have a tendency to lock up in the bowels the supposed source of irritation?

This is the precise question to which I would earnestly direct attention. The premonitory diarrhoea which rages in cholera-smitten districts is *not* a diarrhoea *sui generis*: it depends on various causes in different individuals; and, although it may generally be treated successfully, no treatment can be trusted, which does not proceed upon an intelligent perception of the cause existing in the individual case.

1. In a large majority of these cases, the chief cause is *panic*. Neither men nor women will confess it; but the family and friends observe it, and know it. Among the weak-headed and idle, who always abound where cholera rages, a large proportion are absolutely horror-smitten at the cholera when once it invades their dwellings or their immediate neighbourhood. And who does not know that both the bowels and the bladder are more readily excited by fear to discharge their contents, than by any other stimulus? Then, sympathy with the attacked directs the attention of those around (unconsciously, it may be) to their own abdominal movements and sensations, and thus excites over action. Anxiety and fatigue, night watching and nursing, all lend their aid to relax the bowels; and bowels so relaxed are easily quieted by taking almost any medicine whatever, which the patient expects will control the complaint; and those which, besides acting on the mind, give a sensible glow to the stomach, are still more certain. Hence, brandy, tincture of rhubarb, laudanum, chalk, cinnamon, cardamoms, *et hoc genus omne*, will often cure the "premonitory" symptoms, and thus, *as we are told*, prevent cholera in a whole neighbourhood.

2. Dyspepsia, from intemperance or other causes, may have irritated the bowels, and some offensive matter requires to be removed. In this case, astringents, opiates, and stimulants, all do harm. Here the calomel and rhubarb plan will succeed best.

3. Exposure to cold, want, or wet, may have excited mucous inflammation. Here the hot sand, salt, or flannels, the mustard cataplasms and fomentations, the warm bath, castor-oil and Dover's powder, are the remedies. And there are cases in which fear, dyspepsia, and cold, have each their share in relaxing the bowels; and these are cases which are liable to run on to cholera. The neglect of any part of this treatment may then be wrong. *In the first instance*, it is very important that any offending material should be removed; and in practice it is almost certain that a dose of calomel and rhubarb will more or less check the disease; but this should be speedily followed up by the external application of heat to the whole abdomen, spine, and extremities; by effervescing drinks mingled with aromatics, if sickness supervene; and ultimately, if the diarrhoea persist, by chalk, opiates, and astringents. Some practitioners prefer the mineral acids, which exercise an immense control over the diarrhoea, which results in mere relaxation of the vessels, or in an excessive effort of nature to expel the enemy.

But, 4. The choleraic poison itself may have taken up its abode in the blood or bowels of the patient, producing, in the first instance, a severe diarrhoea of the bilious character, and then, in a very few hours, a frightful discharge of serous fluid, without, *as yet*, any other symptoms of epidemic cholera. What will stop this "premonitory" diarrhoea? Will chalk or opium, lead or catechu, calomel or rhubarb, sulphuric acid or sulphur, brandy or salt, baths or bleeding, hot air or hot sand, or any other plans or appliances which ingenious practitioners have invented, and sanguine practitioners publicly vaunted—will any of these "prevent" the cholera, and save the patient? I doubt it much; but I would try them all, not blindly, but in obedience to the movements and dictates of nature. Supposing there is no vomiting, I would administer *first* a full dose of calomel and rhubarb; for it cannot be doubted that *here* there is a poison to be discharged, or the patient must die. Next, I would follow the instincts of the patient as to diet. Let him drink any quantity he likes of any fluid he prefers:

cold water, salt and water, and cider, are generally the drinks he looks for; and, to the application of external heat, friction should be added. When the purgative has had an hour or two to perform its work, astringents and opiates may be given. But of opiates I would be very shy; the cramps of the limbs are but efforts of nature to accelerate the venous circulation, and to hurry along the treacherous stream towards the right ventricle of the heart, where it is less likely to stagnate than in the veins; and opiates are apt to check these salutary efforts. But I am now trenching on the treatment of cholera itself, on which more has already been written than I have had time to read.

Alfred Place, Bedford Square, Oct. 8, 1853.

MADEIRA AS A RESIDENCE FOR INVALIDS:

WITH A SERIES OF METEOROLOGICAL OBSERVATIONS
MADE IN THE ISLAND.

By F. D. DYSTER, M.D.

