up has been more speedy, requiring no aid of opiates and purgatives; and it is my sincere conviction that chloroform lessens the chance of puerperal inflammation and fever. I usually employ my inhaler to administer it, and with it I fancy I can administer it with greater success and precision. I have kept patients under its influence from half-an-hour to twenty-eight and a half hours. I have used it in cases in which bronchitis was present, and one lately, in which, at the time, there was complete aphonia. She has never had so good a time—in four or five previous births she had protracted recoveries, floodings, peritoneal attacks, etc.; these she has wholly escaped. I have used it in turning and forceps cases, etc., and in all I have been most successful. In administering chloroform I adopt your plan of inducing rapidly complete sleep, and afterwards keep up the effect by repeating the inhalations at each recurrence of uterine effort. The other day I had three cases, the one after the other, which strikingly illustrated the various effects of the chloroform on different subjects—1st, a primipara, æt. twenty (administered in the second stage), kept under its influence four and a half hours, slept sweetly, without any movement of the body, save during the last expulsive efforts, when the usual abdominal muscles were called into action. The 'pains' under the chloroform became more frequent and powerful, and a fine male child was born. After the expulsion of the child the patient awoke, refreshed as from a sweet sleep, without any pain or evidence (save the advent of her first-born) of having been confined, as far as her own feelings were concerned. The uterus contracted firmly, and I instantly left her for the second—a lady of title. She required to inhale for a minute and a half (in the third stage) before the induction of anæsthesia was complete, when she became very talkative, addressing her maid in French, speaking of matters wholly foreign to her condition, and of an amusing character, as though she was perfectly at ease and had nothing to do with the labour, which advanced rapidly, and terminated in an hour from the first exhibition of chloroform. On recovery, in about three minutes after, she was introduced to a fine boy, and remarked she had been sound asleep.

The third case had the chloroform in the *second* stage; she slept soundly in half-a-minute, and then became conscious of what was going on—described to the nurse, who stood by, the progress of the labour, and observed that she was greatly delighted that she had her senses, and yet was wholly unconscious of any pain or distress of any kind. She remarked, that with the entire freedom from suffering, she was sensible of the progress of the labour, as though it was effected by an agency with which she was altogether unconnected. During the last throe I made her sleep; so she was ignorant of the birth of a fine boy: in this and the second case the placenta immediately followed; the abdomen and uterus contracted firmly. In all three cases no after-treatment was required, and they make rapid recoveries.—Yours, etc. PROTHEROE SMITH."

From Dr. RIGBY, London.

"Of course I use the chloroform in midwifery, but I cannot think that a large dose at first is the best way. In almost all private patients, you can draw the line between loss of pain and loss of consciousness. I give but a small quantity at a time; the patient takes it herself (on a strip of sponge), and when it is exhausted she asks for more.—Yours, etc. E. RIGBY."

From Mr. LANSDOWNE, Bristol.

"I have now used ether or chloroform in seventy-one midwifery cases; I have two modes of administering it, the one with a bladder in which is placed a brass pipe with a stopcock, and into this is screwed, after I have poured the chloroform into the bladder, a piece of elastic tubing with a mouthpiece, the whole being pierced with a bore $\frac{3}{8}$ of an inch, through which the vapour can be readily inhaled. If I find I am likely to be giving the chloroform for a long time, I use the apparatus, both for the sake of convenience, and also of economy, as ℥j will last me nearly or quite an hour with this; and, should I use it many hours, it not only effects a great saving of material, but does not so frequently require replenishing, and is always ready at the approach of each separate uterine action; and it may (as has been the case with me) be used by any friends, or by the nurse, should the practitioner require to be absent for a short time. The other apparatus is an inhaler, such as is commonly sold; it is made of a thin and pliable lead, adapted over the nose and

mouth, having a piece of perforated zinc in its front, and containing a piece of sponge, over which the chloroform is thrown: the depth of this inhaler is such as to prevent the nose being touched by the chloroform. It is home manufactured, not expensive, and very easy of construction. This latter I make use of if I am likely to be wanting it for a short time only; it requires to be supplied afresh every five or ten minutes, and, accordingly, I use ʒj or ʒss, which latter is my quantity when about to extract a tooth. If the action of the uterus causes great pain, as is frequently the case in an early period of the labour with the first child, I commence giving it as soon as the os uteri is sufficiently dilated for the head to pass; I have given it when the opening has not exceeded the size of half-a-crown. I believe it may be given with impunity as early in the labour as we please, and the only obstacles to its being so used that I can see, are the inconvenience to the medical attendant in being thus occupied with one patient for such a length of time, and also the very great expense which such a lengthened use of it must entail. On the patient's account, I can see no possible reason why it may not be used for a whole day, or even more; indeed, I cannot see why a limit should be set to the length of time in which it may be used. I have no doubt but that it will soon be the anodyne generally used at the latter stages of painful cancerous diseases. The greatest length of time in which I have used it has been $16\frac{1}{2}$ hours, a fresh inhalation being made at every renewal of the action of the uterus; in other cases, I have given it $11\frac{1}{2}$ and 12 hours, and the only reason of the inhalation being limited to this time, has been the cessation of the necessity for its use, namely, that the child has been born, otherwise it would have been continued until such event had taken place.

"I have found that nearly all my patients have recovered very rapidly; most of those who have had children previously, have been astonished at the unusual rapidity of their recovery.

"I find no difference as to the expulsion of the placenta and the subsequent discharge, when administering chloroform, to what takes place in the usual natural labour. I have, upon two occasions, used it for very severe after-pains—pains so severe that their cries could be heard at a considerable distance; indeed they appeared worse than the pains of actual labour; in both cases the pain was completely subdued by its use. Both these persons had determined not to avail themselves of the benefit of the chloroform during labour, neither did they, but they were delighted afterwards with its sooth-

ing effects. The former of these I had long resolved to give it to for this express purpose; it was her thirteenth child; her labours have always been very rapid, scarcely any pain accompanying them; but no sooner has the child been born than her agony has been almost past bearing, the pain recurring at intervals for a fortnight. Upon this occasion I gave it to her three times within the first ten hours, and she had nothing to complain of afterwards.

"As regards sickness, I have not found that symptom, except where fluid has been previously taken; on the contrary, if the patient has been sick, the chloroform has almost invariably checked it. The cramp I have not heard them suffer from whilst under its influence. I have never yet met with anything which has caused me to regret having used it.—Yours, etc.

"J. G. LANSDOWNE."

During the course of last winter and spring, the practice of anæsthesia in midwifery was tried in London by a very intelligent young Russian physician, Dr. Haartman of Helsingfors, while, for the sake of information, he was living as house-surgeon in the great Lying-in Hospital at Westminster. Dr. Haartman subsequently visited Edinburgh, and kindly drew up for me, before leaving England, the following account of the results of his practice. I have much pleasure in publishing his letter, both on account of its own intrinsic value, and because the results of the practice of anæsthesia in this Hospital have been in no small degree misrepresented, both publicly and privately.

RESULTS OF ANÆSTHESIA IN THE WESTMINSTER LYING-IN HOSPITAL.[1]

"I fulfil with the greatest pleasure your wish, in writing down my observations on chloroform as used in midwifery. I do it certainly not without much embarrassment, the greater the more I think of my ignorance of the English idiom; but, encouraged by your indulgence, I do not hesitate to begin the task, more especially as I know that some erroneous ideas of the use of the chloroform in the General Lying-in Hospital in London have been spread about.

"During the three months I was house-surgeon in that hospital, I had the opportunity of observing 105 women in labour, of whom about 25 were attended by my friend Dr. Delafield of New York,

[1] From *Edinburgh Monthly Journal of Medical Science*, October 1848, p. 225.

the other 80 by myself. But having been most of the time the only house-surgeon, and otherwise much engaged, I was not able to exhibit the chloroform oftener than in 25 cases, which, I need scarcely say, were in no way selected. The most of these patients, were, however, *primiparæ*. In the administration of the chloroform I followed your method, using simply a towel, upon the interior of which the liquid was diffused; but I held it as a rule to exhibit it in the beginning of each case rather in small doses, and with caution, watching, in the meantime, the pulse, as the surest indicator of any danger. But when the patient got accustomed to it I gave it freely, not removing the towel till the patient was in deep sleep. In this state, the respiration was in general sonorous, sometimes stertorous; the pupils were usually somewhat contracted; sometimes, however, dilated; sometimes not at all affected. The pulse was, in the beginning, either a little accelerated, or quite natural, I think, when the patient was not frightened. Yet I have seen the pulse, without any bad consequence, go down to 40, when the chloroform was used for a long time or in large doses. Its strength was very seldom altered, being then weaker. The patient usually lay quiet, all the muscles being relaxed till the uterine contractions came on, during which they all, with few exceptions, pressed down as in common labour. Some of them complained, although they afterward said, 'they felt no pain whatever.' Few talked or laughed, except the Irishwomen, whom I found rather refractory to the anæsthetic influence, probably because they are, in general, too much accustomed to drinking spirits. All the cases were, with few exceptions, observed with the greatest care, and both the duration, the frequency, and the nature of each uterine contraction, both before and after the chloroform was exhibited, were marked down. Thus I find, by comparing all my annotations, arranged in tables, that during the full effect of the chloroform the uterine contractions became less frequent, and, I should say, less powerful, but, when the effect had passed off a little, they then became more frequent and shortened in duration; for example, if the interval between two uterine contractions before the use of the chloroform was 6 minutes, and the duration of each $1\frac{1}{2}$ minute, both these periods were reduced after its exhibition. The interval, from 6 to 5, 4, or 2 minutes, and the duration of each pain from $1\frac{1}{2}$ to 1 or $\frac{1}{2}$ minute. Besides, I think that in this state the uterine contractions in general came on more suddenly than in common labour, a circumstance which, however, may depend on an illusion, by the absence

of pain existing and announcing the action of the uterus in the usual labour. I have never observed the uterine contractions quite arrested, yet often somewhat delayed. The supposed relaxation of the soft parts seems to me at least uncertain and accidental. In two cases in which I thought the chloroform had very much relaxed the rather rigid parts, I had, notwithstanding, rupture of the perineum. Among the 25 cases of chloroform, I had only in one a slight hemorrhage—a fact the more remarkable, as at the epoch at which I used chloroform most frequently, I can say I had hemorrhage, and that often very severe, in almost all the other cases in which chloroform was *not* used.

"After the delivery, I found the patient in general quite unconscious of any occurrence during the anæsthetic state, some of them saying, nevertheless, that they had felt something going on, although they did not know what. They seldom complained of headache, and it usually disappeared during the following day. The mothers recovered all speedily and perfectly, with one exception, worthy your attention, although I am fully convinced that the accident by no means was the result of the chloroform, of which only two drachms were used—my provision at that time not being larger. During the use of the chloroform this patient was, as usual, insensible and quiet; but, for want of chloroform, the effect could not be kept up for a long time, and she was delivered with the usual pains. She was the following day perfectly well, and continued so till the eighth day after the delivery—she had then complained to the other patients of headache. I was, however, not called for, before she, in the afternoon, had had a fit of what is commonly called apoplexia nervosa, after which she got paralytic on the right side. I need not give you a tedious description of the proceeding and the treatment of the disease; allow me only to state that the patient began, by the use of strengthening medicine, to walk about in a month.[1]

"Of all the 105 children, six were either still-born, or died sooner or later after the birth—two only of them belonging to the twenty-five cases of chloroform. But, before I relate these cases,

[1] The "puerperal paralysis" was in this, as it is in most other cases, probably connected with albuminuria. One of my patients, who was confined for the second time four months since, was attacked two years ago with hemiplegia immediately after the birth of her first child. I have a patient at present under my care, from Forres, who several years ago was attacked with hemiplegia at the time of delivery.

I beg permission to point out some general remarks. In the majority of the cases I could not discover any change whatever upon the child, some of the children being rather bloodfull, others anemic—states depending, I think, as usual, on the duration and the nature of the labour. In one case, however, in which Dr. Fergusson was present, the newly-born infant was found rather strange, or to use the eminent doctor's expression 'tipsy.' It breathed less frequently and more abruptly than usual, the sound of its scream being rather singular. It recovered, nevertheless, perfectly. I must here observe that the mother (Irish), who, after the confinement, had brought up a large quantity of gin and broom, confessed having taken spirits before admittance into the hospital. Of the two children who died, the first was rather weak and thin, but continued pretty well till the seventh day, when it got peritonitis, and died on the tenth day after birth. The second, a large and fat child, died four hours after birth; in this case, when the head was born, I observed around the neck small blue spots, which I found to be blood extravasated under the skin. After birth the same kind of spots began to appear over nearly the whole of the body. The child breathed well in the beginning, but would not suck. It died without convulsions. By the *post mortem* examination, the spots were found to be blood extravasated from the capillary vessels of the skin; a large quantity of half-coagulated blood was found in the cavity of the peritoneum, and small blood-coagula in both lateral ventricles of the brain. The lungs and the brain were slightly congested. The blood seemed to be quite natural, and had no anormal smell.[1]

"Before I finish, I beg to add that I have used the chloroform in a case of turning with the greatest advantage. I was called out from the hospital to a poor woman in labour, and found a presentation of the cord, and the uterus so contracted that I could nowhere introduce my hand, although I made repeated attempts without

[1] This special case of purpura has been often mentioned. Every one acquainted with infantile pathology knows that purpura is not a rare occurrence at birth, as the cases and observations of Andral, Billard, Otto, Lobstein, and Graetzer, etc., amply prove. A patient who came some time ago from London, to be confined in Edinburgh, under my care, was gravely assured by a distinguished London physician, that if chloroformed, her child would be sure to be, as they all were in the Westminster Hospital, "either dead-born, or with their blood in a putrid and dissolved state!" The want of the head in the anencephalic child born at Dundee (see Dr. Paton's letter, p. 230), might have been as logically ascribed to the mother's use of chloroform, as the purpura in the above instance.

success. I then gave a large quantity of chloroform, and I was quite astonished at the great change which took place, it being now uncommonly easy to perform the turning.—Yours, etc.

"CHARLES DE HAARTMAN."

Of the use of chloroform in midwifery in Ireland, I have few details. From Dublin, my friend Dr. Tyler wrote me, in June last, that the practice has not yet been much tried. He observes—

"As to the progress of chloroform in obstetric practice here, I regret to state that its virtues have not yet been fairly put to the test by any of our Dublin accoucheurs, owing to a dread of bad consequences resulting, although I am unaware of any case followed by such being brought home to it, except a rumoured one of *threatened* convulsions and spasms, where —— was administering it.

"As to the surgeons, I hear them all speak in the highest terms of it. The resident surgeon of Stevens's Hospital, Mr. Wilmot, told me yesterday that he administers it previously to every operation there, now in some hundred cases, without meeting with any untoward result.—Yours, etc. A. TYLER."

Subsequently, Dr. Tyler informed me that Mr. Shekleton, master of the great Dublin Lying-in Hospital, was trying the effects of chloroform; and, at a still later date, Dr. Denham, assistant physician in that hospital, while on a visit to Edinburgh, informed me that Mr. Shekleton had now exhibited chloroform in upwards of forty cases of labour, most of them of an operative or instrumental kind, and that the effects had been such as to induce him to go on with the trial of it. No unfavourable results, either as regards the mothers or children, have been observed to follow the use of chloroform. Some of the principal private practitioners in Dublin are also, Dr. Denham informs me, beginning to employ the practice.

Regarding the use of chloroform in midwifery on the Continent of Europe, I possess little or no information. Dr. Krieger, of Berlin, tells me that in that city most medical men are opposed to the practice of anæsthetic midwifery, but still, five or six accoucheurs constantly make use of it. Writing to me in July last, Dr. Krieger says—

"About five or six accoucheurs of this place, I don't think there will be more of them, use chloroform in almost every case they

attend; many more only in cases of morbid labour; the majority not at all. The cause of this curious occurrence—*curious*, because in *every* surgical operation chloroform is made use of—may be found, not only in the disinclination of the public at large to such extraordinary means in quite a natural process (as they take labour for), but perhaps also in the expense, chloroform being still a costly article. I don't know whether the preparation we get here is less strong than it ought to be, or not, but I seldom require less than six drachms or an ounce, sometimes more, for one delivery, and the price is as much as half-a-crown per ounce at apothecaries' shops. Professor Martin of Jena has used ether in seven, chloroform in ten cases of morbid labour, and cannot strongly enough recommend the latter anæsthetic agent, stating that he never witnessed but favourable consequences. I am sorry I cannot give you any more details about the progress chloroform has made in Germany—but the perplexing political affairs have so very bad an influence on scientific publications, that we get but very little medical news from anywhere on the Continent, and those only such as were long prepared before we fell victims to revolutions.—Yours, etc. E. KRIEGER."

In a letter which I have lately received from Vienna, the writer, Dr. Arneth, informs me that the use of anæsthetic agents in midwifery is, as a general rule, discountenanced in that city by the two leading professors of midwifery in the University, but that, in several late cases, they have used it in operative delivery. He further states, that in Würzburg, the use of chloroform in obstetric practice is more common. He states—

"While I am writing this letter, Professor Kiwisch, of Würzburg, tells me that in that place no woman is confined, neither in private nor in hospital practice, without having been chloroformed. In two cases of eclampsy, he saw decided effect of these vapours—viz. the fits subsided.—Yours, etc. F. H. ARNETH."

The preceding kind of evidence, in relation to the practice of anæsthesia in midwifery, in this and other countries, might, as I have already stated, be very easily increased and multiplied by a little more extended inquiry, and by an appeal to the experience of the numerous accoucheurs here and elsewhere that have employed it. But the evidence as it stands—and spontaneous as it is in most cases—is amply sufficient to show both the great extent to which

the new practice has already been adopted, and the great success that has attended it. Every innovation in medicine which implies, like the present, a violent and extensive change in existing doctrines and old-established practices, has always been, for a length of time after its introduction, stoutly decried and resisted. The history of the first introduction and subsequent progress of the three greatest modern improvements in practical surgery, midwifery, and medicine —viz. the ligature of arteries, the induction of premature labour, and the discovery of vaccination—afford sad but strong historical proof of this observation ; and we have many minor instances of the same constant enmity to change, in the bitter opposition which the first employment of antimony, ipecacuanha, cinchona bark,[1] and other medicines encountered. And I believe that I am correct in stating that probably no innovation, embodying so very direct and decided a deviation from all the former routine and rules of practice, as the employment of anæsthesia in midwifery implies, ever, in the

[1] The London physicians have, on several occasions, specially distinguished themselves by their determined and prejudiced opposition to all innovations in practice not originating among themselves. In the whole pharmacopœia, there is perhaps no one remedy which, at the present day, is acknowledged to be of greater value, or to have saved more human lives, than cinchona and its preparations. In the seventeenth century, the proper time and manner of using the cinchona bark, for the cure of the then prevalent intermittent fevers of England, was made out by Robert Talbor, a medical practitioner in Essex. When Talbor subsequently removed to London, and began to use with success the new remedy, in the cure of the common agues of the metropolis, he found that, as he gained in the favour of the world, he lost that of the physicians of London ; and apparently their persecution of him became such, that the King at last was obliged to interfere, and in the year 1678, King Charles II. sent a royal mandate to the College of Physicians, commanding the president, Dr. Micklethwait, "and the rest of the College of Physicians," not to give Talbor "molestation or disturbance in his practice." Among the list of London physicians averse to the new practice of curing ague by cinchona bark, De Bergen mentions the illustrious names of Sydenham, Harvey, etc. In 1698, a Dutch physician, Dr. Groenvelt, published a work entitled, *De tuto cantharidis in medicina usu interno.* A few years previously—viz. in 1693, when Groenvelt practised in London—the President of the College of Physicians imprisoned him in Newgate for daring to recommend and use the new remedy whose virtues he had discovered. Six or seven years after vaccination began to be generally used throughout England, Dr. Moseley, a member of the London College of Physicians, suggested to his College the propriety of putting down "the beastly new disease," as it was termed, of cow-pox ; and in 1805, he boasted that the middle and inferior classes of London had then "renounced the delusion." In the last number of a respectable London medical journal, a London medical practitioner questions whether the practice of relieving women, by anæsthetics, from the pains and agonies of parturition, should not "be considered criminal according to law."—See *London Medical Gazette* for Sept. 8, p. 424.

same short period, made such extensive way and progress as it has done among the profession. As a matter of course, however, it has called forth also abundance of published and unpublished opposition and objection. No small share of the resistance against it has taken the form of personal or professional abuse of me as the introducer of the practice. All that I most willingly pass over and excuse, as, judging from all past experience in medicine, it was nothing more nor less than I was entitled to expect under the circumstances.

CHAPTER V.

RESULTS OF THE PRACTICE OF ANÆSTHESIA IN MIDWIFERY.

Edinburgh, October 1848.

SINCE January 1847, up to the present time (October 1848), I have, in my own practice, delivered about 150 patients under a state of anæsthesia. The results to the children and mothers have been as follows :—

Results to the Children.—In the 150 cases, all the children were born alive except one. In this exceptional instance, the infant was expelled in a decomposed and putrid state, between the seventh and eighth month of utero-gestation. It had not been felt to move, nor had I been able to hear the fœtal heart with the stethoscope, for two or three weeks previously. The mother had, before the present pregnancy, borne several premature dead children. Though the infant was small, yet the suffering attending upon its expulsion threatened to be excessive, and, to relieve the mother of this unnecessary agony, I placed her under the influence of chloroform.

During the few weeks of my obstetric attendance after delivery upon these 150 cases, only one of the children died—namely, a child who sank under the symptoms of cyanosis. Nor am I aware that any of them has suffered, up to this time, from "cerebral effusions," or "convulsions," or "hydrocephalus," or any other of the affections which have been prophesied as certain to befall all such infants as would be borne in labours rendered painless by art. Perhaps it may be proper to add, that none of the children have shown any symptoms of what has been calmly averred, in more than one publication in London, as a possible, or rather probable, result of anæsthesia—viz. idiocy.—"Dixerit insanum qui te, totidem audiet."[1]

[1] How can we "know or ascertain the possible consequences of the use of such an agent on the brain of the child? And how can we calculate what may be the ultimate consequences of its action in reference to the development of the mental faculties?"—Dr. Malan, in *Lancet* for April 29, 1848.

"It is admitted by all that the pulsations of the fœtal heart are greatly increased during inhalation—indeed, to such an extent has this been noticed, that in some instances the pulsations could not be counted, so much were they accelerated.

Results to the Mothers.—Among the 150 mothers, the more immediate and direct effect of anæsthesia has been the alleviation or abolition of the physical sufferings attendant upon the latter stages of labour. And, certainly, if the object of the medical practitioner is really twofold, as it has always, till of late, been declared to be—viz. "the alleviation of human suffering and the preservation of human life,"—then it is our duty, as well as our privilege, to use all legitimate means to mitigate and remove the physical sufferings of the mother during parturition. The degree of these sufferings is, as a general rule, assuredly such as to call for this aid and mitigation. In proof of their severity, I might cite the unprejudiced testimony of various obstetric authors. At present, I shall content myself with one. Professor Meigs of Philadelphia—a declared opponent of the innovation of artificial anæsthesia in labour—when speaking of the sufferings of the mother in human parturition, fully admits their intensity. "*What* (says he) do you call the pain of parturition? There is no name for it but *Agony;*"[1] and he elsewhere speaks of the pains in the last stage "as absolutely indescribable, and comparable to no other pains."[2] Now, surely, if it be the duty of the physician (and who doubts it?) to relieve and remove the pains of colic, of pleurodyne, of headache, neuralgia, rheumatism, etc. etc., it is his duty to relieve pain so severe as to be "absolutely indescribable, and comparable to no other pains." There is not one code of humanity for one class of pains and patients, and a different and opposite code for another class of them.

From November last, when I began to use chloroform in labour, up to the present time, none of the patients, with one exception, at

Are not effusions to be feared from this? Are not convulsions after birth likely to ensue? And may not that occur which would make the most heartless mother shudder at the bare possibility of herself, by her want of courage, being instrumental in producing? May not *Idiocy* supervene? Of this we have as yet no experience, nor shall we have, perhaps, for years; but when one such case occurs, will there then be found any one who will afterwards be persuaded to submit herself to etherisation during pregnancy?"—Mr. Gream, in *London Medical Gazette* for 7th September 1848. It is perhaps superfluous to add, that the premises of the preceding paragraph are as gratuitous as its conclusions; and that the pulsations of the fœtal heart are little, if at all, increased in rapidity when the mother is anæsthetised. "The action of the child's heart," says Professor Siebold, "was found to continue quite unaltered, not the slightest change in its frequency and regularity being detected."—Siebold on the Employment of Ethereal Inhalations in Midwifery, in the *Medical Gazette* for 11th June 1847.

[1] *Females and their Diseases*, p. 49. I leave the italics the same as in the original.

[2] *Philadelphia Practice of Midwifery*, p. 153.

whose delivery I have attended, has been aware of these last "absolutely indescribable" pains; the state of artificial anæsthesia having always been induced for a longer or shorter time before their supervention. And I have kept up this state for a period varying from a few minutes to four, five, six, or more hours before delivery. In the exceptional case referred to, the patient's sufferings were greatly mitigated; but the state of anæsthesia was not, as usual, perfect and complete, the patient having been unexpectedly taken in labour when not in her own house, and the attendant anxiety and confusion of herself and her attendants being such as totally to preclude the requisite degree of quietude. When employing ether, I repeatedly saw cases in which the patients were thus only partially and not completely anæsthetised—where, in other words, they were not entirely asleep, but were aware of the presence of the uterine contractions, and sometimes experienced from them sensations in some degree painful, but of a very mitigated and blunted character.

Besides thus alleviating and abolishing the sufferings of the mother during labour, the practice of anæsthesia carries along with it other advantages. A number of patients have spontaneously told me, that the prospect of being enabled to pass through the ordeal of parturition with the assistance of anæsthetic agents, and without their usual painful agonies, has destroyed, in a great measure, that state of anxiety and dread of anticipation, which, in former pregnancies, had, for weeks and months previously, silently annoyed and haunted them. If we can thus add to the happiness of our patients, by imparting to them feelings of safety and immunity under one of the severest trials to which nature exposes them, we surely follow out, in its truest sense, that which Dr. Meigs correctly describes as the office of a physician—namely, "a great mission of benevolence and utility."

But the practice of anæsthesia in midwifery not only saves the mother from the endurance of unnecessary mental anxiety and unnecessary physical agony; it saves her also from some of the dangers attendant upon parturition, by husbanding her strength and warding off the effects of that exhaustion and nervous depression which the pains and shock of delivery tend to produce. In most cases the mothers, after delivery, on waking from their anæsthetic sleep, have expressed surprise at their own feelings of strength and perfect well-being; and many, who have borne children previously, have gratefully declared to me the great difference which they have found between their condition after being delivered under anæsthetics,

and without pain and suffering, and their state of prostration after former labours, when they were subjected to the endurance of all the usual "pangs and agonies" of parturition. Nor does the benefit end here. By annulling the parturient pains and shock, and their direct and primary depressing effects upon the constitution, we ward off, I believe, to a more or less marked extent, the chances and dangers of those secondary vascular excitements which are always apt to follow indirectly upon them. We increase the chances of a more speedy and a more healthy convalescence. And both patients and practitioners have, as a general rule, had occasion to observe that the period of convalescence has been evidently curtailed and shortened by the previous adoption of anæsthesia during delivery.

Such certainly has been my own experience. For, since following the practice of anæsthesia, my strong conviction is, that I have seen both more rapid recoveries than formerly, and fewer puerperal complications. One patient, however, had a short attack of peritonitis, requiring leeches, etc. It was her third accouchement and her first living child; and, after her two former deliveries, she had required to be bled, and treated for similar inflammatory attacks. At her first labour she suffered severely from puerperal convulsions. In two others of my patients the convalescence was delayed, in one by an attack of the affection described by Dr. Marshall Hall as "intestinal irritation" in the puerperal female; and in the other by a fit of jaundice, which supervened two or three weeks subsequently to delivery, and after the patient had been for several days in the drawing-room. In December and January last, an epidemic of puerperal fever swept fatally over Edinburgh and other parts of Scotland. During the period of its prevalence, two of my patients were seized with it and died. But the previous employment of anæsthesia in these cases had nothing to do with this distressing result. Some of my professional brethren here and elsewhere, who were not using ether or chloroform, were much more unfortunate than I was. In a district in the neighbourhood of Edinburgh, one of the medical attendants informed me that at that time above twenty mothers were attacked and died, and in none of them whom the disease seized upon did ether or chloroform happen to be used; while several who demanded chloroform during their labours all fortunately escaped. The first of the two cases which I met with was after a second labour. The patient's first labour was extremely tedious and prolonged, and, at last, symptoms supervened which demanded the delivery of the child by the forceps. In her second

delivery, the labour was much shorter; the second stage lasted only for about twenty minutes, and during it she was completely anæsthetised. For fifty hours after delivery she progressed most favourably; and after seeing her at that time with a pulse at eighty, and otherwise well, I was suddenly summoned, in consequence of extremely severe pain having come on in the uterine region after some muscular exertion. Rigors, rapidity of the pulse, tympanitis, etc., supervened, and she speedily sank, with all the usual symptoms of puerperal peritonitis. The second case alluded to was in a primipara. The labour was tedious, the pain severe, and the patient was anæsthetised for four or five hours before delivery. For some days after delivery she went on prosperously, until she became unhappily and greatly excited by discovering intemperate habits on the part of the monthly nurse who was taking charge of her infant and herself. A fit of convulsions (a disease to which, in earlier life, she had been long subject) immediately supervened, and recurred several times. Fatal febrile symptoms then set in, with tympanitis and excessive diarrhœa.

I may add that, in the period during which these 150 cases occurred, I have had under my professional charge 20 or 30 other cases of labour in which anæsthesia was not employed, from the rapidity and facility of the delivery, from the patient being too late to send for assistance, from an aversion on the part of patients to the use of anæsthetics, more especially when ether first began to be used during last year, or from other causes.[1] One of the children in these cases was still-born, and a second died two or three days after delivery. Two of the mothers suffered from crural phlebitis; a third had a severe attack of puerperal fever, but recovered. Two others died; one of them under an attack of puerperal convulsions and coma, which supervened fourteen days after delivery.[2] In the other fatal case, the patient, who had suffered much in her previous labours, came to the immediate neighbourhood of Edinburgh to be confined, and with the view of using chloroform. But the labour proved unusually rapid, and she was delivered before the call for assistance reached my house. Her recovery went on uninterruptedly for two weeks, when a severe attack of dyspnœa supervened. My friend Professor Miller, her ordinary medical adviser, saw her in my absence, and suspected some acute affection of the heart. When we visited her

[1] Since November last I have used chloroform in all the cases of labour, where I have been called in time, except two.

[2] See details of it in *Monthly Journal* for 1847, p. 213.

together shortly afterwards, the symptoms were then apparently those of acute endocarditis. She was submitted to the usual antiphlogistic treatment, and in four or five days felt again so well as to insist upon being allowed to rise, which was forbidden. In the course of a few hours afterwards, another fit of dyspnœa suddenly supervened, and before Mr. Miller reached the patient's house, she was dead. We did not procure an autopsy. If unfortunately she had used chloroform during the labour, as was her intention and wish, many of the objectors to its employment would have, I fear, unhesitatingly attributed the fatal issue in this case to its previous employment.[1]

In addition to the 150 cases of artificial anæsthesia that have occurred in my own midwifery practice, and to which the preceding remarks apply, I have witnessed, during the last eighteen months, a considerable number of instances in which anæsthetic agents were employed in consultation and hospital practice; and I have frequently had recourse to their assistance in various obstetric operations which I have been called upon to perform, as in the separation and removal of the placenta, in various cases of turning, in one craniotomy case, and in several patients who required to be delivered

[1] In order to show the caution that is necessary in reasoning upon cases of death apparently from the exhibition of chloroform during surgical operations, I may add that, since November last, scarcely an operation has been performed in Edinburgh without previous anæsthesia, except where the throat or mouth was the seat of incisions, or the operation itself slight and trivial. Amid all the numerous patients thus operated on in public or private practice, when under the use of chloroform, no kind of misadventure or accident has happened; except one case of temporary fainting, a few minutes after recovery from the state of anæsthesia, be regarded as such. On the other hand, among the *few* exceptional cases in which, since November last, patients have been operated on in this city without chloroform, two have died on the table. One of the two was being operated on by Professor Miller for a hernia, which had been strangulated for about fifteen hours; when, after the skin merely was divided, the patient complained of great faintness, vomited, and died with the operation unfinished. This occurred on the 8th of November, two or three days after the anæsthetic effects of chloroform were discovered, and nearly proved the first operation in which it was tried. The second case, a patient of Dr. Pattision, had an abscess high up in the neck, requiring simple puncture for its evacuation. He died without hemorrhage, or admission of air, or other apparent cause, a minute or two after the puncture was made. If chloroform had been used in these cases, would it not by some have been blamed for the result?

Twelve or fourteen months ago, Professor Syme was performing primary amputation of the thigh in the hospital, upon a patient upon whom there was no sign of reaction, and who was not etherised for the operation. "Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of

instrumentally by the long or short forceps.[1] In all these varieties of operative practice, the previous superinduction of anæsthesia has appeared to me to be of the greatest and most undoubted benefit. For, besides freeing the mother from the additional corporeal suffering and additional mental anxiety attendant upon operative delivery, the state of anæsthesia enables the practitioner to apply any operative interference that may be necessary with more ease and facility to himself, and consequently also with more safety and success to his patient. When the state of anæsthesia is rendered adequately deep, it renders the patient quiet and unresisting during the required operative procedures ; it prevents, on her part, those sudden shrinkings and changes of position which the boldest and firmest woman cannot sometimes abstain from when her mind and body have been worn out, as happens in most operative cases, by a previous long and protracted endurance of exhausting but still ineffectual labour pains ;—the introduction of the hand into the maternal passages, or of the hand to guide our instruments, is greatly facilitated both by the passiveness and apathetic state of the mother, and by that relaxation of the passages which deep anæsthesia almost always induces ; and, lastly, this state of relaxation and dilatability renders the process of the artificial extraction of the infant through these passages alike more easy for the practitioner, less dangerous for the child, and more safe for the structures of the mother.

immediate dissolution. "Before tying," says he, "the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr. Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life ; but by and by the actions of the system were gradually restored, and maintained through the use of stimulants."—See *Monthly Journal of Medical Science*, vol. 1847-48, p. 76. Such dangerous symptoms, coming on in an anæsthised patient, might have been mistaken for the effects of the anæsthesia.

Some time ago, before either ether or chloroform was used in surgery, Dr. John Argyll Robertson was called, a few miles out of Edinburgh, to perform the operation for strangulated hernia. After having shaved the groin for this purpose, his patient complained of sickness and faintness, and died before any incisions were made.

Last year, Dr. Girdwood of Falkirk came to Edinburgh to see the practice of anæsthesia, in order to be able to apply it in a case of amputation. The day for the operation and anæsthesia was fixed ; but, some hours previously, sudden apoplexy came on, and the patient died. See also p. 152. [Ed.]

[1] In one case of placenta prævia to which I was called, the mother had lost much blood, and her lips were pale, and her pulse very weak. On administering chloroform, the circulation and pulse rallied ; I separated the placenta, no bleeding recurred ; and several hours afterwards the child was born. The mother made an excellent recovery.

Besides, in midwifery, as in surgery, the utility of anæsthesia before operating, is not, I believe, limited to the mere annulment and abrogation of conscious pain on the part of the patient, and the rendering of the operation itself more easy to the practitioner, but it adds to the safety of our instrumental or artificial interference. For, in modifying and obliterating the condition of conscious pain, the "nervous shock" otherwise liable to be produced by such pain, particularly wherever it is extreme in degree or duration, or intensely waited for and endured, is saved to the already tried and shattered constitution of the mother; and thus an escape is so far gained from those states of immediate vascular and nervous depression, and of subsequent febrile and inflammatory reactions, that are always apt to follow more directly or indirectly in its train.

PART VI.

LOCAL ANÆSTHESIA.

CHAPTER I.

PRELIMINARY NOTICE REGARDING ARTIFICIAL LOCAL ANÆSTHESIA.

Edinburgh, July 1848.

I HAVE already attempted to show[1] that the artificial production of a state of general anæsthesia before the performance of surgical operations is not altogether an idea of modern times.

The ancients, also, seem to have entertained the idea of the possibility of producing a state of *local* and limited anæsthesia in any part to be operated upon. Dioscorides, who, as we have seen, repeatedly mentions the production of *general* anæsthesia by mandragore, states it as a matter of report that *local* anæsthesia in a part was capable of being produced by the Memphian stone. "The Memphian stone," says he, "is found in Egypt near Memphis, of the size of a calculus, fatty, and of different colours. They say that this, when bruised, and spread over parts that are to be cut or cauterised, produces in them a dangerless anæsthesia."

Whilst we may entirely doubt whether local anæsthesia was capable of being produced by such an apocryphal application as the above, the passage is curious, as evidence that the idea of obtunding a single part of the body against the pain of an operation, was not unknown or unthought of in former times. Nay, many old authorities believed that against the fire-ordeal, any part of the body could be so protected and defended by previous applications, that the human hand, for instance, should not feel the contact of red-hot iron. The writings of Eusebe Salverte, and of Beckmann, contain ample notices on this disputed question. Upwards of half-a-century

[1] See *ante*, Part I. Chap. I.

ago, our countryman, Dr. Moore, ingeniously proposed to effect a colal anæsthesia of any limb that was to be operated upon by previously compressing with tourniquets and pads the nervous trunks going to that limb; and he has left us an interesting account[1] of a case of amputation at St. George's Hospital, in which this plan was tried, apparently with partial success, by John Hunter.

[1] See his *Method of Preventing or Diminishing Pain in several Operations of Surgery*, London, 1784.—"I communicated," says Mr. Moore, p. 30, "the experiments I had made, and all my ideas on the subject, to Mr. Hunter, who was so obliging as immediately to offer me an opportunity of trying the effect of my compressor, at St. George's Hospital, on a man whose leg he was to take off below the knee, within a few days. I went to the Hospital the day before the operation to try the instrument. The patient had lost all his toes, and had a large ulcer on his foot. This was so much inflamed, and so irritable, that dressing it in the gentlest manner gave him the most acute pain. I applied the instrument; after the compression had been continued for about half-an-hour his limb became so insensible that rubbing pretty smartly with the finger upon the ulcer gave no pain. Next morning, the patient being carried into the operation room, I began the compression of the nerves at a quarter before eleven o'clock. The numbness of the limbs followed at the usual time. At a quarter before twelve I gave him one grain of opium to diminish the smarting of the wound after the operation, when the compression should be taken off. A few minutes after twelve the tourniquet was applied, and the amputation performed by Mr. Hunter, at the usual place below the knee.

"At the circular incision through the skin, the patient did not cry out, change a muscle of his face, or show any symptoms of pain. At the subsequent parts of the operation, particularly during the sawing of the bones, he showed marks of uneasiness in his countenance, but did not cry out. As it was thought necessary to take up no less than five arteries, the operation lasted a longer time than is usual, and towards the end he grew faintish, and desired to have some water, and afterwards asked if they were nearly done. When the operation seemed to be over, and the bleeding stopt, the tourniquet was relaxed, and I also removed the compressor; but a small vessel bleeding unexpectedly, it was thought necessary to tie it also. Here the patient showed very strong marks of pain, and afterwards declared that tying this last vessel gave him much more pain than all the others, although the great nerves had been included in the ligatures. When he was put to bed the wound smarted, as is usual after amputations. The compressor being now entirely removed, this was to be expected. But some time after, being questioned concerning the pain he had suffered during the operation, he declared that he had felt hardly any, except (as he himself expressed it), at the rasping of the bones, which, he added, had shaken his whole limb. This seems a little extraordinary, as sawing the bones is usually the least painful part of amputations." * * * "This trial had all the success I expected; there was evidently a most remarkable diminution of pain, particularly during the first incisions through the skin and muscles, which are generally by far the most severe parts of the operation, and I am convinced that what pain the patient felt was chiefly owing to some small branches of the lumbar nerves which extend below the knee, and were not compressed." Perhaps some of our modern surgeons will not consider inclusion of the great nerves in the arterial ligatures as any compliment to Mr. Hunter's surgery.

The possible production of local anæsthesia by this or other means is certainly an object well worthy of study and attainment. Surgeons everywhere seem to be more and more acknowledging the facility, certainty, and safety, with which the state of general anæsthesia can be produced at will before operating, and the moral and professional necessity of saving their patients from all unrequisite pain. But if we could by any means induce a local anæsthesia without that temporary absence of consciousness which is found in the state of general anæsthesia, many would regard it as a still greater improvement in this branch of practice. If a man, for instance, could have his hand so obtunded that he could *see* and yet not *feel* the performance of amputation upon his own fingers, the practice of anæsthesia in surgery would, in all likelihood, advance and progress still more rapidly than ever it has done.

In the following remarks it is my object to state the results of a number of experiments which I have performed—1*st*, Upon the lower animals; and 2*d*, Upon man, with a view to the possibility of the production of such a state of local anæsthesia, by the local application of chloroform or other anæsthetic agents, to individua parts of the body.

CHAPTER II.

ON THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE LOWER ANIMALS.

Edinburgh, July 1848.

At a meeting of the Medico-Chirurgical Society of Edinburgh held on the 17th May 1848, I took occasion to state that I had successfully chloroformed several of the lower animals—annelida, crustacea, fishes, etc.; that in some, especially in the common earthworm (*Lumbricus terrestris*), I had been able to produce local anæsthesia by applying the chloroform vapour locally; and had thus, at will, rendered anæsthetic individual parts and portions of the worm, as the head merely, or the tail merely, or the middle part of the worm merely, the head and tail remaining unaffected.[1] At one of the February meetings, I stated that in the human subject local anæsthesia of a portion of the gums could be produced by rubbing the part with hydrocyanic acid. After the date of the above meeting I was led to make some additional experiments upon the possible production of local anæsthesia in man, which I here intend to detail at greater length.

Nothing could be more curious or satisfactory than the experiments alluded to on the production of local anæsthesia by the local application of chloroform vapour to different parts of the body of the earthworm. The resulting degree of local anæsthesia in the part exposed is generally, in the course of two or three minutes, most complete as regards both sensation and motion; in fact, after being sufficiently exposed, the chloroformed portion of the animal is

[1] Dr. Simpson stated that he had successfully chloroformed some of the lower animals—fishes, crustacea, annelida, etc., and last year he had etherised the sensitive plant (*Mimosa pudica*). He had found, for example, the common earthworm (*Lumbricus terrestris*) very susceptible of the action of the vapour of chloroform. Dr. Simpson further stated that he had found he could, with chloroform vapour, render anæsthetic individual parts of the worm merely, the head and tail remaining unaffected. He could produce this local anæsthesia in the worm by applying the vapour locally.—See *Monthly Journal* for July 1848, p. 48. [Ed.]

quite flat and flaccid, does not move under any irritation, and can be doubled and twisted upon itself like a piece of loose wetted cord. If the part paralysed by the chloroform is small, it will be dragged along by the movements of the other unaffected portions of the worm. Generally, in the course of a few minutes, it gradually regains its powers of motion, and its irritability and contraction under stimuli.[1]

The easiest method of performing this experiment, is to place a small quantity of chloroform in the bottom of a tumbler, paste over the mouth of it a covering of paper, and make an aperture in this covering sufficient only to admit the portion of the animal that is to be chloroformed. When held in this position, the part of the animal below the paper and exposed to the vapour of chloroform, is generally thrown into violent movements for a minute or two before the state of anæsthesia supervenes. I have repeated the same experiments with the vapours of sulphuric ether and of bisulphuret of carbon.

I have tried the same experiment with the same results upon the medicinal leech.

The results were, if possible, still more marked in another of the Articulata that was submitted to experiment. A small myriapode or centipede (*Julus sabulosus?*) was rendered completely anæsthetic and motionless in the posterior segments of its body, by exposing that part alone for a few minutes to the vapour of chloroform. The five or six last rings of the centipede, with the suspended and motionless feet attached to them, were, for a short time afterwards, dragged about in a kind of paraplegic state, by the brisk and lively movements of the anterior and unaffected portion of the animal. The animal soon and completely recovered, each segment with its corresponding feet regaining its power of motion; and this in regular order from before backwards.

In other centipedes experimented upon, a small quantity of fluid chloroform was applied by a slight brush to the head, or to two or three of the middle rings of the animal, or to the tail, and always with the effect of anæsthetising and paralysing the part or parts only with which the chloroform came in contact. Sometimes,

[1] These experiments become the more interesting in a physiological and toxicological point of view, when we recollect that in the Articulata the vascular system is general and distributed *longitudinally*, while both the nervous and respiratory system of this class of animals is, in a great measure, segmentary and *transverse*, like the action of the anæsthetic.

when the head and anterior rings were alone touched, the animal, after vainly trying to push its motionless head forwards, suddenly reversed the movements of all its limbs for a time, and dragged the paralysed head behind it. All of the centipedes experimented on recovered in a very short time from the effects of the chloroform.

By immersing the tail of the water-newt in chloroform vapour, the sensibility and motions of that part were rapidly destroyed, and returned in a few minutes afterwards. By a longer degree of immersion of the tail alone, the whole animal became anæsthetic; and in several experiments it was found possible, but difficult, to give the animal in this way a fatal dose of the vapour.

The hind-leg of the frog becomes anæsthetic when exposed for four or five minutes to the vapour of chloroform. Immediately after, it drags the limb in progressing, and bears, apparently without feeling, pricking and irritation of it; but a galvanic current passed through it excites both sensation and motion. In one case, the motory power of the limb was not restored at the end of the third day. No effect appeared to result from keeping the hind-leg of this animal immersed in strong tincture of Indian hemp.

One hind-leg of a healthy, active rabbit, was confined in a large bladder containing the vapour of chloroform. At the end of an hour the common sensibility of the limb to pinching and squeezing was much impaired; but a current of galvanism passed through it produced crying and signs of pain. The power of moving the limb seemed unimpaired.

The hind-leg of a guinea-pig, similarly treated, exhibited the same phenomena at the end of an hour; but the anæsthesia was more complete. The skin of the leg was red and congested.

The posterior extremities and pelvis of a strong guinea-pig were enclosed in a bag containing the vapour of chloroform. At the end of an hour, no signs of pain could be extracted by pinching and squeezing either limb; and a current of electricity passed through a hind-leg evidently caused much less pain than when the same current was passed through a fore-leg. The whole hinder parts were very red and congested. The animal was also in some degree paraplegic, and dragged itself along by strong efforts with its anterior limbs.

In an interesting communication addressed, on the 7th of June 1848, to the Yorkshire Branch of the Provincial Association, and published in the number of the *Provincial Journal* for June 28, Mr. Nunneley of Leeds has stated that he has produced complete para-

lysis of the individual limbs of frogs and toads, by immersion or exposure of them for a few minutes to the vapour of chloroform; that by a similar but longer immersion he had rendered the leg of the rabbit sufficiently anæsthetic to bear mutilation without pain; that he had immersed his own finger in anæsthetic fluids for about half-an-hour or an hour, and at the end of that time it was nearly powerless and insensible, nor was it entirely recovered for forty-eight hours; that in operating on the human eye he had rendered the organ anæsthetic by previously exposing it for about twenty minutes to the vapour of chloroform; and that, in his opinion, the action of all, or of most anæsthetic agents might be produced locally by their local application, the sensorium being unaffected, consciousness retained, and the limbs not subjected to their influence being unaffected. This naturally leads us to the consideration of—

CHAPTER III.

THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE HUMAN SUBJECT.

Edinburgh, July 1848.

EARLY in February 1848, I was led to make experiments relative to the artificial production of *local anæsthesia* in a portion of the human gums by friction with hydrocyanic acid, in consequence of being assured, on what I believed to be satisfactory evidence, that a dentist at Limoges, in France, M. Pernot, had the secret of extracting teeth with little or no pain, in consequence of previously applying some obtunding agent to the gums. I tried at the time a great variety of substances, in order to obtain this local anæsthesia, such as various ethers, bisulphuret of carbon, benzin, aconite, etc. Among all the agents employed the effect of prussic acid was by far the most decided and complete; any part of the gum strongly rubbed by it, speedily became benumbed and insensible; but the resulting degree of anæsthesia was by no means sufficient for the purpose required. The results of these experiments were stated orally to the Edinburgh Medico-Chirurgical Society, at their meeting on the 16th February 1848.

Before that date I had met with one instance in which local anæsthesia of the human hand had been produced in a young lady, in consequence of her accidentally holding in it for a considerable time a scent-bottle containing some chloroform. I tried at various times to reproduce a similar result in myself and in others, by keeping my hand wrapped in a napkin soaked in chloroform and other anæsthetic agents, but with little, or indeed no decided success, until I used the vapour of chloroform raised by heat, the hand for the purpose having been immersed in a deep jar, into which a small quantity of chloroform was poured—the jar temporarily placed in a basin of water of the temperature of 130° or upwards, and the wrist or forearm being surrounded by handkerchiefs, so as to prevent the escape of the vapour.

A number of circumstances influence, however, the effect and the degree of the state of local anæsthesia; and as I have made a considerable variety of experiments, both upon myself and upon others, in order to ascertain these points, I will shortly state the results. Let me premise, that in the experiments upon which the following results are founded, the hands of the same individual were generally held simultaneously in two different jars, differently arranged in regard to material or otherwise, in order to make two different and comparative experiments at the same time; and the relative degree of anæsthesia in each hand was ascertained, during the experiment, by pinching the fingers with the thumb-nail, without removing the hand from the jar. After they were removed, these and other more severe measures were used with the same view, as tests of the degree of anæsthesia. Most of the experiments referred to were repeated and tried upon several different individuals. The general results were the following:—

1. When the hand is exposed to an anæsthetic vapour, it betimes presents the sensations of a limb benumbed, by compression of its larger nervous trunks—the sensations, in fact, of partial or commencing paralysis. Usually, after a short time, a glowing or burning feeling is perceived in the parts most exposed, and gradually there supervenes a sensation of thrilling and tingling (like a limb asleep), which deepens more and more. The skin turns red, and the hand at last becomes stiff and clumsy, and feels as if enlarged, and painful impressions, as pricking, pinching, etc., are felt less and less than in the other unexposed hand. After the hand experimented upon is removed from the vapour, it is generally half-an-hour or more before its usual normal feelings are quite restored. The nerves of motion are usually apparently as much affected as the nerves of sensation.

2. When the jar containing the chloroform, or other anæsthetic fluid experimented upon, was immersed in warm water, so as to raise the vapour of the fluid more fully and quickly, the resulting anæsthetic effect was always greatly increased, both in rapidity and intensity.

3. The vapour of chloroform proved stronger than any other that was tried. When one hand, for instance, was immersed in a jar containing the vapour of sulphuric ether, and the other in a jar containing the vapour of chloroform, both jars containing similar quantities, and being subjected to the same degree of heat, the hand in the chloroform jar was both more speedily and more deeply

affected than the other. In addition to the vapour of chloroform and ether, I have tried comparative experiments with the vapours of aldehyde, bisulphuret of carbon, iodide of methyle,[1] etc. The aldehyde had little or no effect of any kind. The iodide of methyle produced a very severe burning sensation, and left the hand intensely red and erythematous for a day or two afterwards, but with no marked anæsthetic influence. Among several of us that tried the vapour of bisulphuret of carbon, only one bore it for any length of time (about an hour), and in him it did not render the hand anæsthetic in any very appreciable degree; in myself and others the sensation of heat and burning became so utterly intolerable, as to force us to withdraw the hand in two or three minutes. Immersion of the hand for half-an-hour in Dr. Fleming's very powerful tincture of aconite,[2] or in a strong tincture of opium, or of Indian hemp, or in a strong solution of belladonna, produced no appreciable anæsthetic effect.

4. The hand, when plunged in liquid chloroform, is usually somewhat more deeply apathised than the other hand simultaneously held in the vapour of chloroform. This was the more general result with those who tried the experiment; but in some the chloroform vapour was as anæsthetic, or more so, than the liquid. Few persons can keep the hand for any adequate length of time in liquid chloroform; the sensation of burning becomes so intense and insufferable as to force them to withdraw it in a very few minutes. On one occasion I held my hand for upwards of an hour in liquid chloroform without the part being more deeply apathised than it would have been by exposure to the vapour. One of my pupils, Mr. Adams,

[1] Two or three months ago I began a series of experiments with the intention of testing the anæsthetic or other therapeutic effects of the various respirable compounds of amethyle, acetyle, etc. I was prevented from proceeding far in the inquiry, in consequence of the effects following the inhalation of the compound mentioned in the text, viz. iodide of methyle. I found it very powerfully anæsthetic, but dangerously so. After inhaling a very small quantity of it for two or three minutes, I remained for some seconds without feeling much effect; but objects immediately began to multiply before my eyes, and I fell down in a state of insensibility, which continued for upwards of an hour. I did not completely recover from the effects of it for some days.

[2] Some other forms or preparations of this and the other substances may possibly produce different results, as we know that aconite, when chewed, causes a numbness and tingling in the lips and tongue, lasting for some hours. See Christison on *Poisons*, p. 2. In making this experiment with aconite, it appeared to me that the sensibility of the tongue, etc., was not decreased to pinching, etc., when the part was tingling from the effects of the chewing of the aconite.

perseveringly retained his hand in the liquid chloroform for upwards of two hours: no great or very marked degree of local anæsthesia resulted. In these cases, in which the hand was long steeped in liquid chloroform, the sensations of burning returned severely, from time to time, as if in waves, during the experiment; and on removing it from the jar, the feelings of heat were temporarily aggravated. The normal sensibility of the parts speedily returned, and was completely restored within an hour or two in all, but the skin sometimes remained red and injected for a longer period; occasionally for several hours.

5. The anæsthetic effect of chloroform, etc., is increased, both in rapidity and in degree, by immersing the hand, with the cuticle softened and moist. When one hand, for instance, is immersed without any preparation, and the other is prepared by being bathed and fomented for ten or twenty minutes previously, the latter almost immediately begins to tingle under exposure to the vapour, the dry hand not for some minutes. The degree of anæsthesia is also ultimately deeper in the moistened hand.

6. The degree of delicacy of skin in the person or part exposed to the anæsthetic vapour influences the result. In females I have seen the degree of the local anæsthesia of the hand that was produced, much greater and deeper than I could ever render it in the male subject. In applying the vapour of chloroform in small cupping glasses, etc., to different parts of the body, as the insides of the arms, etc., the resulting degree of local anæsthesia seemed, in a great measure, regulated by the tenuity of the skin of the part experimented upon. The skin of the axilla seems too tender to allow of the vapour being applied for a length of time sufficient to produce anæsthesia. One of my students, who kept both his lower extremities enveloped in strong chloroform vapour for three continuous hours, felt no appreciable local anæsthetic effect from it.

When strong chloroform vapour is locally applied to mucous surfaces, the attendant sensations of heat and smarting are too severe to allow of its sufficient continuance; at least, this is the result that I have obtained in applying it with small glasses to the inside of the lips, the tongue, and eye. Mr. Nunneley states, as we have already mentioned, that before operating on a difficult case of artificial pupil, he had applied for twenty minutes a small quantity of the vapour of chloroform to the eye, by means of a small jar, which accurately filled the orbit, with the effect of rendering the parts nearly insensible. Dr. Duncan and myself have

repeatedly tried this experiment upon ourselves; but in none of the trials which we made, with the eye either shut or open, could we endure the burning action of the vapour upon the part above two or three minutes, and with no other result except always rendering the eye experimented upon red and injected, and suffused with tears.[1]

7. The degree of anæsthesia produced in the hand by exposure of it to the strong vapour of chloroform, does not, in general, perceptibly increase after fifteen or twenty minutes. The same sensations continue if the hand is still retained in the jar; but an increased length of exposure does not, after a time, produce a corresponding increased degree of local insensibility.

But, finally and specially, I would observe that the degree of local anæsthesia produced in the human hand or skin, by exposing it to the local action of the vapour of chloroform, has never, in my experiments, been by any means so deep and complete in its character as to give the chance of annulling the pain of any severe operation, such as the deep incision or amputation of a finger. As compared with the other non-exposed hand, the chloroformed hand is generally rendered, to a marked amount, *less* sensitive; but the insensibility is never, I fear, so entire and perfect as will save the patient from the pain of the surgeon's knife. In short, I altogether doubt, whether, in the human subject, we shall ever be able to reduce the knowledge of this possible production of partial local anæsthesia to any practical purpose. It is principally interesting in a toxicological and physiological point of view, and in relation to the doctrine of the mode of action of anæsthetic agents.[2] Its

[1] I have tried the application of various anæsthetic gases and vapours to the vagina, in cases of vaginal irritation and neuralgia. The stronger forms cannot be borne. I was induced to try them in consequence of the following curious statement regarding carbonic acid, published by Dr. Pereira (*Materia Medica*, vol. i. p. 155).—"A lady who had suffered a considerable time from some uterine affection, and had derived no relief from the treatment adopted, was advised to consult a physician in Italy (Dr. Rossi). After he had examined the condition of the uterus, he assured her there was no organic disease, but merely a considerable degree of irritation, for which he proposed to apply carbonic acid as a sedative. This was done by means of a pipe and tube, communicating with a gasometer situated in another room. The patient obtained immediate relief, and although she had been obliged to be carried to the doctor's house on account of the pain experienced in walking, she left it in perfect ease. On her return to England she had a relapse of the complaint, and applied to Dr. Clutterbuck to know whether she could have the same remedy applied in London, in order to save her the necessity of returning to Italy.

[2] The distinction which exists between the structure and functions even of the

bearings are more upon the theory than upon the practice of anæsthesia.

These remarks relate to local anæsthesia as capable of being produced by the anæsthetic agents with which we are at present acquainted. Others may no doubt yet be detected, much more powerful than any we at present know,[1] and their local application may enable us to effect the local anæsthesia desired. At the same time, this consummation, even, seems doubtful; for, perhaps, any agent possessing a deeper and more rapid anæsthetic local power, would by absorption affect the system generally, and it may be, dangerously, before complete local insensibility of a part could be effected. Some time ago, in attempting to produce local anæsthesia in my hand, by exposing it to the vapour of hydrocyanic acid, ere the hand was much or very perceptibly benumbed I began to feel the constitutional effects of the poison; my respiration became irregular, and I felt giddy and faint, when my assistant removed my hand from the jar. All due care was taken to prevent me breathing any of the vapour, and I sat during the experiment in a current of air. I felt the benumbing influence of the acid extending from the hand upwards along the arm a minute or two before the experiment was stopped.

In addition to the liquids and vapours experimented upon, I have tried long immersion of the hand in various gases, as carbonic acid and common coal-gas (both of them powerful general anæsthetics when inhaled), without any effect. Chaptal, however, alleges

skin in the lower animals, as compared with the skin of the human subject, perhaps sufficiently explains the differences in the degree of local anæsthesia capable of being produced by the local application of anæsthetic vapours and fluids in the one and in the other. "In animals," says Dr. Jackson, see *Amer. Cyclopædia of Pract. Med. and Surgery*, etc., vol. i. p. 115, "whose skin is moist, and which possess a very delicate cuticle, cutaneous absorption is a constant and important function. Such are frogs, salamanders, and similar animals. The experiments of Edwards have established the skin in them to be entirely absorbing, and instrumental in their support."

[1] Perhaps some special modification in the application of electricity, galvanism, or magnetism, to the part to be locally anæsthetised, may be yet found capable of effecting this object. I have tried several experiments of the kind, but, as yet, without much success. The possibility of deep *local* anæsthesia existing in diseased states (as in colica pictonum, hysteria, hypochondriasis, etc.), without the general sensibility or consciousness being affected, is well shown in a late interesting essay of M. Beau, in the *Archives Générales de Médecine* for January 1848. He relates one case of an insane patient who broke his fibula, and continued to walk about without pain until the limb became inflamed and gangrenous. It was then amputated, and still without the patient apparently suffering in any degree during the operation.

that he had felt the limbs plunged in carbonic acid much benumbed; and Collard de Martigny[1] found that, when the general surface of his body was immersed in carbonic acid (arrangements being made so that none of it was breathed), giddiness, ringing in the ears, and the other symptoms produced by the action of this gas, supervened in eight or ten minutes, proving that it was absorbed. Davy[2] felt the premonitory exhilaration of nitrous oxide gas by exposing the surface of his body to it in the same way.

Chaussier[3] enclosed the leg of a dog in a bag containing sulphuretted hydrogen, and found that he could in this way, in a few minutes, induce the poisonous effects of this gas; and similar experiments were repeated by Lebkuchner[4] and Nysten,[5] and my friend Dr. Madden,[6] on the rabbit, etc., with similar results. I have held my hand enclosed in a bag filled with the constantly-renewed vapour of sulphuretted hydrogen for about half-an-hour, without feeling any local effects.

And even supposing that we could, by any means, so benumb the sensibility of a part to be operated upon as to render it anæsthetic, I doubt much if this state of local anæsthesia would place the parts in a condition at all favourable for being subjected to operation. It is quite possible, nay probable, that other more powerful agents than those which I have experimented upon, may, by their local application, produce a greater and deeper local anæsthesia on the human subject than I have yet witnessed; but all, or almost all, of those that I have used, have, along with the anæsthesia, led to such a congested and injected condition of the part, as to give every likelihood, both of a greater tendency to hemorrhage at the time of operating, and of a greater tendency to inflammation in it immediately afterwards.

Other and more simple agencies than any which I have mentioned are capable of producing a certain amount of local anæsthesia. A mere exposure of the hand, for instance, to great changes of temperature, either in the way of increase or diminution, has the power of deadening the sensibility of the part. I have tried, and seen

[1] *Archives Génér. de Méd.*, vol. xiv. p. 209.

[2] *Researches on Nitrous Oxide*, 1800, p. 485.

[3] Sedillot's *Journal de Méd.*, vol. xv. p. 25.

[4] *Diss. Inaug. utrum per viventium adhuc animalium membranas materiæ ponderabiles permeare queant*, p. 10, etc.

[5] Nysten. *Recherches Physiologiques*, p. 137, etc.

[6] See his excellent *Experimental Inquiry into the Physiology of Cutaneous Absorption*, p. 13.

others try, to hold the hand, with a view to this, immersed in broken ice, or dipped in ice-water, and with the effect of inducing a degree of anæsthesia in the part, as deep, if not deeper, than exposure of the same part for a greater length of time to the local action of chloroform would have effected. In his admirable and classical *Lectures on Inflammation*, the late Dr. Thomson remarks (p. 617), that "the sensibility to external impressions of the parts exposed to cold, is always more or less impaired, and the diminution in the sensibility of the nervous system seems to admit of degrees, from the slightest perceptible numbness to that of the most complete insensibility." But he also correctly adds — "This diminution however, of the sensibility to external impressions, is not unfrequently accompanied with severe degrees of pain." In fact, in making the experiments to which I have above adverted with the pounded ice, few of us could allow the hand to be retained in it above two or three minutes, in consequence of the intense pain which ensued; but still that brief period produced, as I have said, a most distinct and well-marked degree of local anæsthesia.

Keeping the hand immersed in water, of as high a temperature as it can conveniently bear, has also the same effect of rendering it, in five or ten minutes, partially anæsthetic and benumbed, as compared with the other unexposed hand; and this without the pain and suffering connected with the other extreme of temperature.

It is not my object here to inquire in what relative diseased or other conditions of a part, heat or cold are respectively calculated to act anæsthetically. But I would beg to make this general remark, that the action of the above, and of other applications, which we used to inflamed, burned, neuralgic, and pained surfaces and parts (such as preparations of opium, conium, aconite, belladonna, tobacco, etc. etc.), still require to be studied, which they have not yet been, in another and a new light, viz., as LOCAL ANÆSTHETICS. Therapeutical writers will, I believe, betimes consider and describe them in this novel point of view; and when attention comes to be directed to them with this object, some new facts and precepts may be elicited that will enable both the physician and surgeon to exhibit and apply local anæsthetics with more science, precision, and success, than hitherto.

The experiments and observations which I have detailed in the preceding pages, perhaps entitle us, in the present state of our knowledge, to draw the following conclusions :—[1]

[1] Through the kindness of Professor Balfour, I have had various opportunities

1. In animals belonging to the class of Articulata, complete local and limited anæsthesia can be produced by the local and limited application of the vapour or liquid of chloroform to individual parts of the body of the animal.

2. In Batrachian reptiles, the tail, or an individual limb, can be affected in the same way with local anæsthesia, by the local application of the chloroform; but, in addition, general anæsthesia of the animals usually results in a short time, in consequence of the chloroform absorbed by the exposed part coming to affect the general system.

3. In the smaller Mammalia a single limb, or even the whole lower or pelvic half of the body, can be rendered anæsthetic by local exposure of these parts to the influence of chloroform.

4. In the human subject, partial, and perhaps superficial, local anæsthesia of a part, as the hand, can be produced by exposing it to the strong vapour of chloroform; but the resulting degree of this local anæsthesia is not sufficiently deep to allow the part to be cut or operated upon without pain.

5. Any agent possessing a stronger local benumbing, or an anæsthetic influence, would probably be dangerous, by its acting too powerfully on the general economy, before the local anæsthesia was established to a depth sufficient for operating.

6. Artificial local anæsthesia, from any known anæsthetic agents, seems objectionable in any part intended to be operated upon, in consequence of the vascular congestion and injection which attend upon and accompany this local anæsthesia.

7. There are few operations in which there is not previously a local broken surface; and the application of chloroform, etc., to such a surface, would be far too painful to be endured, no small degree of suffering sometimes arising from even the exposure of the unbroken skin to their action.

of trying the effect of chloroform in vapour upon the sensitive plant (*Mimosa pudica*). When the vapour was either too strong or too long continued, the plant was destroyed. When it was weaker and applied only for a few minutes, the leaflets in some plants closed as when irritated, and did not expand again for an unusual length of time. In other plants, under exposure to the chloroform vapour, no closure of the leaflets took place, and, in a few minutes, the plant became so anæsthetised, that the mechanical or other irritation of the leaflets or stalk did not produce any of the common movements; nor did their irritability become restored for a considerable time afterwards.

CHAPTER IV.

PRACTICAL APPLICATION OF CHLOROFORM AS A TOPICAL ANÆSTHETIC TO MUCOUS AND CUTANEOUS SURFACES.

Edinburgh, February 1856.

In 1853, Dr. Hardy of Dublin published in the November number of the *Dublin Quarterly Journal of Medical Science* an interesting communication on the same subject, entitled, "On the Local Application of the Vapour of Chloroform in the Treatment of various Diseases."

The principal peculiarity in Dr. Hardy's essay consisted in the proposal of a special valved instrumeut—the Anæsthetic Douche as he termed it[1]—for the purpose of applying, in an intermittent stream, the vapour of chloroform to any part or surface that was wished to be affected.

But in projecting a stream of chloroform vapour upon any point, I have generally made use merely of a common enema syringe; and it will be found, I believe, to serve as well, if not indeed better, than any of the complex and expensive special instruments invented for the purpose. In fact, a larger and more powerful stream of vapour can be kept up by an enema syringe than by any of the special anæsthetic douches which I have seen.

Any of the usual forms of pea-valve enema syringe will answer the purpose, provided their lower or receiving extremity be immersed in the vapour of chloroform, and the instrument worked in the usual way employed for the transmission of water or other liquids. The

[1] Dr. Hardy's original Anæsthetic Douche "was formed of a caoutchouc bottle, having attached to one side of it a metallic chamber and egress-pipe provided with two valves to regulate the admission and the egress of air and vapour. The metallic chamber was perforated at the side to admit a sponge sprinkled with chloroform, and this perforation was closed with a screw stopper." See figure of the instrument in *Dublin Journal of Medical Science* for November 1853. Subsequently Dr. Hardy invented and used another Anæsthetic Douche, far more complex and expensive, of which he has given a description and drawing in the *Dublin Medical Press* for April 1854. It was proposed to take out a patent for the Douche.—(See *Medical Press* for April 26, p. 268.)

vapour of chloroform, etc., or rather of air loaded with the vapour, passes readily through the canal of the syringe, and is projected in an intermittent stream from its orifice.

The syringe which I have generally used for this purpose is the barrel syringe of Mr. Higginson. It consists of three pieces of caoutchouc tubing, the middle or thickest portion being provided at either extremity with the common pea or ball valve, and altogether forms, in my opinion, by far the simplest, most durable, and at the same time the cheapest description of syringe yet suggested for injecting fluids into the rectum or vagina. When used for the transmission of chloroform vapour it requires to be worked in the usual way for the transmission of liquids, but with its lower or inferior extremity placed in air loaded with the vapour of chloroform. In order to effect this last arrangement, all that is necessary is to place this lower extremity of the instrument in the neck of a phial or bottle containing liquid chloroform. The lower extremity of the barrel enema syringe is generally made of the size and form of the two last joints of the little finger; and the tube is encircled with a projecting ridge or shoulder above this point. When employed as an anæsthetic douche, this finger-like end of the instrument is passed into the neck of a chloroform bottle sufficiently large to admit it easily; whilst at the same time the circular projecting ridge of the tube rests on the mouth of the phial. For this purpose the common six-ounce phial or bottle, with a mouth four or five lines wide, answers perfectly. An ounce of chloroform placed in the bottom of the phial will enable it to serve as an anæsthetic douche for a long time. Before using it, the shaking of the bottle will impregnate the air in it more thoroughly with chloroform vapour. When patients themselves employ the syringe and bottle, perhaps it will be found necessary to explain to them that they are not to inject the liquid chloroform through the tube, but only the vapour of it, or rather air loaded with the vapour.

The preceding simple arrangement converts a common enema or vaginal syringe into an anæsthetic douche, equally, or indeed more, powerful than the ingenious instrument specially invented by Dr. Hardy for the purpose. As a proof of this, let me merely state, that in various trials upon various individuals, I have never seen the stream of vapour from Dr. Hardy's instrument, when fully charged, produce a state of general anæsthesia when the jet from it was projected into the mouth; but I have found that result to follow in some instances when the same experiment was made with the

stronger and more sustained stream of chloroform vapour sent through the common syringe.

When the inferior end of the enema syringe employed is of such a shape that it will not pass into the neck of a bottle containing chloroform, other arrangements may be required to supply it with chloroform vapour. For this purpose the lower end of the syringe may be placed upon the hollow of a concave sponge bedewed with chloroform; or a piece of lint, flannel, or the corner of a handkerchief, or other such material, freely wetted with it, may be lightly rolled around the lower extremity of the instrument. Sometimes, with the same view, I have placed the end of the syringe in the bottom of a cup or tumbler in which there was a bit of sponge or lint soaked with chloroform; for the vapour of chloroform being nearly four times heavier than atmospheric air, fills always the lower part of such a vessel. By any of these means a sufficient quantity of chloroform vapour can be supplied to fill the instrument, and to make a stream of it pass from its superior orifice, when the syringe is worked in the usual manner for transmitting liquids.

I have used the injection of chloroform vapour into the vagina by the preceding method in many cases of painful and neuralgic conditions of the uterine and pelvic organs. In most instances, after the first sensations of warmth produced by the injection have passed away, relief has been found to follow for a greater or less length of time; and to sustain this state of freedom from suffering, the injection has generally required to be repeated by the patient after the lapse of a few hours. This treatment has appeared to me more particularly useful in neuralgic states of the uterine organs and passages; in those organic diseases that are occasionally accompanied with suffering, as carcinoma uteri; in some cases of severe feelings of bearing down, and incapacity to stand and walk, complicated with displacements and enlargements of the uterus; and in various spasmodic conditions of the uterus attended with pain, as in threatened abortions; in after-pains; and most markedly in severe dysmenorrhœa. But at the same time I would beg to remark that in various instances in which the preceding morbid states were present, and in which I fully expected the usual anodyne effect of the vapour to be experienced, the treatment has failed to give the usual relief; probably because the mere superficial anæsthesia which results from the anæsthetic vapour was not sufficient in depth or in degree to produce an anodyne effect. In other instances, on the contrary, in consequence, perhaps, of the peripheral extremities of the nerves

distributed to the genital mucous surface being specially affected, or having a special reflex influence upon the deeper-seated parts and pains, the chloroform vapour has succeeded not only in producing temporary relief, but in producing even a speedy and a permanent cure, under circumstances where the previous duration and severity of the symptoms seemed, *a priori*, to forbid the hope of a restoration to health by this means alone. I had, for example, lately under my care a patient who, in consequence of severe pelvic or uterine pain, had been obliged to keep the supine position upon the bed or sofa for nearly six months previously. All attempts at standing or walking brought on renewed paroxysms of suffering. The uterus was slightly retroverted, but otherwise apparently healthy. After being brought with some difficulty to Edinburgh from a distant part of England, the only treatment to which she was subjected consisted of the injection of chloroform vapour several times a-day into the vagina, which at once relieved, and ultimately altogether removed, the uterine pains. Within a week the morbid sensibility of the parts entirely disappeared. There was, about a month subsequently, a short relapse, in consequence of indiscretion in exercise and exposure to cold, but the attack speedily yielded to the same treatment. I never had the pleasure of watching such a speedy and perfect restoration to health and happiness from that state of hysteralgia which so often entails upon patients misery and suffering for long months and years.

I have repeatedly applied chloroform to the maternal passages during labour in cases of rigidity of these passages, and particularly in rigidity of the cervix uteri when co-existing with morbid irritability and sensibility of the parts. In these instances I have used sometimes the chloroform vapour injected by the usual means; sometimes a few drops of fluid chloroform, mixed up with oil, or with a small solid mass of butter or ointment. The practice has appeared to me to be very often followed by two beneficial results—first, the abatement of the supersensibility of the maternal canals; and secondly, very often also with an increased secretion of mucus, and apparently an increased susceptibility to relaxation and dilatation in the rigid structures.[1]

[1] *Note on the mode of dilatation of the maternal passages in labour.*—During parturition the maternal canals—viz. the cervix uteri, vagina, and vulva—are, no doubt, dilated principally by the results of muscular uterine action and mechanical pressure. But they evidently become also dilatable and relaxed by another and an additional process, which is so far independent both of muscular

In the preceding remarks I have hitherto spoken of chloroform when applied as a local anæsthetic to the genital mucous canals. Its local anæsthetic action on other mucous surfaces has not yet been much studied. I have seen, however, the injection of the vapour of chloroform into the rectum useful also in some instances of morbid irritability and sensibility of the lower end of the intestinal canal, in tenesmus, etc. The mucous membrane of the eye seems in most individuals—especially in its diseased states—too irritable to bear the contact of very concentrated chloroform vapour, such as I employed in some early experiments alluded to in the last chapter; but in cases of photophobia and supersensibility to light connected with scrofulous ophthalmia, etc., the vapour of chloroform diluted with air or aqueous vapour, acts sometimes very markedly and beneficially as a local anæsthetic. I have seen the intolerance of light connected with ulcerative corneitis at once relieved by exposing the eye to the vapour of chloroform, raised by pouring a small quantity of the fluid into a cup of warm water. The patient will thus sometimes immediately be enabled to open the eye freely and without pain; and the chloroform vapour often serves also as the best possible medicinal application to the ulcerated surface.[1] The dentist can occasionally relieve the pain of toothache, by the local anæsthesia resulting from the application of a drop of fluid chloroform to the exposed interior of the tooth; or by directing a

action or mechanical pressure. In proof of this, we find the whole length of the canal of the vagina relaxing and widening during a protracted labour, *before* the head has yet passed the brim, or fully opened the os uteri. This vital process of dilatation seems to me to consist of a rapid development of cells within the tissues of the walls of the maternal canals—just as the thick mucous secretion thrown out upon the free surface of these canals during labour (and indicative, when present in great quantity, of great dilatability in these canals) is essentially, and in its ultimate physiological analysis, a rapid development of cells upon the free surface of their mucous coat. The application and stimulus of various substances, as simple warm water, of warm aqueous vapour, oils, simple or stimulant, etc., apparently promotes the dilatability of the tissues of the cervix uteri and vaginal canals, by promoting probably the more rapid formation of these cells. And from various cases which I have seen, I am led to believe that chloroform, both in the form of vapour, or of fluid diluted with oil or lard, will be found specially successful in producing this result, or at least—be the explanation what it may—in producing the required relaxation in cases of anormal or morbid rigidity.

[1] The vapour of prussic acid carefully applied to the eye, by means of a proper cup or glass, acts probably upon the same principle. It was much recommended some years ago by Dr. Turnbull; but indiscriminately in almost all ocular diseases. From what I have seen in practice, I believe that the use of dilute chloroform vapour and of carbonic acid will become common in affections of the cornea and conjunctiva connected with intolerance of light and supersensibility. They both act not as powerful local anæsthetics merely, but also as excellent medicinal applications to any existing ulcers, etc.

stream of chloroform vapour upon it. In painful and spasmodic states of the respiratory canals, when chloroform is applied to their mucous surfaces by inhalation, it is difficult, or indeed impossible, to tell always whether the resulting relief is the effect of local or of general anæsthesia. In some cases of spasmodic asthma, relief is occasionally obtained by doses too slight to have acted by any general anæsthetic effects; but I have seen other instances of the same disease where the paroxysm was not effectually arrested till a complete state of general anæsthesia was produced. A similar observation holds true with regard to different cases of laryngismus. Sometimes that troublesome affection, hysterical or spasmodic aphonia, is at once cured by a few inhalations of chloroform vapour, acting perhaps as much upon the principle of a local as of a general anæsthetic. The irritability of the cough in cases of phthisis, bronchitis, pneumonia, etc., is often effectually relieved by doses apparently too small to have acted otherwise than as local anæsthetics. Lastly, in reference to the topical anæsthetic influence of chloroform upon mucous membranes, let me add, that the swallowing of a few drops of fluid chloroform in oil, cream, soda water, or any other convenient vehicle, sometimes speedily abates nausea, vomiting, obstinate hiccup, etc.—perhaps upon the principle of its acting as a local and limited anæsthetic upon the walls of the stomach.

The preceding observations are limited to the local anæsthetic effect of chloroform upon mucous surfaces and canals. On the skin it produces a topical action, similar in principle, but far less in degree. When the epidermis is removed, or when the skin itself is destroyed, the surface of any existing sore, such as an irritable abrasion, an excoriated nipple, or a benign or carcinomatous ulcer, can be very remarkably anæsthetised and benumbed by the local application of chloroform vapour; but the feelings of great heat and pain, which in the first moments accompany its application, more than counterbalance, in most subjects, the subsequent sedative effects derivable from its use. The various experiments which I have detailed in the preceding chapter show that chloroform fluid or vapour, when applied to the unbroken human skin, produces a degree and depth of local anæsthesia, that is sufficiently great to be sometimes useful in medicine, while it is not sufficiently great to be useful in operative surgery. In medicine, for example, the local anæsthetic effects of chloroform often prove most beneficial in local neuralgia, local rheumatism, etc.; and chloroform mixed with equal, or with varying parts of olive oil, according to the sensitiveness of the patient's skin, is sometimes, in such cases, the most efficient

form of cutaneous topical anodyne which we can employ. The amount of local anæsthesia, however, thus capable of being produced, is not, as I have just stated, by any means deep enough to enable the patient to endure any operative or surgical procedure. In the earlier part of 1854, however, a variety of experiments was made in the Parisian hospitals, under the full belief that a stream of chloroform vapour projected against the skin might produce such an amount of local anæsthesia in any given part of the cutaneous surface, as would allow that surface to be cut or operated upon by the surgeon, without pain to the patient. Dr. Hardy's anæsthetic douche, or some modification of it, was the instrument usually employed in these experiments. Several alleged cases of the perfect success of this local cutaneous anæsthesia were published in the French journals. It was averred, for example, that M. Dubois had opened with the knife, and without pain, an abscess in the axilla, that M. Nelaton opened an abscess in the foot—the vapour of chloroform having in each case been previously applied to the skin ; and that M. Danyan, also without pain, made a caustic issue on the neck—the skin being prepared by the anæsthetic douche. But additional trials very speedily proved the inutility of the practice, as far at least as the possibility of producing by it immunity from the pain of surgical operations was concerned. At the end of these trials, in commenting upon the subject in the Parisian hospitals, M. Latour, the learned editor of the *Union Médicale*, observes—"I have felt, I avow, distressed and humbled with all the noise that has been made, and with the recital of all the numerous experiments that have been tried in this matter. I have not desired to accumulate the record of them here ; and I would wish that all trace of these facts were, for the honour of French physiology, blotted out as speedily as possible."[1]

In fact, the whole of these experiments and inquiries into the possibility of producing a sufficient amount of local anæsthesia for surgical purposes, by applying chloroform to the unbroken skin, resulted in the conclusion which I had already ventured to publish several years previously, namely, that "in the human subject, partial, and perhaps superficial local anæsthesia of a part, as the hand, can be produced by exposing it to the strong vapour of chloroform ; but the resulting degree of the local anæsthesia is *not* sufficiently deep to allow the part to be cut or operated on without pain."[2]

[1] *L'Union Médicale* for 4th March 1854.

[2] See this and other conclusions anteriorly, p. 268.

CHAPTER V.

CARBONIC ACID GAS AS A LOCAL ANÆSTHETIC.

Edinburgh, 1856.

CARBONIC acid is usually recognised by toxicologists as a very powerful narcotic poison, when inhaled in sufficient quantity. The rapidity and potency with which it acts as a general anæsthetic have long been described by travellers in the experiment of temporarily plunging a dog into an atmosphere of carbonic acid in the Grotto del Cane, at Pozzuoli.

Carbonic acid gas acts also as a local anodyne or anæsthetic. The anæsthetic effect of a stream of carbonic acid, when applied locally, is easily proved by holding any exposed portion of the surface of the body where the skin is thin—as the wrist or forearm—over a jet of the gas, escaping from a common gas-receiver. In a minute or two, the surface of the exposed part will be found benumbed, as when it is exposed to the vapour of chloroform; and pinching or irritating it is not followed with so much pain as a similar amount of pinching and irritation applied to the opposite and unexposed wrist or forearm. The degree of local anæsthesia capable of being produced in this way by the vapour of chloroform applied to a portion of the cutaneous surface, is, as I have already stated, not by any means deep and complete; but, in the relative experiments which I have made, it has generally appeared to me to be greater when a jet of carbonic acid gas was used, than when chloroform vapour was employed. And, like chloroform and other analogous agents, carbonic acid gas acts more powerfully when applied locally to mucous, than when applied to cutaneous surfaces.

In one respect, carbonic acid will be found preferable to chloroform vapour as a topical anæsthetic to the vagina and uterus. Though the application of the vapour of chloroform to the mucous membranes of the mouth, nose, pharynx, and bronchi, in the way in which it is usually inhaled, does not produce any very marked feeling of warmth in these mucous surfaces; yet, its introduction into the genital mucous canals generally creates a disagreeable, and, in

some instances, a very painful, though temporary feeling of heat and burning. The injection of carbonic acid gas into the vagina is not followed by any such painful sensation.

I have used carbonic acid as a local anæsthetic, principally in neuralgia of the vagina and uterus, in dysmenorrhœa, and in morbid states of the pelvic organs accompanied with pain, as in carcinoma, etc. I have found it also sometimes of use in irritable states of the neighbouring organs. Two years ago I had under my care from Canada the wife of a medical gentleman, who was suffering much from that most distressing disease—dysuria and irritability of the bladder. Many modes of treatment had been tried in vain. The injection of carbonic acid gas into the vaginal canal several times a-day at once produced relief, and ultimately effected a perfect cure. She has remained well since her return to America, and lately became a mother. Occasionally relief follows immediately. In two or three instances I have seen the use of the gas continued daily for months. I have notes of one case where the patient was invalided and almost entirely kept to the supine posture for years, from feelings of pain and bearing down in the uterus and neighbouring parts, particularly on attempting to sit or walk. Many modes of treatment were tried by myself and others, with little or no benefit. She has, however, at last regained in a great measure the power of progression, and freedom from suffering in the erect posture—a result which she herself ascribes to the local application of carbonic acid gas, which I recommended to her some months ago; and in the use of it she has since regularly persevered.

Since applying carbonic acid as a local anæsthetic application in uterine disease, I have met with two or three notices of this use of it by modern authors. In his work on *Diseases of Females*, published in 1835, the late Dr. Dewees, of Philadelphia, when discussing the treatment of carcinoma uteri, observes, "We have enabled several patients to derive much comfort, as well as temporary relief, from the extrication of carbonic acid gas within the cavity of the vagina, by means of a flexible tube of sufficient length and size, attached to the mouth of a bottle, in which there is mixed diluted sulphuric acid and the carbonate of lime. This may be introduced into the vagina several times in the twenty-four hours. In two or three instances this substance has relieved the severity of pain whenever it was employed, as well as diminished the offensiveness of the discharge."[1]

[1] *Treatise on the Diseases of Females*, p. 269.

In cases of painful menstruation or dysmenorrhœa, Professor Mojon of Geneva has recommended the local application of carbonic acid gas.[1] Dr. Mojon states that in the disease in question he has employed the injection of carbonic acid gas into the vagina in a great number of instances, and generally with decided advantage, the pain being almost always relieved by this treatment. He directs the remedy to be used two or three times a-day, and for five or six minutes each time. Like Dr. Dewees, he recommends the gas to be obtained by pouring dilute sulphuric acid on some pieces of chalk in a flask, from which a curved flexible tube carries it into the vagina.

