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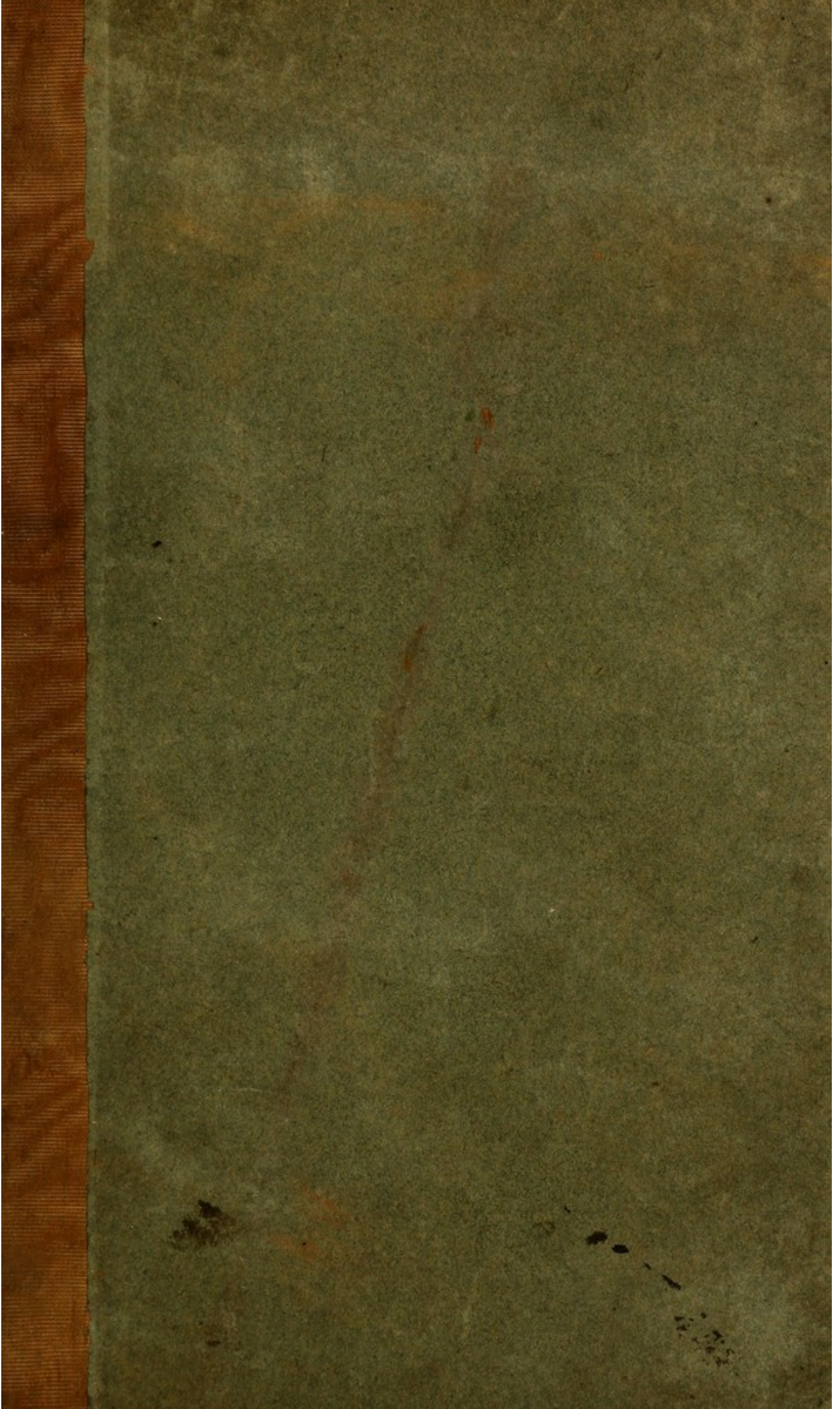
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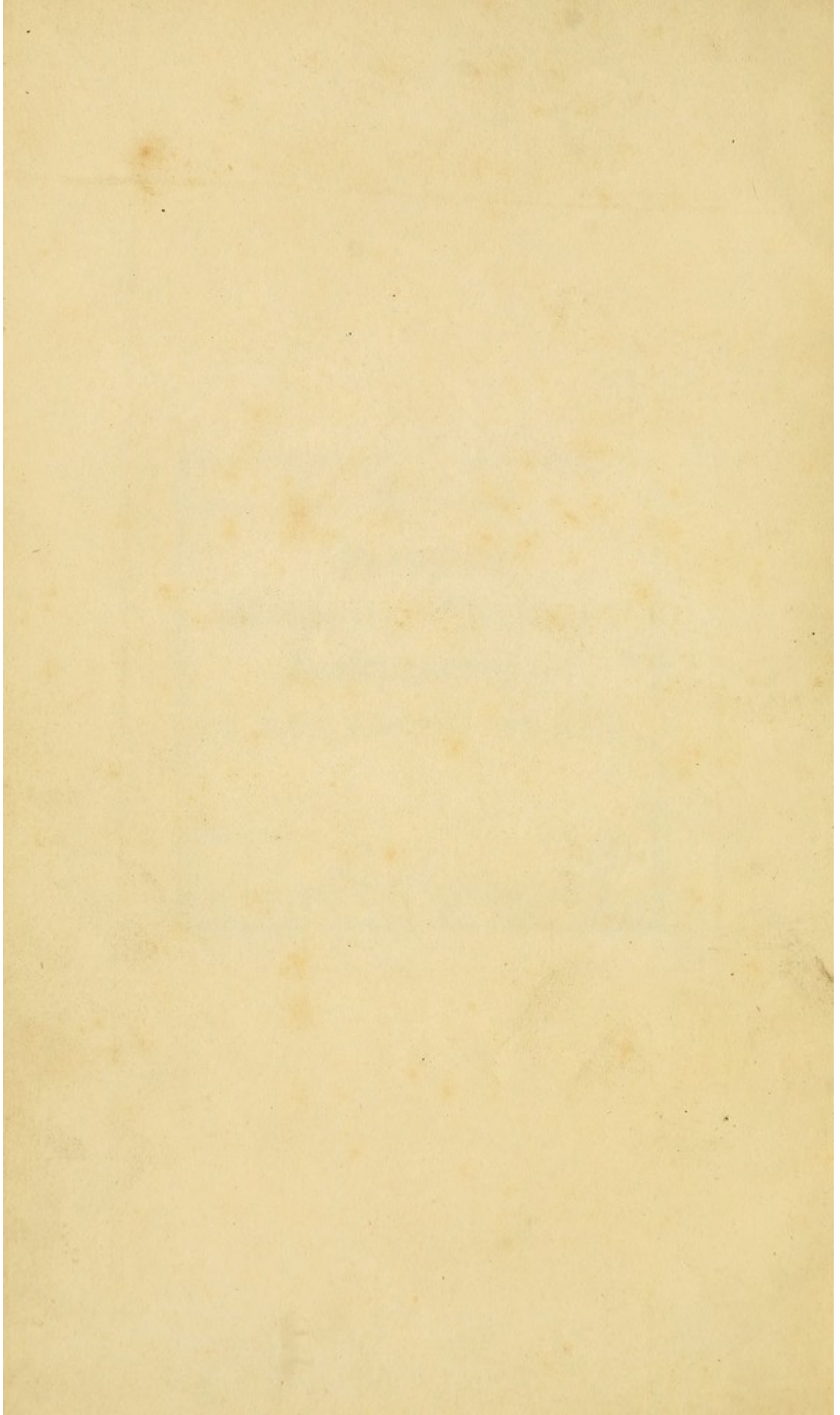
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LONDON

CLINICAL LECTURES
ON THE
CONTAGIOUS TYPHUS,

EPIDEMIC IN GLASGOW, AND THE VICINITY,

DURING THE YEARS 1831 AND 1832.

By RICHARD MILLAR, M.D.,

SENIOR PHYSICIAN TO THE ROYAL INFIRMARY,
AND REGIUS PROFESSOR OF MATERIA MEDICA IN THE UNIVERSITY OF GLASGOW.

Extremum hunc ——— mihi concede laborem.
VIRG.

GLASGOW :

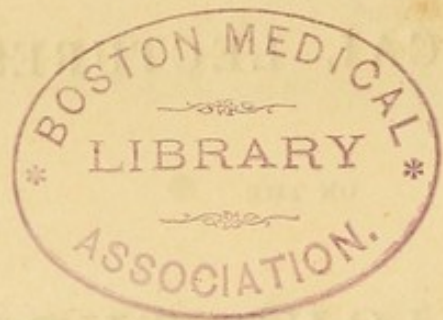
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From

College of Physicians,
Glasgow.

BY RICHARD MILLAR, M.D.

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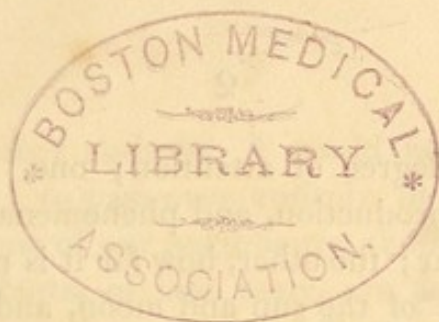
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CLINICAL LECTURES

ON

CONTAGIOUS TYPHUS.

IN the following Lectures, gentlemen, I propose to offer you some remarks on the contagious Typhus, at present, March 1832, epidemic in Glasgow and the vicinity, and of which you have seen so many cases treated in our Infirmary. To point out, by a laboured enumeration of motives, how much it concerns every student to be well acquainted with this disease, I hold to be superfluous labour. It will be sufficient if I remind you, that there is no malady of more frequent occurrence than fever, and none to which you will be oftener called, in the course of your future practice. Whatever relates to it, therefore, you will readily perceive, well merits your utmost attention.

