"Intermissions are seldom seen, and remissions even are rarely obtained. Organic visceral complications are almost universal, the liver and spleen being chiefly affected; and not unfrequently the disease assumes a malignant typhoid type. Should the patient recover from the first attack of the fever, it still lingers upon him for months, or even years, recurring at the new or full moon with the greatest regularity. In 1849 and 1850, a considerable party of Europeans was located at Mohamreh, where they were encamped on a dry plain, about a mile to the north of the town, just outside the gardens. They suffered severely from fever, which proved very fatal amongst the escort of Turkish troops; but all were affected with a disagreeable complaint, to which they could assign no cause, and which no precautions could prevent. This was a constant vomiting after meals, which affected not only the party residing there, but also their visitors who remained with them for a day or two. It at length became so general and so distressing, that they gave up eating breakfast, and all became emaciated to a great degree. That this was not caused by anything deleterious in the diet, was proved by the fact that two parties who had separate messes suffered equally, and that many of the men in one of the H.C.'s vessels which was lying off Mohamreh suffered in the same way." The editor adds a note, to state that the Bombay Fusiliers were similarly affected in the Punjab. In these cases, the action of the malaria seems to have told on the nerves of the stomach producing a morbid amount of irritability. This was a primary neurosis, or neuralgia. Is there not very much similarity between this and some of our gastrodynias?

Dr. Morehead, in an excellent report on *Pneumonia*, as observed in the Hospital at Bombay, has noticed the occurrence of this affection as complicating intermittent or remittent fever. His remarks relative to the effect of treatment seem to me most significant as to the real cause of the lung disease. He says: "From five to eight grains of quinine, with from one-tenth to one-quarter grain of tartar emetic, given at intervals of two or three hours for five or six doses, will in general suffice to check and then stop the febrile recurrences. When this effect on the febrile symptoms has been produced, it will generally be found that improvement in the pneumonia will at once commence; and, in a large majority of cases, if the recurrence of the febrile state be prevented for some days, the inflammation of the lung will be speedily removed." He adds in a note: "Indeed, I am not acquainted with anything more striking and satisfactory in the whole range of rational therapeutics than the progressive but speedy restoration of an hepatised lung, coexisting with fever of remittent type, *when the exacerbations have been controlled by the adequate use of quinine*. It is true that small local detractions of blood, the application of small blisters, and the use of quarter-grain doses of tartar emetic, have been had recourse to at the same time; but it is quite impossible for any one familiar with disease, and the actions of these means in these degrees, to attribute the benefit chiefly to them, and not to the circumstance of the prevention of the febrile exacerbation by the actions of the quinine." The pneumonia seems in these cases, which Dr. Morehead distinguishes from ordinary or "primary" pneumonia, to be the result of that action of malaria to be presently considered, which produces local determination of blood, or congestions. The number of the febrile cases was 27, that of the primary 76, making a total of 103 in the course of six years.

Abnormal increase of the secretion of various glands may, I believe, be stated unhesitatingly as one of the results of malaria. Dr. Copland describes a variety of jaundice as depending upon excessive production of bile. It is most frequent in miasmatous and tropical countries, but also occurs in the temperate. I have known it precede remittent fever, or rather become converted into it as the patient was removed from the malarious locality. According to the authority just cited, it is connected with biliary remittent fever in temperate climates and in European constitutions, and " is most appropriately treated by the means most serviceable for the constitutional affection; but it sometimes continues or returns after the fever has disappeared." Fluxes from the lachrymal and salivary glands are common attendants upon neuralgia of the nerves supplying them and the adjacent parts. Torti relates a case of pernicious tertian fever, in which (he himself being the sufferer) an extraordinary most profuse sweat broke out, and increased along with the fever. After a violent shock of neuralgic pain in the thighs, "as if they had been suddenly cut across," he describes himself "colliquari ex continuâ, et semper adauctâ sudoris, etiam malè tepentis, profusione." Frequently recurring perspirations, in a less degree, are exceedingly common phenomena in obscure aguish disorder. Dr. McCulloch mentions an instance of intermitting diabetes connected with intermittent fever, the commencement of the saccharine secretion never differing from what had been the former hour of the attack of the intermittent, or proceeding beyond its ancient limit, viz., six hours.

[*To be continued.*]

ON FÆCAL FERMENTATION AS A SOURCE OF DISEASE.

By C. H. F. ROUTH, M.D., Physician to the St. Pancras Royal General Dispensary; Assistant-Physician to the Samaritan Hospital for Women and Children; Vice-President of the Medical Society of London; etc.

