

ON THE DIGESTION OF VEGETABLE ALBUMEN, FAT, AND STARCH.

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DIGESTION OF VEGETABLE ALBUMEN AND FAT.

NEARLY three years ago, Dr. A. Buchanan communicated to me the fact, that he had frequently observed the serum of the blood to present a white appearance when a person happened to have been bled for some affection of such a circumscribed nature as not to interfere with the appetite or digestion; but an opportunity did not occur to me of examining this peculiar serum till the end of 1843, when Dr. Buchanan was so kind as to place some of it at my disposal. The fluid presented the appearance of thin milk, being covered, after standing, on the upper surface with a whitish scum of a denser character than that which was diffused through the fluid. When the fluid was filtered, a portion of the scum remained on the filter while the liquid passed through, possessing still a milky aspect, but obviously being deprived of a considerable portion of its denser particles. Dr. Buchanan, however, observed that this white matter might be accumulated in larger quantities, or at least collected into smaller bulk, by saturating the serum with common salt, when the white matter speedily rose to the surface in the form of a creamy layer, which did not change its physical character even when kept for months. I found that the white matter, both when separated simply by the filter and also when precipitated, if we may so speak, by common salt, contained a substance which was insoluble in æther and alcohol, and that when dissolved in caustic potash, and the solution was boiled with acetate of lead, a black precipitate of sulphuret of lead fell. These experiments were several times repeated carefully with the same result, and were witnessed by Dr. Buchanan, so that we had no doubt in our own minds that the presence of traces of an albuminous substance in the white matter of this serum was established; and we had, subsequently, opportunities of obtaining this matter in larger quantities, so as to confirm the previous experiments.

These results led to a series of researches upon the effect of food on animals, and also on man; from which it was clearly demonstrated that the white colour of the serum in healthy animals is dependent on the introduction of food into the system. The detail of one or two experiments will place this conclusion in a distinct point of view. It was necessary for this purpose to observe the characters of the serum of an individual who had not tasted food for such a space of time, that the effects of the previous meal should have disappeared. On the 10th of March, 1844, from a stout young man, aged thirty, who had tasted no food from the preceding evening at six o'clock, P.M., two ounces of blood were taken at noon, or at an interval of eighteen hours after a meal. The blood coagulated, on standing, in the usual manner, and the supernatant serum was found to possess a pale-yellow colour, and to be perfectly clear and limpid.

After the abstraction of the blood, the individual dined upon twenty-four ounces of a pudding consisting of two parts of wheat flour and one part of suet, seasoned with salt. At three o'clock, or in about three hours after the food had been swallowed, seven ounces of blood were taken by venesection from the arm. The clot formed as usual, no appearance of a buffy coat presenting itself. The serum was whitish and opaque; when heated, it became more translucent, apparently from the solution of some of the solid particles diffused through it, or, as was afterwards apparent, from the liquefaction of the fatty matter diffused through the serum. The whole fluid possessed a somewhat syrupy cast of appearance, and was very heavy, its specific gravity being as high as 1029.8. Comparing this density with the average density of serum, as we find it in physiological works, 1026½ by Dr. Thomson, 1027 to 1029 by Müller, it must be pronounced high; but as the specific gravity of this fluid has never been properly estimated in perhaps the truly healthy condition of animals, and more especially in parallel cases with that described, no conclusion can be deduced in reference to the density of the serum as compared with the normal standard. On throwing the serum upon a filter, a portion of white matter remained attached to its interior surface, while the liquid which passed through retained still a milky aspect; but perhaps the most interesting result obtained by this experiment was, that on drying the filter and holding it between the eye and the light, it was found to have imbibed a considerable amount of oily matter. The white substance remaining on the filter presented characters similar to those of albuminous or fibrinous matter.