The details of our subject naturally arrange themselves under two heads, or sections. One of these is purely practical, and will contain merely the history, and treatment, of the distemper. The other is more speculative, and controversial, and will embrace the consideration of various disputed points, such as the dogma of crises, and critical days; and, more especially, the question whether the malady be idiopathic, or topical, sthenic, or asthenic: in other words, what is to be esteemed the true nature, or essence, of our low contagious typhus? Perhaps two other speculations might

challenge some degree of attention; one, how far is fever subjected for its production, and phenomena, to the different seasons of the year; the other, how far it is ruled in its course by the influences of the sun and moon, and other Heavenly bodies.

In Clinical Lectures, we require the aid of some nosological system of nomenclature, and arrangement. The one I have uniformly been accustomed to follow, is that of my old master Cullen. According to this nosologist, there exist three species of continued fever, viz., synocha, synochus, and typhus. Synocha is a sthenic distemper, and is exactly the same fever that attends his order of phlegmasiæ, only without their essential accompaniment of topical inflammation. It may be doubted, I think, whether this species of Cullen have any real existence; but such inquiry here would lead us into too long details. His typhus is just the antipode of his synocha, for its leading marks are those of asthenia, or weakness. The heat of the skin, here, he says, is rarely augmented, and the urine little changed; the pulse, if not always quick, is always feeble; the sensorial functions are uniformly observed to be materially deranged, or embarrassed, while the general vigour of the body lies prostrate before the disease. The synochus of Cullen is a mixture of these two preceding species; in its commencement a synocha—in its course, and termination, a typhus. Such, by many, has been considered the usual continued fever of Great Britain, and of other northern regions. Without altogether disputing the general truth of this position, it must, nevertheless, be kept in mind, that our epidemic shows far greater leaning to the latter, than the former type, and that so far from being always a synocha at the commencement, it, often, from the very starting, discovers no symptoms but those of pure, and genuine, typhus. It is this last distemper, exclusively, or the fever that springs from contagion alone, I now, once for all, apprise you, that is to engage our attention, and to form the subject of the ensuing lectures.

After pointing out to you these distinctions of Cullen, it may, perhaps, seem superfluous to trouble you with the

synonyms of other authors. I would hold it, however, an omission, were I to pass over entirely, in silence, the nosological speculations of our neighbours, the French, considering how many students, of late years, are accustomed to resort for instruction to the schools of Paris. In fact, the great father of all nosology was a Frenchman, Boisseir De Sauvages, a professor in the University of Montpellier, and a man of great eminence in his day. His first essay was published in 1732, exactly a hundred years ago, but it was afterwards much amplified and improved. Notwithstanding numerous errors of theory, more imputable to the age, than the individual, Sauvages' work is one of great merit, and practical research—not a mere catalogue of names, like other nosologies, but abounding in curious cases, and observations. I recommend it, therefore, strongly, for your perusal. I have read most of it long ago, and old as I am, I still read it with advantage, and pleasure. Since Sauvages' time, various other nosologies, more or less complete, or nosographies, as the French choose to call them, have appeared in France, especially of late years. A numerous catalogue might be given, as those of Vitet, Pinel, Baumes, who has arranged all diseases on chemical principles alone, Tourdes, Tourtelle, Alibert, Recamier, Villars, Lafond. Of these, by far the most in repute is Pinel, who is now, and has long been, the great nosological authority in France. In his work entitled "Nosographie Philosophique," will be found synonyms for only two of Cullen's species, his synocha and typhus—for synocha, "Fievre Angiotenique;" while he has split Cullen's typhus into two species, assigning to the one the name of "Fievre Adynamique," analogous to our putrid fever; and to the other, that of "Fievre Ataxique," apparently the same with our malignant, low, or nervous, fever. There seems little ground for this distinction of Pinel, our common typhus comprising in itself both varieties, at any rate, these two types, frequently being observed to originate from one, and the same, contagion. Cullen's synochus has not been recognised by Pinel; his system contains for it, therefore, no equivalent, or synonym.

SYMPTOMATOLOGY, OR DIAGNOSIS.

Besides the above symptoms enumerated by Cullen, there are various others that require attention, such as alternate heats and chills of the surface, headach, more or less urgent, with pains extending over the limbs, and trunk, and particularly affecting the back. Not unfrequently we perceive the body dotted on the breast, and other parts, more or less extensively, with a number of purplish or reddish spots, and these, when small, are styled petechiæ, when larger, vibices. They appear to consist, for the most part, of venous blood, either extravasated beneath the surface, or congested in the veins themselves. Generally speaking, they are seldom seen till after the disease has attained some maturity. When you examine the tongue, you will find it dry, or loaded, and, as might be expected from the circumstance, the whole of the chylopoietic viscera, more or less affected; hence, inappetence, constipation, sometimes vomiting, or purging. In the list of symptoms, ought to be mentioned also, some strange uneasy feelings about the præcordia, for which, however, though often highly distressing, language has as yet devised no name. There is something, too, about the countenance of a typhus patient, that strongly indicates the distemper; but this to be understood must be seen, for it cannot be described in words.

Such, joined to those enumerated by Cullen, may be reckoned the principal symptoms of contagious typhus, and when they are well marked, the diagnosis is sufficiently plain, and you are not likely to mistake the disease. The only difficulty occurs about the beginning, when some of the signs are not yet clearly developed; we have mere general pyrexia, and it is not always easy to tell what particular febrile affection may, eventually, ensue. If you know that low typhus is raging as an epidemic, and can trace the case of your patient distinctly to contagion, you will then understand thoroughly how to proceed. You must abstain from all profuse sanguineous depletion, even though the pulse be a little

hard : wait only a few days, and you will find the excitement of the circulation gradually subside of itself. This rule, it is obvious, is applicable only when there is mere hardness of arterial beat, and without the accompaniment of any local affection decidedly sthenic. In these last circumstances, should any evacuation be judged necessary, a purgative, or diaphoretic, will be found capable of accomplishing every useful purpose.

ETIOLOGY, OR CAUSES OF TYPHUS.

The grand cause, and as many contend, the sole cause, of typhus, is contagion, a subtle emanation, or poison, proceeding from the bodies of the diseased, and capable of communicating the disease to others. This virus may act either when floating in the atmosphere of the sick chamber, or as deposited on the walls, furniture, clothes, or other loose articles, with which last, as it adheres firmly, it may be transported to any distance. Substances thus embued, in medical language, are termed fomites.* If you ask how this contagion was originally engendered, I can make no reply. It seems a separate substance, or existence; you might as

* To show the power of fomites, proofs are endless. A taylor who, in the way of his trade, went from house to house in Dublin, had a blanket given him to sit upon whilst at work, which had formerly covered a typhus patient. He took the disease on the fourth day. (Barker; Rep. of Cork Street Fever Hosp. Irish Med. Trans., Vol. ii. p. 522.)—In the House of Industry, Dublin, of the washerwomen who received charge of the infected clothes, not one escaped. (Dr. Reid in Irish Med. Trans., Vol. iii. p. 94.)—In our own hospital at Mile-End, a young carpenter who had been employed to fit up a closet where the cast apparel of the patients had been deposited, in a few days afterwards sickened, and died. The destructive efficacy of fomites is often still more strikingly illustrated, when multitudes of men happen to be collected together, as in courts of justice. Four remarkable instances are on record, often quoted, those at the Oxford assizes in 1577, those at Exeter, and Taunton, in 1586, and those at the Old Bailey, in 1736, and 1750. On all these occasions, the prisoners, and other inmates of the jail, though free of the disease themselves, yet, by the fomites of their persons, or clothes, scattered it all around, among the judges, jury, counsel, and strangers, who came to witness the trials. Of all these classes, several afterwards died. The individuals who thus spread the distemper, seem to have been indebted for their own immunity to their long familiarity with the poison, so that their bodies had been rendered quite callous to its impressions.

well require me to inform you how a man, or a horse, came to be at first created. It is of more importance to ascertain the circumstances that give it activity. Two may be confidently assigned; one, whatever forcibly depresses the powers of life; the other, the peculiar virulence it acquires by confinement, or concentration, in small, and unventilated houses, or apartments.

Among the circumstances inducing typhus, by lowering the powers of life, none are found of more fatal, and irresistible efficacy, than the privations of extreme indigence, or poverty. These, as is well known, are many, and various. The following may be esteemed the principal: imperfect nourishment, or exhausting labour; want of defence against the inclemency of the seasons, from deficiency of apparel, fuel, and lodging; despondency, and other enfeebling passions of the mind; habitual intemperance.* The paramount efficiency of these causes has been too often illustrated. For the same reason it is, that if individuals previously weakened by any other malady happen to come within the range of the poisoned atmosphere, they are, on that account, more liable to suffer from the contagion.

At the same time, it must not be concealed, that the power of the above causes, in promoting the activity of typhus contagion, has not unfrequently been denied, and on the specious ground that the spread of the fever, in different periods of distress, is not always uniform, being much greater at one

* The history of every epidemic shows the power of these causes, and it has been too often demonstrated, of late years, among our operatives, both in Great Britain, and Ireland, during those periodical suspensions of employment, and of course, diminished means of subsistence, that have pressed so severely, in both countries, on that part of our population. In Ireland alone it is computed, that at least a million of the inhabitants, within the last twenty years, have, from this source, suffered the infection of typhus. Even bodily fatigue, and anxiety of mind, are found sufficient of themselves, to excite the distemper, as we see in private families, where individuals, from duty, or affection, have been induced to watch over, daily and nightly, the course of the disease, in some near or beloved relative. Such persons, from mere exhaustion, very generally catch the fever, and I think I have observed, that in these cases it, for the most part, terminates fatally.

time than another, though the degree of privation pressing on the indigent classes may have been nearly, or precisely, equal. The fact is certain, but taken as a general position, the conclusion is wholly erroneous, because it holds good only in those instances where these periods closely approximate, or follow each other in rapid succession; whereas, if they be separated by sufficiently long intervals, the depressing causes are observed to operate with their usual mischievous energy.

