AN ACCOUNT
OF
A SUBSTANCE
OBTAINED FROM A
DISEASED OVARIUM,
WITH SOME REMARKS ON
DISEASED SECRETIONS OF AN ANALOGOUS NATURE.

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AT one of our late meetings a paper by Dr. Merriman was read, containing an account of a tumour of the ovary, from which was discharged by puncture an unctuous fluid, which, as it acquired the temperature of the atmosphere, assumed a solid form*. The doctor having sent me a portion of it for examination, I shall lay before the Society a detail of some of the experiments that I performed upon it; to this I shall add a few observations upon other substances of an analogous nature that I formerly examined, and I shall conclude with some remarks upon the relation which

* See page 71.

these bodies bear to each other, considered as morbid secretions, derived from the same organ in different states of action.

The substance procured by Dr. Merriman, when in its solid form, was of a brownish yellow colour, nearly without odour, and of a consistence precisely similar to that of pure lard or butter. Heat was gradually applied to a portion of it; at 65° it remained perfectly solid, at 70° it was sensibly softened, and at 80° it became completely fluid; at 160° it began to boil, and by being still further heated, it swelled up and burned, emitting an animal empyreumatic smell. When the experiment was reversed, and the substance, after being melted, was slowly cooled, it completely retained its fluidity at 80°; it then gradually became less fluid, until at 70° it was of a thick viscid consistence, and at 65° it again assumed the solid form; the processes both of melting and of solidifying seemed to be gradual, and I could not perceive that there was any exact melting point. Although the substance, while solid, appeared to be homogeneous, yet when it was melted a number of small films were perceived to be floating in it. When the heat was withdrawn, and it regained the solid form, it did not exhibit the least tendency to crystallization, or to any specific arrangement of its parts; when melted on hot water, and afterwards cooled, the under surface was found to be perfectly smooth. The tests for alkali were applied to it, both in its

solid and its fluid state, but the effect upon them, if any, was extremely slight; nor could this or any other salt be detected by the appropriate tests, in water on which the substance had been kept for some time in a state of fusion, and with which it had been repeatedly agitated.

A portion of the substance was boiled with alcohol, and the fluid was filtered while hot; no visible solution had taken place, but by the addition of water a very slight turbidness was produced, and also, upon the cooling of the fluid, a minute film was perceptible upon some part of its surface. The fatty matter was heated with pure potash, when a saponaceous compound was formed, which was capable of being diffused through water, or suspended by it, so as to convert it into a uniform milky fluid; by the addition of muriatic acid the fatty substance was precipitated in the form of dense white flakes. I was not able to detect the presence of albumen in the fatty matter, by employing the usual tests for this primary compound; the effects of a high temperature seemed rather to indicate its presence, but these may be attributed to the small films which were observed in it, probably of a membranous nature, and which, I apprehend, were only accidentally mixed with it. On this point, however, I conceive that the analysis of animal substances is still imperfect, as I believe that we have no accurate method by which we can detect a small quantity of albumen when united to

a large proportion of a concrete oil. I may farther remark concerning this substance, that it was very little disposed to become rancid ; after being kept for four months, it shewed no disposition to putridity, nor to any kind of spontaneous change.

I have selected the above observations as what I apprehend are sufficient to enable us to form an opinion of the nature of this peculiar body, sufficiently accurate for all purposes of physiology or pathology, and from them we learn that it consists of a concrete animal oil, nearly in a state of purity, very similar to butter or lard ; that it contains no alkali or other saline ingredient, and very little, if any albumen, or other primary animal compound.

The result of the above examination led me to recur to some experiments which I had formerly made upon the fluids obtained from some cases of encysted dropsy. Of one of these an account has already been laid before the Society in my paper on the nature and analysis of animal fluids* ; this I shall therefore notice in a very brief manner only. Its chemical composition was considerably different from that of the fluid of dropsy in its ordinary state ; the most important alteration consisted in a part of its albumen being converted into a matter exactly resembling mucus, or that substance which gives the specific character to the nasal and salivary secretion. It was doubtful whether this

* Med. Chir. Trans. Vol. IV. p. 81.

fluid had been collected in the ovarium, or in a cyst connected with some other of the viscera ; in either case, however, in its ordinary state it would have been entirely albuminous, or would have consisted of water holding in solution a greater or less portion of albumen, with a definite quantity of the uncoagulable matter of the serum, and of the salts which exist in the blood.