[*Read before the Medical Society of London, February 23rd, 1856.*]

PART I *(concluded)*.

SINCE writing the former paper, my attention has been directed by Dr. Richardson to a report by Dr. Rennie, in a parliamentary report on *Further Correspondence on the subject of Convict Discipline and Transportation*, of the results obtained by overfeeding convicts in Western Australia. These men receive upon an average from 7 lb. 8 oz. to 7 lb. 14 oz. of food daily, from 59 to 67 oz. of which were solid food. This gross amount of food resulted in the production of a disease which affected the individual in various ways, sometimes bringing on dyspepsia, dysentery, severe constipation, or other analogous bowel complaints; sometimes a severe kind of ophthalmia; sometimes a cutaneous eruption. These diseases yielded to powerful purgatives and low diet. In one case (that of a man named Nain), the patient took 5 purgative injections, 480 grs. of compound jalap, 8 oz. of Epsom salts, 32 other purgative pills, 1½ oz. castor oil, 5 drops of croton oil, 8 grs. of scammony, and 6 grs. of gamboge, before the cure was effected. During this period, he voided 30 lbs. of feculent matter in a state of decomposition.

The daily average number of prisoners was 504; of sick, 42. The total number of sick in one year was 2,290; of whom 959 had diseases of the digestive organs; 598, diseases of the eyes; and 633, diseases of skin, including ulcers and abscesses. These diseases, as showing their origin from the same cause, were cured by the same mode of treatment and low diet, and were frequently metastatic one to another.

But what I wish to call attention to, is the fermentative character of the disease which was set up in connexion with the putrid character of the evacuations passed. Dr. Rennie says: "With respect to the peculiar nature of the blood disease, I stated that, from finding on a microscopic examination of certain forms of skin-diseases, a low form of vegetation, resembling the yeast plant (one of the most primitive illustrations of organic life), there were rational grounds for supposing that the large and badly constituted diet might lead to fermentation and the development of this low form of vegetation, which, after becoming absorbed and circulating for a time in the blood, ultimately might become expelled in the form of local disease. As a proof of the fermentative process going on in the intestinal canal, I mentioned that one of the most common appearances which the evacuations presented in the early stages of dysentery was that of copious, frothy, watery stools in a high state of

fermentation, and bearing a general resemblance to yeast." (p. 119.) In a table given at page 128, where he records the cases seriatim, the amount of medicine taken, and the characters of the stools, the fermenting process going on is well exemplified. In 4, the fæces are described as being in a high state of fermentation; in 6, putrid; in 2 only, tolerably healthy; in 3, the matters voided were like pea soup; and in the rest, pus, blood, mucus, shreds of membrane, etc., existed in abundance. The number of cases so tabulated amount to 22, affected with ophthalmic and skin diseases; half only, or 11, being affected with dysentery. This class of cases proves, I think satisfactorily, that fæces putrefy in the body if long kept there, and will give rise, if not removed in time, to serious blood disease.

Part II.

II. Effect of Fæcal Matter when taken after solution or suspension in water.

In entering upon the consideration of this subject, I feel bound to express the obligations under which, personally, we all lie to Dr. Snow, to whom the merit particularly belongs of having first called our attention to a source of disease previously ignored. Indeed, I find it difficult to express, in terms which I think would do justice to him, my sense of the importance of the discovery made by Dr. Snow, and which he has so ably enunciated, and proved, as I think, to demonstration, in his work on *Cholera*. It is admitted, with few exceptions, by men of science. Besides, in its results, it has conferred already great advantages on the public, having originated the adoption of decisive measures in regard to the supply of water in this metropolis, and in other places. The results of a further development in this direction on the social condition of man may, like Jenner's discovery, be the means of preventing the spread of fatal disease, and saving thousands of lives to the nation. In this sense, I think our thanks to Dr. Snow cannot be too strongly expressed; and, for one, I feel I owe him a great debt of professional gratitude.

In speaking of *putrefaction* in the first part of this essay, I referred to the necessity for the presence of certain agencies to enable substances to pass into fermentation—*i. e.*, to absorb oxygen.

1. That *water* is necessary, is proved (among other circumstances) by the experiment made by Gay Lussac, who found that, if chloride of calcium were placed at the bottom of a jar of oxygen in which meat was suspended, the meat remained fresh many days; the chloride of calcium, from its affinity for moisture, keeping the gas in a perfectly dry state. Hay, straw, wool, if perfectly dry, will keep for any length of time; but, if moist, they will become slowly charred, undergoing a degree of oxidation which may pass into combustion if exposed to the atmosphere.