The following remarks, it is presumed, will serve, in a great measure, to explain this apparent exception to the general rule.

If we attend to the history of what are termed the morbid poisons, of which that of typhus is one, we will find that nothing can exceed the difference of susceptibility with respect to them, that prevails among different individuals. Some immediately sink beneath their influence, others resist so obstinately, as almost to set them at defiance.

During the plague in Turkey, every Turk does not catch the distemper, though he take no precaution, but is freely exposed to its miasm; if a dozen of persons be bitten by a mad dog, or other rabid animal, the whole do not experience hydrophobia, only two or three, perhaps, while the rest escape.* Hence there appears to be a set of persons who seem, as it were, exempted by nature from the influence of these poisons. But it is no less true, so far as regards a certain number of them at least, that a similar immunity may be obtained by long familiarity with their presence, or impressions, or by having actually passed through their ordeal. Thus no man, except in very rare instances, suffers twice from small-pox, or measles. Unquestionably a similar law, though within much narrower limits, regulates the poison of typhus. To illustrate this point, let us suppose two years to occur at no great distance from one another, each distinguished by

* Some remarkable proofs are upon record; one will be sufficient. A gentleman had a tame wolf which run mad, and broke his chain. Ere he could be destroyed, he had bitten 19 different individuals. Only one took the distemper. (Hamilton collect. concerning Hydrophobia.)

unusual indigence, and distress, and the first attended by a fatal, and wide-spreading, typhus; it by no means follows, that the second of these years is to be accompanied by an equal degree, and extent, of the same calamity. The laws of the animal economy above alluded to, will always interpose here, and prevent, or counteract, more, or less, of such result. The two periods, in so far as regards poverty, and privation, are, no doubt, alike, but in so far as respects the production, or spread, of typhus, they must be accounted essentially different. During the first epoch, the susceptibility of the population, with regard to the poison, was fresh, and unbroken;* in the second, it is exhausted, and impaired. Many of those who might have caught the malady during the second, have sunk under it during the first invasion, and now sleep in their graves:—many of the survivors have passed through its ordeal, and have become thereby more or less fortified against a second attack. A third portion, from some natural immunity which we cannot explain, pass through this second visitation of the malady, as they did through the first, untouched, and unharmed—while a still greater number, by long exposure to the poison, become, as it were, callous, and insensible, to its assaults. It seems to be by some such salutary process, that contagious maladies, at length, gradually terminate, die away, or wear themselves out. The longer they last, they find fewer individuals with sufficient susceptibility for their invasion, and when they do fasten upon a victim, their morbid effects will be found comparatively feeble, and moderate.†

* Previous to the epidemic in Ireland of 1826-7, there had been a cessation of the disease for several years, so that the susceptibility of infection was greatly increased. We have a good example in the Cork Street Fever Hospital, Dublin. Here, out of 57 nurse tenders, no less than 52 caught the fever within the twelvemonth, besides the register and matron. (O'Brien Rep. in Irish Med. Trans. Vol. v. p. 588.) In this instance many of the nurses were new; old nurses, on the contrary, from habitude, as is well known, generally attend the sick with perfect impunity.

† Were it not for this prophylactic power of the constitution, it would be next to impossible for any person to recover from typhus, more especially in a fever hospital, where, being constantly surrounded not only by the emanations

Such appears the real amount of the objection that has been urged against poverty as a predisponent cause of typhus, but the truth of the general doctrine remains unimpaired: nor ought we to guard the less against this source of the disease, because the poison apparently weakens itself by the very frequency of its own ravages, or in time loses a portion of its original activity. The present evil in this way, no doubt, may be more, or less, mitigated, or removed, but the future danger will not be averted. The mountain torrent that deluges our fields, and sweeps away their produce in harvest, is but little less to be dreaded, because, after it has done its mischief, it at length subsides of its own accord, and finally retires within its accustomed channel. *

of his own body, but those issuing from the inmates of the same chamber, he must run a constant risk of being again precipitated back into the distemper. A remarkable example to show the sheltering power of this prophylaxis, has already been mentioned, in the instance of the prisoners who spread the disease at the Oxford, Exeter, and Old Bailey, assizes.

* The same miseries and privations that produce the disease among starving artizans and peasants, often give rise to it also in fleets and armies, more especially the latter. Low fever, indeed, must be counted one of the ordinary scourges of war. Examples are endless, for they are afforded by almost every campaign. To give instances from modern times only: Napoleon's discomfiture before Moscow filled all the north of Europe with typhus. The same calamity prevailed no less fatally, and almost as extensively, among our own armies in the Peninsula, as, for example, during the flight, or retreat of Wellington, after his absurd attempt upon Burgos, and during the forced and precipitate march of Moore to Corunna. The following list of fevers among our troops in Spain, within the short period of three years, from the undoubted testimony of Sir James MacGregor, is altogether frightful. These amounted

During	1812 to	16,923
...	1813 to	11,294
...	1814 to	5,007

In all, 33,224

Affording a practical and biting comment on the madness of the military contests of nations. (Sir J. MacGregor in *Med. Chir. Trans.*, Vol. vi. p. 413.)—Want of due ventilation is not a source of the disease in the dwellings of the poor alone. The same cause often spreads or excites it in over-crowded ships, jails, and hospitals, giving rise to three usual appellations of the malady, the *ship*, *jail*, and *hospital* fever; to these names, from its prevalence among troops under canvass in the field, has been added that of *camp* fever. For a very ample and interest-

With regard to the other cause assigned for the augmented energy of this poison, its concentration in small unventilated houses, or apartments, it is merely an additional link, or proof, in the same chain of argument. The want of fresh air, or wholesome ventilation, in their town residences at least, it must always be recollected, is no less a privation of the indigent, than scanty fare, or than deficient fuel, or raiment. Into these miserable abodes, alike on account of situation, and structure, the genial, and invigorating gales of Heaven, find but imperfect access; and if contagion be generated within, wanting the admixture of the atmosphere, its only efficient diluent, or corrector, it soon attains its utmost virulence, while receiving continual accessions from the bodies of the sick, it, at length, fills every nook, and cranny, and deeply imbues every thing around with its pestiferous exhalations. It is for this reason that the typhus so generally makes its first appearance, as well as rages with its highest intensity, in the narrow, airless, and crowded lanes, and alleys, of our great cities, the well known habitations and resort of mendicity, and indigence. These are its primary and favourite abodes, as their inmates are its earliest victims, and from them it issues forth as from a common centre, or focus, to contaminate the rest of the community.* From

ing collection of proofs to show the power of hardship, privation, and poverty, in producing typhus, also notices of the different epidemics, these causes gave rise to in different wars, see Dr. Barker's Report, towards end, in Irish Medical Trans. Vol. ii.

* The following fact from Haygarth, will show the dreadful power of concentrated contagion. Of 138 persons who were exposed to it, men, women, and children, both day and night, in small, close, and dirty rooms, the whole except 8 caught the distemper; in other words, only 1 in 23 escaped. (Haygarth's Letter to Dr. Perceval.) The pertinacity of its adhesion to substances once imbued with it, is no less remarkable. In one instance, it remained with all its original virulence during two whole years, in the same room of a lodging house, regularly infecting every new inmate. (Clarke, Collect. of Papers, Vol. i. p. 7.) As already remarked, crowded and ill aired places of cities seldom escape, and are apt to preserve a constant focus of contagion. From the authority last cited we learn, that in Liverpool, during the year 1796, 7000 persons were found located in cellars below ground, and 9000 more in back houses, incapable of ventilation. Fever was never wanting. (Ib. pp. 18, 19.)—But it is unneces-

a like cause it is, that it so often attacks the more indigent portion of our operatives, during those periodical suspensions of industry that, of late years, have caused so much distress among that part of our population. In fact, this fever might not inaptly be termed the *poor man's disease*. Accordingly, whenever the price of manual labour sinks so low as not to command the ordinary comforts or necessaries of life, we may then look for typhus, and we will seldom look for it long in vain. By the same reasoning may we explain its almost everlasting presence in our sister kingdom of Ireland. It is an unhappy truth, that in that country, from want of poor laws—from long-continued misrule, or oppression, aided, perhaps, by some peculiar prejudices or habits among the people, destitution, and mendicity, often proceed to an extreme unknown elsewhere; and the visitations of typhus have accordingly been found proportionably frequent, extensive, and severe.* So deeply rooted, indeed, does this plague

sary to go to a distance, gentlemen; we need only quote our own ill aired lanes and closes. For further illustration, were such requisite to show the power of poverty in spreading or producing typhus, see Dr. Reid, in Irish Med. Trans. Vol. iii. from p. 548 to p. 556, and from p. 583 to p. 586.—In contrast with all this, but in proof of the same etiology, I mention the following striking fact. While fever raged all over Ireland, it never penetrated to one of the different garrisons, those of Dublin, Cork, Limerick, Waterford, Clonmel, Kilkenny, Belfast, enjoying the most complete immunity from the disease. The reason is obvious. Even the privates of our armies are now so well fed and lodged, as to be quite beyond the reach of the grand exciting cause of typhus, those miseries and privations, that pressed so hard, at the period alluded to, on the artizans and peasantry of Ireland. One street of Dublin, Barrack Street, so named from its containing the principal barracks of the Metropolis, furnished more patients to the hospital than any other in the city; yet, notwithstanding this circumstance, and that it was full of prostitutes, ready enough to communicate with the soldiers, yet the infection never reached a single individual within the barrack wall. (Cheyne in Dub. Hosp. Rep. Vol. ii. pp. 51, 53.)