In a paragraph which I have cited in a preceding chapter, from the second edition of the work of Dr. Pereira, on Materia Medica, carbonic acid is described as having acted speedily and beneficially in one case as a local sedative in uterine pain. "A lady," he observes, "who had suffered a considerable time from some uterine affection, and had derived no relief from the treatment adopted, was advised to consult a physician in Italy (Dr. Rossi). After he had examined the condition of the uterus, he assured her there was no organic disease, but merely a considerable degree of irritation, for which he proposed to apply carbonic acid as a sedative. This was done by means of a pipe and tube, communicating with a gasometer situated in another room. The patient obtained immediate relief, and although she had been obliged to be carried to the doctor's house on account of the pain experienced in walking, she left it in perfect ease. On her return to England she had a relapse of the complaint, and applied to Dr. Clutterbuck to know whether she could have the same remedy applied in London, in order to save her the necessity of returning to Italy."[2] This case and paragraph, however, seemed to Dr. Pereira of so little value, that he has omitted the details of it in the last edition of his work. But from the time of reading it I have tried at various times, and more particularly during the last two or three years, the local application of carbonic acid to the mucous membrane of the vagina and cervix uteri in different painful conditions of the uterus and neighbouring parts; and, whilst it has failed in some instances to afford the expected relief, it has in others proved, as I have already stated, of great, and occasionally of almost instantaneous benefit.

In practice I have generally used a common wine-bottle for the

[1] *Bulletin de Therapeutique.—American Journal of Medical Sciences*, vol. xxii. p. 469.

[2] *Elements of Materia Medica*, vol. i. p. 155.

formation of the carbonic acid gas, and formed the gas by mixing in the bottle six drachms of crystallised tartaric acid with a solution of eight drachms of bicarbonate of soda, in six or seven ounces of water. A long flexible caoutchouc tube conducts the gas from the bottle into the vagina. The cork fixing this tube into the mouth of the bottle should be adapted so as to prevent any escape of the gas by its sides. With this view the cork should be perforated by a metallic tube, and covered externally with a layer of caoutchouc.

The use of carbonic acid as a local anæsthetic to the uterine mucous surfaces and to other parts of the body is not a discovery of late times. I have found that in this, as in many other examples, what appeared to me at first novel, was, when fully investigated, a practice known previously in its essence, and perhaps in its more minute details also. Besides, here, as elsewhere, when once we detect a principle, such as the anæsthetic power of carbonic acid gas when applied topically, we can explain by it the good effects of modes of practice, which previously perhaps we were inclined to ridicule and reject.

1. In some, for example, of our oldest works on female diseases, as in the Hippocratic writings,[1] and in the chapters of Paulus Ægineta,[2] Rueff,[3] Paré,[4] etc., referring to uterine affections, directions are given for the relief of local pains, etc., in the uterus, by a system of practice which at least included, and perhaps which essentially consisted of the local application of carbonic acid gas to the mucous membrane of the genital canals. I allude to the burning of various herbs, aromatic and medicinal, and the application of the fumes arising from their combustion, by appropriate tubes and instruments, to the interior of the vagina. It is now known to all

[1] See Kuhn's edition of *Hippocrates*, vol. ii. *De Natura Muliebri*, pp. 567, 597, etc. *De Morbis Mulierum*, p. 859, etc. "Theophilus describes the process very minutely, but it will be readily understood that it consisted in introducing the fumes of strong-smelling things, such as frankincense, spikenard, cassia, and storax, into the vagina by means of a funnel." Dr. Adams's edition of the works of *Hippocrates*, vol. ii. pp. 742 and 748. See also on the Hippocratic method of fumigating the mucous surface of the vagina, Halle and Nysten in *Diction. des Sciences Médicales*, vol. xvii. p. 135.

[2] Adams's edition of *Paulus Ægineta*, vol. i. p. 642, etc.

[3] *The Expert Midwife* (1637), compiled in Latin, by James Rueff, part ii. p. 52, etc.

[4] Johnston's *Translation of the Whole Works of Ambrose Paré*, p. 944, with a description and figure of "a vessel made with a funnell or pipe for to fumigate the wombe."

that the combustion of dried plants and vegetable substances gives rise to the formation of carbonic acid; and the fumigations of the ancients, when they acted beneficially, probably acted much more by the mere topical application of this gas, than by anything aromatic or medicinal contained in the smoke of the burned ingredients.

2. Further, the knowledge of the marked local anæsthetic effects of carbonic acid gas appears to me to explain the good effects sometimes derived from a system of modern practice regarding which I formerly felt great scepticism. I have been often assured by patients of the soothing and sedative effects of the direct injection from the spring, of streams of various mineral waters, as practised at different German baths. It always seemed to me impossible that the slight amount of alkaline salts which these injection baths or streams contain, could be followed by the sedative effect so often ascribed to them. But from the inquiries that I have made, I believe it will be found that all the mineral waters which, when locally applied, produce this sedative or anæsthetic effect, contain a greater or less quantity of free carbonic acid, rapidly and constantly escaping from them; and the practice in its true therapeutic analysis probably consists only of the local application, in a somewhat clumsy form, of carbonic acid to the genital mucous surfaces. At some of the German watering-places, as Neuheim, Marienbad, etc., the large quantities of carbonic acid thrown off from the mineral wells has latterly been collected, and applied *per se* in the form of baths, jets, and streams, to different parts of the cutaneous and mucous surface—as to the uterus in neuralgia, etc., to the limbs when ulcerated, to the eye in irritable chronic ophthalmia, etc. etc. These uses of carbonic acid have been followed out, without, I believe, any tenable rationale having been suggested of the probable mode of action of the treatment. The utility of the practice, which I have been assured by my friend Dr. Funck of Frankfort is most marked in some diseased states, will find, I believe, its true explanation in the local anæsthetic effect of carbonic acid. And if so, it is scarcely necessary to add, the remedy may be artificially made, and readily applied at any time, and at any place, and in the practice of any physician.

3. A knowledge of the topical anæsthetic effects of carbonic acid serves perhaps also to afford an explanation of other points in common therapeutics. There are, for example, circumstances in medical prac-

tice in which we either incidentally or intentionally apply carbonic acid to the gastric and intestinal mucous membrane. In gastric irritability and nausea, physicians, since the time of Riverius,[1] have constantly been in the habit of prescribing effervescing saline draughts, or artificial aerated waters, which throw off a great quantity of carbonic acid, after their introduction into the stomach. In some instances the antacid action of the alkali may explain this effect; but is not the sedative action of these draughts, in most instances, dependent upon the local anæsthetic effect upon the mucous surface of the stomach of the large quantities of carbonic acid which they eliminate?

Dr. Pereira refers to Mr. Parkins's late recommendation[2] of the topical application of carbonic acid gas as a clyster in dysentery and diarrhœa. But neither Dr. Pereira nor Mr. Parkins seems to be aware that the practice is not altogether novel. As long ago as 1772, in a case of diarrhœa, complicating continued fever, Mr. Hey of Leeds threw up into the rectum, on two successive days, injections of carbonic acid gas. This practice had, as he supposed, the best effects—both the frequency and fœtor of the stools being apparently diminished by it.[3] Dr. Percival of Manchester also published in Dr. Priestley's work "two similar instances of the salutary effects of mephitic air thus administered as an enema." The same practice was also adopted in the last century by Dr. Warren of Taunton,[4] and Dr. Rotheram.[5] "May we not," asks Dr. Percival, "presume that the same remedy, viz. enemata of carbonic acid gas, would be equally useful in dysentery?"[6] Dr. Henry subsequently recorded two cases of dysentery in which the use of carbonic acid clysters was, in his opinion, followed by very marked relief of the abdominal pains and swelling, and a diminution of the fœtor of the discharges.[7]

[1] The celebrated anti-nauseant and anti-emetic potion of Riverius was composed of a solution of a scruple of salt of wormwood (an impure carbonate of potass), mixed with a table-spoonful of lemon-juice. He describes it as a "remedium præstantissimum, præsertim in vomitu qui febribus malignis solet contingere."—*Praxis Medica*, lib. ix. cap. 7; *et Centuria Observationum*, obs. 15.

[2] On the Efficacy of Carbonic Acid Gas in the Diseases of Tropical Climates, etc.—*London Medical Gazette*, vol. xviii. p. 777.

[3] See "A Letter from Mr. Hey concerning the effects of Fixed Air applied by way of Clyster," in Dr. Priestley's Work, vol. ii. p. 292.

[4] See Dr. Dobson's *Medical Commentary on Fixed Air*. London, 1787, p. 14. Dr. Priestley on *Air*, vol. ii. p. 375. Dr. Priestley's *Experiments, etc., on the Different Kinds of Air*, vol. i. p. 305.

[5] See Percival's *Essays*, vol. iii. p. 237. [6] Priestley on *Air*, vol. i. p. 305.

[7] *Experiments and Observations on Different Subjects*, p. 125.

4. Carbonic acid when applied to the surface of the body—the skin having been previously removed—acts as a local anæsthetic. A practice which probably depends for its utility on this principle has long been in vogue. The old yeast poultice (*Cataplasma cerevisiæ*) exhales from its surface a quantity of carbonic acid gas; and, perhaps, the beneficial and anodyne effects which surgeons formerly ascribed to it in the treatment of irritable and sloughing sores was in a great degree owing to the carbonic acid eliminated from it serving as a local anæsthetic and antiseptic. The constantly recurring motion, however, of points in the surface of the poultice, from bubbles of the gas mechanically raising and bursting it, does, no doubt, more than counteract, in some instances, any sedative effect that is derivable from the topical anæsthetic application of the carbonic acid. And perhaps far simpler means could be easily devised to keep an open and painful ulcer or wound in contact with a sufficient quantity of carbonic acid. Dr. Percival states that in the application of pure carbonic acid gas to a carcinomatous sore, the sanies of the cancer was "sweetened by it, the pain mitigated, and a better digestion produced."[1] Dr. Ewart of Bath applied it locally in two cases of ulcerated cancer of the mamma. In the first of these cases the ulcer, which was nearly five inches long, three inches broad, and about two inches at its greatest depth, entirely, though temporarily, closed up and cicatrised in three months, under the constant local application of carbonic acid gas. In the second case the cancerous ulcer was larger, very irregular on its surface, and discharged a thin mucous fœtid matter. It filled up and contracted somewhat in its dimensions under the use of carbonic acid; and at the date of his report, viz. at the end of two months' application of it, Dr. Ewart observes that at least the gas "has kept a person in ease and comfort, who for so great a length of time before had known only agony and torture." "What," he elsewhere observes, "strikes us in the two preceding cases with the greatest astonishment, is the almost instantaneous relief of pain, which never failed to follow the application of the gas."[2] The celebrated Dr. Ingenhouz, who saw one of

[1] Priestley's *Work on Airs*, vol. i. p. 302. Dr. Percival cites also the following case:—"A physician who had a very painful aphthous ulcer at the point of his tongue, found great relief, when other remedies failed, from the application of fixed air to the part affected. He held his tongue over an effervescing mixture of potash and vinegar, and as the pain was always mitigated, and generally removed by this vaporisation, he repeated it whenever the anguish arising from the ulcer was more than usually severe."

[2] Dr. Ewart's *History of two cases of Cancer treated by Carbonic Acid*. London,

these cases, in which a "large cancerous ulcer of the breast" had temporarily cicatrised under the local application of carbonic acid, and then re-opened, states that still the ulcer "gives no pain when she (the patient) applies the air."[1]

The application of the carbonic acid gas was, however, speedily recognised as not capable of producing a cure, but as capable of producing alleviation only. "The public prints," observes the celebrated French chemist Fourcroy, "contain accounts of several instances of the cure of cancer made in England by the application of the carbonic acid. We can, nevertheless, assert that this means has been used by ourselves and others, without success, several times. After the first application the cancerous ulcer exhibits a more favourable appearance; the sanies which commonly flows, becomes white, consistent, and laudable; the flesh assumes a lively colour; but these flattering appearances do not continue; the ulcer soon returns to its former state, and passes through the usual changes with unabated violence."[2]

In reference to the effects of carbonic acid upon raw surfaces and wounds, Dr. Ingenhouz mentioned to Beddoes the following experiment:—"Blister your finger, so as to lay bare the naked and sensible skin. The contact of air will produce pain; put your finger into vital air (oxygen), and this will produce more pain; introduce it into fixed or azotic air (carbonic acid or nitrogen), and the pain will diminish or cease." In relation to this statement, Dr. Beddoes informs us that he made the following experiments on three different persons:—*First*, The raised epidermis of a blistered finger, after all action from the cantharides had ceased, was cut away in carbonic acid gas. No pain was felt. *Secondly*, A second blister being opened in common air, smarting pain came on. In a bladder of fixed air, this pain soon went off. *Thirdly*, After opening a third blister, the finger was instantly plunged into oxygen. It felt as when salt is sprinkled on a cut. In carbonic acid gas the pain in two minutes quite subsided; but returned when the denuded skin was again exposed to the atmosphere.[3]

If there be no source of fallacy in these experiments, they cer-

1795, p. 48. *On the Medicinal Effects of Factitious Air*, part iv. By T. Beddoes, M.D., and James Watt, Engineer. *Table of Cases*, p. 4.

[1] See Part iii. of the *Essay of Beddoes and Watt*, p. 118; and Ingenhouz's *Miscellanea*, etc. 1795.

[2] Fourcroy's *Elements of Chemistry and Natural History*. Prof. John Thomson's translation, 5th edition, vol. i. p. 395.

[3] *On the Medicinal Uses of Factitious Airs*, pp. 43-45.

tainly point to one kind of important improvement in the treatment of some painful burns, wounds, etc. For they appear to me to suggest the possibility of the suffering which is attendant on such injuries being controlled and cancelled by keeping the pained parts in contact with carbonic acid, or with some other gas or fluid, capable of acting as a local anæsthetic. If the reports of Ewart, Beddoes, and Fourcroy are correct, we ought also, indeed, to find carbonic acid an excellent application even as far as the mere healing and cicatrisation of the broken surfaces are concerned.

Note on painless Extraction of Teeth.—I have previously (at p. 260) stated that it was reported that M. Pernot, a dentist at Limoges, in France, had the secret of extracting teeth with little or no pain, in consequence of previously applying some obtunding agent to the gums. More recently, I have been informed by several persons, some of them medical men who had practised at Hong-Kong, etc., that the extraction of teeth is sometimes performed in China without pain. The alleged secret of thus annulling the pain incident on tooth-extraction seems to be possessed by a particular guild of dentists in the Celestial Empire. They are said to apply locally the anodyne, and to be able to pull out the tooth with very little force or pain some hours subsequently. The substance employed is generally reputed to be extracted from the head of a fish; and, if so, may probably be some form of phosphoric acid. Is it possible that any form of this acid can soften the tooth, or its bony socket, as acids generally soften bone, permitting its removal without pain, while the surrounding soft textures remain uninjured?

CHAPTER VI.

PARTS AND SURFACES TO WHICH CARBONIC ACID MAY BE APPLIED.

Edinburgh, April 1858.

In the preceding remarks we have seen that carbonic acid has been formerly applied in some form as a local anodyne or anæsthetic to various parts or surfaces—as,

1. The vagina and uterus.
2. The rectum and lower end of the intestinal canal.
3. The interior of the stomach.
4. The surface of the tongue.
5. The ulcerated surface of the skin, mamma, etc.

There are other surfaces and structures upon which I have found the local application of carbonic acid act sometimes with remarkable success as a local anodyne or anæsthetic—as,

6. *The Mucous Surface of the Eye.*

I have used it principally in cases of photophobia and hyperæsthesia of the eye connected with scrofulous ophthalmia, where often it gives speedy and marked relief. A few drops of chloroform evaporated from the palm of the patient's hand, and held near the eye, will generally, in the same way, allow a photophobic eye to open, and form an application far more easily used, and as curative, as any medicated liquids or collyria dropped into the eye.

7. *The Mucous Surface of the Bladder.*

I have already mentioned a case, in which, after many modes of treatment had failed, the injection of carbonic acid gas into the vaginal canal several times a-day at once produced relief, and ultimately effected a perfect cure. I lately heard of this patient—a Canadian—remaining perfectly well.

Before 1732 the celebrated Dr. Hales had described to the Royal Society, the injection, without injury, into the bladder of the dog, of a menstruum, consisting, to use the words of Dr. Willis, "of

a mixed solution of bicarbonate of potash, sulphate of potass and *carbonic acid* in water."[1] In consequence of his attention being directed to the subject by my paper,[2] as analysed by M. Follin in the *Archives Générales de Médecine*, M. Broca injected carbonic acid into the bladder, and published some of the successful results which he obtained in the *Moniteur des Hôpitaux* for August 1857. More lately (March 1858), Dr. Johns has brought the same method of treatment under the notice of the profession in Dublin. Perhaps I will be excused for remarking that there is no class of ailments more distressing, or more difficult to treat, than the different forms of morbid irritability of the bladder, and in the management of the affections I have obtained far more favourable results from the local application and injection of carbonic acid than I could have ventured to anticipate. The freedom and safety with which carbonic acid and some other medicinal fluids and liquids may be injected into the cavity of the bladder will probably be ere long more fully acknowledged by the profession, and great practical advantage taken of this fact in the treatment of dysuria and other morbid states of the bladder.

In one of his letters to Dr. Priestley, dated 1775, Dr. Percival states that he had found "by repeated trials that calculi are soluble in water impregnated with fixed air," or carbonic acid; and, he adds, that it had acted in his experiments "upon every calculus which was suspended in it." He and Dr. Home hoped that carbonic acid given in effervescing drinks by the mouth would reach the bladder as carbonic acid, and there act as a lithontriptic upon the contained calculus. But if carbonic acid can act upon some forms of urinary calculus as a dissolvent, we know that it can be introduced freely and continuously by a double catheter into the bladder. In a most interesting case reported by Sir Benjamin Brodie, the repeated injection of a very weak solution of nitric acid into the bladder successfully dissolved and removed a phosphatic calculus; and he has found the same solution relieve chronic inflammation of the lining membrane of the bladder. On the contrary, it has been further long known that a calculus, probably of lithic acid, was broken down and removed by Professor Rutherford and Mr. Butter by free and frequent injections of tepid lime-water into the urinary bladder. Surely the time is not far distant when a higher chemistry will thus enable us to remove some calculi at least without the horrid necessity of the knife or lithontrite.

[1] *Urinary Diseases*, p. 330.

[2] *Supra*, Chap. V.

8. *The Mucous Surface of the Trachea and Lungs.*

In forming carbonic acid for application as a local anæsthetic, I have generally placed together six drachms of crystallised tartaric acid and eight drachms of bicarbonate of soda in a common wine-bottle, added six or seven ounces of water, and allowed the gas to escape through a perforated cork, and attached a caoutchouc tube to the part to which it was applied. In a considerable number of instances of chronic bronchitis, asthma, irritable cough, etc., I have directed the patient to breathe the carbonic acid, which escaped from the above mixture, by placing the end of the tube in his mouth. In a large proportion of these cases the relief obtained has been most striking; and in several chronic instances the benefit has been at once both speedy and permanent. The quantity of gas thus set loose and inhaled is not so very great in quantity as the rapid and continuous rush of it into the patient's mouth would lead a person to suppose; and it acts, I believe, in these cases as a local sedative or anæsthetic applied to the whole lining pulmonary membrane, like the smoke of stramonium, or the vapour of chloroform. The common idea that spasm of the glottis will come on whenever carbonic acid is breathed in any considerable quantity will be found quite incorrect. I will perhaps take an early opportunity of stating at length the unexpected results of this practice; one which, even after all, is, I find, not quite novel, as, in the last century, Dr. Percival tried in phthisis pulmonalis the inspiration of fixed air or carbonic acid "by inhaling the steams of an effervescing mixture of chalk and vinegar, or of vinegar and potash," and Drs. Lettsom, Withering, and Hulme, tried a similar method.

9. *To External Wounds and Burns.*

Seeing, 1st, the great and speedy relief to pain in cancerous and other sores obtained through the local application of carbonic acid by Ewart, Ingenhouz; and 2d, the tendency to cicatrisation observed even in some malignant ulcers when carbonic acid was kept in contact with them, I ventured in my paper on the subject to suggest the topical employment of carbonic acid to surgical wounds and burns, as at once both relieving suffering, and being one of the best means for producing rapid healing and cicatrisation. If found successful, it would not be difficult to devise simple means of applying it as a constant dressing. But no sufficient experiments, so far as I am aware, have been as yet made upon the matter.

In the preceding remarks I have scarcely referred, except incidentally, to the question of the utility of carbonic acid as a local anæsthetic when applied to various mucous surfaces and exposed external structures. Let me only in the meantime add, that all my subsequent experience has in my opinion more than confirmed the views which I ventured to publish two or three years ago with regard to its practical utility and efficiency as a local sedative or anodyne agent. The late observations of Drs. Churchill, Johns, etc., in our own country, and of Follin, Broca, Bernard, etc., in France, all tend further to prove and establish the advantages to be often obtained in practice from the employment of this therapeutic agent as a topical anæsthetic.

HOSPITALISM.

CHAPTER I.

COUNTRY AMPUTATION STATISTICS.

1. Preliminary Remarks.

Perhaps one of the most weighty and momentous questions to which, at the present day, the physician, the surgeon, and the accoucheur can direct his attention, is the proper reconstruction and arrangement of our hospitals. The vast importance of the subject depends upon this point, that it involves the study and rectification of influences that seem at present to set utterly at defiance all the proudest advances of practical medicine. When the two largest hospitals in Scotland—viz. the Infirmaries of Edinburgh and Glasgow—were opened in the last century, the buildings of which they then consisted were new and fresh, and comparatively small. In the Edinburgh Infirmary, out of the first 99 cases in which the limbs were amputated, 8 of the patients died, or 1 in 12. Out of the first 30 amputations for disease in the Glasgow Infirmary, 1 patient only died. At the present day, in these now greatly-enlarged and palatial hospitals, the mortality from the same operations has latterly become higher than 1 in every 3 operated upon. But surely during the last fifty or a hundred years surgery has made much signal and striking progress in various ways and in various directions. Amputation, for instance, as an operation, has, like many other operations, been mightily improved in the modes of its performance, in the modes of arresting the attendant hemorrhage, in the modes of dressing the stumps, etc.; but still in these hospitals the mortality from limb-amputations has, since the last century, become increased instead of diminished. This increase is traceable, I believe, chiefly or entirely to our system of huge and colossal hospital edifices, and to the hygienic evils which that system has

hitherto been made to involve. If it be so, then that system counteracts and cancels all the advances and improvements which modern surgical and medical science has evoked; and we cannot, in my opinion, hope for adequate and commensurate progress in the public practice of the healing art, till our system of hospitalism is more or less changed and revolutionised.

Above twenty years ago, in speaking of the effects and evils of our large hospitals—as these hospitals are at present constructed—I took occasion to remark: "There are few or no circumstances which would contribute more to save surgical and obstetric patients from phlebitic and other analogous disorders, than a total change in the present system of hospital practice. I have often stated and taught, that if our present medical, surgical, and obstetric hospitals were changed from being crowded palaces,—with a layer of sick in each flat,—into villages or cottages, with one, or at most two, patients in each room, a great saving of human life would be effected; and if the village were constructed of iron (as is now sometimes done for other purposes) instead of brick or stone, it could be taken down and rebuilt every few years—a matter apparently of much moment in hospital hygiene. Besides, the value of the material would not greatly deteriorate from use; the principal outlay would be in the first cost of it. It could be erected in any vacant space or spaces of ground, within or around a city, that chanced to be unoccupied; and in cases of epidemics, the accommodation could always be at once and readily increased."[1]

Since the date mentioned, I have conversed on many occasions with many medical men upon this subject. I have found, however, that to most professional minds it seemed to be altogether a kind of medical heresy to doubt that our numerous and splendid hospitals for the sick poor could by any possibility be aught other than institutions as beneficial in their practical results as they were benevolent in their practical objects. When acting in 1867, at Belfast, as President of the Public Health section of the National Association for the Promotion of Social Science, I spoke of the subject of hospitalism at some length in my inaugural address, and propounded the questions, "To what extent are hospitals, as in general at present constituted, banes or blessings? and how can they be changed so as to convert them from the former to the latter?" I concluded my remarks on this point by again suggesting publicly, that to render our hospitals as healthy and useful as possible, and in order to

[1] *Edinburgh Monthly Journal of Medical Science*, November 1848, p. 328.

acquire sufficient space and air and isolation for their sick inmates, they should be changed "from wards into rooms, from stately mansions into simple cottages, from stone and marble palaces into wooden, or brick, or iron villages." On the same occasion, after speaking of the relative treatment of some medical diseases, as fevers, etc., in and out of hospitals, and after showing (chiefly from the large statistics of M. Lefort) that, as a general rule, parturient women recovered in a much larger proportion when delivered in their own homes than when delivered in lying-in hospitals, I proceeded to ask, "In regard to surgical patients in hospitals, as compared with surgical patients at home, does the same law hold good as in respect to obstetric patients? At the present time, medical science is, I believe, in want of any sufficient data to determine the question. The general mortality in hospitals after operations is confessedly very great, far greater than was believed a quarter or half a century ago, when no sufficient statistics had been collected on the matter. The man laid on an operating-table in one of our surgical hospitals is exposed to more chances of death than the English soldier on the field of Waterloo. Some authors have collected, on a large scale, the statistical results of some special operations, and particularly of amputation of the limbs. Out of 1656 cases of amputation performed in the hospitals of Paris, and collected by MM. Malgaigne and Trelat, 803 of the patients died, or nearly 1 in every 2.[1] Dr. Fenwick has collected together from various sources 4937 cases of amputations of the limbs. Of these, 1562 died, or nearly 1 in every 3 or 4. 'The assertion,' observes Dr. Fenwick, 'that one person out of every three who suffers an amputation perishes, would have been repudiated a few years ago as a libel upon the profession, and yet such is the rate of mortality observed in nearly 5000 cases.' Are the results of amputation in

[1] M. Trelat's list contains, besides the major amputations of the limbs, minor amputations also of the hand and foot. Dr. Bristowe and Mr. Holmes of London, in visiting the Parisian hospitals in order to draw up a report upon them for the medical officer of the Privy Council (Mr. Simon), obtained from the government official archives the results of the major amputations of the limbs—that is, of the thigh, leg, arm, and forearm—during the year 1861 in all the Parisian hospitals taken as a whole. The mortality among those operated upon was as high as 1 in 1½; or 3 out of every 5 died. We shall afterwards see that this is nearly double the death-rate which attends upon the same operations in our large and metropolitan British hospitals, in which the mortality is fully 1 in 3; while, as Mr. Simon points out, in reference to our smaller and rural British hospitals, the special death-rate from amputations "in the London hospitals is half as high again as in the country hospitals."

dispensary, private, or country practice, as deplorable? Adequate data on the matter have not been collected. Certainly the general belief of the profession is, that in country practice amputations are not so frightfully fatal."[1]

I have often thought of trying to collect the data referred to in the preceding sentences, and as often delayed the task, as being apparently more a duty pertaining to others. Circumstances, however, connected with the rebuilding of the Edinburgh Hospital, have lately induced me to attempt this statistical inquiry; and the object of the next chapter is to state the results.

2. Mortality of the Major Amputations of the Limbs in Private Country and Provincial Practice.[2]

With the hope of collecting sufficient data to approach, if not to determine, the rate of mortality generally attendant upon ampu-

[1] *Transactions of Social Science Association* for 1867, p. 115.

[2] *Other Operations used as Standards of Comparison.*—Several operations have been suggested and employed as forming in their results statistical criteria or tests of the relative salubrity and success of surgery in different hospitals, and in different practices, and under different conditions. Those operations that have been chiefly used for this purpose are,—Lithotomy, Herniotomy, and Amputation of the Limbs. The two first are not fitted to form tests or standards, in such an inquiry as the present, between the results of hospital practice and of private country practice. For it would be impossible to procure from the country a sufficient number of cases of lithotomy, for example, to make the required comparison, as that operation is rarely performed by the country practitioner; and the patients, being usually quite able to move and travel, usually place themselves under the care of skilled hospital or metropolitan surgeons. Herniotomy, again, is an operation in the successful performance and result of which much depends upon the attainment of the proper period of operating, as well as upon the surgical dexterity and delicacy with which the operation itself is performed. Hence it is not very capable of being used in any comparison between its relative mortality in the hands of a rural practitioner and in the hands of an accomplished hospital surgeon. It would be, on the large scale, a test rather of the operative skill of the practitioner than of the influence upon the patients of external circumstances and surroundings that are independent of the mode in which the operation is conducted. But in this last respect, and for the purpose of comparing the results of surgical operations in country and in hospital practice, the major amputations of the limbs undoubtedly form the best, if not the only adequate, standard. The operation itself of amputation, formidable as it is in its character,—since, generally, it involves the loss of a limb to save the loss of a life,—is not a proceeding attended with much surgical difficulty, or requiring much surgical skill, so that it can be done readily in the country as well as in the hospital; it is resorted to for the same classes of injuries and for the same classes of diseases in the one locality as in the other; and, in rural as well as in hospital practice, it is performed so frequently as to afford sufficient cumulative masses of data for correct statistical deductions.

tations of the thigh, leg, arm, and forearm, when performed in private country and provincial practice, I addressed the following application—along with the accompanying form of schedule—to numerous medical gentlemen practising in England and Scotland :—

"The relative success of the graver operations in surgery, as performed, first, in hospital practice, and, secondly, in private practice, is at present attracting much attention here and elsewhere.

"There is reason to believe that some of the greater operations—as the various amputations of the limbs—are attended with less mortality in private and in country practice than in hospital practice.

"Already there have been published by various authors ample statistics of the results of amputations of the limbs from many different surgical hospitals. But hitherto there has not been made any collection showing the results of the same amputations, or of any similar operations, in private, and particularly in country practice.

"With a view of making the comparison in question, I should feel deeply obliged if you would kindly fill up the included form with the results of all the amputations which you may have had in your own practice. However few may be the amputations of the limbs which you have performed, the notification in the table of their nature and their results (whether the case or cases ended in recovery or in death), will be regarded as a very great favour.

"An accumulation of several hundred returns—even though the numbers in individual practice may not exceed two or three—will go far, it is believed, to throw much light on the momentous subject of the present inquiry.

"It would be an additional favour if you could inform me whether your cases of amputation were in persons belonging to (1) the upper, (2) the middle, or (3) the lower classes ; and whether their status in society, or the character of their habitations, appeared to influence the results. It is thought by many that, after amputations and similar great operations, the poor recover in a greater proportion in their own homes than they do in the wards of our very best hospitals."

The included schedule was in the following form :—

"*Results of Amputation of the Limbs in Private Practice.*

Seat of the Amputations.	Primary, or for Injury.		Secondary, or for Disease.	
	Number of Cases.	Number of Deaths.	Number of Cases.	Number of Deaths.
Amputation of Thigh . .				
Amputation of Leg . .				
Amputation of Arm . .				
Amputation of Forearm .				
Total . .				

Signature, _____

Residence, _____

Date, _____"

The two blank pages of the schedule which followed were

headed "Remarks," for the purpose of eliciting observations from the gentlemen filling up the returns.

In selecting the practitioners to whom the application and schedules were sent, I avoided, as far as possible, including in the list any members of the profession residing in our large hospital cities and towns, as my object was to obtain the returns principally or entirely from country and provincial professional men. For example, with this view I did not apply in Scotland to any practitioners in Edinburgh, Glasgow, Aberdeen, Dundee, Dumfries, etc. In England the application was chiefly made to practitioners connected with the Poor Law service; but others were included, who appeared likely to be able to furnish the required returns.

Some did not return the schedule; others returned it blank; and, in doing so, they usually stated either that no cases of amputation had occurred in their practice, or that they were so near some hospital or another as to have been always in the habit of forwarding there any such patients as required amputation.

Several of the returned schedules contained cases of amputations of the limbs, performed not by the practitioners who filled in the schedule, but by their friends. These returns I have not of course used, except when there were data given that enabled the two classes of cases to be separated; because my application was, in each instance, for all the cases of limb-amputation performed by the gentleman himself who returned the schedule; and not for such as he might have seen or known to have been done by others, as such reports of these last did not include and show the results of all the cases operated upon by those other practitioners.

Two or three instances have been reported in the schedules where the amputation was performed in the country, and the patients forthwith sent on into city hospitals. As these hybrid amputations were neither truly hospital, nor truly country cases, I have omitted them altogether from the Table. One of these cases was, during their hospital residence, attacked with erysipelas, and another with gangrene.

A number of gentlemen have informed me that they have repeatedly performed amputations, but have kept no such record of them as to be able to report them with statistical accuracy. Some who have often operated, while unaware of the extent of their

numbers, have assured me that they recollected all their fatal cases, and that their number was comparatively small. Thus, in one of the last unfilled schedules which I have received, Dr. Redwood of Rhymney, practising in an iron-work district in Monmouthshire, writes me as follows:—"Unfortunately I have kept no account of my cases of amputation of thighs, legs, arms, and forearms. I believe I have had between forty and fifty; certainly more than forty. They all recovered except two, that died from the shock of the accident; one on the table, and the other in a few hours after operating. My successful amputations include three at the shoulder-joint, and one of both legs." In compound fractures (he adds), "where there is tissue left that will continue the circulation below the seat of injury, we save the limb. Assistants fresh from the hospitals are often astonished at what is attempted and effected in this way. . . Some of the patients are rather hard-drinking men."

In the following Table, No. I., I have entered all the available data that have been furnished to me in answer to my inquiries, whether they were good, bad, or indifferent. In the first part the returns are almost entirely Scottish; the other portions are partly from England and Wales, and partly from Scotland. The numeral in the first row of the Table is the number attached to each schedule as it was returned and entered, so as to facilitate reference to it, etc.

TABLE I.—OF AMPUTATIONS AND THEIR RESULTS—PRIMARY OR FOR INJURY, AND SECONDARY OR FOR DISEASE—OF THE THIGH, LEG, ARM, AND FOREARM, PERFORMED IN PRIVATE PRACTICE BY COUNTRY AND PROVINCIAL PRACTITIONERS; AMPUTATIONS THROUGH THE JOINTS NOT INCLUDED.

No of the Schedules.	PRIMARY.								SECONDARY.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1	2	0	..	..	2	0	..	..	..	..	..	..	..	..	..	..
2	..	..	..	..	1	0	1	0	..	..	..	..	..	..	..	..
3	1	0	..	..	2	0	1	0	..	..	1	0	..	..	..	..
4	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
5	1	0	..	..	1	0	1	0	..	..	..	..	..	..	..	..
6	1	1	1	0	4	0	..	..	..	..	..	..	..	..	..	..
7	1	0	2	0	2	0	3	0	..	..	1	0	..	..	..	..
8	..	..	..	..	..	..	..	..	..	..	2	0	..	..	..	..
9	..	..	2	1	..	..	2	0	1	1	..	..	..	..	..	..
10	1	0	2	0	..	..	1	0	1	0	1	0	..	..	2	0
11	1	1	1	0	..	..	..	..	..	..	..	..	..	..	..	..
12	2	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..
13	..	..	1	1	..	..	..	..	1	0	..	..	..	..	..	..
14	1	0	1	0	2	0	1	0	1	0	..	..	..	..	..	..
15	..	..	..	..	..	..	..	..	2	0	1	0	..	..	..	..
16	1	1	..	..	1	0	..	..	..	..	..	..	..	..	..	..
17	..	..	..	..	1	0	3	0	4	1	2	0	2	0	..	..
18	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
19	1	1	1	1	..	..	..	..	..	..	..	..	..	..	..	..
20	1	0	..	..	1	0	..	..	..	..	1	0	..	..	..	..
21	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
22	1	1	5	0	..	..	3	0	2	1	..	..	..	..	..	..
23	1	1	1	0	..	..	..	..	..	..	..	..	1	0	..	..
24	2	0	..	..	3	0	1	0	4	0	1	0	..	..	..	..
25	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
26	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
27	..	..	..	..	2	0	..	..	2	1	2	0	1	0	..	..
28	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
29	..	..	..	..	1	0	1	0	2	1	..	..	1	0	1	0
30	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
31	2	0	5	0	4	0	3	0	4	0	1	0	..	..	..	..
32	..	..	2	0	1	0	5	0	..	..	..	..	..	..	..	..
33	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..
34	4	1	1	0	1	0	..	..	1	0	..	..	..	..	1	0
35	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
36	..	..	..	..	2	0	2	0	1	0	..	..	..	..	..	..
37	..	..	..	..	..	..	..	..	..	..	2	0	..	..	..	..
38	2	0	2	0	2	0	..	..	..	..	1	0	1	0	..	..
39	3	0	..	..	..	..	..	..	1	0	..	..	..	..	..	..
40	3	1	1	0	1	0	1	0	..	..	..	..	..	..	..	..
41	1	0	1	0	..	..	..	..	..	..	..	..	..	..	..	..
42	..	..	..	..	..	..	..	..	..	..	..	..	1	0	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
43	1	0	..	..	2	0	1	0	5	0	5	1	1	0	..	..
44	4	1	10	2	13	1	13	0	12	0	8	0	..	..	..	..
45	..	..	1	1	1	0	..	..	..	..	..	..	..	..	..	..
46	..	..	..	..	1	0	..	..	..	..	1	0	..	..	..	..
47	..	..	1	0	1	0	..	..	..	..	..	..	..	..	..	..
48	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
49	1	0	5	0	3	0	..	..	..	..	1	1	..	..	..	..
50	5	1	6	0	4	0	3	0	..	..	..	..	..	..	..	..
51	..	..	..	..	2	0	..	..	..	..	1	0	..	..	..	..
52	1	1	1	0	..	..	..	..	8	0	..	..	..	..	..	..
53	..	..	1	0	..	..	..	..	..	..	1	0	..	..	..	..
54	1	0	..	..	..	..	1	0	1	0	..	..	..	..	..	..
55	2	0	..	..	..	..	1	0	1	0	..	..	1	1	..	..
56	..	..	1	1	1	0	1	0	..	..	..	..	..	..	..	..
57	2	0	..	..	..	..	..	..	..	..	1	0	..	..	..	..
58	..	..	..	..	..	..	1	0	..	..	1	0	1	1	..	..
59	..	..	3	0	1	0	2	0	1	0	..	..	..	..	..	..
60	..	..	1	0	..	..	..	..	1	0	..	..	..	..	..	..
61	5	1	4	0	2	1	..	..	1	1	4	0	..	..	..	..
62	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
63	..	..	..	..	..	..	..	..	..	..	..	..	1	0	..	..
64	..	..	..	..	1	0	..	..	2	0	..	..	..	..	..	..
65	..	..	1	0	1	0	1	0	1	0	..	..	..	..	..	..
66	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
67	..	..	1	0	..	..	1	0	..	..	1	0	..	..	1	0
68	3	1	4	1	4	0	3	0	..	..	..	..	..	..	..	..
69	..	..	..	..	1	0	..	..	..	..	1	0	1	0	..	..
70	..	..	1	0	..	..	2	0	..	..	..	..	..	..	..	..
71	2	1	3	0	4	0	3	0	4	1	2	0	1	0	..	..
72	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
73	..	..	..	..	1	0	1	0	1	1	..	..	..	..	..	..
74	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
75	1	0	..	..	1	0	1	0	3	0	..	..	..	..	..	..
76	..	..	1	0	1	1	..	..	..	..	2	0	..	..	1	0
77	2	0	..	..	..	..	..	..	..	..	2	0	..	..	..	..
78	1	0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
79	2	0	4	2	1	0	..	..	..	..	..	..	..	..	..	..
80	1	0	2	0	1	0	2	0	1	1	..	..	..	..	1	0
81	..	..	..	..	..	..	1	0	..	..	1	1	..	..	..	..
82	..	..	2	1	..	..	..	..	..	..	..	..	..	..	..	..
83	1	0	..	..	1	0	3	0	6	2	6	1	2	0	..	..
84	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
85	..	..	2	0	1	0	1	0	1	0	2	0	..	..	..	..
86	1	0	..	..	2	0	..	..	2	1	..	..	..	..	..	..
87	..	..	1	0	..	..	1	0	2	0	1	1	..	..	..	..
88	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
89	..	..	..	..	..	..	3	0	6	1	3	0	3	0	1	0
90	1	1	7	0	1	0	2	0	2	0	..	..	..	..	..	..
91	..	..	..	..	2	0	..	..	..	..	..	..	..	..	..	..
92	11	0	4	0	3	0	4	0	1	0	..	..	..	..	..	..
93	..	..	..	..	..	..	..	..	2	0	..	..	..	..	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
94	2	1	4	0	..	..	..	..	2	0	1	0	..	..	..	..
95	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
96	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	..
97	1	0	..	..	1	0	1	0	1	0	..	..	..	..	..	..
98	..	..	2	0	2	0	3	0	..	..	4	0	..	..	..	..
99	1	1	..	..	..	..	..	..	1	0	..	..	..	..	..	..
100	3	1	2	0	1	0	1	0	..	..	..	..	..	..	..	..
101	..	..	2	1	..	..	..	..	..	..	1	0	..	..	..	..
102	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
103	..	..	..	..	2	0	..	..	..	..	..	..	..	..	..	..
104	3	1	4	0	2	0	3	0	..	..	2	0	1	0	1	0
105	..	..	3	0	..	..	..	..	1	0	..	..	..	..	..	..
106	..	..	2	1	1	1	..	..	5	0	2	0	..	..	1	0
107	..	..	..	..	1	1	..	..	7	0	..	..	..	..	..	..
108	..	..	..	..	..	..	1	0	.	..	..	..	..	..	..	..
109	..	..	..	..	..	..	..	..	..	..	..	..	1	0	..	..
110	..	..	..	..	2	0	..	..	1	0	..	..	..	..	..	..
111	1	0	..	..	..	..	1	0	..	..	..	..	..	..	..	..
112	..	..	2	0	..	..	2	0	1	0	..	..	1	0	1	0
113	..	..	..	..	1	0	..	..	1	1	..	..	..	..	..	..
114	..	..	..	..	..	..	..	..	4	1	..	..	1	0	..	..
115	..	..	1	1	2	0	2	0	..	..	..	..	2	0	..	..
116	1	0	..	..	..	..	..	..	..	..	1	0	..	..	..	..
117	..	..	..	..	..	..	..	..	..	..	2	1	..	..	..	..
118	..	..	1	0	..	..	1	0	1	1	..	..	..	..	..	..
119	1	1	1	0	1	0	..	..	1	0	..	..	..	..	..	..
120	..	..	..	..	..	..	..	..	..	..	1	0	..	..	..	..
121	2	0	..	..	1	0	1	0	..	..	..	..	..	..	..	..
122	..	..	1	0	1	0	3	0	1	1	..	..	..	..	1	0
123	..	..	2	1	..	..	..	..	..	..	2	1	2	0	..	..
124	..	..	..	..	2	0	..	..	2	1	..	..	1	0	..	..
125	2	1	..	..	..	..	1	0	..	..	..	..	..	..	..	..
126	2	1	2	0	1	0	1	0	1	0	..	..	..	..	..	..
127	..	..	1	1	..	..	..	..	7	0	2	0	..	..	..	..
128	..	..	2	1	..	..	2	0	..	..	..	..	..	..	..	..
129	..	..	4	0	..	..	..	..	..	..	..	..	1	0	..	..
130	..	..	..	..	..	..	..	..	2	0	..	..	..	..	..	..
131	..	..	..	..	..	..	..	..	2	1	1	0	1	0	..	..
132	..	..	1	0	2	1	2	0	2	1	..	..	1	0	..	..
133	4	1	2	0	3	0	..	..	3	0	2	0	..	..	..	..
134	3	2	2	0	1	0	1	0	..	..	..	..	..	..	..	..
135	1	0	..	..	3	0	1	0	3	0	..	..	..	..	..	..
136	..	..	1	1	..	..	..	..	..	..	..	..	..	..	..	..
137	1	1	2	0	3	0	1	0	3	0	..	..	..	..	..	..
138	..	..	..	..	1	0	..	..	2	1	..	..	..	..	..	..
139	..	..	..	..	1	0	1	0	..	..	..	..	..	..	..	..
140	2	0	1	0	2	0	4	0	2	0	..	..	..	..	..	..
141	..	..	1	0	1	0	1	0	2	0	1	0	..	..	1	0
142	..	..	..	..	..	..	..	..	5	0	4	1	3	0	4	0
143	..	..	..	..	..	..	..	..	2	0	1	0	..	..	..	..
144	..	..	..	..	1	0	1	0	..	..	..	..	..	..	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
145	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
146	..	..	..	..	1	0	..	..	1	0	..	..	..	..	..	..
147	..	..	2	0	..	..	..	..	..	..	..	..	..	..	..	..
148	..	..	1	1	..	..	2	0	..	..	1	0	..	..	..	..
149	4	1	7	1	7	1	5	1	5	0	1	0	4	0	2	0
150	..	..	..	..	..	..	2	0	..	..	..	..	..	..	..	..
151	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	..
152	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
153	1	0	..	..	1	0	..	..	..	..	2	0	..	..	..	..
154	..	..	..	..	1	0	..	..	..	..	1	0	..	..	..	..
155	2	0	1	0	1	0	1	0	..	..	..	..	..	..	..	..
156	..	..	..	..	1	0	..	..	3	0	..	..	2	0	..	..
157	..	..	..	..	1	0	..	..	..	..	..	..	1	0	..	..
158	2	2	3	0	..	..	..	..	..	..	..	..	..	..	..	..
159	..	..	..	..	1	0	..	..	2	1	2	0	..	..	..	..
160	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
161	..	..	1	0	..	..	2	0	1	0	2	0	..	..	..	..
162	1	0	1	0	1	0	1	0	1	0	1	0	..	..	..	..
163	..	..	2	0	..	..	..	..	..	..	..	..	..	..	..	..
164	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
165	..	..	1	1	..	..	..	..	..	..	..	..	..	..	..	..
166	..	..	..	..	..	..	1	0	..	..	1	0	..	..	..	..
167	..	..	..	..	..	..	..	..	4	2	..	..	..	..	1	0
168	..	..	..	..	..	..	..	..	..	..	1	0	1	1	..	..
169	1	0	1	0	1	0	1	0	..	..	2	0	2	0	2	0
170	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
171	8	0	6	2	4	0	4	0	2	0	..	..	1	1	..	..
172	..	..	1	0	..	..	..	..	1	0	..	..	..	..	..	..
173	..	..	1	0	1	0	..	..	2	0	1	0	..	..	..	..
174	..	..	..	..	..	..	1	0	2	0	..	..	2	0	..	..
175	..	..	2	0	2	0	1	0	1	0	1	0	..	..	1	0
176	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
177	..	..	..	..	..	..	..	..	1	1	..	..	..	..	2	0
178	2	1	..	..	..	..	..	..	..	..	..	..	1	0	..	..
179	..	..	1	0	..	..	1	0	2	1	3	0	..	..	..	..
180	..	..	..	..	..	..	..	..	4	1	..	..	1	0	..	..
181	1	1	2	1	2	1	..	..	1	0	1	0	1	0	1	0
182	2	1	..	..	1	0	1	0	6	0	1	0	1	0	1	0
183	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
184	1	1	3	0	..	..	..	..	..	..	..	..	..	..	..	..
185	..	..	..	..	..	..	1	0	1	0	..	..	..	..	..	..
186	1	1	1	1	1	0	5	0	..	..	..	..	..	..	..	..
187	1	0	..	..	1	0	1	0	1	1	..	..	..	..	..	..
188	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
189	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
190	..	..	2	1	..	..	..	..	..	..	..	..	..	..	..	..
191	5	0	9	0	7	0	8	0	..	..	5	0	..	..	..	..
192	1	0	..	..	1	1	..	..	..	..	..	..	..	..	..	..
193	1	0	..	..	1	0	1	0	..	..	..	..	..	..	..	..
194	22	6	23	3	14	1	4	0	..	..	3	1	1	0	..	..
195	..	..	..	..	..	..	..	..	2	0	1	0	..	..	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
196	..	..	..	..	1	0	1	0	1	0	..	..	..	..	..	..
197	..	..	..	..	1	0	1	0	..	..	..	..	..	..	..	..
198	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
199	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
200	..	..	3	1	1	0	1	0	1	0	..	..	..	..	1	0
201	..	..	..	..	1	0	..	..	1	0	..	..	..	..	..	..
202	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
203	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
204	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
205	1	0	7	0	1	0	..	..	..	..	..	..	..	..	..	..
206	..	..	3	1	..	..	1	0	..	..	..	..	..	..	..	..
207	..	..	..	..	..	..	1	0	2	0	1	0	..	..	..	..
208	..	..	..	..	1	0	..	..	1	0	..	..	..	..	..	..
209	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
210	3	1	1	1	1	0	2	0	3	1	..	..	..	..	..	..
211	2	0	2	0	3	0	1	0	1	1	..	..	..	..	..	..
212	1	0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
213	..	..	..	..	1	0	..	..	13	1	..	..	..	..	..	..
214	4	0	..	..	1	0	1	0	2	0	1	0	1	0	..	..
215	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
216	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
217	..	..	1	0	..	..	..	..	..	..	1	0	..	..	..	..
218	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
219	..	..	1	0	1	0	2	0	..	..	1	0	..	..	..	..
220	..	..	..	..	1	1	..	..	1	0	..	..	..	..	..	..
221	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
222	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	0
223	..	..	..	..	1	0	..	..	1	0	..	..	..	..	..	..
224	1	0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
225	..	..	..	..	..	..	..	..	1	0	2	0	1	0	..	..
226	..	..	2	0	1	0	1	0	..	..	..	..	..	..	..	..
227	20	6	7	2	6	0	5	0	6	0	3	0	..	..	..	..
228	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
229	1	0	..	..	..	..	..	..	..	..	..	..	1	0	..	..
230	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
231	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
232	..	..	..	..	2	1	3	0	..	..	..	..	..	..	..	..
233	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
234	..	..	..	..	..	..	1	0	1	0	..	..	..	..	..	..
235	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
236	2	1	3	1	1	0	..	..	..	..	..	..	..	..	..	..
237	..	..	1	0	2	1	2	0	.	..	..	..	..	..	..	..
238	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
239	..	..	..	..	1	0	2	0	..	..	1	0	..	..	..	..
240	1	1	3	0	1	0	1	0	..	..	..	..	..	..	..	..
241	..	..	..	..	1	0	1	0	..	..	..	..	..	..	..	..
242	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
243	1	0	1	0	..	..	2	0	..	..	..	..	..	..	..	..
244	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
245	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
246	..	..	1	1	..	..	..	..	..	..	1	0	..	..	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
247	..	..	1	0	1	0	..	..	..	..	..	..	..	..	..	..
248	..	..	..	..	..	..	..	..	2	0	..	..	..	..	..	..
249	..	..	1	0	1	0	2	0	..	..	1	0	..	..	..	..
250	..	..	..	..	..	..	1	0	..	..	..	..	1	0	..	..
251	1	1	..	..	2	0	..	..	..	..	2	1	1	0	..	..
252	..	..	1	1	..	..	1	0	2	0	2	0	1	0	2	0
253	2	0	1	0	..	..	..	..	..	..	..	..	..	..	..	..
254	2	2	..	..	1	0	..	..	..	..	..	..	..	..	..	..
255	2	0	..	..	1	0	3	0	1	0	..	..	..	..	..	..
256	..	..	5	0	1	0	2	0	4	1	2	0	..	..	..	..
257	2	1	2	1	1	0	..	..	2	0	..	..	..	..	..	..
258	2	2	1	1	1	0	1	0	1	0	..	..	..	..	..	..
259	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
260	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
261	1	1	1	0	..	..	..	..	..	..	..	..	..	..	..	..
262	2	0	4	1	4	0	6	0	1	1	2	1	4	0	2	0
263	..	..	..	..	1	0	..	..	1	0	..	..	1	0	..	..
264	..	..	1	1	1	0	..	..	..	..	1	1	1	0	..	..
265	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
266	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
267	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	0
268	1	0	1	1	..	..	..	..	1	0	..	..	..	..	..	..
269	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
270	1	0	..	..	..	..	1	0	..	..	..	..	..	..	..	..
271	1	0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
272	2	0	1	0	..	..	2	0	..	..	..	..	..	..	..	..
273	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
274	1	0	1	1	..	..	1	0	1	0	..	..	..	..	..	..
275	2	0	1	0	2	0	1	0	..	..	..	..	..	..	..	..
276	..	..	2	0	..	..	1	0	..	..	1	0	..	..	1	0
277	..	..	..	..	..	..	..	..	2	0	2	0	..	..	..	..
278	1	1	3	0	1	0	1	0	..	..	..	..	..	..	..	..
279	..	..	..	..	..	..	..	..	2	0	1	0	..	..	..	..
280	..	..	..	..	..	..	..	..	..	..	1	0	..	..	..	..
281	..	..	..	..	..	..	..	..	1	0	3	0	..	..	..	..
282	10	1	5	0	1	0	2	0	2	0	2	0	2	0	1	0
283	1	0	6	0	3	0	7	0	..	..	..	..	..	..	..	..
284	3	1	..	..	4	0	3	0	..	..	..	..	..	..	..	..
285	1	0	4	0	2	0	3	0	..	..	..	..	..	..	..	..
286	..	..	..	..	..	..	..	..	1	0	..	..	..	..	2	0
287	..	..	..	..	..	..	..	..	7	0	5	1	2	0	3	0
288	10	0	17	0	20	0	5	0	2	0	3	1	2	1	..	..
289	..	..	2	0	..	..	..	..	3	0	4	2	..	..	..	..
290	..	..	..	..	3	0	..	..	3	0	..	..	..	..	..	..
291	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
292	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
293	..	..	..	..	1	0	..	..	2	0	..	..	..	..	..	..
294	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
295	..	..	1	0	..	..	..	..	3	0	..	..	1	0	..	..
296	..	..	1	0	..	..	..	..	..	..	2	1	..	..	..	..
297	..	..	..	..	2	0	..	..	..	..	3	0	..	..	4	0

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
298	1	0	..	..	1	0	..	..	..	..	..	..	..	..	..	..
299	..	..	3	1	1	0	3	0	..	..	..	..	..	..	..	..
300	..	..	..	..	..	..	..	..	1	0	1	0	..	..	..	..
301	..	..	..	..	..	..	..	..	5	0	2	0	..	..	..	..
302	..	..	..	..	..	..	..	..	..	..	1	0	1	0	..	..
303	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
304	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
305	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
306	1	0	1	1	..	..	..	..	2	0	3	0	..	..	1	0
307	1	0	1	0	..	..	..	..	1	1	1	1	..	..	..	..
308	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
309	..	..	1	0	..	..	..	..	..	..	1	0	..	..	..	..
310	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
311	..	..	..	..	..	..	..	..	..	..	1	0	..	..	1	0
312	1	1	1	1	..	..	..	..	..	..	..	..	..	..	..	..
313	..	..	..	..	1	0	..	..	1	0	..	..	1	0	..	..
314	..	..	1	0	2	0	1	0	..	..	..	..	1	0	..	..
315	..	..	..	..	..	..	1	0	..	..	1	0	..	..	..	..
316	..	..	1	0	1	0	..	..	..	..	..	..	..	..	..	..
317	2	1	1	0	2	0	1	0	1	0	..	..	..	..	..	..
318	..	..	..	..	..	..	1	0	2	1	..	..	..	..	..	..
319	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
320	1	0	..	..	..	..	4	0	2	0	..	..	..	..	..	..
321	..	..	..	..	..	..	1	0	..	..	..	..	..	..	..	..
322	4	0	3	0	1	0	2	0	1	0	1	0	1	0	1	0
323	3	2	..	..	1	0	..	..	..	..	1	0	..	..	..	..
324	..	..	..	..	..	..	3	1	..	..	..	..	..	..	..	..
325	..	..	1	0	..	..	..	..	..	..	..	..	..	..	..	..
326	..	..	1	0	..	..	1	0	1	0	..	..	..	..	1	0
327	..	..	..	..	..	..	..	..	1	0	1	0	..	..	..	..
328	..	..	5	0	..	..	..	..	..	..	..	..	..	..	..	..
329	..	..	2	1	..	..	..	..	1	0	..	..	..	..	..	..
330	..	..	1	0	..	..	..	..	2	0	..	..	..	..	1	0
331	2	0	4	0	3	0	2	0	3	0	2	0	1	0	2	0
332	..	..	2	1	..	..	..	..	..	..	..	..	..	..	..	..
333	..	..	1	1	..	..	..	..	4	0	5	0	2	0	..	..
334	..	..	..	..	..	..	1	0	..	..	1	1	..	..	..	..
335	1	1	1	0	3	1	1	0	1	0	1	0	..	..	..	..
336	..	..	1	1	3	0	..	..	1	0	5	2	6	0	2	0
337	..	..	..	..	1	0	..	..	..	..	..	..	..	..	..	..
338	3	0	5	0	4	0	6	0	..	..	..	..	..	..	..	..
339	4	2	3	0	..	..	..	..	..	..	..	..	..	..	..	..
340	1	1	..	..	2	0	..	..	3	0	..	..	1	0	1	0
341	..	..	1	0	..	..	1	0	..	..	..	..	..	..	..	..
342	..	..	1	0	1	0	..	..	1	0	..	..	..	..	1	0
343	..	..	..	..	..	..	..	..	4	0	2	1	..	..	..	..
344	3	1	1	1	2	0	2	0	1	0	2	0	..	..	..	..
345	2	0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
346	2	1	1	0	..	..	..	..	..	..	..	..	..	..	..	..
347	..	..	2	0	..	..	4	0	1	1	1	1	..	..	..	..
348	2	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..

No. of the Schedules.	Primary.								Secondary.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
349	1	0	1	0	..	..	1	0	..	..	..	..	..	..	..	..
350	..	..	..	..	3	0	..	..	2	0	2	0	..	..	..	..
351	..	..	2	1	2	0	..	..	..	..	..	..	..	..	..	..
352	..	..	..	..	2	0	..	..	..	..	..	..	..	..	..	..
353	..	..	1	1	..	..	..	..	1	0	..	..	..	..	..	..
354	2	0	3	1	..	..	3	0	1	0	..	..	..	..	..	..
355	..	..	1	0	1	0	1	0	..	..	..	..	..	..	..	..
356	..	..	..	..	..	..	1	0	1	0	..	..	..	..	..	..
357	..	..	..	..	..	..	..	..	..	..	..	..	1	0	..	..
358	2	0	2	0	..	..	1	0	1	0	..	..	..	..	..	..
359	..	..	..	..	3	0	..	..	..	..	..	..	..	..	..	..
360	..	..	..	..	..	..	..	..	1	0	1	0	..	..	..	..
361	..	..	..	..	2	0	1	0	..	..	1	0	1	0	..	..
362	2	0	2	0	3	0	2	0	6	1	1	0	1	0	1	0
363	1	1	2	0	..	..	1	0	..	..	..	..	..	..	2	0
364	..	..	1	0	1	0	1	0	5	1	..	..	..	..	..	..
365	..	..	..	..	2	0	3	0	3	1	1	1	..	..	..	..
366	1	1	..	..	1	0	2	0	..	..	..	..	..	..	..	..
367	3	1	3	0	3	0	2	0	..	..	1	0	..	..	..	..
368	..	..	2	0	1	0	..	..	..	..	..	..	..	..	..	..
369	..	..	1	0	..	..	..	..	..	..	2	0	..	..	..	..
370	2	1	5	1	3	0	1	0	..	..	..	..	1	0	..	..
371	..	..	..	..	..	..	..	..	1	0	..	..	..	..	..	..
372	1	1	4	0	..	..	1	0	5	0	..	..	..	..	..	..
373	2	0	7	2	4	0	3	0	..	..	..	..	..	..	..	..
374	1	0	3	0	2	0	2	0	1	1	1	0	1	0	..	..

General Summary of Table I.

The general summary of the results shown in the preceding Table may be stated under the following four heads:—

1. *Total Mortality of all the Amputations.*

When all the amputations are placed together, and all the deaths from them, the result as to the general mortality is as follows:—

Total number of cases, 2098;
Total number of deaths, 226;
Or 1 in every 9·2 died; or 10·8 in every 100.

2. *Mortality of the Individual Amputations.*

When we calculate the mortality attendant upon the four

individual amputations of the Thigh, Leg, Arm, and Forearm, the results stand thus :—

Thigh	cases,	669 ;	deaths,	123 ;	or 1 in	5·4 ;	or 18·3	per cent.
Leg	„	618 ;	„	82 ;	or 1 in	7·5 ;	or 13·2	„
Arm	„	433 ;	„	19 ;	or 1 in	22·8 ;	or 4·3	„
Forearm	„	378 ;	„	2 ;	or 1 in	189 ;	or 0·5	„

3. *Mortality from the Amputations that were Primary or for Injury.*

The death-rate among the class of amputations for injuries or their results may be represented thus :—

Thigh	cases,	313 ;	deaths,	80 ;	or 1 in	3·9 ;	or 25·5	per cent.
Leg	„	409 ;	„	57 ;	or 1 in	7·1 ;	or 13·4	„
Arm	„	344 ;	„	14 ;	or 1 in	24·5 ;	or 4·0	„
Forearm	„	318 ;	„	2 ;	or 1 in	159 ;	or 0·6	„

4. *Mortality from the Amputations that were Secondary or for Disease.*

The death-rate among this class stands as follows :—

Thigh	cases,	356 ;	deaths,	43 ;	or 1 in	8·3 ;	or 12·0	per cent.
Leg	„	209 ;	„	25 ;	or 1 in	8·3 ;	or 12·0	„
Arm	„	89 ;	„	5 ;	or 1 in	17·8 ;	or 5·6	„

Of amputations of the forearm for disease, 60 cases were reported in the schedules. None of the 60 patients died.

CHAPTER II.

STATISTICS OF { 2098 COUNTRY AMPUTATIONS. / 2089 HOSPITAL AMPUTATIONS.

1. On some Minor Points pertaining to the Collection of Amputations in Table I.

My chief object in collecting the ?000 and odd cases of amputation recorded in Table I. was to ascertain what was the mortality in private country and provincial practice of the four major amputations of the limbs[1] when taken as a class of operations,—when taken as individual operations,—and when performed for the effects of injury or for the effects of disease. As already stated, however, there was added to the schedule which was intended to elicit these leading data, a vacant space, headed "Remarks," under the belief that valuable observations of various kinds might be appended by some of the reporters. In this hope I have not been disappointed. Perhaps I might have added several more special and direct questions on various minor matters; but I avoided doing so under the conviction that, if I asked too much or too many things, I should be liable to get the primary and simple points regarding the mortality, buried and lost in points of infinitely less importance; or, indeed, receive no answer at all.

In the present and following chapters I shall throw together some of the more interesting items of information contained in these "Remarks."

Double Amputations in twenty-three of the Cases.

The cases enumerated in Table I. amount to 2098. This is the number of patients operated upon; but the actual number of amputations performed was greater, being 2122 in all. For in twenty-three instances of severe injuries, involving two extremities, the

[1] Let it be here explicitly noted, that the present collection of limb amputations in country and in hospital practice is limited to the *four* amputations through the bones of the thigh, leg, arm, and forearm, and does not include any of the other *six* amputations of the limbs through the joints—viz. the hip, knee, ankle, shoulder, elbow, and wrist joints. To have taken into the investigation disarticulations or amputations through the joints, would have complicated and extended the whole inquiry very greatly and very needlessly.

patients were subjected to more than one amputation; as the thigh and leg, two legs, etc. Sometimes, in the schedules, as they were returned to me, these double amputations were entered as two amputations; but in accordance with the practice followed I believe, in most hospital returns, I have given in the table each double amputation as one case; entering it under the head of the greater of the two amputations that were performed, when the two amputations were on different parts of the two limbs. Several correspondents who have added few or no "remarks," have, if I may judge from the number of their primary or traumatic cases, possibly or probably met with additional double amputations, though they have not considered them of sufficient interest to report upon in their returns.[1]

TABLE II.—OF THE RESULTS OF TWENTY-THREE DOUBLE AMPUTATIONS AFTER COMPLEX INJURIES.

No. of Schedule.	DOUBLE PRIMARY AMPUTATIONS.	No. of Cases.	Deaths.
7	Both forearms	1	0
22	Both legs	1	0
24	Thigh and arm at shoulder-joint	1	0
31	Both arms	1	0
34	Thigh and arm	1	0
	Thigh and leg	1	1
43	Both forearms	1	0
44	Both legs	1	0
49	Both legs	1	0
	Leg and arm	1	0
59	Both legs	1	0
65	Both forearms	1	0
78	Thigh and leg	1	0
107	Arm and forearm	1	1
125	Both thighs	1	1
133	Both thighs	1	1
134	Thigh and arm	1	1
135	Both arms	1	0
184	Thigh and leg	1	1
200	Both forearms	1	0
344	Thigh and forearm	1	0
	Arm and forearm	1	0
	Both thighs	1	1
	Total	23	7

The mortality among these cases of primary double amputations —viz. 7 deaths in 46 amputations, performed upon 23 individuals, is strikingly small, when we take into consideration the terrible

[1] The three cases of double amputation in the Table occurred to Mr. Hinton of Hinton. Mr. Moodie of Stirling and Dr. Thomson of Motherwell have each reported two double amputations.

character of the compound injuries under which the patients suffered. In our large and metropolitan hospitals, as we shall see in the sequel, the mortality attendant upon *single* amputations, primary and secondary, is greater. And, in these hospitals, double amputations seem to be attended by a frightful mortality. Out of a list of the last 11 double primary amputations performed in the Edinburgh Infirmary, and furnished to me by Mr. M'Dougall, 10 of the patients died. The case which survived was a double amputation through the knee-joints, and does not therefore properly belong to the present inquiry, as it excludes all operations *through* the joints.

Some of the double amputations are shown in the table to be greatly more dangerous than others; those involving the thigh being apparently much the most perilous.

Upper Extremities.—Out of 4 double amputations in which both forearms were removed, all the 4 patients recovered; 2 other patients, in whom both arms were amputated, recovered also; as did 1 in whom the opposite arm and forearm were removed; and 1 only out of the 8 in whom the double amputations were confined to the upper extremities died. In this eighth and fatal case, the right arm and left forearm were amputated.

Lower Extremities.—In 4 cases both legs were amputated; all the patients survived. In a fifth the amputation of the leg and arm proved successful. In 10 cases in which amputation of the thigh formed one of the forms of dismemberment, 6 deaths occurred. In 1 the thigh and forearm, and in 2 the thigh and arm, were amputated; all three recovered. In 3 cases a thigh and the opposite leg were amputated; 2 out of the 3 succumbed. In 3 cases both thighs were amputated; all three patients died.[1]

[1] In an annotation to his schedule, Mr. Anderson of Castle-Douglas relates a case where a double amputation was performed by machinery, and not by the knife of the surgeon; and which is, therefore, not included in Table I. But this double amputation deserves to be recorded. "A girl," he states, "about ten years of age, amusing herself putting straws into a lint-mill, had her hands caught by the machine, and gradually drawn inwards, till stopped at the shoulders, when they were completely detached close to the shoulder-joints. There was no bleeding, although the arteries were seen on the surface of the wounds pulsating strongly. One sharp projecting piece of bone was removed by pliers. Nothing more was done, as the attempt to remove ragged pieces of muscle and skin produced such violent excitement in the girl—it was before chloroform was known—that we were compelled to desist. Wet rags were laid over the wounds; everything went on well; granulations sprang up and covered the ends of the bones; and in due time cicatrisation was completed with so little assistance that, it may be said, the mill amputated the arms, and the *vis medicatrix naturæ* effected the cure of the stumps."

Age of the Patients.

I originally sent out no query about the ages of the patients operated upon, because it seemed to be unnecessary. For there appeared no reason whatever for believing that the general average of ages of those submitted to amputation in country practice would differ in any important respect from the general average of ages of those submitted to amputation in hospital practice. Some of my correspondents have incidentally given the ages of their patients. These ages vary from 1 up to 84 years. The youngest subject noted is an infant about twelve months old. The case occurred in the practice of Dr. Livingston of Wishaw. The child's arm was smashed by a railway-waggon, and Dr. Livingston was obliged to amputate the limb high up. The little patient did well for ten days, but ultimately sank. Several gentlemen speak of the amputations which they practised being in "old" or "very old" individuals, without specifying more particularly this state of advanced age. In others of these instances the age of the patients is specified, and the following table shows the cases of this kind which I have found in the annotations of my correspondents:—

TABLE III.—RESULT OF AMPUTATIONS BY COUNTRY PRACTITIONERS ON PATIENTS OF 70 YEARS AND UPWARDS.

No. of Schedule.	Age of Patient.	Special Amputation.		Recovered.	Died.
		For Injury.	For Disease.		
289	70	.	Leg	0	1
188	70	Arm	.	1	0
45	70	Leg	.	0	1
118	Above 70	.	Thigh	0	1
273	72	Arm	.	1	0
268	Above 72	.	Thigh	1	0
43	73	.	Leg	1	0
131	73	.	Arm	1	0
354	74	Leg	.	0	1
240	74	Leg	.	1	0
257	74	Arm	.	0	1
222	75	.	Forearm	1	0
359	75	Arm	.	1	0
43	77	.	Leg	0	1
295	78	Leg	.	1	0
144	Between 70 and 80	Forearm	.	1	0
106	80	Leg	.	1	0
27	Above 80	.	Thigh	0	1
117	„ 80	.	Leg	1	0
141	82	.	Thigh	1	0
1	84	Thigh	.	1	0
17	84	.	Thigh	0	1
			Total	14	8

This mortality rate of 1 death in 2·7, or of 8 out of 22, is, as we shall see subsequently, not more than the rate of mortality in most of our large and metropolitan hospitals in patients of *all* ages; and consequently forms an amount of mortality much smaller than could be *à priori* expected in a set of patients operated upon after they had reached or passed the "threescore years and ten."

Injuries and Diseases necessitating the Amputation.

Though the causes leading to the amputations are often mentioned in the returns, I do not know that anything special can be adduced under this head.

In the traumatic or primary cases, the injuries noted are chiefly for railway and other accidents connected with mining, iron-works, and various descriptions of machinery. Some are the results of high falls; several, and particularly the amputations of the forearm, from gunshot wounds, bursting of guns, etc. The unguarded country thrashing-machine is very often mentioned as the source of the mutilations that led to amputations in the upper extremity. The supervention of gangrene following upon injuries, such as in compound fractures, etc., is repeatedly mentioned as the reason for having had, sooner or later after the accidents, recourse to amputation. In two or three cases, suppuration of the knee-joint, following upon its puncture or injury, is noted as the cause leading to the operation.

Among the pathological series, or amputations for disease, I find annotated most frequently diseases of the joints and bones. In various instances, the existence of cancerous deposits and ulcers in the amputated portion of limb, of fungus hæmatodes, of tumours (one lived for twenty years after amputation),[1] of burns and their effects, of frostbites, of dry or senile gangrene, led to the operation. In one case it was deemed necessary to stay the bleeding from a popliteal aneurism; in another instance it was had recourse to in the faint hope of arresting tetanus. The patient, in the practice of Mr. Boyle of Newquay, had a compound fracture of the leg, and progressed favourably for twelve days, when symptoms of tetanus supervened. Amputation was then adopted as a last resource. Death took place sixteen hours afterwards.

[1] It was a case of amputation of the thigh, in the practice of Dr. Falla of Jedburgh. The tumour was fourteen pounds in weight, extended from the knee to the ankle, was of a fatty-like structure internally, and "had a bleeding fungus of nearly five inches in diameter" on a portion of its surface. The man (writes Dr. Falla) is in perfect health now—twenty years after the operation.

2. Causes of Death in the Fatal Cases of Amputation.

The primary questions of the actual death-rate—whether high or low—attendant upon all the four major amputations of the limbs, collectively and individually—and attendant upon these same amputations when performed for the results of injury and for the results of disease, form the special objects of our present investigation; and the whole inquiry has been conducted more particularly with a view of ascertaining the relative effects of place or locality, or of the conditions of hospitalism and the conditions of private rural practice upon the results.

My object has been to ascertain as far as possible the differences, if any, in the death-rate of the major amputations under the different circumstances last alluded to. It does not come within the scope of the present inquiry to push the investigation into other more minute matters, such as the modes of performing the amputations by the flap or circular or ovoid method, the modes of arresting the hemorrhage, or the modes of dressing the stump, etc., or the effects of age, sex, season, etc., upon the results. Nor does the pathological cause or causes of death, in the cases which ended fatally, form in any direct way a special object in our inquiry. But as few or no data on this last point exist in reference to amputations in rural practice, perhaps the following particulars may interest some readers:—

Out of the 227 deaths tabulated in Chapter I. 2, the apparent causes of the fatal issue have been returned in above 160 of the cases as follows:—

I. *Shock.*—This is entered as the most frequent cause of death; the patient sometimes dying of it upon the operating table; or within a few hours; or within a day or two. In some cases the shock from the injury and the attendant hemorrhage itself is mentioned as being so severe as to offer but a very forlorn hope for the success of the amputation. "Shock" is returned as the cause of death in 63 cases, including 36 amputations of the thigh, 21 of the leg, and 6 of the arm. Of these 63 cases, 56 were amputations for the results of injury, and 7 for the results of disease.

II. *Exhaustion* is returned as the cause of death in 28 cases—viz., in 19 amputations of the thigh, and 9 of the leg. Of the 28 amputations ending ultimately, after a few days, and sometimes after weeks, in alleged "exhaustion," 13 were primary and 15 secondary amputations. In some, the exhaustion is spoken of as combined with delirium tremens.

III. *Pyæmia* is described as the pathological cause of death in 8 cases—all traumatic—viz., in 5 amputations of the thigh and in 3 of the leg for injury or its consequences. Perhaps some surgeons would have considered a few of the cases entered under other heads as referable to forms of pyæmia.

IV. *Gangrene of the Stump* is returned as the cause of death in 18 cases; 12 of them primary and 6 of them secondary amputations. In several it existed before amputation. Of the 18 cases, 8 were amputations of the thigh, and 9 were amputations of the leg. In the only 2 amputations of the forearm that were fatal, gangrene is given as the cause of death.

V. *Secondary Hemorrhage* is referred to as having produced the fatal issue in 9 cases—viz., in 4 amputations of the thigh, and 5 of the leg. Of these 9 amputations, 4 were primary or traumatic, and 5 were secondary or for disease. In 1 of the thigh-amputations, the reporter, Mr. Edwards of Wiveliscombe, observes, "Death was caused by hemorrhage before and after operation, the case being one of fungus hæmatodes." This was the only unsuccessful case in 18 limb-amputations performed by Mr. Edwards.

VI. *Tetanus.*—From it 11 deaths resulted, all of them, with one exception, after amputations for injury; 2 of them in the practice of one surgeon. Of the 11 deaths, 4 were after amputations of the thigh, 6 after amputations of the leg, and one after amputation of the arm.

VII. *Internal Injuries.*—In a considerable number of the cases entered in the Table as fatal after primary amputations, the alleged cause of death is some injury or injuries, generally internal, received at the same time with the injuries to the limb that necessitated its amputation. In 13 of these cases, 7 were amputations of the thigh, 4 of the leg, and 2 of the arm. The coexistent internal lesions returned as the causes of death were in different cases injuries of the brain, lungs, bladder, fractures of the skull, ribs, etc.[1]

[1] *Internal Injuries in some of the fatal Primary Amputations.*—Take, for example, the following extracts as illustrations of the remarks in the text:—"The death in the primary arm case cannot fairly be put down as a death from amputation, as the person was otherwise severely injured, and actually died of brain concussion and compression a few hours after the operation."—*Mr. Troup of Auchtermuchty.* "The case of fatal primary amputation of the leg was one of railway smash, and was complicated with fracture of the skull and concussion of the brain. The cause of death was encephalitis."—*Dr. Drew of Chapeltown.* "In the fatal primary amputation, the patient really died in consequence of compression of the brain from extravasated blood, having a fracture of the skull."—*Mr. Grindrod of New Mills.* In one fatal case out of nine amputations, "the one

VIII. *Miscellaneous Causes.*—Of chest affections returned as causes of death are several inflammatory complications, as 4 cases of fatal pneumonia, 3 of bronchitis, 2 of pleurisy, and 1 of hydrothorax. Pulmonary phthisis is entered as the cause of death in 7 or 8 patients. Gastric irritation of several weeks' duration after the amputation, and chronic and acute diarrhœa, are reported as the causes of death in three or 4 cases; convulsions in 2 or 3 others; encephalitis in 1; hectic fever and suppurations in 2 more; and a large abscess of the neck in a third; erysipelas formed a fatal complication in 1 case; 1 died of scarlatina; 1 in the eighteenth day after operation, of an attack of influenza; 1 some weeks after, of general anasarca, the stump being "perfectly healed before death," etc.

Date of Death.—In some returns of amputations which have been published in this and in other countries, it has been sometimes held justifiable to enter a case as successful when the patient survived the operation above a certain limited period, as four, five, or six weeks. I have, however, entered the cases as fatal, even when death did not take place for a longer period, and the pathological cause of it was not directly connected with the operation, as in the last case mentioned in the preceding paragraph. Or take phthisis pulmonalis as an example. In relation to one of his thigh-amputations for chronic disease of the knee, Dr. Gavin of Strichen remarks, "Death took place about a month after amputation. It was really," he adds, "a case of death from pulmonary consumption: for at no time were there any symptoms directly caused by the

death was not the result of the operation, but rather of the extent and severity of the injury which rendered the operation necessary."—*Mr. Baillie of Markinch.* In a fatal thigh-amputation, the patient "died in consequence of internal injuries."—*Dr. Paterson of Bridge-of-Allan.* In two fatal primary amputations of the thigh and leg, "the deaths resulted directly from hæmorrhage *prior* to the operation, and therefore, it is scarcely fair to count them."—*Mr. Eames of Pulham.* "Hardly expected my patient to recover from his amputation, in consequence of the severe nature of his injury and the loss of blood; for he had his leg completely severed at the knee-joint by the friction of a heavy pit-chain, and lost a great amount of blood before I saw him."—*Mr. Dow of Dunfermline.* "Out of fourteen cases, with two deaths, my leg case was fatal from injury to the bladder, and I had a shoulder-joint amputation which was fatal from injuries to the lungs."—*Dr. Maclatchy of Kilmarnock.* "The death after amputation of the thigh occurred in the case of a man who fell to the ground from a great height. He sustained a bad compound fracture of the thigh, and was, besides, much injured internally, surviving the operation only 48 hours. I do not consider his death fairly assignable to the operation. Several of his ribs were broken, his lungs injured, and general emphysema supervened."—*Dr. Steele of Montrose;* etc. etc. etc.

operation that produced any anxiety." Out of other instances in which tubercular phthisis is returned as the cause of death after amputation, there is one of amputation of the thigh by Mr. Day of Harlow, where the patient died after six weeks; another of thigh-amputation by Mr. Ronald of Ayr, where the patient died two months after the operation; and a third by Dr. Henry of Arroquhar, where death did not supervene till three months. But I have placed all these instances as amputations followed by death, though the date of death and the cause of it might possibly, in the opinion of some, have taken them and other like cases out of the category of fatal issues. Again, Dr. Forrest of Motherwell, in reporting 13 cases of amputation which he had performed, remarks, in reference to one fatal primary leg-amputation,—"The amputation *did well*, but the thigh was severely bruised, deep-seated abscess formed, and he died from exhaustion at the end of four months." Perhaps in respect to this and similar cases it might be argued that, as "the amputation did well," and was not apparently the pathological cause of the patient's death, the death should not be entered as the result of the amputation. I have followed, however, in this and other examples, the safer statistical rule of holding such cases to be amputations ending in death; as is done, I believe, in those hospital returns that are the most to be relied upon for their accuracy.

3. Classes of Patients on whom the Two Thousand Amputations were performed; and the General Character of their Habitations.

In the schedules sent out, in addition to the results of the four amputations of the limbs, I stated that it would be considered an additional favour if my correspondents would inform me whether their cases were in persons belonging to the upper, the middle, or the lower classes, and whether the character of their habitations appeared to influence the chances of death or recovery.

A very few of the two thousand patients—not above half-a-dozen—are reported to me as belonging to the upper classes; and a much larger number—perhaps eight or ten per cent of the whole—as belonging to the middle classes of society. But the great mass of those operated upon were artisans, labourers, farm-servants, miners, iron-workers, quarrymen, etc., or some members of their families,—in short, individuals belonging to such a class as, in our large towns and cities, would be generally sent into hospitals.

The house accommodation of this latter class, upon whom the amputations were thus chiefly or almost entirely performed, was not such as most hospital surgeons would deem eligible. In many cases the chamber in which the patient was laid, even when small, was no doubt clean, comfortable, and tidy; but in most the bed and other accommodation was sufficiently stinted and limited. A few extracts, however, from the notes of some of my correspondents will illustrate this observation better than any didactic statement.

A large number of the cases of amputation were performed at their own houses on workers in mines, iron-factories, etc. Their house accommodation is, as a rule, usually rather poor; but the general free ventilation of their cottages and hovels more than compensates for their other deficiencies. Thus, Dr. Stewart of Kirkintilloch, in sending me a list of 25 limb-amputations which he had performed, with 24 recoveries and one death, remarks:—"All my operations, except one, were among miners. Their dwellings were composed of *two* rooms, but the doors were always kept open. The only death in my surgical practice (being one in ten primary amputations of the thigh) was in a fisherwoman, who met with a railway accident. She died of bronchitis ten days after the operation. In the operations for the scrofulous diseases of joints, I found that the patients immediately began to improve after the operation was performed, and all made good recoveries." In his schedule, Dr. Boyd of Slamannan observes:—"In most of my cases the domestic accommodation has been of the most defective nature, but country air and thorough ventilation from open doors and constant fires atone for many other deficiencies. . . . I would consider it," he adds, "my duty to undertake a formidable operation in a *colliery row* rather than send in the patient to the best-conducted hospital, notwithstanding the immeasurable superiority of diagnostic skill and operative dexterity to be had there." Out of 6 limb-amputations, Dr. Boyd has lost only one—viz., a primary thigh-amputation in a stoker, for a severe railway accident. He died of shock in two hours. Writing from the same locality, and practising, I believe, among the same class of patients, Mr. Waddell reports to me 10 cases of primary amputations of the limbs, all of them successful. One of my correspondents, Mr. Cribbes of Gorebridge, after speaking of all wounds in his district—and the wounds are many among his colliery patients—healing, as regards both time and results, rapidly and satisfactorily, states:—"In none of them have I known erysipelas or fatal results ensue;" and he adds,

"these remarks apply wholly to the mining population, who are, after all, blest with nothing approaching to sanitary measures excepting the free blast of heaven, which whistles merrily through their ill-conditioned dwellings."

Some of the patients subjected to limb-amputation were located in habitations still more stinted and squalid than the cottage of the collier and iron-worker. In many rural villages and towns the poorer types of mechanics and their families all live in a house of a single room; and the country ploughman and labourer and their families are often similarly situated. In sending me a report of seven cases of amputation of the limbs, with one death,[1] Dr. Lindsay of Lesmahagow remarks:—"The whole of the cases were in persons belonging to the lower classes, and were treated in small houses, most of them of *one* apartment." "The six amputations performed by me," writes Dr. Jeffray of Ayton, "have all been in persons belonging to the lower classes, and all for injury. All the operations, with one exception, were performed in dwellings where there was only one apartment, into which all inquiring about the patient were freely admitted, and where there was no great ventilation. One of the operations—amputation of the leg—was performed in a hut upon the North British Railway, when in course of formation, and the end of the table upon which the patient was laid had to be brought to the door of the hut, in order that I might have sufficient light." In a case in which Mr. Bloxham of Hales-Owen was amputating the thigh, "in consequence of aneurism of the popliteal artery, which had been opened by a quack," the patient was living "in a den about six feet square, not high enough to stand upright in, and two farthing dips afforded the only light." The patient recovered. Describing the accommodation in a case of amputation of the thigh, Mr. Cade of Spondon remarks, "The operation was performed in a miserable hovel, for it would be hardly fit to call such a place a house,—in a miserable bed; and yet the case did very well." In reporting to me three cases of primary limb-amputation—two of the thigh, and one of the leg—which all succeeded, Mr. Girvan of Maybole says, "In each case there was but one apartment for the whole members of the family to live, cook,

[1] The fatal case was in a case of thigh-amputation. "The operation," Dr. Lindsay writes, "was undertaken with scarcely any hope of success. It was a case of compound fracture,—not a severe one,—which resulted in gangrene a few days after the accident, apparently from some defect of constitution. Although a line of demarcation had formed in the leg, there was great infiltration of the tissues upwards, even into the pelvis."

eat, and sleep in." Mr. Balding of Rayston, in forwarding the results of two cases—one of them amputation of the leg, and the other of the thigh—strongly points out what the differences between healthy and unhealthy cottages may lead to:—"The two cases of amputation," says he, "were both performed in cottages. The case of amputation of the leg was in a dirty and unhealthy cottage, surrounded by every description of filth. There was no attempt at union in the wound, the flaps sloughed, and the patient died nine days after the operation. The other case, that of amputation of the thigh, was performed in a clean and healthily-situated cottage. The patient's recovery was more rapid than was ever witnessed in any hospital. At the expiration of a fortnight he may be described as having been almost well."

