instance, in certain forms of chronic heart-disease—how is it that this remedy, when administered to patients in those special conditions which we have been considering, fails to be a remedy—proves to be, for the occasion at any rate, powerless? Several years ago, Dr. Lauder Brunton, while studying the effect which high temperature in fever would have upon the vagus nerve, and the way in which this might modify the action of the remedies then employed, surmised that possibly the failure of digitalis to act upon the pulse, as observed by Thomas, might be due to paralysis of the inhibitory apparatus in the heart, through which the vagus acts upon it; and he performed experiments in order to test the truth of this supposition. The experiments were made on rabbits; but as these animals were found, from the natural quickness of their pulse, not so suitable for the investigation, Dr. Brunton has recently, in conjunction with Dr. Theodore Cash, repeated the experiments on the cat, in which animal, as in man, the inhibitory influence of the medullary vagus centre is more marked. Omitting details, which will be found in the *Practitioner* for October last, I feel I cannot do better than give the conclusion at which these observers arrived in their own words—namely, “that a high temperature lessens the inhibitory power of the vagus centre in the medulla to such an extent that digitalis, and probably all drugs which act like digitalis on this centre, lose, to a great extent, their power to restrain the action of the heart and slow the pulse. The administration of digitalis, or of drugs which act like it, to patients in a febrile condition is, therefore, likely to have much less effect on the pulse than at the normal temperature, and, if the temperature be very high, they may have no effect at all while this persists.” As already intimated, clinical illustration of this teaching is amply supplied in the tabulated cases. To take only that one before referred to for another purpose: A girl, aged 21, on the seventh day of her attack of croupous pneumonia, having a temperature of 104.2° and a pulse of 160, had one drachm of the tincture of digitalis given to her in divided doses during the ensuing twenty-four hours. At the expiration of that time, her temperature was 104.4°, and her pulse remained exactly at 160. I should add, the girl ultimately made a good recovery.

What, then, is the practical outcome of this subject? Shall we henceforth discard digitalis in our treatment of acute febrile disease? I do not see that this should follow. 1. Although I have no adequate results from experience to offer on the point, I am disposed to think that, by combining some antipyretic, as quinine, with the digitalis, better success may be obtained; if we were not able to hasten on the defervescence by this means, a good remission would be no small gain, whereby the action of the vagus would be for a brief space restored, and the digitalis would in the same measure find its opportunity. 2. It should not be forgotten that the crisis, or rapid defervescence with critical discharges, in fevers, and particularly in pneumonia, is in itself not unattended with danger. Symptoms of collapse may supervene, nay, a fatal collapse, as I have witnessed. Digitalis, if given judiciously, and in moderate doses, before the crisis, I think is some protection against this formidable event.

In conclusion, it is most important to bear in mind that the digitalis, given with so little manifest result during the fastigium, may take effect with cumulative energy after the crisis; otherwise, the unwary practitioner, getting no immediate response, may be tempted to push the dose, with the result by and by, when defervescence sets in, of having a grave toxic explosion. Drs. Brunton and Cash append a caution on this point, and more than one of my cases give a hint in the same direction.

SEVERE VOMITING IN PREGNANCY: MISCARRIAGE: DEATH, WITH REMARKS.

By P. HORROCKS, M.D.,

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M. L., aged 39, came to Guy's Hospital amongst my outpatients last July, complaining that she had been vomiting incessantly for three weeks. She considered herself two months pregnant. On examination, I found the uterus about the size of a two months' gestation; there was no malposition, and, with the speculum, no erosion. She appeared very anæmic, and in answer to inquiry, said she was always more or less pale, but that she had become much more so during the last few weeks, owing, she thought, to the vomiting, which prevented her from keeping any food down at all, so that she was practically starving. After treating her in vain for a fortnight, I took her into the hospital, and by rest in bed, with very careful dieting, and the trial

of various drugs, of which tincture of iodine, in minim doses, appeared to be of most service, she got so much better that she went out apparently cured on September 20th, having been in the hospital five weeks, and being now between three and four months pregnant.