The facts detailed having shown that there was a decided dis-

tinction between the serum of a person previous to and after taking food, it became an interesting point to pursue the inquiry, and to ascertain how long the milky aspect would continue in the blood. Accordingly, at six o'clock P.M., or six hours after the meal, the individual was a third time bled to the extent of seven ounces. The serum was in this case very milky, and threw up, on standing, a considerable white scum to the surface, which could be drawn up by a sucker and examined. On throwing the serum on a filter, a small portion of white matter adhered to it, possessing albuminous characters; the serum passed through with a milky colour, but no imbibition of oil could be detected on the filter itself. A careful examination of this specimen of serum could detect not a trace of starch by means of iodine and chlorine, and this led to the investigations detailed in the subsequent parts of this paper.

The inferences deducible from this experiment appear to be, —1st, that the serum of a healthy individual at a certain period of time, after partaking of a meal, is clear and limpid, and corresponds with the description of this fluid as we find it detailed in physiological works of authority; 2nd, that in three hours after a meal, when the food consists of vegetable albuminous matter and oil, or fat, the albumen begins to make its appearance in the blood, while a still larger quantity of fat in relation to the amount of the albumen taken into the stomach exists in the blood; and, 3rd, that in six hours, while the quantity of albuminous matter in the blood can be detected, the fatty matter has comparatively disappeared—a conclusion which is completely in accordance with the chemical properties of these substances, since we know that some kinds of fat liquefy nearly at the temperature of the human body, and will, of consequence, be in a condition immediately after their introduction into the stomach to enter the sanguineous circulation along with the water present in the stomach, since that fluid appears capable of permeating, with great facility, the coats of the intestinal canal throughout its whole length.

To determine the manner in which the food thus passes into the circulating system is scarcely the province of the chemist. At the same time, since it appears to exist in the current of the blood without having undergone much modification of state from that which it originally possessed in the stomach, there seem no obvious arguments to present themselves against the conclusion that the food is directly absorbed from the alimentary canal by the bloodvessels themselves.

In confirmation of the experiment already detailed, the physiological part of which was conducted by Dr. Buchanan, whose great merits as a physiologist and original observer require no encomiums from me, various repetitions on the inferior animals were conducted by myself, which it would serve no purpose to enumerate minutely, since they all conducted to the same results.

In the majority of these cases, calves were fed on gruel and milk, and after various intervals they were slaughtered. The serum, on examination, when the animal was killed from three to six hours after the meal, was found to be milky, and to leave a greasy stain on filtering paper when the amount of milk or fatty matter used was considerable; while the serum taken from an animal which had been subjected to starvation for a space of time, varying from twelve to twenty-four hours, presented generally a clear aspect. It has been frequently remarked that the serum of diabetic patients exhibits often a milky appearance, and the circumstance of the co-existence of disease with the white serum was considered as a proof that the disease was the cause of the colour of the liquid part of the blood; but it has been very properly remarked by Dr. Buchanan, that the large amount of food consumed by individuals affected with this disease, affords a satisfactory mode of accounting for the presence of such profuse quantities of albuminous and fatty matter in the blood in such instances. I have recently had opportunities of studying this form of serum through the kindness of Dr. William Thomson, and I have found no reason to conclude that the blood of diabetic patients, so far as its milkiness is concerned, is more diseased than that of healthy individuals after a full meal. It is no doubt highly probable that the blood in these instances remains for a longer time loaded with the white matter than in the healthy state of the organism, and that the appetite may, by continually urging in a new supply of food, thus produce a diminished rate of digestion or assimilation in the circulating system; but the presence of the white matter in the serum it would be erroneous to consider as a symptom of disease, at least in so far as the data entitle us to draw such an inference.