The next fluid which I shall describe, was one, the source of which was more clearly ascertained to be from a dropsical ovarium ; its properties were as follows. Shortly after it had been discharged from the body it began to separate into two parts ; a white semi-fluid substance very slowly subsided, while the remainder was left nearly transparent ; the fluid was poured off from the sediment, and they were each of them examined. Without descending into a minute detail of experiments, it may be sufficient to state with respect to the fluid part, that it was properly albuminous, similar to the serum of the blood, except that it contained a considerably less proportion of albumen, but with the usual quantity of uncoagulable matter and salts. The sediment, which very much resembled cream in its appearance, was then submitted to a number of experiments, the general result of which was that it consisted of minute flakes, which seemed to be similar to coagulated albumen, mixed with a proper oily matter. By agitation in water these two substances were separated from each other ; a fine

white flaky matter very slowly subsided, while the water was left milky, and seemed little disposed to recover its transparency. By boiling the sediment with the oxymuriate of mercury the separation of the albuminous part was more completely effected. The specific gravity of the fluid in its entire state was 1.0103; that of the sediment, carefully separated from the albuminous fluid, was 1.023; when the sediment was slowly dried it acquired a yellow colour; by still farther increasing the heat it was charred, and at length burned with a considerable burst of flame, exhaling an odour very like that of toasted cheese. The creamy sediment did not exhibit any indication of uncombined alkali by the application of the appropriate tests, but the quantity of neutral salts, especially of the muriate of soda, was considerably greater than in an equal weight of the solid contents of the serum; it differed from pus essentially in not exhibiting any distinct globular appearance.

In this case we observe a still farther departure from the ordinary condition of the parts; the secreted fluid, which naturally contains albumen, and which, in the second instance that has been referred to, had this substance converted into mucus by a kind of imperfect coagulation, here had a part of its albumen completely coagulated, together with the addition, if not of a proper oil, at least of an ingredient nearly resembling oil in its leading properties. The change was still farther

advanced in the case that occurred to Dr. Merri-
man, where the whole, or nearly the whole, of the
albumen had disappeared, and was replaced by a
concrete oil, while we may perhaps be allowed to
extend the series, by connecting with it those ex-
amples of ovarian tumours, which contain perfectly
organized solids of various kinds, similar to those
which enter into the constitution of the body.

If we may be permitted to regard the different
substances mentioned above, which are the pro-
duce of the ovary, as proceeding from the same
secreting surface in different states of action, we
arrive at the conclusion, that according to the na-
ture or degree of this action the same organ may
generate the common dropsical fluid, which essen-
tially consists of albumen, a proper mucous fluid,
and a perfect adipose matter, together with various
shades or combinations of them. It may indeed
be objected to this view of the subject, that the
secretion was originally discharged in its natural
condition, and was afterwards converted into mu-
cus or fat by an interchange among its constituent
particles, after it had been deposited in the sac.
To this objection I should answer, that nothing
resembling the conversion of albumen into a con-
crete oil has ever been observed in the laboratory,
nor does it seem possible to be effected without
the co-operation of the vital powers, that the pri-
mary constituent elements of these substances are
not the same, so that it would be very difficult, if
not impossible, for this change to take place in a

close cavity, where nothing could be received or discharged during the process, and lastly I may remark that the saline ingredients of the substances in their altered state are not the same with those that exist in them in their ordinary form, as I found by sufficiently decisive experiments.

Perhaps the most important of all the improvements in physiology is that which was brought about by Cullen, who discarding the learned hypotheses of his predecessors, who attempted to account for the ultimate changes that were induced upon the body by chemical or mechanical agencies, substituted in their place the action of the vessels, and particularly of the capillaries of the arterial system, in which, as he correctly stated, there is every reason to suppose that the most important operations of the animal economy are carried on. The remark applies particularly to the function of secretion; here a portion of the blood, or some of its constituents, after it leaves the larger vessels, acquires a new form, having its chemical and physical properties more or less changed during its passage through the secreting arteries. This action of the vessels is to be considered as a *vital* operation, depending upon a property or properties possessed by these vessels, which belongs to living organized matter alone; yet as the result of the operation is to induce a physical change in the substances subjected to its action, it becomes a legitimate object of inquiry what is the nature of