Dr. Irvine of Pitlochrie has performed amputation of the limbs in 12 cases, and makes an observation upon the houses in his Highland district, which seems to me of importance. The only case of the 12 that died was an old drunkard of 84 years of age. "Excepting this instance," Dr. Irvine writes, "I have lost no cases after any capital operations. The 12 amputations were all in the lower classes. Some of their houses were small and dark, but as the walls were badly built, and the roofs thatched, the ventilation was therefore good." Dr. Irvine believes that the introduction into Highland and other districts of the close slated instead of the open thatched roof, is proving unsanitary where the families are not yet educated up to the necessary amount of tidiness and cleanliness.

In addition to the occasionally wretched house accommodation of the patients, their beds were sometimes of the worst construction in a sanitary point of view, being made on the old plan of building them closely off, with an inclosure or box of wood all around, and alike on the sides and top. Dr. Johnston writes me, that in the earlier years of his practice, and before becoming connected with the Montrose Hospital, he performed several limb-amputations in the country. "All the patients," he says, "belonged to the labouring class, and were treated in the obnoxious box-bed which is in universal use among this class in the rural parts of Forfar and Kincardineshire."[1]

In reporting to me 12 cases of amputation of the limbs which he

[1] Of the 11 patients, 2 died from shock shortly after primary amputations, one being a woman of almost 80 years of age, with comminuted fracture of the leg and compound dislocation of the ankle; and the second a lunatic, in whom the arm was smashed and destroyed

has performed, Dr. Hamilton of Dalry makes a remark, which is, I think, highly worthy of citation. "It has long," he states, "been my belief that the success of the treatment of amputation and of compound fractures was greater in private than in hospital practice, even when the comforts and surroundings were totally unequal to those of the Hospital. I believe," Dr. Hamilton adds, "that *isolation has much to do with it.*" I doubt not that the segregation of the sick from the sick—every diseased man being a focus of more or less danger to the diseased around him—is a principle of no small moment and value.

4. Severity of some of the Cases that recovered.

It has been sometimes maintained that all the most severe and formidable cases, and classes of cases, of injury and disease among the country poor are more likely to be sent into hospitals than treated at home; and that this circumstance alone specially accounts for the higher hospital danger and mortality in limb-amputations.

The most dangerous class of amputations of the limbs consists of primary amputations required for injury; and the most perilous of all amputations in the continuity of the bones is amputation of the thigh for injuries inflicted by railways, mining, machinery, etc. In his very able work on Surgery, Professor Erichsen observes:—"It is more especially primary amputations of the lower extremities, and particularly those of the *thigh*, that are attended by very fatal results. Of the 46 cases of primary amputation of the thigh recorded by Malgaigne, 34 perished. And of 24 cases recorded by South, Lawrie, and Peacock, as occurring at St. Thomas's Hospital, the Glasgow Infirmary, and the Edinburgh Infirmary, every one proved fatal. This similarity of result," Mr. Erichsen adds, "occurring in different institutions, shows clearly that this operation is one of *the* most fatal in surgery, and that the great mortality attending it is inherent to it, and not dependent upon local or accidental circumstances."

According, therefore, to the theory that the cases requiring the most formidable and fatal operations are generally, if not systematically, forwarded from the country into the city hospital, primary amputations of the thigh should be found in far greater numbers in the returns of large city hospitals than in rural practice. Is it so?

Among the 2098 cases of limb-amputations collated in Table I. from rural and provincial practice, there are 313 cases of primary

amputation of the thigh, with 80 deaths; or very nearly 1 death in every 4 operated upon.

Among 2089 cases of limb-amputations collated in Table XI. (see the sequel) from eleven large and metropolitan hospitals, there are 304 cases of primary amputation of the thigh, with 196 deaths; or 1 death in every 1·5 operated upon.

The proportionate number, therefore, of primary amputations of the thigh, is thus as nearly as possible the same in country practice as in large hospital practice—being 14·8 per cent of the whole in the first, and 14·5 per cent of the whole in the last; but the mortality attendant upon the operation is more than *twice and a half* as high in large hospital practice as it is in country practice.

Injuries, indeed, so serious as to require such a grave operation as amputation of the thigh or leg ought, perhaps, as a general rule, not to be forwarded from the country into a city infirmary. Patients so damaged and shattered would have a far better chance of life if they were operated upon and kept in a railway shed, or in a country hovel, than by being carried to a distance into the richest and best conducted hospital. Chance has sometimes preached this lesson. Thus Dr. Carmichael of Burntisland—a town lying on the opposite side of the Forth, at a distance of some seven miles from the Edinburgh Infirmary—states to me that since he began to practise there, three years ago, he had sent 6 cases of severe injuries requiring amputation of the limbs to the Edinburgh Hospitals. All the 6 died. In the last case, however, which happened, the patient was a middle-aged man of not a very robust constitution, who received such very severe injuries of both legs that it was deemed utterly hopeless to attempt to carry him as far as Edinburgh. Dr. Carmichael, consequently, retained the man in comparatively poor accommodation at Burntisland, and performed upon him the necessary double amputation of one thigh and the opposite leg. He made an excellent recovery.

We have already in Table II. entered 16 cases of compound amputations of the limbs that recovered in despite of the tremendous severity of the accidents and injuries that led to the dire necessity, in each of them, of a double amputation.

In some of the remarks forwarded to me along with the schedules, the severe and desperate character of the injuries requiring the primary amputations is incidentally alluded to. Thus, Dr. Lawrence of Cumnock, who has performed 20 amputations of the limbs without a death—5 for disease, and 15 for injury—

remarks, "About one-third of these cases were so bad that I thought it was *impossible* they could recover." Six of his cases were amputations of the thigh.

Dr. Cullen of Airdrie, who has performed, without a death, 10 primary amputations of the thigh, and 17 primary amputations of the leg, among an iron-working and colliery population, says that the deplorable severity of some of the mining injuries was probably much greater in the past than it will be in the future, in consequence of the protection now afforded to the men by the Mines' Inspection Act. But accidents and mutilations of an appalling type still occur in some localities. Mr. Davis of Aberdare, who has with his own hand performed 22 primary amputations of the thigh, and 23 primary amputations of the leg, adds that 7 of the thigh-amputations were for one terrific form of accident—viz. compound dislocation of the knee—an accident which seems to be the result, in his district, of the mode in which the workmen are in the habit, by applying their backs, of staying the loaded and descending trucks in the mines.

Various instances have been recounted to me of the desperate and complicated nature of some of the injuries from which the amputated patients recovered. As examples, let me cite from Dr. Kirk of Bathgate, the two following illustrative cases of injury and recovery in miners. Whilst working in the pit, above half-a-ton of solid rock fell upon one of these men. This enormous mass required to be broken up before the man could be removed from beneath it. He had the following series of injuries:—1. Fracture of the left thigh-bone; 2. Compound comminuted fracture of the right leg, for which amputation was performed below the knee-joint; 3. Dislocation of the right hip-joint; 4. Lacerated wound of the perineum, extending into the right iliac fossa; and 5. Compound comminuted fracture of the metacarpal bones of the left hand. He had afterwards pyæmia, with tympanitis; and a large abscess formed over the dislocated thigh. Under the kind and able care of Dr. Kirk, this patient recovered; was enabled to get about in three months; and now works in the pit every day. To quarry off the block of rock which fell upon him, required two or three hours' work on the part of his comrades; and he was subsequently removed from the pit-mouth in a cart to a room two miles distant. Dr. Kirk further informs me that he treated shortly afterwards in the same room another miner from the same pit, upon whom a mass of rock had also fallen. It entirely comminuted and ground the bones

of one knee-joint, so as to necessitate the amputation of the thigh; and his head and face were very severely injured. This miner was a debilitated man, advanced beyond fifty, addicted to hard drinking, and the subject of a chronic bronchitis. Yet, notwithstanding his age and his weakness, his drunken habits, and his troublesome cough, he recovered rapidly, and without a drawback. Would these two poor fellows have had much or any chance of escape, if, instead of being treated at home, they had been carried away into a distant city hospital?

The preceding remarks on the severity of some of the cases that recovered, refer chiefly or entirely to primary amputations, or to those required for injuries or their results. They tend to show that these primary amputations in country practice were necessitated by as formidable injuries as could well be met with in hospital practice. But in the country, secondary amputations, or those for disease, are also in many instances necessitated by as unpromising morbid local lesions and morbid constitutional states as are met with in large hospitals. A few of the secondary amputations entered in Table I. had been, indeed, previously despaired of when the patients were the inmates of hospitals, and yet afterwards proved successful operations in the country. Thus, for example, in some notes on his amputations, Mr. Breach of Aston-up-Thorpe observes:—"All the cases proved successful. One (a thigh-amputation) was turned out of the —— Infirmary as incurable, the surgeon fearing to operate, as the patient appeared to be in the last stage of hectic, from ulceration of the cartilages of the knee-joints and caries of the femur. The patient begged of me to operate to relieve her of her extreme pain. I reluctantly yielded to her request. About the third or fourth day erysipelas of the stump took place. Betimes a ring of bone exfoliated, and she made henceforward a rapid recovery. She is now in robust health,—it being just nine years since the operation." In speaking of another thigh-amputation, Mr. Brookes of Shaldon observes:—"The patient was a sailor in the Royal Navy, and was brought home at his own request from —— Hospital. The case was one of diseased knee, and the operation was performed with little hope of success. It was, however, the only chance, and all went on well." Dr. Corbett of Orsett writes me in relation to two thigh-amputations which he has successfully performed, that one of his cases was in a labouring man, aged 72, who "had been in the —— Hospital for some months, and was discharged to die, as there was a difference of opinion as to the possibility of recovery if an

operation was performed." In reference to a case of amputation of the leg, Mr. Hallet of Axminster remarks that "the operation on an old sailor was performed on account of caries of the os calcis, and exostosis of the lower end of the tibia and fibula, of many years' standing. He had been sent to an hospital; but the surgeons declined operating, fearing that, as his health was such, he would sink under its effects. He, however, recovered without a single bad symptom, and lived in comfort for many years afterwards."[1]

Occasionally country patients, by the time that they require to be the subjects of secondary or pathological amputations, are already so utterly sunk and debilitated as to be entirely incapable of being moved off to an hospital; and yet sometimes make good recoveries when the operation is had recourse to at home. Dr. Pairman of Biggar has sent me reports of two cases, in regard to which he observes—"Both were in the humble ranks of life, and operated on by me because reduced to a condition of such extreme weakness that they could not be removed to an hospital. Both, however, recovered admirably. One of them was almost hopeless from prostration before the operation, and, as I think, would almost certainly have died in any hospital."

The patient, before at last agreeing to amputation, may have lapsed down into the very lowest stage of weakness and almost of hopelessness—a result which, if I may judge from the remarks sent to me, not unfrequently occurs in country practice. Thus, Mr. Lawrence of Mintlaw states, in reference to an amputation of the thigh for disease which was performed by him, that the patient "had long laboured under disease of the knee-joint, and was so weak and attenuated that he could *not* be moved even to a table for the operation; but his recovery was rapid, and he got very stout."

Several correspondents speak of the recovery of some of their patients in the country[2] from secondary amputations, as conditions

[1] In some of the amputations for disease, there was a remarkable succession of operations. Thus, Mr. George of Keith, in commenting upon one of his cases of amputation of the leg for disease of the ankle-joint, remarks that the patient, a farm-servant, aged 57, suffered a year afterwards so severely from affection of the knee-joint, that "it was necessary to remove the thigh about the middle. Eighteen months after the removal of the thigh his right hand and wrist became diseased, for which he had the forearm amputated. Previously to the removal of any of his limbs the operation of lithotomy was performed upon him in Elgin Hospital; and he died, three years after the last amputation, of disease of the bladder."

[2] Mr. Harper of Holbeach has sent me the results of above 80 operations

that scarcely could have been realised if these same patients had been the inmates of city hospitals. Mr. Haig of Airth, for example, in reporting to me several successful cases of amputation, remarks, in regard to three of his six secondary operations, "I feel certain that, owing to the great debility induced by the discharges from the knee and elbow joints, a satisfactory result could *not* have been looked for if the patients had been confined in the wards even of the best of hospitals." In reference to four cases of secondary amputation in his practice, in all of which the state of debility of his patients was complicated and extreme, and the cottage accommodation most defective, Mr. Blackburn of Barnsley asserts, "All recovered; yet my own belief is, that every case would have died if removed to an hospital."

Many correspondents have casually, and yet so strongly expressed in a similar spirit the results of their experience as to amputations and other operations succeeding much better in country than in hospital practice, that perhaps the collation of a few such opinions may be interesting.

5. Opinions as to Amputations, etc., succeeding better in Private Country than in Public Hospital Practice.

There are various reasons why cases of disease or injury among the poor in the country requiring amputation are sent off to city hospitals. Enumerating these reasons, in a late letter to me, Dr. Whitelaw of Kirkintilloch remarks—"In my opinion, country patients are sent to the city hospital, *first*, by their relatives, because they cannot give them food at home; because they cannot pay for continued surgical attendance; because they have great faith in the head doctor, from whom there is no appeal; and because they know nothing of the perils of hospital pyæmia, erysipelas, etc.; *secondly*, they are sent by the country practitioners, because the accommodation, nursing, and resources at home are frequently quite inadequate; because once away to the hospital, the patient, probably a poor man, is off the doctor's mind, and his maintenance does not longer concern either his employers or the parochial board; and because,

which he has performed in country practice, with a view of showing their relative safety in the country. Among these 12 were limb-amputations, 1 died; 4 were lithotomies, 1 died; and 7 were cases of strangulated hernia, 2 died. Only these four deaths occurred; and the recoveries included cases of lithotrity, of excision of tumours, of removal of portions of lower jaw, of amputation of mamma, extravasation of urine, amputations of the hand, fingers, etc. etc.

if the case turn out badly in the hospital, the tongues of the village gossips cannot reflect on the doctor, 'for the patient had the best of skill.' Yet it is my conviction," adds Dr. Whitelaw, "that ordinary amputations and compound fractures would result in more numerous and more satisfactory recoveries, if treated in the country with fair skill, than if sent to a city hospital." "As surgeon," observes Mr. Garman of Wednesbury, "to large iron-works and collieries, I may be perhaps allowed to add, that compound fractures and other formidable surgical lesions appear to do better in the squalid homes of the patients, although of course suffering great deprivation and inconvenience, than under hospital care. My partner and myself have long made it a rule not to send any formidable accidents to the hospital if possible. The cases we send are for the most part simple fractures, and chronic sequences of accidents." Mr. Carter of Pewsey states: "All my cases of amputation occurred in my pauper practice, the patients being of the lowest class of an agricultural population. But my undoubted experience has ever been, that the poor recover much more readily at their own houses than from the best of treatment elsewhere." "I can give," avers Mr. Wilson of Alton, "my testimony in favour of operations being performed among the poor at their own houses; and there are few cottages indeed in the south of England in which a free current of air may not be obtained." Dr. Monckton of Rugeley has sent me a note of the results of 6 amputations and 80 various surgical operations which he has performed. "Among all these," he writes, "the primary amputation through the thigh is the only case which had a fatal result, whereas I remember being shocked as a student to see operations performed in a London hospital for comparatively small ailments or deformities, which were often followed speedily by a fatal result from pyæmia, erysipelas, etc.; and to this day I hear students remark upon the same unfavourable issue to small operations most ably performed by the first surgeons of the land." "Having," observes Mr. Cann of Dawlish, "been house-surgeon at Guy's Hospital, London, I can, without hesitation, pronounce in favour of operations being done in private houses." "I have had," says Dr. Guppy of Falmouth, "many compound fractures among our sailors and dock-labourers, and my opinion is that the mortality from the graver operations and injuries is *much* less in private and country practice than in hospitals." Mr. Hardy of Byer's Green, Willington, in sending a report of a series of limb-amputations, writes: "I have been long of opinion that severe accidents in the country are

more likely to do well than when they are sent to an hospital, and I have never hesitated to express this opinion; for, living in a colliery district, where severe injuries often occur, I have found them to do better in their own houses than when sent away to an hospital, and consequently I have been very chary in even advising the removal of such parties. Most of the cases of amputation which I have recorded have been caused by railway accidents. All the patients suffered from shock, and the deaths recorded have been from that cause; none from symptoms of blood-poisoning." In the same spirit Dr. Thomson of Motherwell observes, "My experience is against sending any case to hospital that can be treated at home. I have only sent for years past lodgers, or those having no house accommodation." "I have always," writes Dr. Loudon of Hamilton, "studied to keep the patients in their own homes when their means would admit of it, and have only sent those cases to hospital when parties refuse to admit them, as in the case of lodgers."

I might easily multiply similar extracts, but it seems unnecessary. Let me add, however, one remark in relation to the fixed belief with many city and hospital surgeons, that almost all severe cases of injury and disease likely to require amputation are forwarded by the rural practitioner to the city hospital. The great mass of cases of country limb-amputations which I have collected, affords in itself a strong answer to this allegation. Besides, in distant districts the removal of severely injured or diseased patients for amputation to city hospitals is a matter of impossibility. And where the practice is possible, it is often not at all followed. From the medical gentlemen of Airdrie, for example, and its surrounding villages, situated within a few miles of the Royal Infirmary of Glasgow, I have received returns of about one hundred and fifty limb-amputations performed by them at their patients' own homes.

6. Evidence of increased Success in Amputations from increased Experience.

Out of the 2098 amputations of the thigh, leg, arm, and forearm, performed in private provincial and country practice, and entered in Table I., 226 of the patients died; or 1 in every 9·2, or 10·8 per cent. Of the 2098 amputations, 1382 were primary, traumatic, or performed for injuries or their results. Of these 1382 cases, 151 proved fatal; or 1 in 9 died, or 11 in the 100. On the other hand,

716 of the 2098 amputations were secondary, pathological, or for disease and its results; and of these 716 cases, 74 proved fatal; or 1 in every 9·6 died, or 10·3 in every 100.

Of the 374 gentlemen who have reported to me these 2098 cases, a large number have had little opportunity of becoming experienced by much practice in the performance of amputation. Many have only been called upon to have recourse to the amputating-knife once, twice, or thrice, in the whole course of their lives. But in all operations, the surgeon acquires accumulated dexterity and skill by the repetition of an operative proceeding which, like this, involves a combination of manual and mental qualities. It is therefore natural to expect that those practitioners who have performed amputation with considerable frequency should be somewhat more successful in their results than those who have not had the same amount of actual experience. To test on this ground the relative success of the operation in the hands of those provincial and rural practitioners who had practised amputation comparatively seldom, with those who had practised it more frequently, I have taken out of Table I. the results of the operation as observable, 1*st*, in the experience of those medical men who had performed amputation rarely, as only once, or twice, or at most under half-a-dozen times; 2*dly*, in the experience of those medical men who had practised amputation six times or oftener; and 3*dly*, in the experience of those medical men who had used the amputating-knife twelve times or oftener. These analytical results appear to me of no small interest, as showing that, if the rural practitioner had as much experience as the Hospital surgeon, his present great success over the Hospital surgeon would be greater still; and his proportion of deaths from the major amputations would be even less than what the general Table shows—of 1 death in every 9 patients operated upon.

First, In the columns of Table I. are contained the results of 629 limb-amputations, performed by 255 practitioners who have operated less than six times; 72 of them having amputated in 1 case only, 82 in two cases, 36 in 3 cases, etc.

The following summary shows the mortality among the individuals operated upon in these 629 amputations:—

Their total number of cases was 629; of deaths, 85; or 1 in every 7·4 died, or 13·5 in every 100.

Total number of amputations for injury, 401; of deaths, 53; or 1 in every 7·2 died, or 13·2 per cent.

Total number of amputations for disease, 228; of deaths, 32; or 1 in every 7·1 died, or 14 per cent.

Mortality of the Four Major Amputations for Injuries and for Diseases among 255 Practitioners who have amputated less than six times.

Thigh	cases,	193 ;	deaths,	44 ;	or 1 in 4·4 ;	or 22·7	per cent.
Leg	„	178 ;	„	32 ;	or 1 in 5·5 ;	or 18·0	„
Arm	„	134 ;	„	8 ;	or 1 in 16·7 ;	or 6·0	„
Forearm	„	124 ;	„	1 ;	or 1 in 124 ;	or 0·8	„

SECONDLY, I find in Table I. that 119 gentlemen practising in the country and provinces have had occasion to perform the major amputations of the limbs six times or oftener, with the following consequences :—

Total number of cases, 1469 ; of deaths, 141 ; or 1 in every 10·4 died, or 9·5 in every 100.

Total number of amputations for injury, 983 ; deaths, 100 ; or 1 in every 9·8 died, or 10·1 in every 100.

Total number of amputations for disease, 468 ; deaths, 41 ; or 1 in every 11·8 died, or 8·7 in every 100.

Mortality of the Four Major Amputations for Injuries and for Diseases among 119 *Practitioners who have amputated six times or oftener.*

Thigh	cases,	476 ;	deaths,	79 ;	or 1 in 6·0 ;	or 16·5	per cent.
Leg	„	440 ;	„	50 ;	or 1 in 8·8 ;	or 11·3	„
Arm	„	299 ;	„	11 ;	or 1 in 27·1 ;	or 3·6	„
Forearm	„	154 ;	„	1 ;	or 1 in 154 ;	or 0·4	„

THIRDLY, The proportion of successful amputations becomes greater still when we analyse the results of those practitioners who have operated twelve times or oftener. In Table I. are to be found 37 returns in which the number of amputations performed by one practitioner was twelve or upwards. These 37 practitioners have had occasion to perform the four major amputations of the limbs upon 821 patients, with the following results :—

Total number of cases, 821 ; of deaths, 67 ; or 1 in every 12·2 died, or 8·1 in every 100.

Total number of amputations for injury, 561 ; deaths, 46 ; or 1 in every 12·2 died, or 8·2 in every 100.

Total number of amputations for disease, 260 ; deaths, 21 ; or 1 in every 12·4, or 8 in every 100.

Mortality of the Four Major Amputations for Injuries and for Diseases among 37 *Practitioners who have operated twelve times or oftener.*

Thigh	cases,	260 ;	deaths,	35 ;	or 1 in	7·4 ;	or 13·4 per cent.
Leg	„	250 ;	„	25 ;	or 1 in	10 ;	or 10·0 „
Arm	„	179 ;	„	6 ;	or 1 in	29·8 ;	or 3·3 „
[1] Forearm	„	132 ;	„	1 ;	or 1 in	132 ;	or 0·7 „

The preceding three summaries of results show—

1. That, up to a certain point at least, limb-amputations become more and more successful in the hands of rural and provincial practitioners in accordance with the experience which they have had of the operation.

2. That hence the country limb-amputations entered in Table I. would, in all probability, have shown a still higher rate of success than they do present, if they had been all performed by men who—like city hospital surgeons—were by experience accustomed to the operation. And,

3. That the three successive summaries show how, with increased experience, there occurs not merely an increased scale of success in limb-amputations taken as a whole, but also in the different limb-amputations taken individually. Thus, for example, amputations of the thigh are lost among the three classes of practitioners in correspondence with the amount of their experience in the following proportions : 22·7 per cent ; 16·5 per cent ; 13·4 per cent ; and so on with regard to the other special amputations.

7. On the Results of Limb-Amputations in Private Practice in other Countries.

The statistics of limb-amputations in country practice, collated in Chapter II. 2, are altogether derived from the experience of surgeons living in the rural and provincial parts of England, Wales, and Scotland. No statistics of a similar kind have, so far as I know, been collected previously in Great Britain or elsewhere. But, a short time ago, I sent to my friend, Dr. Nicolaysen of Christiania, a copy of the printed schedule which I had used here for collecting

[1] There are only 2 deaths under this heading in the whole Tables, 1 of which happens to be in this class, and thus alters the proportion so as to make it look worse than in the other Tables.

cases and their results; and he immediately began a series of similar inquiries regarding the danger and death-rate of amputations in Norway. The following tables, drawn up by himself, show the result of his investigations; and these are important in one respect. As far as they go, they confirm the conclusion that the average death-rate, after amputation of the limbs in private country practice, is about 1 in 9; and they show that the mortality is the same in Norway as in Great Britain.

Results of 82 *Amputations of the Thigh, Leg, Arm, and Forearm, in Private Practice in Norway.*

Thigh	cases, 29 ;	deaths, 5 ;	or 1 in 5·8 ;	or 17·24	per cent.
Leg	„ 30 ;	„ 2 ;	or 1 in 15 ;	or 6·6	„
Arm	„ 8 ;	„ 1 ;	or 1 in 8 ;	or 12·5	„
Forearm	„ 15 ;	„ 1 ;	or 1 in 15 ;	or 6·6	„
Total	cases, 82 ;	deaths, 9 ;	or 1 in 9·0 ;	or 10·9	per cent.

Of these amputations there were, *Primary or for Injury,—*

Thigh	cases, 8 ;	deaths, 2 ;	or 1 in 4 ;	or 25·0	per cent.
Leg	„ 9 ;	„ 0			
Arm	„ 6 ;	„ 1 ;	or 1 in 6 ;	or 16·6	„
Forearm	„ 11 ;	„ 1 ;	or 1 in 11 ;	or 9·1	„
Total	cases, 34 ;	deaths, 4 ;	or 1 in 8·5 ;	or 11·7	per cent.

Secondary or for Disease.

Thigh	cases, 21 ;	deaths, 3 ;	or 1 in 7 ;	or 14·3	per cent.
Leg	„ 21 ;	„ 2 ;	or 1 in 10·5 ;	or 9·5	„
Arm	„ 2 }	no deaths.			
Forearm	„ 4 }				
Total	cases, 48 ;	deaths, 5 ;	or 1 in 9·6 ;	or 10·4	per cent.

Seeing that limb-amputations are fatal in rural and provincial private practice in the proportion of 1 in 9, or less, let us next inquire what, on the contrary, is their death-rate in large and metropolitan hospitals.

8. Mortality of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in Large and Metropolitan Hospitals.

Formerly, when writing of the relative mortality of the four major amputations of the limbs in rural practice and in hospital practice, I ventured to lay it down as a proposition, that these amputations were about three times more fatal in our large and metropolitan hospitals than they were in the country; for while they were fatal in country and provincial private practice in the proportion of 1 in every 9 operated upon, they were fatal in the large hospitals of Edinburgh, Glasgow, and London, in the proportion of 1 in every 3 operated upon. (See *Medical Gazette* for January 16, 1869.)

To elucidate this important proposition, we shall therefore now investigate the mortality of these four amputations in the largest hospitals in Great Britain; and in doing so I shall use, as far as I have been able to procure them, the latest returns from these institutions. Our seven largest hospitals in this country are the Royal Infirmaries of Edinburgh and Glasgow; and in London, Guy's, St. Bartholomew's, St. George's, the London, and the Middlesex Hospitals. Each of these institutions has upwards of 300 beds; most of them about 500. St. Thomas's Hospital when rebuilt will, I believe, be still larger. At present, in its temporary quarters, it has only about 200 beds. As the problem refers to metropolitan hospitals, we shall include the statistics of other smaller London hospitals, as King's College, St. Mary's, the Westminster, the Royal Free Hospital, etc.

As the whole of the present inquiry has originated in questions connected with the rebuilding of the Edinburgh Hospital, let us begin with it.

I.—Royal Infirmary of Edinburgh.

This hospital contains 519 beds.[1] The mortality in its amputation cases has, at different and distant dates, been published by Dr. Monro, Dr. Reid, Dr. Peacock, and others. Dr. Fenwick of

[1] In these figures relative to the number of beds in different hospitals, I have chiefly followed the excellent official report of Dr. Bristowe and Mr. Holmes (see *Sixth Report of the Medical Officer of the Privy Council*), and Mr. Churchill's *Medical Directory*.

London, when writing on the mortality of amputations in 1848, states the number of cases of the major amputations performed in the Edinburgh Infirmary for 3½ years to be slightly more than 1 in every 2 (or more correctly 1 in 1·96). For the following Table of its amputation statistics during the last eight years, I am indebted to Mr. M'Dougall, the highly esteemed superintendent of the hospital, who drew it up with the permission of the Managers :—

TABLE IV.—RESULT OF THE FOUR AMPUTATIONS OF THE THIGH, LEG, ARM, AND FOREARM, IN THE ROYAL INFIRMARY, EDINBURGH, FROM 1859 TO 1868 INCLUSIVE.[1]

Year.	FOR INJURY.								FOR DISEASE.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1859-60	3	3	7	1	0	0	0	0	13	1	0	0	0	0	1	0
1860-61	4	3	5	1	3	1	4	0	9	1	3	0	1	0	0	0
1861-62	10	7	7	5	2	0	3	0	19	3	2	0	1	0	2	0
1862-63	4	4	10	5	1	0	7	0	12	6	5	1	1	1	2	1
1863-64	6	3	6	3	3	3	2	0	21	10	5	3	2	0	5	2
1864-65	5	5	3	2	3	1	7	2	14	4	3	2	1	1	5	4
1865-66	12	8	5	4	4	3	3	1	11	3	5	1	0	0	2	0
1866-67	10	9	8	3	4	3	8	1	14	10	1	0	1	1	1	0
1867-68	11	6	7	5	1	1	5	1	21	10	4	2	0	0	1	0
Total .	65	48	58	29	21	12	39	5	134	48	28	9	7	3	19	7
Mortality per cent.	73·8		50·0		57·1		12·8		35·8		32·1		42·8		36·8	
Or proportionally 1 in	1·3		2·0		1·7		7·8		2·8		3·1		2·3		2·7	

Total number of cases, 371 ; of deaths, 161 ; or 1 in every 2·3 died, or 43·3 in every 100.

Total number of amputations for injury, 183 ; deaths, 94 ; or 1 in every 2·0 died, or 51·3 in every 100.

Total number of amputations for disease, 188 ; deaths, 67 ; or 1 in every 2·8, or 35·6 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the different individual amputations during the above period in the Edinburgh Infirmary was as follows :—

Mortality of the Individual Amputations.

Thigh cases, 199 ; deaths, 96 ; or 1 in 2·1 ; or 47·2 per cent.
Leg „ 86 ; „ 38 ; or 1 in 2·2 ; or 44·2 „

[1] Ten fatal double amputations through the continuity of the bones, alluded to previously in Chapter II. 1, are not included.

Arm cases 28; deaths, 15; or 1 in 1·8; or 53·6 per cent.
Forearm „ 58; „ 15; or 1 in 4·8; or 20·7 „

II.—Royal Infirmary of Glasgow.

This great institution contains from 500 to 600 beds. The statistics of the amputations in the Glasgow Royal Infirmary at different periods have been published by Drs. Lawrie, Steele, M'Ghie, Watson, and others. The following Table includes the statistics of the limb-amputations in the hospital from 1850 to 1868. These returns are taken from the published yearly reports of the hospital, for access to a collection of which I am indebted to the excellent Registrar of the hospital, Dr. Thomas:—

Table V.—Result of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in the Royal Infirmary, Glasgow, from 1850 to 1868 inclusive.

Year.	For Injury.								For Disease.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No of. Cases.	Deaths.	No. of Cases.	Deaths.
1850	1	1	3	1	2	1	5	0	5	4	7	4	2	1	0	0
1851	1	0	1	0	0	0	1	0	6	5	8	3	0	0	0	0
1852	0	0	1	0	2	0	5	0	5	2	6	3	1	0	4	1
1853	0	0	4	1	7	1	2	0	16	7	10	4	5	2	2	0
1854	0	0	4	2	3	3	1	0	10	7	5	0	4	1	3	0
1855	3	1	2	2	2	1	2	0	5	3	10	1	0	0	1	0
1856	2	2	4	1	2	2	2	1	11	5	1	0	0	0	0	0
1857	4	3	5	3	2	1	3	2	8	2	1	1	1	0	0	0
1858	4	2	5	4	1	0	4	1	8	5	3	1	0	0	2	0
1859	5	4	7	5	6	2	5	0	7	1	2	0	0	0	0	0
1860	3	0	4	3	9	1	4	0	8	2	3	2	3	0	0	0
1861	7	4	9	6	3	2	6	2	14	7	6	3	2	1	2	0
1862	12	4	9	2	6	2	3	0	7	0	2	0	3	0	1	0
1863	13	10	3	1	6	2	5	1	14	4	3	0	0	0	0	0
1864	7	4	9	3	9	2	8	2	9	3	4	1	1	1	0	0
1865	8	6	7	6	14	2	6	0	12	4	1	0	0	0	0	0
1866	18	12	7	4	12	6	2	0	4	1	5	3	0	0	3	0
1867	3	1	8	5	6	5	2	0	13	2	3	0	0	0	0	0
1868	9	6	1	1	9	5	0	0	15	4	2	1	1	0	1	0
Total .	100	60	93	50	101	38	66	9	177	68	82	27	23	6	19	1
Mortality per cent.	60·0		53·7		37·6		13·6		38·4		32·9		26·0		5·2	
Or 1 in	1·6		1·8		2·6		7·3		3·0		3·0		3·8		19·0	

Total number of cases, 661; of deaths, 259. Hence 1 in every 2·5 died, or 39·1 in every 100.

Total number of amputations for injury, 360; deaths, 157; or 1 in every 2·3 died, or 43·6 in every 100.

Total number of amputations for disease, 301; deaths, 102; or 1 in every 2·9 died, or 33·8 in every 100.

Mortality of the Four Major Amputations, combining together Operations for Injuries and Operations for Diseases.

Thigh	cases, 277;	deaths, 128;	or 1 in 2·1;	or 46·2 per cent.
Leg	„ 175;	„ 77;	or 1 in 2·2;	or 44·0 „
Arm	„ 124;	„ 44;	or 1 in 2·8;	or 35·4 „
Forearm	„ 85;	„ 10;	or 1 in 8·5;	or 11·7 „

III.—St. Bartholomew's Hospital, London.

Of the London hospitals, this is the oldest in its foundation, and has always been held in high esteem. It contains 650 beds. I am obliged to Mr. Alfred Willett, the Surgical Registrar to the hospital, for copies of the official statistical reports, and for the following Table of the limb-amputations performed there since 1863, with their results.

Table VI.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in St. Bartholomew's Hospital, London, from 1863 to 1868 inclusive.

Year.	For Injury.								For Disease.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1863	7	5	10	4	3	1	4	2	22	10	5	0	1	0	1	0
1864	4	3	7	5	10	3	3	0	21	8	10	4	1	0	3	0
1865	7	3	4	4	2	0	5	0	14	7	14	6	5	0	3	0
1866	4	2	6	1	3	0	2	0	5	1	3	2	2	0	1	0
1867	2	1	4	1	1	0	2	0	9	4	2	1	3	0	1	0
1868	1	1	8	6	2	0	7	1	13	7	10	3	3	0	2	0
Total . .	25	15	39	21	21	4	23	3	84	37	44	16	15	0	11	0
Mortality per cent.	60·0		54·0		19		13·0		44·0		36·3					
Or proportionally 1 in	1·6		1·8		5·2		7·6		2·2		2·7					

Total number of cases, 262; of deaths, 96; or 1 in every 2·7, or 36·6 in every 100.

Total number of amputations for injury, 108; of deaths, 43; or 1 in every 2·5, or 40 in every 100.

Total number of amputations for disease, 154; of deaths, 53; or 1 in every 2·9, or 34·4 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations during the above period in St. Bartholomew's Hospital is as follows:—

Mortality from the Individual Amputations.

Thigh	cases, 109;	deaths, 52;	or 1 in 2·1,	or 47·7 per cent.
Leg	„ 83;	„ 37;	or 1 in 2·2,	or 44·5 „
Arm	„ 36;	„ 4;	or 1 in 9,	or 11·1 „
Forearm	„ 34;	„ 3;	or 1 in 11,	or 8·8 „

IV.—The London Hospital, Whitechapel.

The London Hospital is, according to Dr. Bristowe and Mr. Holmes, "the greatest surgical institution in the metropolis." It contains 500 beds, of which 350 are devoted to surgical cases. For the amputation returns for the year 1862 I am indebted to the report of Dr. Bristowe and Mr. Holmes; and for the years 1863, 4, 5, and 6, to the summaries given in the four published volumes of the *Clinical Lectures and Reports* of the Hospital. Those of the last two years, 1867-8, have been furnished me by Mr. Jonathan Hutchison, Surgeon to the Hospital, and Lecturer on Surgery at its Medical School.

Table VII.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in the London Hospital, from 1862 to 1868 inclusive.

Year.	For Injury.								For Disease.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1862	4	2	14	9	7	5	0	0	9	3	1	1	1	0	0	0
1863	4	4	7	5	6	3	0	0	5	1	5	0	0	0	3	0
1864	9	8	5	5	5	3	3	0	7	3	4	2	0	0	1	1
1865	9	7	7	6	4	1	0	0	11	6	3	2	0	0	0	0
1866	5	2	4	3	6	2	3	0	9	4	2	2	0	0	0	0
1867	3	3	7	2	2	1	2	0	15	4	3	1	1	0	0	0
1868	4	2	1	0	1	0	2	0	12	2	4	1	3	0	1	0
Total. .	38	28	45	30	31	15	10	0	68	23	22	9	5	0	5	1
Mortality per cent.	73·6		66·6		48·4		...		33·8		41		...		20	
Or proportionally 1 in	1·3		1·5		2		...		2·9		2·4		...		5	

Total number of cases, 224; of deaths, 106; or 1 in every 2·1, or 47·3 in every 100.

Total number of amputations for injury, 124; of deaths, 73; or 1 in every 1·7, or 58·8 in every 100.

Total number of amputations for disease, 100; of deaths, 33; or 1 in every 3, or 33 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations in the London Hospital is as follows:—

Mortality of Individual Amputations.

Thigh	cases, 106;	deaths, 51;	or 1 in 2·0;	or 48·1 per cent.
Leg	„ 67;	„ 39;	or 1 in 1·7;	or 58·2 „
Arm	„ 36;	„ 15;	or 1 in 2·4;	or 41·6 „
Forearm	„ 15;	„ 1;	or 1 in 15;	or 6·6 „

V.—Guy's Hospital, London.

This institution can make up 580 beds. My friend, Dr. Steele, the very able superintendent of the hospital, has for many years past kept up, among other things, its Statistical Register with most exemplary care and accuracy. I am under obligations to him for the data in the following Table:—

Table VIII.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in Guy's Hospital, London, from 1861 to 1868 inclusive.

Year.	For Injury.								For Disease.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1861	6	2	2	0	6	2	0	0	8	2	6	0	1	1	3	1
1862	7	3	8	4	1	0	4	0	7	4	6	1	2	0	0	0
1863	7	4	4	3	3	0	0	0	12	4	10	2	1	0	0	0
1864	5	4	11	6	1	1	1	1	11	4	4	1	1	1	0	0
1865	7	5	12	6	4	2	1	1	7	4	6	1	3	1	2	1
1866	4	2	8	6	5	2	3	1	10	1	6	1	1	1	1	0
1867	5	4	8	2	9	3	2	1	14	5	9	3	3	1	1	0
1868	5	2	0	0	2	2	4	2	14	3	2	0	1	0	1	0
Total . .	46	26	53	27	31	12	15	6	83	27	49	9	13	5	8	2
Mortality per cent.	56·5		51		38·7		40		32·5		18		38·4		25	
Or proportionally 1 in	1·8		1·9		2·6		2·5		3		5·4		2·6		4	

Total number of cases, 298; of deaths, 114; or 1 in every 2·6 or 38·2 in every 100.

Total number of amputations for injury, 145 ; of deaths, 71 ; or 1 in every 2, or 49 in every 100.

Total number of amputations for disease, 153 ; of deaths, 43 ; or 1 in every 3·5, or 28 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations in Guy's Hospital is as follows :—

Mortality of the Individual Amputations.

Thigh	cases, 129 ;	deaths, 53 ;	or 1 in 2·4 ;	or 41·0 per cent.
Leg	„ 102 ;	„ 36 ;	or 1 in 2·8 ;	or 35·3 „
Arm	„ 44 ;	„ 17 ;	or 1 in 2·6 ;	or 38·6 „
Forearm	„ 23 ;	„ 8 ;	or 1 in 2·8 ;	or 34·7 „

VI.—St. George's Hospital, London.

This hospital contains 350 beds ; 200 of which are set aside for surgical cases. Like all the large London hospitals, the edifice consists of four flats or storeys. For the following data, in regard to the limb-amputations performed in St. George's during the last five years, I stand indebted to the kindness of Mr. Leigh, Registrar to the hospital.

Table IX.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in St. George's Hospital, London, from 1864 to 1868.

Year.	FOR INJURY.								FOR DISEASE.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
1864	1	0	4	0	0	0	1	0	7	2	7	2	0	0	2	0
1865	0	0	0	0	4	2	1	0	12	4	7	2	4	0	0	0
1866	3	2	0	0	0	0	0	0	13	7	13	3	1	1	0	0
1867	0	0	1	0	1	0	0	0	11	7	8	2	2	2	1	0
1868	1	1	3	2	2	2	0	0	11	5	1	1	1	1	3	1
Total . .	5	3	8	2	7	4	2	0	54	25	36	10	8	4	6	1
Mortality per cent.	60		25		57		...		46·3		27·7		50		16·6	
Or proportionally 1 in	1·6		4		1·8		...		2·1		3·6		2		6	

Total number of cases, 126 ; of deaths, 49 ; or 1 in every 2·5, or 38·8 in 100.

Total number of amputations for injury, 22 ; of deaths, 9 ; or 1 in every 2·4, or 41 in every 100.

Total number of amputations for disease, 104 ; of deaths, 40 ; or 1 in every 2·6, or 38·4 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations in St. George's Hospital is as follows:—

Mortality of the Individual Amputations.

Thigh	cases, 59;	deaths, 28;	or 1 in 2·1;	or 47·4	per cent.
Leg	„ 44;	„ 12;	or 1 in 3·6;	or 27·2	„
Arm	„ 15;	„ 8;	or 1 in 1·8;	or 53·3	„
Forearm	„ 8;	„ 1;	or 1 in 8;	or 12·5	„

VII.—Results of Limb-Amputations in Nine Metropolitan Hospitals.

I have been kindly furnished by Mr. Arnott, Mr. Bell, Mr. Murphy, Mr. Holt, and Dr. Black, with the amputation statistics of five other London hospitals, in addition to those of the four chief metropolitan hospitals adduced in the four preceding Tables. But as these five hospitals are all smaller, and hence their returns not so important, nor in some respects so complete or continuous, it is unnecessary perhaps to print them at length. In the succeeding Table, therefore, I will take the liberty of conjoining them and the four preceding hospital returns—so as thus to have a general and connected view of the mortality attendant upon limb-amputations in these nine metropolitan hospitals, taken either individually or as a whole.

Table X.—Latest Results of the Four Amputations of the Thigh Leg, Arm, and Forearm, in Nine London Hospitals.

Name of Hospital.	Years of Observation.	For Injury.								For Disease.							
		Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
		Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
St. Bartholomew's	1863-68	25	15	39	21	21	4	23	3	84	37	44	16	15	0	11	0
St. George's . .	1864-68	5	3	8	2	7	4	2	0	54	25	36	10	8	4	6	1
Guy's	1861-68	44	26	53	27	30	12	15	6	83	27	49	9	13	5	5	2
London	1862-68	38	28	45	30	31	15	10	0	68	23	22	9	5	0	5	1
Middlesex . . .	1867-68	1	0	4	3	1	1	0	0	0	0	0	0	1	1	1	1
King's College . .	1863-68	1	1	1	1	0	0	1	0	14	5	5	1	1	1	4	2
Royal Free . . .	1862-68	9	6	23	15	6	2	8	1	6	1	2	0	1	1	1	0
Westminster . .	1861-67	14	9	5	3	0	0	0	0	5	4	14	7	4	1	3	0
St. Mary's . . .	1868	2	0	1	0	1	0	5	1	6	1	1	1	0	0	1	0
	Total .	139	88	179	102	97	38	64	11	320	123	173	53	48	13	37	7
Mortality per cent . .		63·3		57		39·1		17·1		38·4		35·7		27·0		18·9	
Or proportionally 1 in .		1·6		1·7		2·5		5·8		2·6		3·2		3·7		5·2	

Total number of cases, 1057 ; of deaths, 435 ; or 1 in every 2·4 died, or 41·1 in every 100.

Total number of amputations for injury, 479 ; deaths, 239 ; or 1 in every 2·0 died, or 50 in every 100.

Total number of amputations for disease, 578 ; deaths, 196 ; or 1 in every 2·9 died, or 33·9 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations in the above-mentioned London hospitals was as follows :—

Mortality of the Individual Amputations.

Thigh cases, 459 ; deaths, 211 ; or 1 in 2·1 ; or 46·0 per cent.
Leg „ 352 ; „ 155 ; or 1 in 2·2 ; or 44·0 „
Arm „ 145 ; „ 51 ; or 1 in 2·8 ; or 35·1 „
Forearm „ 101 ; „ 18 ; or 1 in 5·5 ; or 17·8 „

VIII.—Eleven Large and Metropolitan British Hospitals.

If we throw together into one table the data spread over the preceding seven tables, including together the amputation statistics of the Edinburgh and Glasgow Infirmaries, and of nine of the leading London Hospitals, the result is as follows :—

Table XI.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in Eleven Large and Metropolitan Hospitals ; all Amputations through the Joints being excluded.

Hospitals.	FOR INJURY.								FOR DISEASE.							
	Thigh.		Leg.		Arm.		Fore-arm.		Thigh.		Leg.		Arm.		Fore-arm.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
Edinburgh Infirmary .	65	48	58	29	21	12	39	5	134	48	28	9	7	3	19	7
Glasgow Infirmary . .	100	60	93	50	101	38	66	9	177	68	82	27	23	6	19	1
Nine Metropolitan Hospitals	139	88	179	102	97	38	64	11	320	123	173	53	48	13	37	7
Total . .	304	196	330	181	219	88	169	25	631	239	283	89	78	22	75	15
Mortality per cent . .	64·4		54·8		40·1		14·8		37.8		31·4		28·2		20	
Or proportionally 1 in .	1·5		1·8		2·5		6·7		2·6		3·2		3·5		5	

Total number of cases, 2089 ; of deaths, 855 ; or 1 in 2·4, or 41 per cent

Total number of amputations for injury, 1022; of deaths, 490; or 1 in 2·1, or 48 per cent.

Total number of amputations for disease, 1067; of deaths, 365; or 1 in 2·9, or 34·2 per cent.

Mortality of Individual Amputations.

Thigh	cases, 935;	deaths, 435;	or 1 in 2·1, or 46·5	per cent.
Leg	„ 613;	„ 270;	or 1 in 2·2, or 44·0	„
Arm	„ 297;	„ 110;	or 1 in 2·7, or 37·0	„
Forearm	„ 244;	„ 40;	or 1 in 6·0, or 16·4	„

Mortality from the Amputations for Injury in the Edinburgh and Glasgow Infirmaries, and in Nine London Hospitals.

Thigh	cases, 304;	deaths, 196;	or 1 in 1·5, or 64·4	per cent.
Leg	„ 330;	„ 181;	or 1 in 1·8, or 54·8	„
Arm	„ 219;	„ 88;	or 1 in 2·3, or 40·1	„
Forearm	„ 169;	„ 25;	or 1 in 6·7, or 14·7	„

Mortality from the Amputations for Disease in the same Hospitals.

Thigh	cases, 631;	deaths, 239;	or 1 in 2·6, or 37·8	per cent.
Leg	„ 283;	„ 89;	or 1 in 3·1, or 31·4	„
Arm	„ 78;	„ 22;	or 1 in 3·5, or 28·2	„
Forearm	„ 75;	„ 15;	or 1 in 5·0, or 20·0	„

9. The Proportionate Death-rate and the Excess of Mortality of Limb-Amputations in large and Metropolitan Hospitals, as compared with Country Practice.

The total number of limb-amputations collected from private country and provincial practice contained in Table I. amounts to 2098. On counting up the limb-amputations collected from eleven large and metropolitan hospitals, and contained in Table XI., the total number happens accidentally to be very nearly the same, for it amounts to 2089.

The whole collected number operated upon is thus nearly similar in hospital practice and in rural practice. But the results as to the relative number of lives lost in these two types or places of practice is immensely different.

After the 2098 limb-amputations in the country, 226 of the patients died. After the 2089 limb-amputations in eleven large and metropolitan hospitals, 855 of the patients died.

The mortality after limb-amputations in the country is thus 1 in 9·2 (see Chapter I. 2), and after limb-amputations in large and metropolitan hospitals 1 in 2·4 (see Chapter II. 8). Hence the number that die after these operations is in such hospital practice, when compared with rural practice, nearly FOUR TIMES GREATER.

But the experienced country surgeon loses—as we have seen (see Chapter II. 6)—only 1 in every 12·4 of his patients upon whom he performs limb-amputation. Hence the experienced country surgeon operating upon his patients in poor cottages and villages is —as compared with the experienced city surgeon, operating upon his patients in rich and magnificent hospitals—FIVE TIMES MORE SUCCESSFUL.

In some minor amputations—and hence, I believe, in other minor operations also—the contrast is still more marked between the success of amputation in country practice and in the practice of large hospitals. In country practice, after amputation of the forearm, 2 died out of 377 cases, or 1 in every 188 operated on. In eleven large and metropolitan hospitals, out of 244 cases of amputation of the forearm, 40 died, or 1 in every 6 operated on. Hence, according to these data, the death-rate in hospital practice was, as compared with the death-rate in rural practice in this individual operation, THIRTY TIMES GREATER.

The tremendous differences between the two practices may, perhaps, be more pointedly and simply stated thus :—

Out of **2089** amputations in hospital practice, **855** died ;
Out of **2098** amputations in country practice, **226** died ;

Giving an excess to hospital practice of **629** deaths.

This excess, in about 2100 limb-amputations, of 629 deaths in hospital practice as compared with rural practice—in our palatial hospitals as compared with our rural villages and cottages—in large wards as compared with isolated rooms—is certainly much greater and more pronounced than I myself expected when I began the present inquiry. But must the calling of this dismal death-roll still go on unchallenged and unchecked ? Shall this pitiless and deliberate sacrifice of human life to conditions which are more or less preventable, be continued or arrested ? Do not these terrible figures plead eloquently and clamantly for a revision and reform of our existing hospital system ?

CHAPTER III.

SOME PROPOSITIONS ON HOSPITALISM, BASED ON STATISTICS OF COUNTRY AND HOSPITAL AMPUTATIONS.

PROPOSITION I.—*The aggregation or isolation of patients regulates, in a marked degree, the results of operations, etc., upon them.*

IF we take, for instance, as Mr. Holmes[1] and others have done, the mortality accompanying the four major amputations of the limbs—viz. amputation of the thigh, leg, arm, and forearm—as a test of the salubrity of the different hospital systems and conditions under which patients may be placed, the results shown by the amputation statistics which I have collected appear to me to open up some new views as to hospital and domestic hygiene. I have collated about 7000 limb-amputations performed of late years in Great Britain, in hospital and private practice. I have already given the details of above 2000 cases in private country practice, and above 2000 cases in the practice of our large and metropolitan hospitals. Nearly 3000 cases which have been reported to me from provincial hospitals—large and small—require to be reduced. The general outline of the whole inquiry may, as it at present stands, be shown in this table :—

TABLE XII.—PROPORTIONATE MORTALITY OF LIMB-AMPUTATIONS IN GREAT BRITAIN, AS REGULATED BY THE SIZE OF THE HOSPITAL AND THE DEGREE OF AGGREGATION OR ISOLATION IN WHICH THE PATIENTS ARE PLACED.

Size of Hospital, etc.	Death-rate.
1. In hospitals from 300 beds to 600 . . .	1 in 2½ die
2. In hospitals from 100 beds to 300 . .	1 in 4 ,,
3. In hospitals from 25 beds to 100 . . .	1 in 5½ ,,
4. In cottage hospitals under 25 beds . . .	1 in 7 ,,
5. In isolated rooms in country practice . .	1 in 9 ,,

[1] The Report of Mr. Holmes and Dr. Bristowe is embodied in one of the Government Blue-books for 1863, entitled *Sixth Report of the Medical Officer of the Privy Council*. Mr. Simon has written, as usual, for the volume, a very able preface or commentary.

These few figures teach a lesson of vast import in relation to hospital hygiene; and yet they seem to discourse so plainly as to require no comment.

Mr. Holmes started apparently, in his inquiry and reasoning, upon the opposite idea, that our present palatial hospital system is comparatively perfect; and that more especially the size of an hospital does not affect its success or unsuccess in the treatment and recovery of the patients admitted. He seems to be swayed and influenced throughout by this fundamental error. Thus, in his official Report, he observes: "If all the subordinate parts of the hospital have been well constructed for ventilation and space, and all are kept in perfect order by drainage and cleanliness — cleanliness, that is, of the patients and their bedding, as well as of all parts of the building — we have no hesitation in saying that the *size of the hospital is a matter of absolute indifference;* whether it contains a hundred patients or a thousand does not appear to exercise the least influence on the health of any individual in the hospital."

In a paper in the *Lancet*, Mr. Holmes, in a similar spirit, remarks: "With respect to small and large hospitals, I have searched in vain for any evidence that there is any difference in their salubrity, providing the cases are the same;" and "the alleged greater salubrity of cottage hospitals, or of small city hospitals, as compared with large ones, rests on no evidence whatever."[1]

But while Mr. Holmes thus regrets the want of any evidence to show the salubrity of small as compared with large hospitals, the very evidence which he thus professes to lack existed in his own elaborate Report of the hospitals which he visited under his commission, and whose statistics he published in his official Report. He has so far misinterpreted his own data in his own Report. He has given a table of the amputations in upwards of sixty British hospitals. Seventeen of these hospitals contained 100 beds or less, and the others from 100 to 600 beds. The following Table presents the relative mortality accompanying limb-amputations in these two classes of hospitals, and shows the death-rate in the hospitals under 100 beds to be even smaller than my data indicate.

TABLE XIII.—MORTALITY OF LIMB-AMPUTATIONS AS REGULATED BY THE SIZE OF THE HOSPITAL, FROM THE DATA PUBLISHED IN MR. HOLMES'S OFFICIAL REPORT (1863).

Size of Hospital.	Death-rate.
1. In hospitals from 100 to 600 beds . . .	1 in 4 die.
2. In hospitals under 100 beds	1 in 6 „

[1] *Lancet*, July 1869, p. 230.

The seventeen hospitals with 100 beds or less, contained in Mr. Holmes's list, reported to him 209 limb-amputations, with 36 deaths.

In the course of their inquiry, Mr. Holmes and Dr. Bristowe, singly or together, "visited (says Mr. Simon) in succession almost every considerable hospital in England and Scotland, and all the more important hospitals of Ireland" (*Sixth Report*, p. 38). Unfortunately, however, they seem to have looked upon "cottage hospitals" as undeserving of being included within their official Reports, though they visited some of them (see p. 643). They refer to them, however, in rather deprecatory terms, as bearing upon the present question of the aggregation and isolation of patients in relation to the salubrity of hospitals. "Very small hospitals," they pointedly observe (p. 510), "are really as healthy as large ones. . . . But it is," they add, "a *farce* to compare such places (cottage hospitals) to large roomy hospitals, considered abstractedly with reference to their relative fitness for the treatment of acute cases." Yet limb-amputations are (see Table I.) nearly three times as fatal in our large, roomy, and metropolitan hospitals, as they are in our small cottage hospitals!

As to the diversities displayed in the death-rates in Table XII. of limb-amputations in different classes of hospital, and under other conditions, I believe them to be regulated on a large scale by the degree of aggregation or isolation of the sick; for, at least, we have reason for laying down the following, as another proposition, viz.—

PROPOSITION II.—*The preceding differences in the death-rates of different classes of Hospitals are "not to be explained by differences either in the constitution of those operated upon, or in the nature of the injuries or diseases for which they were admitted."*

According to one of the many wise observations on the question of hospitalism made by Miss Nightingale, and quoted authoritatively and approvingly by Sir Ranald Martin in Mr. Holmes's *System of Surgery*, "many well-intentioned persons at the present day" incorrectly account "for the high rate of mortality in civil hospitals—that only the worst cases were sent there, and sent only to die." (See Mr. Holmes's *Surgery*, vol. iv. p. 1028.) Mr. Holmes himself, indeed, has used this same argument, in a greater or less degree, to account for the high death-rate after amputations in the London hospitals, as compared with country hospitals, and above all with

country private practice. But, at the same time, he practically gainsays it altogether and rather effectually, in one part of his own official Report; and thus he saves me from the task of answering his reasoning on this point by answering it himself. For the facts stand as follows:—

Under the sanction of the Lords of the Privy Council, Mr. Holmes and Dr. Bristowe, besides visiting the leading British and Irish hospitals, were authorised to make also an inspection "of the chief Parisian hospitals and of their records" (p. 38). In connection with this latter portion of their work they have given (at p. 563) a lengthened table of the amputations in the Paris hospitals during 1861. According to this table, there were performed in the Paris hospitals during that year, 102 amputations of the thigh, leg, arm, and forearm; and 67 of the 102 patients died; or nearly 1 in $1\frac{1}{2}$ (or 1 in 1·5). In the elaborate official quarto Report of the Parisian Hospitals for 1861, published by M. Husson, the number of limb-amputations for this same year (1861) in all the Parisian hospitals is given as 81; and he shows in his tables that 64 out of these 81 died; or 1 in 1·2. The mortality, then, from limb-amputations in the hospitals of Paris is greatly higher than in the hospitals of London. For—

In the Parisian hospitals above 1 in $1\frac{1}{2}$ die;
In the London hospitals about 1 in $2\frac{1}{2}$ die.[1]

Mr. Holmes and Dr. Bristowe comment upon this great relative surplus in the mortality of the limb-amputations in the hospitals of Paris as compared with the hospitals of London, and strongly observe: "The death-rates (in Paris) are *evidently enormous*, and surely *not* to be explained by differences either in the constitution of those operated upon, or in the nature of the injuries or diseases for which they were admitted." We quite agree with them in these observations; but if the observations hold true of the Parisian hospitals as compared with those of London—do they not hold with equal truth of our large and small British hospitals, as compared with each other?

In Tables XII. and XIII. we have seen that the hospitals in our own country vary, as a whole, in their salubrity as far as limb-amputations are concerned—in relation to their size, and the degree of aggregation or isolation of the sick. We know of no other general cause that can explain this general result, and when we compare the mor-

[1] The mortality is, to state it in other words, four or five times greater in limb-amputations in the Parisian hospitals than it is in the smaller hospitals of England and Scotland.

tality from limb-amputations in the large and metropolitan hospitals of London, Edinburgh, and Glasgow, of 1 in every 2½ operated upon, with the amputation death-rate in the smaller or provincial hospitals of Bristol, Norwich, Leeds, Aberdeen, etc., of 1 in every 4 operated upon, or the diminishing death-rate in the still smaller hospitals of Canterbury, Salisbury, Truro, Paisley, etc., of less than 1 in every 5 or 6 operated upon, we confess, in the words of Mr. Holmes and Dr. Bristowe, that the death-rates in our large British hospitals, as compared with the death-rates in our smaller British hospitals, "are evidently enormous, and surely *not* to be explained by differences either in the constitution of those operated upon, or in the nature of the injuries or diseases for which they are admitted."

An amputation, for example, of the thigh, or of the forearm, for injury or for disease, is assuredly the same operation, and called for by the same circumstances, whether it be performed within the walls of a metropolitan or of a cottage hospital ; but the differences in the results of such operations upon a large scale in these diverse localities are, in the language of Mr. Holmes and Dr. Bristowe, "truly enormous." And if the death-rate in such limb-amputations in our smaller hospitals, and still more in isolated houses and dwellings in the country (see Table XII.) is "the normal rate" of mortality of limb-amputations, then is not the excess over that normal rate observed in our large hospitals a painful and preventable waste of human life, capable of being averted by new hospital arrangements? One chief aim, let us not forget, of all surgical as well as all medical science, is the prevention of all preventable deaths.

PROPOSITION III.—*Limb-amputations are more than three times as fatal in our large and metropolitan hospitals than the same operations are in private country and provincial practice.*

This contrast, however, in regard to the relative perils of the four major amputations of the limbs, requires to be stated more explicitly than I have already stated it,[1] and in various correlative propositions, as it bears greatly upon the present discussion. Let me therefore premise that above twenty years ago I published the opinion that a great saving of human life would be effected in our hospitals (1) medical, (2) obstetrical, and (3) surgical—if they were

[1] See Table XII. 1 and 5.

all changed from palaces to villages, and from large wards into isolated rooms.[1] At that time no statistical proofs had, as far as I was aware, been collected on the subject.

I elsewhere[2] pointed out two years ago that any direct evidence on the question of (1) *medical* hospitals was still wanting. But perhaps it will yet be found, in ascertaining the relative mortality of typhus or typhoid fever, or some other medical disease as treated in hospitals or at the patients' own homes in the city or in the country.

Latterly, large statistics have been collected on the same question in relation to (2) *obstetrical* hospitals. Dr. Lefort of Paris, for example, in a remarkable work on Maternities, has collated the results of nearly a million women delivered in the lying-in hospitals of Europe, and nearly an equal number of pauper patients confined in their own homes, with the following results :—

TABLE XIV.—RELATIVE MORTALITY ATTENDANT ON PARTURITION AMONG POOR PATIENTS DELIVERED IN LYING-IN HOSPITALS, AND DELIVERED AT THEIR OWN HOMES.

Of 888,302 delivered in hospitals .	30,394 died, or 1 in 29.
Of 934,781 delivered at home . .	4,405 died, or 1 in 212.

These two sets of patients belong, according to Dr. Lefort, to the same class of society; and, without entering into various questionable points connected with the table, we cite it here as sufficient to show—what is now generally acknowledged—namely, the greater danger to life which women undergo who are confined in lying-in hospitals, as these and other hospitals are at present constructed and conducted.

Two or three years ago[3] I suggested as an important correlative question in regard to surgical patients in (3) *surgical* hospitals, as compared with surgical patients treated at home, that the same law holds good in respect to them as in respect to obstetrical patients. In other words, are not operations relatively more dangerous and fatal among the poor patients lodged in the wards of our large hospitals than among the poor patients who are submitted to the knife of the surgeon in their own isolated dwellings?

The criterion most frequently adopted for testing the general success of surgical operations as a whole—under different conditions and in different hospitals—is the relative mortality of the four

[1] *Edinburgh Journal of Medical Science*, for November 1848, p. 328.
[2] *Transactions of the Social Science Association* for 1867, p. 115.
[3] *Transactions of Belfast Meeting of Social Science Association.*

major amputations of the limbs. For limb-amputations do not require for their execution any such high exercise of surgical skill as herniotomy, lithotomy, excision of joints, and other operations do ; and besides limb-amputations are performed in such numbers, both in hospital and private country practice, as to afford an adequate basis of comparison.[1]

In consequence, I have collated and published the results of 2098 limb-amputations performed in country private practice, and 2089 limb-amputations performed in our large and metropolitan hospitals. The difference between these two classes of amputations is shown in the following table :—

TABLE XV.—RELATIVE MORTALITY BETWEEN TWO SERIES OF LIMB-AMPUTATIONS IN RURAL PRACTICE AND IN THE PRACTICE OF OUR LARGE AND METROPOLITAN HOSPITALS.

Total country cases, 2098 ; deaths, 226, or 1 in 9 died.
Total hospital cases, 2089 ; deaths, 855, or 1 in 2½ died.

But the contrast between the success of limb-amputations in rural private practice and in city hospital practice, becomes even more marked when we use for comparison the results obtained by experienced country surgeons with the results obtained by city hospital surgeons, who are relatively much more experienced in such operations than is the general run of country medical men. In other words, it may be laid down as another

PROPOSITION IV.—*In country practice increased experience in amputations gives a still higher ratio of success to the results of the operations.*

The 2098 limb-amputations which have been reported to me from the country were performed by 374 practitioners.

Of these 374 medical gentlemen, 255 had amputated less than six times ; 72 of them only once ; 82 twice, etc. ; and altogether they had removed 629 limbs. Of the 374 practitioners, 72 had amputated from six to twelve times, and removed 648 limbs in all. While of the 374, the remainder (37) had amputated twelve times or oftener, and had cut off altogether 821 limbs.

The bearing of the effects of experience upon the results is shown in the following table.

[1] According to Professor Lawrie, amputation "in hospitals exceeds manifold all the other capital operations combined."—*Medical Gazette* for December 1840, p. 394.

TABLE XVI.—THE DEATH-RATE OF LIMB-AMPUTATIONS IN COUNTRY PRACTICE AS SO FAR VARIED BY THE EXPERIENCE OF THE OPERATORS

Those who had under 6 amputations . .	lost 1 in 7.
Those who had from 6 to 12 ditto . . .	lost 1 in 9.
Those who had 12 or more ditto	lost 1 in 12.

The result is thus so far influenced that—as shown in the last line of the table—the experienced country surgeon operating upon his patients generally in cottages and villages, is, as compared with the experienced city surgeon operating upon his patients in rich and magnificent hospitals—*five times more successful.*

In supplement to Proposition III. we may add as

PROPOSITION V.—*Limb-Amputations in country practice are far more successful as compared with the practice of large hospitals, not only when taken as a whole, but when the amputations are taken singly and individually.*

Under the third proposition we have compared together the country and hospital mortality attendant on the four major amputations of the limbs when considered together or as a whole. The four single amputations, when taken individually, present—as might be expected—analogous differences. Out of 669 amputations of the thigh, collated from country practice, 123 died, or about 18 per cent; out of 935 amputations of the thigh in our large hospitals 435 died, or about 46 per cent. Out of 618 cases of amputations of the leg in country practice 82 died, or 13 per cent; while out of 613 leg-amputations in hospitals 270 died, or 44 per cent. Out of 344 cases of amputations of the arm in country practice 14 died, or 4 per cent; while out of 297 similar cases in our large hospitals 110 died, or 37 per cent. And out of 378 cases of amputations of the forearm in country practice 2 died, or about ½ per cent; while out of 244 cases of the same operation in hospital practice 40 died, or about 16 per cent. These data may appear more clear if tabulated thus:—

TABLE XVII.—DIFFERENCES IN THE PERCENTAGES OF DEATH AFTER INDIVIDUAL AMPUTATIONS, AS OBSERVED IN 2089 LIMB-AMPUTATIONS IN HOSPITAL PRACTICE, AND 2098 IN COUNTRY PRACTICE.

Site of Amputation.	In large Hospitals.	In Country.
Thigh .	46 in 100 . .	18 in 100
Leg .	44 „ . .	13 „

Site of Amputation.	In large Hospitals.	In Country.
Arm . .	37 in 100 . .	4 in 100
Forearm . .	16 „ . .	0·5 „

The preceding data point to a still more important and unexpected result, which may be stated in another proposition as follows :—

PROPOSITION VI.—*The contrast between the mortality of amputations in country practice, as compared with hospital practice, is proportionally more marked and pronounced in the slighter than in the greater amputations of the limbs.*

The four amputations of the limbs vary in their mortality, as is well known, in the following order—namely, 1, and most perilous, the thigh ; 2, the leg ; 3, the arm ; and 4, the forearm. The fatality, however, attendant in country practice, as compared with city hospital practice, upon amputation of the thigh, is much less marked than is the difference attendant upon amputation of the forearm. The following table points out this and other circumstances pertaining to the present proposition :—

TABLE XVIII.—CONTRAST OF THE DEATH-RATES IN COUNTRY AND CITY HOSPITAL PRACTICE AMONG THE FOUR MAJOR AMPUTATIONS OF THE LIMBS.

Individual Amputations.	Death-rate in Hospitals.	Death-rate in Country.
Amputation of thigh . .	1 in 2 die . .	1 in 5½ die.
Amputation of leg . .	1 in 2⅓ die . .	1 in 7½ die.
Amputation of arm . .	1 in 2½ die . .	1 in 22 die.
Amputation of forearm .	1 in 6 die . .	1 in 189 die.

Thus, whilst in amputations of the lower extremity the success of country operations over hospital operations is somewhat about three times greater, that of amputations of the arm in the country is about eight or nine times greater than in the practice of our large and metropolitan hospitals. And the relative differences in amputations of the forearm under the two conditions are so marked as to deserve some separate consideration—the more so as Mr. Holmes adverts to them specially.

PROPOSITION VII.—*Amputations of the forearm are about twenty or thirty times more successful in country practice than in the practice of our large and metropolitan hospitals.*

In his account of the amputations at St. George's Hospital, Mr.

Holmes discards the statistical worth of a tabulation of amputations of the forearm in the following terms:—"Nor have I thought it necessary to tabulate the amputations of the forearm, inasmuch as death after this operation is so rare that no useful end would be gained by such tabulations."[1]

I believe that Mr. Holmes will get few or no statisticians to join him in this peculiar opinion, for in reality the statistics of forearm amputation are—in such an inquiry, for example, as the present—full of paramount interest and importance.

In my collection of 2098 country and 2089 hospital amputations, the proportions of forearm amputations and their results stand as follows:—

TABLE XIX.—NUMBERS AND RELATIVE FATALITY OF FOREARM-AMPUTATIONS IN COUNTRY AND IN HOSPITAL PRACTICE.

In country practice, out of	378	cases,	2 died, or 1 in 189.
In hospital practice, out of	244	cases,	40 died, or 1 in 6.

With reference to these 40 forearm-amputations in the last line, Mr. Holmes demands how is it possible to know that this excess of 40 deaths in hospital practice "is caused by hospital arrangements, without knowing the *causes* of death?" and he cites a conversation with me on the matter, in which it seems I mentioned the *causes* of death were not necessary elements in an inquiry into the simple *rates* of death after forearm or other amputations under different conditions of practice. Mr. Holmes has here, it appears to me, confounded two things that are quite different, or rather two different and distinct kinds of inquiry. In Table XIV. we have seen Dr. Lefort laying it down as the result of his extensive statistical investigations into the mortality of parturition in hospital practice and in home practice, that whilst 1 in 29 women die who are delivered in hospitals, only 1 in 212 die who are delivered in their own isolated homes. That important statistical induction as to the high rate of death of women in lying-in hospitals, is a deduction totally independent of what is the cause of death in these "1 in 29 women" who thus die, according to Dr. Lefort, in our maternity hospitals after parturition. The causes of the excessive mortality of parturient women in hospital practice form a totally different question from the mere statistical fact of that great excess itself. So, in the same way, the startling fact that amputation of

[1] See *St. George's Hospital Reports*, vol. i.

the forearm is twenty or thirty times more dangerous and lethal in the practice of our large and metropolitan hospitals than in private country practice, is a fact totally independent of what are the causes of death which lead to this peril and fatality in the wards of our palatial hospitals from comparatively so simple an operation, and one, seldom or never almost either preceded or followed by any great shock to the system, such as we see in amputations of the thigh and leg. The *rates* of death and the *causes* of death after amputations are, in short, two different questions.

But the causes of such mortality form a subject of investigation which, while it is distinct from, is at the same time second only in importance to, the ascertainment of the mortality itself. Mr. Holmes seems inclined to argue that the causes of the excessive mortality of the forearm-amputations in hospitals are not traceable to "hospital unhealthiness." But we may lay down the contrary as a simple and just inference—thus:

PROPOSITION VIII.—*The deaths after amputation of the forearm in hospitals result, in the main, from those pathological causes which are usually ascribed to morbific hospital influences.*

The general and acknowledged cause of death after operations in hospitals is some of the forms of surgical fever—as pyæmia, erysipelas, phagedena, etc.

Mr. Holmes himself has ably described these complications as the general pathological causes of death after surgical operations in surgical hospitals. "All surgeons" (observe he and Dr. Bristowe in their Report, p. 544) "attached to large metropolitan hospitals are aware that their operations are apt occasionally to be carried off by the supervention of erysipelas, pyæmia, or hospital gangrene, or some allied unhealthy form of inflammation. . . Their frequent recurrence, or their prevalence in an endemic form, is an indication of unhealthiness in the hospital. . . Erysipelas sometimes, but not frequently, proves fatal to surgical operations. Pyæmia is certainly the *most* commonly fatal of secondary surgical affections" (p. 546).

Mr. Holmes states[1] that during the last fourteen years 4 patients have died in St. George's Hospital out of 41 upon whom amputation of the forearm has been performed. In the *St. George's Hospital Reports*, vol. i., he gives an account of the causes of death in these cases as follows:—"One died of pyæmia in fifty-two days." In a

[1] See *Lancet*, August 1869, p. 2978.

second fatal case "the wrist-joint had been disorganised by an attack of pyæmia, from which the patient had recovered. It became necessary to remove the hand, but the pyæmia recurred, and he died on the forty-fifth day after amputation." In a third case "the patient died of secondary hemorrhage on the thirty-third day, the vessels being diseased." "No hospital arrangements (pleads Mr. Holmes) will prevent brittle arteries from bleeding sometimes;" but in isolated country practice, the vessels, and indeed the wound itself, would in all likelihood have been closed, and all chances of hemorrhage averted, long before the thirty-third day of the cure. In the fourth fatal case, which, says Mr. Holmes, "was a primary amputation," the patient (aged eighty-one) was attacked with senile gangrene in the legs, and "she died on the thirty-eighth day." In the same paper Mr. Holmes states that in the Amputation Book of St. George's Hospital, kept for the last sixteen years, "there have been 4 deaths after amputation of the forearm among 41 cases," giving a mortality of 1 in 10. But Mr. Holmes has described what is apparently a fifth case. To use his own words, "the man was very old—nominally sixty-seven, really perhaps many years older,—and obviously near the end of his life when admitted. The operation was done to free him from the exhaustion caused by abscess of the wrist; but he gradually sank, as he had been sinking before admission," etc.[1]

Without stopping to debate whether these cases chiefly sank, as I think, from causes of death pertaining more or less to hospitalism and its effects, let me add that the experience of other hospitals leaves no doubt on this subject.

Mr. M'Dougall and Dr. Aitken have, for example, ascertained for me the registered causes of death in the fatal cases of amputation of the forearm that have taken place in the Edinburgh Infirmary during the last four years; for it seemed quite unnecessary to push the inquiry farther. From 1864 to 1868 there occurred in the infirmary 23 primary amputations of the forearm with 5 deaths, and 9 secondary or pathological amputations with 4 deaths. The cause of death in 1 of the primaries is noted as unknown or unascertained; in 2 it resulted from tetanus; and in

[1] If this is, as it appears, a fifth case of death, then the mortality in forearm-amputations at St. George's in the sixteen years referred to would be 5 in 40, or 1 in 8. It appears not to be the fourth fatal case referred to in Mr. Holmes's paper in the *St. George's Reports*, because the patient was a male, the other a female; the operation was for disease, and not "primary;" and the patient was sixty-seven years of age or more, the other eighty-one years.

the remaining 2 from pyæmia. Of the 4 deaths after amputation for disease, 1 was entered as from shock, and the 3 others all died from pyæmia.

Of 8 deaths after amputation of the forearm at Guy's Hospital, Dr. Steele informs me that 3 were from pyæmia; a fourth was from pleurisy (perhaps merely one form of pyæmia); a fifth from tubercular phthisis; and a sixth from phagedenic sloughing and pleurisy (possibly pyæmic). A seventh death was from tetanus; and the eighth from cancer of internal organs.

Perhaps another piece of evidence in regard to the fatality of forearm-amputations in hospitals may be added. Mr. Holmes and Dr. Bristowe had, as already stated, an opportunity given them by the Lords of the Council of visiting the hospitals of Paris, and of inspecting them and "their records." They have published, as we have already seen, from these official records the amputation statistics of the Parisian hospitals for 1861. In the authorised records of that year, as published by M. Husson, there are reported 7 cases of amputation of the forearm in the different Parisian hospitals. Of these 7 cases 4 died. Of the 7 cases, 4 are noted as having been attacked with erysipelas; 1 of the 4 had in addition abscesses, and 1 is entered with "purulent" infection or pyæmia—all of them so many forms and varieties of that surgical fever which is so destructive in hospitals.

In M. Husson's Report of the Statistics of the hospitals of Paris for the subsequent year (1862) 18 cases of amputation of the forearm are recorded, with 6 deaths. The cause of death after one of these 6 fatal forearm-amputations is not reported. Of the remaining 5 cases, one died of phthisis, a second of erysipelas, and the remaining three of purulent infection or pyæmia.

When speaking of the "enormous" death-rates after limb-amputations in the Parisian hospitals, as compared with the London hospitals, Mr. Holmes and Dr. Bristowe remark that "pyæmia and such diseases are in great measure the causes of these alarming results." So are they also the apparent causes of the higher death-rates after operations in our own large as compared with our smaller British hospitals, and of the "alarming results" attendant upon amputations of the forearm in country practice and amputations of the forearm in hospital practice.[1]

[1] The two fatal cases reported to me after amputation of the forearm, out of 378 cases operated on in country practice, are both returned as having died of "gangrene."

After such evidence, no surgical pathologist will, I am inclined to believe, be disposed to deny that the excess of mortality of forearm-amputations in hospital practice, as compared with country practice, is mainly and directly due to deleterious hospital influences. For, according to Mr. Holmes's own evidence in regard to deaths after amputation in hospital practice, "the rate of mortality varies with the prevalence of pyæmia;"[1] and in the above extracts we have seen how greatly pyæmia, with its congener types of disease, was mixed up with these fatal cases of forearm-amputation.

As additional evidence of the relative safety of limb-amputations, and consequently of other operations, in isolated country dwellings, as compared with hospital wards, let me add two paradoxical-like propositions regarding the greater safety, in rural practice, of double amputations, and of amputations upon the aged.

PROPOSITION IX.—*Double amputations are very fatal in hospital practice, but are recovered from in private country practice in as great proportion as single amputations are recovered from in large and metropolitan hospitals.*

I do not know of any statistics to show the mortality attendant on double amputations on the same individual in hospital practice; but they are known to be extremely fatal, especially when one of the two amputations is of the thigh. In the last 10 double amputations performed in the continuity of the bones of the extremities in the Infirmary of Edinburgh, all the 10 perished.

Out of 23 double amputations reported to me and performed in country practice for complex injuries, making 46 limb-amputations in all, only 7 of the patients died—less than 1 in 3.[2] In our large and metropolitan hospitals the mortality already stated attendant upon *single* amputations is greater than this, as already shown in Tables XII., XV., etc.

PROPOSITION X.—*Limb-amputations, when performed on persons above seventy years of age, form very fatal operations in hospitals; but in isolated rooms in country practice they are not more lethal than are limb-amputations in hospital practice when executed upon persons of all ages.*

In his excellent paper on the statistics of the hospitals of Paris, published in 1862, Mons. Trelat remarks, with reference to the

[1] See *St. George's Hospital Reports*, vol. i. p. 328. [2] See Table II.

mortality of amputations, that "beyond seventy years of age it (this mortality) becomes so large as 95 per cent—that is, 1 only recovers out of 20 operated on." Hence, M. Trelat, remarks Dr. Ranking, "reprobates amputation in those above seventy years of age."[1] In a similar spirit Mr. Holmes observes, in regard to limb-amputations after the age of seventy, "The larger amputations would be hardly justifiable except for accidents, though even here the prospect of recovery is so slight that it becomes a question whether it would not be better to allow the patient to die unmolested by the operation."[2]

But while limb-amputations are very deadly and fatal when performed upon the old in our rich hospital wards, the very same operations are not more fatal to the old, in rural practice, than are limb-amputations performed upon people of *all* ages in our large and metropolitan hospitals. In the 2098 country amputations reported to me, 22 are incidentally stated to have been performed on persons above seventy. Of these 22 cases, 6 were amputations of the thigh, 9 were amputations of the leg, 5 of the arm, and 2 of the forearm. Of the 22 patients 8 died, or 1 in 2·7 ; whilst the mortality in our large and metropolitan hospitals, in limb-amputations, upon persons of *all* ages, is 1 in 2·4.

In his paper in the *The Lancet*, Mr. Holmes propounds a surgical syllogism :—"No hospital arrangements (says he) will make an old man into a young one." I subscribe to this position ; but at the same time I submit that in the preceding facts we have something not unlike Mr. Holmes's own suggested metamorphosis effected—as far at least as regards the power of sustaining the dreadful operation of dismembering a human being. It seems to be effected, further, by no sorcery more weird and mystical than placing aged patients, when they are to be operated upon, *out* of the conditions of hospital aggregation, which he recommends, and *into* the conditions of separation and isolation which I have ventured to recommend. And if such simple necromancy produce such very strange and unexpected results upon very old surgical patients, why should it not produce equal and corresponding salubrious changes upon surgical and other patients of all ages ?

In previous propositions we have seen that, as general laws—liable, of course, like all other general laws in medical science, to various local and other exceptions—amputations of the limbs in

[1] See Ranking's *Half-yearly Abstract of Medical Sciences*, vol. xxxvi. p. 197.

[2] See *St. George's Hospital Reports*, vol. i. p. 301.

hospitals become more and more dangerous and fatal in their results in proportion as the hospitals in which they are performed are increased in size; and, on the contrary, they become less and less perilous and lethal in proportion as the hospitals in which they are placed are decreased in size. In other words, we have found that the congregation of patients upon a given spot, or within a given establishment, enhances the chances of death to those that are there operated upon and treated; while, on the other hand, the more that patients are separated and isolated they recover the more surely from the knife of the surgeon, and in all probability, also, from other accidents and diseases. There is safety in segregation — danger in aggregation.

The comparative degree of safety and danger pertaining to patients obliged to undergo amputation of the limbs, when placed under different external conditions and amounts of aggregation, may be shown in percentages in such a table as follows.[1] In this table I have placed on the first line the mortality from limb-amputations in the larger hospitals of Paris,[2] as officially given by M. Husson, for the years 1861-63, the latest statistics hitherto published by him. The other calculations are death percentages in round[3] numbers, calculated from upwards of 7000 limb-amputations[4] in Great Britain which I have collected.

[1] See previously its analogue in proportional numbers in Table XII.

[2] I believe that St. Bartholomew's, London, is the only British civil hospital which contains upwards of 600 beds. Mr. Holmes and Dr. Bristowe speak of their number as 650. Several of the Parisian hospitals—as the Hôtel Dieu, Pitié, Lariboisière, Saint Louis, and Enfans Malades—contain from 600 to 800 beds. The Parisian hospitals, and even those that are comparatively small, seem to have generally their wards more crowded with beds and patients than the hospitals of England and Scotland.

[3] The actual or true decimal figures in this table (Table XX.) are, reckoning from below upwards, 10·8, 13·8, 17·7, 22·6, 29·6, and 41·1.

[4] I have already given in detail (See Chaps. I. and II.) the statistics of the amputations in our large and metropolitan British hospitals, and in country private practice; and betimes I will give, in the same manner, the full details of upwards of 3000 limb-amputations reported to me from the provincial hospitals of Great Britain, and which I have now summarised. The round percentages in the table give a just and faithful view of the general results. If any mistakes in the numbers or calculations can be pointed out, I shall of course be happy to rectify them; but surely, in a professional discussion such as the present, this may be done without that discourtesy of words that is resorted to in the last *Lancet* by a London hospital surgeon.

TABLE XX.—PERCENTAGES OF DEATH AFTER LIMB-AMPUTATIONS UNDER DIFFERENT CONDITIONS AND DEGREES OF AGGREGATION AND ISOLATION.

Condition and size of Hospital.	Percentage of Death-Rate.
In the large Parisian hospitals . . .	62 in 100 die.
In British hospitals with 300 to 600 beds .	41 „
In „ „ with 300 to 201 beds .	30 „
In „ „ with 200 to 101 beds .	23 „
In „ „ with 100 to 26 beds .	18 „
In „ „ with 25 beds or less .	14 „
In isolated rooms in country practice . .	11 „

Mr. Holmes, as we have already seen, properly, I believe, holds that the difference between the death-rates after amputation in the large hospitals of Paris and of Great Britain is "*enormous;*" and, to use his own words, "surely not to be explained by differences either in the constitutions of those operated upon, or in the nature of the injuries and diseases for which they are admitted." The preceding table shows the difference between the death-rate in the Parisian and in our own large British hospitals to be 20 per cent. In other words, out of every 1000 subjected to limb-amputations in the Parisian hospitals, 200 more die than die out of the same number of amputation cases in the large hospitals of London, Edinburgh, and Glasgow. But our table further shows that out of every 1000 cases of limb-amputation performed in our largest British hospitals, nearly 200 more die than there die of the same operation in smaller British hospitals containing from 100 to 200 beds, and 400 more than are lost when the operation is done upon patients living in isolated rooms in country or provincial practice.