2. If, when water or moisture be present, a small quantity of fermenting matter be added, the process of fermentation will rapidly progress—much more rapidly than if time be given for the independent development. An experiment was brought to our attention, detailed by Dr. Ayres, on the occasion of reading the first part of this paper, which applies so much to my subject that I cannot help again referring to it. Sir J. Pringle took the putrid yolk of an egg, into which he dipped a small thread. A bit of this thread was cut off, and put in a phial, with half the yolk of a new laid egg, diluted with water. The other half, with as much water, was put into another phial, and both, being corked, were put by the fire to putrefy. The result was, that the thread infected the first yolk very speedily; for the putrefaction was sooner perceived in the phial that contained it than in the other. (*Phil. Tran.* 1750, p. 554.) If, then, the ferment be thus supplied, the process in the water will progress most readily.

3. Water may become superoxygenated, both *(a)* by living animalcules and *(b)* by vegetable matters.

(a) By *animalcular life.* I have said before that oxygen is essential to bring about fermentation. Now water is, under certain circumstances, found to contain this gas in unusually large quantity. We all know that, while there exist in water certain kinds of animalcules which absorb oxygen and give out carbonic acid, thus exhaling a gas which, in course of time, will render that water unfit for animal life, so there are certain other animalcules which seem to act under the influence of light, as plants, giving out oxygen in large quantities. Thus (I quote from Liebig), Count Rumford noticed the fact some seventy years ago, that, if pieces of cotton, silk, and other organic substances, were placed in water, the water, after three or four days, became green, and was filled with minute spherical bodies, and evolved pure oxygen gas. No confervæ or plants of any kind were observed in this water. Again, "in the salt pans of the salt works of Rodenberg, in Hesse, a slimy transparent mass, which covers the bottom to the depth of from one to two inches, is found to consist entirely of such animalcules. This mass is everywhere interspersed with large air-bubbles, which ascend in great number through the supernatant fluid, when the pellicles enclosing them are torn by agitating the mass with a stick." "Pfanfuch, upon investigation, found this air to be pure oxygen gas, so that a wood splinter, the flame of which had been just extinguished, rekindled in a flame when immersed in it. Wohler found that this mass consisted of living infusoria. Messrs. Charles and A. Worren, in the *Transactions of the Academy of Brussels*, 1841, showed that water, with the co-operation of organic matter, evolved a gas containing as much as sixty-one per cent. of oxygen; and that this phenomenon was to be ascribed to the presence of *glamidonas pulviculus*, and some other green and red animalcules belonging to a still lower grade in creation." The same fact was confirmed by Liebig himself, who, after filtering a water in a trough in his garden, coloured green by the presence of various species of animalcules, filtered it through a fine sieve, to get rid of all confervæ and vegetable matters; and then, by exposing it to the light of the sun, in an inverted broken glass, the aperture of which was confined by water, found that, after a lapse of a fortnight, more than thirty cubic inches of gas had collected in the glass, which proved to be so rich in oxygen that a glowing splinter at once burst into a flame in it (*Liebig's Letters*, pp. 240, 41, 42.) It is true that, in these waters, there must be the necessary conditions of life present, contained in solution or suspension in the water, so that this may become a fit nidus for the development of the germ, fissa, or ovule of the future animal; but, in any case, we here see one way in which water highly saturated with oxygen may more readily and more rapidly cause animal or vegetable dead matter to pass into fermentation.

(b) By *vegetable life.* At any rate, if animalcules of the kind just now referred to be not present, to explain this hyperoxygenation of water, it may be due to the presence of certain water plants which have the property of keeping water fresh and continually oxygenated, although replete with animal life, such as fish, snails, etc., and other animalcules which consume oxygen, and though the water be not changed. Such are the various kinds of *valisneria* and *anacharis*, which emit oxygen gas. These facts are well known in these days, when so many keep aquaria. Many other water weeds have the same effect. Now, it is clear that more oxygen will remain in this water when the atmosphere is heavy and the barometer necessarily high; and even in cold weather, from the same reason. In this latter case, the water, especially at some depth, is often hotter than the external atmosphere; and in both these instances fermentation will take place more readily in it.

