whilst in one the upper, and in the other the lower jaw, suffered necrosis. These men were "dippers;" and it is but fair to suppose that the hand would have been the first to suffer, had the effect been direct. It must, however, be confessed, that the fact of the *upper* maxilla being affected, in Case 1, militates against the following passage, which we find at p. 75 of Mr. Stanley's excellent treatise: "Against the opinion that the phosphoric vapour acts merely as a local excitant, the objection has been urged, that it produces no effect on the periosteum of the bones of the nasal passages through which the vapour is directly inhaled." That these cases should not be classed among ordinary necrosis of bones is sufficiently obvious, the entire absence of the least attempt at the regeneration of bone being the most characteristic differential feature. Nor could this regeneration well take place, as, to use Mr. Stanley's words, (p. 75,) "there is here a total want of the essential conditions for the reproduction of bone—namely, inflammation in healthy structures, with health in the general system." The grey, pumice-stone-like, newly-formed osseous substance, found by Dr. Heyfelder on the outer surface of the portions of bone which he extracted in similar cases, likewise points, as Mr. Stanley remarks, to an affection distinct from the usual death of bone. We sincerely hope that the etiology of these affections will fix the attention of the many able investigators in surgical science of which this country can boast.

ON THE USE OF CHLOROFORM.

By CALEB RADFORD, Esq., F.R.C.S., Uckfield.

The case, in your last number, of the death of a man in St. Thomas's Hospital, illustrates the great susceptibility there is with some individuals to be influenced by chloroform—in short, that a dose which in one instance is quite inoperative, in another is fatal.

Shortly after chloroform came into use, I gave ten minims by sprinkling on a handkerchief, and applying over the face of a tall, athletic woman, about thirty; in half a minute she was completely under its influence; I opened her mouth and extracted a molar tooth. The insensibility continuing, she was placed opposite a window, and cold water thrown in her face; in a few minutes she revived, but complained of feeling very faint; a little stimulant soon relieved this.

In another case, thirty minims were sufficient to make a man wholly insensible, while I amputated his thigh; and in a third instance, twenty minims rendered painless the excision of an inch of the median nerve above the wrist; but more than two drachms of the chloroform produced only a very partial effect in a healthy woman who wished to take it for extraction of a tooth. The whole of these persons took the drug from the same supply, and in a few days of each other; the strength of the chloroform, therefore, would be uniform.

Since there seems no means of knowing who will be easily affected, and who will require a full dose, the only safe plan is to begin with a very small one.

Of the persons I have named, the woman who took but ten minims was remarkable for her strength and determination; the man whose limb was removed was fifty-six years of age, and was exhausted a good deal by knee-joint disease. The other man was seventy years of age, and suffered from neuralgia, extending from the finger to the axilla.

November, 1849.

OBSERVATIONS ON THE

REPORT OF THE COLLEGE OF PHYSICIANS

RELATIVE TO THE ORGANIC BODIES DISCOVERED IN THE EVACUATIONS OF CHOLERA PATIENTS.

By J. G. SWAYNE, M.D. Lond.
LECTURER ON MIDWIFERY AT THE BRISTOL MEDICAL SCHOOL.

It is but fair and right that the profession, before adopting the conclusions contained in the recent Report of the College of Physicians upon the bodies in cholera discharges, should be perfectly satisfied as to the accuracy of the premises from which these conclusions are drawn. The Report, it should be remembered, does not represent the collective wisdom of the College of Physicians, but is based merely upon the observations of three or four individuals whose conclusions are placed in opposition to those of the two or three persons who first described these peculiar bodies. The question, also, is not one which can be settled by authority, nor is it one upon which experience has been brought to bear; for the communication of Messrs. Busk and Marshall, and Drs. Griffiths and Jenner, clearly show that the investigation, from its peculiar nature, was as new to them as it was to Mr. Brittan, Dr. Budd, and myself; and it is but fair that these gentlemen should give proofs of the superior accuracy and completeness of their observations over ours, before they are entitled to implicit belief. Now, the accuracy of my colleagues and myself has been so repeatedly questioned in this Report, that I feel that it is but due to ourselves to show that the statements contained in the Report are by no means unassailable in this respect. I think I shall be able to prove, in the following analysis of the Report, that it contains some conclusions, founded upon incomplete observations, as well as others which are strikingly at variance with one another.