the change by which the process is effected, or by what means does the action of a vessel produce a change in the nature of its contents? The following will suggest themselves as among the most obvious: 1. the relative proportion in which the constituents of the blood enter the vessel; 2. the nature of the blood with respect to its arterial or venous state; 3. the rapidity with which the fluid is propelled through the vessel; 4. the degree of compression or agitation which it experiences during its passage; and 5. the situation in which it is deposited after it leaves the vessel, whether it be in a close cavity or exposed to the atmosphere, or to any other agent, or whether it be so placed that a portion of it is subject to be taken up by absorption as rapidly as it is discharged.

Although it is probable that we may never be able to disclose many of the mysterious operations which produce the ultimate changes of the body, we may remark, that by a proper combination of the above circumstances, we are in possession of a number of agencies which are adequate to the production of a great variety of results. At the time when the blood is undergoing that change in the lungs, by which it is converted from its venous into its arterial state, a quantity of carbonic acid is expired; we may therefore presume that venous contains more carbon than arterial blood, and it may consequently be conjectured that if there be two vessels in all respects similar, except that in

one of them the blood is in a more arterialized state, the fluids secreted from it will contain a smaller proportion of carbon, or approach more to the nature of oily or adipose substances. There are several circumstances which lead us to conjecture that some of the constituents of the blood are more perfectly fluid, or in a state of more complete solution than others ; for example, that the salts are the most soluble, the uncoagulable matter probably the next so, afterwards comes the albumen, and last of all the fibrine. Now, upon mere mechanical principles, but which will apply to all fluids passing through tubes of any description, we must admit that the salts will find their way through the most minute of these passages, while the fibrine, on the contrary, will be the most easily obstructed ; and this agrees with what we actually observe to occur with respect to the secreted fluid,

Without going farther into detail, it will, I conceive, be admitted that the other circumstances which were pointed out may, in the same manner, apply to the case of the fluid passing through the capillaries, and will influence the nature of the results. The next point is for us to examine whether we have any grounds for applying these data to the principles of pathology, whether we can form any opinion respecting the condition of the vessels from the substances proceeding from them. The investigation is confessedly obscure, and what I can offer will be almost entirely conjectural ; but

if conjectures be offered with diffidence, and not obtruded as established positions, they may assist in pointing out the path to important truths.

In the case of a surface, which in its healthy state secretes an albuminous fluid, we find this fluid to contain the same saline ingredients with the blood, and the same quantity of uncoagulable matter, a variable portion of albumen, but always less than that in the serum, while the fibrine is totally absent. Here then the action of the capillaries is simply to separate from the blood a part of its albumen, and the whole of its fibrine, a change, which we easily suppose is effected, merely by the fluid passing through a tube of a certain size and with a certain rapidity, so that the more completely soluble parts alone are suffered to pass, while those that are less soluble are detained,

Let us now suppose that by some cause the capillaries have their action so far altered that their size is increased, that their contraction is more powerful, and that the temperature of the organ is raised. We might conceive, under these circumstances, which may be considered as constituting a certain stage of inflammation, that the albumen will be discharged in great quantity, and may also be partially coagulated, thus forming a mucous secretion, while we may conjecture, that in a higher degree of the inflammatory state, where the arterics are more distended and act with more force,

and where at the same time the temperature is still further increased, a larger proportion of albumen will be discharged in a more coagulated form, together perhaps with a quantity of fibrine, thus producing the various kinds of exudations which are occasionally observed under these circumstances.

I think it unnecessary to pursue these speculations to any greater length, for I must be understood as offering them rather as illustrating the way in which we may hereafter throw some light upon pathology by the researches of animal chemistry, than as supposing that we have any actual proof that the operations to which I have now alluded really exist. The Society will, I conceive, agree with me, that if it were possible to ascertain the exact condition of the action of the capillaries in their various morbid states, by means of the nature of the fluids elaborated by them, we might hope in this way, not only to obtain a valuable indication in the means of cure, but that we might perhaps resolve many curious problems respecting the operations of the animal economy, which are at present quite incomprehensible.