There is also this point in relation to this excess of oxygen in water, not to be lost sight of. The very excess of animal life (I allude to those animals which consume oxygen) is proof that this gas abounds to support this increase of animal life. But, even in those countries where we have no such index of animal or vegetable life to guide us, by reason of the intense cold, oxygen may be in excess, and in like manner its abundance will explain the prevalence of epidemic disease—Asiatic cholera, for instance—in Moscow and St. Petersburgh during winter. The atmosphere at that season, from

its greater weight and density, contains more oxygen in a given volume; and the porous snow necessarily contains also an increased proportion. Here then is a great supply of oxygen, and fermentation is only kept back by the intense cold. On the other hand, two habits, which I believe are common to inhabitants of snowy regions, assist to bring about this result. One is, that, owing to a want of proper water-closets, the slops are generally emptied in the neighbourhood of the houses. The other is, that, owing to the expense of fetching water from the river, and the natural unwillingness to face the intense cold longer than is absolutely necessary, the inhabitants prefer collecting the snow around their houses, and melting it down for drinking water; and thus water impregnated with fæcal matters, or at any rate very impure, is drank; and hence, the moment it meets in the alimentary canal, or in the hot houses, the circumstances favourable to fermentation, this process is readily set up. Owing, moreover, to the excess of oxygen, it is very rapid when once it has begun, and thus any disease so induced becomes speedily fatal. To talk of emanations at temperatures as low as zero, and below that, which may be sensible to the organism, is, I deem, unphilosophical; and if we deny contagion to be in operation, the only place in which those circumstances of heat and moisture are present, and in which they may be developed, is in the alimentary canal, and through the ingesta introduced. So far as I know, this is the only reasonable explanation that can be afforded.

But this influence of a dry state of the atmosphere is not confined to cold weather. It has long been noticed in summer, although, so far as I know, not sufficiently insisted upon. Experience proves that, in some periods of the greatest intensity of an epidemic, the weather has been dry, and the barometer high, and the atmospheric temperature not necessarily low. These circumstances are, as I before stated, precisely those most likely to contaminate the water and eatables taken by a population; and the reverse of those which favour the evaporation of poisonous miasmata, namely, heat, moisture, and a fall of the barometer. It probably explains the reason why the Thames water is more fatal in dry weather with a high barometer, and this at a period of autumn when the temperature is not so elevated. To this fact also the immunity of some low districts may occasionally be referred, when the high suffer most. The poisonous particles, otherwise suspended in the atmosphere by the ammonia and moisture, are kept to the ground, and thus become part and parcel of the water we drink. It is remarkable how this dry state of the atmosphere was observed in the London epidemics of cholera. Thus Mr. Glaisher remarks: "In the year 1854, the pressure of the atmosphere was very great; the temperature generally high; sky overcast; direction of the wind, north-east and south-west; and the velocity of the air was less by one-half than its average for some time before; and, at the time of the greatest mortality from cholera, the barometer reading was remarkably high, and the temperature above its average. A thick atmosphere, though at times clear, everywhere prevailed; weak positive electricity; no rain. In low places, a dark mist and stagnant air, with a temperature in excess; temperature of the Thames very high; a high night London temperature; a small daily range; an absence of ozone; and no electricity." (*Board of Health Reports.*)

The three epidemics of 1832, 1848, and 1854, were attended with a particular state of the atmosphere, characterised by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases, the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854, the temperature was above its average; and a total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion (*Journal of Public Health*, No. IV, December 1855). As if, moreover, denoting the absence of ammonia in the air, Dr. R. D. Thomson mentions that the air in choleraic wards as well as the external air was *acid*, and alkaline only in sewers.

4. There is also a general remark which will explain the exemption of certain parts, even though these were possessed of waters containing the same general excess of oxygen. I believe, in London—and here I speak especially of the West End—that one of the reasons of the innocuity of these waters was their alkalinity. My attention was forcibly called to this point by the case of a patient for whom I had ordered on one occasion the infusion of roses. I was soon afterwards sent for in a hurry to see my patient, who was supposed to have been poisoned, the lips and tongue having assumed a bright yellow brown colour. The medicine had been taken, and afterwards a glass of water. The whole was explained on testing the water, which was found very alkaline, owing to an excess of *lime*. The water in Dorset Square and Montagne Square, at my own and my patient's house, as well as that of the Bryanstone Square pump, was strongly alkaline from the same cause. This innocuity of the water, even if it contains fermenting matters, is analogous to the fact observed by Pariset in Egypt. Pariset believes that the reason that the overflowing of the Nile proves so salubrious in its effects is, that its waters are *very alkaline;* and hence, as it spreads over the land, it reaches the dead, and temporarily neutralises the miasmata; and thus the plague is stayed (*British and Foreign Medico-Chirurgical Review*, p. 249).

