method of cultivating on solid media, though, in my paper on abscesses, I still maintain that fluids have advantages in certain cases. Thus, when I came to work at tubercle, one of the first things I did was to work with fluids, thinking that perhaps Dr. Koch had limited himself to cultivating on solids. Indeed, before Koch published on tubercle at all, I had already planned out a series of experiments on tubercle, in repetition of Schüller's, which would have been begun in earnest in the spring or summer of 1882. But, during the winter of 1881, I had made several preliminary experiments by cultivation in fluids; I had introduced portions of tubercles from tubercular guinea-pigs into various infusions, and kept them at the body-temperature, without in any case getting any development, thus at once disproving Schüller's results. Indeed, anyone who has worked practically at these matters will see at once where Schüller's errors lie. Since the publication of Koch's paper, I have often inoculated tubercles directly into cultivating fluids, but always with the same negative results; although in some instances, where portions of broken-down cheesy glands were introduced, there was slight development at the bottom of the flask; and I have also got slight development where broken-up crusts of growth on blood-serum have been introduced into infusions. In no case, however, was there any turbidity of the fluid, nor any growth of which use could be made. It is a great pity that Dr. Creighton's criticism comes at a time when one is no longer working at this subject, and when, for my own part, I had come to regard the causal relation between the bacilli and tuberculosis as settled; had it come earlier, I could have shown flasks in support of my statements.

But with the fact that these bacilli do not grow in fluids, Dr. Creighton's whole argument falls to the ground. It was not possible to separate the bacilli from the tubercular material in any other way than was done, and the question is, was that method sufficiently satisfactory? I maintain that it was. You have seen, in these little vessels, how the bacilli spread away from the tubercular material, which cannot follow them. When, then, the marginal growths are picked up, it is clear at once that we have got the bacilli free from the tubercular material; and if this process be continued through a number of successive generations, there can be no possible doubt that the tubercular material has been got rid of; and much more certainly got rid of than could be the case by the method of fractional cultivation as described by Dr. Creighton. Thus, let a flask of meat-infusion be inoculated with a crushed tubercle, and let us suppose for one moment that the tubercle-bacilli could develop freely in the fluid. You remove a drop of this fluid for inoculation, but in the fluid are floating bacilli, and also the *débris* of the tubercular material. Who is to say that you do not take a considerable quantity of tubercular *débris*? And who is to say that, in every successive inoculation, you do not take *débris*? In the fluid, the bacteria and the tubercular matter would be floating about together. On the solid, the tubercular matter is fixed, while the bacilli spread away from it, and may be removed without touching the tubercular matter at all. And further, the stained specimens of the growth, to which I have previously alluded, and to which I would earnestly beg your attention, show absolutely that all the tubercular material has been got rid of, and that the growth consists only of tubercle-bacilli; for if any remains of tubercular material were present, I know from practical experience that they would, with this method of staining, have come out blue.⁵ And the experiments on animals with these bacilli show definitely that they are the real virus. Introduce a piece of tubercle and a mass of bacilli of equal size into two animals, and the latter will be far advanced in tuberculosis before the earliest symptoms are evident in the former.

I think, Mr. President, I have said enough to show that the statements which I took as texts are each and all erroneous; that Dr. Koch, in cultivating the tubercle-bacillus, has not departed from the original principle of cultivation on solid media; that the method employed does actually separate the tubercle-bacilli from the tubercular material, and is the best mode which can be employed; that fluid media are unsuitable, because the micro-organisms do not grow, or grow only with difficulty, in it; and that it is not correct to say that, if portions of tubercle be introduced into cultivating fluids, bacteria grow rapidly, and are no longer motionless rod-shaped bacilli. All the assertions I have made can be supported by facts, and to-night I have brought facts before the meeting in support of most of them. I should like to see some facts brought forward by those who take the opposite view; surely they might show us something in support of their statements.

I hope it will not be supposed by the way in which I have spoken,

⁵ It was remarked after the meeting that, if the virus were gaseous or fluid, it would not stain blue. It is, however, generally admitted, and has been proved in a large number of instances, that contagia are particulate.

that I object to fair criticism; far from it. But a purely literary criticism of experiments ought not, I think, to be dogmatic, for dogmatic statements as to what would have occurred if certain things had been done are, in my opinion, only warranted when the critic knows from personal experience that he is correct, or when he quotes from researches about the accuracy of which there can be no possible doubt.