The Report commences by detailing the results of several experiments, performed with a view of detecting "annular bodies" in the air. The experiments thus performed were seven in number, and were attended with purely negative results. But when I mention that Mr. Brittan, although he has met with some failures, has obtained positive results in at least an equal number of cases to the above, and that he has submitted them to some of the first microscopists in London, it will at once be evident that his testimony is of more value than any of a purely negative character, whether deduced from examinations of condensed atmosphere, or from "cobwebs," and washed from the broken glass of windows."

Had the experimenters succeeded in discovering anything identical with the "annular bodies" in the atmosphere of uninfected places, the Report would have been justified in arriving at the conclusion that these bodies have nothing to do with cholera. Hitherto, however, nothing of the kind has been detected except in infected localities. The same may be said of the seventeen negative experiments which were made upon drinking-water. But it must be also recollected, that many of these experiments were performed upon water some time after the cholera had left the districts from which it was taken.

The next part of the Report is occupied with observations upon the microscopic bodies found in cholera dejections, and commences as follows:—"We next proceed to show how various are the bodies which have been confounded together under the terms annular bodies (Mr. Brittan), cholera cells (Mr. Swayne), and cholera fungi (Dr. Budd)." This remark is illustrated by references to a drawing contained in the Report, in which the objects seen by us have been most cruelly misrepresented. In answer to this, it may be replied that a very cursory examination of our respective publications will at once show that Mr. Brittan, Dr. Budd, and myself, are in the main agreed as to the most usual form of these bodies. The varieties of description are not more than would be likely to occur when each person follows out a different line of investigation. In proof of this, I may refer to the Report, and point out "how various are the bodies which have been confounded with" the cholera cells — viz., the spiral and annular tissues from "cabbages, potatoes, and onions, the withered style of wheat-grain and portions of cane in sugar (Mr. Marshall), chalk mixture (Dr. Griffiths), and the detached nuclei of epithelium, ("ourselves,") with the smaller bodies; and the "rust, smut, and bunt of grain" (Mr. Marshall); "the *urido segetum* or bunt," (Mr. Busk); the "contents of bran cells" and "altered starch grains" (Mr. Busk); and fatty bodies (Mr. Marshall), with the larger bodies.

In the Report, the cholera bodies are divided into four classes, the first three of which are found in the evacuations. These are:—1. "Things which enclose a free area, and which are often broken." 2. "Globular or oval cells." 3. "Bodies having apparently the form of discs with thick rounded edges and centres of indistinct structure." The Report then proceeds to deal with each of these bodies separately. It treats of, 1, "the rings," which, "when closely examined, are seen to be of different kinds, some perfectly continuous in their entire circle, others formed by a curled fibre, some round, some oval, others lozenge-shaped." From this vague description, and the still more vague plate accompanying it, it would really appear doubtful whether those who drew up this Report had actually seen the bodies in question. Mr. Brittan and myself have examined specimens from more than sixty cases, and can distinctly state that we have never seen any of these smaller bodies which were either "formed by a curled fibre" or "lozenge-shaped."

However this may be, we are told that "these bodies have been traced to their true source" by Mr. Marshall, who finds that they are formed by the curled fibres of vegetables of different kinds, which have been mentioned above. But this explanation by Mr. Marshall would hardly account for the

presence of these bodies in cases where they occur in such numbers as to constitute the chief part of the flocculent deposit of a rice-water evacuation, (as I have seen in Cases 8 and 20, from Mr. Brittan's paper in the *Medical Gazette*, and Case 29, from my paper in THE LANCET;) and it also fails to explain their absence in healthy *human* evacuations.

There is one very important statement by Mr. Marshall, which is contained in a letter addressed by him to the Cholera Committee of the College of Physicians, but which has been totally omitted in the body of the Report. It is as follows:—

"There still remains a series of minute bodies, some oval, and others circular, which have an annular appearance, but which, owing to their extreme precision of form and smoothness of outline, are not referrible to either of the kinds yet described. Some which are oval appear like minute, clear cells, or like the remains of such cells ruptured. Very small circular bodies have the same character. Probably these are really the spores of fungi, or are cellular fungi introduced with the food or drink, and only imperfectly digested."

No drawing is given of these bodies, and no evidence adduced to show that they are not, as is very probable, the peculiar bodies which have been considered by Mr. Brittan and myself as characteristic of cholera evacuation.