* Examples of extreme and peculiar destitution in Ireland are painfully numerous. I shall only mention one or two. In Nicholson's Court, Dublin, an hundred and fifty-one persons were found all huddled together into twenty-eight small apartments. Among the whole multitude could be discovered only one bedstead and one blanket; that is, not quite a single bed and a single blanket to each seventy-five individuals.—(Dub. Hosp. Rep. Vol. ii. p. 48.) Dr. Grattan assures us that from 8th June, 1817, to the end of 1818, seven thousand and four hundred mendicants were registered in Dublin.—(Grattan in Irish Med.

seem to be in Ireland, even for ages, as to have long obtained for itself the name of the *Irish Malady*.

Before quitting this subject, I must not forget to mention an opinion maintained by many, that typhus, though usually engendered by contagion, may yet exist independent of that source, or take its rise from other causes, as cold, intemperance, &c. This is a point not yet settled among medical men. The contagionist contends, that since the disease, in innumerable cases, is known to proceed from contagion alone, it must be presumed to do so in all instances whatever, because it is unphilosophical to assign for any phenomenon more than one cause, when it can be adequately explained by one. If you tell him that the malady often occurs where it is impossible to trace it to any such source, he admits the fact, but denies the inference. He quotes against you the example of other contagious diseases, small-pox, measles, scarlatina, distempers which no man imputes to any cause but a specific poison, yet in many cases of which, all allow it is impossible to tell how, when, where, or by whom the infection has been communicated. He applies this reasoning to typhus. He affirms that many persons labouring under the disease, and of course exhaling its poison, are yet able enough to go about, so as to infect others, and that it may still be more insidiously or secretly, conveyed through the medium of clothes, though the wearers themselves may be free of the disorder, as well as by other fomites. All this is plausible, and it cannot be denied that the contagionists have the strength of the argument on their side.* Nevertheless, that there may exist a low con-

Trans. Vol. iii. p. 38.) And from Dr. O'Brien we learn that during the epidemic of 1826—7, in the Liberties of Dublin alone, the Inspectors found no less than twenty thousand persons in a state of starvation.—(O'Brien *ib.* Vol. v. pp. 515, 557. See farther Report of same Physician in *Irish Med. Trans.* Vol. ii. pp. 477, 478, 479.) It was no uncommon thing among the poor of Ireland to feign themselves ill of typhus, so that by gaining admission into some fever hospital, they might procure a morsel of food.

* In aid of this reasoning, Dr. Gregory, Physician to the Small-pox Hospital, London, has lately assured us that of the patients he admits, not one in twenty can tell when or by whom the disorder was communicated.—(*Lond. Med. Gazette*, No. 220, p. 748.)

tinued fever, independent of contagion, or from other causes, as cold, &c., I cannot help considering as an almost indisputable truth, though I am not prepared to maintain its exact identity with that proceeding from infection; on the contrary, I hold the two diseases to be somewhat different. In the fever from cold, I have seldom observed the pulse so uniformly feeble and languid as in the other; nor is the duration of the two maladies alike. At the same time, it must be allowed that many cases occur where such differences cannot easily be detected. In a mere practical point of view, I reckon it of some importance that the two distempers be discriminated from one another. If the case decidedly and indisputably arise from contagion, it will be matter of prudence to abstain from rash and profuse depletion, in the confident expectation, whatever symptoms of synochus may appear at first, that these will soon vanish, and nothing remain behind but the usual signs of ordinary and genuine typhus.

PROGNOSIS.

We proceed here both by synthesis and analysis. Synthetically, we begin with taking a general survey of the whole symptoms in their union with one another, always keeping in mind that typhus is a disease which, once fairly begun, will run out its own course in spite of all our efforts, and that each case will assume its own morbid character, which character it will generally maintain to the end. We must then study the nature and bearings of that character, so as to estimate its gentleness or malignity, together with the degree of its mildness or violence, deducing from the first, a prosperous, from the last, an unfortunate issue. Our other steps are by analysis. As all the organs, and functions, of the body are more or less, implicated in fever, we examine them all by turns, to ascertain how far each of the first may be injured, and, to what extent, each of the second may be impaired. Such is the usual mode resorted to in forming our prognosis. With respect to details, the best order will be that of our customary queries in the chamber of the sick. We begin with the

PULSE.

This must always be a main element of prognosis, more especially in fevers. If the pulse is found unusually quick and feeble, it will be an unfavourable, if moderately slow and firm, a favourable, sign. As to mere celerity, allowance must be made for age. In children, as is well known, the course of the blood is naturally more rapid than in adults, and if in the last, the number of arterial beats, during typhus, as usually happens, run from a hundred to a hundred and twenty in the minute; in a child of six or seven years, you may look for a hundred and forty, though without more risk in the one case, than in the other. A very feeble pulse, though dangerous, is not necessarily fatal, provided the accompanying symptoms are not particularly urgent. I have repeatedly restored patients, where the circulation in the arm was a mere thread, or flutter: in the following remarkable instance, it was extinguished altogether. Going my rounds, in the hospital, one day, I came to a bed where the occupant seemed in the last extremity. Among other deadly symptoms, the pulse at the wrist was entirely gone. So hopeless, indeed, did I think him, that I passed on without any prescription. About an hour afterwards, however, some signs of vitality appearing, my clerk luckily conceived the idea, that something might be done by powerful stimulants, and as deglutition remained, he plied him with large and repeated doses of wine, spirits and sulphuric ether. The experiment succeeded. The pulse soon rallied, though feebly; the previously threatening symptoms declined by degrees; and at my visit to the hospital next day, instead of dead, I found the man lying with his powers of life revived, the pulse beating respectably firm, the other functions improved. Not the slightest relapse occurred. In two days the patient was out of danger: after the usual time he was dismissed cured. This is no doubt a solitary instance, but it shows that we should never despair. However low we may find the pulse, or even extinguished at the wrist, yet if the sensorial functions be not wholly overwhelmed, and, above all, if there be yet, left some animation in the eye, we must not

abandon the case, but with activity and perseverance, resort to every mode of resuscitation in our power.

We next seek information from the tongue. The tongue is always more or less affected in typhus. It is either loaded, or morbidly dry. What is called loading, consists of an exudation of various thickness and consistence, and of different colours, white, yellowish, brown, blackish, the last sometimes investing it like a crust, and peeling off in films. It is the middle and point, chiefly, that suffer these alterations. On other occasions, it is free of exudation, but dry and shining. In more rare instances, no change can be perceived, except preternatural redness. It is alleged by authors, that this last appearance always indicates some internal inflammation; but in typhus, I have not found the remark verified by experience. A return of the tongue to its natural state is always reputed a favourable sign, and the amelioration generally takes place first at the sides; the mark, however, is not always infallible. Occasionally the tongue recovers itself, though the other symptoms do not yield, while, on the contrary, it sometimes continues foul or dry, after the other morbid appearances recede. In the first instance, we look for a return of appetite, but are disappointed; while in the second, some desire of food returns, though unexpected. When the disordered state of the tongue, thus survives the fever that gave it birth, it can no longer be considered as an index, or guide, to the general condition of the system: it is a mere insulated sequela—in its nature purely topical, and to be treated accordingly.* After the tongue, we naturally turn our attention to the abdominal viscera. We cannot expect a typhus patient to swallow and digest food, but

* A curious circumstance with respect to the tongue, may be often noticed in patients labouring under delirium. If you desire them in this state, to put it out, they readily obey, but unless you urge them frequently, they often neglect to draw it back. The functions of memory, and of the association of ideas, are here impaired, along with other sensorial powers. The delirious person almost instantly forgets that the tongue is thrust out of his mouth, and the trains of the usually catenated muscular motions being for the time interrupted, he feels no impulse to replace it in its natural situation.

he may continue exempt from more violent disturbance, as pain of stomach, gripes, purging and vomiting. These are untoward symptoms, but their danger is to be estimated rather from their complications, than themselves. The same remark is still more strictly true of tympanites and hiccup, both of them little to be feared at the beginning, but greatly to be dreaded, if they occur towards the close of the distemper. Bloody stools constitute a still more formidable appearance. In early stages, patients may survive, but in the middle and latter periods of fever, I never knew a single instance of recovery after profuse discharge of blood from the intestines.

With respect to the revival of appetite, it is needless to say how gladly it is to be hailed, provided, at least, it be coincident with other favourable signs. A curious example to show the necessity of this prognostic limitation occurs in delirium. During this state, patients are not unfrequently observed, not only to receive food unconsciously, when presented to them, but to swallow it, with all the apparent greediness of hunger. Hence nurses are often imposed upon. If you ask a nurse how the sick person takes his food, she will reply perfectly well: but if you find that at the same time the pulse, the tongue, and sensorial functions, have attained no improvement, you will then detect the fallacy, and discover that this reported restoration of appetite is merely illusory, and is neither more nor less than a symptom of delirium, or of the hallucination of mind that forms so prominent a symptom in our low contagious typhus.

We must next advert to the condition of the skin and surface.

Chills and heats, are known to be the almost invariable concomitants of every fever; in our epidemic of the present and former years, neither have been found remarkable.

Of the two, the former is more to be dreaded than the latter. Thus, if you find the extremities, more especially the hands, of icy coldness, and, at the same time, covered with a clammy moisture, it is of most evil omen, since it may imply either that the heart is too weak to drive the blood to the ex-

tremities, or that the brain and nerves, have so far lost their energy, as to be unable to furnish, if such be their actual function, a due supply of heat to the system. But whatever be the theory, this is a symptom that must always be viewed with dread, by every experienced practitioner.*

Inequalities of temperature, in different parts, so that one portion of the body is found morbidly hot, while another is preternaturally cold, are believed to indicate danger, but the appearances are not of frequent occurrence.