EXCISION OF THE CÆCUM FOR EPITHELIOMA: DEATH ON THE THIRTEENTH DAY.

By WALTER WHITEHEAD, F.R.C.S.E., F.R.S. EDIN.,
Surgeon to the Manchester Royal Infirmary, etc.

I AM not aware that the cæcum has hitherto been removed during life. Mr. John Marshall (*Lancet*, 1882, May 6th and 13th) collected seven cases of colectomy, including one of his own, and he also mentions other attempts which were unsuccessful. With one exception, the descending colon was the site of the operation. Mr. Treves (*BRITISH MEDICAL JOURNAL*, December 16th, p. 1208) has also recorded an instance of resection of two inches of the descending colon for epitheliomatous stricture through an incision in the middle line; the patient died in twelve hours.

In placing on record what I believe to be the first instance of excision of the cæcum, I have felt it my duty to enter more fully into details than a case of more ordinary occurrence would warrant.

The patient was J. W., aged 38, tramcar-driver, married. His father was killed at the age of 62; his mother died when 64 years of age; his nine brothers and sisters are alive and healthy. He had always been healthy himself previously to his present illness; for ten years he was a free drinker, generally taking spirits.

His present disease manifested itself eighteen weeks before admission, with diarrhoea accompanied by pain in the right lumbar region. The pain was paroxysmal, and extended towards the umbilicus, and appeared to be aggravated by a distended bladder or loaded rectum. These symptoms persisted, with only a few consecutive hours of intermission, supposed to be due to remedies taken. At the end of ten weeks, a small tumour, of the size of a walnut, was discovered in the right lumbar region. It felt hard, was freely movable, and free from pain on pressure. The patient at this time was troubled with a constant desire to pass urine, and had to urinate seven or eight times during the night. The urine he described as clear and free from smell. The action of the bowels was irregular; sometimes they would act three or four times in the day, and on other days not at all. The motions were generally pale in colour, and clay-like in consistency. He had no pain on defæcation, and the evacuations never contained either blood, pus, or mucus. The tumour rapidly increased in size, and the paroxysms of pain in frequency and intensity. In eighteen weeks, he lost forty pounds in weight, although his appetite and digestive powers remained fairly good.

He was admitted into the Manchester Royal Infirmary on October 24th, 1884. His complexion was sallow, and he wore an anxious pinched expression. In the right lumbar region there was a mass, about four and a half inches long by three inches broad, and apparently about the same in thickness. The long axis was in the transverse direction. The tumour was free, movable over the whole of the right half of the abdomen, and moved in harmony with respiration. It felt hard and lobulated, was dull on percussion, and slightly tender on pressure. No other enlargement could be felt in the vicinity of the growth. The liver was normal in size. The patient complained of periodic attacks of colicky pains, sometimes of a mild, and at other times of an intense, character. During these attacks, the tumour became much more prominent, and the left half was then found to be tympanitic. There was frequency of micturition; without, however, anything abnormal being detected in the urine. The motions were natural.

The clinical features of the case undoubtedly pointed to a tumour of the colon in the vicinity of the cæcum; and most probably a rapidly growing epithelial new formation. The tumour being movable, it appeared reasonable and justifiable to attempt the excision of the whole disease. It was decided beforehand to make the incision along the outer border of the rectus muscle in preference to that for lumbar colotomy, as it was believed that it would allow greater facilities for dealing with any enlarged mesenteric glands which might be present, and to be better adapted than a median incision for the establishment of an artificial anus. The prudence of this decision was confirmed during the operation, and in the subsequent progress of the case.