The Report next proceeds to describe certain bodies "intermediate between these and the third class of bodies," but which, in reality, appear to be of the same size, and ought to be included in the same category, as those last mentioned. There really seems to be no reason for placing them under a separate head, except to prevent any clashing of opinion between Mr. Marshall and Dr. Griffiths and Mr. Busk, by the two last of whom these bodies are considered to be calcareous structures, derived from chalk mixture. I have myself frequently examined chalk mixture, and have detected these bodies, but never in any abundance. They certainly resemble the smallest cholera bodies more closely than anything else that I have seen. When I state, however, that the smallest cholera bodies (as I have frequently ascertained) do not polarize light, and that I have met with them in great abundance in cases where I can prove that not a grain of chalk has been taken, I think that I have said enough to disprove the identity of the two. This last objection, however, is thus met in the Report: "These minute bodies from the chalk are of course not found in all cases; and we think it not unlikely, that in their absence the separated nuclei of animal and vegetable structures, as well as the vegetable rings above described, may sometimes have been mistaken for fungi." With respect to this last sentence, I may remark that none of these bodies could ever have been "mistaken for fungi" by either Mr. Brittan or myself, for we have never once applied such a term to them.

Mr. Busk has until very lately held a different opinion from that of either Mr. Marshall or Dr. Griffiths respecting the nature of the bodies last described. For instance, in a communication to the *Medical Gazette* of October 26th, he states, "That the smaller discoid annular bodies averaging about $\frac{1}{3000}$th of an inch in diameter, are altered blood discs." This conclusion, however, was based upon the examination of a single specimen of evacuation from Birmingham; and Mr. Busk has since, with praiseworthy candour, acknowledged it to be erroneous, (see *Medical Gazette* of November 2nd, page 761,) and now holds the same opinion respecting their nature as Dr. Griffiths.

The Report next treats of the larger bodies found in cholera evacuations, and begins with the second class—viz., globular or oval cells, chiefly of the middle size, which have a thick wall, with numerous small eminences on its surface, and contain a granular mass, in some instances separated by a clear space from the wall of the cells." As the Report states, these have been "distinctly figured by Mr. Swayne only." Being, therefore, solely answerable for these, I may be pardoned for making a few remarks upon them before replying to the objections of the Report upon this head. I may possibly be in error respecting the identity of these bodies with the large "annular bodies," or cells, having a discoid appearance, which are so constant in cholera evacuations. I have met with the former in only two specimens,* and with the latter constantly. In the two specimens mentioned, both kinds were present in abundance, and afforded as unequivocal evidence as it is possible to obtain, of their being the same bodies in different stages of development and decay. They were acted upon in the same way by chemical agents, and presented a strong similarity in form and in colour. For instance, in one specimen which I have, there are two large oval cells side by side, which are of the same brownish-yellow colour, and show the same thickness of wall. One, however, is perfect, and is covered externally with buds, and contains within it a round granular mass. The other is ruptured at one extremity, appears empty and somewhat flattened, so as to look "cupped" when out of focus, and is covered externally with flocculent projections instead of buds. From several comparisons of this kind I felt justified in arriving at the conclusion, that the latter bodies were an imperfect and *effete* condition of the former.

The Report states that "the globular bodies" (solely described by me) have been clearly identified by Mr. Marshall with the spores of different kinds of uredo, the rust, smut, and bunt of grain." Now it does not appear from Mr. Marshall's communication at the end of the Report, that he has ever seen the globular bodies which I have described, as he neither gives any description, nor makes any allusion to them, but merely notices the spores of different kinds of uredo which he has seen in cholera evacuation. So that there is no proof whatever of his having "clearly identified" the bodies in question with the uredo. It is stated, however, just afterwards, that "Mr. Busk has made the same observation, and identifies them with the uredo segetum or bunt." How far this statement is founded on fact we shall presently see. To continue the words of the Report,—"The fact is illustrated by the following figures, which may be compared with the one copied from Mr. Swayne's paper." Two figures are then given of the uredo, magnified at least 1200 diameters, which are to be compared with a figure of the cell described by me, magnified only about 250 diameters! Such a comparison is obviously unfair, if not disingenuous.*

If by Mr. Busk's "identifying" these bodies with the uredo segetum we are to understand that he shows them to be the same, we need only refer to a letter, by Mr. Busk, in the *Medical Gazette* of October 29, to show how far this statement is borne out by his words. He says—"The species of uredo described as present in a preparation of choleraic evacuation furnished by Mr. Swayne is not, as your Report would imply, the same species as that which I found in some common brown bread. That it is of the same genus, however, there can be no doubt. The species in the cholera discharge is at least three times the size of the other, and is oval instead of round." This is surely a very different thing from "identifying" the two, and is only one example amongst many in which "we" of the Report have been less cautious and reserved than those from whom "our" information was obtained.