As to petechiæ and vibices, I hold them of little account. By the elder writers, indeed, they seem to have been greatly dreaded, and were exalted into such importance, as to furnish one of the names (petechial) for our low fever, but so far as my own experience warrants, they hardly indicate increase of

* As to the danger to be dreaded from the extreme coldness of the limbs in typhus, there can be only one opinion, yet even when a sinking of the heat takes place in a far inferior degree, it is an unfavourable circumstance. During the late epidemics of Ireland, great attention was paid to the temperature of their patients by different physicians of the Dublin Hospitals, and this was the uniform result of their observations:—they express more apprehension from the decrement, than increment, of the natural warmth. In quoting this authority, I cannot omit the opportunity of expressing the deep obligations I conceive incurred by the whole medical world, for the numerous and able descriptions of typhus we derive from the Irish physicians, as in the Irish Medical Transactions, and the Dublin Hospital Reports. For my own part, I gratefully acknowledge that I have found them of the utmost utility, both in guiding my practice, and in compiling the present lectures. A long list of names might be enumerated here, which I set down just as they occur to my recollection, and without order or preference, as those of Perceval, Grattan, Cheyne, O'Brien, Macdonnell, Barry, Stoker, Barker, Hagan, Bracken, Mills, Reid, Pickells, Marsh, Stokes, Harkan. Such is the unhappy prevalence of typhus in Ireland, that compared to the experience of these gentlemen and others of their countrymen, our own sinks into nothing. From 30th September, 1817, till 30th September, 1819, there were received into the different Hospitals of the city of Dublin alone, no less than forty-one thousand seven hundred and seventy-five patients with fever. Adding to these, the extra Hospital cases, the whole number in the metropolis, did not fall short of fifty thousand. O'Brien in Irish Transactions, Vol. iii. pp. 452-53. What a field of practice, comprehending every possible variety and contingency of the distemper, must have been here laid open.

danger. I have never observed that they either add risk to the malady, or retard its cure.

The flying pains that occasionally attack the superficial muscles, and a rheumatism, require but little attention, they either vanish of themselves, or yield to the slightest diaphoretics.

There is an affection of the surface which I find myself unable to designate by a single word, and can only describe by saying, that it is an inability to sustain the superincumbent weight of the body, or to support the degree of pressure inseparable from a recumbent posture. It, of course, manifests itself chiefly in those places, where the pressure is apt to be greatest, as the sacrum, hips, haunches, back of the head, shoulders, elbows, ankles, in short, those angular, or projecting surfaces, which, to use a French term, might be styled the principal "points d'apui" of the body. At one or more of these points, the skin dies, assumes a black or livid hue, degenerates into a foul ulcer, phagedenic, or gangrenous, and sometimes of frightful extent. Some writers have fancifully supposed that such ulcers may, occasionally, prove serviceable by acting like an issue, or a counter-irritant to the febrile diathesis. A more solid opinion is, that they are always to be prevented, if possible, since, besides being sometimes fatal, they always protract the cure, and prove a source of the utmost distress to the patient. They presuppose, likewise, great exhaustion of muscular strength, as the evil of pressure must be much enhanced by this cause, the sick from weakness, being unable to vary their posture. The same ulcers, seem now and then, produced by the acrimony of involuntary discharges from the rectum and bladder, an affection sufficiently deadly, and deplorable of itself, and the complication rarely admits of cure. It is worthy of remark, that this liability to breach of surface is, by no means the same, in all patients, though exposed alike to its causes; there is peculiarity of constitution here, for many escape it in circumstances that would infallibly produce it in others.

We come now to what perhaps may be reckoned the most

important of all prognostic signs, those deducible from the state of the Sensorial Functions.

There seems here abundant ground for the belief, that the first impression of the typhus poison, or exhalation, when applied to our bodies, is always made on the nervous fibre, or tissue, and that hence arise those various derangements of its grand centre, the cranial brain, sometimes the spinal, that form such prominent features of the whole disease. I shall begin with headach. This is a symptom almost invariably accompanying the commencement of typhus, yet I do not know that it furnishes a basis for prognostic. We observe it of different degrees of constancy and intenseness, yet generally yielding to remedies long before the termination of the fever. If peculiarly violent, it may indicate some serious disorder of the brain, or its membranes, and so far, must be considered of evil import; yet we seldom can draw from it any certain prediction concerning the final issue. Delirium, its frequent concomitant, or sequela, is a far more important, or terrifying, appearance. This, too, admits of great variety, in point of severity, and continuance. Sometimes it appears only during night, and ceases during day; at other times, it harasses the patient equally during both periods. At one time it is violent and furious, requiring coercion, and the strait jacket; at another, quite mild and tractable. The hallucinations of mind attending it are no less variable. Sometimes we observe the trains of ideas to be sad and mournful, at other times cheerful, joyous, and full of exultation. Patients occasionally think they are pursuing their ordinary trades, and occupations, with all their usual cares, and anxieties.* One man

* Fever Hospitals abound in such instances. One of Dr. Cheyne's patients in the Hardwicke Hospital, Dublin, was a cooper. He was caught, one night, taking asunder the frame of his bed, in order to make a tub out of the spars.— A cow-herd, confined in the Cork Hospital, mistook the sick around him for so many cows, and was frequently trying to rouse them by means of his accustomed cries. Dr. Pickells, in *Irish Med. Trans.* Vol. iii. p.108. A carter in our own wards was incessantly driving his cart, and talking to his horse. This hallucination continued no less than four days and nights; but the man recovered.

appeared actuated by deep remorse and despair, insisted that he was condemned to be hanged, and was constantly talking about the day of execution. In some, the predominant feeling is terror, and terror in the most intense degree; when you approach their bed, they scream out, start up with the most agonizing fear pictured on their countenances, try to escape, or hide themselves beneath the bedclothes.*

Under all these, or other shapes, delirium must ever be regarded as an alarming symptom. How far one variety may indicate greater danger than another, I confess myself unable to decide. Judging from my own experience, where extreme and unappeasable terror is the predominant feeling, I would consider it of fatal augury. Under this unfortunate hallucination, I never knew a single instance of recovery. At the same time, the examples that have fallen under my notice are too few to draw from them any certain or general conclusion.†

Equally to be dreaded with active delirium, is that profound coma, where the patient cannot be roused—where the pupils are either morbidly contracted, or dilated, and where the pulse is, at the same time, tardy, or intermittent. No man will view these symptoms without the most serious apprehension. Luckily, they do not occur with very great frequency in typhus. In opposition to all this, however, there is a species

* A patient in our female ward, after escaping from fever, was seized with acute parotitis, which occasioned violent delirium. Under its delusion, she insisted on having her head cut off, not with any view of self-destruction, for that never entered her mind, but solely, it appeared, for relief of the pain. To obtain her purpose, she first of all applied to the clerk, but being unsuccessful, had recourse to me, at the usual visit. Being again refused, she burst into tears, exclaiming that she was shamefully, and cruelly used, and that nothing was done for fever patients, in the Glasgow Infirmary.

† Upon the whole, there is no single symptom of fever that indicates greater danger than delirium; every experienced practitioner, I believe, will subscribe to this opinion. Dr. Grattan of Dublin asserts that if this evil subsist so long as three days and three nights, there are few patients that will escape with life, such is the exhaustion its constant excitement produces.—*Irish Med. Trans.* Vol. iii. pp. 440-41. This, perhaps, is taking too gloomy a view. You have seen various cases treated successfully where the symptom was of at least three days' duration.

of coma that, however paradoxical it may appear, I hold rather desirable than to be dreaded. The patient here lies in a state of mild stupor, or, more correctly, seems buried in a profound, and imperturbable, sleep. The senses, for the time, may be said to enjoy a holiday; for he neither tastes, nor smells, nor feels, nor sees, nor hears. What passes within we know not, but there is complete interruption to every disturbance from without. All external impressions pass over him unheeded, and unfelt. When you rouse him up—and this capability of being roused forms the distinction between this and the other dangerous coma above noticed—he mutters a few words, and then sinks back into his wonted repose. This singular state, the *coma somnolentum* of nosologists, will often continue for several days, and patients under it seem, for a time, free from all contest with the fever, and, as it were, to lay in a reserve of strength to meet the future contingencies of the disease; at any rate, it is rather a favourable than unfavourable sign. I never knew an individual die who had passed fairly through its ordeal.

In direct contrast or contradistinction to this *coma somnolentum*, we sometimes meet with another affection, for which, however, nosology has not yet furnished a name, where the different senses, instead of being preternaturally obtuse and torpid, are found, on the contrary, morbidly and inordinately, acute. Common impressions, in place of being indifferent or pleasurable, excite nothing but uneasy feelings. The mildest light is painful to the eye—the softest sound is torture to the ear. But the chief seat of the evil seems to lie in the nerves of the skin. So immoderately tender and sensitive are these nerves, that the patient cannot suffer himself, even for a moment, to be touched. If you lay hold of his hand in order to feel his pulse, he instantly screams out, and entreats you, sometimes with anger, sometimes almost with tears, to desist. The same unnatural sensitiveness extends over the whole body. The pressure of the bed or bedclothes is intolerable, and he is constantly changing his position, which, instead of alleviating, only enhances his uneasiness. Jactitation is extreme, and he is entirely deprived of sleep. It is needless to remark how much such a condition must necessarily aggravate every

existing symptom of fever. As to the exact prognostic inference, or how much danger it may indicate, I have seen too few instances of this curious affection to determine. Experience, though limited, has taught me to regard it of dangerous, or fatal, tendency. Two cases have appeared in our wards during the last twelvemonth, both corroborative of this conclusion; one, that of a man where, though it assumed a chronic shape, the issue was finally unfortunate: the other that of a female, where it was highly violent and acute, and where the patient died the very next day after its appearance.