I HAVE read with great interest the paper on the "Climate of Madeira", in the ASSOCIATION JOURNAL of September 2nd; and, if I now publish a few remarks on the same subject, it is not that either the facts are doubtful, or Dr. Lund's paper incomplete, but that I desire to add, if possible, to the weight of his testimony, and to diminish the probability of any phthisical patient, to whom this wonderful climate of Madeira is likely to prove useful, being intimidated or misled by the assertions of Dr. Burgess.

The table of meteorological observations which I append applies to the year 1844; and the observations were *all* made by me.

The means for the year, excepting the latter part of June, all July, August, September, and October, were obtained at Funchal, in a house about 250 feet above the sea level. The barometer was *within* the house, at a south window, always open; the instrument a mountain one of Bate's. The register thermometer and the hygrometer (Mason's) were placed at a north exposure, and, as nearly as can be, free from foreign influences. The tables were kept for my own amusement, little expecting that any one would ever be found hardy enough to assert that the climate of Madeira was damp and variable. I possess in perfection the tendency of John Bull to grumble at climate, and therefore all my bias was to grumble when I could. The opportunities were rare enough.

During the months of June (the latter part), July, August, September, and October, I and my meteorology removed into the mountains, about 2,500 feet above the sea; but even there Dr. Burgess would be a long time before he collected enough dew to wash his face with.

The tables speak for themselves; and the only comment I will offer is a comparison between the absolute mean dryness of England and of Madeira in 1844. The number of days on which rain fell was eighty-four: I believe five above the average. Out of these eighty-four, on twelve the rain occurred at night, and invalids ought to have had nothing to do with it. During the seven years I was in Madeira, I once, and once only, saw 42° registered on my minimum thermometer; and my lamented friend Dr. Renton told me that, in his previous experience, he had only once seen the same.

My own history tells a favourable tale as regards Madeira. In 1838, I had pneumonia, being at the time suffering from disease of the antrum, which destroyed the greater part of the upper maxillary bone. The pneumonia terminated in gangrene. The winters of that year and of 1839 were spent in two rooms. I was worse in the autumn of 1840, and resolved to go to Madeira. Some of my kind medical friends advised it; others (one especially of the most eminent in London) recommended me to stay at home. I should die in three months any where, he said; and I should debase more comfortably in England. His dia-

gnosis was wrong; but his prognosis was perfectly fair, on a single examination. My left lung was as solid as a board, excepting a cavity as big as an orange, which the sphacelated lung had left. The upper part of the right was deeply congested, and did little or no work. I was in the last stage of emaciation, spat half a pint of pus a day, had hæmorrhage about every fortnight or three weeks, with bits of necrosed bone coming away from my jaw continually. But I had a capital stewpan to set against these things, which never failed in its duty; and a great indisposition to sympathise constitutionally with local irritations.

My first year in Madeira enabled me to "hold my own". At the commencement of the second, I made a great and sudden stride towards health. My third found me free from all symptoms, and able to live and do as other people. In 1847-48, I went to Pau; and since that time I have remained in England, and find myself able to do as much as most, and more than many. It is true, I believe, that I had no tubercles; but my lungs required, after the rough assaults they had sustained, entire repose, and the removal of all atmospheric causes of irritation; and in Madeira they found it. During my first winter there, I was only kept in doors by weather on *three days*. What is said in Dr. Burgess's book about dew, is a ludicrous blunder. I do not think I saw dew twenty times in my *town* garden, in seven years, and a comparison of the range of temperature with the dew points will explain this.

With regard to the frequency of phthisis among the Portuguese, I am disposed to believe it to be greater than Dr. Renton supposed. But this is easily accounted for without my attempting to impugn the correctness of that accurate observer and skilful physician. The poverty of the

people has progressively increased, and tuberculosis with it. Hard work, scanty food, insufficient clothing, damp windowless houses in the mountains, dark dirty ones in the towns, are all powerful allies of phthisis, and the frequency or rarity of its occurrence among the Portuguese does not apply to the question on which Dr. Burgess brings it to bear. It only proves that climate *alone* will not prevent tubercular disease, which no sane man ever supposed to be the case.

Some returns sent to Mr. Robertson, of Manchester (*Edinburgh Med. and Surg. Journal*, Oct. 1846), on the period of menstruation among the lower classes of Portuguese, point to the existence of the same condition which favours the development of phthisis. The average age at which 144 girls first menstruated, was (if memory serves me) fifteen and a half. If these had been well nourished, well lodged, and well clothed, there is no doubt that puberty would have been accelerated, as it is among the easy class both of Portuguese and English residents.