If such be, however, the case with water containing excess of alkali, it is not so with sea water, especially if much diluted with fresh water, or fresh water with chloride of sodium. An experiment of Pringle proves that sea salt, *if in small quantity enough*, favours putrefaction. "One drachm of salt preserves two ounces of fresh beef in two ounces of water above thirty hours uncorrupted, in a heat equal to that of the human body; or, what amounts to the same thing, this quantity of salt keeps fresh meat about twenty hours longer sweet than pure water. But half a drachm of salt does not preserve it above two hours longer. . . . Now, I have since found that twenty-five grains have little or no antiseptic virtue, and that ten, fifteen, or even twenty, manifestly both hasten and heighten the corruption. It is, moreover, to be remarked, that, in warm infusions with these smaller quantities, the salt, instead of hardening the flesh as it does in a dry form in brine, or even in solutions such as our standard, here softens and relaxes the textures of the meat more than plain water, though much less than water with chalk or tartareous powders" (Pringle, *Philosophical Transactions*, 1750). Pringle found, moreover, that, of the quantities ten, fifteen, and twenty grains, in two ounces of water, the former was the most putrefying in its effects. Now, sea water contains between twenty-five and twenty-six grains in two ounces; and therefore, if diluted with more water, as where the sea is in connexion with large rivers, so that the proportion of sea salt is still further diminished, it will putrefy much sooner. Hence, perhaps, the reason that cholera is more rife along seaports, where large rivers are also present, because putrefaction is more rapid. This view explains, moreover, the following fact commonly observed. It is known that a tough duck or curlew will eat very tender if steeped over night in weak brine or sea water. Tenderness of meat denotes incipient decomposition, although to a small degree. Fresh water will not, except in very warm weather, produce a similar effect. I have verified the same point by another experiment. Sea water from Brighton will keep sweeter in stoppered bottles than sea water from Southend. Some of the former, collected on April 20th, of specific gravity 1018, was fresh up to the 10th June; some of the latter, collected May 12th, of specific gravity 1022, was putrid long antecedent to the 10th June. This is not surprising. The Southend waters, contaminated more or less by the sewage matters of the Thames, and yet containing less sea salt, would be, *cæteris paribus*, more readily decomposed. Besides, sea water contains a large quantity of sulphates—from four in our channel to seven parts per 1000 in the Mediterranean; and these salts, in contact with much animal matter, readily decompose, giving out sulphuretted hydrogen.

5. The existence of a poison in the water drunk may be inferred from the following considerations.

There must be in many cases something more than an emanation or miasma to explain the recurrence of some epidemics. The mere stench or overcrowding of a locality will not always account for the spread of disease. Take the cholera epidemic at Berlin in 1853. "In Prenzlau Street and Gollnow Street, in Berlin, there are a great many overcrowded cottages, and yet the number of cases here was small. The solution of the puzzle is all the more difficult when it is considered that the houses in Büsching Street, Wine Street, and in front of the new King's Gate, stand much more open than the rest of the houses of the whole of the (ninth) medical district. There can be no question here of confined space and want of fresh air—circumstances which are usually supposed to favour the origination of cholera; and yet there were four to five times more cases in proportion to the number of the inhabitants than in the narrow Gollnow Street, in which the circulation of air is much impeded..... It certainly seems extremely surprising that, of the forty streets which compose the medical district, precisely the most openly situated of them were those which were most visited" (*Journal of Public Health*, vol. i, p. 280). Now, this something more is often, without doubt, bad water. Thus Scilla, a small village about fifteen minutes distance by sea from Valetta, in Malta, has always escaped cholera, while it has prevailed extensively in every other part of the island. All Malta (except this little village, which is supplied by tanks) is supplied by an aqueduct. And similar cases abound.