Operation.—Chloroform was administered. The surface of the abdomen was thoroughly cleansed and dried; a large mackintosh, with an aperture five by eight inches cut out of the centre, and the under-surface of the circumference of the aperture made adhesive with plaster, was fixed to the skin, so that the opening corresponded to parts that would be involved in the operation. The abdomen was opened in the ordinary manner, in the situation previously decided upon, and the tumour exposed. It was examined, and found to be situated in the ascending colon, rather than in the cæcum, although practically it involved the latter. It was deemed advisable to detach the tumour above the valve. A double catgut-ligature was first passed through the mesentery, and round the upper part of the ascending colon, well above the tumour. The ligatures were firmly tied, and the bowel divided between them. The cut surfaces of the bowel were freely washed with carbolic lotion, until they were free from any trace of faeces. A second double ligature was then passed round the ileum in the same manner, and the gut divided with similar precautions. A small aperture was then made in the upper layer of the mesentery, through which the fingers were passed, and the two layers of the mesentery separated. The superior layer was cautiously divided close to the bowel by the use of scissors. An indurated mesenteric gland, of the size of a split walnut, was then discovered some distance from the growth, whilst others somewhat smaller were found inseparable from the gut. Up to this stage of the operation, no difficulties whatever had been met with. The removal of the gland being deemed essential to the ultimate success of the operation, a careful dissection was commenced. The gland had almost been cleared from its surroundings, when a sudden gush of dark blood took place, the origin of which it was impossible to determine at the moment. That it was from an unusually large vein there could be no doubt, and for a time the vena cava was suspected. Whilst pressure was maintained over the bleeding aperture, the tissues above and below were separated, and finally the superior mesenteric vein was fully exposed, and found distended to the size of a first finger. A catgut-ligature was placed above and below the gland round the vein, and the intermediate part of the vein and the gland removed together. The other layer of the mesentery was then cut across without any bleeding, and the mass removed. The abdominal cavity was cleared of all the blood, which had amounted to very little throughout the operation, notwithstanding the accident to the vein. The ligature was then removed from the small bowel, and the gut stitched to the skin at the lower end of the incision, and, after removing the other ligature, the colon was secured to the skin at the upper portion of the wound. A very large number of silver sutures were employed for this purpose, and great pains were taken to leave no opening for the penetration of faecal matter into the deeper tissues. The remaining portion of the wound was next brought together by strong silver wire passed through skin, muscle, and peritoneum. The surface was dressed with iodoform.

During the operation, which lasted upwards of two hours, I had the valuable assistance of Mr. Southam. The patient appeared to suffer very little from either the chloroform or the operation. Great care was taken—and upon this great stress ought always to be laid in such operations—that the patient was kept thoroughly warm with hot water bags placed in every convenient position under the blankets surrounding his body.

The following details are extracted from the note-book of my house-surgeon, Mr. Gough.

First Twenty-four Hours after the Operation.—He recovered very quickly from the effects of the chloroform, and expressed himself feeling very comfortable. Faeces passed from the small bowel. Urine passed without assistance. Enemata of peptonised beef-jelly, egg, and brandy were administered every three hours. He took from ten to fifteen drops of Battley's solution every four hours. The wound was dressed with iodoform-gauze and wood-wool, which was changed about every six hours. He slept well. Highest temperature, 98.4°; pulse, 100; respiration, 22.

Second Day.—He slept well during the night; had no pain. He had taken half a pint of milk. There was no tenderness over the abdomen. Highest temperature, 99°; pulse, 112; respiration, 32.

Third Day.—Non-bilious faeces passed from the small bowel. He had a capital night. Tongue clean and moist. He had no pain. Highest temperature, 99.4°; pulse, 112; respiration, 20.

Fourth Day.—He had a good night. The wound was looking healthy. The abdomen was slightly distended; no pain. Light coloured faeces passed freely from the small bowel. He was taking milk and having enemata regularly. Highest temperature, 98.2°; pulse, 110; respiration, 20. During the evening, he complained of slight pain in the right iliac fossa, which was immediately relieved after passing flatus from the small bowel.

Fifth Day.—He passed a fair night. The abdomen was more distended, but without pain or tenderness on pressure. He passed some dark offensive faeces from the orifice of the transverse colon. Highest temperature, 101°.

Sixth Day.—Early in the morning, he complained of intense pain across the abdomen, and more particularly in the right hypochondriac region. Large quantities of faeces escaped from both openings. The opium was increased, and repeated frequently. At 9 A.M., he was much better—pulse, 135; respiration, 22; temperature, 99.4°; no pain. He passed flatus freely by the rectum. The urine was alkaline, and deposited a large quantity of phosphates. The wound was healthy. At 11 P.M., he was comfortable. Temperature, 100.6°; pulse, 118; respiration, 22.

Seventh Day.—He had a fair night. He had talked a little incoherently at times, and made attempts to get out of bed. He did not complain of pain. The faeces from the small bowel appeared to contain bile for the first time. The discharge from the large bowel was of a light green colour, and very offensive. At 5 P.M., he had been sick for the first time. There was no pain; the tongue was clean. He was restless; was again sick, and was purged by the rectum; the faeces were very offensive, and light in colour. He was perspiring freely. Highest temperature, 99.6°; pulse, 120; respiration, 22.