Changes produced on Starch in Digestion.—As it appeared to be a matter of importance in the investigation of the changes occurring in the stomach during digestion, in order to prevent complication of the phenomena, that the food should be as simple as possible, I have chosen for the experiments about to be detailed

cases in which animals were fed on vegetable food alone. In general, the species of food selected was porridge, or a mixture of oatmeal and water well boiled. In such experiments as I have seen detailed, in reference to the free acid of the stomach, there appears to have been too little attention paid to the possible results which might arise from a difference in the nature of the food; thus the products of the digestion of starch we should naturally expect to be different from those of the digestion of animal fibrin or albumen, since we know that lactic acid can be produced by a modification of starch, although the same change does not attend the decomposition of the animal substances enumerated. For example, in the preparation of starch an acid liquor collects on the surface of the vessels in which the starch is digested, and in the formation of *sowans* from oat-husks and water by steeping, an acid liquor is developed,—in both instances a consequence of the production of lactic acid at the expense of starch; while in the German dish, saurkraut, the same acid is generated by an analogous action. I do not at present intend to discuss the nature of the acid which presents itself in the stomach during the digestion of animal food, but simply to detail the results of a series of experiments upon the changes produced on vegetable food and starch during digestion. I shall merely content myself with stating, that I have never found a volatile acid in the stomachs of animals which were digesting animal food alone, while in these cases I have invariably found an acid to be present which was fixed at the temperature of 212° , and even considerably higher.

The nature of the Acid developed during the digestion of Starch.
—It does not appear an invariable rule that an acid reaction should always characterize the liquid present in the stomach during digestion, as appears from the following experiment:—On the 11th of July, 1844, the different stomachs of a sheep killed twenty-four hours after partaking of grass, contained between two and three pounds of finely-divided green matter, exhibiting a pulpy consistence, but being entirely destitute of either an acid or alkaline reaction. The pulpy masses were tested in each of the stomachs with the same negative result, and they were afterwards mixed together, diluted with distilled water, and filtered, but still without producing any effect on litmus paper. Experience teaches us, that to determine the presence of a volatile acid in the stomach, great care must be taken in reference to the mode in which the distillation is performed, since if the heat of an open fire is applied to the retort, an indication of the presence of hydrochloric acid will be found in the liquid contained in the receiver by the addition of nitrate of silver. The following experiment affords evidence in favour of this position, and it might be strengthened, if necessary, by the results obtained by other experimenters. The contents of the stomach of a dog which had been fed on porridge made of oatmeal and water, and which afforded an acid reaction with litmus paper, were mixed with distilled water, and filtered. On exposing the distilled liquor to the heat of an open fire in a retort, a fluid passed over, possessing an acid reaction, and becoming opalescent on the addition of a solution of nitrate of silver, and without disappearance in contact with nitric acid; a result plainly indicative of the presence of chlorine in some form in the liquor of the receiver, but, as appeared by the subsequent inquiries, to be explained by the fact that muriate of ammonia must have been carried over by the vapour of water in consequence of the excess of heat.

To determine in a satisfactory manner, therefore, the presence of a volatile acid, it is necessary to distil the fluid contents of the stomach in a water-bath. With this consideration in view, the following experiment was made:—June, 1844, a pig was fed on potatoes and greens, and was killed in half an hour after the food was swallowed. The stomach was slit open, and as much of the fluid contents as possible were poured off; the solid contents were then digested in cold distilled water. Both fluids were filtered and mixed, each, previous to being united, being found to exhibit an acid reaction with litmus infusion. To ascertain whether the statement made by a French physiologist (Blondlot) is correct,—viz., that the acid of the stomach cannot be saturated with chalk,—a quantity of pure carbonate of lime was prepared by dissolving Irish limestone in hydrochloric acid, precipitating a small portion of phosphate of lime and iron which it contains with caustic ammonia, and then throwing down the lime with carbonate of ammonia, collecting the precipitate on a filter, washing it well with distilled water, and heating it to redness in a platinum crucible. An excess of this chalk was then added to the filtered fluid of the stomach, and the mixture was allowed to remain for twenty-four hours, disturbed only by frequent agitation. It was then filtered and found to have been completely neutralized. Having often repeated this experiment on various specimens of gastric fluid with the same effect, I can only account for the different result obtained by Blondlot, by supposing that he had attempted to complete his neutralization of the fluid while it was

in a heated state, and that possibly, if acetic or lactic acids were present, these acids might resist neutralization at an elevated temperature, since it has been found by manufacturers of pyroligneous acid that they cannot succeed in forming a neutral acetate of lime at a temperature approaching that of the boiling-point by chalk alone, but that they require to add milk of lime in order to overcome the acid reaction. Blondlot has deduced the inference from his experiment, that the stomach owes its acid reaction to the presence of an acid phosphate of lime; but as the experiments now detailed do not coincide with those of the French physiologist, it is sufficiently obvious that they do not support him in his conclusions.