Previously I have ventured to lay it down as a proposition (see Proposition II.) that to account for the appalling loss of life after amputation in our larger as compared with our smaller British hospitals, the relative severity of the injuries and diseases for which the amputations are performed, or the constitutions of the patients, etc., afford no adequate explanation. Further, I hold that in this inquiry this may be laid down as another and equally valid

PROPOSITION XI.—*The differences between the death-rates after limb-amputations in country practice and in hospital practice are (to re-employ Mr. Holmes's words) "evidently enormous, and surely not to be explained by differences either in the constitution of those operated on, or in the nature of their injuries or diseases."*

Few minds, I believe, studying this matter *de novo*, will feel any difficulty in assenting to the truth of this proposition, more especially when the gradation of the death-rate in amputations is seen, as shown in Tables XII. and XX., to decrease so regularly and methodically in proportion as the patients are more and more separated and isolated.

But Mr. Holmes—as yet at least—strongly objects to our present proposition, mainly, I believe, from want of adequate knowledge on his part of the peculiarities and status of country practice in many districts of Great Britain. In this respect, matters both abroad and in our own country are mightily changed since the beginning of the last century, when Dionis tells us he could not find in the whole city of Marseilles a single practitioner who had performed the excision of a diseased mamma. For there is now scarcely a village in England and Scotland in which we would fail to meet a practitioner capable of performing that and far more serious surgical operations; and some country practitioners, especially in the mining and manufacturing districts, have had as large a share of primary amputations as most hospital surgeons.

Mr. Holmes objects to the statistics of country amputations as compared with hospital amputations on a variety of grounds. Some are so irrelevant as not to require any reply. But on several objections he and others seem to place much reliance, and therefore it may be proper to answer them. It matters little how those objections are arranged. One of the most fundamental, if it held true, may be stated as follows:—

OBJECTION I. *The data are so far too few, as they give only a "small sample" of country amputations.*—The collection of limb-amputations which I have made from British hospital and private practice is, I believe, the largest ever yet brought together for any statistical inquiry in Europe—amounting as it does now to upwards of 7000 cases, above 2000 of which were amputations performed by 374 country and provincial practitioners in single or isolated rooms. The collection is more extensive than any of the series of amputations, resections, lithotomies, and herniotomies, made under Govern-

ment and official influence by Mr. Holmes and Dr. Bristowe. But Mr. Holmes objects to the 2098 rural cases reported by me as being only a "*small sample*" of the mass of amputations that must be performed in the country. This number of amputations, however, is at least somewhat larger than he and Dr. Bristowe were content with collecting from all the hospitals of England, Scotland, and Ireland that they visited, and which smaller data—viz. 1837—they then deemed sufficient for their various deductions and reasonings.

Another London surgeon, who has, like Mr. Holmes, worked ably and written much upon the data of amputations in metropolitan and provincial hospitals, took—when I had only 1000 cases collated—an opposite view to him; and I willingly leave them to decide who of the two is correct, and whether or not both are wrong. This second metropolitan surgeon, after stating that, with all his extensive statistical experience, he "should not have known in the least *where* to turn for the facts;" for, as he argues, "amputations in the habitations and cottages of the poor are not matters of every-day occurrence. . . From what source," he further asks, "*can* the record of 1000 amputations in private practice or country practice be obtained?—for we are not sure that Sir James Simpson excludes small provincial hospitals—with a guarantee, or anything like a guarantee, that *all* have been given."[1]

I have excluded, however, though it is here doubted, from the 2000 amputations in private practice all hospital cases; and, in asking country practitioners for their returns of cases, I specially requested, as stated in the letter of application addressed to each, to report in his schedule "*all* the amputations which you may have had in your *own* practice;" and "whether the cases ended in recovery or in death."[2]

To this just and simple request Mr. Holmes brings apparently forward as another

Objection II. "*The length of time comprised in these returns;*" *in some* "*it extends back more than twenty years.*"—Surely so strange an objection requires no reply. Would Mr. Holmes have had me ask each practitioner not for *all* his amputation cases, but only those that had occurred within some specialised term of years? Surely the request would have been perplexing and indefensible in such a statistical inquiry. But let Mr. Holmes's metropolitan colleague answer him on this point; and in this colleague's opinion every

[1] See *Medical Times and Gazette* for Jan. 23, 1839, p. 98.

[2] See letter in the *Edinburgh Medical Journal* for March 1869.

statistician will, I believe, coincide. Referring to the modes of collecting country amputation returns, he observes :—"To pick up stray cases from the journals for such a purpose would be worse than childish; nor would the plan of taking a *few years*' report from one surgeon, and a few from another, be much better."

OBJECTION III. "*Sir James Simpson's comparison between the statistics of private practice in the country and those of hospital practice in town is unfair*," *because the things thus compared are dissimilar, and the comparison is fallacious.*—In a letter published some months ago, Mr. Holmes stated this objection more explicitly thus :—"Before any fair comparison can be made between the results of amputations in different hospitals—how much more between hospitals and private practice—it is necessary to know something more than how many amputations have been performed on account of accident, and how many on account of disease, or the relative numbers which have been performed at the several seats of amputation. It is necessary to be acquainted with the condition of patients at the time of operation, and with the actual cause of death in those who die after operation." [1]

In this last clause Mr. Holmes proposes to obtain acquaintance with the *causes* of death after amputation, without even inquiring into the *rates* of death—two investigations which I have shown, under Proposition VII., to be quite different. In my tables the rates of death are all strictly attended to.

The condition of persons at the time of amputation will engage us in the sequel, when we shall find that in more than half of the whole—namely, in the primary amputations—both the country and hospital patients were in a state of ordinary or normal health immediately before the amputation.

But let me here add a few remarks on the arrangement of the amputations so as to make the comparison between country and hospital operations as easy and certain as possible on all points; for I maintain, contrary to Mr. Holmes's strange assertion, that I have *not* been "scrupulous to compare things which are dissimilar."

In the 7000 and odd major amputations which I have collated, I have studied, as far as possible, to arrange them and their results so that the comparison between them should on all points be as easy and free from fallacy as possible. For this purpose, after giving both in the lump and in detail the amputations of each country practitioner and each hospital, I have divided the whole amputations

[1] *British Medical Journal*, Jan. 23, 1869.

into six classes, according as they were performed in single isolated rooms or in hospitals of various sizes. With the object of attaining simple and correct comparison, I have subdivided further each of these six classes of cases into (1) limb-amputations taken as a whole, (2) limb-amputations the result of injury, and (3) limb-amputations the result of disease. By the method adopted we can also compare the death-rate of the various individual amputations—as the thigh or the forearm, for instance—in country practice, with the death-rate of amputation of the thigh or the forearm under five or six separate conditions of hospitalism ; and ascertain readily, and at once, many different points of relation and comparison.

In this way it appears to me all preventable sources of fallacy have been avoided to a degree that has not hitherto been generally attempted in amputation statistics. For example, look, as a matter of contrast, at the official list of amputations offered to the Privy Council by Mr. Holmes and Dr. Bristowe. In their table, pages 555-558, they have congregated and lumped together their 1800 and odd amputations without succeeding in dividing them into the two great and significant classes of traumatic and pathological amputations, or amputations for injury and for disease—confessedly a very grievous omission. But other still more unfortunate errors crop out in Mr. Holmes's table. He has, for instance, confused the four major amputations of the limbs through the continuity of the bones with amputations through the joints ; thus commixing amputations of the shoulder-joint with amputations of the arm, amputations through the hip-joint with amputations of the thigh, and amputations of the leg with amputations of the ankle and foot. To show the kind of mistakes to which such confusion may lead in surgical statistics, let me point to one instance only in his table. Mr. Holmes enters the death-rate in the metropolitan hospitals from amputation of the leg, under which he includes amputation of the foot, as 30 in 100 ; while the actual rate of mortality of amputation of the leg in metropolitan hospitals is, as I have elsewhere shown, as high as 44 in 100. Mr. Holmes's method thus involves an error of not less than 14 per cent in this one amputation alone, and so far seriously detracts from the value of this and other portions of his amputation statistics.

Objection IV.—*The comparison between hospital and country amputations is "unfair," for the success of the country amputations is represented as too high.*—Mr. Holmes evidently lays his chief weight on this objection ; let us therefore consider it more at length.

Now the list of country limb-amputations which I have collated and published amounts to 2098; and of these 226 died, or 1 in 9. "I think," writes Mr. Holmes, "no unprejudiced person would hesitate to admit, what for my own part I firmly believe, that the mortality from all causes in the country is higher than this list shows."

On the contrary, I submit that any such belief on Mr. Holmes's part is simply and purely the effect of prejudice; and, for one, I do not wonder at it in a London surgeon, seeing that in the metropolitan hospitals the mortality which he witnesses is, to use Mr. Holmes's words, so "evidently enormous." I feel assured that any unprejudiced mind must come to an opposite conclusion to Mr. Holmes; for we find from his own amputation statistics and table[1]—and altogether contrary to his own prejudices too—that this mortality is so far regulated by the size, etc., of the hospitals, as to vary as follows:—

In metropolitan hospitals he makes it .	1 in 3·3[2]
In provincial „ „ .	1 in 4
In rural „ „ .	1 in 5·4
And in the smallest of these hospitals .	1 in 6,

where the beds were 100 or less, as I have previously shown from Mr. Holmes's own statistics in the discussion of Proposition II. But if the mortality decreases thus with the segregation of the patients, would any "unprejudiced" mind not expect it to be still more decreased when the place was a cottage hospital with 25 beds; or where the isolation of the patient was more complete, as when the operation was performed in single rooms in country and provincial practice?

Swayed by his prejudices against the far greater success of limb-amputations in single rooms in the country than in the wards of a large London hospital, Mr. Holmes declares it as his "conviction, from reading Sir J. Simpson's figures, that his list is composed of returns from surgeons who, having been gratified by their success in the emergencies of practice, have treasured up records of that suc-

[1] See the table in the *Official Report*, p. 558.

[2] I have shown, from more ample and accurate data than those that Mr. Holmes used, that the death-rate from the four major amputations in the London hospitals, instead of being 1 in 3·3, is as high as 1 in 2·5 on an average, and that this average varies only by two or three decimal points in the four largest London hospitals.

cess, and been glad to communicate it. Nothing," Mr. Holmes adds, "can be more natural, nothing more legitimate than this." But let me remind Mr. Holmes of one little fact which is quite subversive of this ethical theory of his. The names of the contributing "surgeons" were not published with these returns; and they have not been spoken of by name, unless when their returns contained superadded "remarks" which seemed to me to be deserving of citation for some purpose or another.[1] There was in this way none of the self-gratification to be thus obtained which Mr. Holmes thinks so natural and legitimate an object. But even if it were otherwise, would that have interfered with the accuracy of the country returns themselves? Have not some hospital returns been published in London and elsewhere for this same ethical reason of Mr. Holmes? But are they the less valuable on that account?

But further, in relation to this fourth objection—namely, "*that the comparison between hospital and country amputations is 'unfair,' for the success of the country amputations is represented as too high*"—Mr. Holmes selects four of my country returns, being Nos. 92, 171, 191, and 288 in the published table, as to him specially questionable. Apparently with some view of detracting from the statistical value of these returns, Mr. Holmes observes that they "show, when taken together, no fewer than 125 amputations of all the limbs for injury —the thigh having quite its fair proportion—occurring in the practice of four surgeons, and comprising their entire experience, *without a single death.*" Here there is a grave and serious error on Mr. Holmes's part, made, I have not the slightest doubt, quite inadvertently and not willingly, yet forming a very great and reprehensible misstatement in such a discussion as the present. These four gentlemen have, according to their own certified and written testimony, amputated for injury in 125 cases; but 2 of the patients died, as explicitly shown in the table from which Mr. Holmes takes his data. The same four practitioners have performed amputation, as pointed out in the same table, upon 137 cases in all, 5 of which died, or 1 in 27. If fortunate in their operations for injuries, they were not so in their operations for disease; as they lost 3 out of their 12 pathological amputations, or 1 in 4.

[1] Mr. Holmes might have taken out of the list other returns amounting to nearly the same number, as Nos. 13, 149, 194, 227, and in which four returns the number of amputations reported is 128. Out of these 128 amputations 25 died, or 1 in 5. Should these four returns therefore be discarded as too low? The eight returns, 92, 171, 191, 288, 13, 149, 194, and 227, when added together, give 30 deaths out of 265 amputations, or 1 death in every 8 or 9 operated upon--the

Does Mr. Holmes really mean to argue that I should not have included those 125 primary cases, because, in his opinion, they were too successful? And, on the same ground, should I have rejected the high mortality met with by the same four surgeons, in their amputations for disease, because it was greatly above the usual mortality? The chief value of any statistical inquiry like the present consists in taking in all, extreme as well as all intermediary, numbers, and striking an average or mean out of the whole. What would be thought, for example, of a statistician who, in striking the mean mortality of any population for a given time, objected to admitting among his data all who died after seventy years of age or before five years? Mr. Holmes doubts whether any such success is "possible" to be attained in hospital practice. Now in St. Bartholomew's Hospital in London in 1855, 17 males suffered amputation, and all recovered. During the same year, in 25 amputations, 12 of which were thigh cases, there was only one death. In 1861 there were 24 consecutive amputations with only 1 fatal case. In this way, in that metropolitan hospital, during these periods, out of 49 limb-amputations only 2 died, or about 1 in 25. But the reporter, Mr. Callender, shows "how necessary it is to mass together a considerable number of consecutive operations before we have a chance of arriving at tolerably just conclusions."[1] For in 1863 to the end of September there were 13 deaths to 28 recoveries after amputation in St. Bartholomew's: in 1858 and 1863 there were 5 and 8 consecutively fatal cases. At the present time, and for some years past, the death-rate after amputations at St. Bartholomew's is as high as 1 in 2·7, instead of 1 in 25. Great runs of success in this way are not unfrequent in other hospitals, both sur-

usual mortality in country amputations. Mr. Holmes thinks that the occurrence of such success as the four country practitioners in the text met with, would, "in the practice of four hospital surgeons"—as, for example, the four surgeons of St. George's Hospital—"be nothing less than '*miraculous*.'" It would of course be so unless the insalubrious conditions of St. George's Hospital were totally changed; and it would be so in another sense truer than Mr. Holmes imagines. The four returns are selected by him, I believe, as being the four best in the total series of 374. There are perhaps nearly an equal number of hospital surgeons in England; and let us suppose them to be 374 also. Now Mr. Holmes, or any of the other four surgeons of St. George's, might certainly have a fair prospect of being one of the four successful surgeons out of the 374, if the hospital conditions were duly changed for the safety of the patients; but the idea of all "the four surgeons" of St. George's standing at the head of the 374 is a result which, in the calculation of chances, is as millions to one—something more truly "miraculous" than Mr. Holmes, perhaps, dreamt of when he penned the sentence.

[1] See *Medico-Chirurgical Transactions*, vol. xlviii. p. 95.

gical and obstetrical, as well as in country practice, where, from the death-rate being so small and pyæmia so much rarer, it is much more likely to occur. But the Nemesis of figures always at last certainly and ruthlessly reduces the statistics to their proper mean when the numbers become sufficiently large. For here, as elsewhere in such statistical inquiries, while there is always a great uncertainty and instability in regard to the results of a small and limited number of cases, a larger and larger aggregation of cases comes to afford conclusions which are comparatively certain and stable.[1] In a return which was sent to me last week, by Dr. Strange, of the mortality of limb-amputations in the Worcester Hospital, I find that since 1862, 22 cases of primary amputation of the leg, and 8 cases of primary amputation of the arm, or 30 in all, were performed in the hospital in question without a single death. That most excellent surgeon, Mr. Carden, when first describing his new form of flap-amputation, reported 17 hospital cases in succession of thigh-amputation for disease without a single death ; but he had 31 other amputations in all, and out of the remaining 14 cases 5 died, or above 1 in 3. In the first 30 amputations for disease in the Glasgow Infirmary, Dr. Lawrie tells us, only 1 died. Now, in that same hospital, they lose 1 in every 3 in their amputations for disease. At the Leeds meeting of the British Medical Association, Dr. George Macleod of Glasgow, a most able and skilful surgeon, reported the result of 50 amputations of the ankle-joint which he had performed. Only 1 out of the 50 died. But in the statistics of the Glasgow Infirmary, which are kept with most commendable accuracy, I find that out of the last 50 cases operated on in the hospital, 10 of the patients sank, or 1 in 5. The York hospital is specially adverted to by Mr. Holmes, in his excellent Official Report, as at one time, from want of

[1] Elsewhere I have tried to show, at some length, that the great and leading principle upon which statistical inquiry is grounded consists in the fact that in unities or entities of a doubtful chance, while the result or event in individual instances is ever variable and uncertain, the result and event when calculated from or upon masses of instances becomes comparatively certain and invariable. (See *Edinburgh Monthly Journal of Medical Science*, Nov. 1847.) Perhaps one of the most remarkable instances of a statistical death-rate becoming rectified by increased numbers is that of Mr. Martineau of Norwich. In the eleventh volume of the *Transactions of the Medico-Chirurgical Society of London*, Mr. Martineau published an account of 74 cases in which he had performed the operation of lithotomy in the Norwich Hospital, from the year 1804 to 1840. Only 2 of these 74 died ; or 1 in 37. We learn further, however, from a paper by Dr. Yellowly, that Mr. Martineau operated in the same hospital on 73 additional cases, or 147 in all. Of these 73 additional cases, 15 died ; or more than 1 in 5.

ventilation, suffering under "an extreme degree (as regards surgical patients) of hospital unhealthiness" (p. 549). It is now greatly improved; and during the years 1867 and '68, out of 24 limb-amputations performed within it, only 1 patient died. Perhaps it may not be out of place to add here, that since publishing my table of country amputations in March 1869,[1] I have been told of various country practitioners who have performed, with remarkable success, larger numbers of limb-amputations than any that were at that time reported to me.

Secondly, Mr. Holmes, in addition, states and argues that my table of country amputations contains only two leading sets of operations—viz., in the returns (1) of those who had great experience and much practice in amputations, and (2) of those who had very few operations to return. As shown in Proposition IV., this is scarcely the fact, as out of the 2098 cases reported, 821 of the cases were performed by surgeons who had amputated twelve times or oftener; 648 of them by practitioners who had operated from six to twelve times, and 629 by country medical gentlemen who had operated five times or less. Surely this is an equal enough division, even to satisfy Mr. Holmes.

But, thirdly, he professes to have another objection to my table of country amputations, by which he thinks the success in it is also made too high: I have given, he avows, as one kind of returns, "very small numbers of amputations for disease, in many instances single cases, which have almost *uniformly* proved successful." This, I humbly submit to Mr. Holmes himself, is very wrong. He will find in the table of returns eight or nine reports, at least, of one or two cases only in which the amputations, instead of being represented, as he avows, as "*uniformly* successful," are returned as *uniformly* fatal. The table contains, on the whole, 71 returns of cases in which the practitioner had only performed amputation once. Of these cases, 23 were forearm-amputations, and therefore accompanied in the country with little or no danger. Of the remaining 48, as many as 7 of the cases, or 1 in 7, are reported as fatal; of the whole, about 1 in 10 died. Should not Mr. Holmes at once retract this with his other objections, as, to use his own word, "unfair"?[2]

[1] See Chapter I.

[2] An excellent young practitioner — Mr. Cribbes of Gorebridge — whose remarks Mr. Holmes quotes as those of "very probably one of the (four) surgeons" spoken of in the text, belonged, not to that class, but to the present class of practitioners, who had returned one amputation. Mr. Holmes erroneously makes

OBJECTION V. "*I assert,*" *says Mr. Holmes,* "*that such a comparison as Sir James Simpson has made between private and hospital practice must always be unfair, because the cases are selected on different principles in private practice*" *from what they are in hospital practice.*— "The fact (he adds) *must* be as I say," for, to quote his own words, "hospital surgeons refuse nothing in the way of operation which comes to their hands."[1] On the contrary, I feel and hope that, in the main, this argument is merely an illusion on Mr. Holmes's part. It would, indeed, be painful and revolting to most professional minds even to suppose that the poor, who form the patients in our rich hospitals, were cut and operated upon by hospital surgeons on principles different from those on which patients were treated out of hospital. Dr. Brotherston of Alloa, who has performed above sixty amputations of the limbs, has already rather indignantly criticised Mr. Holmes's remarks on this point.[2] There is, he observes, "certainly not one rule for hospital patients and another for private patients. The very idea is wrong. The operations in the country are as serious and difficult as in the town, and the number of primaries as large or larger. Arms and legs are saved in the country that would have been amputated in town [hospitals]. The joints, etc., heal in the country, aided by uncontaminated air and proper treatment."

For his strange belief in the present objection Mr. Holmes offers two reasons. First, he observes, "the private practitioner would very rarely be permitted to perform such a grave operation as an amputation, unless he could hold out much more confident hopes of success than any candid man could do in many of the amputations which we perform" (but which he certainly can, as his amputations are three times more successful than those of the city hospital surgeons); and, secondly, "there is always the habit which the poor have in doubtful and grave cases of seeking the advice of the hospital surgeons."

Mr. Holmes appears to fancy that in the country everything goes on exactly as in London. But in many country districts in

him testify against the presence of "tetanus" in mining populations. But most certainly Mr. Cribbes does not commit himself in any such way. Out of the deaths in country practice which I have collected, 10 or 11 were from tetanus. (See Chapter II. p. 311.)

[1] In Chapter II. p. 320, I have quoted several cases, and have heard of others, where patients sent out of hospital underwent amputation and recovered. Dr. Philip Maclagan of Berwick told me of a case lately occurring to himself.

[2] See *Medical Journal* for March 6, 1869.

Great Britain where amputations are performed, and performed frequently, especially for injuries, Mr. Holmes forgets that there is often no hospital surgeon sufficiently near to consult in a case of shattered limb, and they would have to wait six, twelve, twenty-four hours or more, to get an hospital surgeon; and in some country districts—in Scotland at least, and I could name some in England too—there is no desire to consult them even when within reach, as the patients frequently rather dread hospitals, and have the most full and implicit confidence in their own local practitioners. The number of cases of limb-amputations which I have collected from country practitioners are surely sufficient to gainsay Mr. Holmes's strange assertion, "that they are very rarely permitted to perform such a grave operation." The country surgeon no doubt sometimes meets with opposition at first—at least at the idea of amputation; but perhaps, after all, not much oftener, if oftener, than the hospital surgeon.[1]

Mr. Holmes holds that there are some causes which of late have increased the mortality of amputations in large hospitals. For we may state it as, on his part, another

OBJECTION VI. *In hospitals, conservative surgery increases our hospital "mortality after amputations by withdrawing all the better or more hopeful classes of cases entirely from the amputator."*—Mr. Holmes here refers particularly to the excision of diseased joints, instead of amputation of the limb. "Consequently," he argues, "as surgery improves, the rate of mortality after amputation tends to increase. If it be true," he continues, "that in the provinces practice inclines more to the old than the modern methods, this would tend to increase the success after amputation in the country. This," he adds, "is a matter which cannot be dealt with by *figures*."

But I doubt this last allegation of Mr. Holmes's; for statistics seem to me to be able to prove by their figures that any such

[1] When lately visiting a patient at Coniston with Mr. Bywater, that gentleman told me, in the course of conversation, that he had performed many limb-amputations; and he had lost only one patient amongst them, a case of gangrene of the arm. On putting to him Mr. Holmes's hypothetical objection about the supposed difficulty of getting country patients to submit to amputation, he told me, in reference to one of his earliest amputations, that he was at first asked to do the operation without seeing the case, a message being merely left for him to bring forthwith his "tools" with him to a patient, as she wanted her leg to be lopped off. Here the patient and her friends wished to decide the question without asking at all the aid of the surgeon as to the mere propriety of the operation.

changes exert less influence than he imagines in increasing the hospital death-rate after amputations.

Thus, in the Glasgow Infirmary, from its opening in the last century in 1794 to the end of 1838, the death-rate in limb-amputations was, according to the researches of Professor Lawrie, 1 in 2·7;[1] but from 1860 to 1868 the death-rate was very nearly the same, or as 1 in 2·5.[2]

In the Edinburgh Infirmary, during three years and a half, Dr. Fenwick, writing in 1848, calculated the mortality of the limb-amputations to be 1 in 1·96. During the last eight or ten years it has been rather diminished than increased, being 1 in 2·3.[3]

I am not aware of the existence of any London hospital amputator's statistics a quarter or half a century old by which we could test the idea that in hospitals, after amputation, the mortality is increasing; but the preceding data scarcely favour that opinion.

OBJECTION VII. "*A comparison*," *says Mr. Holmes*, "*between perfect* [*hospital*] *and imperfect* [*private country*] *returns must be delusive.*"—Is in reality the one form of the return thus more highly reliable than the other? Do the hospital returns contain more "perfectly authentic data," as I have heard earnestly insisted on by various parties?

It would very ill become me, as one who has received so much kindness and aid in this inquiry from professional brethren connected with the hospitals of Great Britain, to comment upon this matter. But I may be permitted, perhaps, to state, that from a considerable number of hospitals I have received no reply; from

[1] See *Medical Gazette* for December 1840, p. 397.

[2] See Table V.

[3] According to the official returns of the last two years—embracing the time since antiseptic dressings, etc., were profusely introduced in most of the wards—the amputation mortality of the Edinburgh Infirmary has increased to above 1 in 2. From 1859 to 1866 it was 1 in 2·5. In 1867 and 1868 it was 1 in 1·8, or had risen from 40 per cent of those operated upon to 53·6 per cent. In his work on Carbolic Acid (1863 and 1865, 2d edit., p. 364), Dr. Lemaire of Paris, Dr. Declat, and others, taught that pyæmia in surgical wards could be prevented by the use and emanations of carbolic acid. (See *The Lancet* for September 2, 1867, p. 547.) During the several years from 1861 downwards, while M. Maisonneuve used carbolic-acid dressings in his wards in the Hôtel Dieu (which he has now, I believe, abandoned for alcoholic and other applications), it was not, I am aware, observed that there was any diminution of pyæmia among his patients. But the attempt to diminish it and other diseases which has been made from time to time by sulphur-fumes, chlorine, euchlorine, carbolic acid, ozone, cressol, etc., and other disinfectants and antiseptics, is certainly a line of inquiry well worth further investigation.

professional officers connected with others I have been told that it was useless to apply, as no authentic records of the kind required were in existence; and from various quarters I have been informed of the doubts, the difficulties, and the impossibilities, of filling up the schedules. But I believe the British returns which I have now collected and summarised contain a fair and reliable account of the limb-amputations belonging to each hospital in the list. I willingly leave any criticisms on this point to the distinguished officer of the Privy Council, Mr. Simon, who, five years ago, in his preface to the published Report of Mr. Holmes and Dr. Bristowe, speaks of the "present grievous imperfectness of hospital statistics" as impeding in England the comparison of the success and healthiness of English hospitals with each other; and he observes, as the result of the experience of Mr. Holmes and Dr. Bristowe, that, "in the *vast majority* of hospitals, no scientific records were kept; and, in the *rare* instances where reasonably good records were seen, these, as kept at different hospitals, were commonly so unlike to each other that no exact or minute comparison between them could be made" (pp. 31 and 38). This was written in 1864. During the last few years matters in this respect have, I believe, become greatly changed for the better.

On the contrary, are not the country amputation returns more authentic and reliable? I believe that they are so for more than one reason. In the country returns the schedules were usually filled in, and their accuracy always authenticated and attested by the signature of the operating surgeon. In the hospital returns the schedules were most usually signed by the house-surgeon, or by one of the professional staff of the hospital. In the country returns, the signatures have a *personal* responsibility for accuracy; while, on the contrary, in the hospital returns, the signatures have only a *corporate* responsibility attached to them—a point of some difference in relation to perfect legal authenticity.[1] The accuracy of the country data, as reported to me by the 374 country surgeons, is proved, I repeat, by the fact that they are all duly and personally attested by the returning operators. If a man had a few cases only of amputa-

[1] This difference seems to have been felt strongly by some that returned the hospital schedules. Thus, in one of the last sent in to me, from a large hospital in England, the house-surgeon does not adhibit his signature, but conscientiously remarks that, being lately installed, he had merely extracted the cases from the operation book. "For its accuracy," he adds, "I would rather not own any responsibility; perhaps some cases, successful or not, have been omitted by chance."

tion in the course of his practice, he was of course sure to remember them as so many white stones placed along the pathway of his professional life ; those that had more had sufficient entries and notes to guide them. But I failed in obtaining specialised returns from several practitioners, who had performed a large number of amputations, because they had kept no records of the peculiarities and results of their operations. Further, that the country returns present a fair mean in showing 1 death in 9 is, to my own mind, proved by the fact that when the schedules were collected, and summed up from time to time by my assistants, Dr. Aitken and Dr. Munro, they kept on the whole near this figure, though sometimes the general mean ran higher than 1 in 10, and again lower than 1 in 8. When I collated and summarised above 2000 cases, I ceased from extending the collection ; believing I had brought together enough for a satisfactory and reliable mean, and believing also that in no question connected with amputation statistics had so large a body of data ever before been deemed necessary.

OBJECTION VIII. *The private cases of amputation in hospital cities and towns should have been collected.*—"If," observes Mr. Holmes, "Sir J. Simpson had not been so scrupulous to compare things which are dissimilar, if he had not, as he says he did, in selecting the practitioners to whom the applications and schedules were sent, avoided as far as possible including in the list any members of the profession residing in our large hospital cities and towns [*as my object was to obtain the returns principally from country and provincial professional men*], his eyes would have been opened to the fact that, in order to test the effects of hospital arrangements upon cases, the cases themselves must be otherwise under similar circumstances."

In this quotation I have restored, in brackets and italics, an important passage which Mr. Holmes has omitted, because it shows that my object was to compare the results of amputation upon patients in our large hospitals with, as far as possible, socially the same class of patients in the country. As far as I can understand the object of Mr. Holmes's remarks, I should also have applied for and collected private amputation returns from towns and cities in which hospitals existed. I asked for them many months ago, in the *British Medical Journal* for January 1869, but have received none. If they were brought together, they would scarcely afford, I fear, a fair and just comparison with hospital returns ; as the latter would include the results of operations upon the poor, the other upon the rich ; for in towns and cities with hospitals the poor become, almost

all, for operative procedures, the inmates of the hospitals. Surely Mr. Holmes himself could scarcely expect me to find the hospital town amputation returns which he desiderates. For he observes: "Almost all grave surgical cases among the poor in London are removed to hospitals." And if this happen in London, let me ask Mr. Holmes how or why he expects it to be different in other hospital cities and towns? One of the greatest and most celebrated surgeons of the present day, Professor Billroth, of Vienna, lately wrote me that he had never yet had an amputation in private practice. How many has Mr. Holmes himself had, and what was their result?

To the statement that the death-rate after limb-amputations is three times more excessive in the wards of our large and metropolitan hospitals than in single rooms in private country practice, one or two additional points of opposition have been raised—chiefly, however, by others, more than by Mr. Holmes. It has been especially suggested and averred that the class of cases operated upon by the hospital surgeon must be more perilous than the class of cases operated upon by the country surgeon; that the state of the patients at the time of operating must be more desperate and unfavourable in infirmary urban than in private rural practice; that the nature of the causes leading to the amputations must be more serious in the former than in the latter class of patients; and that altogether the operations must be radically of a more dangerous and fatal kind in large hospital practice than in private country practice. For none of these arguments against the comparison of hospital and country amputations is there, I believe, any just foundation. Of the two classes of limb-amputations—those (1.) for diseases and (2.) for injuries—there is not a particle of evidence, as far as I am aware, to show that in severity and danger the pathological amputations in the country differ in any respect, in causation or otherwise, from the pathological amputations in the hospitals. On the large scale, any 100 of the one are on a par with any 100 of the other. But the question is different with amputations for injury; for in this traumatic class all the chief points are much more easy of comparison, and all the relative questions of hospital and country practice much more readily studied and solved. For instance, we may, in reference to them, lay down the following observation as—

PROPOSITION XII.—*The country amputations, though far more successful, ought to be more perilous and fatal than the hospital amputations, inasmuch as they contain a marked excess of amputations for injury ; which traumatic amputations are more dangerous than amputations for disease.*

It is a well-known circumstance in surgical statistics that amputations for injury are, on the large scale, much more fatal than amputations for disease. The writings of Phillips, Lawrie, Malgaigne, Steele, Hayward, Norris, Gross, and various other writers, all contain abundant proofs of this fact. In the 2089 cases which I have published in Chapter II., collected from large and metropolitan British hospitals, 1022 were amputations for injury, and 1067 for disease. Of the former 48 per cent died ; of the latter only 34 per cent ; or 14 in the 100 more died after traumatic than after pathological amputations. In the Glasgow Hospital returns, published by Dr. Lawrie, the proportion of deaths after amputations for injury was more than twice as great as it was after dismemberment for disease.

It was at first repeatedly suggested to me that country amputations would be found to be chiefly operations for disease, and that this would go far to account for their greater success over hospital amputations. "We all know," writes a London hospital surgeon in the *Medical Gazette* of Jan. 23, "how *very* different are the results of amputations for injuries and of amputations for disease. Is Sir Jas. Simpson prepared to show that these two classes of cases were in equal proportions in the two classes of cases which he compares?"—namely, those belonging to large hospital practice and those pertaining to private country practice. I am prepared to show that the advantages in this respect are all in favour of the hospital returns, and the very reverse of what this writer evidently supposes.

Of the 2089 hospital amputations, 1022, as just stated, were operations for injury. Of the 2098 country amputations, 1382 were operations for injury, and 716 for disease ; or, to state it in a more tabular form—

In 2098 country amputations	.	1382	were primary
In 2089 hospital „	. .	1022	„ „
Giving thus a *surplus* of	. .	360	primary amputations to the country returns.

Such an excess of primary or traumatic amputations in the country returns ought to have weighed heavily against the chances of success of the country operations, and no doubt did so to some extent; and it ought to have weighed correspondingly in favour of the hospital returns. Yet, notwithstanding these advantages, the success on the whole on the part of country as compared with hospital practices, was, as we have seen, more than three to one.[1]

Repeatedly I have found it urged as an objection to the comparison of country with hospital amputations, that possibly the country patients were not so exhausted and weakened at the time of operating as were the hospital patients. But the large class of primary cases at once offers, as an argument against this reasoning, the following proposition, viz.—

PROPOSITION XIII.—*The state of the patients at the time of operating in these (traumutic) amputations is precisely the same in country practice and in hospital practice.*

For the amputations in this class for injuries being necessitated by accident, the patients immediately beforehand must be held as all similar in their state of strength and vital force. They were all alike in the condition of ordinary or normal health a few hours, or a very short time at least, before the operation was resorted to.

[1] In *Edinburgh Medical Journal*, December 1869, Sir J. Y. Simpson wrote:—"In other points there is an advantage of the contrary kind against the hospital and in favour of the country amputations. For example—

"*The hospital amputations contain a greater list of amputations of the lower extremities than the country returns.*—Of the 2098 country amputations, 1287 were amputations of the lower extremity, and 811 were amputations of the upper extremity. Of the 2088 hospital amputations, 1548 were amputations of the lower extremity, and 541 were amputations of the upper extremity. But the operation is much more fatal in the thigh and leg than in the arm and forearm. In the returns of the eleven large and metropolitan hospitals included in Table XI. the amputations of the lower extremity proved fatal in 45 per cent, the amputations of the upper extremity in 28 per cent. As already pointed out in Chapter II. 1, the greater number of amputations of the forearm and arm in country practice is owing to the greater frequency of the accidents to which men are exposed in country life from gunshot wounds, and from the injuries produced by the unguarded thrashing-machine of the agriculturist. In the country returns there are 811 amputations of the upper extremity reported; in the hospital returns 541 amputations of the upper extremity are given. There is a slightly larger number of the most dangerous amputation of all—viz. of primary amputation of the thigh—given in the 2098 country than in the 2089 hospital amputations; but the latter contains, for reasons given elsewhere, a greater number of thigh amputations for disease;—though this operation, for this cause is, when performed, nearly three times more successful in country than in hospital practice." [ED.]

Frequently, however, the hospital surgeon amputates in these cases in a less dangerous set of limb-injuries; inasmuch as more cases of them are saved from the amputating knife in country than in hospital practice. For, according to another legitimate

PROPOSITION XIV.—*Primary amputations are chiefly for the worst forms of compound fractures of the limbs; and the hospital surgeon of necessity sometimes operates in a less severe form of these injuries than the country surgeon, and hence ought to be more successful, but is not.*

The surgeon to our large hospitals is not with this, any more than with other complications, so successful as the country surgeon. He loses 48 per cent of his amputations for injuries; while the country surgeon loses only 11 per cent, or four times less. Yet the country surgeon operates, on the whole, on a more formidable and dangerous, and hence less favourable, class of cases; for he can save from amputation, and conserve and cure without the dismemberment of the patient, various less severe types of compound fracture, which, when removed to hospital, require to be cut off. On this point I have elsewhere adduced the strongest evidence from various authorities, to the effect that severe compound fractures often do perfectly well in the healthy isolated country cottage which would require to be inevitably amputated in the wards of a large hospital.[1] "In compound fractures," observes, for example, Dr. Redwood of Rhymney, "where there is tissue left that will continue the circulation below the seat of injury, we save the limb. Assistants fresh from hospitals are often astonished at what is attempted and effected in this way." Mr. Davidson of Seaton-Delaval, who has had immense experience in these cases, has related to me the same fact in nearly the same words, and told me how often his assistants (seven in number) are astonished at the results in these cases, as compared with what they had seen in hospital practice. One of the most thoughtful and accomplished hospital surgeons of England, Dr. Heath of Newcastle, who sees much hospital as well as much country practice in these cases, lately stated to me in the strongest terms the same observation as the result of his very extensive experience.

There is another kind of evidence in proof of the greater severity of the cases of injury amputated by the country surgeon—namely,

[1] See the evidence on this point of various observers in Chapter II.

the greater number of deaths by mere "shock" in the practice of the country surgeon. For we may lay this down as another

PROPOSITION XV.—*In consequence of the relative severity of the cases of injury treated by amputation in the country, more die of simple shock in country practice than in large hospital practice.*

In the returns of deaths from the country amputations, as many as 36 per cent are returned as having died of shock; or 63 cases in all. Of these 63 cases, in 56 the deaths from shock were after amputations for the results of injury, and in 7 after amputations for the results of disease.

Out of 41 deaths in 149 amputations at St. George's Hospital recorded by Mr. Holmes only two are reported as having died of "shock." A larger proportion will probably be found to die from this cause at other metropolitan and large hospitals. But I may recur to this subject under Proposition XVII.

It has been occasionally averred that our large hospitals are likely to receive the worst forms of injury requiring amputation; and it may be so in some districts and localities, but certainly not in all, for the preceding remarks so far prove the contrary. Some time ago I received from Dr. Bellenden of Dudley a return of limb-amputations in private practice, too late to be included in the general table in Chapter I. He reports to me 22 amputations without a death, 13 of them being amputations of the thigh; and he adds a remark, showing that in some places cases of minor injuries are forwarded to hospital, and the graver accidents retained at home, the men being too damaged to be forwarded to a distance. "Most of the removable accidents," writes Dr. Bellenden, "in our collieries and works are taken to the South Staffordshire Hospital, the worst cases are left behind." "I do not hesitate," he adds, "in asserting, that cases sent to the hospital are much more lingering than similar cases treated in their own homes."

Formerly it was sometimes supposed and argued that hospital amputations were so much more dangerous and fatal than country amputations, in consequence of the hospital cases being more severe, and the patients dying from the mere effects of shock. These observations rebut entirely this kind of vague reasoning. Nay, more: we have seen under Proposition VII. that the greatest amount of proportionate difference between the death-rate of limb-amputations in hospital and in country practice is in amputations of

the forearm, forearm-amputations being twenty or thirty times more fatal in large hospitals than in private country practice; and yet no one could argue that this vast difference was ascribable either to shock before or shock after this, the least of the four major amputations of the limbs.

PROPOSITION XVI.—*Amputations of the thigh for disease are more common in infirmary than in country practice, but are three times more dangerous in hospital than in rural practice.*

Tubercular disease of the joints and bones is by far the most common cause of pathological amputations of the thigh, and, to a less degree, of the leg also. "In common practice," long ago observed Sir Charles Bell,[1] "amputation is performed, ninety times in a hundred, for ulcer with carious bone, and for white swelling of the joints." These, like other cases of scrofulous disease, are far more likely to be cured—where a cure is possible—when the patients are under salutary country influences than when shut up in the wards of a large city hospital. But they form a class of cases which naturally gravitates towards large hospitals. The patients are invalided, crippled, and bed-ridden for weeks and months, and sometimes for years. If belonging to the poorer classes, they often thus exhaust betimes their own little means, the means of their relatives, friends, and neighbours; and perhaps get aid begrudingly from official sources. The prospect of a possible cure, of more generous diet and wine, and of more absolute rest, draws them naturally to a city hospital; and the country medical practitioner has no objection, but the reverse, as it rids him of much responsibility and trouble in such an unfavourable set of cases. If the case is not a fit one for resection of the joint, or removal of the diseased bones, amputation as a matter of necessity is ultimately and properly had recourse to. In the 2098 country, and 2089 hospital, amputations which I have already published, there was in each set a nearly equal number of primary or traumatic amputations of the thigh—viz. 313 in the country lists, and 304 in the hospital lists. But of pathological or diseased amputations there were only 356 amputations of the thigh for disease in the country returns, and as many as 631 in the city hospital returns.

When the operation, however, of amputation of the thigh for disease was practised upon the patients in the country in their isolated homes, the success of the operation was far greater than

[1] See his *System of Operative Surgery*, vol. i. p. 366.

when they were immured within the walls of a large hospital. For, when operated upon in their own houses only 12 in the 100 died; whilst, when operated upon in hospitals, as many as 37 in the 100 died; or, in other words, the thigh-amputations for disease proved three times more deadly in city hospital wards than in country private dwellings.

Mr. Holmes, as we have seen under some previous propositions, confuses the two questions of the rates of death after amputation, and the causes of death after amputation. On, however, this latter and favourite subject of his I have published some returns as to the 2000 odd country amputation cases, which are, I think, of no small value; for they go to prove, as another proposition, this fact, viz.—

PROPOSITION XVII.—*The causes of death after limb-amputations differ in some important points in large hospitals and in private country practice.*

Among the 2098 country amputations reported to me, 227 died. The causes of death have been returned to me in 173 out of these 227 cases, giving in every probability a fair average of the whole. Of these 173 cases, 63 are stated to have died of shock; 8 of pyæmia; 28 of exhaustion; 18 of gangrene of the stump; 9 of secondary hemorrhage; 11 of tetanus; 13 or more of internal injuries; 23 of miscellaneous causes, as pneumonia, phthisis, diarrhœa, convulsions, anasarca, etc.

Contrast this with the causes of death in a metropolitan hospital —such, for example, as the one to which Mr. Holmes belongs— viz. St. George's. Apparently from faith in the fact that amputations form the best test of the salubrity and status of an hospital, an "amputation book" was begun in St. George's in 1852, and, with slight exceptions, has been continued downwards to the present time. In the *Medical Times* for 6th April, 1861, Mr. Holmes has published the results of the first 149 cases entered in this amputation book.[1] Of these 149 operations 41 proved fatal. The causes of death are entered by him in percentages. The following table shows the differences between these 41 cases and their causes of death after amputation in a metropolitan hospital, and 173 cases and their causes of death in private country practice.

[1] In the first volume of *St. George's Hospital Reports* Mr. Holmes has more lately published an account of the first 300 cases in this "amputation book;" but the account of the deaths is so involved, that I have, for the sake of certainty, followed the more simple account in the *Medical Times*.

TABLE XXI.—PERCENTAGE OF DIFFERENT CAUSES OF DEATH AFTER AMPUTATION.

Causes.	In 41 fatal cases in St. George's Hospital.	In 173 fatal cases in country practice.
Shock[1]	5 per cent.	36·4 per cent.
Pyæmia	58·5 „	4·6 „
Exhaustion, without hemorrhage	17 „	16·2 „
Exhaustion, with secondary hemorrhage	9.7 „	5·2 „
Visceral diseases	9·7 „	12.7 „
Diffuse inflammation and gangrene	2·4 „	10·4 „
Tetanus	... „	6·3 „
Other injuries	2·4 „	7·5 „

I have interpolated this proposition respecting the causes of death in a metropolitan hospital like St. George's and in country practice, for the purpose of adding the following as another and still more important proposition—namely,

PROPOSITION XVIII.—*The causes of death which Mr. Holmes points out as the reasons for the greater mortality of amputations in the Parisian as compared with the London hospitals, are the same causes that produce the greater mortality of amputations in our large British hospitals as compared with country practice.*

After describing the different death-rates in Parisian and in London hospitals, and showing those of Paris to be "evidently enormous" as compared with those of London, Mr. Holmes states that the differences are certainly not explicable by differences either in the constitution of those operated upon in the French hospitals, or in the nature of the injuries or diseases for which they are admitted ; and then adds, "Indeed it is allowed that pyæmia and such diseases are in great measure *the causes* of these alarming results" (p. 563). Elsewhere, after showing, by instances, that in English hospitals the "intensity of the traumatic atmosphere is competent to engender erysipelas and pyæmia," Mr. Holmes adds—"The above teachings are entirely in accordance with the

[1] Mr. Holmes gives the deaths from shock in one table ; but not in his final table of percentages. Hence this percentage column appears so far in excess in its numbers.

opinions of those who maintain that the prevalence of erysipelas, pyæmia, and the like, in the Paris hospitals, is due to hygienic defects. It is known that some French authorities, while admitting the fact of the greater prevalence of these affections in their hospitals than in the hospitals of England, maintain that the difference is chiefly due to constitutional differences between Frenchmen and Englishmen. If it could be clearly shown that the relatively greater number of deaths among Frenchmen after operations and injuries was due to shock, this view might possibly be tenable ; but it can scarcely be accepted as the true one if the deaths can be shown to be (as in fact they are) due to the supervention of special complications, known to be natural consequences of certain defects of hygiene, which very defects prevail, with scarcely an exception, in the hospitals of Paris" (p. 549).

In these passages we have laid down for our guidance by Mr. Holmes two pathological principles or rules :—

1st. That if the "enormous" Parisian death-rate were the result of some general debility in the constitutions of the Parisian hospital patients, they ought to present a "relatively greater number of deaths due to *shock*." But—

2d. It is "pyæmia and such diseases" that are, in great measure, the causes of the enormous mortality of surgical operations in the Parisian as compared with the London hospitals.

Exactly, however, in the same way, we have equal and indeed still stronger evidence to the effect that the "enormous" death-rate after limb-amputations in large British hospital practice, as compared with country practice, is not owing to constitutional debility and consequent greater number of deaths by shock in the hospital patients ; but, on the contrary, is the result of pyæmia and those congener affections which are the "natural consequences" of defective hospital hygiene.

I depend for the present upon the proof of all this as furnished in the writings of Mr. Holmes himself. Let us glance first at the question of the evidence of the amount of—

1. *Death by Shock.*—Under Proposition XVII. we have seen evidence regarding the relative frequency of death by shock after amputations in St. George's Hospital and in country practice as amounting to this :—

In country practice 36 in 100 die of shock.
In hospital „ 5 in 100 „

Hence, therefore, as we cannot account for the "enormous" differences between the death-rate in Parisian as compared with English hospitals by finding the Parisians less able to bear amputation, as tested by more of them dying of shock, so we cannot account either for the "enormous" differences between the death-rate in large British hospital practice as compared with country practice by finding the hospital patients dying in comparatively greater proportion by shock; for the very reverse is the fact.

2. *Death by Pyæmia, etc.*—According to Mr. Holmes and most modern surgical pathologists, pyæmia or surgical fever is the most common cause of death in hospitals after amputation and other operations (see the quotations under Proposition VIII.); and in the passage cited one or two paragraphs back, Mr. Holmes expresses the general opinion that the high mortality of the amputations in the Parisian hospitals is due to this pathological cause. Pyæmia is shown to be the cause of death in *more than a half* of those that die in St. George's Hospital. (See Table XXI.) Mr. Bryant, the well-known surgeon of Guy's Hospital, in an excellent paper "On the Causes of Death after Amputation," came to the conclusion that, in the hospital to which he is attached, "pyæmia is the cause of death in 42 per cent of all fatal cases of amputation;"[1] and Mr. Holmes criticises this as probably too small. Perhaps we may justly hold, as a mean between the two, 50 per cent of the deaths after amputation in our large hospitals as the result of pyæmia.[2]

To what extent is this fatal complication, pyæmia, the cause of death in country amputations? Pyæmia is returned as the cause of death in 8 out of 173 fatal cases in which the various pathological causes of death are reported to me.[3] Hence we have this general result from those observations, that

In fatal country amputations, 5 in 100 die of pyæmia.
In fatal hospital amputations, 50 in 100 die of pyæmia.

If, then, pyæmia and its analogous diseases form, by their relative prevalence, as Mr. Holmes correctly maintains, the great cause of the "enormous" difference between the death-rates after amputation in the hospitals of Paris and the death-rates after

[1] *Medico-Chirurgical Transactions*, vol. xlii. p. 87.

[2] In a table kindly drawn up for me by Mr. M'Dougal of the causes of death in 72 fatal limb-amputations in the Edinburgh Infirmary from October 1865 to October 1868, 34, or nearly a half, died of pyæmia, and 11 of shock.

[3] See Chapter II.

amputation in the hospitals of London, it forms also, with its congeners, one of the great, or indeed the greatest cause of the enormous difference, in the death-rates in England, between large hospital practice and private country practice, as expressed in our present proposition.

The relative prevalence of this formidable and fatal complication within the walls of St. George's Hospital, with three or four hundred inmates, and the district without, from which the hospital chiefly derives its patients, containing a population of thousands, or tens of thousands, is presented by a statement made by Mr. Holmes in one of his papers in *The Lancet*. From 1865 to 1868 there originated 81 cases of pyæmia within the walls of St. George's Hospital; while during the same period 9 cases applied for admission from without. Few facts perhaps could show more clearly the usual hospital origin and character of this dreaded disease.

CHAPTER IV.

SOME PROPOSITIONS ON HOSPITALISM—(*continued*).

WHY IS HOSPITALISM DANGEROUS TO THE SICK?—Seeing, as we have in the last proposition, that pyæmia and its allied affections form the main sources of death in hospital patients after limb-amputations, and that this class of diseases is, as stated by Mr. Holmes and others, the consequence of defective hospital hygiene, or the result of hospitalism, let us try briefly to inquire *how* the aggregation of invalids within an hospital leads to the frequent production of this and other forms of surgical fever in surgical wards; while they are relatively so rare in country and private practice, where the patients are separate and isolated.

There exists, I think, evidence on this question, tending to show that the constitution of the surgical patient in surgical wards is liable to be endangered sometimes by the influence of morbific contagious materials from the bodies of the other inmates, though the blood-poisoning which leads on to pyæmia is generally produced by the inhalation of organic and other materials which usually exist in the air of hospital wards, but which are not contagious. Besides, the aggregation, or compression into a limited dwelling place like an hospital ward, of men even in a state of relative bodily and physical health, leads to some danger by hospitalisation, and renders the mortality among them higher than when the same class of men are lodged in separate and private dwellings.

PROPOSITION XIX.—*Surgical patients in surgical wards seem sometimes to have pyæmia or surgical fever induced by the accidental inoculation of the morbific secretions formed in the bodies of other patients previously affected.*

The obstetric physicians of Great Britain generally agree, I believe, as to the occasional spread of puerperal fever in this mode from

the affected to the healthy by the unhappy and indirect medium of the physician, nurse, etc., as well as by contagious miasmata. The great liability to the occurrence of this dire malady renders the consideration of the proper dimensions and construction of a lying-in hospital not a mere problem of space and size, but of adequate isolation also for the individual inmates. It is hence, with regard to obstetric hospitals, a question of contagion as well as a question of crowding. Surgeons seem, however, to consider the production of pyæmia by contagion, direct or indirect, as far more rarely a cause of surgical than it is acknowledged to be of puerperal fever. In some observations which I published several years ago upon the various analogies of puerperal and surgical fever, I suggested the occasional spread of the latter disease by means of occasional contagion and inoculation (see *Edinburgh Monthly Journal of Medicine* for November 1850); but the matter has hitherto attracted little attention from our hospital surgeons.

There are, perhaps, other modes in which the blood becomes more frequently vitiated in surgical wards, so as to lead on to the production of pyæmia. For we may hold the following as a more established proposition :—

PROPOSITION XX.—*The air breathed by patients aggregated in surgical wards becomes sometimes morbific and dangerous by its containing various inorganic and organic materials, and by the inmates mutually vitiating it more or less by the exhalations and excretions from their wounded, ulcerating, and sick bodies.*

In treating of the insalubrity of surgical hospitals and wards, Baron Larrey observes :—"The danger of surgical operations depends upon the vitiation of the atmosphere, especially during the night. The natural excretions of the sick, the breath, the fetid perspirations, the expectorated matter, the intestinal and urinary evacuations, the suppurations from wounds and ulcers, and sometimes the putridity of mortification or of hospital gangrene, are so many sources or foci of contamination; without counting the odours of medicine, tisanes, and poultices, the evaporation from liquids, the emanations from the soil, from the oil or gas used for illumination, from the bed-linen, and from the too closely situated or badly constructed latrines."

The air of surgical and hospital wards contains, according to later observations, various materials additional to those mentioned by

Baron Larrey. "The extent," observes Mr. Simon, writing in 1864, "to which organic matter may be present as floating dust in such atmospheres as that of an ill-ventilated hospital ward is only beginning to be recognised;" and he adduces the evidence of Dr. Thomson and Mr. Rainey as to epidermic scales and minute hairs, vegetable fibres and starch granules, and living forms, both animal and vegetable, vibriones and the mycelia of fungi, having been found floating in the air of the cholera ward of St. Thomas's Hospital. These bodies, with bacteria, etc., have latterly been detected abundantly in the atmosphere of other hospitals and crowded dwellings, and especially by Drs. De Chaumont, Frank, Hewlett, and other medical officers of the English army. Pasteur, Gratiolet, Lemaire, and others, maintain that the atmosphere is full (especially in localities where, as in hospitals, the air is otherwise impure and tainted) of living spores and germs of various infusoria, etc., which, when they find a proper nidus, lead on by their development to fermentations, putrefaction, suppurations, etc.

Lately, in the air of a prison, Lemaire detected various round or oval bodies of the same form as, in uncleanly persons, exist in the sweat. M. Pouchet states that he has discovered abundance of organic débris in the air; and Chalvet found in the wards of St. Louis Hospital, at Paris—what Revert had previously found at the Lariboisière—namely, floating organic matters in the surgical wards; and more in them than in the medical wards. The extent to which epithelial and other cells contribute in forming the organic matter in badly-cleaned hospitals is shown by the dust collected in the wards of St. Louis, this dust being found to contain in one experiment 36 per cent, and in another 46 per cent. These organic ingredients of the hospital-dust give out an odour of horn when burnt, and a fetid, putrid smell when moistened and allowed to decompose. When lying on the floor of the ward, this organic powder can be readily lifted by the passing draughts of air, and by the force of evaporation of water.

Dangers occasionally seem to exist in other directions. "The walls and floors of hospitals," writes Dr. Parkes, "absorb organic matters, and retain them obstinately; so that, in some cases of repeated attacks of hospital gangrene in a ward, it has been found necessary to destroy even the whole wall;"[1] and "the bedding and

[1] See Dr. Bristowe and Mr. Holmes's account of one of the wards of old St. Thomas's Hospital, where pyæmia persisted and returned in spite of the emptying of the ward, painting of the walls, etc.—P. 593.

furniture also absorb organic substances, and are a great cause of insalubrity."

Some years ago it was announced by Eiselt that pus-corpuscles existed in the air of a ward at Prague in which ophthalmia was prevailing; and Stromeyer, Dr. Parkes, and others, hold that in erysipelas, hospital gangrene, etc., dried and disintegrating pus-cells and putrefying organic particles pass into and float in the atmosphere. All these various ingredients, when floating in the air, are of course liable to be inhaled, and some of them absorbed after inhalation, by the patients in the wards.

Important results, both in a pathological and hygienic point of view, will, probably, ere long result from the more elaborate pursuit of this class of inquiries. Through the inhalation of such materials floating in the atmosphere, or in some such way, in all probability "certain diseases," remarks Dr. Parkes, "are propagated; the dried substance—as, for example, the evacuations of cholera or dysentery—floating through the air, and being finally swallowed or inhaled into the lungs. The specific poison of small-pox derived from the skin; of scarlet fever derived from the skin, throat, and urine (?); of measles derived from the skin and lungs (?), etc., must also be molecular organic matter, or even formed corpuscles, though as yet they have not been recognised."

It is well known, both from observation and experiment, that when once any such ingredients exist in the atmosphere, they are, in consequence of the laws of the diffusion of air and gases, so swiftly spread as speedily to travel through an hospital ward, and also into any adjoining passages, corridors, or staircases,—and even into the distant wards or rooms. This fact might be variously illustrated; but I shall content myself with citing one example as sufficient to enforce the remark. Some time ago an empyema, containing very fetid pus, was opened in a ward towards the end of the long corridor, or gallery, in the gigantic hospital at Netley. The smell from this putrid pus diffused itself along the corridor, so that, as I am informed by Dr. Parkes, it was ere long felt and complained of in rooms or wards up to five hundred feet distant on one side. But there are morbific and probably organic materials far more subtle, inscrutable, and deadly than merely bad aromas; as the contagious and malarial entities capable of producing typhus fever, scarlatina, ague, etc.

Every human being seems to exhale from his body an aroma, so far individually distinct that the dog can trace by it the footsteps of

his master. "*More* organic effluvia," to quote Dr. Parkes's words, "are given off from the bodies and excretions of *sick* men."[1] When two sick men are laid down in the same room, there is always a chance—slight in many cases it may be—of one of them deleteriously affecting in this way, by his exhalations, the bodily state of the other. When a sick or wounded patient is placed in a room or chamber by himself all such mischances from others are averted; and hence the advantages of perfect isolation of the sick. The danger, however, on the other hand, no doubt multiplies as the number of patients aggregated together is increased. In a ward of ten patients there is, *cæteris paribus*, a greater likelihood of harm than in a ward of two, and that specially on account of two reasons. For, first, they are all respiring the air mutually deteriorated by the whole increased number of sick; but, secondly, and chiefly, there is every chance that among this increased number of sick there may be one invalid, if not more, whose corporeal exhalations infect and pollute the air of the ward—and consequently the air breathed by the other sick inmates of the ward—in such a manner or in such a degree as, when long inspired, to be specially and specifically dangerous to the health and constitutions of one or others of the neighbouring patients. The hazard from this cause necessarily increases with the increased number of sick persons in a ward; and consequently also in an hospital under one roof, and whose wards all more or less intercommunicate by passages, corridors, etc.; for the hospital comes in this case to be, as far as the intermixture of its whole atmosphere is concerned, only one enormous and gigantic chamber. Suppose, for the sake of illustration, that there is a risk of danger in a ward of ten invalids, from the polluting and morbific exhalations proceeding from the diseased body of one of the ten,—then, in a ward, or series of communicating wards, containing, say, fifty patients, we may very roughly and conjecturally estimate that five out of the fifty will be similar sources of perilous deterioration and vitiation of the general atmosphere; in an hospital of one hundred beds, there may be ten such special centres; in an hospital of two hundred beds under one roof, twenty similar nuclei of infection; and in one of five hundred beds, fifty such sources of danger and disease to the included inmates. The effect of special vitiations of the air mutually produced by different patients has not, of course, it must be further observed, necessarily the same influence upon all. The effects of these and other etiological poisons vary with the

[1] See his admirable *Manual of Practical Hygiene*, p. 323.

susceptibility and state of predisposition of those who are subjected to them. An atmosphere full of typhus poison does not by any means strike down all that breathe it. Nor does exposure to air full of malarial poison produce ague in all exposed to it; and again, occasionally, when it causes ague in some, it creates—according to their condition of predisposition—dysentery in some, sickness and headache in others, etc. What special varieties or forms of hospital poison produce, when inhaled, pyæmia, has not yet been fully established by surgical science; nor are we at all aware of the special predispositions in patients which enable these poisons to develop the disease. The inquiry is one full of moment, both in itself, and in reference to the occasional though far rarer appearance of pyæmia in private as well as in hospital practice.

Effects of Hospitalism on Healthy Men.—In hospitalising men, or in aggregating them into well-filled rooms, wards, hospitals, and the like, we decrease their health-rate and increase their death-rate, even though the men thus hospitalised are comparatively in a state of good corporeal or physical health. I allude to the matter as showing that, in despite of some of the special evil influences of hospitals above adduced, the mere aggregation of men together in hospital and hospital-like establishments is attended with deleterious effects to health and life.

This circumstance has been more than once challenged in the pages of the *Lancet* during the course of the present year. But of its truth there cannot, I believe, be any reasonable doubt. As shown by Dr. Farr of London, Dr. Duncan of Liverpool, and Dr. Gairdner of Glasgow, the average mortality of our country and town districts, of our cities and their more crowded districts, increases with marked regularity according to the density of the inhabiting population. When men are in numbers aggregated or compressed into rooms, or wards, or hospitals, the same law, in a marked measure, overrules the result. For example, when soldiers—men in the prime of life, and selected for their healthy constitutions—are shut up in crowded barracks, they begin to suffer and die in numbers disproportionate to the civil population—chiefly from phthisis and typhoid in this country, and from fever, cholera, and dysentery in India. The different Government commissions which have made inquiries into the health of the British troops in barracks leave no doubt on this point as one cause of the otherwise unaccountable mortality in our army. In treating of the subject of barracks, Pro-

fessor Parkes observes (p. 304) : "At all times the greatest care is necessary to counteract the injurious effects of compressing a number of persons into a restricted space. In the case of soldiers the compression has been extreme. . . No expense," he adds, "has of late years been spared, but yet the fact remains that the very habitations erected for their shelter and comfort have proved to the soldiers a source of suffering and death."[1]

From these and similar data it might be reasonably argued and inferred that hospitalisation, or the compression of a number of persons, even in health, within a given house, dwelling, or ward, would be, in itself, more or less prejudicial to their health and life. But there is one piece of evidence on the question, more precise perhaps than the preceding, and which I may throw into the form of another proposition.

PROPOSITION XXI.—*The residence in hospital wards of men in a state of relative bodily health is attended with a loss of life markedly greater than when the same class of men are not thus hospitalised, but allowed to reside in private cottages and dwellings.*

The evidence in proof of this statement is as follows:—The Board of the Commissioners in Lunacy for Scotland have for some years past allowed pauper chronic lunatics, chiefly dements and idiots, who were mentally incurable, and beyond all hope of treatment from detention in a lunatic asylum, to be, for the sake of economy and other reasons, removed from asylums, and lodged either —1, in lunatic wards attached to poor-houses ; or 2, in the houses of peasants and others, with whom they are boarded, usually at a small expense. Above 1600 are thus resident in private dwellings, and above 500 in the lunatic wards of poor-houses in Scotland. These two classes of incurable lunatics are, I am informed, similar in all essential points, as far as a comparison of their health-rates and death-rates is concerned. Of the chronic lunatics resident in the wards of poor-houses, 8·5 per cent died annually, on an average, from the years 1861 to 1867 inclusive. Of the chronic lunatics

[1] In the case of cavalry horses, where the question is more simple than in the case of soldiers, in consequence of the "absolute similarity, in different places, of their food, water, exercise, and treatment," the effect on health and life in these animals from diminution of numbers in the stables, and increased ventilation and cleanliness, has proved very striking. See Dr. Parkes's *Treatise*, p. 84. "In cowhouses and kennels," he adds, "similar facts are well known ; disease and health are in the direct proportion of foul and pure air."

resident in private dwellings there died annually, on an average during these same seven years, 5·6 per cent. In other words—

In private dwellings, 56 in 1000 die annually.
In lunatic wards, 86 in 1000 die annually.

The mortality among these pauper patients in private dwellings is thus seen to be much less than among patients in establishments. Yet the latter, or those lodged in the parochial asylums of poor-houses, and among whom the greater yearly mortality steadily occurs, are more carefully tended than the former, because all the rules for their management are under more immediate official control. Careful rules have been laid down in order to ensure their comfort, cleanliness, good feeding, and proper clothing. They are, however, *hospitalised;* whilst the 1600 amongst whom the lesser yearly mortality occurs are not hospitalised, but are boarded out in the families of the peasantry and the poor, and are scattered over various districts of the country. In speaking of the respective treatment of these hospitalised and unhospitalised lunatics, and of the marked difference of mortality between the two classes, the Commissioners in Lunacy observe: "We can offer no explanation of this fact beyond the conjecture that the manner of living in private dwellings, involving, as a rule, greater freedom and greater variety, and the respiration of an atmosphere less loaded with animal exhalations, more than counterbalances the advantages which better diet, better clothing, better bedding, better housing, and greater cleanliness, might be supposed to convey" to the lunatics lodged in parochial asylums, as compared with those permitted to live in private dwellings

CHAPTER V.

STATISTICS OF 3077 PROVINCIAL HOSPITAL AMPUTATIONS.

1. MORTALITY FROM LIMB-AMPUTATIONS IN THE PROVINCIAL HOSPITALS OF GREAT BRITAIN.

WITH the view of following out these inquiries as to the death-rate of limb-amputations under different conditions, I have tried to collect statistics on the subject from the Provincial Hospitals of Great Britain. For it seemed important in relation to some points in the investigation to be able to contrast the results of amputations in our more limited provincial hospitals with the results of the same operations in our large and metropolitan hospitals on the one hand, and with their results in private country practice on the other hand. Besides, the inquiry promised to offer the most satisfactory kind of solution that could be obtained to the question as to the size of hospitals influencing or not, as a general law, their degree of salubrity or their degree of mortality.

The following was the form of schedule sent out to the different provincial hospitals of Great Britain, with the hope of procuring a return of the cases in which the four major amputations of the limbs had been performed in them in the continuity of the bones :—

"RESULTS OF THE FOUR AMPUTATIONS OF THE THIGH, LEG, ARM, AND FOREARM, IN THE HOSPITAL FROM 1862 TO .

(*Amputations* through *the Joints are not to be included.*)

Year.	FOR INJURY.								FOR DISEASE.							
	Thigh.		Leg.		Arm.		Forearm.		Thigh.		Leg.		Arm.		Forearm.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
18																
18																
18																
18																
18																
18																
18																
Total .																

Signature, _____

Residence, _____

Date, _____

REMARKS."

I asked only for the cases from 1862 onwards, partly because I was desirous of procuring the latest returns; and partly because many of the returns from these hospitals had been already published up to that year, or even inclusive of it, in the elaborate official reports drawn up by Dr. Bristowe and Mr. Holmes for the Medical Officer of the Privy Council.[1] One or two hospitals furnished me with returns from an earlier year, which I have, of course, not hesitated to include.

In consequence of the schedules issued, I have, up to the time of tabulating and calculating all the data, obtained returns of the results of amputation of the limbs from seventy-four provincial hospitals in Great Britain. From the remainder of these hospitals I have failed in procuring the necessary data. In some no register of the results of operations is kept.

The total number of cases of limb-amputation reported from those seventy-four British hospitals amounts to 3077.

The seventy-four hospitals vary much in size. In the tables which follow, I have arranged them and their results in accordance with their individual extent; or, in other words, in accordance with the number of beds which they each contain. For this purpose, I laid down the following four divisions or series, and arranged the hospitals subsequently under their respective heads, viz.—

1. Hospitals with 25 beds and under;
2. Hospitals with 26 to 100 beds;
3. Hospitals with 101 to 200 beds;
4. Hospitals with 201 to 300 beds.

The only hospitals in Great Britain which at present contain a larger number of beds are the Royal Infirmaries of Edinburgh and Glasgow, and the four Metropolitan Hospitals of St. Bartholomew's, St. George's, Guy's, and the London Hospital in Whitechapel, which can each accommodate from 300 to 600 patients or more. I have already, in Chapter II. 8, given at length the annual amputation statistics for some years back of these several hospitals. In adducing the statistics of the provincial hospitals, I shall begin with those of the largest size, and proceed from thence downwards in the series. In doing so I shall collect together into separate tables all the amputation returns furnished to me pertaining to each series individually; and use for this purpose the general sums of all the

[1] See the *Sixth Annual Report of the Medical Officer of the Privy Council:* London, 1864.

amputations for all the years which I have received from each hospital. It would take up unnecessary space to print all the returns for all the included years of each provincial hospital—as I have already done of the six largest city or metropolitan hospitals above referred to. Besides, the statistical data furnished by the provincial hospitals individually are usually too small by themselves for statistical conclusions; but they become adequate for this purpose when collated and calculated together in sufficient masses.

FIRST SERIES.

Provincial Hospitals with 201 to 300 Beds.

There are seven hospitals only included under this head; the largest of the seven, the Royal Infirmary of Liverpool, containing 270 beds, and the smallest, the General Hospital of Birmingham, containing 223 beds. One of the seven hospitals, the Margate Royal Sea-bathing Infirmary, is (to quote the words of my friend Dr. Rowe, in sending me the return of the operations) "special in its character; casualties as a rule are not admitted, but are sent to the Canterbury Hospital. Hence the entire absence in the report of operations for injury."

TABLE XXII.—RESULTS OF THE FOUR AMPUTATIONS OF THE THIGH, LEG, ARM, AND FOREARM, IN SEVEN BRITISH HOSPITALS CONTAINING FROM 201 TO 300 BEDS.

No.	Name of Hospital.	No. of Beds.	Years.	FOR INJURY.								FOR DISEASE.							
				Thigh.		Leg.		Arm.		Fore-arm.		Thigh.		Leg.		Arm.		Fore-arm.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Liverpool Infirmary . . .	270	1862-8	22	16	24	10	15	4	11	1	54	18	62	7	7	3	18	0
2	Dundee Infirmy.	260	1861-8	8	4	9	6	32	13	18	1	5	2	4	4	4	2	1	0
3	Newcastle ,,	250	1866-8	12	6	15	8	13	8	8	1	8	2	2	0	1	0	0	0
4	Margate Royal Seabathing Infimary . .	250	1864-9	0	0	0	0	0	0	0	0	23	7	19	0	5	0	2	0
5	Bristol Infirmary	242	1862-8	5	3	13	2	8	1	5	1	32	5	20	1	2	1	4	0
6	Aberdeen ,,	229	1860-8	15	10	10	3	10	0	5	0	41	9	21	4	3	1	7	1
7	Birmingham Gen. Hospl.	223	1862-4, 1866-8	19	9	39	20	33	8	18	4	54	16	23	4	5	0	9	2
			Total .	81	48	110	49	111	34	65	8	217	59	151	20	27	7	41	3
	Mortality per cent . .			59·2		44·5		30·6		12·3		27·1		13·2		25·9		7·3	
	Or proportionally 1 in .			1·7		2·2		3·2		8·1		3·6		7·5		3·8		13·6	

Total number of cases, 803 ; of deaths, 228 ; or 1 in every 3·5, or 28·3 in 100, died.

Total number of amputations for injury, 367 ; of deaths, 139 ; or 1 in 2·6, or 37·8 in 100, died.

Total number of amputations for disease, 436 ; of deaths, 89 ; or 1 in 4·9, or 20·4 in 100, died.

Mortality of Individual Amputations.

Thigh	cases, 298 ;	deaths, 107 ;	or 1 in 2·8 ;	or 35·9	per cent.
Leg	,, 261 ;	,, 69 ;	or 1 in 3·7 ;	or 26·4	,,
Arm	,, 138 ;	,, 41 ;	or 1 in 3·3 ;	or 29·7	,,
Forearm	,, 106 ;	,, 11 ;	or 1 in 9·6 ;	or 10·3	,,

SECOND SERIES.

PROVINCIAL HOSPITALS WITH 101 TO 200 BEDS.

This series includes twenty hospitals. Most of them range in number of beds from 101 to 150. Indeed, only one hospital on the list rises to a higher number, viz. the Sussex County Hospital, which contains 165 beds. There are four hospitals with 150 beds each ; and six with 120 beds each. The hospitals of Leeds and Nottingham, and the Northern Hospital of Liverpool, yield in this series the largest number of amputation returns. The Infirmary of Liverpool, a larger institution than the Northern Hospital of that town, is included in the preceding or first series.

TABLE XXIII.—RESULTS OF THE FOUR AMPUTATIONS OF THE THIGH, LEG, ARM, AND FOREARM, IN TWENTY BRITISH HOSPITALS CONTAINING FROM 101 TO 200 BEDS.

No.	Name of Hospital.	No. of Beds.	Years.	FOR INJURY.								FOR DISEASE.							
				Thigh.		Leg.		Arm.		Fore-arm.		Thigh.		Leg.		Arm.		Fore-arm.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Sussex	165	1862-68	2	1	15	7	6	1	3	0	10	6	13	2	6	0	1	0
2	Chester	150	1864-69	5	3	16	3	6	0	5	0	1	0	5	0	1	1	0	0
3	Bristol	150	1863-65 & 1868	9	5	6	4	2	1	2	0	18	1	8	0	0	0	2	0
4	Derbyshire	150	1862-68	10	5	15	3	3	0	2	0	13	5	16	0	2	0	6	0
5	Norfolk and Norwich	150	1862-68	9	2	11	2	2	0	15	3	23	6	10	3	9	1	5	0
6	Radcliff, Oxford	149	1862-68	2	1	10	3	5	0	5	0	16	1	12	2	1	1	0	0
7	Nottingham	142	1862-68	20	5	20	3	15	2	14	1	32	5	13	2	12	2	9	0
8	Leeds	140	1862-68	18	14	43	19	41	17	55	3	44	11	35	7	12	2	7	1
9	Shrewsbury	140	1863-69	13	6	20	5	20	3	6	0	14	3	14	1	3	0	4	0
10	Liverpool, North.	134	1862-68	24	16	35	12	13	4	14	3	6	2	3	1	1	1	1	0
11	Paisley	132	1862-68	6	0	17	2	7	1	9	0	0	0	9	2	3	0	0	0
12	No. Staffordshire	125	1862-68	13	4	41	13	12	1	6	1	15	4	7	0	0	0	0	0
13	Inverness	120	1862-68	1	0	4	1	0	0	1	0	2	1	1	1	1	0	2	1
14	Bath Royal United	120	1862-68	3	2	5	3	5	1	5	0	26	7	12	2	2	0	1	0
15	Reading (Royal Berks)	120	1862-68	8	1	10	2	6	0	10	3	26	3	16	0	2	0	2	0
16	Liverpool, South.	120	1862-68	7	2	13	6	17	5	4	0	7	1	8	0	4	1	1	1
17	Kent and Canterbury	120	1862-68	2	1	1	0	6	0	8	0	8	0	5	1	1	0	9	0
18	Buxton	120	1861-67	1	0	1	1	0	0	0	0	0	0	0	0	0	0	0	0
19	Greenock	119	1862-68	9	5	5	0	11	4	1	0	1	0	1	0	1	0	0	0
20	Rochester	108	1863-68	5	1	4	0	1	0	1	0	2	1	3	0	0	0	1	0
			Total	167	74	292	89	178	40	166	14	264	57	191	24	61	9	51	3
	Mortality per cent			44·3		30·4		22·4		8·4		21·5		12·5		6·7		5·9	
	Or proportionally 1 in			2·2		3·2		4·4		11·8		4·6		8		14·7		17	

Total number of cases, 1370; of deaths, 310; or 1 in every 4·4, or 22·6 in every 100, died.

Total number of amputations for injury, 803; of deaths, 217; or 1 in every 3·7, or 27 in every 100, died.

Total number of amputations for disease, 567; of deaths, 93; or 1 in every 6·1, or 16·4 in every 100, died.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations is as follows:—

Mortality of Individual Amputations.

Thigh	cases, 431;	deaths, 131;	or 1 in 3·3;	or 30·4 per cent.
Leg	„ 483;	„ 113;	or 1 in 4·2;	or 23·4 „
Arm	„ 239;	„ 49;	or 1 in 4·8;	or 20·5 „
Forearm	„ 217;	„ 17;	or 1 in 12·7;	or 7·8 „

THIRD SERIES.

Provincial Hospitals with 26 to 100 Beds.

This list includes thirty British hospitals. Four of them contain 40 ; three of them 50 ; three of them 60 ; three of them 90 ; and five of them 100 beds each. The largest numbers of amputations returned in this series are from two hospitals belonging to maritime towns—namely, Plymouth and Cardiff.

Table XXIV.—Results of the Four Amputations of the Thigh, Leg, Arm, and Forearm, in Thirty British Hospitals containing from 26 to 100 Beds.

No.	Name of Hospital.	No. of Beds.	Years.	For Injury.								For Disease.							
				Thigh.		Leg.		Arm.		Fore-arm.		Thigh.		Leg.		Arm.		Fore-arm.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	York	100	1862-8	4	1	2	0	1	0	1	0	5	0	5	0	4	0	1	0
2	Worcester	100	1863-8	9	4	22	0	8	0	9	1	0	0	10	1	1	0	2	1
3	Lincoln County	100	1862-8	6	2	5	1	5	2	4	0	11	1	8	1	4	0	4	0
4	Dumfries	100	1862-8	2	1	5	3	4	1	2	0	2	2	9	1	3	0	0	0
5	R. South Hants	100	1862-8	3	2	7	3	2	0	8	0	8	3	2	0	2	0	3	0
6	Salisbury	98	1862-8	7	4	2	0	4	0	1	0	17	3	4	0	4	1	8	0
7	Cheltenham	90	1862-8	5	3	5	1	4	1	1	0	7	0	3	1	0	0	0	0
8	Plymouth	90	1863-8	13	2	10	2	7	1	5	0	9	1	2	0	2	1	4	0
9	Taunton	90	1866-8	1	1	2	1	2	0	4	0	3	0	0	0	0	0	0	0
10	Montrose	70	1863-9	1	1	0	0	2	1	0	0	5	2	0	0	1	0	4	1
11	Lancaster	70	1865-8	2	1	7	2	1	0	3	1	0	0	0	0	0	0	0	0
12	Truro	60	1862-8	7	2	8	0	7	0	2	0	11	0	6	0	1	1	2	0
13	Chichester	60	1862-9	1	0	3	0	3	2	3	0	4	1	6	3	1	0	1	0
14	North Riding	60	1864-9	4	1	7	3	2	1	1	0	1	0	4	1	1	0	0	0
15	Royal Surrey Co.	54	1866-9	5	1	1	1	1	1	0	0	4	1	2	0	0	0	2	0
16	W. Norfolk and Lynn	52	1862-8	2	1	1	0	7	1	1	0	10	2	7	1	2	0	0	0
17	Monkland	50	...	0	0	0	0	0	0	0	0	1	0	0	0	1	0	0	0
18	Cardiff	50	1862-9	10	4	17	6	7	0	3	0	3	0	4	1	0	0	0	0
19	Halifax	50	1862-8	2	1	7	3	15	3	7	1	6	0	10	0	1	0	0	0
20	Balfour, Kirkwall	48	...	0	0	0	0	1	1	3	0	6	0	9	0	0	0	1	0
21	Huntingdon	42	1862-8	3	1	4	1	3	1	2	0	3	1	3	1	2	0	0	0
22	Denbigh	40	1862-9	1	1	0	0	0	0	3	1	4	0	4	1	1	0	2	0
23	Bridgewater	40	1862-8	4	2	4	0	0	0	5	1	3	1	1	1	0	0	0	0
24	Stamford	40	1862-8	3	1	2	1	2	0	1	0	8	2	3	1	1	0	1	0
25	Carmarthenshire	40	1862-8	2	0	7	1	1	0	1	0	0	0	1	0	0	0	3	0
26	Hartlepool	36	1865-9	1	0	3	0	1	0	0	0	0	0	0	0	0	0	0	0
27	Hertford	35	1863-9	3	1	2	1	1	0	4	0	0	0	0	0	0	0	0	0
28	Swansea	34	1862-8	2	1	17	3	6	1	1	0	2	0	7	0	2	0	1	0
29	Great Yarmouth	28	1862-9	1	0	2	1	1	0	5	0	2	0	3	0	1	0	1	0
30	Ramsgate	26	1865-8	2	0	1	0	0	0	1	0	0	0	0	0	0	0	0	0
			Total	106	39	153	34	98	17	81	5	135	20	113	14	35	3	40	2
Mortality per cent				36·8		22·2		17·3		16·1		14·7		12·4		8·5		5	
Or proportionally 1 in				2·7		4·5		5·7		16·2		6·7		8		11·6		20	

Total number of cases, 761 ; of deaths, 134 ; or 1 in every 5·6, or 17·6 in every 100, died.

Total number of amputations for injury, 438 ; of deaths, 95 ; or 1 in every 4·6, or 21·6, in every 100, died.

Total number of amputations for disease, 323 ; of deaths, 39 ; or 1 in every 8·3, or 12 in every 100, died.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations is as follows :—

Mortality of Individual Amputations.

Thigh	cases,	241 ;	deaths,	59 ;	or 1 in 4 ;	or 24·4	per cent.
Leg	„	266 ;	„	48 ;	or 1 in 5·5 ;	or 18	„
Arm	„	133 ;	„	20 ;	or 1 in 6·6 ;	or 15	„
Forearm	„	121 ;	„	7 ;	or 1 in 17·2 ;	or 5·8	„

FOURTH SERIES.

Provincial Hospitals with 25 Beds and under.

This series includes within it chiefly the so-called Cottage Hospitals of Great Britain. They have been usually opened, in the first instance at least, in houses that had been previously used as private dwellings. According to Mr. Churchill's Directory, the largest "cottage hospital" in England, and one of the earliest, is that of Middlesborough in Yorkshire, which contains 25 beds. Some of them are much smaller. The hospitals at Fowey in Cornwall, and Shedfield in Hampshire, contain only four beds each ; and that of Shotley Bridge, Durham, though it is only provided with five beds, has already a list of ten limb-amputations. The first cottage hospitals in England were, I believe, founded about ten years ago ; and one or two of the returns include the interval from that time to this. But most of them are much later. The small general Infirmaries of Kidderminster, Bridgnorth, and Brecknock, which contain 11 or 12 beds each, date their returns from 1862.

TABLE XXV.—RESULTS OF THE FOUR AMPUTATIONS OF THE THIGH, LEG, ARM, AND FOREARM, IN SEVENTEEN BRITISH HOSPITALS WITH 25 BEDS AND UNDER.

Name of Hospital.	No. of Beds.	FOR INJURY.								FOR DISEASE.							
		Thigh.		Leg.		Arm.		Fore-arm.		Thigh.		Leg.		Arm.		Fore-arm.	
		Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
Middlesborough	25	3	1	20	3	7	1	0	0	1	0	6	0	1	0	0	0
Penrhyn . . .	18	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0
Barrow . . .	18	1	0	2	0	4	0	4	0	0	0	0	0	0	0	0	0
Teignmouth . .	17	1	0	1	1	0	0	0	0	0	0	0	0	0	0	0	0
Gravesend . .	15	5	1	1	1	0	0	4	2	1	0	0	0	0	0	0	0
Kidderminster .	12	0	0	5	1	3	0	5	0	0	0	0	0	0	0	0	0
Brecknock . .	12	0	0	1	0	2	0	1	0	0	0	3	0	0	0	0	0
Bridgnorth . .	11	2	1	3	1	0	0	2	0	0	0	0	0	0	0	0	0
Savernake . .	10	0	0	0	0	0	0	1	0	1	0	0	0	1	0	0	0
Dinorwic . . .	8	1	0	0	0	0	0	1	0	2	0	0	0	0	0	0	0
Cranleigh . .	6	1	0	0	0	0	0	1	1	2	1	0	0	1	0	1	0
Shotley Bridge .	5	7	2	2	1	1	0	0	0	0	0	0	0	0	0	0	0
Fowey . . .	4	3	0	1	0	0	0	0	0	0	0	2	0	0	0	0	0
Oswestry . .	...	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
Walsall . . .	24	0	0	1	0	4	0	3	0	3	1	6	0	2	1	2	0
Shedfield . . .	4	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0
Ditchingham .	10	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0
Total . . .		24	5	38	8	22	1	24	3	10	2	17	0	5	1	3	0
Mortality per cent		20·8		21		4·5		12·5		20		0		20		0	
Or proportionally 1 in		4·8		4·7		22		8		5		0		5		0	

Total number of cases, 143 ; of deaths, 20 ; or 1 in every 7·1, or 14 in every 100.

Total number of amputations for injury, 108 ; of death, 17 ; or 1 in every 6·3, or 15·7 in every 100.

Total number of amputations for disease, 35 ; of deaths, 3 ; or 1 in every 11·6, or 8·6 in every 100.

If we combine together the amputations for injury and for disease, the mortality from the individual amputations is as follows :—

Mortality of Individual Amputations.