It appears, then, that no proof has yet been adduced of these bodies being identical with the uredo from bread; and it next becomes a question whether they belong to the genus uredo at all. Although Mr. Busk and others believe that they belong to that genus, they have never yet been able to fix their species. I have shown specimens of these bodies to several eminent cryptogamic botanists, who distinctly declare that they are not uredos.

I have looked over M. Tulasne's elaborate papers in the *Annales des Sciences Naturelles* for 1847, and have examined the different kinds of uredo from wheat, without being able to find anything corresponding to these bodies. The only species at all resembling them is the uredo caries, one of the largest infesting wheat, and which seems to be the same as that discovered by Mr. Busk in brown bread. There is a slight resemblance between the cholera body and this uredo in colour, as also in the circumstance that both have thick walls with external projections, and that both are not acted upon by liquor potassæ. The differences between the two, however, are many and striking, and are as follow:—

1. The cholera cell (as can be proved by accurate measurement) is more than twelve times as large as the uredo.

2. The spores of the uredo are nearly all of uniform size and development; the cholera cells, on the contrary, are met with of all sizes, and in every stage of development.

3. The uredo is studded all over with sharp points, occurring at regular intervals. The cholera cell is covered somewhat irregularly with blunt projections or buds, resembling bulbes. These are occasionally observed to be pediculated, and sometimes entirely detached from the parent cell, so as to give evident proofs of germination. Nothing of this kind can be observed in the uredo.

4. The uredo is always round. The cholera cell is mostly oval, and contains within it a round, granular mass, which does not quite fill its interior; no structure of this kind is apparent in the uredo.

* Since writing the above I have examined a specimen from Bridgwater which contains these perfect cells in great abundance.—J. G. S.

* To show the real difference in size between the two, I have made a correct drawing of both from the microscope with the steel disc. The uredo is taken from a specimen which Mr. Busk kindly sent me. Both are magnified 420 diameters.

5. Nitric acid has no action on the uredo, beyond rendering it rather paler. It slowly dissolves the cholera cell, first turning it of a bright-yellow colour, then resolving its granular contents into three or four oily-looking globules, and finally reducing its wall to a very thin pellicle, with scarcely a trace of buds.

Mr. Busk, in a communication to myself, laid great stress upon a fenestrated, or rather, perhaps, reticulated, appearance of the outer wall, as evidence of close relationship between the two bodies, and described circular spots, indicating holes through their walls. I have been able to discover no such structure in the cholera bodies, which I have examined for this purpose by a twelfth-of-an-inch object-glass, after rupturing them by pressure, and squeezing them perfectly flat.

The identity, therefore, of these cells and the uredo is but a mere assumption after all, and a much more gratuitous one than any which has been laid to the charge of Mr. Brittan and myself, when we affirmed that the larger and smaller cholera bodies were the same, as also those contained in the atmosphere, and the smaller bodies. Our views in this respect were confirmed by Mr. Quekett, who, in a letter addressed to Mr. Brittan, and published in the *Medical Gazette*, states that he considers the larger and smaller bodies in his preparations to be "successive stages of development of the same body."

The third class of bodies referred to in the Report comprises those which are met with by far the most commonly in choleraic evacuations, and which are described as "discs with thick, elevated, and somewhat irregularly curved margins, the central area flattened and obscurely granular." Dr. Griffiths, as we have seen above, compared the smaller "annular bodies" with chalk. Mr. Marshall, as if to demonstrate the difference between the two, compares these larger bodies with cheese. He states, that by compressing rich cheese between two plates of glass, with or without ether, he can obtain bodies like these discs, which he considers to be fatty on account of their solubility in ether, and absence of organized structure. Mr. Busk, however, regards the smaller bodies of this kind as altered starch-grains, and the larger ones as the altered contents of bran-cells. So that here Mr. Busk and Mr. Marshall are at issue, and the conclusion arrived at in the Report—viz., "We are not yet able to account for the origin of these peculiar discs"—is, perhaps, the best which could be expected under the circumstances.

I have examined some of these bodies from cheese, after treating it in the way recommended by Mr. Marshall. They differ from the discs just mentioned, in being many times smaller, and in not presenting the appearance of a thick wall, which is so characteristic of the latter. That the cholera discs are not fatty bodies like these may be proved by their insolubility in ether. I have repeatedly verified this upon some good specimens which I obtained last week from Bridgwater.

