about an ounce of fluid. The heart weighed fourteen ounces and a half. There was no excess of superficial fat, but the muscular tissue had a yellow colour and a flabby consistence. Under the microscope, the fibres were very fatty; a string of fat-globules or of fat-crystals often occupied the centre of a fibre. The left ventricle was quite uncontracted. The valves were healthy. The blood was everywhere fluid. The lungs were healthy.

Mr. Holmes exhibited the hearts of the above two cases before the Pathological Society. In the abstract of the cases published in the *Transactions*, I find that in the case of Henry C., he says that the "breathing and pulse ceased simultaneously." This is certainly an error: the patient continued to breathe for some time after the pulse had stopped.

In relating the case of Richard J., Mr. Holmes says that "the patient took chloroform well, and was kept under its influence for the necessary time—about seven minutes." This also is an error. The facts are as stated above. No doubt seven minutes were spent in the attempt to administer the chloroform, but, owing to the failure of the pulse, it had to be left off, and a second attempt was made with the same result. But the patient never was completely anaesthetised.

In this case, the heart was certainly very much diseased; but, as I have said before, I have seen cases with healthy hearts in which the pulse presented precisely the same characters. I attach comparatively but little importance to the state of the pulse before inhalation. What I am careful in watching is the effect produced by the chloroform, and especially at that stage of anaesthesia where unconsciousness comes on. A pulse that is weak before chloroform is commenced may improve under its effects, especially when the influence of the mind is removed. But I am quite sure that in some cases the converse is equally true; namely, that a pulse which is good at the commencement of inhalation, becomes weak, irregular, or intermittent, as narcotism is produced. If, instead of improving, the pulse become worse as consciousness is abolished, I think it shows that the chloroform acts dangerously on the cardiac ganglia. In all such cases, chloroform should be discontinued. I fear many deaths which might have been avoided occur from this cause, through not attending to the timely warning which the pulse gives. It is impossible to estimate the amount of mischief effected by the directions given on this point by Mr. Lister, in his article on Anaesthetics in Holmes's *System of Surgery*, when one considers the deservedly high reputation of the author, and the importance of the work in which the article appeared. The burden of Mr. Lister's remarks seems to be on the importance of devoting the whole attention to the breathing to the exclusion of every other duty. Now, I admit, as all who are at all familiar with the subject must admit, that obstruction to the breathing is the most common of the dangerous effects of chloroform. But any embarrassment to the respiration, and the evil consequence that may result therefrom, are so obvious to the most casual observer, as Dr. Snow long ago pointed out, that I cannot imagine why Mr. Lister should labour so much to prove a point which every one already admits.

With reference to the pulse, Mr. Lister states (*System of Surgery*, p. 495, second edition) that "the safety of the patient will be most promoted by disregarding it altogether." Now, it is very remarkable that during the last ten years, from the time when the article containing this paragraph was written, there seem to have been many more deaths from chloroform than in the previous decade. How much of this might be due to the doctrines inculcated by Mr. Lister, is of course impossible to determine. It is, however, a significant fact, that two deaths (both of which were published in this JOURNAL) occurred from chloroform during the last year at the Edinburgh Infirmary, where, it is said, no deaths had happened before. And we find that, since the article referred to was written, Mr. Lister himself has met with two or three cases which almost proved fatal. These cases seem to have made some impression upon his mind, and to have somewhat shaken the views he enunciated in the original article; for we find that he admits, in the last edition, that "with these examples before us, of deviations from the usual order in which the various functions are affected by chloroform, no one can say it is impossible that here and there an individual may be found so constituted that, without any undue proportion of the narcotic vapour to the air inhaled, the cardiac ganglia may fail before the respiration is interfered with." This is just what I have endeavoured to insist upon; but also I maintain that this failure of the cardiac ganglia may show itself gradually as well as suddenly, and that the pulse, therefore, becomes a most important signal of approaching danger. It is to be hoped that, in the next issue of his article, Mr. Lister will be induced, after increased experience of chloroform, also to admit the importance of carefully watching the pulse as well as the respiration. I confess experience has taught me that chloroform, even when most care-

fully administered, is more dangerous than is generally supposed. If I were, unfortunately, compelled to take an anaesthetic, nothing would induce me to take chloroform.

[To be continued.]

ON DILATATION OF THE CERVIX UTERI BY BOUGIE.

By J. H. AVELING, M.D.,
Physician to the Chelsea Hospital for Women.

I HAVE read with great interest the papers by Dr. Matthews Duncan which have appeared in the last two numbers of the JOURNAL; for, having partly gone over the same ground some years since,* I was anxious to see how far his observations and conclusions agreed with mine. With regard to the relative values of tangle- and sponge-tents, we are, in the main, agreed; but against the practical conclusion at which he arrives I must raise my feeble protest. The authority of Dr. Duncan is deservedly so very great, that I tremble to think of the mischief which must be caused should medical men generally adopt his method of forcible dilatation.

The bougie, he thinks, is the best instrument for dilating the cervix uteri in gynaecological practice. I beg respectfully to differ from him for the following reasons.