Being thus foiled in corroborating the deduction of Blondlot, that phosphoric is the free acid of the stomach, it was requisite to look out for another source of the acidity of the gastric fluid. It was therefore necessary to test the muriatic or hydrochloric acid theory, as propounded by the sagacity of Dr. Prout. (See *Philosophical Magazine*, S. 2, vol. iv. pp. 3 and 120.)

The contents of the stomach of a pig which had been fed on porridge two hours before being killed, were mixed with cold distilled water, and filtered. Six ounces of this fluid, which was quite clear and limpid, were introduced into a retort, and distilled in a water-bath. The distillation occupied several hours; three ounces of fluid were distilled over, which possessed the peculiar smell of such fluids, and reddened infusion of litmus slightly. The three ounces of fluid remaining in the retort had a strongly acid reaction, and gave no appearance of acetic acid on the addition of chloride of iron. From twelve ounces of another portion of the same gastric fluid, four ounces of fluid were distilled over by the heat of the water-bath, which presented the same characters as those of the distilled fluid just described; but in neither instance could a trace of hydrochloric acid be detected by nitrate of silver.

With the liquor remaining in the retort, three experiments were made to determine the possible amount of free hydrochloric acid, on the supposition that the preceding experiment did not serve as evidence to prove the impossibility of its presence. Three equal portions of the fluid were measured out, to the extent of two fluid ounces in each portion.

1. To the first portion a solution of nitrate of silver was added until a precipitate ceased to fall; pure nitric acid was then mixed with the liquid, and the temperature raised to the boiling point. The precipitate was filtered, washed, and weighed.

2. The second portion was evaporated to dryness, and ignited; the residue was dissolved in water and precipitated by nitrate of silver, the solution being acidulated with nitric acid and brought to the boiling point.

3. The third portion was exactly neutralized with caustic potash, evaporated, and ignited; the residue dissolved in water, and the solution precipitated by nitrate of silver.

The results of these experiments are indicated in the following table in grains:—

Experiments.		Weight of chloride of silver.		Weight of chlorine.		Weight of hydrochloric acid.
1.	...	7.81	...	1.95	...	2.00
2.	...	7.17	...	1.79	...	1.84
3.	...	7.97	...	1.99	...	2.04

The atomic weights here employed are Dr. Thomson's:—

Oxygen = 1.
Hydrogen = .125
Chlorine = $4\frac{1}{2}$
Silver = $18\frac{1}{2}$

The correspondence between the first and third experiments shows that in the first no organic substance had been in union with the silver, which was precipitated by chlorine alone, and that there is no evidence from these results of free muriatic acid being present. The conclusion is the reverse, since if any free muriatic acid had been neutralized by the potash, the third experiment ought to have given an inferior quantity of chloride of silver, because the sal-ammoniac ought to have been sublimed. The potash which was added, I conceive, in the third experiment, united with an organic acid; the salt formed was decomposed by the incineration, and the potash united to the chlorine previously in union with ammonia; for that sal-ammoniac or a volatile muriate was present is proved by the second result.