Whilst in the hospital, it was found that she had a mitral systolic *bruit*, probably dating from an attack of rheumatic fever twenty years ago. There was no albumen in the urine.

About a week after leaving the hospital, her legs began to swell, and her urine escaped whenever she coughed; but she was not sick, except early in the morning.

Her husband came to the hospital early in November to say that she was much worse. I gave him an order for her admission, but for some days she was too ill to be brought. She was admitted on the second occasion on November 12th, under my colleague, Dr. Galabin, who has kindly allowed me to make use of the case. Sickness was not now a prominent symptom, but she had great dyspnoea and oedema of the lower extremities. The urine was of specific gravity 1010; it contained no albumen. Urine passed involuntarily.

On the 15th, she was very sick all day. On the 17th, she was suddenly delivered of a child, which breathed a few minutes and then expired. It appeared to be about equal to a six months' fetus. She seemed better for a few hours, when she became unconscious, dying the next day without regaining consciousness. At the *post mortem* examination, the uterus was not found to be displaced. There was no erosion of the cervix. It was enlarged, corresponding with the recent delivery, but nothing abnormal could be discovered about it. The liver was extensively diseased by a primary encephaloid carcinoma, the parts not infiltrated by cancer being fatty. The heart was large, and the mitral valve had old fibrous deposits on it. The kidneys were healthy.

REMARKS.—The recent discussion on this very interesting question at the Obstetrical Society drew forth so much, and the time was so limited, that no opportunity was given to myself, amongst others, of making any remarks. Had there been more time, I intended to mention the above case, which seems to me well worth recording.

It may be that the cancer of the liver had nothing to do with the severe vomiting in this case; but, had there been no pregnancy, I do not think anyone would have thought there was anything strange in this symptom with such a lesion, whilst the vomiting itself was so severe as to be, at all events, very unusual from pregnancy only.

The liver-disease is interesting, also, in its bearing upon the reference made by Dr. Matthews Duncan to the researches of Hecker into the question of degeneration of the liver in pregnancy. Dr. Matthews Duncan drew attention to the difference between the sickness of pregnancy and the sickness *in* pregnancy, the former being that due to the pregnancy, and the latter being due to some other cause acting accidentally during the pregnant condition. Obviously these two forms of vomiting are quite distinct; and, in discussing the question scientifically, it is necessary to bear them well in mind, and so avoid all kinds of complications and differences of opinion which arise inevitably when one speaker is talking of one thing, and another of another thing.

But whilst this distinction is clear logically, it is by no means easy to be always certain with which form we are dealing, when a patient, who is pregnant, is attacked with severe vomiting; and I notice that the title of Dr. Graily Hewitt's paper is “Severe Vomiting *in* Pregnancy” and not *of* pregnancy. Now, if a case occur with severe vomiting in pregnancy, *plus* some disease which *in* itself is capable of producing severe vomiting, who can say that the vomiting is due to either the one or the other? Cases are on record where vomiting has not stopped, even after the induction of labour, and yet it is said that the vomiting was due to the pregnancy. Might there not have been some other cause in those cases?

When there is some obvious and well marked malposition of the uterus, and the replacement into its normal position is followed by the cessation of vomiting, it is tempting to say that the malposition had caused the vomiting. But it is quite certain that there are cases of severe vomiting in pregnancy without any malposition, and without any other ascertainable cause for the vomiting than the pregnancy either *ante* or *post mortem*, and which would therefore come under Dr. Matthews Duncan's “*of* pregnancy.” But where there has been no *post mortem* examination in a fatal case of vomiting, I do not think one is entitled to say that the pregnancy caused the fatal vomiting. It may have been the cause and the only cause, or it may have been an aggravation of some other cause, or it may have had nothing to do with it.

That pregnancy is a cause of vomiting is so well recognised, that vomiting is considered a symptom of pregnancy. At the same time, no one has ever yet proved in what way the vomiting is brought about.