1. The uterus is not a body rigidly fixed, but one which, under very slight mechanical influences, tumbles about hither and thither; and this extreme mobility renders it utterly impossible for an operator to be certain that he is applying the wedge-force of the bougie in a proper direction.

2. The dilating action of a metal wedge grazing along the mucous lining of the canal is objectionable and unnecessary, as it may be avoided by using tents and other dilating instruments.

3. The uterus may be in a spongy and flaccid condition, capable of being bent upon itself by the pressure of the point of a bougie acting on either side. In such a case, the extreme liability of the bougie penetrating the side upon which it impinges is obvious.

4. The cavity of the uterus varies in length, so as to make it difficult to determine whether the point of the bougie have arrived at the internal cervix or the fundus uteri. If it, by mistake, be "urged through" the latter, serious consequences might follow.

5. In narrow strictures a thin bougie must be used, and the more resisting the stricture and the thinner the bougie, the greater is the risk of the uterine walls being penetrated.

Dr. Duncan uses the sound in such a heroic manner, that it can only be his very great skill which prevents him from doing much mischief. He says: "I have observed that, in using great force to pass a common uterine probe, the instrument is bent." I have always learnt and taught that no force should be used in passing the uterine sound, and I cannot help thinking that the pressure of "four pounds" (the amount required to bend a sound) is far greater than should ever be employed. The bougie he also urges forward with equal confidence and force. In passing it through the internal os, he says, "the difficulty may be extreme." I am satisfied that I have frequently applied a forward pushing force at the point of a probe equal to 4 lbs., or even more, and with the wedge or conical pointed bougie a force probably exceeding 200 lbs., on the square inch of surface of part undergoing distension. "When a great force is applied, the sudden or jerk-like advance of the bougie through the internal os, and the smearing of the instrument with blood, show that the part has not been gradually expanded, but lacerated—an accident which, I think, it is desirable to avoid."

Dr. Duncan tests the value of the different modes of dilating the cervix uteri by three considerations: (1) The amount of force exercised; (2) the amount of expansion produced; and (3) the amount of time required for producing the expansion. In considering the force exercised, its direction, as well as its amount, must be taken into account. It is true, as Dr. Duncan says, that the force of the bougie is "limited only by the strength of the instrument," but its expanding force, even when exerted in a correct course, is not so direct as that of tents and other dilating instruments. Then the amount of expansion produced by bougies is much less than can be effected by tents and dilators; and the amount of time required to produce the dilatation by Dr. Duncan's method is scarcely less than that required by other plans; for, by a number of "sittings," or by using a "large instrument very long and perseveringly, he only produces a dilatation of about a quarter of

* "On the Relative Value of the Various Substances which have been used in Dilating the Neck of the Womb; with a Plan for Deodorising Sponge-Tents."—*Obstetrical Transactions*, London, December 4th, 1867.

an inch." The question of rapid dilatation is one of great importance, and Dr. Duncan has done good service in drawing attention to it. Tents, of whatever substance they may be made, often produce disagreeable, and sometimes fatal, symptoms. Should, however, rapid dilatation be eventually preferred to the slower process, the instrument used must be one capable of being passed through a stricture without force, and afterwards, by some mechanical contrivance, made to expand so as to act in a direct manner upon the constricted tube. It must be done by art, and not by force.

ON THE SALUBRITY OF DIFFERENT PLACES ON THE NORTH-WEST COAST.

By THOMAS GIBSON, M.D., Bedford.

HAVING paid a good deal of attention to various watering places, more with an idea of their being conducive to health than subservient to pleasure, I have come to the conclusion that various sheltered situations in out-of-the-way corners on the north-west of Lancashire and Cumberland are overlooked on account of their remoteness from large towns or gatherings of the pleasure-seeking community. I write especially to point out some of those which are least conspicuous for the amount of rainfall; and in which, situated as they are upon sandbanks, the absorption of the water is so rapid, and the evaporation so small, that they are peculiarly adapted to those complaints which are benefited by a dry bracing atmosphere.

Southport and Silloth, according to my observation, are amongst those which come under this head, the latter place much more so, having been brought into notice more recently. Although somewhat out of the regular route of places visited, it is nevertheless one, in my opinion, deserving of note. The place is situated about twenty-two miles north-west of Carlisle, on the Solway. In front of the town, which has about a thousand inhabitants, are the Scottish hills of Galloway and Dumfriesshire. "I attribute the extraordinary salubrity," says the Rev. Francis Redford, "to its freedom from fogs, there being on an average not more than two foggy days in the year"; and to the constant presence of that mysterious agent called ozone.

I have paid two visits to this place myself; one about five years ago, and another for three weeks last August; and from my observation on the spot I was led to believe that the extraordinary elasticity of the atmosphere, so to speak, had a good deal to do with the healthfulness of the climate, the air being bracing and congenial, and the ever shifting winds tending to prevent that stifling sensation which one is apt to experience in more sheltered situations in the heat of summer. The Scotch hills in front are not nearer than seven or eight miles across the Solway, and Skiddaw and Helvellyn, which form the south-east background, are more than fifteen miles distant, at a rough guess; while, from the pier, from which boats ply daily to Liverpool, Dublin, and Douglas, may be seen the Isle of Man itself in the far south-west. It is very remarkable that, although heavy winds are not often experienced in the summer, yet there is almost always a pleasant breeze. No time can be fixed on when wind is likely to prevail, on account of the current of the Gulf Stream rushing in between the hills, as it were; and the length of the Channel, with the ebb and flow of the tide, making it, although an arm of the sea, something like a tidal river, may in some measure account for this uncertainty of the prevailing winds, although for the most part and for a longer duration they are south-westerly; *i.e.*, from the main ocean inland.