Thigh cases, 34 ; deaths, 7 ; or 1 in every 4·8 ; or 20·6 per cent.
Leg „ 55 ; „ 8 ; or 1 in every 6·8 ; or 14·5 „
Arm „ 27 ; „ 2 ; or 1 in every 13·5 ; or 7·4 „
Forearm „ 27 ; „ 3 ; or 1 in every 9 ; or 11·1 „

The three first series of provincial hospital returns given in the preceding tables contain respectively 803, 1370, and 761 limb-amputa-

tions. These several masses of data afford perhaps in each series a sufficient foundation for arriving at fair and reliable statistical inferences. In the fourth series, the data are as yet too small for coming to any very certain statistical conclusions on the subject; and a few years will need to elapse before our cottage hospitals can furnish a sufficient basis of data for more decided and determinate results. Perhaps the data which already exist ought to give a more favourable view of the salubrity of these cottage hospitals for operative purposes than the last of the preceding series of tables indicates. For in reference, for example, to the "Oswestry Cottage Hospital," Dr. Blaikie has returned to me under its schedule twelve successful limb-amputations without a death, though I have entered only one of these. This is the only case that has occurred in the newly opened cottage hospital at Oswestry; but the eleven other successful cases had previously occurred in the small Workhouse Infirmary of Oswestry, which was the predecessor of the cottage hospital, and may justly, perhaps, be assimilated with it, and placed, as is done by Dr. Blaikie, in the same table. Again, in consequence of it being stated in Mr. Churchill's *Medical Directory* that the Balfour Hospital at Kirkwall contained 48 beds, I entered it and its results in the third of the preceding tables of provincial hospitals. Since, however, these tables were calculated and finished, I have, in a conversation with Dr. Logie, surgeon to the hospital, been assured by him that the institution has never had above 12 beds, though, being established in a large and old dwelling-house, it might accommodate more; and truly, therefore, it belongs to the series of cottage hospitals. If we are justified, then, in adding to the limb-amputations performed in hospitals with 25 beds or less, the Oswestry and Kirkwall returns, instead of having in the fourth series of these small provincial hospitals 143 limb-amputations with 20 deaths, or 1 in every 7, the list should be extended to 174 cases of limb-amputations with 21 deaths, or 1 death in every 8.

2. The Mortality of Limb-Amputations as regulated by the Size of Hospitals, and the degree in which Patients are aggregated or isolated.

In Chapters I. and II. we have seen that in our large metropolitan hospitals about 41 in every 100 operated on die of those patients who are subjected to the four major amputations of the limbs; while in single or isolated rooms in country practice, patients

die, under the very same class of operations, to the extent only of 10 or 11 in every 100. In the last Chapter we have collected evidence of the death-rate from these same four major limb-amputations in British provincial hospitals of various sizes. If we throw the whole facts thus collected into a tabulated form, the general results may be stated as follows :—

Size of Hospital, etc.	*Death-rate.*
1*st Series.*—In large and metropolitan British hospitals, chiefly containing from 300 to 500 beds or upwards, out of 2089 limb-amputations 855 died, or .	**1 in 2·4**
2*d Series.*—In provincial hospitals containing from 201 to 300 beds, out of 803 limb-amputations 228 died, or	**1 in 3·5**
3*d Series.*—In provincial hospitals containing from 101 to 200 beds, out of 1370 limb-amputations 301 died, or	**1 in 4·4**
4*th Series.*—In provincial hospitals containing from 26 to 100 beds, out of 761 limb-amputations 134 died, or	**1 in 5·6**
5*th Series.*—In provincial hospitals containing 25 beds or under, out of 143 limb-amputations 20 died, or	**1 in 7·1**
6*th Series.*—In British private country practice, with the patients operated on in single or isolated rooms, out of 2098 limb-amputations 226 died, or .	**1 in 9·2**

These data go to point out and establish the general fact or general law in hospital hygiene, that the death-rate accompanying amputation of the limbs,—and, as we may infer, the death-rate accompanying other surgical operations, and many medical diseases also,—is regulated, *cæteris paribus*, in a striking manner by the size of the hospitals, and the degree of aggregation or segregation in which the patients are treated. But, like all general laws in medicine, this law is subject to many exceptions. Thus, a small hospital, if overcrowded with beds and patients, becomes as insalubrious as a large hospital under one roof. On the other hand, a large hospital would be generally made almost as salubrious as a small institution, provided few beds were left scattered over its wards, and these wards were well ventilated and often changed. But such exceptions only establish more securely the great hygienic law, that, in the treatment of the sick, there is ever danger in their aggregation, and safety only in their segregation; and that our hospitals should be constructed so as to avoid as far as possible the former, and secure as far as possible the latter condition.

CHAPTER VI.

HOW OUR PRESENT HOSPITALS COULD BE MOST EASILY ALTERED IN ORDER TO RENDER THEM MORE SALUBRIOUS.

THERE is another question of vast moment in the subject of hospitalism—namely, How could our present older hospitals be most simply and cheaply changed with the hope of improving their insalubrious character? I have elsewhere commented shortly on this subject.[1] Perhaps there are two chief methods by which such

[1] The following is the comment referred to :—

"The idea of changing our hospitals, in order to increase their salubrity and utility, from palaces—containing aggregations of sick in each flat—into wooden, or brick, or iron villages, is one which I advocated and published above twenty years ago, and have often talked of and discussed since that time. But in this country it has, till lately, met with little support. Since the German War, however, the same idea has been reduced to practice to a small initial extent in that country, in consequence of military exigencies having obliged the authorities to place the sick and wounded in wooden and other temporary buildings, which were found to be much more salubrious than their mansioned and palatial hospitals. To-day I have a letter from Professor Gurlt, of Berlin, in which he states, in regard to the present use of huts, cottages, or tents around some of the chief hospitals in Germany, "They are much in favour with us, because they answer excellently well, and give excellent results. Besides three large hospitals (two civil and one military) in Berlin, they are used in Leipsic, Heidelberg, Darmstadt, Frankfort, Kiel, and other places. There is now a wooden double-walled hospital barrack here at the Charité Hospital, and, as far as I know, also at Leipsic, in constant use summer and winter, heated of course in the latter by large stoves." During the late American War there were above 200 temporary hospitals erected, containing above 130,000 beds ; more than one million patients were treated in them ; and the Surgeon-General to the Army of the United States, Dr. Hammond, declared that they were found "*far* healthier than permanent buildings." Why should we reject in civil practice means found so much better and healthier in military practice ?

"If hospitals are still to be built upon a large scale, perhaps means may be devised to invest them with greater safety by reducing them far more to the character of separate cottages and tenements, by each ward or room being provided with a ventilation separate and distinct. This has been already accomplished in the construction of some of the new blocks of tenements for artisans, lately reared in London and elsewhere. But in our large hospitals, as at present constructed, the commixture of the common and contaminated air in wards, staircases, corridors, etc., utterly prevents this desirable consummation.—*British Medical Journal*, January 30, 1869." [ED.]

a revolution may be effected architecturally: first, by making the staircases, corridors, etc., all open; or, secondly, by building up the doors and entrances of the wards into these staircases, corridors, etc., and by making an entrance into each ward from without. One or other of these two plans would, I believe, suffice for the improvement of most hospitals. Other and better plans, however, may possibly be devised.

We have already seen, under Proposition XX., that, in consequence of the rapid diffusion of light and aëriform bodies in the atmosphere of an hospital, the contents, organic and inorganic, which every ward contains, and the exhalations, cutaneous, pulmonary, etc., which emanate from each patient, pass out of the wards themselves to the corridors, galleries, staircases, etc., of the house, and in consequence of all the wards opening into them, pass and repass from one to the other in a more or less diluted and intermixed form, just as if the whole hospital were one single vast chamber. The deteriorated and polluted atmosphere of the wards, passing freely into the staircases, galleries, etc., is retained and imprisoned, as it were, there, and liable to pass onward into other apartments and wards; for there is generally, as is well known, a double and contrary current of air passing at different heights in and out of the doors of such apartments. When the foul air of a house, or apartment, or ward, passes out directly into the outer atmosphere, it then and at once loses any deleterious properties belonging to it, because it is swiftly diffused through the almost ever-moving and ever-changing external air, in which a rapid system of self-purification is constantly going on. By having the staircases, corridors, galleries, etc., of an hospital freely open to the external air, and by thus permitting the external air to be constantly passing and sweeping through them, would we not attain two great objects?—First, the avoidance of the retention and imprisonment to a greater or lesser degree of the *internal* befouled hospital air in these staircases, corridors, galleries, etc., with the chance of its repassing in its deteriorated state into other wards; and, secondly, the filling and occupation of the staircases and other spaces with air having all the purity and purifying powers of the *external* atmosphere. The degree to which this latter important object could be obtained in each individual hospital would depend, of course, upon the degree to which the staircases, passages, etc., were freely and fully opened up. The permanent opening, in half or in whole, of a single window or two in a staircase, or corridor, or gallery, would perhaps do much; but the permanent opening of all

the windows would do far more. If ever, in order to render the opening of these staircases, galleries, etc., more full and free, the removal of a portion of their containing or outer wall was required to be adopted in some instances, this would be, after all, doing nothing more than what has recently been effected in the building schemes of Sir Sidney Waterlow and other philanthropists in the construction, in London and elsewhere, of blocks of houses for artisans, the flats being connected by *open* staircases, and each flat being provided with an *open* balcony, in which balcony all the doors of the tenements of that flat have their entrance. If an hospital were so altered as here suggested, the entrance of each ward from the stair-landings, corridors, etc., would probably require to be provided with double doors, to protect, when necessary, against gusts of wind and cold.

It is not my object here to discuss the question how each separate ward may be best and most thoroughly ventilated. Many artificial plans have been proposed, and have been attempted to be put in practice; but all these artificial plans have hitherto, it must be confessed, ended in failure. We can readily send in carburetted hydrogen gas by tubes into houses and rooms from great distances for the purpose of lighting; and some similar simple plan of pouring into every inhabited chamber, even in our crowded cities, an adequate and continuous stock of fresh and country atmospheric air, may yet betimes be fallen upon; but up to the present hour, no artificial system of perfectly and successfully ventilating individual wards has been suggested, and the "natural system" of ventilation by fireplaces, and opposite and partially-opened windows, is chiefly, and in most instances entirely, depended upon.

Instead of opening up staircases, corridors, etc., if they were left in their present condition in some hospitals, perhaps the perfect isolation and ventilation of the individual wards could be effected more perfectly in the following manner:—Build up the doors, and any other entrances from the wards into the stair-landings, corridors, etc., and make all the wards and all the flats be entered from without, either by new external staircases, if the hospital were not above two storeys in height, or by covered balconies or galleries placed upon the outer wall of the ward, and to which balconies or galleries entrance was given by the existing staircases. If every ward were thus prevented from sending its deteriorated air into the interior of the house, with the mischance of polluting the general atmosphere of the hospital, and if itself it had only communication with the external atmosphere, then each ward would become a

separate cottage hospital, as it were, with a self-ventilation entirely its own. The perfection or imperfection of that self-ventilation would depend upon the number and position of its fires, windows, etc.; but its contained patients would be freed from the danger of breathing an air contaminated by the emanations and exhalations from the patients in the other wards of the institution. One of the end windows in each ward would of course, on this principle, require to be changed into a door-entrance, and to be duly protected by being provided either with a double door, or by being made to open into a small end room, such as are found in almost all hospital wards. These balconies, connecting these ward-doors with doors made by the removal of a window on the stair-landing or a staircase, could perhaps be cheaply and strongly enough made of iron, and covered with it or other material.

An hospital, if changed on this plan, would still have its internal staircases, corridors, wards, etc., as before; but instead of having the air belonging to all of its interior fully and dangerously intermixed, it would have every ward changed into a separate and self-ventilating apartment, opening and sending its air only to the external atmosphere, and deriving its own air only from that external atmosphere. Its wards would be a series, so far, of self-ventilated cottage hospitals, each in its ventilation perfectly isolated, and separate from and independent of all the others that belonged to the institution.

In the late American and German wars, small military hospitals consisting of canvas tents, wooden cottages or barracks, and single-storeyed buildings, erected of various materials for hospital purposes, have been employed to a much greater extent than I believe they ever previously were. Subsequently to the battle of Sadowa, and after many of the palatial hospitals in various cities of Germany had been so filled with the wounded and sick that they could hold no more, the overplus was placed in various hospital grounds and elsewhere, in canvas tents, and wooden sheds or barracks erected for the purpose. The treatment of the patients placed in these small cottage barracks and tents proved so much more successful than the treatment of similar patients who were lodged in the olden, many-storeyed, palatial hospitals, that latterly, in various and extensive cities in Germany—as Berlin, Dresden, Kiel—erections of the same unsubstantial kind have been reared around many of the old hospitals, and employed, because, as proved by military medical experience, they seem to afford far more salubrious dwellings for surgical and medical patients than the olden, many-storeyed hospital mansions.

In other words, what proved so serviceable in military surgery and medicine as an hospital arrangement, has been adopted already, to a considerable extent, in civil surgery and medicine also. They have been tried also to a slighter extent in Paris. With what results?

Success of canvas, wooden, etc., cottage hospitals.

Cottage hospitals of double canvas and wood — some of the latter so formed and built as to serve in winter as well as summer — have been more largely employed in Germany than in France. When writing to me lately on the subject of these German cottage hospitals, Professor Gurlt of Berlin remarks — "In regard to the experiences made in Berlin, and elsewhere in Germany, I can assure you *positively* that the tent and barrack hospitals have proved with us more salubrious and less deadly than the large palatial hospitals. But, nevertheless, it must be added, that cases of septicæmia must occasionally occur in them as everywhere else, being dependent in part upon the severity of the lesion in itself (as we have seen by far too often during the last war, under the most favourable external circumstances). Pyæmia occurs also sometimes in them; the oftener the more the season is unfavourable and admits of no sufficient ventilation by a free access of air; and the longer also the barrack or tent is in continual use, and the more severe cases are accumulated in them. There cannot," Dr. Gurlt adds, "be any doubt that every porous material, stone, wood, canvas, etc., which may be employed for hospitals, must become infected betimes, and the rooms must be emptied therefore, *pro tempore*, and cleaned with the utmost care."

Upwards of twenty years ago, in publishing a Report of the Edinburgh Maternity Hospital, I took occasion to suggest the advantages the village or cottage system would have over our present palatial hospitals.[1]

Several years afterwards, in publicly discussing the same subject, I spoke to the questions—"To what extent are hospitals, as in general at present constituted, banes or blessings? and how can they be changed so as to convert them from the former to the latter?" And I concluded by again suggesting that, to gain sufficient room and air and isolation for the sick inmates, they should be altered "from wards into rooms, from stately mansions into simple cottages, from stone and marble palaces into wooden and brick and iron villages."[2]

"Although the establishment of hospitals is a necessity and

[1] *Edinburgh Monthly Journal of Medical Science* for November 1848, p. 328. Already quoted *ante*, p. 290. [ED.]

[2] *Transactions of the Social Science Association* for 1867, p. 115.

marks the era of an advanced civilisation, it must always be remembered that, if the crowding of healthy men has its danger, the bringing together within a confined area of many sick persons is far more perilous. The risks of contamination of the air and of impregnation of the materials of the building with morbid substances, are so greatly increased, that the greatest care is necessary that hospitals should not become pesthouses, and do more harm than good. We must always remember, indeed, that a number of sick persons are merely brought together in order that medical attendance and nursing may be more easily and perfectly performed. The risks of aggregation are encountered for this reason; otherwise it would be far better that sick persons should be separately treated, and that there should be no chance that the rapidly changing, and in many instances, putrefying substances, of one sick body, should pass into the bodies of the neighbouring patients. There is, indeed, a continual sacrifice of life from diseases caught in or aggravated by hospitals. The many advantages of hospitals more than counterbalance this sacrifice, but it should be the first object to lessen the chance of injury to the utmost. The risk of transference or aggravation of disease is least in the best ventilated hospitals. A great supply of air, by immediately diluting and rapidly carrying away the morbid substances evolved in such quantities from the bodies and excretions of the sick, reduces the risk to its minimum, and perhaps removes it altogether. But the supply of air must be enormous. We are not in a position to say how much; but I question whether even the large quantity of 4000 cubic feet per head per hour, now assigned by the best observers, will not be found to be far below the proper amount for the acute and febrile diseases."

HERMAPHRODITISM.

HERMAPHRODITISM, or Hermaphrodism;[1] *Hermaphrodisia; androgynisme, gynandrisme; hermaphroditisme*, etc., of the French; *ermaphrodismo* of the Italians; *Zwitterbildung* of the Germans, etc.

Many different definitions of hermaphroditism, and almost an equal number of different classifications of the malformations usually comprehended under it, have been proposed by the various authors, ancient and modern, who have directed their attention to this subject. Without stopping to discuss the merits or errors of these definitions and classifications, and without inquiring, as some have done, into the propriety of the word itself, we shall content ourselves with stating that, under it, as a convenient generic term, we purpose in the present article to include an account—1*st*, of some varieties of malformation in which the genital organs and general sexual configuration of one sex approach, from imperfect or abnormal development, to those of the opposite; and, 2*d*, of other varieties of malformation, in which there actually co-exist upon the body of the same individual more or fewer of the genital organs and distinctive sexual characters both of the male and female.

To separate from one another, by as strong a line as possible, the two distinct varieties of hermaphroditic malformation marked out in this definition, we shall divide hermaphroditic malformations, considered as a class, into the two orders of *Spurious* and *True;* the spurious comprehending such malformations of the genital organs of one sex as make these organs approximate in appearance and form to those of the opposite sexual type; and the order, again, of true hermaphroditism including under itself all cases in which there is an actual mixture or blending together, upon the same individual, of more or fewer of both the male and female organs.

Spurious hermaphroditism may occur either in the male or

[1] From the well-known mythological fable of the union into one, of the bodies of Hermaphroditos, the son of Ἑρμης, *Mercury*, and Ἀφροδιτη, *Venus*, and the nymph Salmacis.—See Ovid's *Metamorphoses*, lib. iv. fab. 8.

female; that is, there may be, from malformation of the external sexual organs, an appearance of hermaphroditism in persons actually of the female sex, or from a similar cause there may be an appearance of hermaphroditism in persons actually of the male sex. The differences derived from the diversity of sex in which spurious hermaphroditism occurs, and the particular varieties of malformation in each sex which may give rise to it, will serve as bases on which we shall found some further subdivisions of this order.

True hermaphroditism, as above defined, comprehends also, as will afterwards be more particularly shown, several very distinct varieties of malformation. If we conceive for a moment all the reproductive organs to be placed on a vertical plane, as we may suppose them to be, though not with strict correctness, in the human body when in the erect posture, we shall find that the principal of these varieties may be all referred to three sets of cases:—1*st*, those in which, if we draw a vertical median line through this supposed plane, the two lateral halves are seen to present organs differing in this respect, that they belong to opposite sexual types; 2*d*, others in which, if we bisect the same plane by a transverse horizontal line, there exist organs of a different sex in the upper from those present in the lower segment; or, in other words, in which the internal genital organs belong to one sex, and the external to another. In the two preceding classes of cases there is not necessarily, as we shall afterwards more fully point out, any malformation by *duplicity* in the sexual apparatus of the malformed individual; there is only one set of sexual organs present, but in some parts these organs are formed upon the male, and in others upon the female type. In the 3*d*, and remaining, set of cases, however, there is really present to a greater or less, though most generally only to a very partial, extent, a double set of sexual organs, having opposite sexual characters so that upon the same body, and usually upon the same side, or upon the same vertical line in our supposed plane, we find co-existing two or more of the analogous organs of the two sexes. In accordance with this view, we shall consider the cases of true hermaphroditic malformation under the three corresponding divisions of — 1st, *lateral;* 2d, *transverse;* and 3d, *vertical,* or more properly, *double* or *complex hermaphroditism;* and each of these genera will admit of some further convenient subdivisions. But the mode in which we propose to classify and consider the subject will probably be at once more accurately gathered from the following table, than from any more lengthened remarks upon it in the present place.

Classification of Hermaphroditic Malformations.

- Hermaphroditism
  - Spurious.
    - In the Female
      - From excessive development of the clitoris, etc.
      - From prolapsus of the uterus.
    - In the Male
      - From extroversion of the urinary bladder.
      - From adhesion of the penis to the scrotum.
      - From hypospadiac fissure of the urethra, etc.
  - True.....
    - Lateral .....
      - Testis on the right, and ovary on the left side.
      - Testis on the left, and ovary on the right side.
    - Transverse.
      - External sexual organs female, internal male.
      - External sexual organs male, internal female.
    - Vertical or Double...
      - Ovaries and an imperfert uterus, with male vesiculæ seminales, and rudiments of vasa deferentia.
      - Testicles, vasa deferentia, and vesiculæ seminales, with an imperfect female uterus and its appendages.
      - Ovaries and testicles co-existing on one or both sides, etc.

In commenting upon and illustrating the different varieties of hermaphroditism, in the particular order in which they are placed in the above table, we shall, we believe, by following that order, be able to take a graduated, and, at the same time, a correct and comprehensive view of the subject, beginning with the more simple, and ending with the more complex and complete species of hermaphroditic malformation, as seen in the primary sexual characters, or the structure of the genital parts themselves. We shall then consider at some length the curious and important physiological subject of hermaphroditism as manifested in the secondary sexual characters of the system. After having done so, we shall endeavour to show how far the diversified forms of hermaphroditic malformation can be explained upon our present knowledge of the laws of development; point out the actual anatomical and physiological degree of sexual duplicity which is liable to occur, and the numerous fallacies with which the determination of this question in individual cases is surrounded; and lastly, in conclusion, we shall offer some general observations upon the causes, etc., of this class of abnormal formations.

A.—SPURIOUS HERMAPHRODITISM IN THE FEMALE.

There are two circumstances in the conformation of the genital organs of the female, the existence of each of which has occasionally given rise to doubts and errors with regard to the true sex of the individual on whom they were found—namely, 1*st*, a preternaturally large size of the clitoris; and 2*d*, a prolapsus of the uterus; the enlarged clitoris in the one case, and the protruded uterus in the other, having been repeatedly mistaken for the male penis.

1. *Abnormal development or magnitude of the clitoris.*—In the earlier months of intra-uterine life, the clitoris of the human female is nearly, if not altogether, equal in size to the penis of the male fœtus; and at birth it is still relatively of very considerable dimensions. From that period, however, it ceases to grow in an equal ratio with the other external genital parts, so that at puberty it is, as a general law, found not to exceed six or eight lines in length. But in some exceptional instances the clitoris is observed to retain up to adult age more or less of that greater proportionate degree of development which is presented in the embryo of the third and fourth month, thus exhibiting in a persistent form the transitory type of structure belonging to the earlier stages of fœtal life. In some instances where this occurs, the resemblance of the external female to the external male parts is occasionally considerably increased by the apparent absence of the nymphæ. Osiander[1] endeavoured to show that at the third or fourth month of fœtal life the nymphæ are very imperfect, and so very small as not to be easily observed. Meckel,[2] however, has pointed out that these organs are not in reality of a small size at that time, but they are liable to escape observation from the folds of skin of which they consist, making, at the period alluded to, a perfectly continuous membrane with the prepuce of the clitoris, and forming indeed, in their origin, only one common mass with this latter body. When the ulterior changes, therefore, which these parts ought to undergo in the natural course of development in the latter stages of fœtal existence, are suspended or arrested from about the end of the third month, there may not only co-exist with the enlarged clitoris an apparent want of nymphæ, but the resemblance of the female to the male parts may be still further increased by the persistence of the original in-

[1] *Abhandlungen über die Scheidenklappe, in Denkwürdigkeiten für die Heilkunde*, Bd. ii. pp. 4-6.

[2] *Manuel d'Anat. Gén.* tom. iii. p. 666.

timate connection of the nymphæ with the prepuce and body of the clitoris, and by the consequently continuous coating of integuments, as well as the greater size and firmness of this organ.

Excessive size of the clitoris would seem to be much less common among the natives of cold and temperate than among those of warm countries. The frequency of it in the climate of Arabia may be surmised from the fact of directions having been left by Albucasis and other surgeons of that country for the amputation of the organ; an operation which Ætius and Paulus Æginetus describe as also practised among the Egyptians. According to the more modern observations of Niebuhr [1] and Sonnini,[2] circumcision would seem to be still practised upon the females of that country.

This variety of conformation of the female parts appears to have been well known to the ancient Greeks, and several of their authors have mentioned the women so constituted under the names of τριβαδες and εταιριστριαι, a class in which the celebrated poetess Sappho (*mascula Sappho*) is well known to have been included. Martial, Tertullian, and other Roman authors have noticed the same malformation (*fricatrices*, *confricatrices*), and have alluded to the depravity to which it led.[3]

[1] *Beschreibung von Arabien*, s. 77.

[2] *Voyage dans la Haute et Basse Egypte*, tom. ii. p. 37.

[3] *Mart. Epigr.* lib. i. ep. 91; see also lib. viii. ep. 66. The frequency of this crime in the ancient gentile world may be inferred from the pointed manner in which the Apostle Paul alludes to it, Romans, chap. i. 26. In Greece it was in some places forbidden by law, and in others, as in Crete, tolerated by the state. Seneca, in his 95th ep., when speaking of the depravity of the women of his own age, remarks, "non mutata fœminarum natura, sed vita est. . . Libidine vero, nec maribus quidem cedunt pati natæ. Dii illas deæque male perdant, adeo per versum commentæ genus impudicitiæ viros ineunt." *Op. Om.* Genev. 1665, p. 787. Clemens Alexandrinus, in his Pædagogus, exposes the same vice: "et contra naturam fœminæ, viros agunt (ανδριζονται) et nubunt et etenim uxores ducunt." Also Athenæus, *Deipnosoph.* lib. xiii. p. 605. Justin Martyr, in his *Second Apology*, makes a still broader accusation. This author lived in the second century, and in declaiming against the vices of that licentious age, he alleges that multitudes of boys, females, and hermaphrodites (*androgyni ambigui sexus*) "nefandi piaculi gratiâ per nationem omnem prostant." *Op. Om. Col.* 1686, p. 70. See also Marcus Antoninus, *De Seipso*, ed. Gatakeri, Cambr. 1652, lib. iii., note at the end by Gataker. On the extent, among the ancients, of the vices above alluded to, see Meiner's *Geschichte des Verfalls der Sitten und der Staatsverfassung der Roemer*, Leipzig, 1791; Neander's *Denkwürdigkeiten*, Bd. i. s. 143; Professor Tholuck's of Halle "Exposition of St. Paul's Epistle to the Romans," in the *Edinburgh Biblical Cabinet*, vol. v. p. 102, and in an Essay on the licentious vices, etc., of the Ancients, translated into Robinson's *American Biblical Repository*, vol. ii. p. 441. In the essay last referred to, Tholuck incidentally mentions (p. 422) that the Deity Mitra (Mithras of the Ancient Persians) was hermaphrodite. For

The dimensions which the clitoris occasionally presents are such as to render it, in respect of size alone, not unlike the male penis. It is not unfrequently found of two or three inches in length, but sometimes it is seen five and six inches long. Dr. Clark frequently found the organ an inch long, and thick in proportion, among the Ibbo and Mandingo women.[1]

Haller,[2] and Arnaud[3] have collected numerous instances of preternatural size of the clitoris. The former author alludes, among others, to two cases in which the organ was stated to have been seven inches in length; and to another, mentioned by Chabart, in which it was alleged to have been twelve inches—a size which we can only conceive to have been the result of disease.

When the female clitoris is increased greatly in size, it is not wonderful that it should be sometimes mistaken for the male penis —the female organ in the Mammalia naturally differing from the male only in regard to its smaller dimensions, its not being per-

our own part, we are inclined to believe that many of the idols of the heathenish mythology of Asia could be traced to the deification of various monstrosities in man and quadrupeds. (See the figures of these idols *passim* in Coleman's *Mythology of the Hindus*, Lond. 1832; and Upham's *History and Doctrine of Buddhism*, Lond. 1829.) It is perhaps not unworthy of notice that the Jewish Talmudists, taking the Hebrew noun in the Pentateuch answering to man in its individual and not in its collective sense, considered from Genesis, chap. i. 21, that our original progenitor was hermaphrodite. (See *Jus Talmud. Cod. Erwin.*, c. 2; Heidegg. *Hist. Patriarch.*, t. i. 128; C. Bauhin *de Monstrorum Naturâ*, etc., lib. i. c. 24; and Arnaud's *Mémoire*, p. 249.) It is further interesting to remark that Plato, in his Symposion, introduces Aristophanes as holding the same opinion. "The ancient nature," he observes, "of men, was not as it now is, but very different; for then he was androgynous both in form and name (ανδρογινον και ειδος και ονομα.)" Probably from the licentious purposes alluded to by Justin Martyr, or from the weak and imbecile character of hermaphrodite individuals, the word ανδρογυνος came in latter times to signify effeminate and luxurious. The ancient lexicographer Hesychius gives it this meaning; and Theodoret, in his *Therap.*, speaks of Bacchus as being licentious, effeminate, and androgynous—(γυννις ων, και θηλυδριας, και ανδρογυνος).

[1] Home's *Comp. Anat.* vol. iii. p. 317. On the peculiarities of the external genital organs in various African tribes, see a learned paper by Prof. Müller in his *Archiv für Anatomie* for 1834, Heft iv. s. 319, with ample references to the observations and opinions of Levaillant, Barrow, Peron, Lesner, Lichtenstein, Burchell, Somerville, etc. See also Otto, in his *Neue Seltene Beobachtungen zur Anatomie*, p. 135, showing the very prominent external female parts of different African tribes to consist differently—1. of enlarged nymphæ, 2. of enlarged labia, and 3. of the enlarged clitoris.

[2] *El. Phys.* tom. vii. part ii. pp. 81-2.

[3] *Dissertation sur les Hermaphrodites*, p. 372. See also Homberg, *De Excrescentiâ Clitoridis nimiâ*, Jena, 1671; Tronchin, *De Clitoride*, Lugd. 1736; and Ploucquet's *Literatura Medica*, art. "Clitoris Magna," tom. i. p. 299.

forated by the urethra, and its wanting the corpus spongiosum—a peculiarity of defect of structure that exists as the natural type of formation in the penis of male reptiles. In the human subject, the organs are composed internally of the same kind of erectile tissue, and when we descend in the animal scale, and examine their relations in the male and female of the same species, we find some still more striking analogical peculiarities of structure. Thus, in several of the Carnivora and Rodentia, as in the lioness, cat, raccoon, bear, marmot, etc., the clitoris contains a small bone like that belonging to the penis of the male of the same species ; and amongst the Monotremata and Marsupiata, the clitoris of the female, like the penis of the male, is surmounted by a bifid glans. In a species of lemur (*Loris gracilis* or *Stenops tardigradus*) the clitoris is of a very large size ; and the urethra, as first pointed out by Daubenton,[1] runs forward and opens at its anterior extremity between the branches of its glans, imitating, in this point of structure, the penis of the male among the Mammalia.

In the human subject the mere enlargement of the clitoris alone has seldom of itself given rise to errors with regard to the sex of the individual, except in young children ; but it has frequently happened that along with it other minor malformations have co-existed, so as to render the sexual distinction much more ambiguous. In women possessing this peculiarity of structure we sometimes observe, for instance, the clitoris not only resembling the penis in size, but it has an indentation at the point of the glans, imitating the orifice of the urethra ; and occasionally the glans is actually perforated to a certain extent backwards, or the body of the clitoris is drilled more or less imperfectly with a canal like that of the male urethra. In other instances the canal and orifice of the female vagina are, by an excess of development in the median line of the body, much contracted or nearly shut up, the vulva being closed by a strong membrane or hymen, and the labia cohering, so as to give the parts a near resemblance to the united or closed perineum and scrotum of the male. Further, in one or two very rare cases which have been put upon record, the ovaries and Fallopian tubes seem to have descended through the inguinal rings into the labia, thus giving an appearance of the presence of testicles ; and a fallacy seems to have occurred in some cases from the presence of roundish masses of fat in this situation, simulating more or less the same male organs.

[1] Audibert, *Histoire Nat. des Singes*, tab. ii. fig. 8.

Besides, it often happens in those women who present more or fewer of these peculiarities of conformation in the external genital parts, that the general or secondary sexual characters of the female are wanting, or developed in a slighter degree than natural, owing probably to the malformations of the external organs being often combined with some co-existing anomalies in those more important internal reproductive organs, the healthy structure and action of which at the time of puberty appear to exercise so great an influence on the development of the peculiar general conformation and moral character of the female. Thus the features are sometimes hard, the figure and gait rather masculine, the mammæ slightly developed, the voice is deep toned, and the chin and upper lip are occasionally covered with a quantity of hair. In fact, in some marked cases the whole external character approaches to that of the male, or, more properly speaking, occupies a kind of neutral ground between that of the two sexes. Some of the more striking examples of this first variety of spurious hermaphroditism in the female will sufficiently illustrate the above remarks.

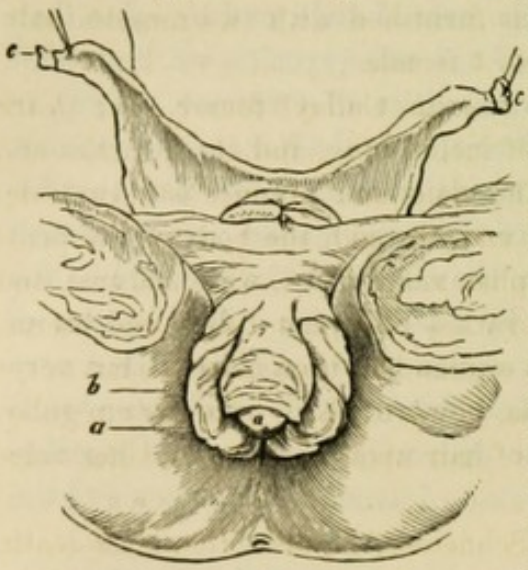


Fig. 1.

Dr. Ramsbotham[1] has briefly described the genital parts of an infant, that was christened and looked upon as a boy, until dissection after death showed that the sex was actually female. The uterus and other female organs (Fig. 1, *cc*) were present and apparently naturally formed; but the clitoris, *b*, was fully as large as, and in appearance closely resembled, the penis of a male of the same age. At its anterior extremity there was a sulcus, *a*, which was not the entrance of the urethra, but terminated in a cul-de-sac.[2]

[1] *Medical Gazette*, xiii. p. 184.

[2] In *Guy's Hospital Reports* for 1840, p. 243, there are a succinct account and excellent drawings of a case of this variety of spurious hermaphroditism. The subject was an adult. The ovaries were small; but the Fallopian tubes and uterus were otherwise normal. The vagina, about three inches long, entered inferiorly into the canal of the urethra. About an inch after this junction the urethra opened externally, as in the female; but there was no vulva. The labia externa were united together, so as to represent a scrotum, and projected on each side as

Columbus[1] and De Graaf[2] give two similar examples of the same form of spurious hermaphroditism in young children, in which the true sex was only fully ascertained by dissection after death. In relation to the clitoris in the case described by Columbus, that author states that this organ was furnished with two muscles only, and not with four, as in the perfect female.

In a reputed hermaphrodite woman, Gallay[3] found after death the clitoris to be three and a half inches long, and three inches and four lines in circumference. The glans and prepuce were well developed. The urethra ran as in man through the body of the penis and its glans. The labia, nymphæ, vagina, etc., were natural, and the internal female organs, the ovaries, Fallopian tubes, and uterus, are described as scirrhous. This woman had been married, but never had any children; her catamenia, however, had been very regular. She had a considerable quantity of hair upon her face, and her voice was harsh and masculine.

In a child, two years of age, Schneider,[4] on dissection after death, could find neither the labia externa nor interna, nor any trace of the ordinary cleft between them. The clitoris was an inch and a half long, and externally resembled most perfectly a male penis furnished with a glans and prepuce; but it was imperforate, having only at its anterior extremity a small spot marking the situation of the opening of the urethra in the male. Some lines below there was an opening by which the urine was evacuated. This opening formed the entrance to the vagina, which was found of the usual length, and with the characteristic rugæ. The canal of the urethra was found entering its roof, but in such a manner that the urine was always evacuated very slowly and by drops only, from the external opening. All the internal female sexual organs were natural.

M. Beclard[5] has left us a very detailed and interesting description of an example of spurious hermaphroditism referable to the present variety, and exhibited at Paris in 1814. The subject of the

if they contained testes; on dissection, however, these projections were found merely masses of fat. The clitoris was elongated to two inches, and proportionately increased in thickness; its glans was large. The case is described by Sir Astley Cooper. Another analogous instance with an autopsy is given by Dr. Neill in the *American Journal of Medical Science* for 1851, p. 558. All the internal organs were female; the representative penis was five inches long; the urogenital opening at its root was very small.

[1] *De Re Anatomicâ*, lib. xv. p. 493.

[2] *Op. Om.* cap. iii. xv., or *De mulierum organis gen. inserv.*, with a plate.

[3] Arnaud, *l. c.* p. 309.

[4] *Jahrbücher der Staatsarzneikunde*, 1809, s. 193.

[5] *Bulletin de la Faculté* for 1815, p. 273.

case, Marie Madeline Lefort, was at that time sixteen years of age. The proportions of the trunk and members, and of the shoulders and pelvis, and the conformation and dimensions of this last part of the body, were all masculine; the volume of the larynx also, and the tone of the voice, were those of an adolescent male; a beard was appearing on the upper lip, chin, and region of the parotids; some hairs were growing in the areola around the nipple; and the mammæ were of a moderate size. The inferior extremities were furnished with an abundance of long hard hairs. The symphysis pubis was elongated as in man; the mons veneris rounded, and the labia externa were covered with hair. The clitoris was 10½ (?) inches (27 centimetres) in length when at rest, but somewhat more when erect; its glans was imperforate, and covered, in three-fourths of its circumference, with a mobile prepuce. The body of this enlarged clitoris was furnished inferiorly with an imperfect canal, which produced a depression in it, instead of that prominence of this part which exists in the male penis. This canal was pierced along its under surface and median line by five small holes capable of admitting a small stylet; and one or more similar apertures seemed to exist in it after it passed backwards within the vagina. The labia were narrow and short, and the vulva or sulcus between them was superficial, being blocked up by a dense membrane, which, under the pressure of the finger, felt as if stretched towards the anus over a cavity. At its anterior part, or below the clitoris, there was an opening capable of admitting a sound of moderate size, and this sound could be made to pass backwards behind the membrane closing the vulva, which, when felt between the point of the instrument and the finger, seemed about twice as thick as the skin. The urine was passed by this opening, and also, according to the report of the individual herself, through the cribriform holes in the canal extending along the inferior surface of the urethra. By the same opening the menstrual fluid escaped, as Beclard ascertained on one occasion by personal examination. She had menstruated regularly from the age of eight years, considered herself a female, and preferred the society of men.

In this interesting case, we have present all the secondary sexual characters of the male, with some of the female genital organs developed in so excessive a degree as to approach in several points their more perfect structure in man. The impossibility, however, as mentioned by Beclard, of finding any bodies like testicles in the labia or in the course of the inguinal canals, and more particularly the

well-ascertained fact of the individual menstruating, can leave no doubt as to the nature of her sex. The perforation of the enlarged clitoris by the imperfect urethra is interesting, when compared with the peculiarities that we have formerly alluded to, of this part in the female Loris, as pointing out —what we have so often occasion to observe in human monstrosities —a type of structure assumed by a malformed organ similar to the normal type of structure of the same organ in some of the inferior animals.

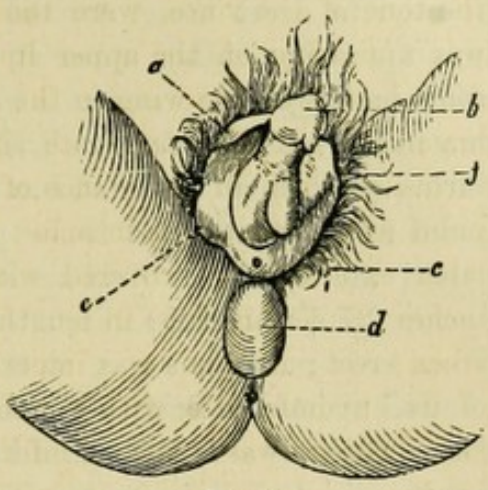


Fig. 2.

Arnaud[1] has represented and described at great length an interesting example of hermaphroditic malformation that seems referable to the head of spurious hermaphroditism in the female, although there are two circumstances in the history of the case which have led some authors to doubt the accuracy of this opinion; and the opportunity that was afforded of ascertaining the true structure of the parts after death was unfortunately lost through carelessness and neglect. The subject of the malformation, aged 35, passed in society for a female, and came to Arnaud complaining of a small tumour (Fig. 2, *e*) in the right groin, which had much incommoded her during her whole life. On examining this body, Arnaud was led to believe that it was a testicle, and he found a similar tumour, *f*, situated nearer the inguinal ring on the left side. The bags that contained them represented very exactly the labia externa. The clitoris, *a*, was two inches and nine lines in length, and placed between the labia at their upper angle. The glans, *b*, was well formed, and, though imperforate at its extremity, it presented a small depression which ran backwards along the whole inferior border of the clitoris, indicating the situation of a collapsed urethral canal, that seemed pervious for some length at its posterior part, as it became distended when the patient evacuated the bladder. The orifice, *c*, however, from which the urine actually flowed, occupied the situation in which it exists in the perfectly formed female. There was not any vaginal opening, and the individual menstruated per anum. At each menstrual period a tumour, *d*, always appeared in the perineum, which gradually increased in size, becoming in the course of three or four days as

[1] *Dissertation sur les Hermaphrodites*, p. 265, pl. x.

large as a small hen's egg. When the perineal tumour had reached this size, blood began to flow from the anus, although no hemorrhoids or other disease of the bowel was present. At these periods the individual had often experienced very alarming symptoms, and in order to avert these, Arnaud was induced to make an opening into the soft yielding space at which the perineal tumour above alluded to appeared; and at a considerable depth he found a cavity two inches in circumference, and about two and a half in breadth, having projecting into it at one point an eminence which was supposed from its situation to be possibly the os uteri. At the next period the menstrual fluid came entirely by the artificial perineal opening, and the usual severe attendant symptoms did not supervene. From inattention, however, to the use of the tent, the opening was allowed to become completely shut, so that at the sixth return of the menses they flowed again by the anus, and were accompanied by the old train of severe symptoms. The individual lived for several years afterwards. Her conformation of body was remarkable. Her skin was rough, thick, and swarthy; she had a soft black beard on her face; her voice was course and masculine; her chest narrow; her mammæ were flat and small; her arms lean and muscular; her hands large, and her fingers of very considerable length and strength. The form, in fact, of the upper part of her body was masculine, but in the lower part the female conformation predominated. The pelvis was wide and large, the os pubis very elevated, the buttocks large, the thighs and legs round, and the feet small.

In this remarkable instance, if we do not go so far as to conceive the co-existence of some of the internal organs of both sexes, we must, from the well-ascertained fact of the menstrual evacuations, allow the person at least to have been a female. In that case we can only suppose the tumours in the labia to be the ovaries descended in that situation; and to the same excess of development which had produced this effect, we may attribute the closure of the vaginal orifice, and the formation of the imperfect urethral canal in the body of the clitoris.

Spurious hermaphroditism from preternatural enlargement of the clitoris has been recognised among some of the lower animals. Rudolphi [1] has noticed a mare of this kind that had a clitoris so

[1] *Bemerkungen auf einer Reise*, etc., Bd. i. s. 79. See also a case figured by Ruysch in his *Thesaurus Anat.* lib. viii. No. 53.

large as almost to shut up the entrance into the vagina. Lecoq[1] has detailed the case of a calf which Gurlt[2] believes to belong to the present head. Neither testicles nor scrotum were observed externally, and the penis or enlarged clitoris, which occupied its normal situation, was apparently perforated by the urethra, and crooked upwards so as to throw the urine in that direction. Mery[3] showed by dissection the true sex of a monkey, the length of whose clitoris had deceived some observers with regard to the true sex of the animal. The enlarged clitoris was furrowed on its inferior surface. The clitoris of the female Quadrumana is, as will afterwards be more particularly mentioned, relatively larger than in the human subject, and retains in a greater degree the size and type of structure of this organ in the embryo.

We may here further mention that, as pointed out by Blumenbach,[4] the clitoris and orifice of the urethra are placed at some distance from the vagina and in front of it, in the rat, mouse, hamster, etc. This normal structure has sometimes been mistaken for a hermaphroditic malformation.

2. *Prolapsus of the Uterus.*—It may at first appear strange that this occurrence should ever lead to any difficulty in ascertaining the sex of the individual, though not only non-professional observers, but even the most intelligent medical men, have occasionally been so far misled by the similarity of the protruded organ to the male penis, as to mistake a female for a male. Of this circumstance some curious illustrations are on record.[5]

M. Veay, physician at Toulouse, has inserted in the *Philosophical Transactions* of London,[6] a brief account of the case of Marguerite Malause or Malaure, who was entered as a female patient in the Toulouse Hospital in 1686. Her trunk, face, etc., presented the general configuration of a female, but in the situation of the vulva there was a body eight inches in length when on its fullest stretch, and resembling a perfectly formed male penis in all respects, except in not being provided with a prepuce. Through the canal perforating this body she was alleged to evacuate her urine, and from its orifice M. Veay had himself an opportunity of seeing the menstrual fluid flow. After being examined by several physicians, she was

[1] *Journ. Prat. de Méd. Vét.* 1827, p. 103.
[2] *Lehrbuch der. Pathol. Anat.* Bd. ii. s. 193.
[3] *Hist. de l'Acad.* 1686, tom. i. p. 345. [4] *Comp. Anat.* p. 335.
[5] Doebel, in *Nov. Liter. Maris Balthici*, 1698, p. 238.
[6] *Phil. Trans.* vol. xvi. p. 282.

pronounced to be more male than female, and ordered by the civil authorities to exchange the name of Marguerite for that of Arnaud, and to wear male attire. In 1693 she visited Paris in her male habiliments, and reputed herself endowed with the powers of both sexes. The Parisian physicians and surgeons who examined her seem all to have accorded in opinion with the faculty of Toulouse, until M. Saviard[1] saw her, and detected the supposed penis to be merely the prolapsed uterus. He reduced the protruded organ, and cured the patient. Upon the enigma of her hermaphroditism being thus solved, she was permitted by the king, at her own request, to assume again her female name and dress.

Sir E. Home[2] detected a case of reputed hermaphroditism, of the same description as the last, in a French woman, twenty-five years of age, who exhibited herself in London, and pretended to have the powers of a male. The cervix uteri was uncommonly narrow, and projected several inches beyond the external opening of the vagina. The everted mucous surface of the vagina had, from constant exposure, lost its natural appearance, and resembled the external skin of the penis. The orifice of the os tincæ had been mistaken for the orifice of the urethra. The prolapsus had been observed at an early age, and had increased as the woman grew up.

Valentin[3] mentions another analogous instance of sexual ambiguity produced by a prolapsus of the uterus. In this case the husband mistook the displaced organ for the penis, and accused his wife of having "cum sexu virili necquicquam commune."

A case quoted at great length by Arnaud[4] from Duval, of reputed hermaphroditism in a person that was brought up as a woman, and married at twenty-one years of age as a male, but who was shortly afterwards divorced and imprisoned, and ordered again by the Court of Rouen to assume the dress of a woman, appears to us to belong very probably to the present division of our subject, the reputed penis being described as placed *within* the vagina. The recorded details of the case, however, are not so precise as to leave us without doubt in regard to its real nature.

In cases such as those now mentioned, in which the prolapsed uterus, or, more properly speaking, the prolapsed uterus and vagina, have been mistaken for the penis, it appears probable that

[1] *Recueil d'Observations Chirurgicales*, p. 150. [2] *Comp. Anat.* vol. iii. p. 318.
[3] *Pandectæ Medico-Legales*, tom. i. p. 38, Casus xii.
[4] *Mém. sur les Hermaphr.* pp. 314-18.

the neck of the uterus must have been preternaturally long and narrow, otherwise it would be difficult to account for the apparent small diameter and great length of the prolapsed organ. In Professor Thomson's collection of drawings of diseased anatomical structures, there is one of a uterus containing in its body a fibro-calcareous tumour, and having a neck three inches in length. M. Cruveilhier[1] has represented a similarly diseased uterus with a neck of between five and six inches. An organ shaped in this manner, whether from congenital malformation or acquired disease, would, when prolapsed for some time, represent, we conceive, a body resembling in form and size those observed in Saviard's and Home's cases.[2] The prolapsus arising from the protrusion of an ordinarily shaped uterus is generally of a greater diameter and roundness.

This second species of spurious female hermaphroditism is not observed among the lower animals.

B. Spurious Hermaphroditism in the Male.

Malformed males have more often been mistaken for females than the reverse. The varieties of malformation in persons actually male, that are liable to lead to mistakes with regard to their true sex, appear to be—1st, Extrophy or extroversion of the urinary bladder; 2d, Adhesion of the inferior surface of the penis to the scrotum; and 3d, and principally, Fissure of the inferior part of the urethra and of the scrotum and perineum.

1. *Extroversion of the urinary bladder.*—For a full description of this malformation, we must refer elsewhere.[3] This malformation is known to occur more frequently in the male than in the female, and when present in the former, it has occasionally given rise to a supposition of hermaphroditism, the red fungous mass formed by the mucous membrane of the protruded posterior wall of the bladder and situated above the pubis, having been mistaken for the female vulva. This error has probably been the more readily committed, from the uterus and seminal ducts, and sometimes also—as in an instance described by A. Fraenkel[4]—a part of the intestinal canal,

[1] *Anat. Pathol.* liv. xiii. pl. iv.

[2] Some years ago there was a case of longitudinal hypertrophy of the cervix uteri in the Edinburgh Infirmary, where the prolapsed and elongated cervix uteri very exactly resembled a male penis.

[3] *Cyclop. of Anat. and Phys.*, articles "Bladder, and Monstrosity."

[4] *De Organorum Generationis Deform., rarissimâ*, Berlin, 1825, with a plate.

opening upon the surface of the exposed portion of bladder. In some instances of this malformation occurring in man, the external male sexual organs are very imperfectly formed, or can scarcely be said to be at all present. In other cases the scrotum is of the natural form, with the two testicles in it; and the penis is of considerable size, though almost always fissured on its upper surface from the epispadiac or open state of the urethra.

An example of supposed hermaphroditic malformation, briefly described by Rueffe,[1] which seems referable to this variety, will be sufficient to illustrate it. "In the year 1519, a hermaphrodite or androgynus," he remarks, "was born at Zurich, perfectly formed from the umbilicus upwards, but having at this part a red mass of flesh, beneath which were the female genitals, and also under and in their normal situation those of the male."

2. *Adhesion of the inferior surface of the penis to the scrotum by a band of integument.*—This state of the parts has occasionally given rise to the idea of hermaphroditism, the penis being so bound down as not to admit of erection, and the urine passing in a direction downwards, so as to imitate the flow of it in the female.

In a boy, seven years of age, regarding whom Brand[2] was consulted, the penis was confined in this manner to the scrotum by abnormal adhesions. He had been baptized and reared as a girl, but by a slight incision the adherent organ was liberated, and the parents were convinced of the mistake that they had committed in regard to the sex of their child. The difficulty of determining the true sex of the boy was increased by the testicles not having descended into the scrotum.

Wrisberg[3] mentions two similar instances, in persons of the respective ages of nineteen and forty-six. He relieved the adherent penis in the first case by operation.

3. *Fissure of the inferior part of the urethra, perineum, etc.*—HYPOSPADIAS.—This species of malformation, which has, perhaps, more frequently than any other given rise to the idea of the person affected with it being the subject of hermaphroditism, evidently consists in an arrest of the development of the external male sexual parts.

At an early stage of the development of the embryo, the various

[1] *De Conceptu et Generatione Hominis*, p. 44.

[2] Case of a boy who had been mistaken for a girl: London, 1788.

[3] *Comment. Med.*, etc., p. 534.

central sexual organs are, like all the other single organs situated in the median line of the body, found to be composed of two separate and similar halves, divided from each other by a vertical fissure, which, after the originally blind extremity of the intestinal canal has opened upon the perineum, forms a common aperture or cloaca for the intestinal canal, and also for the urinary and genital apparatus, both of which are, in their primary origin, prolongations from the lower part of that canal. After a time (about the second month in the human embryo), the opposite sides of this cloaca gradually approximate, and throw out two corresponding folds, which by their union constitute a septum that separates the rectum from the canal, or portion of the fissure, that still remains common to the urinary and generative organs; and, in the same way, by two similar and more anterior folds, the urethra of the female, and the pelvic portion of that of the male, is subsequently produced. After this, in the female the process of median reunion does not proceed farther, and the primary perineal fissure remains, forming the vulva and vagina. In the male, however, the development, when normal, goes on to a greater extent, and the sides of the opening become so far united as ultimately to leave only the comparatively contracted canal of the urethra to serve as a common passage for both the internal urinary and genital organs; and the situation of the line of junction of the opposite sides of the original perineal cleft remains still marked out in the adult, by the raphé existing in the median line of the scrotum. The two lateral parts of the female clitoris unite together into one solid body, having on its under surface a slight groove or channel, indicative of the line of conjunction of its two component parts; and the urethra is left to open at the root of this imperforated organ. In the male, on the contrary, the two primitive halves of the penis, consolidated together at an early stage along the course of their upper surfaces, come, about the third month of development, to unite inferiorly in such a manner with one another as to form a tubular prolongation of the pelvic portion of the canal of the urethra, which is gradually extended forwards, first along the body of the penis, and ultimately through its glans. In the earlier periods of evolution, the præputium is still wanting; but as the penis becomes perforated, the præputium grows so rapidly, as at last to cover and inclose the glans.

Many of the malformations to which the male genital organs are liable may be traced to stoppages in the above process of development, the character of the malformation depending upon the period

of the development at which the arrest takes place, and varying consequently in degree from the existence of a cloaca or permanent primitive fissure common to the intestinal, urinary, and generative organs,[1] to that want of closure, to a greater or less extent in different instances, of the inferior surface of the canal of the urethra in the body of the penis, or in its glans, which is generally known under the name of *Hypospadias*. When the development of the male organs is arrested, immediately after the two septa respectively separating the canals of the intestine and urethra from the original perineal cleft are formed, and consequently when this perineal fissure and that running along the inferior surface of the penis are still open, the external genital parts often come to present at birth, and during the continuance of life, a striking resemblance to the conformation of the external organs of the female, and the resemblance is frequently rendered greater by the co-existence of other malformations of the male organs. In these cases the imperfect and undeveloped penis is generally of small size, and, at the same time, from being imperforate, may readily be mistaken for the clitoris; the two halves of the divided scrotum have the appearance of the two labia externa; the two labia interna or nymphæ are sometimes represented by the lateral divisions of the penis forming two folds, which run backwards along the internal surfaces of the split scrotum; and the cleft in the perineum corresponds in situation and direction, and occasionally also in size and form, with the canal of the vagina; this cleft is generally lined also by a red mucous membrane, that is kept, like the natural female parts, constantly moistened by the secretions of the follicles with which it is provided; its mucous membrane occasionally presents irregular elevations, imperfectly representing the carunculæ myrtiformes; and, further, the opening of the urethra at the root of the diminutive and imperforate penis serves still more to assimilate the malformed parts to the natural conformation of the female organs. In a number of cases, however, the apparent analogy to the female parts is rendered less striking by the perineal cleft being small or altogether absent, the urethral orifice at the root of the penis often forming the only opening leading to the internal urinary and generative parts, and the halves of the scrotum in such instances being frequently more or less united. Generally, the seminal ducts, and sometimes also the ducts of Cow-

[1] See on this malformation in the human subject (the normal form of structure in birds, etc.), Meckel on Kloakbildung in his *Path. Anat.* Bd. i. s. 693.

per's glands, are seen opening on the surface of the urethra or supposed vaginal canal, at a short distance from its external orifice.[1]

In males malformed in the manner described, the testicles are seldom found in the divided scrotum at birth, but commonly they descend into it through the inguinal rings towards the period of puberty; and in several instances on record, in which the sex of the individual had been mistaken for that of a female, the tumours formed in the groin at that time by the organs in their descent, have been erroneously regarded and treated as hernial protrusions. At the same time it occasionally happens that with the descent of the testicles, and the arrival of puberty, the diminutive penis enlarges in size, and the individual assumes more or less fully the habits and attributes of the male. In several instances on record this change has, under venereal excitation, appeared to occur suddenly, and persons formerly reputed female have thus unexpectedly found themselves provided with an erectile male penis. These various changes are occasionally postponed for a considerable period beyond the usual term of puberty.

In a few rare instances one testicle only descends through the inguinal ring, and occasionally they both remain throughout life within the abdomen, in or near the situation in which they were originally developed, imitating in this abnormal state the normal position of the same organs in many of the males among the lower animals. In a number of instances in which the testicles are thus retained within the cavity of the abdomen, they are found small and imperfectly developed, and, from the want of their usual physiological influence upon the constitution, the whole physical and moral character of the malformed individual frequently presents a considerable approximation to that of the female, or, as we should perhaps more justly express it, never attains the perfection of the male, but preserves that kind of common or neutral state exhibited by the constitution of both sexes before the specific sexual characters of each are developed at the time of puberty.

Numerous curious examples of mistakes having been committed with regard to the sex of males affected with the above species of malformation have now been put on record, from the time at which Iphis, the daughter of Ligdus, King of Crete, was conceived to be changed into a man by the miraculous interference of Isis, down to

[1] In the well-known case of Gottlieb Göttlich, Mulder used a small *vaginal speculum* to see these orifices, and obtained seminal fluid, as shown by the microscope, from the orifices of the vasa deferentia.

the present day. Pliny[1] has noticed several cases; and in the treatise of Duval on hermaphrodites, a number of additional instances are collected from Livy, Trallian, and others, some of them no doubt invested (as most of the details regarding hermaphrodites in the older authors are) with much misrepresentation and fable, but others bearing every mark of accuracy and authenticity. In more modern times the sexes of individuals have often been mistaken in consequence of this variety of malformation. Jean Chroker[2] relates, in apparently the most authentic manner, the case of Magdelain Mugnoz, a nun of the order of St. Dominique, in the town of Ubeda, who was changed, as he supposes, into a male, seven years after having taken the vows. He was expelled the convent, assumed the male dress, and took the name of François. The sequel of the story, as told by Chroker, would seem to show that his sexual desires became extremely strong, and he is said to have been ultimately condemned, whether justly or not, under an accusation of rape.[3]

Portal[4] quotes from Tigeon the story of a person who was brought up as a female, and afterwards was considered to be suddenly changed by a surprising metamorphosis into a male; and in citing this case, Dr. Hodgkin[5] of London mentions, on the authority of a friend, a recent instance of an equally sudden development of the male sex in a previously reputed female. Similar instances, in which the proper sex of malformed males was unexpectedly discovered under the excitement of sexual passion at the period of puberty, are mentioned by Paré, Tulpius, and others.

Schweikard[6] has recorded an instance of a person baptized and brought up as a female, and whose true sex was only at last disclosed by his requesting, at the age of forty-nine, permission to marry a young woman then pregnant by him. On examination, it was discovered that the penis was slender, and scarcely two inches long; the right testicle only had descended into the scrotum, and the

[1] Lib. vii. chap. iv.

[2] *Fax. Histor.* cent. i.; and Arnaud, *Dissertation sur les Hermaphrodites*, p. 200.

[3] After this essay was first published in 1839, I received, through Dr. Cowan, a long account and drawings of a similar case, where a child taken into a convent in Malta as a female turned out at puberty to be an amorous hypospadiac male; and subsequently became a sailor instead of a nun. I have in practice been consulted in three cases where hypospadiac male children have been baptized as girls.

[4] *Hist. de l'Anat.* tom. ii. p. 52.

[5] *Catalogue of Guy's Hospital Museum*, part ii. sect. 11.

[6] Hufeland's *Journal der Prak. Heilkunde*, Bd. xvii. No. 18.

urethra opened at the root of the penis, but its orifice was placed in such a manner that during micturition the urine was thrown along the groove or channel on the under surface of the penis, so as to appear to issue from its anterior extremity. The two halves of the scrotum were so far united that they left only a small oval opening between the anterior part of the raphé and the roots of the corpora cavernosa. In this opening, the orifice of the urethra was situated.

Dr. Baillie[1] has mentioned a case which appears to belong in all probability to the present division. The subject of it was twenty-four years of age. She had always passed in society as a woman, and came for consultation to the Nottingham Hospital on account of her menses never having appeared; a circumstance, however, that had in no way affected her health. The spurious vagina consisted of a cul-de-sac two inches in depth. The penis was of the size of the female clitoris, but there were no nymphæ. The labia were more pendulous than usual, and each of them contained a body resembling a testicle of a moderate size, with its cord. The look of the individual was remarkably masculine, with plain features, but no beard. The mammæ resembled those of a woman. The person had no desire or partiality for either sex.

Adelaide Preville, who had been married as a female, died in the Hôtel Dieu of Paris. In examining the body of this individual after death, Giraud[2] found that, except a perineal cleft or false vagina consisting of a cul-de-sac placed between the bladder and rectum, nothing else resembling the female sexual apparatus could be detected, while all the organs belonging to the male sex were present.

Otto[3] has described and represented (Fig. 3), a case of the present species of hermaphroditism, in an individual whose history is remarkable. The person had lived ten years in the state of wedlock with three different men; but at the age of thirty-five, an action of divorce was brought against her by her third husband, accusing her of being affected with some disease of the sexual parts that rendered the connubial act on his part extremely difficult and painful. After some difference of opinion between the two medical men to whose professional examination the wife was submitted, it

[1] *Morbid Anatomy*, 2d edition, p. 410.

[2] *Recueil Period. de la Soc. de Méd.* tom. ii. p. 315, or Moureau's *Hist. Nat. de la Femme*, tom. i. p. 243 (with a figure of the parts).

[3] *Neue Seltene Beobachtungen zur Anatomie*, etc., p. 123.

was at last considered that she was in reality a male ; and the case finally came under the investigation of the members of the Royal Medical College of Silesia, who confirmed this opinion. The imperforated penis, *b*, was one inch and a half in length ; the perineal fissure, *e*, forming the false vagina, was at the posterior part of its orifice, bounded by a distinct frænulum, but was of a size sufficient to receive the glans of the husband for an inch and a half in depth. This cavity, as well as the internal surfaces of the two lobes, *a a*, of the divided scrotum, were lined with a vascular mucous membrane. At the bottom of it, the round orifice of the urethra, *d*, was seen to open ; and at the same point a hard mass could be felt, probably consisting of the prostate gland ; and more upwards and outwards, nearly in the natural situation of the bulb, was seen the split urethra, *c*, with a row of three considerably-sized openings, *f f*, which, under pressure and irritation of the genital parts, gave out several drops of a transparent mucous fluid. Otto considers these openings as the extremities of the ducts of the prostate and Cowper's glands, and of the seminal canals. The right half of the scrotum contained a small testicle about the size of that of a boy ten years of age ; the left testicle lay likewise external to the abdominal ring, and was still softer and smaller than the right. Both were furnished with spermatic cords. The general configuration of the individual was strong, muscular, and meagre ; the beard was thin and soft, and the face, mammæ, thorax, pelvis, and extremities, were evidently masculine.

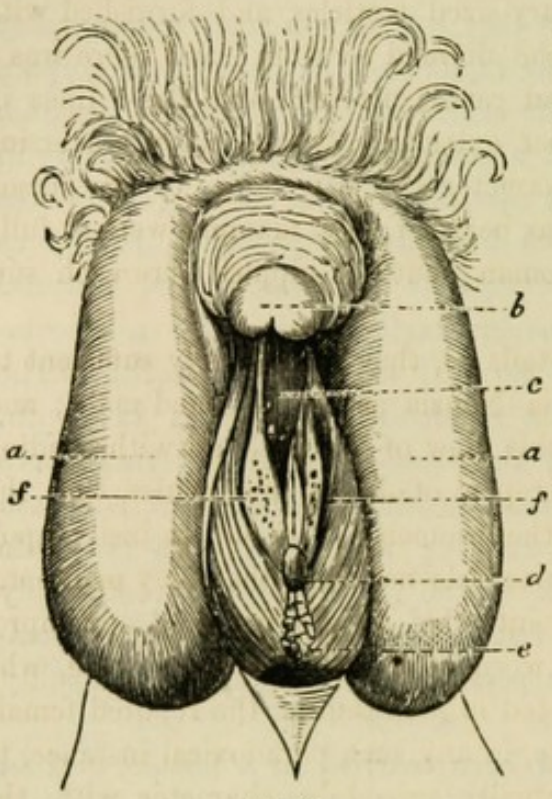


Fig. 3.

Fig. 3. Hypospadiac male.

Along with the preceding instances we are inclined to classify the case of Maria Nonzia, as detailed by Julien and Soules.[1] This

[1] *Observ. sur l'Hist. Nat. sur la Physique et sur la Peinture*, tom. i., p. 18, with a plate.

individual was born in Corsica in 1695, was twice married as a female, and at last divorced in 1739 by her second husband, after having lived sixteen years in wedlock. The penis was two inches in length, but imperforate, and the meatus urinarius was placed at its root. Two bodies like ordinary-sized testicles, and furnished with spermatic cords, were felt in the divided scrotum; and there was a narrow false vagina or perineal canal, one inch and three lines in depth, and crossed at its upper extremity by two small traversing membranous bridles. The character and appearance of the person were masculine; the visage was bearded; the mammæ were as fully developed as in the adult woman; but the nipples were each surrounded with hair.

So far as the preceding details go, they seem amply sufficient to justify us in considering Maria Nonzia as a malformed male; and we are still inclined to take this view of the case, notwithstanding the statement inserted in the report of Julien and Soules, that the menses were present as in other women. For, not to insist upon the circumstance that the reporters do not show that they made any minute or satisfactory inquiry into this alleged fact, and not improbably took it upon the mere word of the subject of the case, who was necessarily greatly interested in maintaining the reputed female character, it would be requisite, in any such paradoxical instance, to ascertain if the discharge actually agreed in character with the menstrual fluid, or was not pure blood, the result of a hemorrhage from the genito-urinary passages, or from the rectum, where, as in other parts of the body, this form of disease frequently assumes a periodical type. We would be inclined to apply even still more strongly these remarks to the celebrated case of Hannah Wild, detailed by Dr. Sampson.[1] This person had evidently the male genital organs malformed in the manner mentioned with regard to the other cases included under the present section, and possessed all the secondary sexual peculiarities of the male; so that we can only receive with great doubt and distrust the alleged existence of the menstrual discharge, and the more so, as this is evidently stated on the report of the subject of the case alone, who, deriving a precarious subsistence from the exhibition of his malformations, had a deep interest in amplifying every circumstance that could enhance the public curiosity with respect to the reality of his hermaphroditic character.

At the same time, however, it must be remarked, that in some

[1] *Ephem. Nat. Curios.* Dec. i. an. iii. p. 323.

instances of spurious hermaphroditism, it is found extremely difficult or even impossible during life to determine with precision the true or predominant sex of the malformed individual; and in regard to several well-known cases on record, we find on this point the most discrepant opinions offered by different authors. Thus, while Morand,[1] Arnaud,[2] and Delius,[3] described Michel-Anne Drouart as a male; Guyot,[4] Ferrein,[5] and Caldani[6] maintained that this person was a female; and Mertrud[7] regarded the individual as an example of a real hermaphrodite.

A useful lesson of caution to us against forming too decided and dogmatic an opinion in cases in which the sexual conformation appears in any marked degree doubtful, has lately been offered in the instance of Maria-Dorothée Duriée, or, as this individual was named in the latter years of his life, Charles Durge. While Metzger[8] considered this person as a specimen of that kind of equivocal sexual formation to which the designation of hermaphroditism is truly applicable, Hufeland,[9] Mursinna,[10] Gall, Brookes,[11] and others,[12] declared the sex of Duriée to be in reality female; and Stark,[13] Mertens,[14] and the Members of the Faculty of Medicine at Paris,[15] were equally positive in regarding the individual as merely a malformed male. The dissection of the body of Duriée by Professor Mayer has, as we shall afterwards state more in detail, shown the sexual conformation of this individual to consist of a mixture of both the male and female organs.

In attempting to determine the true sex in such doubtful instances of sexual formation as those we have now been considering, we are inclined to attribute very little weight to the nature of the sexual desires of the malformed individual, as we have already found Adelaide Preville, the dissection of whose body showed him to be in reality a man, living for some years before death in the capacity of a wife, and the same remark might be further illustrated by a reference to Otto's and other cases.

[1] *Mém. de l'Acad. des Sc.* 1750, p. 165.
[2] *Dissert. sur les Hermaphr.* p. 298. [3] *Frank. Sammlung*, Th. viii. S. 398.
[4] *Mém. de l'Acad. des Sc.* 1756, p. 71. [5] *Ibid.*, 1767, p. 205.
[6] *Mem. della Societa Italiana*, tom. vii. p. 130. [7] Arnaud, *loc. cit.* p. 298.
[8] *Gericht.-medic. Abhandlungen*, Bd. i. S. 177.
[9] *Journ. der Praktischen Heilkunde*, Bd. xii. S. 170.
[10] *Journ. für die Chirurgie Arzneikunde*, etc., Bd. i. S. 555.
[11] *Medical Gazette* for October 1836.
[12] *Von dem Neuangekommen Hermaphrod.* Berl. 1801.
[13] *Neuen Archiv für die Geburtshülfe*, Bd. ii. S. 538.
[14] *Beschreibung der männlichen Geschlechtstheile* von M. D. Durrier: Leipzig, 1802, with two plates. [15] *Med. Gaz.* for October 1836.

A species of spurious hermaphroditism, similar in character to that which we have just described in man, is occasionally met with in the males of our domestic quadrupeds, and has been amply illustrated, as it occurs in these animals, by Professor Gurlt in his work on *Veterinary Medicine*. In instances of this malformation among the animals to which we refer, the hypospadiac male penis has usually been found of a tortuous and winding form, and of small size. In the cases in which the fissure of the parts extends through the scrotum, a false vagina is seldom formed, as in man, for the scrotum in most quadrupeds lies too remote from the perineum, and consequently from the normal situation of the vagina, for this purpose; but in some examples this division appears to be carried upwards into the perineum itself, leaving a vaginal-like opening, in which the urethra terminates. The testicles, as in man, are sometimes retained within the abdomen, and in other instances descend into the scrotum. They are frequently small in size. The mamma or udder seems to be often well developed.

This variety of hermaphroditic malformation has been met with in the horse by Penchenati;[1] in the he-goat by Haller;[2] and in the ram by the same author,[3] and by Wagner,[4] Wepfer,[5] Stark,[6] Gurlt,[7] Kauw Boerhaave,[8] and A. Cooper.[9] We have seen an excellent specimen of this malformation in the last-mentioned animal, in the museum of Dr. Handyside of Edinburgh. In this instance the internal male organs are all perfect; the large testicles are situated in the halves of the split scrotum; the penis is small and imperforate, and a furrow running along its inferior surface is continued backwards and upwards along the perineum to within a short distance from the anus, where it leads into a canal, into which the urinary bladder and seminal ducts open. This canal is evidently formed of the dilated pelvic portion of the male urethra; its orifice is comparatively contracted, but corresponds in situation with the vulva of the female. We have seen a second similar case in a ram, in the possession of Professor Dick of the Veterinary School of Edinburgh.

There is another variety of malformation of the male parts occa-

[1] *Mém. de l'Acad. de Turin*, tom. v. p. 18.
[2] *Comment. Soc. Reg. Sc. Gotting.* tom. i. p. 2, tab. i.
[3] *Ibid.*, p. 5, tab. ii. [4] *Ephem. Nat. Curios.* Cent. i. ii. p. 235.
[5] *Miscell. Nat. Curios.* Dec. i. ann. iii., 1672, p. 255.
[6] *Ibid.*, Dec. iii. ann. v. vi. p. 669. [7] *Lehrbuch*, p. 193.
[8] *Nov. Comment. Acad. Petropolit.* tom. i., 1750, p. 315, tab. xi.
[9] *Catalogue of Guy's Hospital Museum*, No. 2546.

sionally found in quadrupeds, which is allied in its nature to the preceding. In this second species, all the external male sexual organs are small; the short penis lies, when not in a state of erection, upon the posterior surface of the enlarged udder, and the imperfectly developed testicles are generally retained within the abdomen; or, if they have passed out of that cavity, they are found situated in the substance of the udder. The vasa deferentia, prostate, and Cowper's glands, are usually of their normal size and appearance. This imperfect hermaphroditic formation appears to be not rare among horses, several instances of it in this animal having been now described by Arnaud,[1] Gohier,[2] Volmar,[3] Pallas,[4] Virey,[5] and Gurlt.[6] Anselmo[7] and Lecoq[8] have met with this variety of malformation in the bull; and Sandford[9] has described an instance in the calf, which seems referable to the same head. Gurlt[10] also notices the preparation of an analogous case in the calf, as preserved in the museum at Berlin.

TRUE HERMAPHRODITISM.

True hermaphroditism exists as the normal type of sexual conformation in several classes of the vegetable and animal kingdom. Almost all phanerogamic plants, with the exception of those included under the class Diœcia, are furnished with both male and female reproductive organs, placed either upon the same flower, or, as in the Linnæan class Monœcia, upon different flowers in the same individual. In the class Polygamia various exceptional genera are included, which present indiscriminately upon the same individual, or upon different individuals of the same species, male, female, and hermaphrodite flowers, and which thus form a kind of connecting link between the general hermaphroditic form of phanerogamic vegetables, and the unisexual type of the monœcious flowers and the diœcious plants.

From anormalities in development, these normal conditions of

[1] *Sur les Hermaphrodites*, p. 282.
[2] *Mém. et Observ. sur la Chir. et la Méd. Vet.* tom. i. p. 18.
[3] *Archiv für Thierheilkunde*, Bd. iii. S. 292.
[4] *Beschaft. der Gesellschaft naturforch. Freunde zu Berlin*, Bd. iii. S. 296.
[5] *Journal Compl. des Sc. Méd.* tom. xv. p. 140.
[6] *Lehrbuch der Path. Anat.*, Bd. ii. p. 189; and tab. viii. fig. 6.
[7] *Mém. de l'Acad. des Sc. de Turin*, tom. ix. p. 103, figs. 1-3.
[8] *Journ. Prat. de Méd. Vet.* 1827, p. 102.
[9] *Med. and Phys. Journal*, vol. ii. p. 305, with two drawings.
[10] *Loc. cit.* p. 191.

the sexual type in the different members of the vegetable kingdom are occasionally observed to be changed. Thus, among the Dicœcia, individual plants are sometimes, in consequence of a true malformation, observed to assume a hermaphroditic type of structure; or, on the other hand, in hermaphroditic plants more or fewer flowers are occasionally found unisexual, in consequence of the arrested development of one order of their sexual organs; and again, though still more rarely, from an excess of evolution, a double set of male parts, or a double set of stamens, is seen developed on some of the individual flowers.

In the animal kingdom we find instances of a perfect hermaphroditic structure as the normal form of the sexual type in the Trematodes and Cestoides among the Entozoa, in the abranchial Annelida, in the Planaria, and in many of the Mollusca, particularly in the Pteropoda, and in several families among the Gasteropoda. In some of these animals that are thus naturally hermaphroditic, the fecundation of the female organs of the bisexual individual is accomplished by its own male organs; but in others, although the anatomical structure is strictly hermaphroditic, yet the union of two, or, as sometimes happens, of more individuals, is necessary to complete the sexual act; and during it the female organs of each are respectively impregnated by the male organs of the other.

In the Nematodes and Acanthocephali among the Entozoa, and in the Cephalopoda and Pectinibranchiate Gasteropoda among the Mollusca, as well as in all symmetrically formed animals, or, in other words, in those whose bodies are composed of a union of two similar halves, as in the Insecta, Arachnida, Crustacea, and the Vertebrata, the male and female organs of reproduction are placed each upon a different individual of the species, constituting the basis of distinction between the two sexes. In such animals, a mixture of more or fewer of the reproductive organs of the two sexes upon the same individual appears occasionally as a result of abnormal formation; but the male and female organs that co-exist in these cases have never yet been found so anatomically perfect as to enable the malformed being to exercise the proper physiological function of either or of both of the two sexes. This form of true hermaphroditism or abnormal mixture upon the same individual of the organs of the two sexes in the higher animals, has been termed "unnatural" or "monstrous," in opposition to the natural hermaphroditism which exists as the normal type of sexual structure in some of the lower orders of animals, and in phanerogamic plants. The malform-

ation itself is observed to differ greatly, both in nature and degree, in different cases, varying from the presence or superaddition of a single organ only, of the opposite or non-predominant sex, up to the development and co-existence of almost all the several parts of the two sexes upon the same individual. In describing the malformation, we shall classify its various and diversified forms under the three general orders pointed out in our table (p. 408), including—1st, *lateral;* 2dly, *transverse;* and 3dly, *double* or *vertical hermaphroditism.*

A. TRUE LATERAL HERMAPHRODITISM.

According to the opinion of many physiologists of the present day, the two lateral symmetrical halves of the body, and even the two halves of all its single mesial organs, are originally developed in a great degree independently of one another. Granting this point in the doctrine of eccentric development, we can easily conceive how, in the same embryo, an ovary might be formed on one Wolffian body, and a testicle on the other; or, in other words, how female organs might be developed on one side, and male organs on the other. It is the existence of such an unsymmetrical type of sexual structure upon the two opposite sides of the body of the same individual, that constitutes the distinctive characteristic of lateral hermaphroditism.

Instances of this species of true hermaphroditic malformation have been observed in many different classes of animals, as well as in the human subject.

Individual examples are sometimes observed among insects, particularly among the Lepidoptera, in which all the different parts of the two sides or lateral halves of the body are formed after opposite sexual types. We shall afterwards have occasion to notice different examples of this form of lateral hermaphroditism as seen in the general conformation of the body, but may here state that in two or three instances such malformed insects have been carefully dissected, and found to present, in the anatomical structure of their sexual organs, a mixture of the organs of the male and female.

In a *Melitæa didymus* described by Klug,[1] the general external characters were those of the male, but the left eye, palpus, and antenna, and the left sexual fang, were smaller than in individuals belonging to this sex; and the left antenna was annulated with white and yellow at the apex, while the right was of one colour.

[1] *Froriep's Notizen*, vol. x. p. 183.

On dissection, the various male sexual parts were present, and they had appended to them a free female ovary situated upon the left, and united to no other organ.

In a *Gastrophaga quercifolia* dissected by Schultz, and described by Rudolphi,[1] the left side appeared externally male, and the right female, with a distinct line of separation throughout the whole body. On dissection, Schultz discovered an ovarium upon the right side, and two testes upon the left. The oviduct of the ovary joined the canal of the vasa deferentia about two inches before its termination; and the spermatheca was connected with the common evacuating duct. The two testicles on the left side were placed one behind the other, and connected by a thin vessel. The spermatic duct belonging to one of the testicles immediately received, as in the Lepidoptera, the spiral vessel; farther beyond, and on the opposite side, a second vessel, which appeared to consist of the rudimental spermatic duct of the other testicle, opened into it. The oviduct of the ovary joined the canal of the vasa deferentia about two inches before its termination in the penis, and a female spermatheca was connected with the common distended evacuating duct.[2]

A well-marked example of lateral hermaphroditism among the Crustacea has been recorded by Dr. Nicholls.[3] In a lobster (*Astacus marinus*), he found on the right side of the body a female sexual aperture in its normal situation at the root of the third leg, and connected with a regularly formed oviduct, full of ova. On the left side of the animal there was a male sexual aperture placed, as usual, at the root of the fifth leg, and connected internally with an equally perfect testicle and spermatic cord. The general external conformation of the animal corresponded with its internal sexual structure, the right lateral half of the body presenting all the secondary characters and peculiarities of the female, and the left all those of the male; so that if split from head to tail, to use Dr. Nicholls' mode of expression, the animal would have been perfectly female on the right side, and perfectly male on the left.

The investigations of Sir E. Home[4] led physiologists some years ago to believe that among Fishes lateral hermaphroditism constituted the natural type of sexual formation in the genera Myxine and

[1] *Abhandlung. der König. Akad. zu Berlin*, 1825, s. 55.

[2] See also drawings of the body and genital organs of a hermaphrodite *Sphinx populi* in Fischer's *Oryctographie du Gouvernement de Moscou*: Moscow, 1830.

[3] *Phil. Trans.* for 1730, No. 413, vol. xxxvi. p. 290, with drawings of the animal and of its reproductive organs.

[4] *Phil. Trans.* for 1823. Art. xii.

Petromyzon; but the later and more accurate observations of Rathké[1] have shown that these species are strictly bisexual, and that the opposite opinion had arisen from the kidneys of the female having been mistaken for the male testicles. Various instances, however, are on record of fishes, known to be normally bisexual, presenting from abnormal development a lateral hermaphroditic structure, or a roe on one side and a milt on the other. Such a hermaphroditic malformation has been met with in the genera *Salmo*,[2] *Gadus*,[3] and *Cyprinus*,[4] and in the *Merlangus vulgaris*,[5] *Acipenser huso*,[6] and *Esox lucius*.[7]

Of lateral hermaphroditism in Birds, we have one instance recorded by Bechstein,[8] in a chicken that had a testicle on the right side of the body, and an imperfect reniform ovary on the left. The external appearance of the bird presented a mixture of the characters of the two sexes.

Rudolphi has referred to a second and more ancient example of lateral hermaphroditism in the hen, mentioned by Heide.[9] The case, entitled by the author "galli qui putabatur hermaphroditus anatome rudis," is so imperfectly detailed as not to be entitled to much attention.

I have been fortunate enough to meet with two domestic fowls that presented in their sexual form and organisation examples of lateral hermaphroditism. In the first of these cases (Fig. 4), the female sexual organs were placed on the left side of the body, and the ovary, *a*, and oviduct, *b*, were in all respects apparently naturally

[1] *Bemerkungen über den Innern Bau der Pricke*, s. 119. See also additional observations by the same author in Müller's *Archiv für Anatomie, etc.*, for 1836. Heft ii. s. 171. The older error of Cavolini, who supposed that he had detected two ovaries and two testicles in the *Perca marina* and *Labrus channa* (*Sulla Generazione dei Peschi et die Granchi*, Nap. 1787), had been previously shown by Rudolphi to depend upon his having mistaken undeveloped portions of the ovaries for testicles. Schweigger's *Skeletlose Thiere*, s. 204; and *Abhandlungen König. Akad. der Wissenschaft zu Berlin*, 1825, p. 48.

[2] *Commercium Litter. Norim.* 1734, Hebd. 39.

[3] Pipping, *Vetensk. Akad. nya Handl.* 1800, Bd. xxi. s. 33, tab. i. fig. 1; Leuwenhoeck, *Experim. et Contempl.* p. 150; *Eph. Nat. Cur.* Dec. i. ann. i. obs. 125; Du Hamel, *Traité des Poissons*, Part ii. p. 130.

[4] Alischer, *Breslau Sammlung.* 1720, p. 645; Morand, *Mém. de l'Acad. des Sc.* 1737, p. 72; Schwalbe, *Commer. Lit. Norimb.* 1734, p. 305.

[5] Marchant, *Mém. de l'Acad. des Sc.* 1737, p. 12; Baster, *Opusc. Subsecsiva*, tom. i. p. 138.

[6] Pallas, *Reise durch Russe, etc.*, Theil. ii. s. 341.

[7] Reaumur, *Mém. de l'Acad.* 1737, p. 51; Starke, *Eph. Nat. Cur.* Dec. iii. ann. vii. and viii. obs. 109.

[8] *Naturgeschichte der Vögel, etc.*, 1807, Bd. ii. s. 1219.

[9] *Anatome Mytuli: subjecta est Centuria Obser.* Amsterdam, 1684, p. 193, obs. 95.

formed. On the right side, a male vas deferens, *d*, of about half the normal length, ran up the cloaca to opposite the origin of the iliac vessels, *c*, and during this part of its course was bent into those short transverse zigzag folds which characterise the structure of this part in the common cock.[1] When it reached the middle third of the kidney, *d d*, it lost this particular form, became membranous, *e*, and after proceeding upwards for about an inch, in the common course of the canal, at last disappeared. The convuluted or contorted portion ran over a space of about two and a half inches, and if unrolled would have extended three or four times that length. Its canal was about the usual size of the same part in the perfect cock, and perhaps at some parts even more dilated. Its cavity was filled with a whitish seminal-looking albuminous fluid, which at first prevented a mercurial injection from readily passing through it. There was not any apparent vestige of a testicle. The fowl that was the subject of this malformation possessed in an imperfect degree the plumage, comb, spurs, and general appearance of the cock, and when young was considered to be a male until the time it commenced to lay eggs, which it did very constantly, except during the moulting season, up to the time of its death. Its eggs were remarked to be very large. They had repeatedly been tried to be hatched, but always without success. The bird itself was never known to incubate. It was peculiar in its habits in so far that in the barn-yard it did not associate with the other poultry, and at

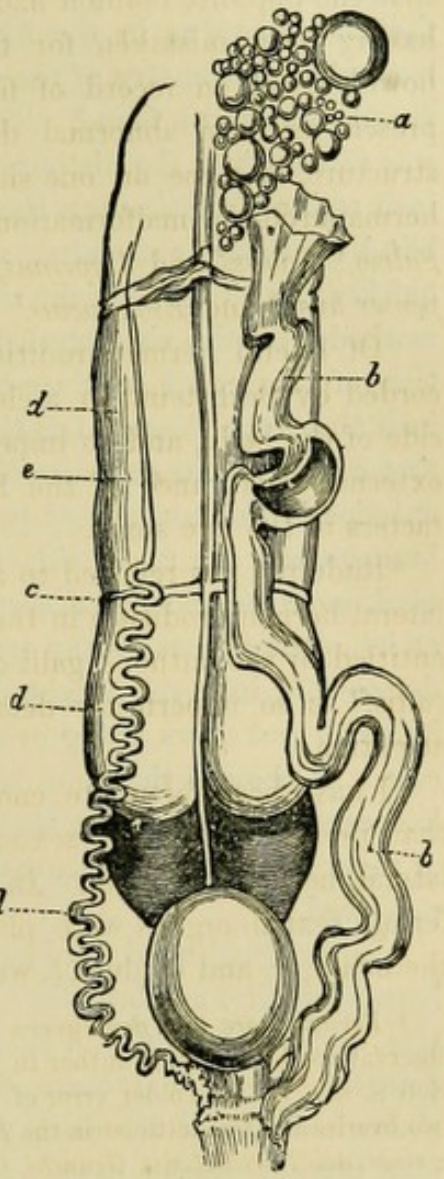


Fig. 4.