It is easy to theorise, and to say it is a reflex action due to stretching of the uterine walls, but no one has got beyond the conception of the hypothesis. But, supposing for a moment that this is the correct interpretation of the ordinary vomiting of pregnancy, we are still a long way off the solution of the question what causes the severe vomiting in pregnancy. For, first of all, we must assume that the vomiting is due to the pregnancy, and not to some other unrecognised condition; in other words, that it is severe vomiting of pregnancy; and then we have got to show what particular part of the reflex loop is at fault. We see the motor end, that is, the vomiting apparatus, in excessive and dangerous action; and the question is, why does this occur.

One might resolve the numerous theories that could be brought forward into three kinds. 1. "Alteration in the stimuli;" by which is meant increase in degree or kind of that condition of the uterus, stretching of its fibres, or whatever it is, which causes the ordinary vomiting of pregnancy; and this increase may be brought about in numerous ways, including malpositions of the uterus. 2. "Increased irritability of the afferent nerves or reflex centres, or efferent nerves, which form the reflex loop between the uterus and the muscles engaged in vomiting." If it could be shown by microscopic examinations that, in these cases of severe vomiting, the spinal grey matter between the entrance of the afferent (uterine?) nerves, and the exit of the efferent nerves for vomiting (gastric, oesophageal, phrenic, pneumogastric, intercostal, etc.) was in a state of increased irritability, either from disease in itself or in neighbouring parts, it would not only help to explain these cases, but would in itself support the reflex hypothesis of the ordinary vomiting of pregnancy. But, so far as I know, no observations have been made in reference to the condition of the spinal cord in the severe cases of vomiting; so that we do not know, either negatively or positively, the condition of the so-called superficial and deep reflexes during life, nor of the normal or abnormal histological appearances of the spinal cord after death. 3. "Increased irritability of the muscles themselves, which produce vomiting." I have often heard Dr. Wilks say that excessive action of an organ is not generally due to disease of that organ; for instance, severe palpitation is not due to heart-disease, and severe vomiting is generally performed by a healthy stomach. Hence one does not expect to find a diseased stomach in these cases. At the same time, the muscular fibres in the stomach, without being diseased, might be more irritable from some cause or other, such as irritable ingredients in the blood, or pathological conditions of neighbouring parts—for example, hepatic disease.

All these theories on theories require supporting by many facts and observations, before they can be accepted as satisfactory.

Dr. Graily Hewitt has brought forward several facts in support of his view that malposition of the uterus is a cause of severe vomiting in pregnancy; but many cases are on record, amongst which I would place my own, in which no malposition existed. Hence it follows that the severe vomiting in pregnancy may be due to different causes.

In women, vomiting is set going more easily than in men; and any cause which would produce sickness in a non-pregnant state would act more potentially, if anything, if pregnancy existed. Hence the possible causes of severe vomiting in pregnancy are as varied as the causes of severe vomiting in a non-pregnant state.

Of the causes of severe vomiting of pregnancy we know very little at present; but one thing seems to me quite clear, and that is, that the excessive sickness is not always due to the same cause; and, even if it be accepted that malpositions of the uterus will sometimes produce the symptom, it will be found that cases occur where this cannot be the explanation.

THE PREVENTION OF CONSUMPTION.

By G. ROSS FRASER, L.R.C.P.Ed., Wark-on-Tyne.

IN the course of Dr. Hawksley's remarks, in the JOURNAL of June 6th, upon Parasitides in the Treatment of Pulmonary Consumption, he raises the question whether the immunity alluded to is to be ascribed "simply to atmospheric exposure, or to the physiological or parasiticide action of the chemical products of certain burning bodies." According to Dr. Morgan, the non-prevalence of consumption among the crofter population of the Hebrides and north-western coast of Scotland is due to the antiseptic properties of peat-smoke, which, from the construction of their dwellings and the nature of the fuel used, they largely inhale. That consumption is eminently preventable, is evident from the fact that, in one set of circumstances, the population in question, to a large extent at any rate, escape this scourge, while in another they become victims.