In the two singular nervous derangements above described, we found the whole of the external senses, without exception, perverted: occasionally, again, it is only two of them—the eye and the ear—that suffer. Deafness is a most common symptom, nor do I think it bodes danger: some practitioners, on the contrary, regard it as favourable. It will at least exempt the patient from the irritation of noises. It is often exceedingly inconvenient to the practitioner, however, by preventing due inquiries into his patient's condition. As for the eye, the usual perversion of vision consists in spectra of small size floating about in the atmosphere around, more particularly assuming the appearance of flies (*muscæ volitantes*), and which, we are told, the sick are perpetually endeavouring to catch. Of this symptom I know but little from personal experience; I have no recollection about it, nor do I think I have seen it above four or five times in my life.

Considering with what a deadly grasp typhus poison lays hold of the brain and nerves, we need not wonder that convulsive and paralytic affections should appear so frequently in the disease, or should constitute some of its principal symptoms. Among the first may be chiefly noted those short, and jerky, motions of the forearm, and hands, so familiar to the malady, which, as they occur in a place where there are so many tendons, both long and short, are usually designated by the sort of technical phrase, *subsultus tendinum*, or jumping of the tendons. Similar convulsive twitches are observable in other parts, as in the extremities, and more particularly about the muscles of the upper lip, where they are by no means unfre-

quent. As to the prognosis, authors generally take a gloomy view, considering them as of very dangerous import; a conclusion, however, not warranted by experience. In our ordinary typhus they are seldom found altogether wanting, and so far as my own observation reaches, as many patients recover after them as of those who never experienced them at all. Along with subsultus tendinum, is also commonly arranged another sign with like technicality, termed *carpologia*, or chaff-gathering, because it consists of a similar series of brief shaking motions, as if the sick were picking fragments of straw, or chaff, from the bedclothes. Perhaps this affection has been wrong placed, and should be considered a mere visual hallucination, rather than a convulsion. Whatever be its nature, it is of rare occurrence. I have certainly met with it, but by no means frequently.

As to the paralytic affections induced by the poison of typhus, none is of more deadly omen than palsy of the sphincter muscles of the bladder and rectum, giving rise to an unconscious, and involuntary, discharge, in bed, of the urine, and fæces. The total oblivion here of all ordinary or established feelings or habits, shows the deep injury that has been inflicted on the sensorial organs, and the risk to life is in proportion. I know of no symptom that ought to inspire a greater despair of recovery in the mind of the practitioner. Exert ourselves as we may, it will generally be found an almost certain precursor of destruction. Another paralytic affection occurs in the bladder itself, though apparently attacking a different portion of the organ. In the former instance, the sphincter alone, was in fault, but here it is sound, while the paralysis affects the muscular fibres of the interior, so as completely to disable the viscus from expelling its contents. There is thus produced what may be called a typhus dysury, and if we believe high authority, it is of more evil augury than even palsy of the sphincter. The late Dr. Gregory of Edinburgh used to declare in his lectures, that he never knew a single patient survive after the occurrence of this symptom.* The disease

* There is no rule however without exception, particularly in medicine. We have met with two cases, where though the disease was far advanced, yet the

here I have imputed to the bladder alone, without implicating the kidney, and I think on good grounds. The urine which we are often obliged to draw off by the catheter, affords sufficient testimony that the gland itself has been performing its function.

Another, and most dangerous, paralytic affection in fever, I had almost forgotten to mention, namely, palsy of the muscles of deglutition. We have lost four or five patients mainly from this cause. The power of swallowing being lost, all access of wine or other medicine to the stomach, of course, ceases. Enemata afford but an imperfect resource, and I commonly found the sick too weak, and exhausted, for Juke's apparatus.

Among other effects of typhus poison, none is more striking than the extreme prostration of strength it is apt to occasion, reducing the whole frame, as it were, to a state of almost infantine imbecility. This happens, sometimes, at the beginning, more frequently, during the currency of the disease, either from the more gradual operation of the virus, or from simple exhaustion. When it exists in excess, and then it is to be highly dreaded, it becomes easily cognizable by the posture of the sick. If you find your patient laying supine, or stretched flat upon his back, for days together, and unable to stir a muscle, there can then no doubt remain, that this perilous, and distressing symptom has fairly taken place. You must endeavour by every mean in your power, to support the languishing strength, though too often your efforts will be vain. The fatal debility continues to creep on, from day to day, or from hour to hour, encroaching more, and more, on the powers of life, till, at last, it becomes a mere step, or prelude, to dissolution.*

patient survived this dangerous symptom. One was that of Mary Murray, Vol. ii. p. 89. The name of the other has escaped my memory.

* Those authors who contend for the inflammatory nature of fever deny that weakness ever forms a real symptom of the distemper, because such doctrine would be hostile to their theory. They consider the notion as purely illusory, and tell us that the powers of life, are merely oppressed, not weakened, as if changing the names were to alter the nature of things. They compare vitality

But of all prognostic appearances in typhus, incomparably the most gloomy, is a slow, deep, and laborious, respiration, if accompanied by feeble pulse, and other unfavourable signs, and not occasioned by any local affection of the chest. The moment this occurs, I set down the sick for lost, nor, unfortunately, have I ever been deceived in my prognostic. I do not recollect to have seen, even so much as in one solitary instance, any patient survive this casualty of the disease. Why it should be thus so invariably fatal, I am unable to explain. I have observed infinitely more distress in breathing, from many thoracic maladies, as asthma, phthisis, peripneumony, but without any such sudden, and certain, termination in death. I have often tried, by the most careful dissection, to discover the cause, in some morbid affection of the heart, lungs, or their membranes, but always in vain. I can ascribe it to nothing but that fearful influence which the typhus poison exerts over the whole nervous system, operating, it is possible, in a particular manner, over the nerves more especially subservient to respiration, the cords, perhaps, of the par vagum, or pneumo-gastric, and the twigs of the phrenic scattered over the diaphragm.

Before concluding the subject of prognosis, I must not omit to notice the signs we are accustomed to derive from the condition of sleep, its enjoyment, interruption, or privation. That this important function of health should be always more, or less, impaired, by the numberless pains, or uneasy feelings, that harass the sick in typhus, was necessarily to be expected, but the amount of the evil is different, in different individuals, and we judge of the danger by the degree. If rest be little interrupted, it is needless to say that the sign is propitious,

during fever to a spring placed under a heavy weight, which, though it overcomes the elasticity for a time, yet leaves it in possession of its original force. Unfortunately they do not inform us how this load is to be lifted up, and removed. They forget that metaphor is not reasoning. Apparently they confound the phenomena of typhus with those of apoplexy, though essentially different from one another. In the last disease, though there be temporary suspension of sensation and motion, yet the pulse for the most part remains vigorous, or full—in the extreme imbecility of typhus, on the contrary, it is either entirely extinguished or reduced to a mere thread.

and if entirely wanting, no less the contrary, the latter circumstance invariably adding to the exhausting power of fever, and, of course, robbing the patient of that reserve of strength which might, otherwise, enable him to bear up under the pressure of so protracted a disease. Besides, almost all the symptoms are sufficient, individually, to banish sleep, and when sleep returns, it may be hailed as a signal that these are surmounted, or, at least, have abated of their violence. On all these accounts, the state of sleep, as forming an important element in prognosis, is always an object of particular scrutiny in the chambers of the sick.

Here, however, I must inform you, that on this point you cannot always trust to the report of the sick themselves. Say that you inquired of a patient how he rested during the preceding night, he will not unfrequently answer, that he never shut an eye, though you are informed, and informed truly, by the nurse, that he passed the whole time in a state of quiet, and comfortable, repose. There is here no intention on the part of the sick person to deceive you, he is, in fact, deceived himself. He labours under the influence of a particular species of delirium, or coma, not unusual in the disease, what has been not unaptly termed *coma vigil*, by authors. This singular symptom may perhaps receive some elucidation from the remarks that follow. Without entering here into any intricate discussion concerning that curious phenomenon of living bodies we are accustomed to designate by the name of sleep, and of which a rational theory is alike wanting to metaphysics, and to medical philosophy, it will be sufficient to remark, that one of the principal means by which we distinguish our waking, from our sleeping, hours, is, by the vivacity, or strength, of the impressions of sense, as they flow directly from their immediate organs, contrasted with the faintness, or indistinctness, of the same impressions as afterwards converted into their corresponding ideas, and reflected back by the imagination, and memory, two faculties, be it remarked, that lose but little of their activity, and vigour, during the time of our nocturnal repose. The same criterion, too, during health, prevents us from confounding

dreams, even the most vivacious, with actual occurrences. But this power of discrimination is occasionally lost in disease. In typhus, memory, and imagination, are no less disordered than other sensorial functions, and their particular perversion here seems to consist in this, that the ideas they present to the mind, are so preternaturally, or morbidly, vivid, that they pass with the patient for the genuine, and original, impressions of his external senses; and hence, under this delusion, he is induced to deny that he slept at all, during the night, but, on the contrary, to maintain that he passed every hour broad awake. His whole train of thought was one continued dream—but a dream so impressive, that it was readily mistaken for reality. Such sleep, however, imperfect as it may be, is better than none. It at least secures rest to the voluntary muscles, and exempts from the tædium, and misery, of entire wakefulness. As amendment proceeds, this excited state of the imagination, and memory, gradually wears away, and along with other functions, they recover their former soundness, so that this impediment to the natural state of sleep no longer continues. A return to the former enjoyment of rest, indeed, may be viewed as, at once, an effect, and cause, of convalescence. As the first, it shows the declining state of the disease; as the second, it exerts a kindly influence over the remaining symptoms, more especially renovating, and restoring, the lost, or languishing, powers of digestion.