Eighth Day.—He had a good night; no pain. The patient was noticed to be much thinner, and to have a pinched look and anxious expression. There were faecal evacuations from the small and the large bowel, and also by the rectum. At 5 P.M., the stomach was distended, and the patient complained of colic. He was ordered five grains of the oxychloride of bismuth, and ten grains of compound rhubarb powder. This was followed in two hours by several copious stools by the rectum, and simultaneously by profuse discharge from the small bowel. The tongue was clear. Highest temperature, 99°; pulse, 110; respiration, 18.

Ninth Day.—He was very much better; slept well; and had no pain. He expressed himself as better than he had ever felt since the operation. The sutures were removed. Highest temperature, 98.6°; pulse, 110; respiration, 22.

Tenth Day.—He passed a capital night. He had no pain; his appetite was good. The skin-wound was gaping. A slough came from the wound, evidently of cellular tissue. The deep wound appeared quite healed. Highest temperature, 98.6°; pulse, 110; respiration, 22.

Eleventh Day.—He had a restless night, and complained of pain across the abdomen. The stomach and abdomen were distended. Highest temperature 99°; respiration, 20; pulse, 120.

Twelfth Day.—He slept only one hour during the night, notwithstanding his having taken twenty drops of Battley's solution every four hours. The surface of the wound was granulating, but the granulations were of a feeble character. The abdomen was more distended. The stomach was washed with iced water by a siphon, and a quantity of bilious mucus was removed. Immediately this had been done, the temperature was taken twice in the axilla, and found to have fallen from normal to 97°. Washing out the stomach appeared to give the patient great comfort, and he expressed himself as feeling very hungry afterwards. At 5 P.M., he complained of severe pain across the abdomen. He had profuse diarrhoea, to check which a chalk and bismuth mixture was ordered. At 9 P.M., he was much worse. He had gripping pains in the abdomen, and was restless and rambling. Highest temperature, 99.2°; respiration, 20.

Thirteenth Day.—During the night the pain increased, and large doses of morphia failed to produce any marked effect. The tongue became dry and brown, and at 7.45 A.M. the patient died exhausted.

The *post mortem* examination was made by Dr. Robert Maguire, and the following is an abstract of his report.

External Appearances.—The body was that of a man below middle age, emaciated; rigor mortis was absent in the jaw, present in the arms and legs. There was dorsal *post mortem* staining; on the right side of the abdomen there was an incision, about four inches in length, running from near the last rib to the crest of the ilium. At the upper end of the incision, the colon opened externally; at the lower end, the small intestine. The skin-edges of the incision were not united, and between them was seen a mass of granulation-tissue covered by iodoform. There was no opening into the peritoneal cavity.—*Abdomen.*—On opening the abdomen, extensive peritonitis was found. There was no fluid in the peritoneal cavity, but the coils of the small intestine were covered and matted together by recent lymph. The peritonitis was most intense, and of oldest date, in the neighbourhood of the incision mentioned, as shown by the firmness of the adhesions at this part. Proceeding hence, the peritonitis decreased in intensity,

and extended upwards and to the left, reaching nearly as high as the stomach. The cæcum, and part of the ascending colon, were wanting. The small intestines were greatly distended with air and yellowish fluid of a fecal smell. The lower four feet or so of the small intestine were greatly congested. The colon, in the whole of its extent, was collapsed and empty. Behind the spot where the small intestine opened externally, there was a considerable collection of greenish putrid pus, lying mainly in the right iliac fossa, and altogether outside the peritoneum. The pus was tightly bound down by the coils of the intestine, matted together by peritonic adhesions. It extended upwards along the large abdominal vessels, and between the layers of the mesentery, reaching nearly as high as the origin of the superior mesenteric artery. The stomach and pancreas were normal; the spleen was enlarged, and, on section, soft and pulpy; the liver was of normal size, but somewhat pale and fatty. The kidneys were of normal size; on section, the cortex was increased in extent, somewhat pale and mottled; the capsules peeled off easily, and left on removal a smooth surface.—*Heart, etc.*—The heart was of normal size, but the ventricles were relaxed. The cavities of the right side contained a quantity of non-adherent *ante mortem* and *post mortem* clot; the tricuspid orifice admitted three fingers. The cavities of the left side contained a little *post mortem* clot, the mitral orifice also admitting three fingers. The walls of the ventricles were of normal thickness, colour, and consistence. The blood-vessels were not stained by the blood-pigment. The lungs were emphysematous and slightly congested. No secondary deposits were discovered.