It is well known that Dr. Prout, by similar experiments, drew the conclusion, that free hydrochloric acid was present in the stomach. That distinguished chemist, however, omitted the first experiment which I have described. Indeed, he could not have made it successfully under the circumstances in which he operated, because the gastric juice in his experiments was not exposed to a heat that could coagulate and separate the albuminous matters, which would then have combined with the oxide of silver and have complicated the result. In the gastric juice employed in the

preceding researches, however, nothing was present but soluble starch or dextrin and sugar, which formed obviously no compounds with silver, insoluble in boiling nitric acid. It is possible, therefore, in the experiments of Dr. Prout, who determined the total amount of chlorine in the gastric fluid by supersaturating with potash, igniting and precipitating with nitrate of silver, that he had formed cyanide of potassium, which would precipitate cyanide of silver along with the chloride, unless the precaution were taken, not merely to add nitric acid, but to boil the solution after the addition of the acid, since cyanide of silver is insoluble, or at least not wholly decomposed by cold nitric acid. I offer this explanation, originally proposed by Leuret and Lassaigne, because, from my knowledge of Dr. Prout, I am quite certain his experiments were most accurately made. Indeed, the testimony of all succeeding experimenters who have obtained the same results is sufficient to establish his accuracy without the addition of any confirmation on my part. It is even possible that, in cases where the food is different, the acid may be of the nature described by Dr. Prout.

The experiments which have been detailed seem to demonstrate that no free hydrochloric acid existed in the stomach of the animal, under the circumstances described, since no acid could be distilled over at a temperature greatly above that at which this acid boils when sufficiently concentrated, while the fluid in the retort became more intensely acid in proportion as the distillation proceeded. A portion of the liquid from the retort was evaporated to dryness and heated to a temperature exceeding 500° without giving out acid fumes; the residue was digested in water, and still retained an acid reaction. The solution of this residue was easily saturated by carbonate of lime, and was not precipitated by chloride of calcium, indicating the absence of biphosphate of lime (contrary to the views of Blondlot) and likewise of free sulphuric acid.

In another experiment the gastric juice was evaporated to dryness in the water-bath, and treated with alcohol and oxide of zinc with the necessary precautions; prismatic crystals were obtained corresponding with lactate of zinc, but in too minute quantity to admit of analysis, the only demonstrative argument. The preceding experiments appear to show, however, that the free acid of the stomach, in the digestion of vegetable matter, at least, of all the known acids, alone corresponds with the lactic. To determine the nature of the volatile acid, which, however, appears to be present always in minute quantity, a portion of gastric fluid was distilled, and the product was obtained in three distinct receivers. Their characters, as determined by infusion of litmus, were as follow:—

	Infusion of litmus.
1st product of distillation } amounted to	1 oz., bright-red colour.
2nd „	$1\frac{1}{2}$ oz., paler than preceding.
3rd „	1 oz., slight red colour.

From these observations, it would therefore appear that the greatest amount of volatile acid was carried over at first, and that as the distillation proceeded its amount in the retort gradually diminished, indicating that the acidity was not due to the decomposition of lactic acid or its eduction by the vapour, but rather to the presence of acetic acid. The quantity present was, however, trifling, since the distilled product of a large amount of gastric fluid could never be detected in a state of effervescence on the addition of carbonate of soda.

Dextrin and Soluble Starch found in the Stomach in the Digestion of Starch.—I have already stated, in a previous part of this paper, that I was unable to detect any traces of starch in the serum of the blood. It was therefore necessary to return to the stomach, and to observe the chemical changes to which the starch was subjected in that viscus. When an animal is fed on porridge, if water be added to the contents of the stomach, the mixture well-stirred and then allowed to stand at rest, the supernatant liquor produces a blue colour with tincture of iodine; but if the liquid be filtered, the colour obtained by mixing the solution of iodine with the filtered liquor is red, indicative of the presence of dextrin, or one of the varieties of soluble starch. I have sometimes found, however, that starch has existed in solution in the gastric fluid even when neither a blue nor a red colour was indicated by iodine. This substance I have isolated by boiling the gastric fluid in order to coagulate the albumen, evaporating to dryness in the water-bath, and then removing the sugar and oil by means of alcohol. The substance thus obtained gave no decided indications with tincture of iodine previous to isolation when dissolved in water, and therefore corresponds with that variety of soluble starch which has frequently been described by chemical writers. It appeared a point of some importance to ascertain whether the transition of starch into dextrin takes place at once in the stomach, or whether the change commences before the food is swallowed. I accordingly prepared a quantity of porridge by