There is still another source of prognosis—that derived from the age of the patient.

It is a curious and certain, but hitherto unexplained fact, that at the beginning of life, typhus commits much fewer ravages than afterwards: indeed, at this era of existence, it seems almost entirely harmless. At what particular year, however, this exemption from the danger of fever is enjoyed in the greatest perfection, when it commences, or when it terminates, I am unable to inform you; yet I think it may be safely enough affirmed, that previous to seventeen, a patient, comparatively speaking, runs very little risk from this malady. Of this assertion, some no less satisfactory than striking proof, may be adduced. Thus in the Cork Fever

Hospital, nine hundred and fifty-five patients under seventeen years of age, were treated by Dr. Barry, and only six died, or 1 in 159 and a fraction. I confess, when I first read this statement, it appeared to me so extraordinary, that I was led to suspect one of two things, either that the cases had not been all instances of genuine fever, or that there was some error in the printed numbers. Now, however, I believe in the correctness of the account implicitly, and you have all witnessed full corroboration of it in our own wards. Here you have seen treated two hundred and sixty patients, under seventeen, one hundred and ninety-six males, sixty-four females, and the deaths were only six; four among the former, two among the latter. But of the patients thus lost, not above two or three at most, were cases of genuine unmixed fever, so as to be properly counted in the catalogue of casualties. A short analysis will place this in a clear point of view, and first of the males.

William Boyd, *æt.* 13, Vol. xxvii. p. 188. This was a case of decided hydrocephalus, as evinced by the symptoms when living, and the dissection when dead. It had no connexion with typhus. The next, David Thomson, *æt.* 10, Vol. xxvii. p. 185, was in like manner, no case of fever. This poor boy had a singular fate. He had administered to him two doses of calomel, each *gr.* iv. mixed with jalap, and both doses acted briskly as a purge, yet from this small quantity of mercury was induced not only such profuse salivation, but such violent tumefaction of the gums, tongue, cheeks, and even the parotid glands, that he sunk under the irritation in less than a week. From various symptoms, I had suspected hydrocephalus, but I was mistaken, for on inspecting the head, no mark of the disease could be found. In this case, as already said, there was no evidence of febrile contagion. The next two were instances of phthisis combined with typhus. One was John Mackie, *æt.* 16, Vol. xxv. p. 85, a scrofulous boy, and with phthisis, well marked during life, and after death the lungs found full of tubercles in all the different stages of their progress. The other was Thomas Gallocher, *æt.* 15, Vol. xxiv. p. 170, a more equivocal case, for we had no dissec-

tion, but he was said to have laboured under phthisical complaints before admission.

As for the two female cases, there was no evidence of febrile contagion in either. One, Mary Pettigrew, *æt.* 15, Vol. xxiv. p. 153, was an example of pectoral dropsy, with large effusion both into the cavity of the pericardium, and each side of the thorax. The other, Kate M'Donald, *æt.* 16, Vol. i. p. 134, though we could not trace infection, might have been a case of typhus. She was not admitted until the fourteenth day, when she was unable to articulate, and she died the third day afterwards. Nothing could be found on inspection, except a few red patches on the intestines.

Of all the above examples of disease, gentlemen, you will easily see that not more than two, or three, at most, can be fairly ascribed to genuine, uncomplicated typhus. Taking the fatal casualties at three, the deaths will be one in 86 and a fraction; estimating them at only two, which perhaps is nearer the truth, they will be no more than 1 in 130 and a fraction, a result nearly approaching to that contained in Dr. Barry's report.

As to the prognosis afforded by the different ages of life that exceed seventeen years, I can give you little or no information; we still want observations and facts on this subject. It would be necessary to have a prodigious number of tables drawn up containing the event of fever at all the different periods of existence, ere we could come to any satisfactory conclusion, and it is obvious, that with all our care, we must meet with a great number of counteracting circumstances that would be continually marring, and perplexing, the results. I have met with but few of such tables, and I shall merely lay before you one derived from the Fever Hospital at Belfast.

TABLE

Showing the number of persons of each age who were admitted, with the number of deaths upon each age, in periods of five years, with a calculation of mortality, as if one hundred persons of each age had been admitted. This table is

extracted from the Registry, between September, 1817, and May, 1820.

Age.	Admitted.	Died.	Per cent.
From 80 to 75	6	2	$33\frac{1}{3}$
70 to 65	11	2	$18\frac{2}{15}$
65 to 60	28	5	$17\frac{5}{7}$
60 to 55	60	8	$13\frac{1}{3}$
55 to 50	46	11	$23\frac{2}{3}$
50 to 45	108	16	$14\frac{2}{3}$
45 to 40	77	7	$9\frac{1}{11}$
40 to 35	222	27	$12\frac{1}{10}$
35 to 30	149	14	$9\frac{1}{10}$
30 to 25	296	24	$8\frac{4}{7}$
25 to 20	300	10	$3\frac{1}{3}$
20 to 15	475	17	$3\frac{1}{9}$
15 to 10	374	4	$1\frac{1}{18}$
10 to 5	271	3	$1\frac{2}{7}$
5 to 1	112	10	$8\frac{3}{4}$

From the above table, we would be led to conclude that the smallest proportion of deaths in typhus fever is to be met with between five and twenty-five years of age, when the deaths amount only to 1 in $41\frac{2}{7}$, or $2\frac{2}{7}$ per cent—from twenty-five to fifty, they are found to be 1 in $9\frac{1}{2}$, or $10\frac{2}{15}$ per cent.; while the maximum of mortality is from fifty to eighty, when the rate is 1 in $5\frac{1}{3}$, or $18\frac{2}{3}$ per cent.—*Annual Report, Fever Hospital, Belfast, ending 1st May, 1821, pp. 11, 12.*

I have subjoined a table of our own patients, showing the respective ages, number of admissions at each age, with the rate of mortality calculated both individually, and by average; and in other columns, the same circumstances according to decades of years. Among the males, the earliest age of admission was at three, among the females at five, while the oldest among the former was seventy-nine; among the latter sixty. As to casualty, the general result is, though not without exception, that after forty, as might be expected, the rate of mortality increases as we approach the close of life. I shall add here the portion of table containing the decades.

MALES.				FEMALES.			
Periods of 10 years.	No. of Patients.	Deaths.	Average.	Periods of 10 years.	No. of Patients.	Deaths.	Average.
1 to 10	68	1	1 in 68	1 to 10	25	0	0
10 to 20	277	8	1 in $34\frac{5}{8}$	10 to 20	131	8	1 in $16\frac{3}{8}$
20 to 30	243	27	1 in 9	20 to 30	129	10	1 in $12\frac{8}{10}$
30 to 40	127	30	1 in $4\frac{7}{10}$	30 to 40	63	8	1 in $7\frac{7}{8}$
40 to 50	53	15	1 in $3\frac{8}{15}$	40 to 50	18	5	1 in $3\frac{3}{5}$
50 to 60	23	9	1 in $2\frac{5}{9}$	50 to 60	7	3	1 in $2\frac{1}{3}$
60 to 70	3	1	1 in 3				
70 to 80	4	2	1 in 2				

I have now concluded, gentlemen, almost every thing I had to say on the subject of prognosis. I have, with this view, run over the principle symptoms of fever, and have endeavoured to ascertain what information they are qualified to supply concerning the event of the disease. But this analysis is not enough: we must consider these symptoms, as already said, not only separately, but collectively, or as they are linked together, or combined, by nature into the same distemper. If, after this survey, we find nothing very threatening in the general aspect, not more disturbance than seems inseparable from a state of fever, our leaning, of course, will be rather to hope than to fear. And here it must always be considered as a general rule, that the more synchronously or simultaneously the favourable signs present themselves, the better insured will be the safety of the patient. Thus, if the pulse come down, the skin offer nothing unnatural to the touch, the sordes, and other febrile conditions, of the tongue, disappear; while, at the same time, the appetite revives, and sleep returns, our anticipations of a happy event will be almost converted into certainty. Nor though some, and even material, symptoms survive the general convalescence, is there room for discouragement, or despondency. I have often known the pulse continue morbidly quick after the fever had entirely ceased, if I may say so without a paradox, and, on other occasions, the appetite remain feeble, and unrestored, for many days. These exceptions to the general state of recovery soon disappear of themselves, and they only show that the heart and stomach, in certain cases, are longer of disembarassing themselves from the poison of typhus, than the other organs.

We come now to the most important part of our subject—the cure, or more correctly speaking, the treatment of typhus.

Here I think it necessary to premise, that the directions I am, at present, to lay before you, relate solely and exclusively, to our low contagious typhus; how far they may be applicable to other forms of fever described by authors, I offer no opinion.

The treatment of low typhus naturally divides itself into two branches, one, comprehending the measures to be followed, at the commencement of the malady, the other, those that may be called for after it has been fairly formed.