Fig. 4. Lateral Hermaphroditism in domestic fowl.

[1] See *Cycl. of Anat. and Phys*, vol. i. p. 354. Article "Aves."

night roosted separately from them. It crowed regularly, especially in the morning, and often attempted copulation with the hens.

In the second case, the ovaries and oviduct on the left side of the body were, as in the former example, natural in themselves; but in the mesometry of the oviduct a tube of the size of the male vas deferens was found. This tube, like the normal vas deferens, was thrown into the distinctive angular folds. It ran for about an inch and a half through the upper portion of the mesometry, was blind at either extremity, and admitted of being injected with quicksilver. On the right side there was also a male vas deferens, marked with the characteristic angular folds. The contorted portion of this canal only stretched—in this instance—to about an inch above the cloaca; but the folds were even stronger than in the first case, and the tube itself was rather more dilated. Above or anterior to this convoluted part, the tube became straight and membranous, and ran up in this form for about two inches in its usual track over the abdominal surface of the kidney; but there was not at its upper extremity any trace of a testicle. This bird presented during life, in a very slight degree only, the appearance of a cock, its comb and spurs being even less developed than in the previous case. It showed the same solitary habits in the poultry-yard. It laid eggs regularly. On three different occasions I had a number of them submitted to incubation, but in none of them was a chick produced.

In the Quadruped, Schlump[1] has mentioned an instance of lateral hermaphroditic malformation. In a young calf he found on the left side, under the kidney, a small testicle having attached to it a vas deferens, which was connected with the peritoneum towards the abdominal ring of the same side, and there became lost in the cellular texture of the part. An ovary and Fallopian tube, with a uterus consisting of a single horn only, were connected to the right side of the loins by a ligament. The neck of the uterus lost itself in the cellular substance beneath the rectum, and there was no vagina. The external organs were male, but imperfectly formed. The udder occupied the place of the scrotum.[2]

[1] *Archiv für die Thierheilkunde*, Bd. ii. Hft. ii. s. 204.

[2] In the *Gazette Médicale de Paris* for 1844, p. 530, Bertrand describes a hermaphroditic sheep, having a testis and epididymis in the left side of the scrotum, and with a vas deferens leading from it to a fully-developed ovary on the right. There was a uterus, and blind Fallopian tubes, and an imperforate vagina; and on the right or female side, a rudiment also, apparently of a vas deferens.

In the human subject, several different instances of sexual malformation have now been met with, referable to the head of lateral hermaphroditism. In these cases, along with a testicle on one side, and an ovary on the other, there has generally co-existed a more or less perfectly formed uterus. The external parts have differed in their sexual characters, in some instances being female, in others male, and in others again of a neutral or indeterminate type.

In man, and in the higher quadrupeds, we have not unfrequently exhibited to us a slight tendency to this unsymmetrical type of sexual structure, constituting true lateral hermaphroditism, in the testicle of one side only descending, whilst the other, in consequence of imperfect development, remains within the inguinal ring. In the single unsymmetrical ovary of most female birds and some fishes,[1] we see a still nearer approach to the state; and it is worthy of remark that, among birds at least, the single ovary is always placed upon the left side. In lateral hermaphrodites in the human subject, the left side also appears to be that on which we most frequently meet with the female type of the sexual organs. We shall divide the following cases according to the particular sides which were respectively male and female in them.

1. OVARY ON LEFT SIDE, AND TESTICLE ON THE RIGHT.

a. M. Sue met, in 1746, with an instance of this subdivision of lateral hermaphroditism in the human subject, in a young person, thirteen or fourteen years of age, whose case was the subject of a thesis sustained by M. Morand.[2] Of the *internal* genital organs,

[1] In the early embryo of birds, the ovaries are originally double, as pointed out by Emmert (see Reil's *Archiv* for 1811); and as was previously known to Wolff and Hochstetter (*Anat. Phil.* tom. i. p. 349).

[2] *De Hermaphroditis*, Paris, 1749. This, according to Arnaud (p. 323), is the same case of lateral hermaphroditism with that described by Lecat. If so, the latter author (probably from drawing his description from memory, and not, as Morand seems to have done, from the parts placed before him), has stated that, along with the testicle and vas deferens on the one side, there existed a vesicula seminalis, and that both sides were provided with round ligaments, the one on the male side forming probably one of the two tubes described by Morand as arising from the testicle. Meckel (Reil's *Archiv*, Bd. xi. s. 322) considers Morand's and Lecat's as two different cases, and points out that what is described as the male side in the one, was the female in the other, and *vice versa*. It is perhaps not unworthy of remark that, in the coloured plate accompanying the translation of Morand's case by Gautier, the male and female sides have been reversed from an error in the engraving; and this circumstance may have contributed to mislead Lecat in his description, provided he happened to look at this notice of the case.

there existed on the *left* side a very distinct ovary, a round ligament which ran outwards to the groin of the same side, and a well-formed Fallopian tube with its usual fimbriated extremity. The other extremity of the Fallopian tube terminated in the fundus of the uterus, which occupied its usual situation between the bladder and rectum. On the *right* side, again, there was a slender elongated testicle, which had moved forwards to the corresponding inguinal canal, but had not proceeded so far as to pass out of the abdominal cavity. On the superior part of the testicle was a body resembling the epididymis, and the testicle itself sent off two tubes, which afterwards united into one immediately before their insertion into the uterus. The *external* genital organs were those of a hypospadiac male, and during life the person had been always looked upon as belonging to the male sex. The perineal canal or vagina terminated between the scrotum and root of the imperforate penis, in a very small opening, which was common to it and to the meatus urinarius.

b. In 1754,[1] a young person of about eighteen years of age died in the Hôtel Dieu of Paris; and in dissecting his body, the anatomist Varole found the reproductive organs malformed in the following manner:—On the *right* side the scrotum contained a testicle, and the vas deferens arising from it opened, not as usual into the neck, but into the middle of the external border of the corresponding vesicula seminalis. On the *left* side the scrotum was empty; and internally on this side there were found an ovary, a Fallopian tube with its fimbriated extremity, a small oval uterus without a neck and somewhat flattened, and a broad and a round ligament, the last of which ran outwards, and was lost in the cellular tissue of the left half of the scrotum. The vesicula seminalis on the right, and the imperfect uterus on the left side, communicated by a canal of an inch and a half in length. The external organs were male; but the penis was very small, had no corpus spongiosum, and was imperforate for half-an-inch at its anterior extremity. The mammæ were as large as in women of the same age. The individual had been regarded during life as a male.

c. In 1825, the late Professor Rudolphi[2] detailed to the Academy of Sciences at Berlin the case of an infant who was reported to have died seven days after birth, and whose sexual organs exhibited the following interesting instance of lateral hermaphroditic conformation. On the left side were discovered an ovary (Fig. 6, *a*), without

[1] *Mém. de la Soc. Méd. de Paris*, tom. iv. p. 342.

[2] *Abhandlung. König. Akad. der Wissenschaft zu Berlin*, 1825, s. 60.

a distinct broad ligament, and a Fallopian tube, *b*, which communicated with the superior and left portion of a uterus, *c*. The left side of the scrotum (Fig. 5, *a*) was empty ; the right, *b*, contained a testicle (Fig. 6, *d*) furnished with an epididymis, *e*, and tortuous vas deferens, *f*. Below the uterus there was a hard flattened ovoid body (Fig. 6, *g*, and Fig. 7, *b*), which, when divided, was found to consist of a cavity with thick parietes, and was considered by

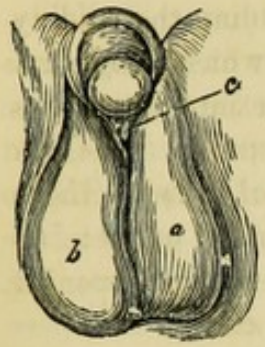


Fig. 5.

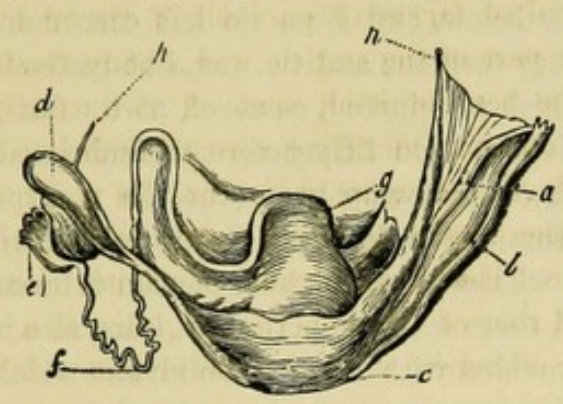


Fig. 6.

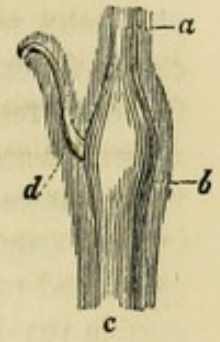


Fig. 7.

Rudolphi as the prostate gland in a rudimentary state. The mouth of the uterus (Fig. 7, *a*) terminated below in the parietes of this ovoid body, and on the right the vas deferens, *d*, penetrated into its substance, but without opening into its cavity. At the inferior part of the uterus there was a true vagina, *c*, which terminated in a cul-de-sac. The anus, rectum, and other organs, were natural. The external sexual parts were male, but the penis was divided inferiorly (Fig. 5, *c*). The testicle and ovary were supplied with the usual spermatic arteries (Fig. 6, *h h*).

d. In 1851, a well-marked case of lateral hermaphroditism was found in Dublin, and I have seen and examined the malformed parts in the College of Surgeons' Museum in that city. The subject of it, a supposed male convict, aged 26, was dissected by Dr. Banon, who has published an accurate and excellent account of the case, with illustrative drawings.[1] Externally the penis was of the usual size and structure of that organ in the male adult ; but it was imperforate—the urethra opening inferiorly at its root, between the bifurcations of a substance resembling the corpus spongiosum. The

Fig. 5. External organs.

Fig. 6. Uterus, turned downwards and forwards to show its posterior surface and connections, etc.

Fig. 7. Os uteri, vagina, prostate, and vas deferens.

[1] *Dublin Journal of Medical Science* for 1852, p. 66.

divided scrotum or labia gave lower down the appearance of the female vulva, with nymphæ and a small vaginal orifice contracted by a crescentic hymen. Internally, this vaginal orifice and a very narrow vaginal canal led upwards to a small well-formed uterus, which bore the exact relations to the bladder, rectum, and peritoneum, that this organ has in the normal female, and was provided latterally, like it, with broad ligaments. The uterus opened into the vaginal cavity below, and from its left corner superiorly there was given off a very tortuous and twisted, but perfectly permeable Fallopian tube, which terminated, as usual, at its farther extremity, in a corpus fimbriatum. On lifting up this fimbriated extremity, it was found to rest on an ovary bound to the peritoneum, and thrown somewhat across to the right side in the recto-vaginal space. There was no trace whatever of a right Fallopian tube or right ovary; but in their stead there was a male testicle, lying also in the recto-vaginal space, and provided with a long epididymis which ended in a permeable vas deferens that took an unusual course, for it first passed forwards and outwards in its normal course, nearly in the direction of the internal abdominal ring, and then suddenly turned backwards and downwards towards the uterus, ran along between the layers of its right broad ligament, became more and more slender, and perforated at last obliquely through the right side of the organ into the uterine cavity. There was no appearance of a vesicula seminalis or prostate gland, and no trace of a second testis or vas deferens. The testicle which was present seemed as perfect as in the normal male; it was provided with a tunica albuginea, and beneath this was found the delicate tunica vascula, and internally the tubular structure peculiar to this organ. A small quantity of fluid taken from the commencement of the vas deferens had the peculiar odour and consistence of the human semen, but on microscopic examination no spermatozoa, only numerous cells containing granules, were seen in it. The shape and connections of the ovary with the fimbriated extremity of the Fallopian tube were such as are seen in the normal female; and on division it presented the usual stromal tissue; but no appearance of Graafian vesicles could be detected in it. The subject of the preceding malformation was baptized as a girl, but afterwards, in consequence of the increasing size of the organ representing the penis, he was brought up as a male, and died of phthisis, when under sentence of transportation for sheep-stealing. His voice was masculine; he excelled in several of the manly exercises; his sexual desires appear to have been slight, but those of a male; and

his general configuration and appearance were those of a man, with the exception that there were but slight traces of beard. The pelvis was fan-like, and somewhat feminine in form.

e. Under the present section of lateral hermaphroditism, we may also, according to Mayer's report, include the celebrated case of Maria Duriée, or Charles Durge.[1] This person was baptized and brought up as a female, but at forty years of age was persuaded to change his name and dress to those of a man. We have already (p. 430) alluded to the great diversity of opinion which was entertained by the medical men of Europe in regard to the true sex of this individual. Even the different parts of his body were at one time referred to the male type, and at another time, and by other persons, to the female. The pelvis was the only part that was generally considered as decidedly female, yet the inspection of the body after death by Professor Mayer showed that even in this respect all were in error.

Of the female sexual organs there existed a uterus, vagina, two Fallopian tubes, and an ovary; and of the male, a testicle, prostate gland, and penis. The uterus was placed in its normal situation between the urinary bladder and rectum, but with its fundus directed in some degree to the left. The organ was extremely narrow, and two and a half inches in length. The cavity of its cervix presented on its inner surface some slight folds, but would scarcely admit a quill; the cavity of its fundus was nearly half-an-inch across. The small canals of two Fallopian tubes opened into the fundus uteri. Their abdominal extremities were shut, but the corpora fimbriata were present. Near the extremity of the right Fallopian tube, which was four inches and four lines in length, a small flattened almond-shaped body was placed, which, on examination, proved to be distinctly a testicle. It was completely enveloped in peritoneum, and received a cord composed of muscular fibres, and of a spermatic vein and artery. Its internal structure was yellow and filamentous, like that of the testicle, and its seminiferous tubes could be easily separated. The left Fallopian tube was an inch shorter than the right; and a little outside and behind its abdominal extremity another small flattened body was found inclosed in the peritoneum. It resembled an ovary rather than a testicle. Its tissue was composed of small granules conglomerated together. The penis was two inches and nine lines in length, and was for the greater part

[1] *Gazette Méd. de Paris*, 1836, No. 39; *Lancet*, vol. i. for 1836-37, p. 140; or *London Medical Gazette* for October 29, 1836.

concealed beneath the mons veneris. During life it was capable of erection, and was then elongated to more than three inches. The prepuce covered only half of the glans. There was not any corpus spongiosum. A fossa or groove, representing a urethral canal divided inferiorly, ran along the under surface of the penis. The two folds of skin forming the sides of the groove separated from each other posteriorly, and might be compared to nymphæ. Towards the root of the penis, by uniting inferiorly with a puckering of the skin of the labia majora or divided halves of the scrotum, they formed a circular orifice not larger than a quill, having some bodies, supposed to be vestiges of the carunculæ myrtiformes, at its lower edge, and leading to a short vestibule, or common canal, into which the urethra, surrounded by a firm but small prostate, entered from above, and the vagina, encircled at its entrance by a vascular ring of varicose veins, opened from below. The vagina was two inches and eight lines in length, and only ten lines at its greatest breadth. Its inner surface was somewhat wrinkled anteriorly, but smooth behind. It terminated above in a kind of spongy isthmus representing the blind orifice of the uterus, and from four to six lines in length. The diameters and form of the pelvis were, on dissection, found to be most evidently masculine.

The general character of Durge was a mixture of the male and female type. When between twenty and thirty, he had been examined by different medical men in Germany, France, and England, and, as we have already mentioned, the most contradictory opinions were offered upon his real sex. The breasts were not much developed, and there was no distinct mammary glandular structure. His stature was small. As he had advanced in age, his voice had become more firm and grave, and a slight trace of beard had appeared; but his head and face presented the aspect of that of an old woman. His neck was short, and the thyroid cartilage did not project much: his chest was fat and full. During the last few years of his life he was subject to epistaxis and hemorrhoids, but did not present any trace of sanguineous discharge from the genital organs —a phenomenon which was alleged to have manifested itself three times during his twentieth year.

The right hemispheres of the cerebrum and cerebellum, particularly that of the latter, were smaller and less developed than the left, and the left side of the occiput was externally more prominent than the right. He is stated by Professor Mayer to have shown a certain predilection for females, without, however, feeling any sexual desire.

2. TESTICLE ON THE LEFT, AND OVARY ON THE RIGHT SIDE.

An instance of malformation of the reproductive organs minutely described by Maret,[1] and which is in all its more essential anatomical points an example of lateral hermaphroditism, may be included under this head.

a. The subject of the case, Hubert Jean Pierre, died in the hospital at Dijon in 1767, at the age of seventeen. On the left side a perfect testicle was discovered, with its usual spermatic vessels, vas deferens, and vesicula seminalis, all occupying the natural situation in which they are placed in the male adult. The vesicula seminalis contained a fluid of the colour and consistence of semen. On the *right* side an oblong cystic tumour was found lying in the iliac fossa, and stretching outwards into the inguinal region. On opening it a quantity of reddish limpid fluid escaped, and then the solid contents of the tumour were seen to consist of a somewhat flattened body, that gave off from the upper part of its right side a short Fallopian tube; and at the fimbriated extremity of this tube an ovary of the natural size, consistence, and figure, was situated. The roundish-shaped body to which the tube was attached was about an inch and a half in its greater, and an inch in its smaller diameter. It contained in its centre a small cavity continuous with that of the tube—a circumstance which, along with the structure of its walls, left little doubt that the body itself was an imperfectly formed uterus. No other opening except that of the tube could be traced into its cavity. Its external surface was attached to the ovary by a kind of ligament. On this same side of the body (the right) there existed also a vesicula seminalis, but smaller and more shrivelled than that on the left. It gave off a vas deferens, which became gradually smaller as it was traced backwards, and at last disappeared altogether without being connected with any structure resembling a testicle. In regard to the external organs of generation, the penis was four inches long and imperforate, but in all other respects perfectly formed. It possessed a corpus spongiosum, which does not exist in the female clitoris. On raising the penis, it was observed to cover a large fissure, the sides of which resembled the labia of a female. In the left labium, or left half of the scrotum, the testicle already alluded to was placed, but there was none in the right. When the labia were separated, two red spongy bodies were seen, resembling the nymphæ in appearance, and

[1] *Mém. de l'Acad. de Dijon*, tom. ii. p. 157.

seemingly consisting of the sides of the split urethra. Between these bodies, and at their upper part, the urethra opened as in the female ; while below there was a very narrow aperture covered by a semilunar membrane, and presenting on one side of its entrance a small excrescence somewhat resembling in figure a caruncula myrtiformis. This orifice led into a membranous canal or cul-de-sac an inch in depth and half-an-inch in diameter. On the lower part of this canal the verumontanum and orifices of the seminal ducts of both sides were discovered.

During life, Pierre had been considered a male, but was not known to have shown any partiality for the female sex. His countenance was more delicate than we ordinarily see in the male sex. There was no beard on the face ; the larynx was not enlarged as in man ; and the mammæ, each of which was furnished with a very large areola, were of a moderate size and roundish form. The configuration of the lower part of the body was more decidedly masculine, and there was none of that enlargement of the buttocks and projection of the thighs, from the increased width of the pelvis, which is observable in young females.

In this case we have on the *left* side of the body male sexual organs, consisting of a perfect testicle, vas deferens, and vesicula seminalis. On the *right* side, again, we have a female ovary and Fallopian tube with a rudimentary uterus, together with an imperfect male vesicula seminalis and vas deferens.

b. Arnaud mentions a very imperfect form of lateral hermaphroditism as having been recognised by M. Boudou, surgeon to the Hôtel Dieu of Paris, on the person of a monk who died in that hospital in 1726. The external genital parts were those of a hypospadiac male. In one of the halves of the scrotum a testicle was found ; the other was empty. The seminal canals and vesiculæ seminales on the side on which the perfect testicle existed were natural in their course and situation. Those of the opposite side lost themselves between the bladder and rectum in a small body, which, in M. Boudou's opinion, was a shrunk uterus.[1]

Among the preceding cases of lateral hermaphroditism in the human subject, there are five in which the left side, and one only in which the right, was the female. In the last instance, quoted from Boudou, the respective sides on which the male and female organs were placed are not stated by Arnaud.

[1] Arnaud, *loc. cit.* p. 283.

B. TRUE TRANSVERSE HERMAPHRODITISM.

In the variety of hermaphroditic malformation which we have last considered, we have found upon the same individual the reproductive organs of one side disagreeing in their sexual type from those of the other. In the present division we have a similar sexual antagonism following a different direction; for supposing the internal sexual apparatus to be divided from the external by a transverse line, we have, in transverse hermaphroditism, on each side of this partition, organs of an opposite sexual type; in other words, the organs of reproduction (in the more correct sense of the word), or the internal sexual organs, do not, in the present species of hermaphroditism, correspond in type with the organs of copulation, or the external sexual parts—a circumstance, the occasional occurrence of which tends to show that these two portions of the generative apparatus are in some degree independent of each other in their normal development and existence, and consequently also in their abnormal formations.

Transverse hermaphroditism varies in its character according to the relative positions occupied by the co-existing male and female organs; the external organs, or all those exterior to the supposed transverse line, being sometimes female, and the internal male, and *vice versa*.

1. TRANSVERSE HERMAPHRODITISM WITH THE EXTERNAL SEXUAL ORGANS OF THE FEMALE TYPE.

In the cases included under this subdivision, the external genital organs consist of a clitoris, vagina, and uterus; the uterus is often rudimentary, and sometimes altogether absent and replaced by the male vesiculæ seminales. The male internal organs are the testicles, generally small and imperfectly developed, and placed either within or without the abdomen, with vasa deferentia terminating in the uterus and vagina.

This variety of sexual malformation has been repeatedly observed among our domestic quadrupeds, particularly among black cattle. Mr. John Hunter, in an essay read before the Royal Society in 1779, and published in their *Transactions*,[1] and in his Observations on the Animal Economy, showed that, as had been long known among

[1] *Trans. Royal Society*, vol. lxix.

agriculturists, when among black cattle the cow brings forth twin calves, one of them a male, and the other apparently a female, the male is a perfect bull calf, but the female, while it has all the external marks of a cow calf, as the teats and udder, is still, with a few exceptions, imperfectly formed in its internal sexual organs, and very generally presents a mixture of the organs of the two sexes in various degrees. Such hermaphroditic twin cattle have long been distinguished in this country under the name of *free-martins*. In some exceptional cases only have they been observed capable of breeding; and generally they show no sexual desire for the bull, or the bull for them. In appearance they resemble the ox or spayed heifer, and have a similar or still greater disposition to become fat under the use of good food.

In the paper to which we have referred, Mr. Hunter has described the dissection of three free-martins; and one of these seems to belong to our present division of female transverse hermaphroditism. The clitoris and external parts appear to have been strictly of the female type, and there was a small udder with four teats. The vagina terminated in a blind end a little beyond the opening of the urethra, and from this point the vagina and uterus were impervious. The uterus at its superior part divided into two horns, and at the termination of these horns, not ovaria, but bodies resembling the male testicles, were found. These bodies had not a perfect internal structure like that of testicles, but resembled these organs in so far that—1*st*, they were nearly as large as the male testes, and much larger than the female ovaries; 2*d*, they were supplied with tortuous spermatic arteries like those of the bull or rigdil; and 3*d*, cremaster muscles passed up to them, as in rigdils, from the abdominal rings. There were two small vesiculæ seminales placed behind, between the bladder and uterus, with their ducts opening into the vagina. Nothing, according to Mr. Hunter, similar to the vasa deferentia was present; but Gurlt is inclined to believe that the parts which Mr. Hunter has described as the horns of the uterus were really the deferent vessels.

Professor Gurlt[1] has himself given, from a preparation in the Museum of the Berlin Veterinary School, the accompanying sketch of the malformed sexual organs of a five-year-old free-martin (Fig. 8), which presents to us an illustration of Mr. Hunter's supposed mistake, at the same that it affords a well-marked example of transverse hermaphroditism. The detail of the anatomical peculiarities of the

[1] *Lehrbuch der Pathol. Anat. der Saug. Th.* Bd. ii. s. 186.

case has been unfortunately omitted by the author, but from the short explanations appended to the drawing, it appears that the clitoris, *a*, and external pudenda, *b*, were perfectly feminine, and that the vagina, short and funnel-shaped, terminated at its superior contracted extremity in two vasa deferentia, *c c c*, which were carried upwards in a duplicature of peritoneum, *d d*, resembling the broad ligament, until they joined the unrolled and lengthened epididymes, *e e*, of two small testicles, *f f*, placed in the position of the ovaries. Near the junction of the vagina and vasa deferentia, bodies resembling the male vesiculæ seminales, *g g*, and Cowper's glands, *h h*, were situated, and the urethral canal, *i*, opened into the vagina, and was shorter than it usually is in the cow.

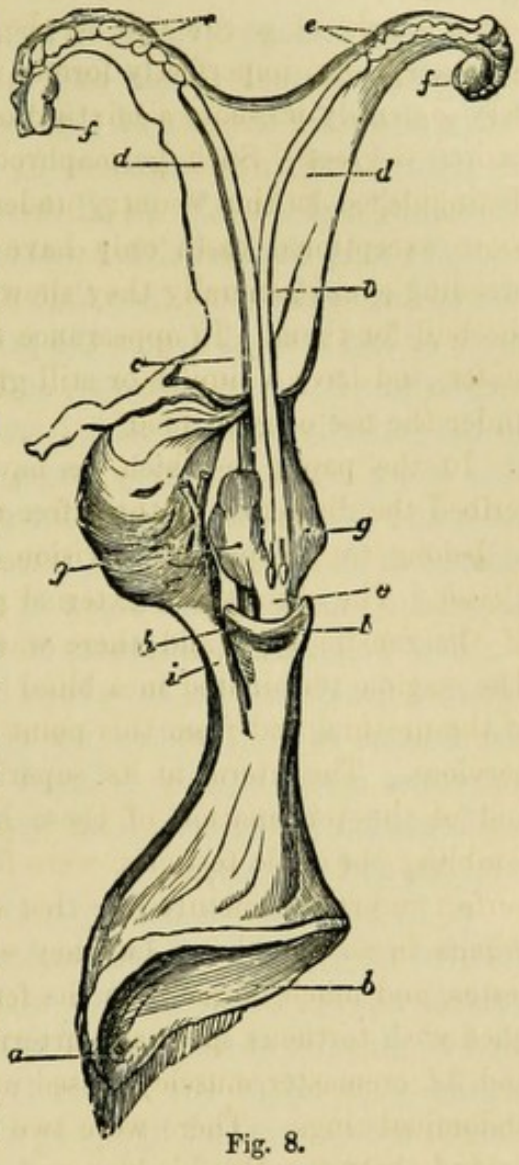


Fig. 8.

We have found upon a free-martin cow a state of the sexual apparatus very much resembling that figured in the above case by Professor Gurlt. The two vasa deferentia, as they ran in the duplicature of the peritoneum, had very much the appearance and shape of an imperfectly developed uterus. The vesiculæ seminales were large; the vasa deferentia were quite impervious throughout their whole course; and the bodies placed at their abdominal extremities were large, but of so indeterminate a structure as not to enable us to pronounce them to be either true testicles or ovaries.

M. Geoffroy St. Hilaire published in 1834 a very distinct case of a hermaphroditic goat which had two male testicles and epididymes with a two-horned uterus and female external parts.[1] M.

[1] *Nouv. Ann. du Muscum d'Hist. Nat.* tom. ii. p. 141.

Isidore St. Hilaire[1] mentions a nearly analogous case in the same animal, and quotes a third from Bomare which was observed upon a deer.[2]

To the present division of transverse hermaphroditic malformation with external female and internal male organs, we may probably also refer the case of the hermaphrodite dog detailed by Sir E. Home,[3] and three instances in the sheep described by Ruysch,[4] Herholdt,[5] and Gurlt.[6] In all these instances, imperfectly developed testicles were situated either within the abdomen or without it upon the udder, at the same time that the external parts exhibited in a more or less marked degree the peculiarities of the female sex; the vagina was, however, narrower, and the clitoris more developed than in the perfectly-formed female; and in the dog mentioned by Home this latter organ was very large, being three-quarters of an inch long, and half-an-inch broad, but still it could not properly be considered as an imperfect penis, since the bone, which forms the distinguishing mark of that organ in the dog, was wanting.

Few well-marked instances of transverse hermaphroditism with external female organs, have been hitherto described as observed in the human subject, unless we regard as an approach to it the numerous cases, already referred to, of spurious hermaphroditic malformation in the male from hypospadiac division of the urethra, scrotum, and perineum.

a. In his essay on hermaphroditism, however, Steghlehner[7] has detailed at great length the particulars of a case belonging to the present variety, which he met with on the body of a woman who died of phthisis at the age of twenty-three. The external sexual organs were all of the female type, and in general well formed, though the clitoris and nymphæ were perhaps smaller than natural, and the orificium vaginæ was rather contracted and half shut up by a hymen. The fossa navicularis was very distinct, and the vagina normally situated, but extremely short and narrow. Its internal surface presented an appearance of transverse and longitudinal rugæ, but its upper extremity formed a blind sac, and no traces could be found beyond it of the uterus, nor indeed any vestiges whatever of

[1] *Histoire des Anomalies*, tom. ii. p. 128.
[2] *Journ. de Phys.* tom. vi. p. 501.
[3] *Phil. Trans.* for 1795, p. 157; *Comp. Anat.* iii. 323.
[4] *Thesaur. Anat.* viii. n. c. iii. tab. 115.
[5] Viborg's *Sammlungs für Thierartze*, 1797, s. 25.
[6] *Lehrbuch*, etc., Bd. ii. s. 186 tab. ix. 2, and xxii. s. 2.
[7] *Tract. de Hermaphr. naturâ*, p. 120.

the other internal female organs, the ovaries and Fallopian tubes. On more minute examination, a testicle with its spermatic cord was found in each inguinal region, placed outside the external ring, and surrounded with their cremaster muscles and vaginal coats. The testicles were flaccid and small, but their internal structure and that of their epididymes was natural; and the slender pervious vasa deferentia arising from them entered the abdomen, descended into the pelvis, and were joined behind the urinary bladder by two vesiculæ seminales of considerable size. Their common ejaculatory ducts opened into the vagina. The form of the thorax and pelvis, and of the body in general, was feminine; and the mammæ and nipples were well developed, but the larynx was rather more protuberant than in females, and the voice approached in tone to that of a man. There had never been any menstrual discharge, but the periodical molimina indicative of its appearance were said to have been observed regularly. There were some hemorrhoidal tumours situated around the anus.

b. If possible, a still more perfect example of the present variety of transverse hermaphroditism in the human subject was some years ago observed at Naples. The malformation occurred in the person of an individual, Maria E. Arsano, who died at the age of eighty in one of the pauper charities at Naples, and who had passed through life as a female, and been married as such. No suspicion of the malformation existed during life, and it was only accidentally discovered when preparing the dead body for demonstration in the anatomical theatre of Professor Ricco, who afterwards carefully dissected the malformed parts in company with Professors Sorrentino and Grosetti. We have taken the following account and sketches from Ricco's published description of the case.[1]

The external organs of generation were those of the female in their natural or normal state, consisting of the mons veneris, with a scanty quantity of hair (Fig. 9, *a*); of the labia externa (Figs. 9 and 10, *b b*) naturally formed, and the nymphæ (Figs. 9 and 10, *d d*); of the clitoris (Figs. 9 and 10, *c*), which was perfectly imperforate, and of the ordinary size of the same organ in the adult female; of the orifice of the urethra (Figs. 9 and 10, *e*) situated below the clitoris; and of the os vaginæ (Figs. 9 and 10, *f*), which was of the usual size and diameter. Altogether, the aperture of the vulva was natural. The canal of the urethra was of the usual length, as seen at *u* in the section of the pelvis represented in Figure 10, in which *s* marks the

[1] *Cenno Storico su di un Neutro-Uomo*, pp. 5, 7.

divided symphysis pubis, and *p* the peritoneum. The os vaginæ showed no vestiges of the membrane of the hymen, or, in other

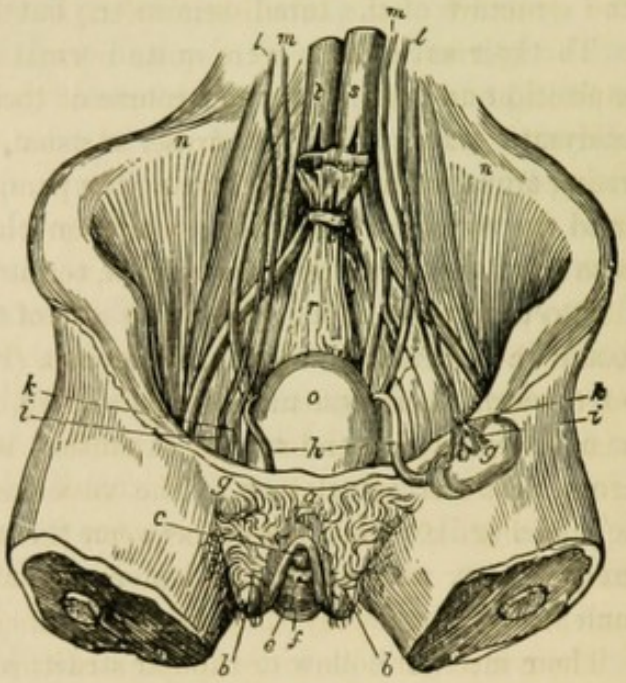


Fig. 9.

words, was without carunculæ myrtiformes. The canal of the vagina (Fig. 10, *v*) was about two inches long, but without rugæ, and it terminated internally in a completely blind extremity or cul-de-sac.

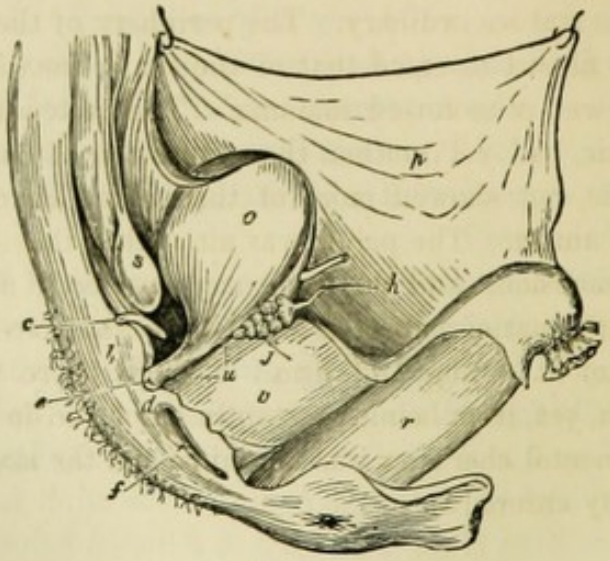


Fig. 10.

The uterus was entirely wanting, as were also the Fallopian tubes and uterine ligaments.

The internal organs of reproduction were, on the other hand, completely male. The two testicles (Fig. 9, *g g*) were situated in the

region of the pubis, and were scarcely clear of the inguinal rings. They were of the usual ovoid figure, and natural in size. They had internally the structure of the tubuli seminiferi, but it was not well developed. The spermatic cords were quite normal both in regard to their composition and the origin and course of their blood vessels. The right spermatic artery (Fig. 9, *l*) arose, as usual, from the renal, and the corresponding vein *m*, after forming the pampiniform plexus, *k*, opened into the vena cava inferior; while on the left side the artery, *l*, arose from the aorta, and the vein, *m*, terminated in the left emulgent. The epididymes of the testes were also of the usual vermiform figure, and the corresponding vasa deferentia (Figs. 9 and 10, *h h*), coursed towards their vesiculæ seminales (Fig. 10, *j*), and terminated in an attenuated membranous expansion without any external aperture or ducti ejaculatorii. The vesiculæ seminales (see the left one, *j*, in Fig. 10) were placed between the urinary bladder, *o*, and rectum, *r*; they were smaller and more shrunk than those of the adult male, though certainly they preserved their naturally oblong form. Their internal hollow or tubular structure was indistinct. The prostate gland was not present. The urinary bladder, *o*, and ureters, *n n*, the rectum *r*, and the other intestinal viscera, with the abdominal blood-vessels (*s*, the aorta, *t*, the vena cava, Fig. 9), seem to have been all quite natural.

The head of the above individual was of the usual size, the neck long, and the stature ordinary. The periphery of the thorax was so expanded as almost to equal that of the male, notwithstanding the presence of well-pronounced mammæ. The face, although entirely free from hair, had yet neither the expression of that of a female nor of a male, but showed more of that mixed character which is seen in the eunuch. The pelvis was altogether that of a male in its form and dimensions, and the limbs were perfectly masculine. According to information collected after death, the voice was deep, and the temperament strong and firm. Though there was never any menstruation, yet, from being constantly employed in domestic occupation, the mental character was feminine, and the married state had been willingly entered into.

2. TRANSVERSE HERMAPHRODITISM WITH THE EXTERNAL SEXUAL ORGANS OF THE MALE TYPE.

The male organs that are present in this subdivision, consist of the penis, which is provided with a regularly-formed prepuce, glans,

corpora cavernosa, and corpus spongiosum, with the urethra perforating it, and of the prostate gland, verumontanum, etc. The coexisting female organs are the ovaries, the Fallopian tubes with their infundibula, and the uterus.

We are not aware of any recorded instances of this variety of hermaphroditic malformation among the lower animals. We have already, under the head of spurious hermaphroditism in the female, from enlargement of the clitoris, etc., mentioned several cases, in which, from excessive development, the external organs in women had assumed some of the characters of the corresponding parts in man; but the two following cases described by Professor Eschricht of Copenhagen, and Bouillaud of Paris, present instances of malformation in which the more exterior sexual organs were all formed upon the male, and the internal upon the female type.

a. The subject of the case described by Eschricht[1] was a twin child that died very shortly after birth, and in whom the external sexual organs were of the male type, and the internal female. The penis (Fig. 11, *a*) and scrotum, *b*, were well developed, but the usual raphé seen upon the latter was absent. The urethral canal of the glans and body of the penis was pervious throughout, and admitted of a sound being easily passed into the bladder. The glans was remarkably thin and slender. The prepuce could be easily pushed back. No testicles could be felt in the scrotum, and internally there was a uterus with Fallopian tubes and ovaries. The uterus, *c*, was about an inch in length, and had the general form presented by this organ in female infants. It contained a cavity marked with rugæ, but had no orifice inferiorly, nor any vagina attached to it. Its blind or imperforate neck was firmly attached to the posterior walls of the urinary bladder, *g*, while its fundus was directed very obliquely downwards and over to the left side. From the left side of the fundus of the uterus a twisted Fallopian tube, *d*, proceeded, having well-developed fimbriæ, *e*, at its abdominal extremity, and the broad ligament or fold of peritoneum along which it ran contained an oblong soft body, *i*, which Eschricht considered as distinctly an ovary, and a round ligament that took its course through the inguinal canal of the same side. On the right side an ovary, *k*, and Fallopian tube, *f*, were likewise discovered, but they were displaced and separated from the body of the uterus. The ovary lay in the iliac region, and above it and towards its outer side was placed the fimbriated extremity of the corresponding Fallopian tube. The

[1] Müller's *Archiv für Anatomie*, etc., 1836, Heft ii.

tube presented towards this extremity a vesicular swelling of the size of a small pea, which admitted of being inflated and filled with quicksilver through a small opening between the fimbriæ. Below this it was impervious, and apparently diverged off into two prolongations, one of which (the round ligament) passed down into the

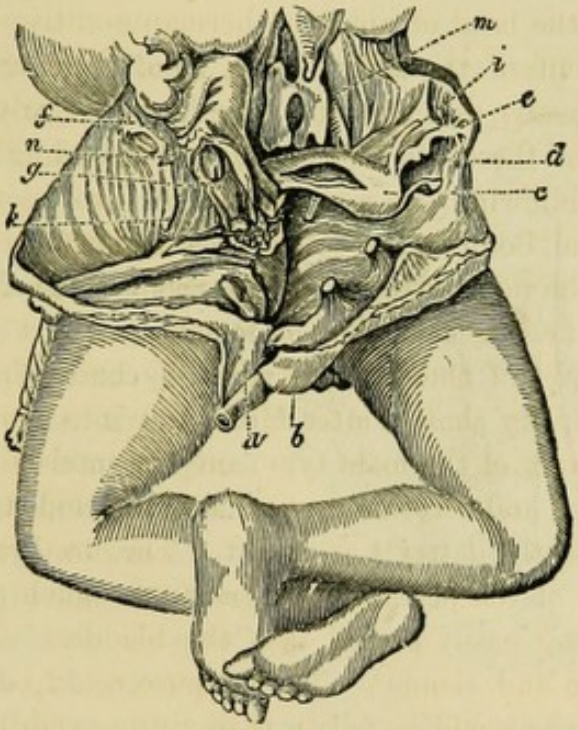


Fig. 11.

inguinal canal, and the other crossed over with a fold of peritoneum to where the rectum and urinary bladder were preternaturally connected together. Professor Jacobson suggested that this latter part was a rudiment of the right half or horn of the uterus. It may perhaps, however, be more properly regarded as the commencement of the right Fallopian tube, and in this case it would, if continued onwards, have been joined to the neck of the uterus—an arrangement which would be quite in accordance with the usual deep and displaced origin of one of the tubes in instances of congenital obliquity of the uterus.

The child was malformed in other respects also. The anus was imperforate, and the rectum, *n*, opened into the urinary bladder, which was very contracted. The kidneys, *m*, were irregularly formed, and lay near the promontory of the sacrum. There was an accessory spleen, and the formation of the heart and large vessels was abnormal. The other twin child was well formed and lived.

b. The case of transverse hermaphroditism observed by Bouil-

laud[1] was even still better marked than that of Eschricht. Valmont, the individual who was the subject of it, died in one of the hospitals of Paris of the epidemic cholera. He was a hatter by trade, and had been married as a male. No further particulars of his history or habits could be obtained. The following was found by MM. Manec and Bouillaud to be the state of the external and internal sexual organs.

Externally there was a penis (Fig. 2) of medium size, terminating in a regularly-formed glans, *a*, and furnished with a prepuce, *b*.

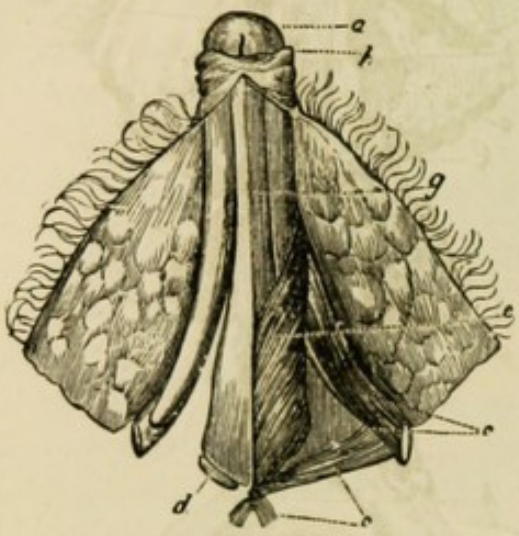


Fig. 12.

The urethra (Fig. 13, *b b*) opened on the inferior side of the glans (Figs. 12 and 13, *a*). In its course from this point backwards to the bladder, it perfectly resembled the urethra of the male, and was surrounded at its origin by a well-formed prostate gland (Fig. 13, *k k*). Cowper's glands were also present (Fig. 12, *d*). The verumontanum or caput gallinaginis was distinct, as well as the orifices of the prostatic follicles; but the usual openings of the seminal canals could not be found. The corpus spongiosum urethræ (Fig. 12, *g*) and the corpora cavernosa (Fig. 13, *m m*) were as well developed as in the perfect male subject. The scrotum was small, and did not contain any testicles; it presented on its middle a line or raphé extending from the prepuce to the anus, and which was harder and better marked than it usually is upon male subjects. The various muscles of the male perineum (Fig. 12, *c c*) were present, and very perfectly formed. The constrictores urinæ muscles, *e*, were particularly long and thick.

In the cavity of the pelvis, two ovaries (Fig. 13, *d d*), similar in form and structure, according to M. Manec, to those of a girl of fifteen or sixteen years of age, or, to adopt M. Bouillaud's statement, two bodies in some sort fibrous, and perhaps intermediate in their structure between ovaries and testicles, were found, along with two Fallopian tubes (Fig. 13, *g g*), having each a fimbriated extremity

[1] *Journ. Hebdom. de Méd.*, tom. x. p. 466. "Exposition Raisonnée d'un cas de nouvelle et singulière variété d'hermaphroditisme observée chez l'homme."

at one end, and opening by the other into the cavity of a uterus, *h*, which occupied the usual situation of that organ in the female, and opened inferiorly into a kind of vagina, *e*. The internal surface of the uterus showed the usual arborescent wrinkles of this organ in the unimpregnated state; the os tincæ was regularly formed; the vagina was about two inches long, and of medium size, and presented internally numerous ridges, such as are met with in virgins. This canal, when opposite the neck of the bladder at *f*, became much contracted, and was continued downwards in the form of a small tube to the membranous portion of the urethra, into which it entered by a narrow orifice. The broad ligaments of the uterus were normally formed; the round ligaments passed through the inguinal canal, accompanied, each, by an artery larger than that of the corresponding one in the female sex.

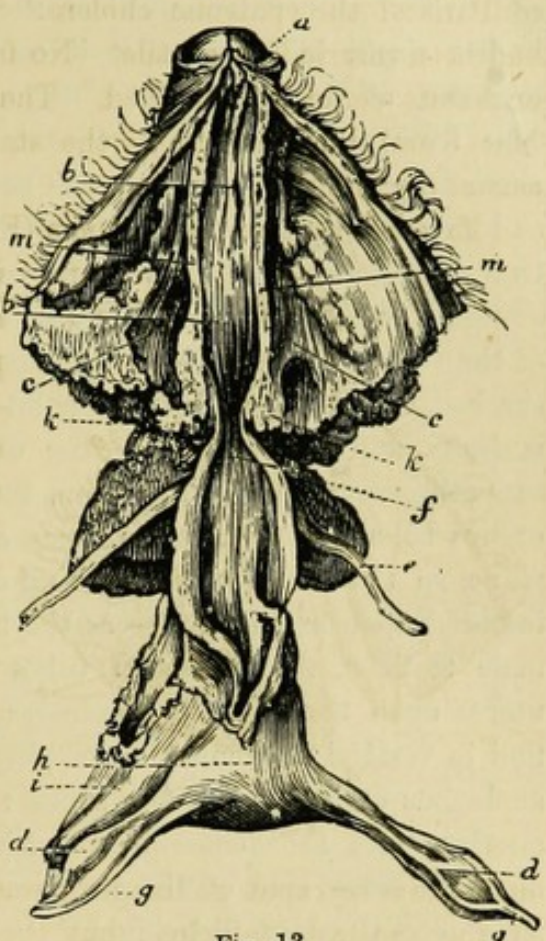


Fig. 13.

The external appearance and form of Valmont are described by M. Bouillaud as having been intermediate between those of the male and female sex. The stature was short; the mammary glands and nipples were well developed; the face was bearded; but the general physiognomy was still delicate. The body was fat; the hands and feet were small; the pelvis was shallow; and the haunches were wider than in a well-formed man.

C. TRUE DOUBLE OR VERTICAL HERMAPHRODITISM.

In the two divisions or orders of true hermaphroditism which have been already considered—the *lateral* and the *transverse*—we have seen re-united upon the body of the same individual more or fewer of the organs of the two sexes, but so arranged as not

necessarily at least to present the occurrence of actual duplicity in any of the corresponding male and female parts. In both lateral and transverse hermaphroditism, the type of the sexual apparatus is in fact *single*, in so far that it consists in almost all cases in the presence, at one part, of an organ or organs differing in sexual type from those that are present at other parts, without there necessarily co-existing at any one point the two corresponding male and female organs. In the present or third variety, however, of true hermaphroditism, we come to a tendency to actual sexual *duplicity*, in the co-existence of two or more of the analogous organs of the two sexes upon the same side, or in the same vertical line of the body. At the malformed point or points the sexual apparatus is *double*, and one part male, the other part female, in type. For, supposing we viewed, either from before or behind, the reproductive organs belonging to the two sexes all stretched out upon the same plane, so that their corresponding organs should be exactly superimposed upon one another—as the two female ovaries upon the two male testicles, the Fallopian tubes upon the vasa deferentia, the uterus upon the vesiculæ seminales and prostate gland, etc.—we find in vertical or double hermaphroditism more or fewer of those analogous organs of the two sexes that were thus placed upon one another, and that consequently lay in the same vertical line, or upon the same side of the body, co-existing together at the same time upon the same individual.

Double, vertical, or complex hermaphroditism, differs much in variety and degree, in different cases, from the imperfect repetition of two only of the corresponding organs of the male and female upon the same body, to the re-union or co-existence of almost all the analogous internal genital organs of both sexes upon one individual.

For the purpose of contrasting and collecting together as much as possible the more analogous cases, we shall arrange the instances of double hermaphroditism under three genera or divisions; the *first* including cases in which there co-existed a female uterus and male vesiculæ seminalis, with a general female type; the *second*, those in which a female uterus, occasionally provided with Fallopian tubes, was added to an organisation that was in other respects essentially male; and the *third* comprehending all examples in which ovaries and testicles are alleged to have been repeated together upon one or both sides of the body. Other divisions of double hermaphroditism may become necessary under the accumulation of new varieties of cases, but we believe it will be possible to arrange all the instances

hitherto recorded under one or other of the above divisions. In classifying and describing these instances, we shall in the meanwhile offer no observations on the probable anatomical mistakes that have been committed in the examination of individual cases; and we shall describe the various malformed parts according to the designations usually given to them in normal anatomy, and by the authors who have described the cases. We reserve the true anatomical and morphological value of the different structures named and involved, for special consideration under a separate and future chapter, where we shall endeavour to show the numerous sources of error with which the observation of individual examples and varieties of complex hermaphroditism is beset.

1. MALE VESICULÆ SEMINALES, ETC., SUPERADDED TO ORGANS OF A FEMALE SEXUAL TYPE.

In this first genus or division of double hermaphroditism, we find two female ovaries, or bodies resembling ovaries, and an imperfect uterus, co-existing with two male vesiculæ seminales, which are occasionally accompanied, also, with rudiments of the vasa deferentia. One of the free-martins described by Mr. Hunter[1] is referable to this variety of double hermaphroditism. The external genital organs and mammæ resembled those of the cow, but were smaller in size. The vagina, beyond the opening of the urethra into it, was, with the uterus itself, impervious. The imperfect uterus divided into two horns, at the end of which were the ovaria. On each side of the uterus there was an interrupted vas deferens, broken off in several places; and between the bladder and vagina these vasa deferentia terminated in two vesiculæ seminales. The ducts from the vesiculæ and the vasa deferentia opened into the vagina. In this instance we have all the female organs present, but imperfect in their development; and at the same time there is superadded to them a tubular structure, formed, according to Mr. Hunter's opinion, of the male vesiculæ seminales and vasa deferentia.

In a free-martin cow, which I had an opportunity of dissecting, an arrangement of sexual parts very similar to that described in the preceding case was found. The uterus, however, though small, was pervious for a distance of some inches above the vagina; and at the abdominal end of each blind Fallopian tube there was a dilated sac

[1] See *An. Econ.* p. 64. Mr. Well's Free-martin.

of considerable size, lined by peritoneum, and opening into the abdominal cavity by a small orifice. These sacs we considered as abortive attempts at the formation of the fimbriated extremities. Imperfect bodies, which we considered as ovaries, were placed in their normal situation near the cavities which we mention. They were small in size, and of an oblong shape. On a section being made of them, they showed internally a kind of dense homogeneous yellow tissue, dotted or crossed with strongly marked white lines. The male vasa deferentia could be traced along each side of the uterus in the form of broken dense cords. The vesiculæ seminales were large and partially hollow, and near them on each side there was an oblong body of considerable size, having the appearance of Cowper's glands. The tubes from them, and from the vesiculæ seminales, opened near the os tincæ into a vagina of nearly the usual size.

2. AN IMPERFECT FEMALE UTERUS, ETC., SUPERADDED TO A SEXUAL ORGANISATION ESSENTIALLY MALE.

In the cases included under this second division of double or vertical hermaphroditism, there exist a male testicle or testicles, vasa deferentia, and vesiculæ seminales, along with a female uterus. The uterus occupies its normal situation between the bladder and rectum. It is sometimes defectively developed, and of a membranous structure; and occasionally it is not provided with Fallopian tubes, or, in the quadruped, with cornua. The cavity of the uterus communicates with a vagina that either opens in its usual situation externally, or, as happens more frequently, joins the male urethra. In some cases the vagina is wanting, and the uterus opens directly into the canal of the urethra.

Several cases of sexual malformation in the ram, goat, and dog, referable to this variety of double hermaphroditism, have been described by different authors; and various analogous instances have now also been observed in the human subject.

In a lamb described and delineated by Mr. Thomas,[1] all the external parts were male, but the scrotum was divided or hypospadiac. Internally there were two perfect male testicles in the situation of the ovaries, with their epididymes, vasa deferentia, and vesiculæ seminales; and a well-formed two-horned uterus furnished

[1] *London Med. and Phys. Journ.*, vol. ii. 1799, p. 1; with a good drawing of the malformed organs of generation.

with its usual ligaments, and with Fallopian tubes that ran up and terminated in a tortuous convoluted manner upon the testicles. The body of the uterus possessed the common rugose structure, but the horns were lined by a smooth membrane, without their usual glandular bodies internally. At the anterior extremity of the fundus uteri, a thick semilunar valve, which seemed to correspond to the os tincæ, passed across, and hardly allowed a fine probe to be entered over its upper edge. The vagina scarcely existed, and formed only a short smooth pouch, terminating below in a cul-de-sac. The male vesiculæ seminales and vasa deferentia entered the male urethra in their normal situation at the caput gallinaginis.

Gurlt[1] has described and delineated the sexual parts of a goat, in which all the internal male genital organs were found with the exception of Cowper's glands (Fig. 14). There was also present a uterus, *e*, provided with long but narrow and curved cornua, *f f*, that accompanied the vasa deferentia and testicles through the abdominal rings, and ended blind at the epididymes. The testicles lay externally upon the udder, which was of considerable size. The scrotum was absent; the penis, *g*, was short, tortuous, and imperforate; and there was a fissure in the perineum into which the urethra, *h*, opened.

Stellati[2] has recorded an analogous case in the same animal. The male sexual organs were not entirely complete, and there were superadded to them a female vagina and an imperfectly-developed uterus, the Fallopian tubes of which ran towards the inguinal rings, and terminated with them upon the epididymes of the testicles.

Another instance of hermaphroditic malformation in the goat, detailed at great length by Meckel,[3] seems also in its principal points justly referable to the present division of cases, although there was at the same time a tendency, in the unequal size of the two cornua uteri, etc., to a degree of lateral hermaphroditism.

Professor Mayer of Bonn[4] has detailed at length the dissection of three hermaphroditic goats, in all of which the conformation of the sexual parts resembled in its more essential features the preceding cases of Thomas and Gurlt. In all the three instances there were found two male testicles with their epididymes, vasa

[1] *Lehrbuch der Pathol. Anat.*, bd. ii. s. 195, pl. ix. figs. 1 and 2, and pl. xxii. figs. 3 and 4.

[2] *Atti del Real Instit. d'Incoragg. alle Sc. Nat.*, Naples, tom. iii. p. 380.

[3] Reil's *Archiv für die Physiologie*, bd. xi. s. 334-38.

[4] *Icones Select. Præparat. Mus. Anat.*, Bonn, pp. 17-20, tab. iv. fig. 5, and tab. v. figs. 1, 2, and 3.

deferentia, and vesiculæ seminales; and at the same time there was present a well-marked two-horned uterus, with a vagina opening into the urethra. In the first case the large hollow coruna uteri

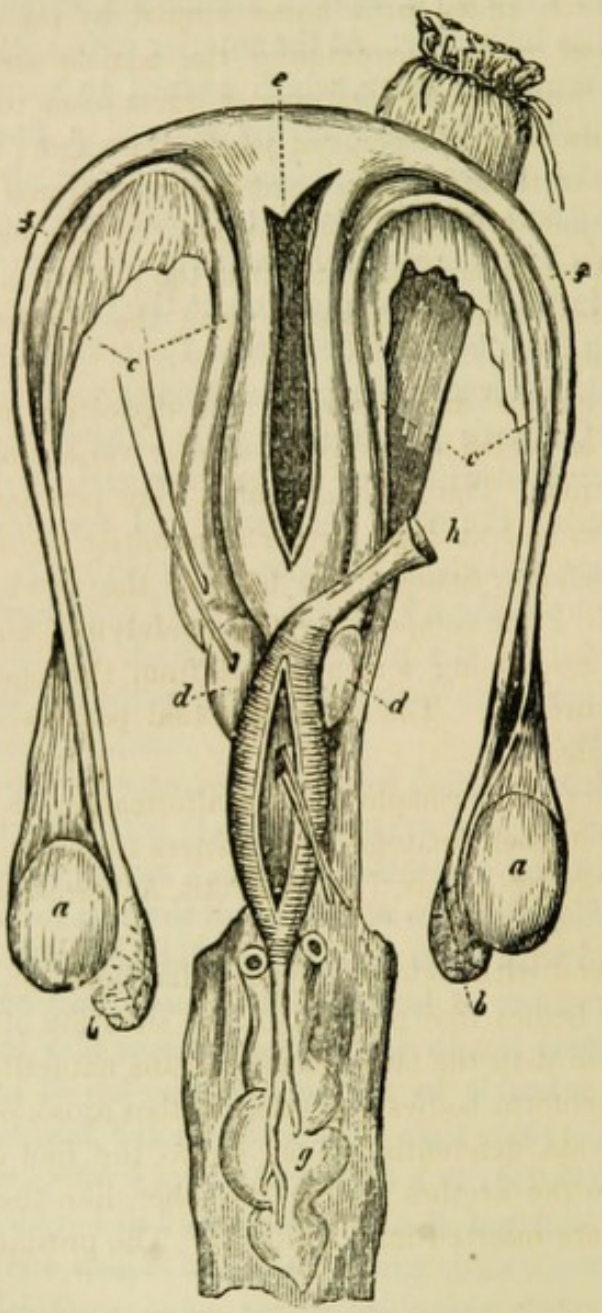


Fig. 14.

terminated in blind extremities, and there were only very short impervious rudiments of the Fallopian tubes. In the second case, at the extremity of the right horn of the uterus, a blind appendiculum was situated, formed by a vestige (according to Mayer) of the

Fig. 14. *a a*, The testicles; *b b*, epididymes; *c c*, vasa deferentia; *d d*, vesiculæ seminales.

Fallopian tube; and from this a ligament was sent off to the corresponding testicle; a similar ligament, but no appendicula, existed on the left side. In the third case both Fallopian tubes were present, and each ended in a bursa formed by the lamina of the peritoneum, and partly surrounding the testicle and epididymes. In two of the instances the ejaculatory ducts seem to have opened into the urethra near the point at which the vagina terminated in it; and in one of the cases they opened into the canal of the vagina itself before it joined that of the urethra. All the external organs were male, but malformed in so far that the penis was short, and in two of the cases somewhat twisted; and the scrotum was either small or wanting.[1]

The same author[2] has described the dissection of a dog, the sexual organs of which exhibited a similar variety of hermaphroditic malformation. The Fallopian tubes were pervious throughout in this instance, and at their further extremities opened upon the neighbouring cellular tissue. The body of the two-horned uterus was very small. On compressing the epididymes and vasa deferentia, a fluid resembling semen issued from the openings of the latter into the urethra. The external sexual parts were those of a hypospadiac male.

Several cases of hermaphroditic malformation in the human subject, similar in their anatomical characters to the preceding, have been described by Columbus, Harvey, Petit, Ackermann, Steghlener, and Mayer.

a. In a person with external hypospadiac male organs, Columbus[3] found two bodies like testicles in the situation of the ovaries, and larger in size than the latter female organs naturally are. From each of these testiform bodies two sets of tubes arose, one of which, like the male vasa deferentia, passed on to the root of the penis and opened into the urethra; while the other, like the female Fallopian tubes, were inserted into a uterus. The prostate gland was absent.

b. Harvey[4] has mentioned a very small hermaphroditic embryo, on which he found a two-horned uterus with two testicles of a very small size, and, near the diminutive penis, some traces of a prostate gland.

[1] For other similar cases in the goat, see Leuckart, in *Cyclopædia of Anatomy*, vol. iv., p. 1426; and Kobelt's *Nebew-Eierstock des Weibes*, etc., p. 38.

[2] *Loc. cit.* p. 16, tab. iv. fig. 3, external parts of generation; fig. 4, internal.

[3] *De Re Anat.* lib. xv. [4] *De Gen. Anim. Exerc.* lxix. p. 304.

c. The observation of M. Petit[1] of Namur is still more complete. On the body of a soldier, aged twenty-two, who died of his wounds, and whose external organs appear to have presented no deviation from the male type except in the absence of the testicles from the scrotum, these bodies, with male vasa deferentia, vesiculæ seminales, and a prostate, were found to co-exist with female Fallopian tubes, and a uterus that was attached to the neck of the urinary bladder, and opened into the urethra between this neck and the prostate. The form of this imperfect uterus, M. Petit remarks, merited for it rather the name of a vagina than of a uterus, and it resembled more this organ in the female quadruped than in women. From the body of the uterus, at three inches from its entrance into the urethra, two Fallopian tubes arose. These tubes were perforated, and were three inches and a half long; their abdominal extremities were not loose and provided with fimbriæ, but were attached to a small soft body on each side, occupying nearly the natural situation of the ovaries, but having the substance or structure of the testicles, and provided with an epididymis and vas deferens. The vasa deferentia were each seven inches and a half long, and were attached to two long and rather slender vesiculæ seminales placed alongside of the uterus. The vesiculæ opened into the urethra by two ducts.

In a note appendid to this case, M. Petit states that he had been consulted by a man who passed blood by the penis regularly every month, without pain or any troublesome symptom. Perhaps, adds M. Petit, this man had also a concealed uterus. I have been informed, on credible authority, of two similar cases, the one in a young unmarried man of seventeen years of age, and the other in a person who had been married for several years without his wife having had any children. In both of these cases the discharge was in very considerable quantity, and perfectly regular in its monthly occurrence. Did it consist in a periodical hemorrhage from the urinary bladder or passages only? Or was it, as M. Petit seems to suppose in his instance, of a true menstrual character, and produced by a female uterus, etc., existing internally, and communicating as usual with the canal of the urethra?[2]

[1] *Hist. de l'Acad. Roy. des Sc.* for 1720, p. 38.

[2] More lately, several similar cases have been recorded of apparent menstruation through a perforate penis, as by Harris, in *American Journal of the Medical Sciences* for July 1847; two cases by Blackman, in *Ibid.* for July 1843, etc. In an American case, that of Suydam, there was one descended testis, and a hypospadiac penis. This person is said to have had a regular catamenial discharge. The question was raised as to whether this person was a male or female, and

d. Professor Ackermann[1] of Jena published in 1805 the following interesting case of the present variety of hermaphroditic malformation. It occurred in an infant that lived about six weeks after birth. On dissection, two testicles were found; one of them had descended into the scrotum or labium; the other had advanced no further than the groin. Both were perfectly formed, and had their usual appendages complete. In the natural situation of the female uterus, there was found a hollow pyriform organ, which, from its locality and connections, was supposed to be a uterus, though its coats were finer and thinner, and its cavity greater, than naturally belongs to that viscus. Duplicatures of peritoneum, resembling the ligamenta lata, connected this imperfect uterus with the sides of the pelvis, and its cavity opened into a kind of short vagina, which soon united with the urethra, and formed one common canal with it, *vagina urethralis.* The vasa deferentia ran from the testicles towards the superior angles of the uterus, and penetrated into its substance at the points where the Fallopian tubes are usually placed. Without opening here, however, they passed onwards under the internal mucous-like membrane of the uterus and vagina, and at length terminated, by very small orifices, in the vagina urethralis. Immediately previous to entering the ligamenta lata, each vas deferens formed a number of convolutions, conglomerated into a mass resembling a vesicula seminalis.

e. Steghlener[2] has described at great length the case of an infant that survived only for half-an-hour after birth, upon whose body he found perfect external male organs (Fig. 15, *a b*), and internally two small elongated testicles, *c c*, with their epididymes, *g g*, and the convolutions of imperforate vasa deferentia, *b b*, distinctly marked. The testes were, as seen in the sketch, supplied with spermatic arteries following their usual course. Between the rectum and bladder there was placed a very large pear-shaped bag or pouch, *f*, with firm, coriaceous, but not thick, walls, and distended with fluid. This bag or imperfect cystoid uterus terminated inferiorly by a narrow neck in a vagina that opened into the urethra in the situation of the verumontanum, and was there dilated into a large bag or ampulla, occupying exactly the site of the prostate gland, and resembling this organ also in its form and position. The internal membrane of the uterus was col-

entitled or not entitled to vote as a *freeman* at a political election. See Dr. Taylor's *Medical Jurisprudence*, p. 613. See also *Ibid.*, p. 256.

[1] "Infantis androgyni historia et iconographia;" *Edin. Med. and Surg. Journal*, vol. iii. p. 202.

[2] *De Hermaphr. Nat.* p. 104.

lected at its neck into numerous valvular-like folds, and that of the vagina had also a rugous or plicated arrangement. From the fundus of the large sac of the uterus, and not from its angles, but from near its middle, two impervious solid ducts (vasa deferentia) arose, and after a somewhat flexuous course reached the testicle, *c c*, lying in the superior part of the iliac fossæ. These ducts had attached to them at one or two points a number of small reddish nodules, *b b*, consisting, according to Steghlener, of glandular granules, and described by Ackermann, in his case, as vesiculæ seminales. The canal of the urethra was obliterated for a short distance towards the fossa navicularis, and the urinary bladder, *j*, and uterus, *i i*, were extremely distended, and the left kidney, *m*, was vesicular.



Fig. 15.

Mayer, in the work already referred to,[1] has described and delineated the following five cases of the present species of hermaphroditic malformation in the human subject, all of which he had himself met with and dissected.

f. In a fœtus of the fourth month, and affected with omphalocele and extroversion of the urinary bladder, he found male testicles (Fig. 16, *a a*) with their epididymes, *b b*, and a two-horned uterus, *c*, terminating in a vagina, *d*, that opened into the posterior part of the urinary bladder, *e*. From the left testicle a contorted vas deferens, *f*, arose, and ran down to the vagina; the right vas deferens, *g*, was shorter, and became threadlike, and disappeared near the corresponding cornu of the uterus. A rudiment only of the left male vesicula seminalis was observable. The external organs were male; the glans penis, *h*, was imperforate.

[1] *Icones Select.* etc., pp. 8-16. See also Walther and Graefe's *Journal der Chirurgie und Augenheilkunde*, bd. vii. hft. 3; and bd. viii. hft. 2.

g. In another fœtus of the sixth month,[1] there existed a perfect set of internal and external male sexual organs—viz. testicles, epididymes, vasa deferentia, and vesicula seminales—with a prostate gland and a normally-formed penis and scrotum. But, besides

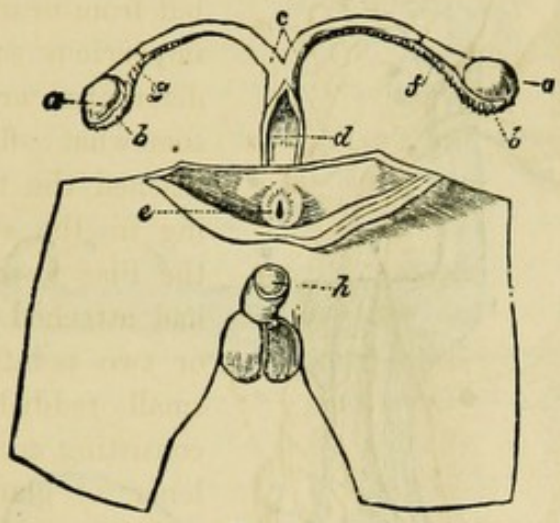


Fig. 16.

these, there was also present an imperfect female uterus, the body of which divided into two cornua, the right longer and incurvated, the left shorter and sacciform. The neck of the uterus was marked internally by its usual arborescent appearance; and it opened into a vagina that terminated in the urethra near the exit of the latter from the urinary bladder.

h. In a third case[2] of hermaphroditic malformation in an infant who died of convulsions when six months old, Mayer found the following blending of the organs of the two sexes. Of the internal male genital organs there were present two bodies at the inguinal rings, that were evidently testicles (Fig. 17, *a a*), as was proved not only by their position, but by their form, coverings, connections, and internal structure, "their substance," says Mayer, "being evidently composed of yellow canals;" their epididymes, *b b*, were also distinctly developed, and each of them sent off a vas deferens, *c c*, which was furnished with a corresponding multilocular vesicula seminalis, *d d*. Of the internal female sexual organs there were found a perfectly developed uterus, *e e*, with its broad, *n n*, and round, *o o*, ligaments naturally formed and placed, and provided with two Fallopian tubes, *f f*, that followed the course of the testicles through the inguinal canals, and a vagina, *g*, which opened into the urethra, *h*, near its external orifice. The ejaculatory ducts

[1] *Icones*, p. 8, tab. ii. fig. 5. [2] *Ibid.*, p. 9, tab. iii. figs. 1 and 2.

of the male vesiculæ seminales opened into this vagina at *l* and *m*. The internal surface of the vagina was already beginning to present the appearance of its usual rugæ. The cavity of the uterus was triangular, and exhibited on the internal part of the cervix its

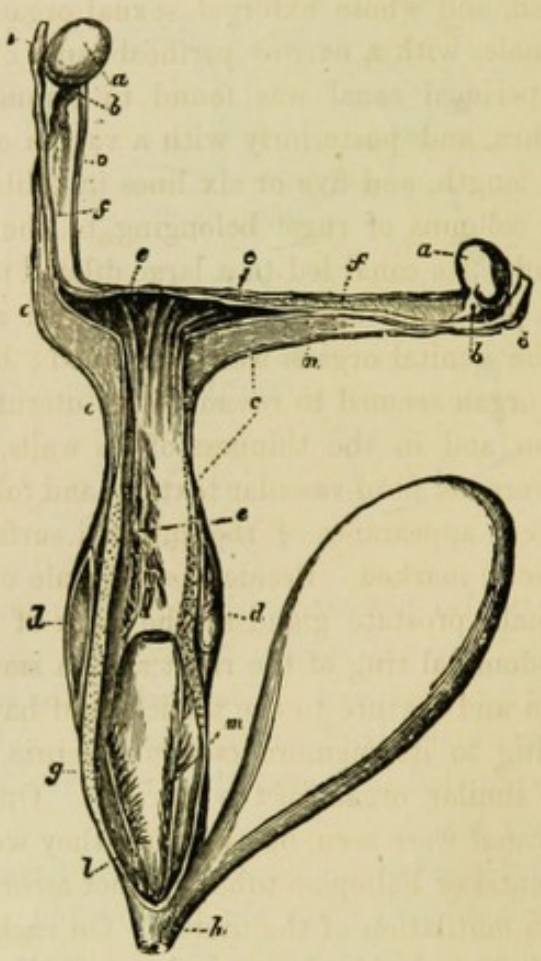


Fig. 17.

characteristic plicated or arborescent structure. The Fallopian tubes were, at their uterine orifices, of a large calibre; their cavity afterwards became suddenly contracted, and then again dilated, and terminated at their ulterior extremities, where they lay in contact with the testicles at the external inguinal rings, in blind sacs, *i i*, without any very distinct appearance of fimbriæ. The external genital parts in this very interesting case were of a doubtful nature, being referable either to those of a hypospadiac male, or of a female with a large clitoris, but without nymphæ, the meatus urinarius being in its normal situation, but leading behind to the cavities of both the urinary bladder and uterus. The circle of the pelvic bones was large.

i. The two other instances described by Mayer occurred in adult

subjects, and the malformation in both of them differed from that found in the cases just now cited, in this, that there was only one testicle present along with the imperfect uterus.

The subject of one of these cases[1] was a person who died at the age of eighteen, and whose external sexual organs were those of a hypospadiac male, with a narrow perineal canal or fissure. On dissection, this perineal canal was found to communicate anteriorly with the urethra, and posteriorly with a vagina of two inches and nine lines in length, and five or six lines in calibre. The anterior and posterior columns of rugæ belonging to the vagina were only slightly marked. Its canal led to a large dilated uterus, the superior part of which was unfortunately cut away with some diseased viscera before the genital organs were examined; but, from the portion left, this organ seemed to resemble the uterus of quadrupeds in its oblong form, and in the thinness of its walls, which were composed of a cavernous fibro-vascular texture, and full of lacunæ. The usual arborescent appearance of the internal surface of the os uteri was very perfectly marked. Beside these female organs, there was a well-formed male prostate gland at the neck of the bladder; and behind the abdominal ring of the right side, a small roundish body, similar in form and texture to the testicle, and having the cremaster muscle adhering to its membranous involucrum. There were no traces of any similar organ on the left side. On both sides some portions of a canal were seen, but whether they were the remains of the vasa deferentia or Fallopian tubes was not ascertained, on account of the previous mutilation of the uterus. On each side of the neck of the uterus there was placed a vesicula seminalis, provided with an ejaculatory duct that opened into the orifice of the vagina. The dimensions of the pelvis approached much nearer to those of the female than those of the male. In the secondary sexual characters of the individual, the female type was further recognised in the want of prominence in the larynx, in the slender form of the neck, and (according to Professor Mayer) in the rounded shape also of the heart, the smallness of the lungs, the oblong shape of the stomach, the large size of the liver, the narrowness of the forehead, and the conformation of the brain; while the individual approximated, on the other hand, to the male in the length and position of the inferior extremities, in the breadth of the thorax, the undeveloped state of the mammæ and the hairy condition of their papillæ, and in the existence of a slender beard upon the chin and cheeks.

[1] *Icones*, p. 11, tab. iii. figs. 3 and 4.

j. In the second adult subject, a person eighty years of age, Mayer found,[1] on the left side of the cavity of the abdomen, and near the inguinal ring, a small oval body exhibiting imperfectly in its internal structure the tubular texture of the male testicle, and having an appendix resembling the epididymis attached to it. From this testicle arose a vas deferens, which was joined in its course by a vesicula seminalis, and ended in an ejaculatory duct. On the opposite or right side, a vesicula seminalis, having no continuous cavity, was present; but no vestige of a corresponding testicle, vas deferens, or ejaculatory duct, could be discovered. The prostate gland was present, and regularly formed. In the cavity of the pelvis a uterus was found with parietes of moderate thickness, and of the usual cavernous texture; its cervix was marked internally with the appearance of the natural arborescent rugæ. Inferiorly it opened into a narrow membranous vagina, that received the right ejaculatory duct, then passed through the body of the prostate, and latterly joined the canal of the urethra. The fundus of the uterus could not be examined, as it had been removed in a previous stage of the dissection. The external parts were male and naturally formed, with the exception of the penis, which was shorter than usual, and had the canal of the urethra fissured inferiorly, and the meatus urinarius situated at its root. The individual was during life regarded as a male, but had all along remained in a state of celibacy. The general appearance of the face and body was that of an imperfectly marked male, but the pelvis was broad like that of a female.[2]

3. CO-EXISTENCE OF FEMALE OVARIES AND MALE TESTICLES.

This third division of complex or double hermaphroditism includes all those cases in which a male testicle and female ovary exist together, either upon one side only, or upon both sides of the body. With this arrangement, other malformations by duplicity of the sexual organs are generally combined; but these are so various in their character as not easily to admit of any useful generalisation. In considering this third division of complex hermaphroditism, we

[1] *Icones*, p. 15, tab. iv. figs. 1 and 2.

[2] For an account in the human subject of two additional cases of this variety of hermaphroditic malformation—viz., an imperfect male organisation, with the addition of a highly developed uterus—see Weber's *Zusätze zur lehre der Geschlechtsorgane*, p. 15; or Maret, in the *Memoires de l'Acad. de Dijon*, 1772, p. 157; and Chevreul in *Journal de Medecine* for 1779, p. 441.

shall mention, *first*, the cases in which *two* testicles and *one* ovary are stated to have co-existed; and *secondly*, those in which there have been supposed to be present *two* testicles and *two* ovaries.

Two testicles and one ovary.—The two dissections that we have previously detailed of lateral hermaphroditic insects,[1] show that in these two cases this variety of sexual duplicity existed. It appears to have been observed also in two instances of hermaphroditic malformation in the quadruped, the histories of which have been described by Mascagni and Mayer.

In a bull, nine years of age, and which was provided with the usual external organs of the male, Mascagni found internally, on dissection, a prostate gland and two perfect vesiculæ seminales, vasa deferentia, epididymes, and testicles. The testicles and epididymes were injected with mercury through the vasa deferentia. In addition, there was discovered near the left testicle, and connected to it by peritoneum and blood-vessels, a body having the structure of the female ovary; and, in its normal situation, there existed a distended double uterus, containing from fifteen to sixteen pounds of a clear fluid. This uterus was furnished with two Fallopian tubes at its upper part, and terminated inferiorly in a vagina, which opened by a small orifice into the male urethra.[2]

In a goat, dissected by Mayer,[3] he found two testes with their epididymes fully developed, and vasa deferentia and vesiculæ seminales. One of the testes was placed without, and the other still remained within the abdominal cavity. At the same time there were present a large female vagina communicating with the urethra, and a double-horned uterus provided with two Fallopian tubes. One of these tubes terminated in a blind canal, but the other had placed at its abdominal extremity several vesicles, resembling, according to Mayer, Graafian vesicles, or an imperfect ovary. The vesiculæ seminales, and, through regurgitation by the urethra and ejaculatory ducts, the cavities of the vagina and uterus, were filled with about four ounces of a whitish fluid, having the colour and odour of male semen. This fluid could not be found by the microscope to contain any seminal animalcules, but only simple and double Monades (*Monades termones et guttulas*). Bergmann, however, is alleged to have found it, on analysis, to contain the same chemical principle that characterises human male semen.

Two testicles and two ovaries.—Various instances have now been

[1] See p. 434, under Lateral Hermaphroditism.

[2] *Atti dell'Acad. delle Scienze di Siena*, tom. viii. p. 201. [3] *Iconcs*, p. 20.

published in which this sexual duplicity has been supposed to exist among cattle and other domestic quadrupeds, as well as in the human subject.

One of the free-martins,[1] described by Mr. Hunter, comes under this variety. In the case referred to, in the situation of the ovaries "were placed," to use Mr. Hunter's words, "both the ovaria and testicles,"—or, as Sir Everard Home, in alluding to this case, more justly expresses it, "an appearance like both testicles and ovaria was met with close together."[2] The two contiguous bodies were nearly of the same size, being each about as large as a small nutmeg. There were no Fallopian tubes running to the ovaries, but a horn of an imperfect uterus passed on to them on each side along the broad ligament. Pervious vasa deferentia were found; they did not, however, reach up completely to the testicle on either side, or form epididymes. The vesiculæ seminales were present, and much smaller than in the perfect bull. The external parts appear to have been those of the cow, but smaller than natural. The vagina passed on, as in the cow, to the opening of the urethra, and, after having received it and the orifices of the seminal ducts, it began to contract into a small canal, which ran upwards through the uterus to the place of division of that organ into its two horns.

Velpeau,[3] in his work on *Midwifery*, mentions that in an embryo calf, he had "found reunited the testicles and ovaries, the vasa deferentia and uterus."

In a hermaphroditic foal-ass, Mr. Hunter[4] found both what he considered to be two ovaries placed in the natural situation of these bodies, and two testicles lying in the inguinal rings in a process or theca of peritoneum similar to the tunica vaginalis communis in the male ass. No vasa deferentia or Fallopian tubes could be detected; but there was a double-horned uterus present, and from its broad ligaments, to the edges of which the cornua uteri and ovaries were attached, there passed down on either side into the inguinal rings a part similar to the round ligament in the female. The horns and fundus of the uterus were pervious; but its body and cervix, and the canal of the vagina from above the opening of the urethra into it, were imperforate. The external parts were similar to those of the female ass; but the clitoris, which was placed within the entrance of the vagina, was much larger than that of a perfectly

[1] *An. Econ.* pp. 63-64, pl. ix. [2] *Comp. Anat.* vol. iii. p. 322.
[3] *Traité de l'Art des Accouchemens*, tom. i. p. 114. [4] *An. Econ.* p. 58.

formed female; it measured about five inches. The animal had two nipples.

Scriba has given an account[1] of a hermaphroditic sheep, in which two large testicles are stated to have been found in the scrotum, at the same time that there existed, in their normal situation, two moderately-sized ovaries, and a small uterus furnished with two apparently closed Fallopian tubes. The external sexual parts appear to have been those of a malformed male, the penis being short and impervious, the scrotum divided, and the urethra opening into a contracted perineal fissure resembling the female vulva. This animal had often attempted connection with the female sheep.

Borkhausen[2] has described a very similar case in the same species of animal. Each half of the divided scrotum contained a testicle, which was regularly formed, but greater in size than usual, and furnished with a large spermatic artery. The pelvis contained a normal uterus, which was smaller, however, than natural; it was provided with its usual ligaments. The Fallopian tubes were present, but imperforate, and the two ovaries were full of vesicles, and inclosed in a strong membrane. The vagina was natural, and opened as in the female. Behind the divided scrotum, the rudiment of an udder with four teats, instead of two, was situated. The male penis was also present, but diminutive and short; its erector muscles were small, and the prostate gland indistinct. The urethra was single as it left the bladder, but it afterwards divided into two canals, the wider of which opened into the female vagina and vulva, and the narrower ran through the male penis. The urine passed in a full stream through the former canal, and only by drops through the latter. The animal is alleged to have attempted coition in both ways.

In 1829, an account of a hermaphroditic goat was published at Naples, which is said to have been provided with both female ovaries and male testicles.[3] The two ovaries occupied their usual situation; no Fallopian tubes were found; but there were present a double-horned uterus with blind cornua, and a vagina which opened externally, as in the female. In the neighbourhood of the ovaries, and more external than them, two small testicles were discovered, having two vasa deferentia arising from them. The vasa deferentia ran downwards to two corresponding vesiculæ seminales,

[1] *Schriften der Gesellschaft Naturforschender Freunde zu Berlin*, Bd. x. s. 367.

[2] *Rheinisches Mag. zur Erweiterung der Naturkunde*, Giessen, 1793, Bd. i. s. 608.

[3] *Brevi cenni su di un Neutro Capro;* or, Gurlt's *Pathologischen Anatomie*, Bd. ii. s. 198.

that were placed alongside of the uterus. In the lower angle of the external pudenda, a body, resembling in length the male penis more than the female clitoris, was situated: it was, as we have already had frequently occasion to mention in regard to the penis in malformed male quadrupeds, of a very tortuous or convoluted form.

I have had an opportunity of examining an excellent preserved specimen of double hermaphroditism in the sow, referable to the present section, which was met with some years ago by Dr. Knox, and I have his permission to state here the following particulars of the case.