boiling it for upwards of half an hour with distilled water, and on filtering it I obtained distinct evidence of the presence of dextrin in the filtered liquid. It may perhaps, therefore, be legitimate to infer that one of the important purposes to be acquired by cooking starch is to facilitate its conversion into soluble dextrin, and that other form of soluble starch already described; and hence the importance of the boiling being continued for a considerable space of time, when some of the harder species of amylaceous food, as rice, sago, tapioca, are used as articles of diet, and especially when they are administered to the delicate stomachs of the sick.

The soluble starch was separated in the manner already detailed, and was found to possess the following constitution when subjected to organic analysis:—

7.29 grains gave	9.86 CO_2
7.11	9.54 CO_2
13.93	7.97 HO
7.29	4.05 HO
7.11	4.03 HO

The result of these analyses per cent. is as follows:—

	1st.	2nd.	3rd.
Carbon.....		36.88	36.59
Hydrogen	6.31	6.17	6.29
Oxygen		56.95	57.12

This composition does not agree with that of starch, except with wheat starch, in the carbon before that substance has been dried, but according to the analyses of Prout, the hydrogen in wheat starch is greater in amount than in the present case. The carbon in the substance under consideration agrees with that of sugar of starch, but the hydrogen is much inferior; and the three experiments detailed agree so closely in reference to the hydrogen, that there can be little doubt of the accuracy of the results, more especially as the last analysis was made with oxide of copper and chlorate of potash, and the matter subjected to analysis appeared to be completely burned. The substance appears to have possessed some intermediate characters between those of sugar and starch.

Sugar in the Blood.—The preceding results show that a matter closely corresponding with sugar is found in the stomach. Experiment was scarcely necessary to prove that sugar exists in the stomach, since as all flour and meal contain sugar, it is obvious that when these articles of diet are swallowed, sugar must be present in that viscus. But as it exists in the stomach, it is natural to expect that it should pass into the bloodvessels, and be capable of being detected in the mass of the sanguineous circulation. That fermentation can be excited by yeast in many of the fluids of the body, was long ago shown by Tiedemann and Gmelin.

Dr. A. Buchanan, during the course of last winter, obtained traces of carbonic acid from serum by fermentation, and I repeated the experiments with success; but I found it necessary to be careful, in such a delicate experiment, that no source of fallacy should be present to complicate the result, and that in order to arrive at a demonstration of the presence of sugar in the blood, it would be requisite to resort to weighing. For this purpose I employed what may be termed a saccharometer. It consists of two light phials or flasks connected by a bent tube, one or both of which possess a safety-tube. The serum or fluid to be examined is weighed out in one of the flasks, which is placed in a vacuum, or in hydrogen gas, to remove any carbonic acid in solution, while into the other is introduced a solution of barytes, the bent tube dipping considerably under the surface of the barytes solution. Yeast being mixed with the serum, the connexion between the flasks is established, and the whole apparatus placed in a warm atmosphere, (70° .) Fermentation speedily begins if sugar is present, and the carbonic acid, as it passes over, precipitates the barytes in the form of carbonate. In twelve hours, the action will be at an end; the flask containing the serum is then to be heated to carry over any CO_2 which may remain in its atmosphere, by means of vapour. I prefer this method to using a safety-tube in the second phial, because it is difficult to wash off the carbonate of barytes which is apt to adhere to the ends of the tubes.

The carbonate of barytes is then to be thrown on a small filter covered with a plate of glass, washed, ignited, and weighed. Every forty-nine grains of it are equivalent to 22.5 of sugar existing in the blood.*

By means of this process, I found that the serum of a pig, which had been starved for at least twelve hours, then fed on porridge, and killed three hours afterwards, afforded by the fer-

* The results contained in this paper were communicated to Professor Liebig in September, 1844. He then suggested the mode of detecting sugar by means of bile and sulphuric acid. I found this test to be ambiguous, as sulphuric acid alone produced a red colour in diabetic urine and in other animal fluids.

mentation of 1000 grains .80 carbonate of barytes = 5.6 grains per pound, or to 2.57 grains of sugar.