The mean annual temperature is about two degrees lower than in Cornwall and Devonshire; while it is less than a degree lower than at Torquay.

The peculiarity of this kind of climate leads me to think that it is of great benefit—in fact, practical results adduce it—in all affections where the general tone of the body is lax and low, and especially in delicate females it is of infinite advantage. Amongst other cases, I met with a gentleman while staying there, who had been under medical treatment both in Edinburgh and London for a long time without any benefit, he being a sufferer from general lassitude and want of appetite. He was a Scotch gentleman, but had been resident in London for the last thirty-five years. His physician, after giving him all the tonics he could devise for two or three years, sent him, as a last resource, to try his native air. After staying in Edinburgh for five months, and getting no better, he was casually made acquainted with this place, and he determined to break his journey and visit it. I saw him after five days' sojourn there, and he informed me that his sensations were such that it was like being in a new world. His appetite and relish for food were such as he had not experienced for some

years, and he confidently hoped that he was beginning a new era in his existence. The climate has seemed to do good in the numbness of the joints which is often complained of by delicate females; and the dryness of the soil renders exercise possible immediately after rain, for the rapid absorption of the water is proverbial.

There is another little place called Grange-over-Sands, not far from Lancaster, which is sometimes called the Torquay of the North. Its principal distinction is that it is remarkably sheltered from the north and east winds; it lies not far from Windermere, the queen of English lakes, and in front of it, stretching for miles in a westerly direction, is the broad expanse of Morecambe Bay.

Having been for a length of time an observer of those localities that are sometimes neglected in consequence of not being fashionable resorts, I have only to add that those places to which I have alluded more particularly will be found as beneficial as most others. For the overwrought man of business, or those whose means will not allow them to move to the generally recognised health-resorts, I would almost guarantee that invalids, of the class to which such a climate is adapted, will find that health and economy in living may be obtained much more readily, because more time can be allowed for a chance, where less expense is involved, of a radical cure in most cases.

In bringing these places under notice, my main object is to elucidate facts. Meteorology is a kindred science to medicine. Climate has a good deal to do with the constitutions of individuals; but where ozone abounds and fogs do not prevail, where marshes are drained and sandhills are in lieu of cesspools, we do not, as a rule, hear much of ague, consumption, or pestilential fever.

Mean Annual Temperature of the Air at the undermentioned Places.

	DEG.	SEC.
Silloth, latitude 54 deg. 51 m. 51 s.	49	1
That part of England between lat. 51 deg. and 52 deg.	49	2
Cornwall and Devonshire	51	0
The Isle of Wight, including Ventnor... ..	51	3
Torquay	50	7
Hastings	48	0
Worthing... ..	49	0
The Isle of Man... ..	48	1
Scarborough	46	5
Tynemouth	45	6

The influence of the Gulf Stream is apparent in the foregoing statement. To this alone can the high mean temperature of Silloth be ascribed, which, it will be observed, is higher than that of Worthing, on the south coast of England, and nearly the same as places situated between latitude 51 deg. and 52 deg. The prevailing wind at Silloth is south-west, which being in contact with the Gulf Stream in its course causes this comparatively mild climate.

The temperature is little below that of Torquay, but without its enervating and depressing influence, owing to the greater dryness of the atmosphere at Silloth. The average number of days in the year upon which rain falls is at Silloth 150; in Cornwall and Devonshire, 186; Torquay, 173; Tynemouth, 179; Isle of Wight, 142.

The mean daily range of temperature—or range from the lowest to the highest readings of the thermometer—is only 13.2 deg. The invalid who suffers from extreme cold will value this. Ozone is scarcely absent one day in the year. The place nearest approaching the quantity of ozone found at Silloth is at Osborne House in the Isle of Wight. The ozone scale at present in use by meteorological observers extends only from 0 to 10 deg. A scale extended to 20 deg. has been sanctioned by the Meteorological Society and the Registrar-General for this station only, and the full extent is frequently obtained.

For these particulars I am indebted to the Rev. Francis Redford, M.A., F.R.S.E., rector of Silloth, who for the last eighteen years has been a painstaking observer, and who has given the annexed table of the rainfall for seventeen years, the mean being 32.71 inches during the whole of that period.

Rainfall at Silloth.

INS.	INS.
1855 22.060	1864 33.367
1856 25.989	1865 28.736
1857 27.486	1866 36.760
1858 32.319	1867 26.477
1859 39.802	1868 37.679
1860 37.810	1869 32.237
1861 42.471	1870 26.387
1862 44.178	1871 30.640
1863 43.845	

Mean of seventeen years, 32.717 ins.