The fatal event of many phthisical cases sent to Madeira is to be accounted for on the grounds indicated by Dr. Lund; namely, an exaggerated opinion of the power of climate, in looking upon it not as a therapeutical agent, but an anti-pathological one, leading invalids to neglect all hygienic precautions, which are as needful in Madeira as in England, and in the fact that cases are sent out when advanced into the third stage. But in the latter case, though cure may not be reasonably expected, the descent to the grave may be greatly smoothed. A brother-in-law of my own who died in my house, spent some hours of the day preceding his decease in my garden, and that, at a period of the year, when exposure to the external air would have been out of the question in England.

METEOROLOGICAL REGISTER KEPT AT MADEIRA, 1844.

Comparative Table of absolute dryness at MADEIRA and in ENGLAND, for 1844.

Month.	Barometer, 9 A.M.	Attach- ed ther- mome- ter.	Barometer, 9 P.M.	Attach- ed ther- mome- ter.	Register thermometer.			Hygrometer, 9 A.M.		Hygrometer, 9 P.M.		Days on which rain fell.	Madeira.		England.	
					Max.	Min.	Range.	Dry bulb.	Dew point.	Dry bulb.	Dew point.		Range.	Dry- ness.	Rge.	Dry- ness.
January . . .	29.982	63.5	29.988	63	65.5	56.5	9	61.5	47.5	59.5	48	9	9	14	41	1.8
February . . .	30.010	62.5	30.045	61	64	54	10	60	48	58	47	5	10	12	32	3.1
March . . .	29.908	62.5	29.900	62	66.5	55.5	11	63	45	59.7	45	3	11	18	32	4.9
April . . .	29.803	63	29.872	62.3	67.3	57	10.3	65.7	51	60	51	9	10.3	14	35	6.4
May . . .	29.821	64	29.829	64	69	59	10	66	53	62	53.3	6	10	15	37	7.9
June . . .	29.942	66	29.939	66	71	60.3	10.7	69	52.5	63	53	1	10.7	17	53	8
June* (latter part of) . . .	27.736	60.5	27.749	62	64.5	56	8.5	62	56	57	55	3				
July* . . .	27.800	65	27.806	67	72	64.3	7.7	68.5	56	63.5	53.5	0	7.7	12	34	6.5
August* . . .	27.757	64	27.747	66	69.8	62.5	7.3	64	58	61	56.5	4	7.3	6	41	6.3
September* . . .	27.764	62.7	27.771	64	67	61	6	63	55	60	53	7	6	8	40	5.5
October* . . .	27.732	60.5	27.738	62	63	57.3	5.7	60	53.5	57.5	53.5	13	5.7	7	41	4.1
November . . .	29.880	68	29.823	67.5	70.5	61	9.5	69.5	61	64.5	53.5	12	9.5	8	39	2.4
December . . .	29.878	63.5	29.790	63	66.5	56.5	10	62	51	60.5	52	12	10	11	38	1.7

During the months marked thus (*), the observations were made at about 2,500 feet above the sea level. The others were made at Funchal at about 280 feet above the sea level. None of them are corrected. The instruments were a mountain barometer, and double register thermometer by Bate, and a Mason's hygrometer.

Tenby, South Wales, October 17th, 1853.

PERISCOPIC REVIEW.

CHEMISTRY.

MILK AND ITS CONSTITUENTS.

This secretion has not met with that amount of attention, lately, that its importance, and the means for investigation now at the command of the physiological chemist, would seem to invite. Before noting, however, what has recently been done in this division of organic chemistry, it may not be amiss to detail a few of the accepted facts respecting this liquid, since these may lead to a readier comprehension of what will follow.

First, then, as to colostrum, and the distinction between it and milk: in colostrum there is not merely an increase in the amount of the saline constituents to twice or thrice of that existing in healthy milk, nor a general augmentation of the solid constituents only, in the ratio of about 17 to 11, and which is, according to Simon, principally due to the increased quantity of sugar, although this point is not absolutely determined; but the difference mainly depends on the presence of granular masses, termed *colostrum corpuscles*, which are invariably present in the colostrum, but disappear, as a rule, in three or four days after delivery, being apt to reappear on the supervention of any acute disease. These colostrum corpuscles are much larger than the true fatty milk globules, and consist of small fat globules imbedded in an albuminous substance; whilst, as they