Among the internal female organs there is present a natural well-formed double uterus, provided with broad ligaments and two hollow cornua, each about six or seven inches in length. The fimbriated extremities are not distinctly marked, the female tubes appearing to end blind at their upper terminations, as they have often been observed to do in similar cases. The os uteri opens inferiorly into a vagina, which seems normal in its structure. At a short distance from the upper extremity of each horn of the uterus, two bodies of considerable magnitude are seen lying in close juxtaposition. The smaller of these two bodies is on either side about the size and shape of a large almond; and though internally of an indeterminate amorphous structure, they are considered by Dr. Knox as answering to the two ovaries. The two larger bodies, which are placed *between* the supposed ovaries and the upper extremities of the cornua uteri, are most distinctly testicles, as shown by their numerous tortuous seminiferous tubes, which have been successfully filled with a mercurial injection. They are of the full size of the organ in the adult male. The seminiferous tubes of each testicle terminate in a vas deferens, which was injected from them; and the two vasa deferentia run downwards through the ligamenta lata of the uterus, and terminate inferiorly in the upper part of the vagina, thus following the course of those natural canals in the female sow that we shall afterwards have occasion to allude to at greater length under the name of Gaertner's ducts, and which Dr. Knox, from the evidence of the present case, believes to be in reality typical of the male vasa deferentia. There is no trace of vesiculæ seminales. Externally, the vagina opened along with the urethra upon the perineum, at a point lower than natural in the well-formed female. The clitoris, in situation and size, was nearly normal.

The animal, at the time of death, was fourteen months old; it was ferocious in its habits; and it had been in vain tried to be

fattened. It had repeatedly shown strong male propensities, and at the season of heat its vagina is said to have presented the usual injected appearance observed in the female sow.

Dr. Harlan of Philadelphia[1] has lately described a still more perfect case of double hermaphroditism than any of the preceding, which he met with in the body of a gibbon or orang-outang (*Simia concolor*) from the Island of Borneo. This animal died of tubercular disease in Philadelphia in 1826, when it was considered to be under two years of age. Dr. Harlan gives the following account of its

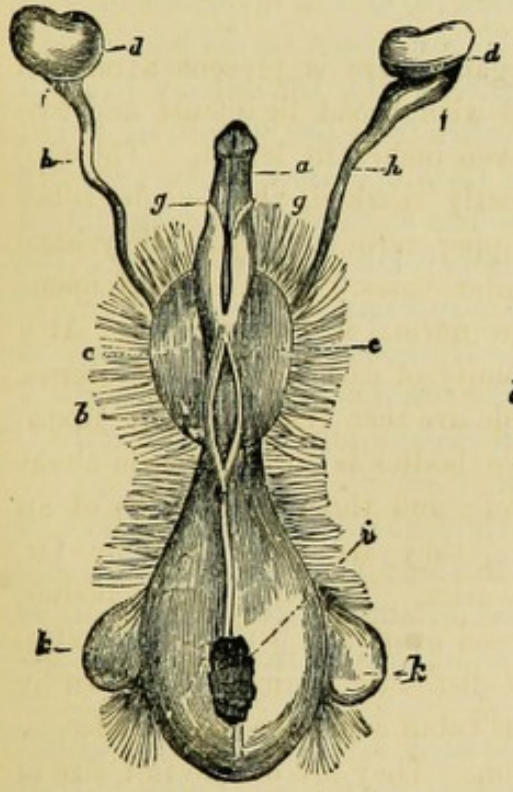


Fig. 18.

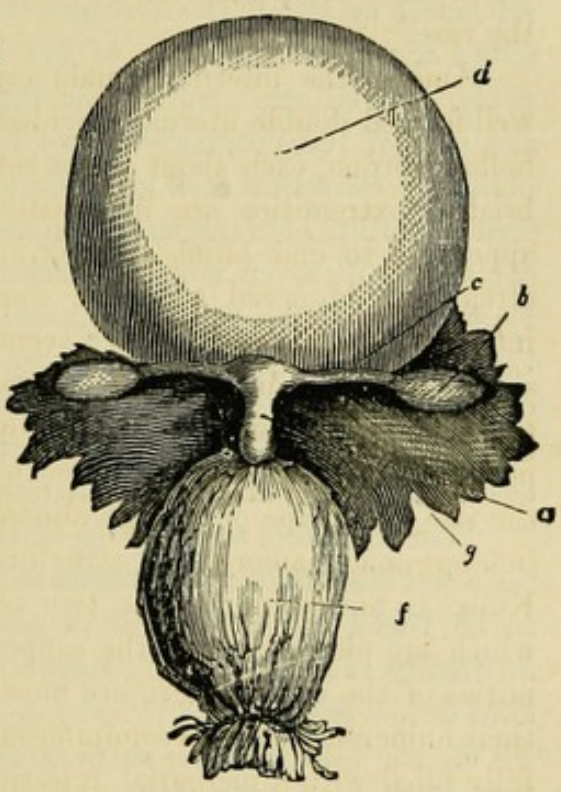


Fig. 19.

sexual formation. The penis (Fig. 18, *a*) was about one inch in length, and subject to erections; it terminated in an imperforate glans; and a deep groove on its inferior surface served as a rudimentary urethra. This groove extended about two-thirds of the length of the penis, the remaining proportion being covered with a thin reticular diaphanous membrane, which extended also across the vulva, *b*, and closed the external orifice of the vagina. The vagina

Fig. 18. External sexual organs and testicles; *g g*, the prepuce; *h h*, the vasa deferentia; *i*, the anus; *k k*, ischiatic protuberances.

Fig. 19. Internal sexual organs seen from behind; *d*, the urinary bladder; *f f*, rectum; *g g*, broad ligaments; *c c*, Fallopian tubes.

[1] *Med. and Phys. Researches*, p. 19.

was rather large, and displayed transverse striæ. Traces of the nymphæ and labia externa were visible. The meatus urinarius opened beneath the pubis into the vagina, but the urine must have been directed along the groove of the penis by the membrane obstructing the orifice of the vulva. The os tincæ was surrounded by small globular glands. The orifice and neck of the uterus admitted a large probe into the cavity of that organ, which appeared perfect with all its appendages. The round and broad ligaments, together with well-developed ovaries (Fig. 19, *bb*), were all found *in situ*. The scrotum (Fig. 18, *c*) was divided, and consisted of a sac on each side of the labia externa, at the base of the penis, covered with hair. The testicles (Fig. 18, *dd*) lay beneath the skin of the groin about two inches from the symphysis pubis, obliquely outwards and upwards ; they appeared to be perfectly formed with the epididymis, *ff*, etc. The most accurate examination could not discover vesiculæ seminales ; but an opening into the vagina, above the meatus urinarius, appeared to be the orifice of the vas deferens. In all other respects, the male and female organs of generation were in this animal as completely perfected as could have been anticipated in so young an individual, and resembled those of other individuals of a similar age.

Four instances are now on record of the alleged co-existence of male testicles and female ovaries in the human subject. It is greatly to be regretted, that some of the instances have not been so accurately examined in their anatomical characters as their importance demanded.

a. The first of these cases is detailed by Schrell.[1] It occurred in an infant who died when nine months old. The testes and all the other internal and external male organs were present and perfectly formed, with the exception of the prepuce of the penis, which seemed divided in front and rolled up. At the root of the large penis was a small vulva or aperture capable of admitting a pea, and provided with bodies having an appearance of labia and nymphæ. This vulva led into a vagina that penetrated through the symphysis pubis, and terminated in a nipple-like body or imperfect uterus, to which structures having a resemblance to the Fallopian tubes and ovaries were attached.

b. A second and still more doubtful case of the alleged existence of both testicles and ovaries in the human subject, was first published by Beclard.[2] The case was met with by M. Laumonier of

[1] Schenck's *Medic. Chirurg. Archiv*, Bd. i.

[2] *Bullet. de la Fac. de Méd.* 1815, p. 284 ; or *Dict. des Sc. Med.* xxi. p. 111.

Rouen, who injected and dissected the sexual parts, and deposited them in a dried state, along with a wax model representing them in their more recent condition, in the Museum of the School of Medicine at Paris. In the wax model, two female ovaries, with a uterus, vagina, external vulva, and large imperforate clitoris, are seen combined with two male testicles, the vasa deferentia of which terminate in the uterus at the place in which the round ligaments are normally situated; these ligaments themselves are wanting. The preparation of the dried sexual parts is far from being equally satisfactory, and, in its present imperfect condition at least, does not bear out by any means the complete double hermaphroditic structure delineated in the model.

c. Dr. Blackman has described and delineated[1] a case of hermaphroditic malformation in an adult human subject, which his statements place in the present division. The external genital organs were entirely those of a male, with the penis large and perforated, and the scrotum of a natural appearance, but without testes. Professor Ackley, of Cleveland Medical College, Ohio, dissected the body after death. Internally into the urethral canal, at the neck of the bladder, there opened a contracted vagina, which communicated above, through a normal os tincæ, with a well-formed female uterus. From the upper angles of the uterus there set off two perfect and permeable Fallopian tubes, which had at their fimbriated extremities two ovaries placed in their usual position. In addition there were found, according to Dr. Blackman's drawing and description, the following internal male organs, namely, two testicles, situated near the two ovaries, two vasa deferentia, and a prostate gland of the natural size and appearance. The vasa deferentia, or "excretory ducts of the testes, were perfect," to use Dr. Blackman's own words; but he omits to state how or where they terminated inferiorly. It is greatly to be regretted, also, that there is no account given of the internal structure of the two testes and two alleged ovaries—a defect which it is to be hoped, for the sake of science, that he or Professor Ackley will yet supply. The stature of this malformed being was large; the external configuration, with the exception of the hips, male; the beard moderate; the habits solitary, with a dislike to women; a discharge like menstruation occurred monthly, it is averred, by the urethra, and was attended with much suffering. Death occurred during one of these menstrual periods; and the vagina on dissection was found, according to

[1] *American Journal of the Medical Sciences* for July 1853, p. 66.

Dr. Blackman, with "its inner surface reddened; and its cavity contained menstrual blood."

d. Perhaps, however, the most complete and most accurately detailed case of double hermaphroditism hitherto found in the human subject, is one dissected by Professor Vrolik of Amsterdam in 1846.[1] In this remarkable instance there were the following male and female organs present—viz., on the left side, and near each other, a testicle and an ovary; on the right side these and other parts were less perfectly developed; besides there existed two rudimentary Fallopian tubes, and two comparatively more perfect vasa deferentia. There was no vestige of a male prostate gland or vesiculæ seminales; but there was found a female uterus and vagina opening inferiorly beneath the arch of the pubis into a narrow uro-genital canal, which ended externally at the root of an impervious but sulcated penis; and the labia externa were united so as to form an apparently perfect male scrotum, the right side of which was enlarged by the presence of an inguinal hernia. The uterus was in its normal situation between the bladder and rectum, but drawn obliquely over to the left or more highly developed side. It was provided and fixed with the usual ligamenta lata, and behind with the ligaments of Douglas. The cavity of the uterus was mucous and smooth on its internal surface, and it ran downwards—without any formation or contraction like the os tincæ and cervix uteri—into the long and narrow vagina. The walls of the upper portion of the vaginal canal were thick, and its mucous surface plicated; inferiorly this canal became thinner and more contracted, especially where it terminated in the urethra or common uro-genital tube. From the angles of the fundus uteri imperfect Fallopian tubes arose, and ran forwards and outwards with the vasa deferentia, etc., through the inguinal canals, and were lost upon the tunicæ vaginales. On the right side the vessels, etc., running to the ovary and testicle, were, as has been already stated, very indistinct; but the left testis and left ovary were more developed, and carefully examined by Professor Vrolik. The testis on this side, though small in size, was provided with a large tunica vaginalis. When divided, the organ did not show the tubuli seminiferi, but its characteristic transverse septa were seen. In a glutinous yellow fluid taken from its divided tissue, the microscope detected small nucleated cells, but no true spermatozoa. The testis was provided with its usual blood-vessels. The vas deferens, after leaving it, became very tortuous

[1] *Tabulæ ad illustrandum Embryogenesin Hominis et Mammalium*, tab. xciv. xcv.

for a part of its course, and then ran down by the side of the uterus and vagina, ultimately opening into the cavity of the latter. On the opposite and less developed side, it is worthy of remark that the more rudimentary right vas deferens opened into the angle of the uterus. Returning to the left ovary—this organ was found lying near the left testicle, and of an oblong form, and provided with its appropriate and characteristic *pampiniform* plexus of veins. The internal stroma of the ovary was, at the circumference of the organ, white, contracted, and indurated, as it is in advanced age, and no Graafian vesicles were visible. Further, near it was seen a congeries of tubes, analogous to the sketch given by Kobelt of the pro-ovarium. The subject of this malformation was 58 years old at the time of death. At 17 years of age, the testes began to descend through the inguinal rings, and a copious beard appeared upon the face. He had up to that time been considered a female, and he continued to dress as such till he was 38 years old. At that time, three medical men—of whom Professor Vrolik was one—examined him, and gave him a certificate that he was a hypospadiac or malformed male, in consequence of which he subsequently assumed the male attire—a conclusion which seemed to be further corroborated by the absence of menstruation, and the "magna in sexum sequiorem propensio." The autopsy showed, however, how difficult or indeed impossible it is, during life, to pronounce always decidedly and correctly on the actual sex of such malformed individuals.

HERMAPHRODITISM AS MANIFESTED IN THE GENERAL CONFORMATION OF THE BODY, AND IN THE SECONDARY SEXUAL CHARACTERS. INFLUENCE OF OVARIES AND TESTES UPON THEM.

In the preceding observations we have principally confined ourselves to the description of hermaphroditic malformations as seen in the resemblance in appearance and structure of the external genital parts of one sex to those of the other, and in the different degrees and varieties of reunion and co-existence of the reproductive organs of the two sexes upon the body of the same individual. Hermaphroditism, however, may appear not only in what are termed the *primary* sexual parts or characters, or, in other words, in the organs more immediately subservient to copulation and reproduction, but it may present itself also in the *secondary* sexual characters, or in those distinctive peculiarities of the sexes that are found in other individual parts and functions of the economy, as

well as in the system at large. We have occasionally an opportunity of observing some tendency to a hermaphroditic type in the general system, without there being in any very marked corresponding abnormality in the sexual organs themselves, but it rarely happens that there exists any hermaphroditic malformation of the primary organs of generation, without there being connected with it more or less of a hermaphroditic type in the secondary sexual characters; and this circumstance often offers us, in individual doubtful cases, a new and perplexing source of fallacy in our attempts to determine the true or predominating sex of the malformed individual. Before, however, describing that variety of hermaphroditism which manifests itself in the general system and in the secondary sexual peculiarities, it will be necessary, in order to understand its nature and origin, to premise a few remarks on the dependence and relation of these secondary characters upon the normal and abnormal conditions of the primary sexual organs.

That the various secondary sexual peculiarities which become developed at the term of puberty are intimately dependent upon the changes that take place at the same period in the organism of the female ovaries and male testicles, seems proved by various considerations, particularly by the effect produced by original defective development and acquired disease in these parts, and by the total removal of them from the body by operation. In considering this point, I shall speak first of the effects of the state of the ovaries upon the female constitution, and shall then consider those of the testicles upon the male.

When the usual development of the ovaries at the term of puberty does not take place, the secondary sexual characters which are naturally evolved in the female at that period do not present themselves; and this deficiency sometimes occasions an approach in various points to the male formation. Thus in a case recorded by Dr. Pears,[1] of a female who died of a pectoral affection at the age of twenty-nine, the ovaries on dissection were found rudimentary and indistinct, and the uterus and Fallopian tubes were present, but as little developed as before puberty. This individual had never menstruated, or showed any signs, either mental or corporeal, of puberty. The mammæ and nipples were as little developed as those of the male subject. She had ceased to grow at ten years of age, and had attained only the height of four feet six inches.

[1] *Phil. Trans.* for 1805, p. 225.

In another analogous instance observed by Renauldin,[1] scarcely any rudiments of the ovaries existed, and the body of the uterus was absent, but the external genital female organs were well formed. The individual who was the subject of this defective sexual development had never menstruated; the mammæ were not evolved; in stature she did not exceed three and a half French feet; and her intellect was imperfectly developed.

In reference to these and other similar instances that might be quoted,[2] it may be argued that they do not afford any direct evidence of the evolution of the sexual characters of the female depending upon that of the ovaries, as the arrestment in the development of both may be owing to some common cause which gives rise at the same time to the deficiency in the development of the genital organs, and to the stoppage of the evolution of the body in general. That the imperfection, however, in the organism of the ovaries may have acted in such cases as the more immediate cause or precedent of the imperfection or non-appearance of the secondary characters of the sex, seems to be rendered not improbable, in regard to some, if not to all of the instances alluded to, by the fact that the removal of these organs, before the period of puberty, as is seen in spayed female animals, entails upon the individuals so treated the same neutral state of the general organisation as was observed in the above instances; or, in other words, we have direct evidence that the alleged effect is capable of being produced by the alleged cause; and further, when, in cases of operation or disease after the period of puberty, both ovaries have happened to be destroyed, and their influence upon the system consequently lost, the distinctive secondary characteristics of the female have been observed also to disappear in a greater or less degree.

Thus, in the well-known case recorded by Mr. Pott,[3] the catamenia became suppressed, the mammæ disappeared, and the body became thinner and more masculine, in a healthy and stout young woman of twenty-three years of age, whose two ovaries formed hernial tumours at the inguinal rings, and were, in conse-

[1] *Séances de l'Acad. Roy. de Méd.* 28th February 1826, and *Medical Repository* for 1826, p. 78.

[2] Davis, in his *Principles and Practice of Obstetric Medicine*, p. 513, refers to several instances in point. We may mention that Dr. Haighton found that after the Fallopian tubes were divided in rabbits, the ovaries became gradually atrophied, and the sexual feelings were lost.—*Phil. Trans.* for 1787, p. 173.

[3] *Surgical Works*, vol. iii. p. 329.

quence of their incapacitating the patient from work, both removed by operation.

Many facts seem to show that the act of menstruation most probably depends upon some periodical changes in the ovaries, or rather in the Graafian vesicles of these organs or their contents; and when the function becomes suddenly and permanently stopped in a woman at the middle period of life, without any indications of the catamenial fluid being merely mechanically retained, we may perhaps suspect, with reasonable probability, the existence of a diseased state, which has destroyed, either successively or simultaneously, the functions of both ovaries. In such a case the distinctive secondary peculiarities of the female sex come to give place to those of the male. Thus Vaulevier mentions an instance in which menstruation suddenly ceased in a young and apparently healthy woman; no general or local disease followed; but soon afterwards a perfect beard began to grow upon her face.[1] Again, in women who have passed the period of their menstrual and child-bearing life, and in whom consequently the functions and often the healthy structure of the ovaries are lost or destroyed, we have frequently an opportunity of observing a similar tendency towards an assumption of some of the peculiarities of the male; an increase of hair often appears upon the face, the mammæ diminish in size, the voice becomes stronger and deeper toned, the elegance of the female form and contour of body is lost, and frequently the mind exhibits a more determined and masculine cast. Women, both young and aged, with this tendency to the male character, are repeatedly alluded to by the Roman authors under the name of *viragines;* and Hippocrates[2] has left us the description of two well-marked instances.

Among the females of the lower animals a similar approach to the male character in the general system not unfrequently shows

[1] *Journ. de Méd.* tom. lxix., and Meckel in Reil's *Arch.* Bd. xi. s. 275. Meckel quotes other similar cases from Seger in *Ephem. Nat. Cur.* Dec. i. ann. ix. and x. obs. 95; Vicat, sur la Plique Polonaise, in Murray's *Pr. Bibl.* Bd. i. s. 578; and Schurig's *Parthenologia*, p. 184. Burlin published an express treatise on the subject, *De Barbâ mulierum ex menstruorum suppressione*, Altorf, 1664. See also Haller's *Elem. Phys.* tom. v. p. 32; Reuss, *Repert. Comment.* tom. x. p. 205; Eble, *Die Lehre von den Haaren in der organischen Natur.* Bd. ii. s. 222, Vienna, 1831, and Mehliss, *Ueber Virilescenz und Rejuvenescenz thierischer Körper*, Leipzic, 1838, who quotes several cases additional to those of Meckel.

[2] *De Morb. Vulg.* lib. vi. ss. 55, 56. "Abderis Phætusa, Pythei conjunx antea per juventam fœcunda erat; viro autem ejus exulante, diu menses defecerunt, ex quo postea dolores et rubores ad articulos exorti sunt. Quæ ubi conti-

itself as an effect both of disease and malformation of the sexual organs, and also in consequence of the cessation of the powers of reproduction in the course of advanced age. Female deer are sometimes observed to become provided at puberty with the horns of the stag,[1] and such animals are generally observed to be barren,[2] probably in consequence either of a congenital or acquired morbid condition of their ovaries or other reproductive organs. In old age, also, after the term of their reproductive life has ceased, female deer sometimes acquire the horns of the male in a more or less perfect degree;[3] and Burdach alleges that roes sometimes become provided with short horns when they are kept from the male during the rutting season, and at the same time furnished with abundant nourishment.[4] Mehliss[5] alludes to two cases in which a virilescent type was shown principally in the hair of the female deer. In one of these instances the hair of the head, neck, and abdomen, the shape of the ears and extremities, and the odour of the animal, gave it the closest resemblance to the male, and it followed the other females as if urged by sexual desire.

This kind of acquired hermaphroditism in aged females has, however, been more frequently and carefully attended to as it occurs in Birds than as met with among the Mammalia, the change to virilescence in the former being more marked and striking than in the latter, owing to the great difference which generally exists between the plumage of the male and female.[6] When old female birds live for any considerable period after their ovaries have ceased to produce eggs, they are usually observed to assume gradually more or less of the plumage and voice, and sometimes the habits also, of the male of their own species. This curious fact, first pointed out

gerunt, tum corpus virile tum universum hirsutum est redditum, barbaque est enata, et vox aspera reddita. Sed cum omnia quæ ad menses deducendos facerent tentassemus, non profluxerunt, verum haud ita multo post vita functa est. Idem quoque in Thaso Namysiæ Gorgippi conjugi contigit."—*Hippocr. Op.* ed. Fœsii, p. 1201.

[1] Camden's *Angl. Norm.*, 1603, p. 821. Langelot, *Eph. Nat. Cur.* Dec. i. ann. ix. and x. obs. 88. Ridinger's *Abbild. Seltener Thiere*, taf. 79; or Meckel in Reil's *Archiv für die Physiol.* Bd. xi. p. 273.

[2] Wildungen, *Taschenbuch für Forst-und Jagd-freunde*, s. 17.

[3] Otto's *Path. Anat.* by South, p. 166, s. 123, n. 18, for list of cases.

[4] *Phys.* vol. i. § 183, p. 318.

[5] *Ueber Virilescenz thierisch. Körper;* or *British and Foreign Medical Review*, vol. vi. p. 77.

[6] It occurs also more frequently among birds than among mammalia, from the former possessing only a single ovary.

by Aristotle[1] in relation to the domestic fowl, has now been seen to occur in a number of other species of birds, but particularly among the Gallinaceæ. It has been in modern times remarked in the common fowl (*Phasianus gallus*) by Tucker, Butler, and Jameson; in the common pheasant (*P. colchicus*) by Hunter and Isidore St. Hilaire; in the golden pheasant (*P. pictus*) by Blumenbach and St. Hilaire; in the silver pheasant (*P. nyctemerus*) by Bechstein and St. Hilaire; in the turkey (*Meleagris*) by Bechstein; in the pea-hen (*Pavo*) by Hunter and Jameson; and in the partridge (*Tetrao perdrix*) by Montagu and Yarrell. Among the Cursores it is mentioned as having occurred in the bustard (*Otis*) by Tiedemann, and in the American pelican (*Platalea aiaia*) by Catesby. In the order Palmipedæ it has been observed by Tiedemann and Rumball in the domestic and wild duck (*Anas boscha*). Among the Scansores it has been seen in the cuckoo (*Cuculus canorus*) by Payrandeau; and among the Passeres in the cotinga (*Ampelis*) by Dufresne; in the chaffinch (*Fringilla*) and *rougequeue* (*Motacilla*) by Prevost; and in the bunting (*Emberiza paradisea* and *longicauda*) by Blumenbach.

This change of plumage in old female birds commences, according to M. Isidore St. Hilaire,[2] much sooner in some instances than in others; it may only begin to show itself several years after the bird has ceased to lay, though depending more or less directly upon this phenomenon, and sometimes it commences immediately after it. The change may be effected in a single season, though in general it is not complete for some years. When it is perfected, the female may display not only the variety of colours, but also the brilliancy, of the male plumage, which it sometimes resembles even in its ornamental appendages, as in the acquisition of spurs, and, in the domestic fowls, of the comb and wattles of the cock. The voice of the bird is also very generally changed. Its female habits and instincts are likewise often lost; and, in some instances, it has been seen to assume in a great degree those of the male, and has even been observed to attempt coition with other females of its own species.[3] In most of the female birds that have undergone this

[1] "Gallini, cum vicerint gallos, concurrunt maresque imitandi subagitare conantur. Attollitur etiam crista ipsis, simul et clunes (uropygium); adeo ut jam non facile diagnoscantur an fœminæ sint. Quibusdam etiam calcaria parva surriguntur."—*Hist. Animal.* lib. ix. cap. 36.

[2] *Edinburgh Jour. of Philosoph. Science*, 1826, p. 308.

[3] Rumball, in Home's *Comparative Anatomy*, vol. iii. p. 330, states having observed an old duck, which had assumed the male plumage, attempt sexual connection with another female. This may perhaps enable us to understand the

change, the ovary has been found entirely or partially degenerated, though in a few cases the morbid alteration is not very marked, eggs having even been present in the organ in one or two instances. In general, however, it is greatly diminished in size, or has become altogether atrophied; but the perfection of the change in the plumage does not seem to bear any direct ratio with the degree of morbid alteration and atrophy in the ovary.

That the change towards the male type, described as occasionally occurring in old female birds, is directly dependent, not upon their age, but upon the state of their ovaries, seems still further proved by similar changes being sometimes observed in these females long previous to the natural cessation of the powers of reproduction, in consequence of their ovaries having become wasted or destroyed by disease. Greve,[1] in his *Fragments of Comparative Anatomy and Physiology*, states that hens whose ovaries are scirrhous crow sometimes like cocks, acquire tail-feathers resembling those of the male, and become furnished with large spurs. The same author mentions also the case of a duck, which, from being previously healthy, suddenly acquired the voice of the male, and on dissection whose ovary was found hard, cartilaginous, and in part ossified.

Mr. Yarrell, in a paper read before the Royal Society in 1827,[2] has stated that in a number of instances he had observed young female pheasants with plumage more or less resembling the male, and in all of them he found on dissection the ovaries in a very morbid state, and the oviduct diseased throughout its whole length, with its canal obliterated at its upper part. He also shows that a similar effect upon the secondary sexual characters of the female bird is produced by the artificial division and removal of a small portion of their oviduct in the operation of making capons of female poultry; and he states that his investigations have led him to believe that in all animals bearing external characters indicative of the sex, these characters will undergo a change and exhibit an appearance intermediate between the perfect male and female, wherever the system is deprived of the influence of the true sexual

reputed cases of hermaphroditism in women, who, as related by Mollerus (*Tract. de Hermaphr.* cap. ii.) and Blancard (*Collect. Medico-Phys.* cent. iii. obs. 80), after having themselves borne children, became addicted to intercourse with other females. Of course we cannot give our credence to the alleged successful issue of such intercourse.

[1] *Bruchstücke zur vergleich. Anat. und Physiol.* s. 45.

[2] *Phil. Trans.* for 1827, part ii. p. 268.

organs, whether from original malformation, acquired disease, or artificial obliteration.[1]

From the frequency with which castration is performed, the effects of the testicles in evolving the general sexual peculiarities of the male have been more accurately ascertained than those of the ovaries upon the female constitution. These effects vary according to the age at which the removal of the testicles takes place. When an animal is castrated some time before it has reached the term of puberty, the distinctive characters of the male are in general never developed; and the total absence of these characters, together with the softness and relaxation of their tissues, the contour of their form, the tone of their voice, and their want of masculine energy and vigour, assimilate them more, in appearance and habits, to the female than to the male type. If the testicles are removed nearer the period of puberty, or at any time after that term has occurred, and when the various male sexual peculiarities have been already developed, the effect is seldom so striking; the sexual instincts of the animal, and the energy of character which these instincts impart, are certainly more or less completely destroyed, and the tone of the voice is sometimes changed to that of puberty; but the general male characteristics of form, such as the beard in man, and the horns in the Ruminantia, most commonly continue to grow. In animals, such as the stag, which possess deciduous horns, the removal of the testicles during the rutting season causes the existing horns to be permanent; and if the operation is performed in an adult animal when out of heat, no new horns in general appear.[2] In the ox, the effect of castration upon the growth of the horns, even when performed before the time of puberty, is quite remarkable; for, instead

[1] On old or diseased female birds assuming the plumage, etc., of the male, see J. Hunter, *Observ. on the An. Econ.* p. 75; E. Home, *Lect. on Comp. Anat.* vol. iii. p. 329; Mauduit, in *Encycl. Method.*, Art. "Faisan," tom. ii. p. 3; Butter, in *Wernerian Soc. Mem.* vol. iii. p. 183; Schneider's Notes, in his edition of the Emperor Frederick the Second's Treatise *De Arte Venandi cum Avibus;* Tucker's *Ornithologia Damnoniensis;* Catesby's *Natural History of Carolina*, etc., i. t. 1; Bechstein, *Naturgeschichte der Deutschlands*, Bd. ii. sec. 116; Blumenbach, *De Anomalis et vitiosis quibusdam nisûs formativi aberrationibus*, p. 8, and *Instit. of Physiology*, p. 369; Payrandeau, *Bull. des Sc. Nat.* tom. xiii. p. 243; Tiedemann, *Zoologie*, vol. iii. p. 306; Geoff. St. Hilaire, *Phil. Anat.* tom. ii. p. 360; Isid. St. Hilaire, *Mém. du Mus. d'Hist. Nat.* tom. xii. p. 220; *Annal. des Sc. Nat.* tom. vii. p. 336, or *Edinburgh New Philosophical Journal* for 1826, p. 302, with additional cases by Professor Jameson, p. 309; Kob, *De mutatione sexûs*, p. 11, Berlin, 1823; Yarrel, *Phil. Trans.* for 1827, p. 268, with a drawing of the diseased ovaries, etc.

[2] Buffon, *Hist. Nat.* tom. vi. p. 80.

of having their development altogether stopped, or their size at least diminished by the operation, as occurs in the ram and stag, the volume of these appendages is even increased by it, the horns of the ox being generally larger but less strong than those of the entire bull. Castration in the boar causes, according to Greve,[1] the tusks to remain small, and prevents altogether the replacement of the teeth. This author also states that the same operation on the horse prevents the full development of the neck, renders the teeth smaller and slower in their growth, increases the growth of the hair, and the size of the horny protuberances on the inside of the legs. The prostate gland, he further alleges, as well as the vesiculæ seminales, become augmented as much as a third in their volume in consequence of the operation.[2]

The removal of the testicles both before and after the period of puberty commonly gives rise to another singular effect—to an increased deposition of fat over the body,[3] and from this circumstance the general form of the body, and in man that of the mammæ, is sometimes modified in a degree that increases the resemblance to the opposite sex. In the sterile of both sexes in the human subject an unusual corpulency is not uncommon, and the same state is often met with in old persons, and particularly in females, after the period of their child-bearing life is past.

The nature of the effects produced by the existence and functional activity of the testicles and ovaries upon the development of the secondary sexual characters of the male and female, may be further illustrated by what occurs in the season of heat to animals such as the deer, sheep, birds, etc., that have periodical returns of the sexual propensity. At these periods all the distinctive general characters of the sexes become much more prominently developed, in conjunction with, and apparently in consequence of, the changes which have been ascertained by observation to occur at that time in the relative size and activity of the internal organs of generation. Thus with the return of the season of sexual instinct, the dorsal crests and cutaneous earlobes of tritons enlarge; in Batrachian reptiles the spongy inflations of the thumbs become increased in size; the various species of singing birds re-acquire their vocal powers; and some, as the cuckoo and quail, appear capable of exercising their voice only at this period of the year. At the pairing season also the plumage of birds becomes brighter in tint, and in some instances

[1] *Bruchstücke zur Vergl. Anat. und Physiol.* p. 41. [2] *Ibid.* p. 45.

[3] See *Cyclop. of Anat. and Phys.*, Art. "Adipose Tissue."

is in other respects considerably changed, as in the male ruff (*Tringa pugnax*), which then re-assumes the tuft of feathers upon his head and neck, and the red tubercles upon his face, that had fallen off during the moulting, and thus left him more nearly allied in appearance to the female during the winter. In reference to this subject, it appears to us interesting to remark, that in certain birds, as in the different species of the genus Fringilla, the male presents in winter a plumage very similar to that of the female,[1] and in the present inquiry it is important to connect this fact with the very diminutive size and inactive condition of the testicles of these birds at that season.

From the remarks that we have now made upon the influence of the ovaries and testicles in developing the general sexual peculiarities of the female and male, it will be easy to conceive that when, in cases of malformation of the external genital organs giving rise to the idea of hermaphroditism, there is at the same time, as sometimes happens, a simultaneous want of development in the internal organs of reproduction, particularly in the ovaries and testicles, the general physical and moral peculiarities distinctive of the sex of the individual may be equally deficient, or have a tendency even to approach in more or fewer of their points to those of the opposite sexual type. In this way we may, it is obvious, have *general* or constitutional hermaphroditic characters, if they may be so termed, added to those already existing in the *special* organs of generation, and rendering more difficult and complicated the determination of the true sex of the malformed individual. Some cases of spurious hermaphroditism in the male, published by Sir E. Home,[2] may serve to illustrate this remark.

A marine soldier, aged twenty-three, was admitted a patient into the Royal Naval Hospital at Plymouth. He had been there only a few days, when a suspicion arose of his being a woman, which induced Sir Everard to examine into the circumstances. He proved to have no beard; his breasts were fully as large as those of a woman at that age; he was inclined to be corpulent; his skin was uncommonly soft for a man; his hands were fat and short, and his thighs and legs very much like those of a woman; the quantity of fat upon the os pubis resembled the mons veneris; and in addition he was weak in his intellect, and deficient in bodily strength. The external genital organs showed him to be a male, but the penis was

[1] Stark's *Elements of Nat. Hist.*, vol. i. p. 243.

[2] *Comp. Anat.* vol. iii. p. 320.

unusually small, as well as short, and not liable to erection; the testicles were not larger in size than they commonly are in the fœtal state; and he had never felt any passion for the opposite sex.

The following cases by the same author strongly illustrate this subject.[1] In a family of three children residing near Modbury in Devonshire, the second, a daughter, was a well-formed female, the eldest and youngest were both malformed males. The eldest was thirteen years of age. His mons veneris was loaded with fat; no penis could be said to be present, but there was a præputium a sixth of an inch long, and under it the meatus urinarius, but no vagina. There was an imperfect scrotum with a smooth surface, there being no raphé in the middle, but, in its place, an indented line; it contained two testicles, of the size met with in the fœtus. His breasts were as large as those of a fat woman. He was four feet high, and of an uncommon bulk, his body round the waist being equal to that of a fat man, and his thighs and legs in proportion. He was very dull and heavy, and almost an idiot, but could walk and talk; he began to walk when a year and a half old. The younger brother was six years old, and uncommonly fat and large for his age. He was more an idiot than the other, not having sense enough to learn to walk, although his limbs were not defective.

A case in a similar manner confirmatory of the preceding remarks is mentioned by Itard de Riez.[2] A young man, aged twenty-three, had no testes in the scrotum, a very small penis, not capable of erection, and a divided scrotum. He was in stature below the middle size. His skin was soft, smooth, and entirely free from hair, the place of the beard being supplied by a slight down. The voice was hoarse; the muscles were not well marked; the form of the chest resembled that of the female, and the pelvis was extremely broad and large. The intellectual faculties were very dull, and the sexual appetite was entirely wanting.

Renauldin, also, in the same work,[3] has recorded another case in point. In a soldier of twenty-four years of age, whose genital organs were extremely undeveloped, his penis being only of the size of a small tubercle, and his testicles not larger than small nuts, the pelvis was broad; the chest narrow; the face and body in general were not covered with hair, with the exception of a small quantity upon the pubis; the voice was feminine, and the mammary glands were as

[1] *Comp. Anat.*, pp. 320-21.

[2] *Mémoires de la Société Méd. d'Emulation*, tom. iii. pp. 293-95.

[3] *Ibid.* tom. i. p. 241.

perfectly developed as in the adult female. The body of this individual was rather lean than otherwise. The mammæ had begun to enlarge when his body attained to its full stature at sixteen years of age. He had all the habits and sexual desires of the male sex.

In quadrupeds, as in man, when the testicles or ovaries are imperfectly formed, the secondary sexual peculiarities are frequently so defectively evolved as to offer a kind of hermaphroditic or neutral type in the general configuration and characters of the animal. Thus, the free-martin does not present an exact analogy in form either with the bull or cow, but exhibits a set of characters intermediate between both, and more nearly resembling those of the ox and of the spayed heifer. In size it resembles the castrated male and spayed female, being considerably larger than either the bull or the cow, and having horns very similar to those of the ox. Its bellow is similar to that of the ox, being more analogous to that of the cow than of the bull. Its flesh, like that of the ox and spayed heifer, is generally much finer in its fibre than the flesh of either the bull or cow, and is supposed to exceed even that of the ox and heifer in delicacy of flavour.[1]

The consideration of the various facts that we have now stated inclines us to believe that the natural history characters of any species of animal are certainly not to be sought for solely either in the system of the male or in that of the female; but, as Mr. Hunter pointed out, they are to be found in those properties that are common to both sexes, and which we have occasionally seen combined together by nature upon the bodies of an unnatural hermaphrodite, or evolved from the interference of art upon a castrated male or spayed female. In assuming at the age of puberty the distinctive secondary peculiarities of his sex, the male, so far as regards these secondary peculiarities, evidently passes into a higher degree of development than the female, and leaves her more in possession of those characters that are common to the young of both sexes, and which he himself never loses when his testicles are early removed. These and other facts connected with the evolution of both the primary and secondary peculiarities of the sexes further appear to us to show that, physiologically at least, we ought to consider the male type of organisation to be the more perfect as respects the individual, and the female the more perfect as respects the species. Hence we find that, when females are malformed in the sexual parts so

[1] Hunter's *Obs. on the An. Econ.*, p. 60

as to resemble the male, the malformation is almost always one of excessive development, as enlargement of the clitoris, union of the labia, etc.; and, on the other hand, when the male organs are malformed in such a manner as to simulate the female, the abnormal appearance is generally capable of being traced to a defect of development, such as the want of closure of the perineal fissure, and of the inferior part of the urethra, diminutive size of the penis, retention of the testicles in the abdomen, etc. In the same way, when the female assumes the secondary characters of the male, it is either, first, when by original malformation its own ovaries and sexual organs are so defective in structure as not to be capable of taking a part in the function of reproduction, and of exercising that influence over the general organisation which this faculty imparts to them; or, secondly, when in the course of age the ovaries have ceased to be capable of performing the action allotted to them in the reproductive process. In both of these cases we observe the powers of the female organisation, now that its capabilities of performing its particular office in the continuation of the species are wanting or lost, expanding themselves in perfecting its own individual system; and hence the animal gradually assumes more or fewer of those secondary sexual characters that belong to the male.

We do not consider it subversive of the preceding view to qualify it with the two following admissions—1*st*, that owing to the energies of the female system being so strongly and constantly directed towards the reproductive organs, and the accomplishment of those important functions which these organs have to perform in the economy of the species, the general characters of the species may be developed in her body in a degree *less* than they otherwise would be, or than actually constitutes the proper standard of the species; and, 2*dly*, that, in consequence of the peculiarities of the sexual functions of the female, some of the individual organs of her system, as the mammæ, are evolved in a degree *greater* than is consonant with the standard characters of the species. At the same time we would here remark that the occasional enlarged condition of the mammæ in hermaphrodites in whom the male sexual type of structure predominates (as in the examples of spurious male hermaphrodites that have been quoted from Sir E. Home, and in those mentioned by Renauldin, Julien, Petit, Rullier, and others in the human subject, as well as in numerous cases among hermaphrodite quadrupeds), would almost seem to show that the full development of the mam-

mary glands is a character proper to the species in general, rather than one peculiar to the female system alone. In males, also, who are perfect in their reproductive organs and functions, the mammæ are sometimes observed to be developed in so complete a manner as to be capable of secreting milk, forming what may be regarded as one of the slightest approaches towards hermaphroditic malformation in the male organisation ;[1] and the mammæ of the infants of both sexes not unfrequently contain a lactiform fluid at birth.

LATERAL HERMAPHRODITISM, ETC., IN THE EXTERNAL OR SECONDARY SEXUAL CHARACTERS OF THE BODY.

In some instances of hermaphroditic malformation, the total form and configuration of the body have been alleged to present not only a general tendency towards the physical secondary characters of the opposite sex, or to exhibit in a permanent state the neutral condition existing before puberty, but different individual parts of it have been occasionally conceived to be developed after a different sexual type. Thus, for instance, we have already mentioned in regard to Hubert Jean Pierre, that the upper half of the body of this individual seemed formed after the female, and the lower half after the male type, the larynx and mammæ being quite feminine, the face showing no appearance of beard, and the arms being delicate and finely rounded, while the pelvis was narrow, and the thighs were marked and angled as in man. In a case described by Schneider[2] the reverse held true, the bust being male with a strong beard and large thorax, and the pelvis being large and distinctly female. A more mixed combination of the secondary sexual characters has been already described as existing in the cases detailed by Ricco, Mayer, Arnaud, Bouillaud, etc. etc.

[1] The secretion of milk in the mammary glands of the male is occasionally observed amongst our domestic quadrupeds. See Gurlt's *Pathologischen Anatomie der Haus-Saugthiere*, Bd. ii. s. 188 ; Blumenbach in the *Hanoverisch Magazin* for 1787 ; and Home in *Comp. Anat.* iii. p. 328. Among the recorded instances and observations upon it in man we may refer to Paullini, *Cynographia*, p. 52 ; Schacher, *De Lacte Virorum et Virginum*, Leipz. 1742 ; Sinnibaldus, *Geneanthrop.* tom. iv. p. 456 ; Alex. Benedictus, *Anatom. Corp. Hum.* lib. iii. p. 595 ; Winslow, *Anatomy*, vol. ii. p. 214 ; Deusing, *De Lacte*, p. 327 ; Kyper, *Anthropologia*, lib. i. p. 490 ; Buffon, *Hist. Nat.* tom. ii. p. 543 ; Bishop of Cork, *Phil. Trans.* vol. xli. p. 813 ; Humboldt, *Personal Narrative*, vol. iii. p. 57 ; Franklin, *First Expedition to the Polar Seas*, London, 1823, p. 157.

[2] Kopp's *Jahrbuch der Staatsarzneikunde*, Bd. x. s. 134.

One side of the body has been sometimes observed to be apparently formed in one or more of its parts on a sexual type different from that of the same parts on the opposite side. Girald, in his *Topography of Ireland*,[1] mentions a reputed female, who had the right side of the face bearded like that of a man, and the left smooth like that of a woman. Mr. King[2] has described an interesting instance of hermaphroditic malformation in an individual whose general character was masculine, but with the pelvis large and wide; the left testicle only had descended into the groin, and the mamma of this side was small comparatively to that of the opposite or right side.

In a hind mentioned by Mr. Hay,[3] and which, he believed, had never produced any young, one of the ovaries on dissection after death was found to be scirrhous. The animal had one horn resembling that of a three-years-old stag on the same side with the diseased ovary; there was no horn on the opposite side. Bomare[4] has given a similar case in the same animal, where a single horn was present, situated also on the same side with the diseased and degenerated ovary; and Russell[5] states, as the result of his experiments on castration in the deer, that when he removed one testicle only from the animal, the horn on the opposite side was the more completely developed of the two. Azara[6] observed in two birds the right side of the tail to possess the characters of the male, and the left those of the female.

In the hermaphroditic lobster previously alluded to as described by Nicholls, the general external configuration of the body was, like that of the sexual organs, perfectly female on one side, and perfectly male on the other.

It is principally, however, among hermaphroditic insects that a difference of sexual type in the general conformation of the opposite sides of the body, and of its individual parts, has been observed; and this malformation is the more striking and easy of observation in this class of animals, on account of the great differences in colour, size, and form respectively presented by the antennæ, wings, and other parts of the body, of the males and females of the same species.

[1] *Topog. Hiberniæ*, in *Camden's Angl.*, etc., 1603, part ii. p. 724.
[2] *London Medical Repository* for 1820, vol. xiii. p. 87.
[3] *Linnæan Transactions*, vol. iii. p. 356. [4] *Journ. de Phys.* tom. vi. p. 506.
[5] *Economy of Nature in Glandular Diseases.*
[6] Kob's *Dissert. de Mutatione Sexûs*, p. 19.

Lateral hermaphroditism of the body in Insects has been most frequently observed by entomologists amongst the class Lepidoptera. It has now been remarked in the following species:—In the *Argynnis paphia*, *Lycæna alexis*, *Saturnia pyri*, *Endromis versicolor*, and *Harpya vinula* (Ochsenheimer); in the *Gastrophaga medicaginis* and *Lycæna adonis* (Rudolphi); in the *Liparis dispar* (Schaefer, Ochsenheimer, and Rudolphi); in the *Saturnia Carpini* (Capieux, Ochsenheimer, and Rudolphi); in the *Gastrophaga quercifolia* (Hettlinger and Rudolphi); in the *Gastrophaga pini* (Scopoli); in the *Gastrophaga cratægi* (Esper); in the *Sphinx convolvuli* (Ernst); *Sphinx populi* (Fischer and Westwood); *Papilio polycaon* (Macleay); *Polyommatus alexis* (Entomolog. Mag., vol. iii. p. 304); *Bombyx castrensis* (Duval); in the *Argynnis paphia* (Allis); in the *Vanessa atalanta* (Schrank and Germar); and in the *Vanessa antiopha* and *Deilephila euphorbiæ* (Germar). Klug and Germar have recorded two instances of it among the Coleoptera—the former in the *Lucanus cervus*, and the latter in the *Melolontha vulgaris;* and Mr. Westwood mentions a third case in the large water-beetle (*Dytiscus marginalis*), as contained in Mr. Hope's collection, any has seen a fourth in the stag-beetle (*Lucanus cervus*).

Out of twenty-nine recorded cases of lateral hermaphroditism in Insects, in which the sexual characters of each side are distinctly specified, I find that in seventeen instances the right side was male, and in twelve female. Burmeister alleges that in by far the majority of cases the right side is male, and the left female—a statement in which Meckel coincides, while Westwood maintains the reverse. The cases I have myself collected are certainly numerically in favour of the former opinion, but the data are as yet so few, and the difference so trifling, as not to warrant any decided conclusion on this point.

In some instances we find among insects an *imperfect* lateral hermaphroditism consisting of some parts of one side, as of one or more of the wings, palpi, or antennæ, being formed according to a different sexual type from the same parts of the opposite side, and from the general body of the animal. Thus in the *Melitæa* described and dissected by Klug (see page 434), the general form of the insect was male, but the left eye, palpus, antenna, and left sexual fang, were smaller than in individuals belonging to this sex; the left antenna was annulated with white and yellow at the apex, while the right was of one colour; the general form of the abdomen was male, but somewhat thick, and the wings were all equal and male.

In a *Pontia duplidice* mentioned by Rudolphi, and which in its general external characters was female, the right anterior wing was formed after the male type, and the sexual organs also resembled those of the male.

Ochsenheimer mentions a *Gastrophaga quercus* with the body and the antennæ and wings on the left side female, and the right wings male; and a second with the body and the right side female, and the left side and two antennæ male, the latter being brown and pectinated.

In this imperfect variety of lateral hermaphroditism, the malformed wing, antenna, or palpus, is sometimes formed after one sexual type and coloured after another. In a male *Melitæa phœbe* noticed by Germar, the right wings and antenna were female in regard to size, but male in respect to colouring and markings. In a female *Deilephila galii* he found the left antenna and palpus of the small size of the male, but agreeing in colouring and markings with the corresponding female parts on the right side. In a *Pontia caradmines*, which was male in all its other characters, Ochsenheimer observed the right superior wing marked as in the female, and he mentions another individual of the same species which had a female form with some male colours.

In another variety of insect-hermaphroditism the sexual difference is sometimes, as we have already noticed in regard to the human subject, expressed not by a lateral but by a longitudinal sexual antagonism, or, in other words, the anterior and posterior parts of the body are formed after the two opposite sexual types. Thus in a *Saturnia carpini* described by Ochsenheimer, the antennæ were male, the superior wings male in form, but coloured as in the female, and the posterior wings, with the exception of a reddish brown spot upon the left, were, with the body and other parts, female.

Lastly, in a third variety of external hermaphroditic conformation in Insects, we find the characters of the two sexes mixed up and crossed in different irregular combinations upon the body of the same individual. In a *Gastrophaga castrensis* described by Rudolphi, and where the male type predominated, with a tendency, however, in all parts to the female form, the right antenna and the wings on the opposite or left side were distinctly female, while the left antenna and right wings were entirely male, the latter being only somewhat larger than in male insects, and the colours brighter than in the female. In a *Bombyx castrensis* alluded to by Westwood, the

wings on the right side, and the antennæ and abdomen of the left, were those of a male, while the left wing, right antennæ, and right side of the abdomen, were those of a female.

GENERAL SUMMARY WITH REGARD TO THE NATURE OF THE VARIETIES OF SPURIOUS HERMAPHRODITISM.

On some of the varieties of spurious hermaphroditism it is unnecessary for us to dwell here. For instance, the first species of spurious male hermaphroditism, or that arising from extroversion of the urinary bladder, is acknowledged by all to be merely one of the many varieties of arrested development or conjunction in the median line of the body, and so need not detain us. Two other forms—namely, the second female species consisting of prolapsus of the uterus, and the second male consisting of an adhesion of the penis to the scrotum—seem both referable to the head rather of disease than of original malformation. This latter indeed appears in all probability only an effect or result of adhesive inflammatory action in the affected parts during embryonic or fœtal life. Both of the two remaining forms of spurious hermaphroditism—viz., those consisting of hypospadiac fissure of the urethra, scrotum, and perineum in the male, and of abnormal magnitude of the clitoris in the female—seem readily explicable upon the doctrine of arrestment and abnormality in the development of the malformed parts.

We have already described at sufficient length the process of development of the different copulative organs, and have shown that those various degrees of hypospadiac malformation which constitute the common form of spurious hermaphroditism in the male, may be traced to arrestment of this process at various periods or stages of its progress. And we may here remark that the earlier this arrestment occurs, the distinction of the true sexual type of the malformed organs will always be the less marked, because the younger the embryo—and, on a similar principle, the lower we descend in the scale of animal existence—we find the differences between the organs of the two sexes proportionately the less pronounced, until at last we arrive at that primitive type in which these organs present altogether a common, neutral, or indeterminate character.

We have also already shown that at a certain early stage of the development of the female organs, the female clitoris holds the same, or nearly the same, relatively larger size to the whole embryo as the penis of the male, and that so far we may consider the occasional

occurrence of spurious hermaphroditism from magnitude of the clitoris, and its resemblance in this respect to the male organ, as a permanent condition of a type of embryonic structure that is normally of a temporary or transitory existence only. But besides this permanence of the embryonic type of the clitoris, we must further, in all the more complete instances of spurious female hermaphroditism, admit an excess of development in the malformed external sexual parts, and more particularly in the line of the median reunion of the two primitive lateral halves or divisions of these parts. In this way the vulvar orifice of the vagina (a remnant in the female of the primitive perineal cleft or fissure) is often in such cases more or less contracted and closed, so much so indeed in some instances as to leave only, as in the male, a small canal common to the genital and urinary passages. If the median junction is extended still farther, this canal comes also to imitate the male urethra in this respect, that it is united or shut up *below* in such a way as to be carried onward to a greater or less length, and in a more or less perfect condition along the under surface of the enlarged clitoris; and occasionally, as in some of the cases we have previously described, the male type of structure is still more completely repeated in the female organisation by the median reunion of the two labia, giving the appearance of the united scrotum and closed perineum of the opposite sex.

If we divide the whole sexual apparatus of the male and female into three corresponding transverse spheres or segments—the first or deep parts including the testicles and ovaries; the second or median comprehending the male seminal canals and prostate gland, and the female oviducts and uterus; and the third or external embracing the copulating organs of the two sexes—we shall find that, relatively speaking, the deep and the external spheres are naturally most developed in the male economy; while the median, comprising the uterus (the principal and most active organ in the female reproductive system), is developed in the greatest degree in that sex. In malformed females presenting a spurious hermaphroditic character, this important portion of the female sexual organisation is, in general, either itself in some respects malformed, or, from the structure of the other parts of the sexual apparatus being imperfect, its specific importance in the economy is cancelled, and therefore the energy of development takes the same direction as in the male, being expended upon the more complete evolution of the organs of the external and deep spheres. Hence the greater size of

the clitoris, and the greater development, which we have just now pointed out, in the median line of reunion of the external sexual parts; and hence also the occasional though rare occurrence, in the same cases, of the descent of the ovaries through the inguinal rings into the labia—an anomaly that certainly consists in a true excess of development, and which we cannot but regard as interesting, both in this respect, and as affording a new point of analogy between these organs themselves and the male testicles.

There is another and equally interesting point of view in which we may look upon this subject. Not only are the forms of spurious hermaphroditism which we have been considering capable of being traced backward to certain transitory types of sexual structure in the embryos of those animal species in which the malformations in question occur, but they may be shown also to present in their abnormal states repetitions of some of the normal and permanent conditions of the sexual organs in various species of animate beings placed lower in the scale of life. Thus the occasionally imperforate penis of the male hermaphrodite has been supposed to have an analogue in the naturally solid penis of the species of some of the genera *Doridium* and *Hyalæa*.[1] Its more or less grooved or hypospadiac condition is similar to the natural type of the same part in some hermaphrodite Mollusca, as in the *Planorbis* and *Murex*:[2] in its occasional diminutive size it approaches the general smallness of the partially fissured penis of most birds and reptiles; and we find it in the Rodentia and Marsupiata tied down by a short prepuce in a way analogous to what is seen in some cases of severe hypospadias. In the sloth (*Bradypus trydactylus*) the penis is small and grooved in its lower surface, and has the urethra opening at its base;[3] and in several of the male Rodentia the scrotum is also cleft, and has its two opposed surfaces smooth, humid, and free from hair, as in most cases of hypospadiac hermaphroditism in man. In Ophidian and in most Saurian Reptiles, the male seminal ducts open at once externally, as in some male hermaphrodites, at the root of the fissured penis.

The fact of the testicles sometimes remaining, in cases of hermaphroditic formation in the human subject, within the cavity of the abdomen, presents to us in a permanent state their original but changeable position in the early fœtus, and at the same time affords

[1] Burdach's *Physiologie*, Bd. i. § 132, p. 231.

[2] Tiedemann's *Zeitschrift für Physiologie*, Bd. i. s. 15; or Cuvier, *Anat. Comp* tom. v. p. 182.

[3] Meckel, *Beiträge zur vergleichenden Anatomie*, Bd. ii. cap. i. p. 125.

a repetition of their normal situation in almost all the lower tribes of animals, and in the Cetacea, Amphibia, Edentata, and some Pachydermata, as the Cape Marmot (*Hyrax*) and Elephant among the Mammalia.

The malformed clitoris in instances of spurious hermaphroditism assumes also, in its abnormal state, types of structure that we find as the normal condition of the organ in various inferior animals. Thus in female Cetacea and Rodentia, and in the animals included in Cuvier's order of Carnassiers, but more particularly among the Quadrumana, the clitoris retains as its permanent normal type that relatively larger size which we observe in the early fœtus, and in female hermaphrodites, in the human subject; and further, as is sometimes seen in such malformed individuals, the clitoris becomes partially traversed by the urethra, as in the Ostrich, Emu,[1] and Ant-eater;[2] and in the Loris, as we have noticed in a preceding page, and Maki, it is completely enclosed, like that of the male, in the body of the organ, forming a continuous and perfect canal through it.

We may here further observe, though the illustrations should more properly belong to the next section, that in cases of true hermaphroditism also in man and quadrupeds, as well as in the above spurious varieties, there may often be traced in some portions of the abnormal structures a sexual type bearing a greater or less analogy to the corresponding parts of those inferior animals that are naturally androgynous. Thus, in instances of true hermaphroditism, the orifices of the sexual ducts or passages occasionally open into a common cavity, as is normally the case in some species of *Doridium*, *Helix*, and other Mollusca; or the female oviducts or Fallopian tubes, and the male vasa deferentia, run closely alongside of each other without any communication between their canals, as in the *Alypsia* and most Gasteropoda. Indeed the occasional co-existence even of both testicles and ovaries in individuals among the higher animals would be only a repetition of, or retrogression to, the normal sexual type of those genera of animals that we have just named, and of the Planaria, Cestoidea, and other natural hermaphrodites.

In this way we see that, as in many other monstrosities, the several varieties of malformation in the sexual organs occurring in spurious human hermaphroditism do not consist of the substitution of an entirely new and anomalous type of structure, but are only repetitions of certain types of the same organs that are to be met

[1] Cuvier, *Anat. Comp.* tom. v. p. 129.

[2] Meckel, *Archiv für die Physiologie*, Bd. v. s. 66.

with both in the human fœtus and in the inferior orders of animate beings. The investigation of the whole subject shows us, in reference to the sexual organs, what is equally true in regard to all the other organs of the body—that their different stages of development in the embryos of man and of the higher orders of animals correspond to different stages of their development in the series of animate beings taken as a whole; so that here, as elsewhere, the facts of Comparative Anatomy are reproduced in those of Embryology, and both are repeated to us by nature on a magnified scale in the anatomy of the malformations of the part—a circumstance amply testifying to the intimate relations which exist between Comparative Anatomy, the anatomy of Embryonic Development, and that of Monstrosities. Indeed, proportionately as our knowledge of malformations has increased, it has shown us only the more strongly that the laws of formation and malformation—of normal and abnormal development—are the same, or at least that they differ much more in degree than in essence, and that the study of each is calculated reciprocally to illustrate and to be illustrated by the study of the other.

REMARKS ON THE NATURE OF TRUE HERMAPHRODITIC MALFORMATIONS, UNITY OF SEXUAL TYPE, MALE UTERUS, ETC.

Of the nature and origin of local malformations by duplicity we at present possess much less precise knowledge than of those of simple defect or simple excess of development; but there are certain facts ascertained with regard to the formation of the internal sexual organs, which may enable us to make an approach at least to accurate ideas of the character and origin of those abnormalities that constitute the several varieties of true hermaphroditism. These facts relate to the interesting subject of the unity of organisation, or common plan of structure, which is manifested in the corresponding male and female reproductive organs of the human subject, and of other species of bisexual animals.[1]

[1] It is right to state that the following observations on the pro-ovarium, prostatic vesicle, etc., and the subsequent deductions from them, have been added during the revisal of this essay for the present work—these being subjects that have only come to be discussed since the essay was originally published in 1839. Three preceding cases—those of Vrolik, Blackman, and Banon—have also been added, with references in the notes to some others, recorded, like them, during the last ten or fifteen years. In other respects the essay stands, almost entirely, as it was originally printed in the *Cyclopædia of Anatomy*. [Ed. of *Obstetric Memoirs.*]

By several of the Greek, Roman, and Arabian physiologists[1] the respective organisations of the two sexes were considered as in some degree typical of one another, the female being regarded as an inverted male, with the testicles and penis turned inwards to form the ovaries and uterus. This doctrine of analogy between the male and female sexual organs has, with various modifications, been very generally admitted by modern physiologists, and in some of its bearings it has been made, more particularly of late years, the subject of considerable discussion. The testicles, or, more correctly speaking, the bodies of the testicles, are still regarded as organs which correspond with the ovaries in their original situation, in their vascular and nervous connections, and in their relative sexual functions. The recent progress of the anatomy of the development of the embryo has also shown that the two organs correspond in their primitive origin. It is now well ascertained that the large masses occupying each side of the abdomen of the embryo at an early stage of development, and which Rathke has named the Wolffian bodies, after their illustrious discoverer, form, in Birds and Mammalia at least, the primordial matrices upon which the urinary and genital organs are developed. On the inner side of each of these matrices a small body is early developed, which seems to become afterwards either a testicle or an ovary, according to the particular ulterior sexual type which the embryo assumes. According to the testimony of Valentin, no difference in appearance or structure can be detected at their earlier periods of formation and development, between the testes and ovaries.[2]

If this were a fit opportunity for discussing, in all its details, the unity of type between the male and female reproductive organs, it would be easy to prove further the justness of those greater analogies that we have mentioned between the ovaries and the proper bodies of the testes, by pointing out other numerous minor, but still strong, points of correspondence manifested in the abnormal conditions and localities of these two representative organs in the higher animals, and in their conformity of structure in some of the lower. Thus among insects, in the genus *Libellula*, the long cylindrical testes of the males correspond with the long-shaped ovaries of the females; in the *Locusta* and *Gryllotalpa*, there are ramose bunched testicles with analogous fasciculated ovaries; in the

[1] Aristotle, *Hist. An.* lib. i. 17; Galen, *De Semine*, lib. ii., and *De Usu Partium*, c. i.; Rhases, *De Re Medicâ*, lib. i. cap. 26; Avicenna, *De Membris Generat.* lib. iii. 21, etc.

[2] *Entwickelungsgeschichte*, p. 391.

Lamellicornia we find compound radiating and united testes, with similar radiating and united ovaries; and sometimes, as in the genera *Melolontha* and *Trichius*, the number of the single bodies in the testicles corresponds with the number of the oviducts.[1]

Organ of Rosenmuller; Pro-ovarium of Kobelt; Canals of Gaertner, etc.—In further following out the unity of type between the genital organs of the two sexes, the epididymis of the male has usually been compared to the infundibula or fimbriated extremities of the Fallopian tubes of the female, and the seminal ducts, or vasa deferentia of the Fallopian tubes themselves. This view, however, has been considerably changed in some important respects, by the later investigations of embryologists and anatomists, and particularly by the observations of Kobelt and others, upon the transformations and relations of the two Wolffian bodies and their ducts. We have already alluded, in the preceding paragraph, to the Corpora Wolffiana as two large abdominal bodies in the early embryo, on the inner side of which the glandular organs constituting the future testes or ovaries are first observed in the study of development. These Corpora Wolffiana are very large in the embryo of some of the lower animals, as in reptiles and birds. In the early Mammalian embryo, the two Wolffian bodies are oblong masses, placed one on each side of the spine, and stretching from the vicinity of the heart to the lower end of the abdomen. They send off two excretory tubes, the Wolffian ducts, which open below into the cloaca, or latterly into that division of the cloaca which forms the uro-genital sinus or common genito-urinary canal. Each Wolffian body consists, when fully developed, of a congeries of transverse cœcal tubes, all of them opening into the common excretory or efferent duct, which passes along the outer side of the organ. Most anatomists have described the Wolffian bodies as temporary embryonic glands, and altogether transitory structures only. "In both sexes," according to the statement of Müller in 1840, "the Wolffian bodies *entirely* disappear, and are not converted into any other organ."[2]—The investigations, however, of Kobelt[3] have latterly proved that this view was erroneous, and that we can still trace remains of the Wolffian bodies in the adult human female, and often, also, in the adult male.

[1] s *Entomology*, § 154, p. 222.

[2] Müller's *Elements of Physiology*, by Dr. Baly, p. 1637. The same opinion is expressed by M. Coste in the *Comptes Rendus* for 1839, p. 331.

[3] *Der Neben-Eierstock des Weibes*, etc., Heidelberg, 1847.

Professor Rosenmuller of Leipsic published, in 1802, a remarkable essay,[1] in which he pointed out the existence in the human female fœtus and infant of a peculiar fan-like structure, formed of diverging ducts, and lying in the duplicature of peritoneum connecting the ovary to the outer or fimbriated portion of the Fallopian tube. He did not appear to have searched for this canaliculated "Corpus Conicum," as he terms it, in any subjects older than two years of age.[2] Kobelt has demonstrated, however, that this body or organ of Rosenmuller exists in the same locality in adult females—namely, between the Fallopian tube and ovary, inclosed between the serous coats or folds of the Alæ Vespertilionis;[3] and he has given to it the name of "Pro-ovarium." He has shown also, by the evidence of the history of this organ or structure, from embryonic to adult life, and by its position and anatomical characters, that it consists in reality of the remains of that Wolffian body which is so very large, and apparently so very important a structure, in early embryonic life. Rosenmuller hints that the "corpus conicum ejusdem ductus,"[4] which he discovered in the female fœtus, may have an analogy to the epididymis and vas deferens of the male; and Kobelt has tried to prove that in man, the Wolffian body so far remains, that it has gone to constitute, in great part, the epididymis; the cul-de-sacs or blind tubes of the former being transformed into the coni vasculosi of the latter body, and the inferior cœca of the Corpus Wolffianum being converted into the so-called vasa aberrantia of Haller. In both the adult female and male some of the higher tubes or cœca of the Wolffian bodies often remain, and become expanded into those cysts which form the sessile and pediculated hydatoid bodies that are so very frequently, or indeed constantly, found in the upper part of the broad ligament, and on the fimbriate edges of the Fallopian tubes, and also on and beneath the surface of the male epididymis.[5]

[1] *Quaedam de Ovariis Embryonum et Fœtuum Humanorum.* An excellent plate illustrates the essay. He thinks Roederer and Trew were probably acquainted with the structure which he describes, p. 8.

[2] At p. 14, *loc. cit.*, he tells us he found this special structure in an infant who died when twelve days old, "admodum magnum, constare e multis canaliculis in basi *corporis conoidei*, inter se convolutis et latioribus." In this specimen he reckoned the canals, or canaliculi, as amounting to about twenty in number.

[3] See also the excellent diagram of its structure and position in H. Meckel's *Morphologie der Geschlechtszeuge*, 1848. Tab. ii. fig. 21, etc.

[4] *Quaedam de Ovariis, etc.*, p. 15.

[5] Rosenmuller delineates (pl. i. figs. 6 and 7), a hydatoid body or "corpusculum sacciforme," attached to the edge of the broad ligament, and connected

According, then, to this modern view of the subject, we ought, in reference to the doctrine of the unity of structure between the two sexes, to regard the epididymis of the male as having its true analogue in the pro-ovarium of the female, and not in the fimbriated extremity or infundibulum of the Fallopian tube.

The female Fallopian tubes and male vasa deferentia were formerly, as has been already stated, very generally considered as typical or analogous structures in the two sexes. Müller,[1] however, and Rathke[2] first pointed out that in birds and reptiles these two sets of tubes were developed separately and distinctly from each other in the early embryo. Bergmann,[3] Leuckhart,[4] and more especially Kobelt,[5] have latterly shown that the same fact is true with regard to the early history of development of these parts in the mammalian embryo and in the human subject. Vrolik[6] and Valentin[7] have published the same morphological views; and I know that their correctness is maintained by Professor Allen Thomson, undoubtedly the greatest authority on such a question in this country. From the researches of these and other observers, it seems now fully proved that in the early human and mammalian embryo, in both sexes, two hollow filaments or tubes on each side pass from the region of the testes or ovaries above to the uro-genital sinus or canal below. These two sets of tubes form at that period the organic link or connection between what latterly become the true reproductive glands, namely, the ovaries or testicles and the external organs of copulation. One of these pairs of tubes on each side consists of the excretory duct of the Wolffian body, and this pair in the course of development becomes transformed in the male

with the body he has described. Follin and Verneuil have latterly called the special attention of the profession in France to the cysts formed out of the persistent tubes of the Wolffian bodies. See Follin's *Recherches sur le Corps de Wolff*, published as a Thesis in 1851; and Verneuil's *Recherches sur les Kystes de l'Organe de Wolff dans les deux Sexes*, in the *Mémoires de la Société de Chirurgie*, tom. iv. p. 58.

[1] *Bildungsgeschichte der Genitalien*, 1830. *Elements of Physiology*, vol. ii. p. 1637. Muller still continued to hold, however, that in Mammalia, both the Fallopian tube in the female, and the vas deferens in the male, were on each side formed out of a single distinct structure, having originally the appearance of a filament running along the outer border of the Wolffian body.

[2] *Entwickelungsgeschichte der Natter*, 1839, p. 212.

[3] See Wagner's *Handworterbuch*, etc.

[4] *Zur Anatomie und Physiologie des Geschlechtsorgane*, 1847, p. 90.

[5] *Der Neben-Eierstock des Weibes*, etc., 1847.

[6] *Tabulæ ad Illustrandum Embryogenesin Hominis*, etc., Tab. xcv.

[7] *Text-Book of Physiology*, p. 661.

fœtus into the two male vasa deferentia. Of these Wolffian ducts or ultimate vasa deferentia, the upper portion on each side is ultimately united in the male subject to the secreting structure of the testicles by tubes that are in part the remains of the Wolffian body itself, and which form the epididymis. On the other hand, in the female embryos of most Mammalia, these Wolffian ducts entirely disappear, or, as in female Ruminantia and Pachydermata, they remain as rudimentary or imperfect ducts, lying between the layers of the ligamenta lata, and leading from the organ of Rosenmuller, or persistent Wolffian body, down to the sides of the uterus and vagina, and open into the latter, forming, in fact, the canals of Gaertner in the cow, sow, etc. The other pair of tubes—the cords of Müller, seen in the early embryo of both sexes running by the sides of those already mentioned, undergo two very different transformations in the male and female. In the female, they remain open at their upper extremity and detached, and come to constitute the canals of the Fallopian tubes and their infundibula or fimbriated extremities. On the contrary, in the embryo of the male human subject, and of most other Mammalia, they early disappear, particularly in their upper portion. In the male beaver, rabbit, goat, etc., they remain as rudimentary extensions of the horns of the organ we shall presently describe—namely, the male utriculus bicornis of these animals. Their lower portions may be considered as forming, or being transformed by their coalescence into, the male utriculus itself, in those species of animals, as in man, where this male utriculus or prostatic vesicle is single in form, and median in position.

Male Utriculus; Uterus Masculinus.—Formerly great diversity of opinion prevailed as to the morphological prototype or analogue of the female uterus in the male genital system. Some anatomists, as Burdach, Steghlener, and Blainville, regarded the uterus and male vesiculæ seminales as corresponding parts; while others, as Meckel, Carus, Schmidt, Ackermann, and Seres, compared the uterus to the male prostate. Instances of malformation on record appeared to favour both opinions, and there were other cases which almost inclined anatomists to believe that the vesiculæ seminales corresponded to the fundus or body of the uterus in the human subject, and to the cornua uteri in quadrupeds; while the prostate represented in the male structure the lower portion or cervix of the same organ.

M. Geoffroy St. Hilaire divided the uterus of the human subject into the body, and the upper part or fundus, the latter correspond-

ing to what constitutes the cornua uteri in the human embryo, and in adult quadrupeds. Further, believing that in the determination of all analogies in type and structure between different organs, the origin and course of the blood-vessels supplying the part ought to be our principal criterion, he was led by the study of the distribution of the branches of the hypogastric arteries to consider the body of the uterus and the vesiculæ seminales as repetitions of each other in the two sexes; and, contrary to the opinion of most anatomists, he conceived that the male vasa deferentia strictly correspond with the fundus or cornua uteri, and that the epididymis represents a coiled-up Fallopian tube, or, in other words, that the Fallopian tube is an unrolled epididymis.[1]

The later investigations, however, of Professor Weber of Leipsic,[2] and others, have thrown a new and most important light upon this question in morphological anatomy, by demonstrating that in man and in other males there exists, distinct both from the vesiculæ seminales and prostatic gland, a small rudimentary uterus—a true representative, in the unity of organisation between the two sexes, of the more highly developed uterus of the female subject.

Early in the last century, Morgagni described in the region of the caput gallinaginis in the male urethra a small sac or cell. He named it the Sinus Pocularis,[3] and has given two excellent and illustrative drawings of its situation, orifice, and cavity. He found this cell or vesicle in fourteen out of fifteen human subjects that he dissected. Of late years, since the attention of anatomists has been specially recalled to this part by Weber, Huschke, Leuckhart, and others, it has received a variety of appellations, as that of *prostatic vesicle*, *prostatic utricle*, *uterus masculinus*, etc.

In man this utricular body is a small oblong cul-de-sac, or hollow, flask-shaped vesicle, situated in the space or angle between the lower ends of the ejaculatory ducts, and opening below by a narrow neck and orifice upon the posterior wall of the urethra, at the anterior edge of the caput gallinaginis or verumontanum. In man its fundus is imbedded between the lobes of the prostate gland, but the lower portion of it, projecting as a narrow ridge on the middle and lower surface of the prostatic portion of the urethra, constitutes

[1] *Phil. Anat.* 1822, tom. i. p. 471.

[2] *Zusätze zur Lehre vom Baue und den Verrichtungen der Geschlechtsorgane*, Leipsic, 1846.

[3] *Adversaria Anatomica*, iv. 1723, p. 6, tab. figs. 1, 2. It was previously figured by Albinus in his *Annotat. Academ.* iv. tab. iii. fig. 3; and by Cowper, in his work entitled *Glandularum nuper detect. Descriptio*, i. 3.

the bulk of the caput gallinaginis. The normal site of its opening into the urethra is, in the human subject and in most Mammalia, at a point intermediate between the two orifices of the seminal or ejaculatory ducts. Sometimes, though rarely, as in two of Morgagni's human subjects, it opens into one of the ejaculatory tubes themselves ; and this, indeed, appears to be its normal structure in some animals, as in the hare. In man, the length of the cavity of the prostatic vesicle varies from three to six lines ; at its upper extremity its breadth is generally two lines ; but it sometimes attains a larger size. The lining membrane of its cavity is covered with small mucous glands, packed closely together. This male utriculus or prostatic vesicle is of much larger size, and in a state of greater development, in the males of some of the lower Mammalia ; and it was principally from studying its various forms and relations in these animals that Weber discovered the true morphological character of the prostatic vesicle, and its typical analogy with the female uterus.

In the embryos of the sheep and sow, at a certain period of development, the uterus or prostatic vesicle of the male is so very like the uterus of the female, that, according to Rathke,[1] the two organs cannot be distinguished from each other. In the adults of some male animals this utriculus, or male uterus, is an organ of considerable size, and presents much of the form and relations of the female uterus in the same species. For instance, in the rabbit it is an organ from one to one and a half inches in length, projecting behind, between the rectum and urinary bladder, and with its fundus divided and stretched out laterally into two short cornua, like those of the female uterus. The long uterus masculinus of the male beaver is single at the cervix, and split or divided above into two horns, like the female uterus in the same species ; and, like it, too, the male organ is provided with ligamenta lata, etc. In the goat its cornua often stretch to the testicles. In some adult animals the male uterus, however, is even less developed than it is in the human subject ; and it seems to vary greatly even in different individuals of the same species.

Few, or indeed none, of the eminent anatomists who have in later years studied the subject of the prostatic vesicle or utricle, as Huschke, Leydig, Rathke, Leuckhart, Bischoff, Arnold, Wahlgrew, Kölliker, Duvernoy, Goodsir, and Allen Thomson, have at all

[1] Weber's *Zusätze*, etc., p. 65. See his comparative sketches of the male and female uterus, in these animals, pl. v. fig. 4.

doubted that this organ is a representative or analogue in the male organisation of the genital canals of the female.[1] But different opinions have been expressed as to whether it morphologically represents the vagina, or the uterus, or both. H. Meckel[2] at one time, and in opposition to almost all other authorities, suggested and maintained that it was the analogue of the vagina rather than of the uterus; Weber considered it as the male prototype of the female uterus; and still more lately, Bimbaum[3] and Leuckhart[4] have shown that this organ may be more truly held as the morphological equivalent of the whole sinus genitalis, both the uterus and the vagina—an opinion now generally shared in by those who formerly took a different view of the subject. Huschke has sometimes found the lower or vaginal portion of the male utriculus separated from the upper and dilated end by a constricted point, as if indicating its division normally into uterus and vagina.[5] Indeed, it is only in accordance with this last doctrine that we can understand the relative positions, and modes of junction, of the genital and urinary canals in some monstrosities, and the fact of the great variety of forms and shapes which the male uterus or prostatic vesicle assumes, when it is found—as so often happens—preternaturally enlarged and disproportionately developed in different kinds of hypospadiac and hermaphroditic malformation.

The only remaining internal organs of generation requiring consideration under the present head are the prostate gland and vesiculæ seminales of the male. Formerly, as we have already seen, these male structures were, in different points of view, usually looked upon as prototypes of the female uterus, or the two were held as analogues, one of the neck, and the other of the body and fundus, or cornua, of that important organ in the female economy. The discovery of the existence of a true uterus masculinus, and the investigation of its morphological import and relations, have entirely dispelled these views. At present, the typical analogues in the female, of the prostate gland and vesiculæ seminales in the male, are, in consequence, by no means fixed and established, and some authorities are

[1] Some of the various diseased states attributed to enlargement, etc., of the third lobe of the prostate gland will yet be found, I believe, to be morbid states of this prostatic vesicle. To the minds of some, the investigation of "the diseases of the Male Uterus" would appear to be almost a paradox in thought and words.

[2] *Zur Morphologie der Geschlechtswerkzeuge*, p. 47.

[3] *Beschreibung einer Bildungshemmung*, p. 15.

[4] *Cyclopædia of Anatomy and Physiology*, vol. iv. p. 1427.

[5] Soemmering's *Lehre von den Eingeweiden*, p. 409.

inclined to look upon these male organs as peculiarly and entirely male structures, that have not any true morphological equivalents representing them in the construction of the female. In their original embryonic origin, as well as in their ultimate adult relations, the vesiculæ seminales seem to be parts and appendages of the male vasa deferentia; and perhaps, morphologically, they ought to be considered merely as highly and peculiarly developed portions of the lower end of the male seminal tubes, and consequently of the analogous ducts of Wolff and Gaertner. The scattered and imperfect form which they present in some hermaphroditic malformations, as in the cases described by Ackermann and Steghlener,[1] seems to accord with this view. The vesiculæ seminales distinctly and essentially belong to the sexual canals and structures; but the prostate gland seems more truly an appendage to the urinary tubes and urinary organisation. Generally, its excretory ducts open, and its secretions are discharged, into the urethral canal somewhat before the male seminal tubes enter into, or join with, that canal. And if we are to find a true prototype in the female of the prostate gland of the male, we shall probably detect it in the follicular glands and structures that exist so abundantly in the course, and at the extremity, of the female urethra—a canal which, throughout its length, is truly an analogue of the posterior prostatic and membranous portion only of the male urethra.

In the observations made under the present head I have spoken only of the analogous organs traceable in the *internal* sexual organisation of the male and female. Already, in preceding sections, the analogous organs and structures observed in the external genital construction of the two sexes has been discussed. If we now attempt to show, in an abridged and tabular form, the unity or common plan of structure between the different parts and organs belonging to the whole sexual organisation of the male and female —the morphological prototypes or equivalents in each may, with the explanations and reservations previously stated, be briefly represented as follows:—

In the Male.		In the Female.
Bodies of Testes	=	Ovaries.
Epididymes	=	Pro-ovaria and bodies of Wolff and Rosenmuller.
Vasa deferentia and their vesicular dilations	=	Ducts of Wolffian bodies or canals of Gaertner.

[1] See previously these cases at p. 465.

In the Male.		In the Female.
Cornua of male utriculus	=	Fallopian tubes and their infundibula.
Male or prostatic utriculus	=	Female uterus and vagina.
Gubernaculum testis	=	Round ligament.
Tunica vaginalis	=	Process of Nuck.
Cowper's glands and their ducts opening into bulb of urethra	=	Glands of Bartholin, and their ducts opening at the roots of nymphæ.
Urethra behind bulb and prostate gland	=	Whole urethra and its follicular glands.
Bulb of urethra	=	Vulva or vulvar end of vagina.
Penis, and its corpora cavernosa, ligaments, etc.	=	Clitoris, and its corpora cavernosa, ligaments, etc.
Prepuce and integumental covering of penis	=	Præputium clitoridis, and nymphæ or labia minora.
Glans penis and corpus spongiosum	=	Vascular nodule of clitoris and the erectile bulbi vestibuli.
Sides of scrotum	=	Labia majora.
Perineal closure of genital sinus	=	Hymen.

In relation to these equivalent or analogous parts in the two sexes, as given in the preceding table, it is scarcely necessary to observe that some of them—as the cornua of the male utricle on the male side, and the ducts of Wolff, or Gaertner, on the female side—are structures pertaining to the *embryonic* condition only, and not normally persistent or traceable in the organisation of the extra-uterine being; though their morphological prototypes are fully developed in the adults of the opposite sex. Further, I would here beg to add the following, as a general remark or law which seems to me to explain many of the hermaphroditic anomalies and sexual combinations of structure that we meet with, that whenever we find the development of the sexual organs of an individual, whether male or female, restrained or arrested, so as to be *below* the normal type or standard, some of the analogous or prototypic organs of the opposite sex will in this same individual be observed to be developed in excess, or *above* their normal type or standard.

Some other organs, besides those we have referred to, and that are, so far as regards their functions, peculiar and essential to one sex only, are nevertheless found to be repeated in the opposite sex in the form of an analogous rudimentary type of structure. Thus, in the male, we may observe the unity of sexual structure maintained in the presence of the rudiments of the mammary gland, which is *functionally* an organ of the female system only. In the human subject, and in animals whose females have pectoral mammæ, these organs occupy the same position in the male; while in those species

of quadrupeds in which they are placed in the inguinal region, we find them in the corresponding males forming the scrotum or bags for containing the testicles. Hence, as we have already seen, the testicles, in cases of malformation in these animals, are often laid upon or imbedded in the udder. In the same way in the Marsupiata, the bone which the female has for supporting the marsupium is repeated in the organisation of the male, although in the latter we cannot conceive it to serve any possible use.[1]

In the female also we observe in some points a similar disposition to the rudimentary repetition of parts that are essential or peculiar only to the male organisation, as in the repetition in the clitoris of some female Rodentia of the penis-bone of the male,[2] and in the formation of rudimentary forms of those processes of peritoneum which constitute the tunicæ vaginales. We are ourselves inclined also to regard, as was seen in the preceding table, the common crescentic form of the hymen of the human female in the same light,[3] and to consider it merely as an abortive attempt at that closure of the sinus genitalis or perineal fissure which we have already described as effected at an early period in the male embryo —an opinion in which we conceive we are borne out both by the history of the development, and the study of the malformations, of the external sexual parts in the female.

M. Isidore St. Hilaire read, in 1833, a memoir to the French Academy,[4] in which, following up the doctrine of his father with regard to the determination and distinction of the type of parts by the particular vessels distributed to them, he endeavoured to show some new points of analogy between the male and female organs, and to develop new views with regard to the origin and particular varieties of hermaphroditic malformations. With Burdach, he divides the whole reproductive apparatus of either sex into three

[1] Home's *Lect. on Comp. Anat.* vol. ii. pl. v.

[2] We have already, when considering spurious hermaphroditism in the female, mentioned several facts illustrative of the analogical peculiarities in structure between the male penis and female clitoris in some species of animals; and Burmeister, who regards the ovipositors and stings of female insects as corresponding to the clitoris in the female Vertebrata, has pointed out a remarkable conformity of structural type between its valves and those of the penis of the male of the same species.

[3] Burdach (*Phys.* s. 137) considers the small cutaneous fold situated at the orifices of the vasa deferentia, and Stiebel, the membrane placed at the extremity of the urethra (Meckel's *Archiv für Physiol.* Bd. viii. s. 207), as the analogue in the male for the female hymen.

[4] *Arch. Gen. de Méd.*, 1833, tom. i. 306.

transverse spheres, and into six portions or segments in all, or three on each side—viz., 1 and 2, the deep organs, including the male testicles and female ovaries; 3 and 4, the middle organs, or male prostate and vesiculæ seminales, and female uterus; 5 and 6, the external organs, comprehending the penis and scrotum of the male, and the clitoris and vulva of the female. Each of these portions or segments is, M. St. Hilaire points out, supplied by an arterial trunk peculiar to itself, and the corresponding organs of the male and female by corresponding arterial branches, as the deep organs of both sexes by the two spermatics, the middle by branches of the two hypogastrics, and the external by some other hypogastric branches, and by the external pudics. The circumstance, he conceives, renders all the segments in a certain degree independent of the others, both as regards their development and existence, and allows of the occasional evolution of any one or more of them on a type of sexual structure different from that upon which the others are formed in the same individual.

Though assuredly we cannot subscribe to the speculations of the elder St. Hilaire, that the development in the embryo of male testicles or female ovaries, and consequently the whole determination of the sex, is originally regulated by the mere relative angle at which the first two branches of the spermatic arteries come off, and the kind of course which they follow[1] (more particularly as it is admitted by most physiologists that the blood-vessels grow, not from their larger trunks or branches towards their smaller, but from their capillary extremities towards their larger branches), yet we believe that the doctrine of the comparative independence of the different segments of the reproductive organs pointed out by the son, is in its general principles correct. At the same time we would here remark that we conceive the doctrine would have been founded more on truth if the influence of the nervous branches supplying the different reproductive organs had been taken into account along with that of their arterial vessels, because, as we shall point out when speaking of the causes of hermaphroditism, there appears to be some connection between the state of the nervous system and the degree or condition of sexual development.