Through the kindness of Mr. A. Young, I am enabled to supply a sketch of the parts removed, and the following report of the general and microscopic character of the growth.

The portion of intestine removed included the last two inches of the ileum, the cæcum with the vermiform appendage, and the greater part of the ascending colon. When moderately distended, it presented the appearance shown in the accompanying sketch. Its total length from

the lower part of the cæcum to the cut end of the colon measured fourteen inches, whilst from the ileo-cæcal aperture to the cut end of the colon it measured nine and a half inches. About two inches beyond the junction of the ileum with the cæcum, an appearance of intussusception was observed, and in this region an enlarged and indurated lymphatic gland (c) was firmly united to the intestinal wall. Corresponding in position to this attachment, the internal surface of the

bowel presented a fairly defined circular area, about three inches in circumference, occupied by an irregular fungoid and ulcerated mass, which projected into the lumen of the gut. This involved only the inner part of the intestinal wall; the remaining part, as well as the rest of the intestine, presented a healthy looking unbroken mucous lining, nor was there any other evidence of invasion of the wall with new growth beyond the area mentioned.

Sections of the new growth showed it to consist of epithelial cells in large numbers, enclosed in the alveoli of a somewhat scanty connective tissue stroma. It thus presented the characteristic appearance of a carcinomatous growth of the medullary type. For the most part, the cells were of irregular shape and of comparatively small size, presenting only occasionally a columnar appearance. Of the lymphatic glands, those removed with the growth, as well as that removed separately, showed secondary infiltration by epithelial cells resembling those of the primary tumour.

REMARKS.—In reviewing this case, there are several points of interest which appear worthy of brief comment.

The diagnosis was made without difficulty, which is unusual in cases of malignant disease originating in the colon, especially when, as most frequently occurs, the disease attacks the descending portion of the bowel. The protracted diarrhoea, the local character of the pain in the right lumbar region, the rapid growth of the tumour, the freedom with which it moved and always returned to the situation of the cæcum, and the quick emaciation of the patient, pointed with marked precision to the locality and nature of the growth.

Confident of the diagnosis, the steps of the operation could be deliberately designed, a rare advantage in operations involving the intestines. The first point to determine was the most suitable situation to open the abdomen. The reasons which influenced the choice in this instance may with equal force, it would appear, be applied to all cases of malignant disease connected with any portion of the colon not extending beyond the sigmoid flexure.

The median incision affords more room for general exploratory purposes than any other, but it is ill adapted for the establishment of an artificial anus, and necessitates a second incision in the loin for that purpose. The lumbar opening is, undoubtedly, the one best adapted for an artificial anus; but, unfortunately, it gives no room for the removal of infiltrated mesenteric glands, should they exist. Mr. Bryant's preference for the lumbar incision, and his assertion that five or six inches of the gut can, with ease, be reached and examined, does not affect this all-important question in suspected malignant disease. An intermediate incision, made three inches from the linea alba, combines the double advantage—not so completely perhaps as could be desired, but, nevertheless, sufficiently to be accepted as a practical compromise between the two. In this case, at least, it confirmed the judgment which was exercised.

The second point to decide was whether or not, after removal of the growth, it was desirable to unite the divided ends of the bowel together. The desire to attempt the latter course was naturally great; but, the main object being kept strictly in view, it was not considered warrantable to subject the patient to such additional risk, especially in an operation which only holds a tentative position in surgery. Whilst admitting the wonderful success that has attended the operation of enterorraphy, it cannot at present be regarded as an established precedent free from objections and disadvantages. The danger of non-union, and the contingency, possibly remote, of future stricture, are risks which do not admit of being lightly passed over. Further, by bringing the two ends of the bowel outside, there always remained the opportunity, should it ever be considered desirable, of uniting them by a subsequent operation.¹

There is no object in subjecting a patient to an operation attended with the greatest risk, unless it be accompanied with a firm determination to remove the whole of the disease, if it be practicable and within the range of surgical art. If the object be simply to temporise and relieve pressing symptoms, such as those resulting from obstruction, colotomy will accomplish all that is desired, and with considerably less risk to life.