In another experiment, the serum of the pig was employed from whose stomach the soluble starch was extracted.

	520.27	grains of serum gave	
	.68	...	carbonate of barytes,
equivalent to	9.01	...	per pound,
...	4.19	...	sugar,

and estimating the quantity of blood in the human body at twenty-four pounds, by the first experiment we should have 61.68 grains in the sanguineous circulation, and by the second 100.56 grains. A pound of urine in diabetes I have found to contain 14.361 grains of sugar. Whether the sugar is burned in the lungs and capillaries, or is previously converted into fat by the agency of the casein of the blood, it remains of course to determine; but the experiments of Pelouze have shown us how this may be effected; and the present determinations seem to form a connecting link between starch and fat, which render the views of Liebig, in reference to the supply of animal fat being derived from starch, still more plausible than was at one time suspected.

The experiments detailed in this paper appear to lead to the following conclusions:—

1. That vegetable albuminous matter and fat when digested can be detected in the blood.

2. That if any free hydrochloric acid exist in the stomach of animals during the digestion of starch, the quantity must be so minute as to be scarcely appreciable.

3. That an acid exists in the stomach of animals fed on starch, which corresponds more nearly with lactic acid than with any other known acid.

4. That dextrin and soluble starch exist in the stomachs of animals which have been fed on farinaceous diet during and for some time after digestion.

5. That sugar exists in the blood of animals which have been fed on starch. And,

Lastly. It is hoped they have in some measure contributed to throw a ray of light upon the important function which starch fulfils in the animal economy.—*Philosophical Magazine*, May, 1845.

ON PUERPERAL FEVER.

By J. SYMONDS, Esq. Surgeon, Oxford.

As my object, in drawing up this paper, has been rather to record my own experience on the subject discussed therein, than to detail the opinions of others, I have not noticed the valuable treatises on this disease which from time to time have been published, either in systematic treatises, or in monographs written for the express purpose of elucidating the pathology and treatment of the malady in question.

There have been three forms of febrile disorder occurring in the puerperal state, which have particularly fallen under my notice; these, for the sake of avoiding needless periphrasis, and for reasons which I shall presently assign, I shall designate respectively as the phlegmonous, the erysipelatous, and the remittent puerperal fever. The first, which I have termed the phlegmonous,—and I use the word phlegmonous as expressive of inflammation, which is not only acute, but is also not superficial, and which pervades the substance of the inflamed organ or structure,—consists of acute enteric inflammation of the uterus or of the peritoneum, and which, in the majority of cases that have come under my observation, have been more frequently met with in conjunction than separately. Certainly, the uterus has been almost always implicated, and for the most part primarily. The attack generally comes on some time within the fifth or sixth day from the patient's delivery, more commonly on the third or fourth day, than at any other period. It is usually ushered in with rigors, more or less severe, though the urgency of the seizure is not always in proportion to the violence of the rigor. In some cases the patient has scarcely been sensible of anything like coldness or shivering, even where the malady has proved of a most formidable and threatening aspect, and has been with difficulty got under. On the other hand, when the rigor has been severe and prolonged, the disease has been subdued with less difficulty than might have been anticipated from the manner of its commencement. Still, a sharp rigor supervening in the lying-in condition, is not an indication to be trifled with, and is too often portentous of coming evil. To coldness succeeds a hot skin, the tongue becomes coated with a white fur, the pulse is rapid, sometimes full and bounding, but more commonly hard and compressed, and ranging in point of frequency from 110 to 130 beats in the minute. The lochial discharge

becomes pale and scanty, and the mammary function is either wholly suspended, or very sparingly performed. The patient complains of severe and almost agonizing pain, attended with exquisite pain on pressure of the hypogastric region, shooting to the back, and upwards to the umbilical and iliac regions, and this more especially when the inflammation extends beyond the peritoneal covering of the uterus.