[1] *Anat. Phil.* tom. i. p. 359.—"L'ordre de variations des sexes tient à la position d'un artère. * * Le plus ou le moins d'ecartement des deux branches spermatiques motive effectivement cette preference. Que les deux branches de l'artère spermatique descendent parallelement et de compagnie, cette circonstance, je le repete, cette circonstance donne le sexe male; qu'elles s'ecartent à leur point de partage, nous avons le sexe femelle."

The consideration of the preceding analogies in structure between the male and female organs is interesting in itself, and, so far as relates to our present subject, important in this respect, that it enables us to understand how it happens, that, without any actual monstrous *duplicity*, we should sometimes find, in an organisation essentially male, one or more of the genital organs absent, and replaced by an imperfect or neutral organ, or by the corresponding or prototypic organ of the opposite sex, and *vice versa*. Hence there is no difficulty in conceiving that, in the body of the same individual, the primitive structural elements of these parts should occasionally, in one or more points or segments, take on, in the process of development, a different sexual type from that which they assume at other points. Indeed, some physiologists, as we shall immediately see, deny that the most complete hermaphroditic malformations ever consist of anything except such a want of conformity between the sexual type of different portions of the reproductive apparatus.

If each of the six segments—and we believe that their number might be shown to be really greater than this—is thus an independent centre of development in the formation of the sexual apparatus, and is consequently liable also in abnormal cases to have its own particular malformations, and to assume, either alone or along with some of the other segments, a sexual type different from the remainder, it is evident that we may have as many varieties of true hermaphroditism, without any real duplicity, as it is possible to conceive differences of arrangement among these six segments. Again, however, one or more of these segments may preserve from development its original indeterminate or neutral sexual type, while the others are variously formed either upon one or upon both sexual types; or one or more of the segments may have evolved within them the prototypic or analogous organs of the two sexes, as the vasa deferentia, and cornua uteri; and if we consider the different arrangements of double and single sexual parts that might thus occur in the six separate segments, we may gain some idea of the great diversities of structure in the sexual parts that are liable to be met with in instances of true hermaphroditism.

This doctrine forms, as it appears to us, the most sound and rational solution of the nature and origin of many forms of true hermaphroditism which physiological science is capable of affording, with our present limited knowledge of the laws of development; and its application to the explanation of the different varieties of

Lateral, *Transverse*, and *Vertical* hermaphroditism, is so obvious as only to require to be alluded to. It offers to us, however, no insight into the probable origin of those varieties of double hermaphroditism in which there is an actual co-existence, upon one or upon both sides of the body, or, in other words, in the same segment of the sexual apparatus, of such corresponding or analogous male and female organs as the testes and ovaries. We can only refer all such instances to the laws which regulate the occasional production of *local* duplicities in different other organs of single bodies, and at the same time confess our present ignorance of what these laws are. We know that various individual muscles, nerves, etc., are not unfrequently found double, and that in internal organs of the body examples of duplicity in individual viscera are occasionally, though rarely, observed in the heart, tongue, trachea, œsophagus, intestinal canal, etc. In the several organs composing the reproductive apparatus, instances of similar duplicity would seem to be even more common than among any other of the viscera. Examples of *three* mammæ upon the same person are mentioned by Bartholin,[1] Borelli,[2] Lanzoni,[3] Drejer,[4] Robert,[5] Petrequin,[6] and others;[7] and cases in which the number of these organs was increased to *four* have been recorded by Faber,[8] Gardeux,[9] Cabroli,[10] Lamy,[11] Tiedemann,[12] Champion,[13] Sinclair,[14] R. Lee,[15] and Moore.[16] An instance in which even *five* mammæ existed upon the same woman is reported to have been seen by Gorré.[17] Valentin[18] and Gunther[19] have recorded supposed cases of duplicity in the male penis; and Arnaud[20] has related an example of an analogous malformation in the female clitoris.

[1] *Acta Med. Hafn.* tom. iii. obs. 93. [2] *Observ. Rar.* cent. i. p. 55.

[3] *Eph. Nat. Cur.* Dec. ii. ann. v. obs. 55.

[4] *Arch. Gén. de Méd.* tom. xvii. p. 88.

[5] *Journ. Gén. de Méd.* tom. c. p. 57.

[6] *Gazette Médicale* for April 1837. Three distinct mammæ in a father, and in his three sons and two daughters.

[7] *Dict. des Sc. Med.* tom. xxxiv. p. 529.

[8] *Eph. Nat. Cur.* Dec. i. ann. ii. p. 346.

[9] *Journ. de Méd. de Corvisart*, tom. ix. p. 378.

[10] *Obs. Anat.* vii. [11] *Fantoni Anat.* p. 267.

[12] *Zeitschrift für Physiologie*, Bd. v. s. 110. One case with three, and three with four nipples. In one case the malformation was hereditary.

[13] *Dict. des Sc. Méd.* tom. xxx. pp. 377-78.

[14] *Statistical Account of Scotland*, xix. p. 288.

[15] *London Med.-Chirurg. Trans.* vol. xxi. p. 266.

[16] *Lancet* for Feb. 24, 1838. [17] *Dict. des Sc. Méd.* tom. xxxiv. p. 529.

[18] *Eph. Nat. Cur.* Dec. iii. ann. iii. obs. 77.

[19] *Cohen vom Stein*, Halle, 1774, p. 107. [20] *Mém. de Chir.* tom. i. p. 374.

Weber[1] met with a double vesicula seminalis on each side; and Hunter[2] alludes to the occasional occurrence of an imperfect supernumerary vas deferens. In 1833, a case of a double human uterus, furnished with four Fallopian tubes and four ovaries, was shown by Professor Moureau to the Académie de Médecine.[3] Blasius[4] dissected the body of a man on whom he detected the co-existence of three testicles; the additional testicle was of the natural form and size, and was furnished with a spermatic artery and vein that joined in the usual manner the aorta and vena cava; it lay in the right side of the scrotum. Arnaud found, on dissection, three testicles in a dog; the third was placed in the abdomen, and of the natural consistence, figure, and size; it was furnished with a vas deferens.[5] Other instances of triple and quadruple testicles of a more doubtful character, inasmuch as the observations made during life were not confirmed by dissection after death, are related by Voigtel,[6] Sibbern,[7] Brown,[8] Rennes,[9] and others.[10] Scharff[11] even gives an alleged case of a man with five testicles, three of which are stated to have been well formed, while the other two were much smaller than natural. And, lastly, Loder[12] is said to have exhibited to the Göttingen Academy drawings taken from the body of a male infant, on whom *all* the sexual apparatus existed double, there being two penes, a double scrotum, and urinary bladder, and, as it was supposed, four testicles.

In all the preceding instances the local duplicity of the particular reproductive and other organs adverted to, existed independently of any duplicity in the body in general, or in any other individual parts of it. And if we once admit, what the preceding instances will scarcely allow us to deny, that there may occur a duplicity of some of the male sexual organs in a male, or of some of the female sexual organs in a female, it is certainly easy to go one step farther, and admit that the double organ or organs may, however rarely, be formed in other instances upon an opposite sexual type. If, for instance, a division or duplicity, original or acquired, of a Wolffian body, or of the neutral organ primarily

[1] *Salzburg Medicinische Zeitung*, 1811, s. 188.

[2] Bell's *Anatomy*, vol. iii. p. 428. [3] *Journ. Hebdom.* tom. x. p. 168.

[4] *Obs. Med.* pars. iv. obs. 20. [5] *Mém. de Chir.* s. i. p. 131.

[6] *Handbuch der Path. Anat.* Bd. iii. s. 393. [7] *Acta Hafn.* tom. i. p. 320.

[8] *New York Medical Repository*, vol. iv. p. 801.

[9] *Arch. Gen. de Méd.* tom. xxiii. p. 17.

[10] See Haller's *El. Phys.* tom. v. pp. 411, 412; Arnaud's *Chem. de Chirurg.* tom. i. p. 128, etc. [11] *Eph. Nat. Cur.* Dec. iii. ann. v. vi. obs. 89.

[12] *Göttingen Anz.* 1802, p. 466.

formed on its inner side, or if any other cause, be capable of giving rise to the development of two testes or two ovaries upon the same side, it is not difficult to conceive that one of the double organs may become female and the other male. Indeed all our knowledge of the unity of structure and development between the various analogous male and female reproductive organs, as well as the fact of the occasional replacement of an organ of the one sex by that of the other in cases in which the sexual type is entirely single, as seen in instances of Lateral hermaphroditism, would lead us, *à priori*, to suppose that, if a local duplicity in any of the sexual organs was liable to occur, this duplicity would sometimes show itself in the double organs assuming opposite sexual characters, and thus constituting some of those varieties of double or vertical hermaphroditism that we have already had occasion to describe.

In the preceding observations we have proceeded upon the opinion commonly received by physiologists, of the fundamental unity of sex among all individuals belonging to the higher orders of animals; or, to express it otherwise, we have assumed that each individual is, when normally formed, originally furnished with elemental parts capable of forming one set of sexual organs only. We do not here stop to inquire whether this single sexual type is, in all embryos, originally female, as maintained by Rosenmuller, Meckel, Blainville, Grant, and others; or of a neutral or intermediate character, as supposed by the St. Hilaires, Serres, Ackermann, Home, etc., and as we are certainly ourselves inclined to believe it.[1] On this subject, however, a physiological doctrine of a different kind has been brought forward by Dr. Knox, and this doctrine is so intimately connected with the question of the nature and origin of true hermaphrodites, that we must here briefly consider it.

[1] Meckel (*De Duplicitate Monstrosâ*, p. 14) and Andral (*Anat. Path.* tom. i. p. 101) assume it, after Haller, as a fact, that a much larger proportion of monsters belong to the female than to the male sex; and while they attribute this circumstance to the genital organs in these beings retaining, from the general defect of development, their *original female* sexual character, they at the same time consider this circumstance to be strongly corroborative of this particular doctrine. Isid. St. Hilaire has shown (*Hist. des Anomal.* tom. iii. p. 387) that the supposed fact itself does not hold true in respect to some genera of monsters, and is even reversed in others; and he doubts if it be of such a degree of generality in respect to monsters in general as to merit being raised into a teratological law. If the views of Meckel were correct, we should certainly expect at least that spurious hermaphroditism, where the development of the sexual parts is commonly abnormal from *defect*, should be much more frequent in the female than in the male. The list, however, of recorded cases of it in the latter is, we believe, more than double the number of it in the former.

Dr. Knox,[1] in conformity with some more general views which he entertains on transcendental anatomy, is inclined to regard the type of the genital organs in man and the higher animals, as in the embryo, originally hermaphroditic, or as comprising elementary yet distinct parts, out of which both sets of sexual organs could be formed; and he believes that, owing to particular but unknown circumstances, either the one or the other only of these sets of elements comes to be evolved in the normal course of development. In those abnormal cases, again, in which, as in instances of double hermaphroditism, more or fewer of both sets of genital organs are present upon the same individual, he maintains that this is not to be considered as a malformation by duplicity, but is only a permanent condition of the original double sexual type, and is attributable to the simultaneous development, to a greater or less extent, of both the male and female sets of sexual elements.

This doctrine of the original but temporary double-sexed character of all embryos, derives, perhaps, its principal support from a source to which Dr. Knox does not advert—we mean the existence of this as the normal and permanent sexual type in most plants and in many of the lower orders of animals. But this argument by analogy certainly cannot by any means be considered as a sufficient basis for the establishment of so broad and important a generalisation in philosophical anatomy. Dr. Knox himself seems to have been induced to adopt the idea principally because it afforded (when once assumed as a fact) a simple and elegant solution, upon the laws of development, of the occasional occurrence of cases of true hermaphroditism; and in doing so, he appears to have proceeded upon the mode in which most such physiological hypotheses have been made—viz., by drawing his premises from his deductions, instead of his deductions from his premises. In the present state, however, of anatomical and physiological knowledge, Dr. Knox's hypothesis, however ingenious in itself, is one to which we cannot subscribe; for, first, it is totally opposed to all the facts which have been ascertained, and all the direct observations which have been made by Rathke, Meckel, Müller, Valentin, and other modern anatomists, upon the sexual structure of the embryos of the higher animals in their earliest state; and, secondly, if we were to admit it merely as a probable hypothesis, it is still, even in this respect, equally as incapable as the old doctrine of sexual unity, of explaining all the cases of malformation by duplicity of the genital organs;

[1] Brewster's *Edinburgh Journal of Science*, vol. ii. p. 322.

for, as we have already shown, there are some apparently well-authenticated instances of the existence of three or four testicles upon the same man, or three or four ovaries upon the same woman; and in reference to all such cases we would, if we proceeded upon the same data and the same line of argument as those adopted by Dr. Knox, be obliged to suppose that the original sexual type is not, as he imagines, double only as respects the two sexes, but double even as respects each sex, and that all embryos had originally not simply the elements of two, but those of three or four testicles and ovaries. In explaining such cases as those to which we allude, Dr. Knox, on his own doctrine, must of necessity admit the existence of a malformation by duplicity of the sexual organs in question; and if we grant this in regard to these instances, it is surely unnecessary to invent a particular and gratuitous hypothesis for the explanation of the analogous anatomical abnormalities observed in hermaphroditism. At present we must, we believe, merely consider the occurrence of anomalous duplicity of the sexual organs, and of various other individual parts of the body, as so many simple empirical facts, of which we cannot, in the existing state of our knowledge, give any satisfactory explanation, or, in other words, which we cannot reduce to any more simple or general fact; though from the success which has attended the labours of many modern investigators in this particular department of anatomy, it seems to us not irrational to hope that ere long we may be enabled to gain much new light upon the question of double hermaphroditism and the whole subject of malformation by duplicity.

ANATOMICAL DEGREE OF SEXUAL DUPLICITY IN THE DIFFERENT FORMS OF HERMAPHRODITISM.

Though the cases which we have brought forward do not present any instances of such perfect hermaphrodites in the human subject or in quadrupeds, as those which are represented upon the ancient Greek statues and medals,[1] or that have been described and delineated by Lycosthenes, Paré, Schenkius, and the older authors on

[1] See Winckelman, *Hist. de l'Art*, tom. i. p. 364; and Caylus, *Recueil d'Antiquités*, tom. iii.; Heinrich, *Commentatio quâ Hermaphroditorum artis antiquæ operibus illustrium origines et causæ explicantur:* Hamburg, 1805. Blumenbach, in his *Specimen Hist. Nat. Antiq. artis*, Göttingen, 1808, mentions and figures (pl. i. fig. 5, p. 15) a small ancient silver cast or impression of a case of hypospadias of the male genital parts, which he supposes to have formed a votive offering from some individual, malformed in the manner represented.

monstrosities, they yet present to us a sufficient number of instances in which, in accordance with the definition we have previously given of true hermaphroditism, there actually co-existed upon the body of the same individual more or fewer of the genital organs both of the male and female.

From the relations and size of the bony pelvis, and the fact of the penis and clitoris being only repetitions, in situation and structure, and organic connections, of each other, in the two sexes, it is useless, perhaps, to expect that we should ever find in any one case all the parts of both sexes present at the same time. For, since the male penis is only a magnified condition of the female clitoris, and since both of these organs are connected by the same anatomical relations to the same part of the pelvis, it would almost require some duplicity in the pelvic bones themselves to admit of the simultaneous presence of both; and in no authentic case has any approach to their co-existence upon the same individual been observed.

Various authors who have written upon the subject of hermaphroditism have gone so far as to endeavour to refer all instances of it to some one or other of those varieties that we have described under the name of *spurious*. Thus, dogmatising in a spirit of unphilosophical scepticism, Parsons[1] and Hill[2] have endeavoured to show that all reputed hermaphrodites are only malformed females, having a preternatural development of the clitoris, and in some instances with the ovaries descended into the labia. Others, on the contrary, as Professors Osiander[3] and Feiler,[4] maintain with equal inaccuracy that every supposed instance of hermaphroditism is referable to a hypospadiac state of the penis and scrotum, in persons that are in other respects essentially male.

Various physiologists, again, while they admit the occurrence of all the different varieties of spurious hermaphroditism, are inclined to deny that any such combinations of male and female organs upon the same body as those which constitute our several varieties of true hermaphroditism, are ever observed to occur in the human

[1] *Enquiry into the nature of Hermaphrodites*, p. 145. We would particularly point out the cases quoted by Dr. Parsons at pp. 14, 26, 30, 88, 95, 130, etc., of his able essay, as directly contradictory of his own doctrine, or as instances of hermaphroditic appearances in persons not of the female but of the male sex.

[2] *Review of the Philosophical Transactions.*

[3] *Neue Denkwürdigk. für Geburtshülfe*, Bd. i. n. 3.

[4] *Ueber Angeb. menschliche Misbildung*, Landshut, 1820.

subject, or among the higher classes of animals.[1] In spite of the recent accumulation of new and authentic cases, Professor Müller of Berlin is, in particular, in his excellent treatise on the development of the genital organs, published in 1830,[2] still inclined to coincide, in a great degree, in this opinion. Few, however, I believe, or indeed no physiologists now refuse to admit the occasional occurrence of a combination of male and female organs upon the same individual, when that combination does not (as in Lateral and Transverse hermaphroditism) imply a true sexual duplicity or repetition of any of the corresponding male and female parts. But some still altogether doubt the probability of any cases of our third division of Double or Complex hermaphroditism. Certainly, in the examination of the cases referable to that section, and especially in reference to the two first varieties of it, a sufficient degree of attention has not been directed to the accurate anatomical distinction of the superadded parts supposed to exist, from others with which it is possible to confound them. We shall here, therefore, shortly recapitulate and inquire into some of the principal sources of fallacy that are apt to mislead the incautious observer in the examination of such instances as those to which we allude, and which have often led to the idea of sexual duplicity, when the sexual type was single only, but malformed; and in doing so we shall consider the various sources of error, in an order comformable with those divisions of double hermaphroditism that we have previously adopted —speaking of the mistakes which may and have been committed in judging of the supposed co-existence, 1*st*, of a female uterus and male vesiculæ seminales and vasa deferentia; 2*d*, of a female uterus and male testicles, etc.; 3*d*, of both testicles and ovaries.

1. FALLACIES IN JUDGING OF THE ADDITION OF MALE SEMINAL DUCTS TO A FEMALE TYPE OF SEXUAL ORGANS.[3]

That form of sexual duplicity which consists in the supposed superaddition of male vesiculæ seminales and vasa deferentia to an organisation in other respects female, appears to have been hitherto observed only among the Ruminantia, or in free-martin cows. In judging of the reality of this variety of hermaphroditic malformation

[1] Thus Portal, *Anat. Méd.* tom. v. p. 474; Haller, *El. Phys.* tom. viii. p. 7, "merito dubitatur;" Voigtel, *Handbuch der Path. Anat.* Bd. iii. s. 364; Lawrence, Art. "Generation," in Rees' *Cyclopædia.*

[2] *Bildungsgeschichte der Genitalien*, etc.; *Physiology*, by Baly, vol. ii. p. 640.

[3] See anteriorly, p. 459.

in any given case, in that animal, there is one source of fallacy that requires to be particularly guarded against, and the consideration of which suffices fully to explain most if not all the recorded examples of this malformation. In the female sexual parts of some Ruminantia and Pachydermata,[1] but particularly in the domestic cow and sow, Dr. Gaertner of Copenhagen pointed out in 1822[2] the existence of two canals or ducts which have since that time been generally described under his name. On each side of the body, one of these ducts arises in the vicinity of the ovary, or toward the fimbriated extremity of the Fallopian tube, runs down first in the duplicature of the broad ligament, and afterwards in the substance of the parietes of the uterus and vagina, to near the meatus urinarius, and there opens into the vaginal cavity. Each duct communicates with several small glands, follicles, or cysts, that are scattered along its course, and which perhaps may not be improperly described as diverticula from the ducts themselves, or nascent vesiculæ seminales. Now when we consider the relations of those imperfect ducts and cysts that are occasionally observed in the free-martin cow, situated along each side of the defectively developed uterus, and which Mr. Hunter has described as male vasa deferentia and vesiculæ seminales, it seems to us that these supposed male organs are only in reality the ducts of Gaertner, with their accompanying follicles or cysts, existing perhaps in a preternaturally developed and dilated condition. They correspond in their origin, course, and position, with the canals and cysts discovered by Gaertner; and certainly, in the present state of our knowledge, we are fully entitled to refer them to this *normal* portion of the female structure, rather than to regard them as *abnormal* and superadded male organs, and as affording, in consequence, an example of true sexual duplicity.

Before leaving the fallacy which we have to guard against in confounding the ducts of Gaertner in free-martin cows with the male seminal canals, it is necessary also to observe, that, as was long ago suggested[3] by Dr. Jacobson of Copenhagen, and, as we have already seen,[4] has been latterly proved by Kobelt, F. Meckel, and

[1] M. Delmas seems to have observed a somewhat similar structure in the kangaroo (*Ephem. Méd. de Montpelier*, tom. v. p. 115); and Coste, in the sheep See *Comptes Rendus de l'Academie des Sciences* for 1839, p. 334.

[2] *Anatomisk Beskrivelse over et ved Nogle Dyr-Arters uterus undersögt Glandulöst organ*, etc., Copenhagen, 1822; *Edin. Med. and Surg. Journ.*, vol. xxi. p. 460.

[3] *Journal de l'Institut*, tom. ii. p. 160; and *Die Okenschen Körper*, etc., Copenhagen, 1830.

[4] See antecedent section on the Unity of Type, Wolffian Bodies, etc., p. 504, etc.

others, these canals are now generally considered by anatomists as merely permament remains of the ducts of the Wolffian bodies. If, however, it should ever happen that further and more accurate observations prove the two to be different, then the possible permanent state of the ducts of the Wolffian bodies must be looked upon as affording another source of error, by which we may deceive ourselves in judging of sexual duplicity from the supposed superaddition of male seminal canals to a female sexual apparatus.

In the course of the preceding pages we have had occasion to allude to cases in the human subject, and in the dog and sheep, in which vasa deferentia were stated to have existed in the same individual along with Fallopian tubes. In these instances, the supposed male seminal ducts were, in all probability, canals analogous to those described by Gaertner in the cow and sow; and in connection with this inquiry it is interesting to remark that Malpighi,[1] who seems to have been well acquainted with the existence of these ducts in the cow, has suggested that they may also exist in a more obscurely developed state in the human female, and may perhaps be identified with the ramose lacunæ described by De Graaf, Bartholin, Riolan, etc. Indeed, A. C. Baudeloque has, in a case published in the *Revue Médicale* for March 1826, described a human uterus which contained in its parietes a canal coming from the region of the right Fallopian tube, and opening upon the internal surface of the cervix uteri; and Moureau and Gardien seem to have met with a second similar instance.[2] Were these canals in the human female not truly the same as the canals of Gaertner in the cow, etc., and consequently the persistent remains of the ducts of the Wolffian body? And would not such cases of abnormal persistence of these ducts or canals in woman be more frequently detected, if the attention of anatomists were more specially directed to the inquiry?

2. FALLACIES IN THE SUPPOSED CO-EXISTENCE OF A FEMALE UTERUS WITH TESTICLES AND OTHER ORGANS OF A MALE SEXUAL TYPE.

We have, in a previous part of this memoir (p. 460 to p. 470), adduced above twenty different instances in the human subject, and in the quadruped, in which a large-sized uterus, or both a

[1] *Philosophical Transactions* for 1684, p. 634.

[2] *Medical Repository* for 1826, p. 571.

uterus and Fallopian tubes, were described as having been found upon the bodies of individuals that were in other respects essentially males.

In reference to these instances, it may be doubted whether the sexual organisation of the malformed animal was not *entirely* male, the large but imperfect uterus which is present being merely a preternaturally or abnormally developed state of the small prostatic vesicle or utricle normally found in the organisation of the male.[1] Thus, in the case detailed by Ackermann, the only male sexual organ that was entirely deficient was the prostate, and the only reputed female organ which was present was an imperfect cystiform uterus, having, as in the normal state of the prostatic vesicle, the vasa deferentia penetrating through its substance without opening into its cavity, and ultimately terminating, as the orifice of the male prostatic utricle actually does, in the posterior part of the urethra. In the analogous instance quoted in a preceding page (p. 465) from Steghlener, a similar arrangement of parts was observed; and in that case there was, in the enlarged ureters and renal infundibula, perhaps sufficient evidence, as we shall afterwards point out when speaking of the probable causes of hermaphroditism, of a distending power having acted upon the whole internal surface of the urinary and genital canals, and with so great a force (we may in the meantime allow) as to be capable of producing a morbid dilatation and enlargement of the cavity and substance of the prostatic vesicle. But, even granting that the instances given by Ackermann and Steghlener, and most other cases, are not at all satisfactory in regard to the reputed existence in them of a variety of true sexual duplicity, and allowing, what seems probable, or indeed certain, that the misshapen and imperfect uteri in these examples were formed by a simple dilatation and enlargement of the natural male utricle or prostatic vesicle,[2] there are still some other cases pertaining to

[1] See antecedently its description, p. 505, etc.

[2] In Ackermann's case, the dilated cystoid utricle perhaps represented the vagina more than the uterus, or the lower partion of the uterus more than its fundus; and the same remark applies, with probably great truth, to Steghlener's hermaphrodite, where the vasa deferentia opened into the cavity of the so-called uterus—as the analogous Wolffian ducts normally do into the utricle of the hare and rabbit, and possibly in the very early human embryo. If the upper portion or fundus of the uterine organ had been represented in these cases, that part of the cystoid utricle would in all likelihood have been double or bifurcated as the uterus is in the early female embryo. In the human hermaphrodite, the ordinary site of junction of the Wolffian ducts, or seminal tubes, with the genital

this division which scarcely fall within the bounds of the explanation, that the form of hermaphroditic malformation in question always consists of nothing more than a male organisation, with the usual normal male utricle developed to an unusual and abnormal size.

In the adult male goat, and in some other animals, the horns of the bilobed prostatic utricle sometimes run and stretch, in their natural state of conformation, alongside the vasa deferentia up even to the testicles themselves. We cannot, therefore, be surprised at finding that in cases of the present division of hermaphroditism, such as the goat described by Gurlt (see p. 461) and represented in our woodcut (Fig. 14), the so-called Fallopian tubes, or perhaps more properly the elongated cornua uteri, passed through the inguinal rings, and were ultimately affixed, as they often are in this division of these malformations, to the epididymes of the testicles. In all this structural arrangement there is nothing incompatible with the idea that the uterus and uterine cornua, which were present, consisted only of a preternaturally enlarged state of the prostatic or male utricle. But in the adult male *human* subject, the male utricle is rounded at its fundus, and gives off no cornua or appearances of Fallopian tubes. Yet, in the division of hermaphroditic malformations of which we are at present treating—viz. those consisting of a male organisation, with the addition to it of a large uterus, etc.—we find several cases in our own species in which the uterus that existed was provided with cornua and Fallopian tubes. Thus, in the person dissected by Petit, the imperfect uterus was furnished with two perforate Fallopian tubes of three and a half inches in length, and at the same time it is distinctly stated that not only the prostate gland, but the vesiculæ seminales and vasa deferentia were also present. The vasa deferentia, between their origin from the testicles and their urethral termination, were each above seven inches long, and they entered the urethra by two apertures that were quite distinct and separate from the orifice of the uterus, which opened into the urethral canal at a point between the neck of the bladder and the

sinus, may sometimes in such exceptional cases, be regulated—1. By the very early period at which the arrestment of development, and consequent persistence of embryonic type, takes place; and 2. By the degree of evolution which the malformed parts subsequently undergo. In Vrolik's case of double hermaphrodite—already cited in a previous section, p. 478, the Wolffian duct, or vas deferens, on the right or most developed side, opened, as usual, into the vagina; while, on the left, or least developed side, it opened into the angle of the uterus.

prostate. In this case the Fallopian tubes of the uterus can scarcely be considered as merely formed at the expense of the normal male or prostatic utricle; and consequently we can only, in one sense, consider the Fallopian tubes or cornua uteri as a *supperaddition* to, and not a *transformation* of, the male structures; or, in other words, we may so far look upon the above as an instance of duplicity in a part of the sexual apparatus.

The same reasoning and remarks might be shown, if it were necessary, to apply in a still greater degree to the analogous example in the human subject given by Professor Mayer, and where he found upon an infant six months old, superadded to a complete male organisation, a vagina, uterus, and two Fallopian tubes, furnished with dilated sacs as infundibuliform extremities. See the description and delineation in a preceding section (page 468, Fig. 17). In an interesting case of the same description detailed by Betz,[1] that anatomist found on the body of a young infant a male organisation (testis, vasa deferentia, etc.), with a uterus in the site of the prostatic vesicle, as large as the female uterus in children of the same age. Further, this uterus had Fallopian tubes running from it on either side, along with the vasa deferentia, to the testicles; and of these Fallopian tubes, the one on the right side, at least, ended at the epididymis of the testis, in an imperfect infundibuliform extremity or dilatation.

In all these latter instances, the form and type of the uterus and uterine tubes were much greater and more perfect than could be produced by simple enlargement and dilatation of the human prostatic vesicle or male utricle, such as we see it in the adult. But still, after all, this does not, we opine, remove such cases of hermaphroditic malformation from the category of those that consist merely of a male organisation, with the superaddition of an enlarged and developed state of the normal male utricle and its elements. In the very early mammalian male embryo, as we have already stated (p. 504), hollow filaments or ducts corresponding to the Fallopian tubes are present, and, in the human and some other embryos they very early also disappear, leaving the male utricle without any appearance of such structural appendages. In the cases of Petit, Mayer, Betz, etc., these transitory uterine appendages were unremoved, in consequence of some contemporaneous arrest or error in development; they consequently remained beyond the period of intra-uterine life, and at the same time they grew, like most other

[1] Müller's *Archivs für Anatomie*, etc., 1850, p. 65.

malformed or persistent embryonic structures, with the general growth of the body, and of the parts more immediately surrounding them. Hence, in these instances we have, *in addition* to the usual male organs, apparently female uterine structures, incapable of being imitated by any mere enlargement and dilatation of the adult prostatic or male utricle; but essentially consisting of a persistent state of those transitory conditions of the male utricle and its appended tubes, that exist in some of the earliest stages of embryonic development. Further, in reference to the general imperfection of the sexual parts observed in these and in other examples of hermaphroditic malformation in the present and neighbouring sections, let me here again repeat, as one of the general laws to which these malformations are subject, an important statement already made—viz. that whenever we find the development of the sexual organs of an individual, whether male or female, restrained or arrested, so as to be below the normal type or standard, some of the organs of the opposite sex will, in the same individual, be observed to be developed, as it were, in excess, or *above* their normal type or standard; and, as in the preceding instance, this apparent excess will usually, we believe, be found to arise from the mere permanence in the adult of some early and transitory form of organisation in the embryo.

3. FALLACIES IN THE SUPPOSED CO-EXISTENCE OF TESTICLES AND OVARIES.

In several of the instances already described, from p. 470 to p. 479, and in which there was supposed to be a co-existence of both testicles and ovaries upon the same side or sides of the body, it seems highly probable that there has been occasionally a fallacy in the observation, owing to a want of knowledge of some anatomical circumstances that are liable to lead us into error in making an examination of such a case.

We have repeatedly had occasion to allude to the existence in the fœtal state, and in both sexes, of the Wolffian bodies, which are placed one along each side of the spine, and occupy at an early period in the embryo a great part of the cavity of the trunk. These bodies shrink and alter in the natural course of development, but never altogether disappear in man, and in the quadruped leave vestiges of their presence even in the adult animal.[1]

[1] See the description which I have given of the Wolffian bodies, and their

This particular type of structure, which is comparatively so very large in the embryo, may, like every other type of the fœtus, from an impediment or arrest in the natural course of the changes occurring in the development of the body in general, or of the genital organs in particular, remain occasionally, we have every reason to believe, permanently enlarged in one or all of its parts, and thus, by its presence in the animal, lead us to suppose that a rudimentary testicle exists in an otherwise well-marked female; or, on the other hand, that an ovary exists in an otherwise well-marked male. Both of these mistakes will be the more apt to be committed if the original excretory duct of the Wolffian body remains, for it may give the appearance of the addition of a vas deferens to the supposed testicle, or of an imperfect Fallopian tube to the supposed ovary.

The error, also, of confounding a permanently large Wolffian body with the testicle will be the more liable to occur, in consequence of the former body being naturally composed of an accumulation of convoluted diverticula which might be readily mistaken by an incautious observer for the seminiferous ducts of the latter.

There is certainly strong cause for doubting whether, in some of the cases that we have cited of the supposed co-existence of testicles and ovaries upon the same side, the permanent embryonic type of the Wolffian bodies and their ducts had not been mistaken either for testicles and vasa deferentia, while the sexual organisation was otherwise truly female, or for ovaries and Fallopian tubes, while the type of structure was in other respects strictly that of the male. This remark may perhaps with confidence be applied, for example, to the case of the free-martin described by Mr. Hunter (see p. 472); and in this and in most other similar instances the supposed testicles and ovaries have not been examined with anything like sufficient anatomical accuracy. At the same time, however, it appears to us quite impossible to explain away all the recorded cases of the supposed co-existence of testicles and ovaries upon this principle. The cases, for example, of testes and ovaries observed on the same side by Mascagni and Vrolik (see pp. 471, 478), cannot be set aside by such an interpretation. And in reference to this point, we would further particularly observe that the consideration of the *relative position* occupied by the reputed testicles and ovaries

persistent conditions in the adult female and male, in a preceding section, p. 502, etc.

may perhaps afford us a useful guide in cases of doubt. In some of the instances that have been previously cited, the relative situation of the supposed testicles and ovaries was exactly such as the Wolffian bodies are known to bear to these parts. In other instances, however, as in the ape described by Dr. Harlan, the relative situation in which the testicles and ovaries were found was that which they occupy in the perfectly-formed male and female; and in such a case as this it would surely be over-sceptical, and at the same time in opposition to all that we yet know of the history of the Wolffian bodies, to suppose that these bodies had imitated the testicles so far as to move out of their original locality and travel downwards through the inguinal rings. At the same time, we must recollect that in this case the distinctive anatomical structure both of the testicles and ovaries seems to have been satisfactorily made out, in so far that the former are described as "perfectly formed," and the latter as having "minute ova visible in them." "The male and female organs of generation," Dr. Harlan adds, "were as completely perfected as could have been anticipated in so young an individual, and resembled those of other individuals of a similar age." Now if we once admit in this, or in any one other particular instance, that the evidence of the co-existence of testicles and ovaries is satisfactory, then certainly we may in any equivocal case be entitled to doubt, until we have some more sufficient criterion for distinction pointed out, whether the dubious double bodies that we may meet with be a rudimentary testicle or ovary conjoined with an imperfect Wolffian body, or really a true instance of the presence both of testicles and ovaries upon one or upon both sides of the body of the same individual.

PHYSIOLOGICAL DEGREE OF SEXUAL PERFECTION IN HERMAPHRODITES.

Among those lower tribes of animals, such as the Abranchial Annelida, Pteropoda, etc., that are naturally hermaphrodite, every individual is in itself a perfect representation of the species to which it belongs. In the higher orders, however, in which the distinction and separation of the sexes come to be marked, each individual, being either solely male or solely female, can, as has often been remarked, be regarded only as representing one-half of its entire species. In most instances of hermaphroditism among these more perfect animals, the malformed being does not even

attain to this degree of perfection, but is in general so defectively constituted as not to have the proper physiological characters and attributes of either sex. In cases of spurious hermaphroditism it would appear that sometimes, though the copulative or external sexual parts are greatly and variously malformed, the internal or proper reproductive organs are developed with sufficient perfection to enable them to perform the functions belonging to them. We have very little proof, however, that in any instances of what we have described as true hermaphroditism, the apparatus of either sex is ever formed with such anatomical perfection as to empower the malformed being to bear a successful part in the reproductive function. Indeed in all, or in almost all, cases belonging to this last order of hermaphroditism, the individual who is the subject of the malformation may, with much more than poetical truth, be described, both anatomically and physiologically, as in the words of Ovid—

> Concretus sexu, sed non perfectus utroque,
> Ambiguo venere, neutro potiundus amore.

There is on record one remarkable instance of apparent exception to this general observation, a notice of which we have reserved for this place, on account of the want of any such precise knowledge of the true anatomical peculiarities of the case as might enable us to refer it to the section which it ought to occupy in our classification. The case to which we allude was described by Dr. Hendy of New York, in a letter dated from Lisbon in 1807, and the subject of it was a Portuguese, twenty-eight years old, of a tall and slender but masculine figure.[1] "The penis and testicles," to adopt the words of Dr. Hendy's own narrative, "with their common covering, the scrotum, are in the usual situation, of the form and appearance, and very nearly of the size, of those of an adult. The præputium covers the glans completely, and admits of being partially retracted. On the introduction of a probe, the male urethra appeared to be pervious about a third of its length, beyond which the resistance to its passage was insuperable by any ordinary justifiable force. There is a tendency to the growth of a beard, which is kept short by clipping with scissors. The female parts do not differ from those of the more perfect sex, except in the size of the labia, which are not so prominent, and also that the whole of the external organs appear to be situated nearer the rectum, and are not surrounded with the usual quantity of hair. The thighs do not

[1] *New York Medical Repository*, vol. xii. p. 86.

possess the tapering fulness common to the exquisitely formed female; the ossa ilii are less expanded, and the breasts are very small. In voice and manners the female predominates. She menstruates regularly, was twice pregnant, and miscarried in the third and fifth months of gestation. During copulation the penis becomes erect. There has never existed an inclination for commerce with the female under any circumstances of excitement of the venereal passion." In the preceding case, if we may confidently trust to the account given of it, we have ample proof of the existence of the internal female sexual organs, in the circumstances of menstruation and impregnation taking place; and at the same time there appears considerable evidence for believing that some of the male organs were present. For, even if we were to argue that the bodies present in the scrotum or united labia might be ovaries and not testicles —or, as happened in Vrolik and Sir Astley Cooper's cases, mere masses of fat—and that the supposed semi-perforate penis was only an enlarged clitoris, still the masculine figure of the individual, the imperfect beard, the narrowness of the pelvis, and the form of the lower extremities, would tend to indicate the probable existence of the rudiments of some male organs; and if we go so far as to admit this, we must further allow the present to be an instance of hermaphroditism, in which *one* of the sets of sexual organs was capable of assuming its appropriate physiological part in the process of reproduction, though perhaps unable, if we may judge from abortion having twice occurred, of ultimately perfecting that process.

The preceding remarks upon the functional reproductive powers of reputed true hermaphrodites, have been meant to apply only to the supposed perfection of *one* order of their sexual organs. It becomes a still more interesting question whether it ever occurs that in any abnormal hermaphrodite among the more perfect tribes of animals, both kinds of sexual parts may be found in so perfectly developed a state as to enable the individual to complete the sexual act within its own body, or, in other words, to impregnate and be impregnated by itself. Though we have assuredly no positive proof to furnish,[1] that a hermaphrodite so physiologically perfect has ever

[1] We do not certainly feel entitled to place among the category of correct observations either the alleged case given by Linnæus (Mangetus' *Bibliotheca Chirurg.*, lib. iv.) of a sow with perfect male organs on one side, and a womb containing several fœtuses on the opposite; or that mentioned by Faber (Hernandez' *Nov. Plant. Anim. Mexic. Histor.* p. 547) and quoted by Haller and Rudolphi, of the co-existence, in a rat, of ovaries and a uterus with nine fœtuses, along with complete male organs.

yet been observed, and should very strongly doubt its occurrence, from the almost universal imperfection, in an anatomical point of view, of the malformed organs; yet we have, on the other hand, no very rational ground, except that of the experience of all observers up to the present date, for denying entirely and unconditionally the utter possibility of it. And, perhaps, we should look upon this possibility with a less degree of scepticism, when we consider that a double hermaphroditism exists as the normal sexual condition of some of the lower tribes of animated beings; and, at the same time, take into account the fact of the more or less direct communication which has been generally found to exist between the female uterus and the male passages, in cases of lateral and of complex hermaphroditism, in the human subject and in quadrupeds.

In one of the cases of hermaphroditism in the goat, previously quoted from Mayer, and where there were present two male testicles, epididymes, vasa deferentia, and vesiculæ seminales, and a female vagina, uterus, and Fallopian tubes, with a body at the abdominal extremity of one of these tubes, that was supposed by Mayer to resemble a collection of Graafian vesicles; the male vasa deferentia opened into the female vagina, and its cavity, with that of the uterus and of all the male sexual canals, was distended with a whitish fluid of the odour and colour of male semen, and containing, according to Bergman, the chemical principle proper to that secretion. It is not, therefore, altogether without some appearance of foundation in fact, that Mayer has added to the history of this case the following problematical remark: "Fuit ergo revera hermaphroditus semetipsum fœcundare studens."[1]

In a similar strain, Dr. Harlan has added to the account that he has given of the very complete case of hermaphroditism, already mentioned as met with in the Borneo orang-outang, the following observations and queries. "Admitting," he remarks, "what in reality appeared to be the fact, that all the essential organs of both sexes were present in this individual, had the subject lived to adult age most interesting results might have been elicited. Could not the animal have been impregnated by a male individual, by rupturing the membrane closing the vulva? or by masturbation, might not the animal have impregnated itself? by this means exciting the testicles to discharge their seminal liquor into its own vagina. The imperfection of the urethra most probably would have prevented the animal from ejecting the semen into the vagina of another individual."[2]

[1] *Icones*, etc., p. 20.

[2] *Medical and Physical Researches*, pp. 23, 24.

It has been sometimes urged as an argument conclusively illustrative of the fact of a double hermaphrodite impregnating itself, that in the hermaphrodite *Gastrophaga pini* described by Scopoli,[1] the insect is stated to have been seen to advance its penis and copulate with its own female organs; and, afterwards, we are informed, the female side laid eggs from which young caterpillars were produced. Before, however, admitting this case to present an incontrovertible instance of absolute hermaphroditism, with the functions of the two sets of sexual organs existing in a perfect condition upon the same individual, it is necessary to recollect a possible source of fallacy in this circumstance, that female Gastrophagæ have been observed to lay fertile eggs, although they had not had previously any connection with the male, as remarked by Professor Baster[2] in one instance in a female *Gastrophaga quercifolia*, and in another in the *Gastrophaga pini* by Suckow.[3] The same fact is further alleged to have been observed in some few instances by Pallas, Treviranus, Bernouilli, and others,[4] in regard to individuals belonging to some other of the higher orders of insects and animals, as in the *Limnæus auricularis*[5] and *Helix vivipara*[6] among Mollusca, thus bringing them in this respect into analogy with the Aphides and Cyprides.

CAUSES OF HERMAPHRODITIC MALFORMATION.

As yet we possess very little accurate knowledge, either in respect to the mode in which the determining causes of hermaphroditic malformation act, or the nature of these causes themselves. Upon the question of the probable or possible origin of actual hermaphroditic or sexual duplicity, we have offered already one or two remarks at pp. 313 and 315.

Most of the varieties of spurious hermaphroditism may, as we have just explained, be traced to an arrest in the development of the sexual organs at one or other period of their evolution, in consequence of which, some of those types of structure in these parts which were intended to be temporary and transitory only, are rendered fixed or permanent in their character. Our knowledge of

[1] *Introd. ad Hist. Nat.* p. 416.
[2] *Mem. de l'Acad. Roy. de Berlin*, 1772.
[3] Heusinger's *Zeitschrift für Organ. Phys.*, Bd. ii. s. 263.
[4] Burmeister's *Entomology*, s. 204; Burdach's *Physiologie*, tom i. §§ 44, 48.
[5] *Isis* for 1817, p. 320. [6] Spallanzani, *Mém. sur la Resp.* p. 268.

the more immediate causes of such arrested development in these and in other individual parts and organs of the body is as yet extremely limited.[1] We may, however, in reference to the particular forms of arrested development observed in hermaphroditism, remark, that in consequence of the great influence which, as we have already pointed out, is exercised by morbid states of the ovaries and testicles, in retarding or preventing the evolution of the sexual apparatus and characters after birth, it has been suggested with considerable probability by Meckel[2] and Isidore St. Hilaire,[3] that, in their ultimate analysis, certain cases of hermaphroditic malformation may be traced in the course of their causation to morbid influences exercised in the early embryo, at a period more or less near to conception, upon the ovaries or testicles, or upon those organs of a neuter or yet undetermined sex which afterwards assume the structure of one or other of these bodies. Further, the effects which this supposed morbid influence exercises directly upon the embryonic ovaries and testicles, and indirectly through them upon the rest of the genital apparatus, and consequently the modifications of sexual structure which it produces, may possibly be much varied according to its extent, duration, and nature, and according to the particular period of development at which it comes into action. It is evident that this explanation of hermaphroditism can only refer to the varieties of the malformation which consist of an imperfection or deficiency in the development, and cannot apply to those instances in which there is a superaddition of sexual organs. If, however, we can once satisfy ourselves that any set of cases whatever are traceable to a morbid action affecting the testicles or ovaries of the early embryo, our investigations into the causes of these cases will necessarily be much simplified, for our inquiries would be reduced from a vague and indefinite search after the production of a number of anomalies of structure affecting several different organs at the same time, to an attempt to trace out the nature of those morbid conditions to which the embryonic testicles and ovaries were subject, and which were capable of so far changing the structure and action of these organs as to give rise to the effects in question. Of the diseased states, however, to which the reproductive and other organs of the system are liable during the progress of their early development, we at present know little or nothing, although in the investi-

[1] See *Cyclop. of Anat. and Phys.*, Art. "Monstrosities."

[2] *Anat. Gén.*, tom. i. p. 609.

[3] *Hist. des Anomal. de l'Organiz.*, tom. ii. 58.

gation of this subject, a key, we believe, may possibly be yet found to the explanation of many of those malformations to which different parts of the body are subject.

Osiander[1] and Duges[2] have suggested that the variety of spurious hermaphroditism which consists of a division of the perineum in the male, may be produced *mechanically* in the embryo by the preternatural accumulation of fluid in the urinary canal, from an imperforate state of the urethra, and the consequent distension and ultimate rupture of the urethra, etc. From cases published by Sandifort, Howship, Billard, and many others, we are now fully aware of the fact that all the urinary canals of the fœtus in utero are occasionally found morbidly distended with a fluid, which, according to the interesting observations of Dr. Robert Lee,[3] would appear to possess the more characteristic qualities of urine. We have dissected one case in which the dilated fœtal bladder was as large as an orange, and have seen, in the Anatomical Museum of Dr. William Hunter at Glasgow, the preparation of another instance in which the bladder of a full-grown fœtus was dilated to the size of that of the adult subject. In one case, mentioned by Dr. Merriman, the distended organ contained half-a-pint of urine;[4] and in another, detailed by Mr. Fearn, it was capable of containing as much as two quarts of fluid.[5]

It is not impossible that the causes in question—namely, the obliteration of the urethra and the consequent distension of all the urinary passages, and probably also of the sexual canals communicating with these passages—may occasionally produce in the male embryo a re-opening of the perineal fissure, giving thus to the external parts the appearance of a female vulva, and perhaps at the same time may lead to the retention and imperfect development of the testicles by the distension of their ducts, and the unusual compression to which these organs may be subjected. Indeed, we have satisfactory evidence, in a few instances, that such a cause may have been in operation, by our detecting the other acknowledged effects of the urinary accumulation in question—such as preternaturally dilated ureters, and a cystic form of the infundibula of the kidneys, as in a case of hermaphroditism given by Mayer, in the human

[1] *Neue Denkw. für Aertzte und Geburtsh.*, Bd. i. pp. 264-267.
[2] *Ephem. Méd. de Montpellier*, tom. v. pp. 17, 45, and 52.
[3] *London Med.-Chirurg. Trans.*, vol. xix.
[4] *London Med. and Phys. Journ.*, vol. xxv. p. 279.
[5] *Lancet* for 1834-35, p. 178.

fœtus,[1] in the kid described by Haller,[2] and in the child whose case we have already quoted from Steghlener. (See p. 466.)

At the same time, the total absence of these collateral proofs in most other cases of hypospadias, our knowledge of the fact that the perineal aperture is in some cases never shut, and the difficulty of conceiving the possibility of its being re-opened when once it is firmly closed, are perhaps sufficient to show that the cause or causes alluded to produce in but few if any instances the effect here attributed to them.

We deem it not uninteresting to point out in this place, under the question of the origin of hermaphroditic malformations, a circumstance which has struck us in considering one or two of the cases in which the sexual apparatus of one side of the body was more imperfectly developed than that of the other—viz. that the opposite side of the encephalon was at the same time defectively formed. Thus in the case of Charles Durge, on the right side of whose body there was a well-formed testicle, and on the left an imperfect ovary, the right hemispheres of the cerebrum and cerebellum, but particularly of the latter, were found by Professor Mayer to be smaller and less developed than the left, and the left side of the occiput was externally more prominent than the right. The same author, in the account of his case of hermaphroditism in a person eighteen years of age, which we have previously quoted,[3] and where there was an imperfect testicle, etc., on the right side, but no trace of testicle or ovary on the left, incidentally mentions that the right side of the cranium was somewhat prominent—"dextra pars cranii paullulo prominet," in correspondence, there is every reason to believe, with a slight predominance in size in the hemispheres of the encephalon of the same side. In adducing these two cases, we do not wish to draw any inference with regard to the relation of causation between the size and development of the encephalic mass and the determination of the sex, but would merely point out the facts themselves in the meantime, for the purpose of drawing attention to the subject in the observation of any future similar instances that may happen to occur.

In connection with the question of the causes of hermaphroditism, it is interesting to remark that in some instances malformations of the genital organs, giving rise to appearances of hermaphroditism, have been observed to be *hereditary* in particular families, both on the

[1] See p. 8 of *Icones*, etc. [2] *Comment. Soc. Reg. Sc.*, Götting., tom. i. p. 2.
[3] *Icones*, p. 12.

maternal and paternal sides; and in other cases to occur among several of the children of the same parents. Thus Heuremann[1] mentions an example of a family, the females of which had for several generations given birth to males who were all affected with hypospadias; and Lecat[2] alleges that a degree of hypospadias is not uncommon among families in Normandy. In Rust's Magazin an instance is related of a degree of hypospadias existing in a father and son.[3] Baum,[4] in his essay on congenital fissures of the urethra, has referred to two instances of the existence of hypospadias in brothers of the same family, the first mentioned by Walrecht,[5] and the second by Gockel.[6] Sir Everard Home[7] found two cases of hypospadias in two children belonging to the same parents. Kauw Boerhaave[8] mentions an example of four hypospadiac brothers, and Lepechin another instance of three.[9] Naegele has reported a case in which two male twins were both hypospadiac;[10] and Katsky and Saviard[12] have mentioned similar instances.

When treating of transverse hermaphroditism, we have already alluded to another fact long and extensively known among our agriculturists, but first prominently brought before the notice of physiologists by Mr. Hunter, that the free-martin cow, or the cow that is born co-twin with a male, is generally barren, and has its sexual organs more or less defectively developed or hermaphroditically formed.[13] In three different instances Mr. Hunter confirmed the fact of the anomalous sexual development of such animals by dissection; and Scarpa[14] and Gurlt[15] have published some additional observations and cases. We have lately had an opportunity of dissecting the sexual parts of two adult free-martins, and found them, as already detailed, formed after an abnormal and imperfect sexual type; and our friend Dr. Allen Thomson made some years ago a similar observation upon a free-martin twin fœtal calf. Cases, however, exceptional to the general fact of the sterility and imperfect sexual conformation of the free-martin twin cow are not unfrequently met

[1] *Med. Beobacht.* Bd. ii. s. 234; and Laroche, *Sur les Monstrosités de la Face*, p. 30.

[2] Arnaud, *loc. cit.* p. 312. [3] *Magazin für die Gesammte Heilkunde*, Bd. xviii. s. 113. [4] *De fissuris urethræ virilis congenitis*, p. 54.

[5] Burdach's *Metamorphose des Geschlechter*, p. 52.

[6] *Eph. Nat. Cur.* Dec. ii. ann. 5, 1686, p. 85. [7] *Comp. Anat.* iii. p. 320.

[8] *Nov. Com. Acad. Sc. Petropolit.* tom. i. p. 61, tab. xi.

[9] *Ibid.* tom. xvi. p. 525. [10] Meckel's *Archiv*, Bd. v. s. 136.

[11] *Acta M. Berol.* Dec. 1, tom. ix. p. 61. [12] *Observ. Chirurg.* p. 284.

[13] See also vol. i. p. 315. [14] *Mem. della Societa Italiana*, tom. ii. p. 846.

[15] *Lehrbuch der Pathol. Anat.* Bd. ii. s. 188.

with. Mr. Hunter found the sexual organs of a free-martin calf, that died when about a month old, apparently naturally constituted. He speaks also of having heard of some free-martins that were so perfectly formed in their sexual parts as to be capable of breeding; and different instances of their fecundity have been published by Dr. Moulson and others[1] since the time that Mr. Hunter directed attention to this subject. In some pretty extensive inquiries which we have made in regard to this point among the agriculturists of the Lothians, we have learned only of two instances in which free-martins proved capable of propagating, and such cases seem to be always looked upon as forming exceptions to the general rule.

We are not aware that among other uniparous domestic animals, as the goat, mare, etc., when a female is born co-twin with a male, this female is sterile, and has its sexual organs hermaphroditically formed, as in the free-martin cow; and we are sufficiently assured that no such law holds with regard to twins of opposite sexes among sheep. Sir Everard Home, in his essay on monstrous formations,[2] mentions that, in warm countries, nurses and midwives have a prejudice that such women as have been born twins with males seldom breed; and we have found the same prejudice existing to a considerable degree among the lower orders in Scotland. Mr. Cribb[3] of Cambridge published in 1823 a short paper in order to refute this notion so far as regarded the human subject. He refers to the histories of seven women who had been born co-twin with males. Six of these had children, and the remaining seventh subject alone had been married for several years without any issue. We have ourselves made a series of extensive inquiries of the same nature as those published by Mr. Cribb, and have obtained authentic information regarding 42 adult married females who had been born as twins with males. Of these 36 were mothers of families, and 6 had no children, though all of them had been married for a number of years. Two of the females who have families were each born as a triplet with two males. In the *Medical Repository* for 1827, an anonymous author has mentioned an instance of quadruplets consisting of three boys and a girl, who were all reared. The female afterwards became herself the mother of triplets. Limited as the data to which we here allude confessedly are, they are still amply sufficient to show that in by far the majority of cases the females of

[1] Loudon's *Magazine of Natural History*, vol. v. p. 765. See also Youatt on Cattle, p. 539, *Farmer's Magazine* for Nov. 1806 and Nov. 1807.

[2] *Comp. Anat.* vol. iii. pp. 33-34. [3] *London Med. Repos.* vol. xx. p. 213.

twins of opposite sexes are in the human subject actually fertile, and, as some of the cases we have collected show, they are occasionally unusually prolific.

As to the cause of the malformation and consequent infecundity of the organs of generation in the free-martin cow, we will not venture to offer any conjecture in explanation of it. It appears to us to be one of the strangest facts in the whole range of teratological science, that the twin existence in utero of a male along with a female should entail upon the latter so great a degree of malformation in its sexual organs, *and in its sexual organs only*. The circumstance becomes only the more inexplicable when we consider this physiological law to be confined principally or entirely to the cow, and certainly not to hold with regard to sheep, or perhaps any other uniparous animal.

The curiosity of the fact also becomes heightened and increased when we recollect that when the cow or any other uniparous animal has both twins of the same sex, as two males or two females, these animals are always both perfectly formed in their sexual organisation, and both capable of propagating. In the course of making the preceding inquiries for females born co-twin with males in the human subject, we have had a very great number of cases of purely female and purely male twins mentioned to us, who had grown up and become married, and in only two or three instances at most have we heard of an unproductive marriage among such persons.

Further, we may, in conclusion, remark that among the long list of individual cases of hermaphroditism in the human subject that we have had occasion to cite, we find only one instance[1] in which the malformed being is stated to have been a twin. Katsky, however, Naegele, and Saviard, have each, as before stated, mentioned a case in which both twins were hermaphroditically formed in their sexual organs.

HERMAPHRODITISM IN DOUBLE MONSTERS.

One of the most curious facts in the history of double monsters is the great rarity of an opposite or hermaphroditic sexual type in their two component bodies, the genital organs of both bodies being almost always either both female or both male.

Physiological science affords us at present no satisfactory clue to the explanation of this singular circumstance. From two cases of double monstrous embryos observed in the egg of the domestic fowl

[1] Eschricht's case of transverse hermaphroditism, see p. 455.

by Wolff[1] and Baer,[2] and from a similar case met with in the egg of the goose by Dr. Allen Thomson, it appears certain that double monsters originate upon a single yolk, probably in consequence of the existence of two cicatriculæ, more or less complete, or of two germinal points, upon a single germinal vesicle, or of two germinal vesicles upon a single yolk.[3] In such a case the two bodies of the double monster are so early and intimately united together as to form, almost from the commencement of development, a single system; and therefore the fact of the uniformity of their sexual character is the less remarkable.

The fact itself, however we may explain it, of the comparatively extreme rarity of both male and female sexual organs upon double monsters seems sufficiently established by various careful investigations made into the subject. Thus, out of forty-two perfectly double monsters which Haller[4] was able to collect at the time at which he wrote, there were only two that were supposed to be of double sex, or, in other words, that had one body male and the other female. Among double-headed monsters with single lower extremities, he found a hermaphroditic type more common, and adduces three examples of it.

In re-investigating this matter, the late Professor Meckel[5] could discover among the numerous class of monsters with perfectly double bodies, united anteriorly or laterally by the thorax and abdomen, only one very doubtful case of exception to the above general fact. In the class of double monsters united in the region of the pelvis, he mentions two exceptional cases from Valentin[6] and Hasenest;[7] of double-headed monsters with single bodies he quotes three similar cases from Lemery,[8] Bacher,[9] and Bilsius;[10] and of monsters with a single head and double body he adduces two cases from Brissæus[11] and Condamine;[12] in which, in a like manner, one body of the monster was supposed to have female and the

[1] *Nov. Comment. Acad. Petropolit.* tom. xiv. p. 456.

[2] Meckel's *Archiv für Physiologie*, etc., for 1827, p. 576.

[3] We have in our possession a preparation from a duck's egg, in which two full-grown fœtuses are developed on opposite sides of a *single* yolk of the common size.

[4] *Opusc. Anat.* 1751, p. 176. [5] *De Duplicitate Monstrosâ*, p. 21.

[6] *Eph. Nat. Cur.* Dec. ii. ann. iii. p. 190.

[7] *Comment. Lit.* Norimb. 1743, p. 58.

[8] *Mém. de l'Acad. des Sc. de Paris*, for 1724.

[9] *Roux' Jour. de Méd.* 1788, p. 483. [10] Blankaart's *Coll. Med.* etc., 1680.

[11] *Six Observat. de M. Brisseau:* Paris, 1734, p. 33.

[12] *Mém de l'Acad. des Sc.* 1733, p. 401.

other male sexual organs. Several of these cases, however, certainly rest upon too doubtful authority and insufficient observation.

Isidore St. Hilaire has still farther extended the data on which the above general fact is founded, by showing that the same uniformity of sex holds good with respect to double parasitical monsters,[1] and even in monstrosities double by inclusion. Thus, out of this last interesting class of double monsters, he alludes[2] to ten distinct cases in which the sex of the included being was ascertained. In six out of these ten cases the including and included body were both male; and in the other four they were both female.

On the whole, therefore, we must consider as founded on a proper induction from the existing data, the axiom of Meckel,—"Sexuum diversorum indicia in eodem organismo, quantumvis duplicitate peccet, non dari, sed unum tantum observari."[3] But while all the data hitherto collected with regard to this subject would seem thus to point it out as one of the most constant and best ascertained laws in teratology, still we are not altogether disposed to consider it, with Zeviani[4] and Lesauvage,[5] as subject to no exceptions whatever. In the study of monstrosities, as in the study of other departments of medical science, we find many general, but no universal laws.

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[1] *Hist. des Anomal. de l'Organiz.* tom. iii. pp. 235 and 386.

[2] *Ibid.* p. 311.

[3] *De Duplic. Monst.* p. 21.

[4] *Mem. della Soc. Italiana.* tom. ix. p. 521.

[5] *Mém. sur les Monstr. par Inclusion:* Caen, 1829; or *Archiv Gén. de Méd.* tom. xxv. p. 140.

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PROPOSAL TO STAMP OUT SMALL-POX

AND OTHER CONTAGIOUS DISEASES.

In despite of the marvellous protective influence of vaccination, the mortality produced by small-pox in Great Britain is still very great and startling. Like other contagious maladies it varies much in the number of its victims from one year to another; yet, during the ten years from 1856 to 1865, small-pox destroyed in this island 51,034 individuals. In one of the last of these years, 1863, not less than 7610 died from it; and in 1864 its amount of mortality reached to 9425.

Such figures as these numerals denote scarcely convey to the mind an adequate idea of the deplorable loss of life still resulting among us from the ravages of this one malady; the more so as the mortality from the disease is distributed through the whole scattered population of the island. But (to state it otherwise) if in any one year some overwhelming catastrophe destroyed all the living population of the counties of Nairn or Kinross; or swept away every living inhabitant of the cathedral cities of Lichfield, Ripon, or Wells; or slaughtered four or five regiments of soldiers; or smothered as many as five or six times the number of members of the House of Commons—such an event would assuredly appal and terrify the public and its guardians; and the strongest measures would, no doubt, be called for, with the view of preventing the recurrence of the catastrophe, provided its prevention were at all possible. Is the similar amount of human slaughter to which our population is constantly subject by small-pox—not once, but continuously; not one year, but each year—preventible? I believe that it is so; and I believe further that the hygienic measures required for effecting this prevention would be found neither specially difficult nor expensive to the country, while they would save annually hundreds, if not thousands, of our population from death, by a disease which, even when it spares life, too often leaves permanent lesions, and a broken and damaged constitution.

To understand the means to which I point, let it be premised

that small pox is—like scarlet fever, measles, and hooping-cough—only a species of disease which, as a general law, attacks once in a lifetime, and is only propagated from an infected individual to a susceptible individual, by a specific poison generated in the course of the malady, and transmitted from the affected to the healthy; first, by the near approach of the one to the other; secondly, by their contact; thirdly, by direct inoculation; or, fourthly, by fomites, or by susceptible individuals being exposed to the virus when it has been imbibed into clothes, etc., with which the sick have been in contact. We would no more expect this known species of disease or poison to originate *de novo* at the present day, under any combination of circumstances, than we would expect a known species of animal or plant—as a dog or a hawthorn—to spring up, *de novo*, and without antecedent parentage.

The beneficial influence of Dr. Jenner's immortal discovery saves from death from small-pox, in our present population in Great Britain, probably about 80,000 lives yearly. As we have already stated, however, there still die from its ravages about 5000 annually. Some among these 5000 have been duly vaccinated, and yet are susceptible of small-pox after cow-pox, just as men formerly were found susceptible of a second attack of small-pox after they had passed through a previous attack of natural or inoculated small-pox. Others seem susceptible in consequence of the vaccination having been performed inadequately with imperfect matter; or without leaving vaccination-scars of sufficient quality or quantity. Again, a large class of those that perish from small-pox consists of individuals who have never been vaccinated at all, or who happen to be exposed to the variolous poison antecedent to the age at which vaccination is usually performed. Doubtlessly a stricter enforcement of the new compulsory laws of vaccination, and a greater amount of attention to its proper performance with proper matter, will betimes diminish the number of the susceptible class. Yet, in the meantime, the disease still revels with fatal power among our population; and the question is, Can it be arrested in its progress?

The public mind has, during the last two or three years, become familiarised with the idea of "*stamping out*" a disease, in the instance of Rinderpest—a malady apparently spreading in this country, as small-pox does, by contagion only; and every one well knows the perfect success with which this affection has been lately banished out of England, while it has also, by due care, been prevented spreading to Ireland and the Isle of Man. I believe the same principle

of stamping out could be as successfully applied to the extirpation of small-pox among us as it has been applied to the extirpation of rinderpest; but of course with great differences. The rinderpest has been stamped out by killing all the animals labouring under the disease; and in many instances all those animals of the same flock which had been exposed to the contagion of it, but which were not yet attacked by the malady. The mission, however, of the human physician is ever to save life, never to destroy it. And yet, in accordance with this leading and divine principle, we could, in my opinion, as surely and as swiftly stamp out small-pox as rinderpest has been stamped out. For all that appears necessary for the purpose is simply the methodic temporary seclusion, segregation, or quarantine, of those affected with small-pox, until they have completely passed through the disease and lost the power of infecting and injuring others. The poleaxe was the chief and leading measure required to stamp out rinderpest. ISOLATION is the chief and leading measure required to stamp out small-pox.

Various rules and arrangements would be necessary to effect the requisite amount of isolation. Without at all entering into details, let me here observe that the following measures would perhaps form the chief points to be attended to in the way of

REGULATIONS.

1*st*. The earliest possible notification of the disease after it has once broken out upon any individual or individuals.

2*d*. The seclusion, at home or in hospital, of those affected, during the whole progress of the disease, as well as during the convalescence from it, or until all power of infecting others is past.

3*d*. The surrounding of the sick with nurses and attendants who are themselves non-conductors or incapable of being affected, inasmuch as they are known to be protected against the disease by having already passed through cow-pox or small-pox.

4*th*. The due purification, during and after the disease, by water, chlorine, carbolic acid, sulphurous acid, etc., of the rooms, beds, clothes, etc., used by the sick and their attendants, and the disinfection of their own persons.

Here, as elsewhere, to obtain a great public good, some private or individual inconvenience must for the time be undergone by those who unfortunately become the subjects of the disease. But since first publicly speaking of the stamping out of small-pox, I find that in 1867 the Legislature has passed the "Public Health Act" of Scotland, a most excellent measure,[1] which gives some of the

[1] The Public Health Act for Scotland has been published by the Messrs. Blackwood, with notes by Sheriff Monro.

leading powers required to enforce a series of regulations like the preceding. The Sanitary Act of 1866 for England and Ireland tends in the same direction, but is not so comprehensive. For the "general prevention and mitigation" of infectious diseases, and other purposes, the Scottish Act has erected a number of local boards everywhere throughout Scotland, consisting, according to circumstances, of the town-councils, of the police-commissioners, or of the parochial boards. These "local authorities" are each entrusted with the power of appointing sanitary inspectors and medical officers under them, and are themselves so far under the central control and advice of the "Board of Supervision." They are bound to provide district hospitals or temporary places for the reception of the sick; to remove to them, by suitable carriages, any person suffering from contagious or infectious disorders: in case of need to direct not the sick to be removed, but to remove all other unaffected persons surrounding them, providing suitable accommodation for these unaffected elsewhere; and to have in each district all necessary apparatus and attendants for the disinfection of woollen and other articles, clothing or bedding, which may have become dangerous from contact with diseased individuals. Such powers are of the highest importance for the protection of the general community against small-pox and other such infectious diseases among the poorer classes of the population; but regulations in the same spirit would equally benefit the highest and richest in the land, both individually and collectively, and the sick as well as the uninfected; the necessary amount of isolation of the sick being of course allowed, to all who wished it and could afford it, to be effected at their own homes.

The Legislature has no scruple in interfering in some other diseases to as great or indeed to a greater extent. It enforces, for instance, the isolation of any individual affected with insanity, be he rich or poor, who is a homicidal lunatic, endangering the lives of others. If, by a law which no one thinks harsh or severe, lunatics are prevented from destroying the lives of their fellow-men, why should it be thought harsh or severe that people affected with small-pox should be prevented from dealing out destruction and death to all the susceptible with whom they happen to come in contact? Homicidal lunatics do not destroy annually in Great Britain above eight or ten, on an average, of their fellow-men. Small-pox patients yearly destroy, on the contrary, hundreds instead of units of their fellow-men in this island. Sixty years ago, when

speaking in the House of Commons of the gross iniquity of inoculating with small-pox the out-patients of a London hospital, and then allowing these inoculated persons to infect others with the disease, Mr. Sturges Bourne strongly but truly remarked :—"I think that the Legislature would be as much justified in taking a measure to prevent this evil by restraint, as a man would be in snatching a firebrand out of the hands of a maniac just as he was going to set fire to a city." A rattlesnake or a tiger escaping from a travelling menagerie into a school full of children would, in all probability, not wound and kill nearly so many of these children as would a boy or girl coming among them infected with, or still imperfectly recovered from, small-pox, or scarlet fever, or measles, or hooping-cough. Most properly the cobra and the tiger—because they are *always* dangerous—are always, as far as possible, prohibited from making such visitations ; and the infected boy or girl should be prohibited also, *during the time* that they are dangerous, by running through the course and convalescence of such contagious diseases ; or, in other words, while they exhale from their bodies a virus of disastrous and deadly potency.

The great object of preventing the diffusion of small-pox in any city, or village, or hamlet, by the stamping-out measures which I have ventured to suggest in this communication, would consist, of course, chiefly, when practicable, in isolating the very first cases. Some time ago a professional friend, to whom I was explaining these views, objected to them, that in the case of the town of Leith, which was the habitat of small-pox in 1861 and 1862, the disease was at one time too diffused to apply them. Dr. Paterson of Leith, however, has kindly informed me that at the time of the visitation of the malady he made an official inquiry into its origin, and found it to be this :—"A beggar woman, on tramp from Newcastle, brought, in the course of her wanderings, to Leith, a child lately affected with small-pox, and with the crusts of the eruption upon it. In Leith she became an inmate of a lodging-house in a "land" or block of buildings full of lodgings for the poorest of the poor. Many of the lodgers in these other houses, with their children, visited the room where the woman and the sick child resided. By the time Dr. Paterson was requested by the magistrates to inspect the tenement, several persons were already dead of small-pox caught from this imported case. One man, who had already in previous life suffered from two attacks of small-pox, visited the infected

tenement, and sickened and died of a third attack of the malady. The disease soon spread to other parts of Leith; and, as I am informed by the registrar of that town, ninety-nine human beings were destroyed by it, and much suffering and sickness produced among the many hundreds in the town who caught the disorder and recovered. But if that first case or cases had been obliged to be reported on at once, and had been forthwith isolated in the hospital or elsewhere, all this unnecessary amount of human mortality and disease would have been avoided; nor would the isolation and maintenance of the first case, or of the first ten or twenty cases, have cost as much money as the purchase of the coffins for the ninety-nine who died. The blowing-up of the powder magazine in the fort at Leith would not likely produce nearly so much danger and destruction of life among the inhabitants of Leith as the advent of the beggar woman and her infected child. Yet how carefully do we guard against the one danger, and how carelessly do we treat the other!

In 1818-19, above 3000 individuals were attacked with small-pox in Norwich, or about a thirteenth part of the whole population of that city. Of those attacked, 530 died. The disease was originally introduced into the town, according to Mr. Cross, by a girl who, in travelling with her parents from York to Norwich, was exposed to small-pox at a market-town in the course of her journey; and the malady appeared on her as soon as she arrived in Norwich. This was in June 1818. In January 1819, a druggist gave a new impulse to the contagion by inoculating three children with the small-pox. The disease destroyed in Norwich, according to Mr. Cross, more human life in the same space of time than had ever taken place from any other cause than the plague. The isolation of the girl first affected, and the prevention of the artificial inoculation of the three children by the druggist, would have prevented all this frightful mortality. To inoculate any one now-a-days artifically with small-pox—as the druggist did—has for many years been established by Act of Parliament as a crime, inasmuch as it tends to imperil the destruction and death of others. Should it not be equally regarded as a crime for a community to allow of a case in their midst (such as that of the girl first affected at Norwich) to remain in circumstances allowing of the deliberate dissemination and unchecked spread of the disease from her to others?

My friend Dr. Stark, who takes such diligent superintendence of the death registration in Scotland, tells me that constantly—as

in these cases at Leith and Norwich—he hears, through his official returns, of small-pox spreading in districts here and there from one imported central case.

In order to stamp out small-pox, the first of the four regulations which I have ventured to lay down (see p. 545), as to the earliest possible notification of the presence of the disease, is indispensably essential. The "Public Health Act" for Scotland enacts that the keeper of any common lodging-house shall, when any of its inmates are ill of fever, or of any infectious disease, "give *immediate* notice thereof," either to the medical officer, or the inspector of the poor, or the inspector of lodging-houses, in order that the medical officer shall forthwith visit and report on the case, and due means of prevention be taken by the "Local Authorities."

It would surely not be reckoned too hard a measure for the public safety that every householder should—by himself or through his medical attendant—be obligated by the Legislature to report upon the existence of any case of small-pox that might appear in his establishment. In the same spirit, every medical practitioner might be bound to report immediately any example of the disease that he met with in practice. All, or almost all, cases of small-pox could thus be brought under official notice comparatively early in the progress of the malady. As the disease does not mature into the stage of infection for some days after the eruption shows itself, a free period would thus be secured for arranging proper measures of isolation, either at home or in hospital, before the date and danger of infection were reached.

Further, with the view of preventing the infection of others by patients that have passed through small-pox or its perils, it will ever be a matter of importance to prohibit and prevent the possibility of infecting others till the power of infection is exhausted. Small-pox patients have apparently the power of dealing out the disease to others, as long as any parts of the incrustation of the eruption are left on their faces, hands, or body. Until that time, and it may be a few days longer, segregation from the susceptible is necessary; and no doubt would be followed by every person of proper feeling, for who would inflict, or run the chance of inflicting, disease and death on his fellow-beings? If he gives the infection even to one individual only, from that individual it may possibly become multiplied and propagated to hundreds. And before mixing again in society, the persons of the sick, as well as of the attendants, should

perhaps—as already suggested—be subjected to bathing and some systematised disinfection. Like other physicians, I have heard of various cases of small-pox and other infectious diseases propagated from the sick at an advanced period of their own convalescence. Several instances have been communicated to me of beggars, in the streets of Edinburgh and elsewhere, importuning for charity by lifting up their children, with small-pox incrustations still upon them, almost against the very faces of those from whom they asked alms, and infecting with the malady those whom they subjected to this outrage. Not long ago, a woman—as I am informed by Professor Gairdner—with her face and hands incrusted with small-pox, was seen selling sweetmeats to the children of a school in Glasgow. I have heard of repeated instances of small-pox obtained by riding in public carriages, which had been employed immediately before by persons still in the stage of convalescence from the malady. The Sanitary Acts of England, Scotland, and Ireland, ought in a great measure to protect the lieges against such abuses for the future, as they forbid, under a penalty, any persons suffering from infectious disorders (as small-pox, hooping-cough, etc.) from entering a public conveyance, or wilfully exposing themselves in any street or public place, or being exposed by others in any street or public place, without proper precautions against spreading the disease.[1]

[1] Other infectious diseases are often spread in very *advanced* stages of convalescence; and lives are constantly destroyed by not remembering and acting upon this all-important fact. I was lately told of a rich merchant-prince building himself a palace in the country. Scarlet fever broke out in the family of one of his gatekeepers shortly after he took up his residence. A certain amount and length of separation was enforced, but not enough; for at last one of the gatekeeper's children, in an advanced stage of convalescence from the disease, was allowed to come to him and deliver a letter. In consequence of this unfortunate communication the merchant himself sickened and died of the scarlatina. Some years ago I lost a dear friend and patient of measles, which broke out three days after her accouchement. She had come thousands of miles to be under my care. She was infected thus:—A girl came to beg at the door of her country residence, near Edinburgh. The girl stated she had been in hospital with measles, and had been dismissed when still too weak to work. The servant who conversed with the girl took measles, and gave it to some of the children. I brought my patient herself into Edinburgh as soon as possible, as she had never had measles, and all the eruptive fevers are, it is well known, almost always fatal when they attack the puerperal mother. But it was too late. I wrote her husband by one mail saying she was quite well, but I was in great fear of her from this exposure. For two days after delivery she was inclined to hold all the precautionary measures as utterly unnecessary; but the third day the fatal disease attacked her, and I was obliged to write her husband by the next mail the sad news of her death. The new-born child took measles, but recovered.

The late stamping-out of rinderpest proved a most successful, but, at the same time, a most expensive proceeding. The disease, and the poleaxe as a means of extirpating it, has, I am informed, cost cattle-proprietors and the country—in the price of the animals destroyed—a sum of about £2,000,000 sterling. To stamp out small-pox from amongst us, and thus save annually hundreds and thousands of human lives by its extirpation, would require no such sum as was expended on the extinction of the cattle-disease, and, indeed, would require little or truly no outlay beyond what the Legislature has already enacted, and exacts for the protection of the public health; for, as previously stated, much of the machinery for its extirpation already exists under the late Sanitary Acts of Great Britain and Ireland. The segregation of those affected with small-pox who belong to classes which are able to keep the sick member or members of their family at home, would, of course, cost the country nothing; while the rules applicable to their isolation could, if faithfully followed, be managed without any special inconvenience, or any injury to their feelings; and generally, if not always, under the superintendence and responsibilty of their own medical attendants. These regulations would involve no restrictions that are not followed out at present in every well-regulated family when infectious disease attacks any of its members; none, indeed, except such as common prudence and common humanity demand for the protection of the bodies and lives of those that are still happily unaffected. Any open breach of rules that tended deliberately to spread the disease, and endanger and destroy the health and lives of others, would of course require to be repressed by proper penalties. The primary separation and the *maintenance* of the poorer classes under the circumstances is already provided for under the Sanitary Acts; and our present sanitary laws are, in relation to the poorer classes, defective in their powers of stamping out infectious diseases, merely and mainly in as far as they do not enforce the isolation of the sick by due cautions *after* they are lodged in hospitals or in houses for their reception. No new outlay of money would require to be legalised; but even if required, the expenditure of a few thousand pounds would surely form a small imperial payment for the preservation, yearly, of some thousands of our human population from death, through one of the most dreaded and loathsome of human diseases. In the eye of the political economist and of the philanthropist, the premature slaughter annually of three or four thousand, or even of three or four hundred, human beings, is a loss that cannot be easily estimated by mere yellow gold.

The measure which I have suggested would probably, in my opinion, stamp out small-pox in Great Britain within six months or a year, provided they were carried out as faithfully and universally as the Legislature can command; and if the extirpation of the disease were thus once effected, any fatal case or cases of the return of the malady to any seaport, city, town, village, or country district, would be speedily notified by a machinery already in full operation —viz. the registration of deaths; and all the requisite powers for stamping out the disease in the newly infected locality could at once be set in full operation. All our sanitary acts provide for any instances of this or other infectious disease when introduced into our seaports by ships—ordering the removal of the sick to an hospital or other place for their reception; but in this (as in the case of our own poor in these same hospitals and places), totally forgetting to regulate their due isolation, so that they may not heedlessly sow and scatter round them the seeds of disease and death.

Measures of quarantine and isolation, similar to those I have suggested in the present paper, have been tried elsewhere in the British dominions, and found to answer. At the various ports of South Australia, all entrance to those affected and capable of spreading small-pox has been so well guarded against, that in only one instance—as mentioned to me by Dr. Grainger Stewart—has the disease spread landward into the city of Melbourne, in consequence of an affected individual getting into the town through the misrepresentation of the captain and surgeon of the ship in which he arrived. He inflicted the disease upon nearly twenty of the residents. The authorities then interfered, placed all the affected in an inland quarantine station, and the disease spread no farther. Thus the malady was at once stamped out. A different fate a few years ago attended the introduction of measles into the colony of Western Australia. I am informed by my pupil, Mr. Page, that towards the latter end of the year 1862 the disease in question was introduced by some persons arriving by the monthly mail steamer which touched at King George's Sound. "As this," he writes me, "was the first attack of measles that had visited the colony, a large proportion of the white inhabitants and all the coloured natives had never before been subject to its influence. The disease spread with such rapidity and fatality that the coloured population in the settled districts was almost swept away by it, a very small number only of those attacked recovering. The whites also suffered severely, many children and

adults dying, but the ravages of the disease were not nearly so frightful amongst them as amongst the coloured natives."

In conclusion, I would beg to make one remark. That formidable quaternion of diseases—Small-pox, Scarlatina, Measles, and Hooping-cough—kill annually in Europe above half-a-million of its inhabitants, and particularly of the younger portion of its population;—carrying sorrow and desolation into thousands of households. My observations in the present communication refer especially to the stamping out of small-pox, for I believe it is the malady whose extirpation could thus most easily be effected. But the same principles apply, and will, I believe, be applied betimes to these other analogous diseases, when the science of public health is more advanced,—for the study of it is yet in its infancy. Scarlatina and measles will become greatly reduced, if not extirpated, by an observance of similar rules. In due course they will be extended to hooping-cough. Typhus, and other communicable diseases, will also come to be controlled by their influence.[1] I have already, at the beginning of these observations, stated that during the ten years from 1856 to 1866 above 51,000 individuals had died of small-pox in Great Britain; and, if we calculate approximately from the population, above 12,000 more in Ireland, or upwards of 60,000 in the United Kingdom. In the ten years from 1856 onwards (I have no later data) there died in the United Kingdom from scarlatina above 280,000; from measles above 130,000; and from hooping-cough above 150,000; or about 600,000 of our population were killed off by these four diseases. To what extent can this terrible decennial death-roll be shortened or abolished by the process of isolation and stamping out? Is not the whole subject a grave and momentous question both for legislators and physicians?

[1] Dr. Adamson of St. Andrews informs me that some time ago a patient, carrying from a distance the infection of typhus, came to St. Andrews and was laid up there with an attack of the fever. From this primary case the malady spread to others, till several died, and a considerable number were laid up with the affection, but recovered. The advent of this infected individual to St. Andrews thus killed and maimed about as many of its inhabitants as were killed and maimed by the late "diabolical explosion" of the barrel of gunpowder or nitro-glycerine against the wall of the prison at Clerkenwell. But how very differently are these two series of human deaths and dangers estimated—and their repetition attempted to be guarded against—by the police, the legislature, and the public!

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CHAP. III. Do. do. do.

PART II.

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CHAP. XI. Letter to Editor of *Medical Times and Gazette*, 19th June 1852.
From article "Chloroform," *Encyclopædia Britannica*, vol. vi. 1855.
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Dr. Maudsley's aim in the preparation of this volume has been to treat of mental phenomena from a physiological rather than, as has hitherto been the habit, from a metaphysical point of view, and in his history of the inductive method, as applied to the interrogation of the mind, he shows conclusively that self-consciousness—the favorite resort of the schoolmen—is inadequate, contradictory, and unreliable. No book of the present day, devoted to the study of the mind, has attracted more attention or caused more comment than this. It is one of those works which mark the beginning of a new era in the study of mental science, and at the same time it is conceded on all sides to be, in its practical portions, a most reliable guide for the diagnosis, description, and treatment of insanity.

"Dr. Maudsley has had the courage to undertake, and the skill to execute, what is, at least, in English, an original enterprise. This book is a manual of mental science in all its parts, embracing all that is known in the existing state of physiology. * * * Many and valuable books have been written by English physicians on insanity, idiocy, and all the forms of mental aberration. But derangement had always been treated as a distinct subject, and therefore empirically. That the phenomena of sound and unsound minds are not matters of distinct investigation, but inseparable parts of one and the same inquiry, seems a truism as soon as stated. But strange to say, they had always been pursued separately, and been in the hands of two distinct classes of investigators. The logicians and metaphysicians occasionally borrowed a stray fact from the abundant cases compiled by the medical authorities; but the physician on the other hand had no theoretical clew to his observations beyond a smattering of dogmatic psychology learned at college. To effect a reconciliation between the Psychology and the Pathology of the mind, or rather to construct a basis for both in a common science, is the aim of Dr. Maudsley's book."—*London Sat. Rev., May* 25, 1867.

"The first chapter is devoted to the consideration of the causes of insanity. It would be well, we think, if this chapter were published in a separate form and scattered broadcast throughout the land. It is so full of sensible reflections and sound truths, that their wide dissemination could not but be of benefit to all thinking persons. In taking leave of Dr. Maudsley's volume, we desire again to express our gratification with the result of his labors, and to express the hope that he has not yet ceased his studies in the important field which he has selected. Our thanks are also due to the American publishers for the very handsome manner in which they have reprinted a work which is certain to do credit to a house already noted for its valuable publications."—*Quar. Journal of Psychological Medicine and Medical Jurisprudence.*

"Then follow chapters on the diagnosis, prognosis, and treatment of insanity, each characterized by the same bold and brilliant thought, the same charming style of composition, and the same sterling sense that we have found all through. We lay down the book with admiration, and we commend it most earnestly to our readers, as a work of extraordinary merit and originality—one of those productions that are evolved only occasionally in the lapse of years, and that serve to mark actual and very decided advances in knowledge and science."—*N. Y. Medical Journal, January*, 1868.

"This work of Dr. Maudsley's is unquestionably one of the ablest and most important, on the subjects of which it treats, that has ever appeared, and does infinite credit to his philosophical acumen and accurate observation. No one has more successfully exhibited the discordant results of metaphysical, physiological, and pathological studies of the mind, or demonstrated more satisfactorily the uselessness of an exclusive method, or the pressing need of combined action, and of a more philosophical mode of proceeding."—*Medical Record, Nov.* 15, 1867.

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