If Dr. Morgan's clearly propounded views be correct, then we may in time be furnished with additional resources in dealing with the causes that lead to the phthisical state. But, surely, the happy immunity enjoyed by these people can, with good reason, be credited to other causes than to the real or supposed prophylactic virtues of peat or any other kind of smoke.

I have seen much of crofter life in the far north of Scotland, and can safely say that few other classes live under conditions so favourable to health and longevity. Their easy out-door life, plain but nutritious diet, by no means deficient in variety, warm homespun clothing, and the equable temperature, and the fairly good internal cubic space of their homely dwellings, go very far to account for their physical vigour and immunity from tubercular disease. Chinnied dwellings are gradually displacing those ancient dark domiciles in Sutherlandshire. These were low, but long in form. The fire was placed against a slightly raised piece of mason-work near the middle of the main apartment, the smoke finding exit through a barrel, minus top and bottom, fixed in an opening in the roof. The side-walls, from six to eight feet high, supported a roof whose ridge rose 10 or 12 feet above them, and, as no ceiling existed, the cubic space internally was generally large. From long exposure to peat-smoke, the bare rafters overhead acquired a black unctuous-looking coating, as if newly coal-tarred. This deposit had a powerful peaty odour, and every object within the hut was similarly affected. The aerial fumigation was complete, and yet consumption, although rare, was by no means altogether unknown. To a casual observer, these sable abodes might seem to reek with impurities. In reality, it was not so; for, while frequently under one roof, and in a sense with a common entrance, a strong walled partition with a door separated the bovine from the human inmates. The byre had openings in the walls and roof, and its door generally stood open, a large supply of fresh air being always necessary where cattle are "housed," on account of their size and normally high temperature (102°).

I have now before me a volume entitled *Dr. Beddoes on Consumption* (London, 1801). A part of the book is devoted to considerations on a modified atmosphere in consumptive cases. This modified atmosphere is no other than that of a well stocked byre. Patients of means grudging no expense in fitting up cow-houses, where they took up their quarters for lengthened periods; and, as usual in new methods of treatment in consumption, case after case is cited to prove its undoubted efficacy.

In support of his theory, Dr. Morgan remarks that, "when these Highlanders migrated to other parts of the country, or took up their abodes in chinnied dwellings, they suffered like their neighbours." This observation is at least equally true of many Highlanders, whether they be of the crofter class or not. The migration necessarily entails changes in their mode of living, not always of a healthful kind. Highlanders, as clerks or as university students, frequently suffer. It must always be so when young men accustomed to country life engage too abruptly in prolonged and sedentary work.

The term "chinnied dwellings" suggests an inquiry of the highest practical value. In reality, the assumed hygienic superiority of many of our chinnied dwellings is, to say the least, doubtful. In the great majority of them, direct communication with the external air by means of open chinnied-flues (as far as bedrooms are concerned) is the exception and not the rule. I speak from observation. There is a widely prevalent custom of cramming bedroom-flues with sacks filled with hay or straw, for the purpose of excluding cold in winter, and in all seasons for protecting the grate and furnishings from the tarnishing effects of descending rains. If the establishment boast of a spare bedroom, the largest and best furnished is chosen for the purpose, while the remaining rooms are often too small and overcrowded, and frequently possessed of no flues to close. Is it surprising that families bred in this manner are seldom all well together? The evil is, no doubt, to a certain extent neutralised by the active outdoor habits of the young people during the day; but who can doubt that many of them, partly from this cause, grow up a "feeble folk," with a predisposition to pulmonary disease? Until the homes of the people, their schools, their workshops, offices, and warehouses, are constructed with more regard than generally prevails to sanitary requirements, no marked advance in the prevention of this disease can be looked for. The loss of many valuable lives by consumption is all the more painful from the certainty that, to a great extent, it might have been avoided. In the absence of pure air, physical exercise, sufficient food and clothing, antiseptics can be of little avail against the inroads of the bacillus. They are useful so far as they destroy atmospheric impurities. Their extended use in medicine and surgery has served a good purpose, by directing more general attention to the advantages of cleanliness and ventilation; but we cannot hope by their most