It is evident from the *post mortem* examination that the man died from peritonitis, and that the peritonitis was of traumatic and not of septic origin, as the inflammatory indications were less intense as it receded from the incision. Each detail of the operation was practically successful. All the parts, with the exception of the skin, had completely united, and there had been no leakage of faeces into the abdominal cavity.

¹ Sir Spencer Wells, in his Inaugural Address on the Revival of Ovariectomy (BRITISH MEDICAL JOURNAL, 1884, November 15th, p. 950), states that, in a recent paper by Ruchel, 121 cases of resection of intestine have been collected, the conclusion being that the two ends of the bowel should not be united at the time of resection, but that an artificial anus should be established.

In looking for satisfaction, after the fatal termination of the case, one has to be content with the reflection that the patient did not die from the immediate effects of the operation, and to be consoled with the knowledge which the *post mortem* examination affords—namely, that the whole of the disease had been removed, and the certainty that the patient must very soon have succumbed to a painful death had no attempt been made to cure him.

The operation was conducted upon strict antiseptic principles, and the abdominal cavity remained aseptic to the end. There was complete union of the peritoneum, and between the muscular walls of the abdomen. There had been no secondary hæmorrhage, nor had there been any violent sickness. The temperature was even remarkable for its uniformity at or about the normal range.

Taking all the facts of the case into consideration, it is reasonable to suppose that, had the man possessed a less dilapidated constitution, and been more temperate in his habits, complete recovery might fairly have been anticipated.

THE PLACE WHICH ALCOHOLIC DRINKS SHOULD OCCUPY IN THE TREATMENT OF DISEASE.

Read at the Hunterian Society.

By ALFRED CARPENTER, M.D., M.R.C.P. Lond.

(Concluded from page 117.)

I now come to the principal difficulty in dealing with this subject. It is said that no one differs with me as to the evils which follow from the abuse of liquor. But it is said by Sir Andrew Clark, and others, "that a small quantity of alcoholic liquor, taken, say, twice a-day with meals, is not injurious." It is also said that a physiological quantity is needed to produce certain appreciable effects in the living body. We have had as yet, say they, no reliable evidence of the harm or benefit of any dose under this physiological quantity; and, therefore, say the antagonists to the total abstainer, "that total abstinence principles would advance much more rapidly if these facts were fully recognised." (BRITISH MEDICAL JOURNAL of November 29th, 1884.) Sir Andrew Clark may be right in the fact that a small quantity, taken twice daily with meals, may be harmless. No doubt, it may act immediately on the food in the stomach with which it may enter into combination, and thus it may be deprived of its venomous character. But what is the good of this? It can only help to make the food more difficult of digestion. It coagulates albumen, it hardens fibrine, it precipitates ptyaline from the saliva, sugar is nearly insoluble in it, and emulsine or synaptase is rendered inert by it. But it is said that, when exceedingly diluted, its action may be different. It is not proved that it is not so. It is hard, indeed, to prove a negative, and it is useless to try; but I ask whether any one supposes for one moment that the effect is so neutralised in the stomach? If it be, why should people desire it? Is not the very object of the drinker to be made to feel its effect on the stomach itself, to increase the flow of gastric juice from the glands in the stomach, and so increase for the moment its digestive power? That is the acknowledged object for which it is taken; and Sir Andrew says it makes life happier, and renders the invalid able to do that which he could not do if he had not taken it.

Now, let me go back to the articles which appeared in the *Contemporary Review* in 1879. Dr. Samuel Wilks wrote: "I believe alcohol soothes a worried nervous system, and, by preventing wear and tear, actually supports the frame;" but, discarding the notion of its stimulating properties, he writes: "I denounce its use in delicate children, and women who feel 'low'." Then Sir William Gull says, in the same series of articles, that "the constant use of alcohol, even in moderate measure, may injure the nerve-tissue, and be injurious to health; and one of the commonest things in society is, that people are injured by drink without being drunkards." But, says Sir William, "cases of feeble digestion you may deal with by light and varied food; but still I think wine is useful, a little wine, and with strict limit, as a medicine for temporary use." Sir William, however, is of opinion that "good food will supply all the wants of the system up to the middle period of life." He does not point out how a little wine, within strict limit, is beneficial. All the writers of those articles (twelve in number) denounce excess; one writes that his desire should be to reduce "the limit of physiological saturation." But, he says also, in many cases of dyspepsia, a palpable improvement follows the use of light wine at dinner. With many, the simplest food causes a degree of oppression until a mild stimulant sets digestion to work by the slight flush of blood it excites in the mucous membrane of the stomach. But the same writer says: "It is our duty to give a clear

reason for moderation, and to define its limits." Here Dr. Kidd states a duty which no one has ever yet succeeded in doing.