The *Board of Health Reports* show that the air was, during these cholera epidemics, in a weak positive electrical state. This, as before said, is precisely that state which attracts oxygen, and, as such, is most favourable to decomposition. It is to be regretted that the condition of the water was not also given. There is room in this direction for much inquiry. A blast of air, a direction of the wind, may determine the poisonous change in water: north and east winds bring usually positive, south and west bring negative electricity. Indeed, Dr. Bressler, in reference to the Berlin epidemic of cholera before referred to, proposes the question, whether, under certain circumstances, the admission of fresh air may not be prejudicial, as actually being the bearer of the miasma. The meeting of an epidemic may thus be synonymous with the meeting of a pernicious current—say a positive electricity, which developes the latent poison; and explains in this manner the healthiness of one place to-day and its unhealthiness to-morrow. Certain atmospheres, as well as waters or subsoils, in themselves harmless at one time, may, by virtue of their being the fittest nidi for the development of ferments, very rapidly multiply these, the moment they come into juxtaposition. This opinion is well set down by Surgeon J. H. Kerr Jones, of the 56th Regiment, in his evidence on the Bermuda fever (p. 62):—"When fever prevailed to a frightful extent among the troops in Upper Scindh, I have often at night been exposed with impunity for hours whilst wild fowl shooting, at what we considered the most concentrated sources of the poison, whose effects, even when infinitely diffused, were supposed to be deadly. The neighbourhood of such situations remained free from any disease. The troops who marched over them, and daily encamped there, continued in the enjoyment of health: it was *only when the epidemic influence prevailing on the rock of Jukkur was encountered*, that every *drain* and *cesspool*, or room abounding in exhalations from the human body, became impregnated with its poisonous property."

6. It is probable that the *chemical rays* of light may in some way be concerned in the development of oxygen. According to recent experiments, these chemical rays vary much in quantity. A solution of quinine (the photographic test) becomes quite bluish and milky looking when these abound, and quite transparent when they are few in number; and hence the reason that photographs are so easy of execution at one time, and so difficult, if not impossible, at another. The effect of chemical rays on putrefaction has not, so far as I know, been studied; still, some facts may exemplify how an effect may be produced on them. A solution of chlorine in water, if exposed to light, will decompose the water, and liberate the oxygen. Some chlorides have an opposite effect. Thus, freshly precipitated chloride of silver will also, if exposed to light, decompose water; but the oxygen set free unites with the silver, forming oxide of silver; and thus the liquor assumes a black colour. Of the several rays of the spectrum, the violet produce this change more rapidly than the red on the moist chloride of silver. "Their characteristic effect is to promote those chemical decompositions in which oxygen is withdrawn from water and other oxides; and hence they are sometimes called *deoxidising* rays." (Graham's *Chemistry*, 1st. ed., p. 93.)

Recent inquiries, at any rate, prove that there is a connexion between these chemical rays and the electrical currents induced. How far, then, the two causes are co-operative, remains to be shown; but that they are often concurrently present, and in their effects materially influence decomposition, cannot be denied.

The six conditions enumerated above all produce decomposition; and we may thence, I think, infer that, although their coexistence may not always be necessary for the development of every epidemic, their coexistence may materially increase the power of its action, as their disunion may diminish its intensity.

[*To be continued.*]

PLACENTA PRÆVIA.

By T. O'CONNOR, Esq., Surgeon, March, Cambridgeshire.

I BELIEVE that placenta prævia is not an uncommon occurrence this year. Within the last six weeks I have had two cases; both have done well.

CASE I was that of a woman, aged 35 years, in her seventh pregnancy; her previous labours had been without complications of any kind. I was called to her a fortnight previously to her labour having come on; and a more frightful case of syncope I have no recollection of witnessing. The bed and clothes were saturated with blood; the woman's face was pale as death; she was lying across the bed apparently lifeless. Having learned from the attendant that she expected to be confined in a month, I made an examination, and found the os uteri undilated; there were no labour pains. Although the hæmorrhage had ceased, I thought it advisable to plug the vagina, feeling that further discharge, in any quantity, must be fatal. As soon as she was able to swallow, I gave a drachm of Hoffmann's anodyne in a little water, and repeated it every fifteen minutes for four or five times. She then became for a moment conscious, then incoherent, and immediately the fainting returned in an appalling degree. This condition, alternated with momentary conscious intervals, lasted two hours, every moment of which I expected to see her expire. By this time, I contrived to administer with a a spoon two ounces of compound sulphuric ether. A tranquil sleep ensued; and I left her with injunctions that I should be sent for when she awoke, or if flooding or any untoward symptom occurred. The sleep lasted two hours, when, agreeably to my instructions, I was again summoned, and found her refreshed and doing well. I then withdrew the plug, as there had been no return of flooding; and as it had been now four hours in the vagina, I enjoined, for the present, the recumbent position. After two days attendance I discontinued my visits, as there was no appearance of labour, and as the woman was verging towards convalescence, having taken care that the bowels were relieved by castor oil.

In a fortnight afterwards, I was again called to the same person early in the morning, in a great hurry. I learned from the messenger that, while turning in bed, she had been seized with flooding to an alarming extent, and had