The altered contents of bran-cells can also be readily distinguished from these discs. The bran-cells have a polygenal, or at all events a more or less angular, outline, which readily distinguishes them from the cholera discs. They are likewise entirely destitute of that appearance of a thick wall, which is so characteristic of the cholera bodies. These differences were very well marked in a specimen which I obtained from Bridgwater, which contained numerous cholera discs, intermixed with the contents of bran-cells.

Towards the end of the Report, an objection is raised as to the connexion of these discs with cholera, because "it is indeed remarkable, that in those dejections which, from the absence of colour, have been usually regarded as the most characteristic of the disease, they are frequently absent." This circumstance, which is of by no means frequent occurrence, has been mentioned and explained by both Mr. Brittan and myself, in our respective papers. We there stated that these bodies have been absent in the first dejection, and present in subsequent ones; and that in other cases where they have not been found in the evacuations, they have been detected in the contents of the intestines after death.

It is stated in the conclusion of the Report, that "bodies which have been thought peculiar to cholera exist in the intestinal evacuations of persons affected with other diseases." Some instances are related, and an illustration is given of these bodies. All of them are much too indefinite and irregular in their outline to be taken for cholera bodies.

There is one serious objection to the part of the Report which relates to the microscopic appearances of cholerasic evacuations, and that is, the entire absence of any reference to the number of cases from which the observations were drawn; and this is the less excusable, when it is considered that these observations are brought forward in order to set aside results which Mr. Brittan and myself have obtained from the examination of specimens from more than sixty cases. I may add that, in addition to the cases already published, I have examined several specimens from Bridgwater, all of which fully confirm our previous results.

There are also some defects as to the general tone and tenour of the Report, which are strikingly apparent. One of these is the assumption—upon which it proceeds throughout—of superior knowledge on the part of its authors, as opposed to ignorance on ours—an assumption which is quite out of place in a philosophical inquiry, and which is quite unwarranted by the vague and contradictory statements in which the Report abounds. It is constantly assumed that we have "confounded" one thing with another, and mistaken for cholera bodies, starch, bran, fat, vegetable cells, spiral fibres, and a variety of other things, of common occurrence in ordinary evacuations. A reference to the published analyses of Mr. Brittan and myself, in which these bodies are recognised and described in their proper place, will show that there is no ground for such an inference.

There is another assumption which frequently occurs in the Report—viz., that we consider the bodies described by us to be fungi, and to be the cause of cholera. Mr. Brittan and myself have never called them fungi, nor have we stated them to be the cause of cholera. I stated, in my paper, that they were peculiar to cholera, and put forth a few conjectures (but nothing more) as to their relation to the disease.

"In conclusion, Sir, permit me to express my admiration of the independent tone which your journal has adopted, with reference to this matter, and which proves it to be "Nullius addictus jurare in verba magistri." Permit me also to apologize for having trespassed so largely upon its columns.

a, Cell from cholera evacuation.
b, Uredo from a specimen of brown bread.

Clifton, November, 1849.

ON THE TREATMENT OF CHOLERA BY PREVENTING THE DISCHARGE OF THE SERUM OF THE BLOOD, OR SUPPLYING IT ARTIFICIALLY.

By C. B. NANKIVELL, M.D., Torquay.

Whatever may be the specific and essential cause of cholera, there can be no doubt that the principal phenomena of its attendant collapse are produced by the loss of the serous portion of the blood and the reduction of that retained within the vessels to a thick and viscid consistence. It is manifest that such blood must be incapable of circulation in the pulmonic and general capillary vessels. Death by asphyxia must obviously be the speedy result of the continuance of such a state.

Considering the most important therapeutic indication to be that of restraining the serous exudations, while means are taken to promote and support the circulation, it was determined by Mr. Toogood and myself, in a severe case of this kind, to try the liberal use of astringents, administered by the mouth and by the rectum. The patient was cold and blue, with imperceptible, or scarcely perceptible pulse, profuse vomitings and purgings, cramps, shrunken features, sunken eyes, and corrugated fingers. Half a drachm of gallic acid, and fifteen to twenty drops of laudanum, with three or four ounces of starch, were injected into the rectum after each dejection; and five grains of the gallic acid, with the sixth of a grain of opium, were given in the form of pills after every vomition; a large hot linseed-meal poultice, sprinkled with turpentine, was applied from the pubis to the clavicles, and retained and kept warm by a flannel binder; slight stimuli and cold water were occasionally given. Under this treatment the copious alvine evacuations were shortly restrained, and in the course of two or three hours entirely arrested; reaction came on, and the patient recovered after slight consecutive fever.