The treatment of this malady must, it is obvious, be strictly antiphlogistic, and no time should be lost in carrying into effect the measures necessary for the removal of the disease. On the promptitude and decision with which such measures are put in force, will be the probability of a successful result. Bleeding, both general and local, must be practised, and that with a freedom and frequency of repetition proportioned to the urgency of the symptoms and the strength of the patient. After the application of leeches, I have found bran, enclosed in a flannel bag, and carefully wrung out in hot water, the most convenient method of fomentation, and the poultice thus prepared should be large enough to cover the lower and indeed larger part of the abdomen. This should be renewed from time to time, and steadily persevered in.

In order to induce free catharsis, as well as nausea and vomiting, I have usually administered, in the form of pills, calomel and extract of colocynth, and in mixture or draught, a solution of emetic tartar, in the dose of a fourth to half a grain of that salt. The pills should be repeated with each dose of the mixture, till they have fairly succeeded in emptying the intestinal tube. The emetic tartar will rarely occasion vomiting after the second dose. It must be remembered, however, that both calomel and emetic tartar are, in these cases, edged tools, and should not be persevered in long after their effect has been fully established.

It will be well to bear in mind, that a not uncommon sequel of this and other forms of puerperal fever is an inflammatory irritation of the mucous coat of the intestines, giving rise to obstinate, and sometimes almost intractable diarrhoea. After the supervention of sickness and purging, I should prefer the milder diaphoretic and relaxant remedies, such as grey powder, with ipecacuanha, in pills, and the citrate and nitrate of potash, dissolved in almond emulsion, to which may, with advantage, be added a drop of tincture of hydrocyanic acid, or a proportionate quantity of laurel-water, to each dose. If after the pulse shall have been reduced, both as to time and frequency, and the febrile symptoms generally mitigated, local pain and tenderness should still continue, benefit will seldom fail to be derived from the application of a blister of tolerable size to the hypogastric and umbilical regions. Blisters in these, as well as in other inflammations, are not, in my opinion, to be regarded merely as counter-irritant remedies, but as instrumental, by the discharge consequent on their application, in unloading the capillary circulation of the inflamed part.

I have more than once tried Dr. Armstrong's plan of giving large doses of opium subsequent to bleeding in these cases. I was at first rather sanguine of its being attended with beneficial results. I had, on reading Dr. Armstrong's paper on the subject, pleased myself with the hope that the opium would check reaction, as well as allay that high nervous excitement which I have often found a most troublesome concomitant of puerperal disorder. I cannot say that I should be disposed to repeat the experiment, though I have made it with impunity, as to the ultimate issue. I must confess that the opium appeared to me rather to mask the symptoms than to assist in subduing the disease, and I am not certain that, instead of superseding the necessity of a repetition of the bleeding, the necessity was not rendered more imperative by the exhibition of the opiates.

The second form of febrile disorder in the lying-in state, which I have termed the erysipelatous, may be considered as puerperal fever, properly so called, as having more of an idiopathic character than the one to which I have already adverted, and which, in strict propriety, should rather be ranked amongst the *phlegmasiæ* than the *febris* of Cullen.

It has been variously disseminated as peritoneal fever, malignant and epidemic puerperal fever. There can be no doubt that it is often both epidemic and contagious, and it is too probable that the accoucheur has not infrequently been the medium of communicating the contagion from one patient to another, more especially if he has not taken the precaution of changing his clothes, or has attended a necroscopical examination of a person who has died a victim to this too often fatal malady.

In addition to the symptoms already recorded of what I have termed phlegmonous puerperal fever, the leading pathognomonic symptom of the epidemic or erysipelatous puerperal fever is a frightful rapidity of the pulse, which is usually softer, less tense, and not so characteristic of acute inflammatory action as in the phlegmonous puerperal fever. The pulse seldom beats less than 130 in a minute, and often rises to 160, and even more. A