Another writer, namely, Sir James Risdon Bennett, would "leave this to a man's instincts." Sir James is guided by his own, and he has, no doubt, a good guide; but, with the majority of men and women, the guide would lead to disastrous results; the 60,000 drunkards convicted every year in our police-courts, and the same number who die a drunkard's death every year, are led away by instinct. Sir James is of opinion that "there are no trustworthy statistics available to prove to what extent disease may be safely and satisfactorily treated without the aid of alcohol." He is unwilling, with Mr. R. B. Carter, to allow theory to take the place of well tested experience. Indeed, he would do all that he possibly could to prevent any trustworthy statistics from being available for the purpose; because he expresses himself as entirely opposed to total abstinence views, "except for the intemperate."

Dr. C. B. Radcliffe does not hesitate to state that it is unfortunate that alcohol is not recognised as a tonic, and that it may be taken habitually; he is of opinion that it keeps up animal heat by supplying easily kindled fuel to the respiratory fire, partly in producing nerve-power by furnishing easily assimilable food to nerve-tissue, and partly in lessening the necessity for ordinary food, by diminishing the waste of the system which has to be repaired by food. I must say that physiology does not support these views; there are no facts in science which tell us that it is an easily kindled fuel, so that it may develop heat. Experiment and examination in the police-cell tell quite a different story. Still, Dr. Radcliffe may affirm that his remarks apply to its use by those who are not already "physiologically saturated," and that, when the stage of saturation is reached, the conditions change.

Dr. Radcliffe prefers to be near to the point of saturation, provided he is on the right side; and, with Sir James Risdon Bennett, he probably believes that "men do their work better, and with more comfort to themselves, if they take three or four glasses of sherry as a part of their daily food;" and he will not believe "that such an one is a worse, but a better, life for an assurance-office than a pledged abstainer." I may say that the assurance-offices will in time fully settle this point, even if they have not done it already, in an opposite sense to Sir J. R. Bennett's views.

I have tried, in these extracts, to put forward the views of representative men upon this question. "A physiological saturation" is acknowledged, beyond which it is dangerous to go. This is agreed to by all parties. Before that stage of saturation is reached, alcohol acts differently from what it does when that stage is passed by. In the opinion of some physicians, it is a tonic, an easily kindled fuel; it helps digestion, and enables a man to do his work with greater pleasure to himself. I am unable now to find any sufficient authority from physiology upon which these opinions are based, though they were the views of physiologists fifty years ago. No well known physiologist now supports them. It is proved very fully that alcohol does not keep up animal heat, and it is as little assimilable as any food can be. I take it that Sir J. R. Bennett's and Dr. Radcliffe's opinions are derived from the feelings which alone guided physiologists before instruments of precision were invented. There is a feeling of warmth, and a heat is transferred to the surface of the body, which is very marked; but if with this heat there is no rise of temperature, and if there is also a decrease of carbonic acid excreted by the lung-tissue, we can understand the reason why radiation produces, after a time, a decreased temperature, such as is found in the dead-drunk man in the police-cell, when that man is not suffering from manifest organic disease, which is ordinarily raising his temperature above normal. The feelings are deceived, the blood being collected in the periphery of the body, and the sense is deceived as much as sight is imposed upon, as to their movements, when we look at the heavenly bodies.

There seems just now, however, to be a consensus of opinion among our fashionable physicians that digestion is improved by small doses of alcohol twice daily; the only proof is personal feeling, and also that no injury is traced to it for the time being. But chemistry is against the fact. Physiology will not support it, except so far as that there is an increase of gastric juice by the exhibition of a dose sufficient to act upon the blood-vessels for the moment. But then pathology tells us very clearly that the continuous effect is to produce a permanent change in those vessels and glands. I contend that anything which assists to perpetuate a change is not a wise or truly judicious treatment, but is an unconscious pandering to custom, to fashion, and to the wishes of the patient. It may be very well to help a lame dog over a stile; to use the whip and the spur to the tired horse may be pleasant for the time being, but to my mind the medical man who prescribes it persistently is only starving off a downfall,