Of the first set of measures the purpose is of the highest importance, no less than to strangle, or extinguish, the disease at its outset, and, of course, avert all future danger. Two plans chiefly have been resorted to with this view, one, the early administration of an emetic, and diaphoretic, the other, the shock of the cold affusion. With respect to the first, the process I have found most advantageous is the following. About eight o'clock in the evening, I order the emetic, generally ipecacuanha, after the operation of which, the patient is enjoined to bathe the feet and legs, in warm water, during at least a quarter of an hour, or twenty minutes, and upon retiring to bed, he is to swallow a large dose of Dover's powder. Sweating soon commences, and it is to be supported and encouraged, by tepid diluents. This method, when administered early enough, I have found singularly successful. My chief experience of it has been among the nurses of our Infirmary, when they happened to catch the contagion. With them, I have seen it, repeatedly, dissipate every symptom of fever. Upon visiting them next morning, I found, that after profuse sweating during the night, the pulse had come down, that the headach, with the pains of the back, and limbs, had vanished, that the tongue had regained its moisture, and that nothing remained but a little debility; in short, that all those threatening signs had disappeared, that, if left alone, would have soon matured themselves into a regular, and genuine typhus. It is almost needless to say, that this happy result is

most looked for only when we have the fever to deal with at its very commencement.*

* The young gentlemen who serve as clerks in our Fever Hospital rarely, if ever, escape the disease, and no wonder, considering the duty they have to perform. This duty consists first in taking a history of each case as it enters the house, an employment which causes them to linger long in near contact with each patient, and many such histories must often be taken the same day: then, they must regularly visit all the wards thrice daily, besides frequent additional calls, both by day and night, so that they live almost constantly in an atmosphere of contagion. Two of them who acted successively in this capacity for me, Messrs. Paxton and Howie, and to whose zeal in acquiring knowledge of their profession, humanity to the sick, and general good conduct, I have great pleasure in bearing this testimony, put the prophylactic plan above recommended to the test of experience in their own persons, and fortunately experienced its full benefit. Both of them warded off, by means of it, the fever that would otherwise have infallibly ensued; Mr. Paxton twice over, or on two several occasions. I subjoin the narrative of each of my young friends in his own words:—

“DEAR SIR,—On the 6th of August last I was seized, in the morning, with an unusual feeling of languor, and difficulty of directing my mind to my usual occupations; this was attended with considerable nausea, which continued the whole day, and I was unable to eat any thing at the usual meal times. Next morning, after having passed a very unquiet night, I awoke with severe head-ach, pain of back and calves of the legs, with a pulse of 108, and white tongue. I remained in bed the whole day, but at four o'clock P.M. I was seized with severe rigours, which continued for some time. A scruple of ipecacuanha taken immediately produced full vomiting. At night, after 15 grains of Dover's powder, preceded by a hot bath, I fell into a profuse perspiration, and next morning felt considerably relieved; my pulse had fallen to about 80, tongue still white. A calomel and jalap purge operated freely, and next day, with the exception of slight debility, all disagreeable symptoms had disappeared.

“I had a similar attack in December last, which seemed to have been arrested by the same treatment.—I am, dear Sir, your obedient humble servant,

“JOHN PAXTON.

“G. R. I. August 9th, 1832.”

“DEAR SIR,—According to your desire, I have noted down the particulars of the attack of fever which I had in May last, the progress of which, I conceive to have been arrested by the early exhibition of an emetic, along with hot bath and diaphoretic as auxiliaries.

“One night in the beginning of May, after having gone to bed, I was awoke out of sleep by a severe rigour affecting my whole body. This continued with unceasing violence for between fifteen and twenty minutes, when it was checked by a full dose of paregoric with hot gruel. I soon fell into a somewhat disturbed sleep, and awoke in the morning with severe throbbing frontal headach.

Another plan resorted to for eradicating typhus fever in its first stages, is by the shock of the cold affusion. This method, however, is more circumscribed in its use than the other, since it is available only when the skin is at once preternaturally hot, and free of moisture, a conjunction not often observable in our low typhus, even at the beginning. With these requisites, however, it is a most powerful remedy, and that it has at different times extinguished fever, is a fact established by the testimony of the most trust-worthy practitioners. Of this plan I have myself, little, or no, experience. I tried it twice only, but both times without success. Like our other expedients for shortening fever, it is too often delayed till the time for using it has elapsed, or till the disease is so firmly fixed as to defy its influence.

It thus appears that there exists in fever what may be called a *preventive* stage, a period during which it remains assailable by medicine, may be effectually checked in its career, or may be utterly extirpated. This important, but fleeting, era, however, for it is only of a few days' duration, is too generally suffered to pass away without advantage, unheeded, and unimproved. The precious opportunity it affords of averting the whole future calamities of the disease, is thus completely lost. The practitioner is seldom, or ever, consulted till the after stage, when it has become too firmly

Unable to take breakfast, I took ℞i. of the pulv. ipecac., which produced free vomiting. The headach continued more or less severe during the day, attended with dull pain of back and legs, with a hot and dry uneasy feeling about soles of feet. In the evening I took the hot bath, and afterwards gr. xv. of pulv. Dov., with plentiful exhibition of hot drinks. I passed a quiet night, and next morning I found all symptoms materially relieved, headach being nearly gone, and my skin, which had been previously hot and dry, now moist, and pleasantly cool. My pulse, which at the early period of the attack was upwards of 100, had now fallen to nearly the natural standard. Tongue still white. Next day, after a purge of castor oil, disagreeable symptoms had altogether disappeared, except some slight debility, which was, however, not so great as to prevent me from following my usual avocations.—I remain, dear Sir, with much respect, your very obedient servant,

“ ROBR. HOWIE.

“ G. R. I., 8th August, 1832.

“ RICHARD MILLAR, Esq., M.D.”

entrenched, to be dislodged, or when it has inseparably fastened itself upon its victim. It will then, too often, run its course in spite of every obstacle we can oppose, and this brings us to the second branch of our present subject, those remedial means which are left us, after this change, or after the malady has thus assumed what might be properly enough termed its Permanent Form, or condition. Before coming to details here, I think it necessary to premise the two following axioms, together with their two corresponding corollaries.

1. That typhus, after it has reached a certain stage, will proceed onwards in its course (a few rare instances excepted), in spite of every obstacle medicine has yet devised to check its career; but that, at the same time, it is part of its nature to decline spontaneously, to exhaust itself, or to wear itself out by degrees.

2. That fever is a disorder of considerable duration, and that while it endures, the whole powers of life lie prostrate before it, more especially appetite, digestion, and assimilation, so that often for ten, or even twenty days, little or no new blood can be created, or prepared, for the purposes of the circulation, while secretion, and excretion, going on continually, to greater, or less, extent, must act as a constant drain; and hence that on all these accounts, more or less languor, exhaustion or debility, must be looked for, sooner, or later, as an essential, or inseparable consequence, or concomitant, of the disease.

From the above two axioms flow the following two corollaries, as appears to me, alike founded on common sense.

1. That since it is beyond our power, generally speaking, to stop the current of the fever, little else will often remain for the practitioner than simply to lie by, and watch the symptoms, administering what aid he may to such as appear the more troublesome, or dangerous.

2. That, founding on the second axiom, he abstain from all inordinate depletion, more especially of blood, so as not to draw too largely on that fund of strength which ought to sustain his patient, and which seems requisite to bear him up through the usual contingencies, and exhaustion, of so protracted a disease.

Building on the soundness of these preliminaries, gentlemen, I proceed now to more particular directions. Let us suppose, then, we have a typhus patient presented to us, and let him be an hospital patient, as over him we have the most absolute control, and let us consider what measures are to be pursued. We commence with the following. His head is first to be shaved, and his clothes being stripped off, and immediately immersed in a tub of water, so as to be completely covered, and surmounted by the fluid, he is to be thoroughly washed and cleansed in the warm bath, and after being dried and furnished with clean body linen, he is finally to be put to bed within some large, comfortable, and well ventilated ward, or chamber. We then proceed to open his bowels, a precaution never to be neglected during any period of the disease, and, as he will be generally harassed more, or less, with flying pains of his limbs, and with broken rest, at night, we order a dose of Dover's powder at bedtime, both to remove his uneasy muscular feelings, and to conciliate sleep. Such, gentlemen, are the initiatory measures, or common routine, I have been long accustomed to pursue. If, after all this, at our next visit, there appear no untoward symptom; if the pulse be neither very rapid, nor very feeble; if there be neither unnatural heats, nor chills; if there be little headach, and no delirium; if, though the patient cannot eat, he neither purges nor vomits; if the pains of the back, and limbs, be only slight, or moderate; if sleep continue pretty regular, or be easily commanded by an opiate, I hold, that for the present, at least, little else is to be done. We may give the patient, from time to time, a little wine to relieve his languor, and it will more speedily recruit his lost strength, if it be blended with his food. Such, gentlemen, is the line of practice I would recommend to you, in all those that may be called the milder cases of typhus. Caution, and forbearance, are to be your leading maxims, here, in spite of all dashing practitioners, and all dashing books. Typhus is an enemy, you may rest assured, often to be most effectually opposed by the Fabian system of tactics, and he will be the most successful combatant, as well as best deserve the eulogy of

the Roman poet, who, while he remains at his post, ready for the encounter, yet prudently abstains from all ill timed, and unnecessary attacks.

————— Tu Maximus ille es,
Unus qui nobis cunctando restituis rem.

Dropping here, however, all figures of speech, gentlemen, I am happy to say that I have it in my power to lay before you the most complete practical proof of the truth, and propriety, of this precept. The treatment it recommends has not only been put to the test of experiment, but has been tried on a great scale, and however simple, has been crowned with the most perfect success. You will find by the journals that no less than 516 patients have been treated according to this plan, that is to say, in these cases nothing seemed to be required, and accordingly nothing was done; we simply lay by, and watched the disease as it proceeded in its course. The only exceptions were, that in 15 of the males, leeches were applied, and in 5 others, a blister; among the females again, neither leeches nor blisters were found necessary, while in the whole 516, setting aside these additions, the sole remedies employed were an occasional laxative, and an opiate at night. If success be the criterion of practice, the result has been most satisfactory, for *out of all this multitude, there was not so much as a single death.**

* Those who are fond of jokes against our profession will have room here for indulging their humour. They will say, and with some plausibility, that many among those to whom we administered numerous and various medicines died, while all those who took nothing, or next to nothing, recovered. The remark is undoubtedly true, and it shows that fever will often run itself out, and terminate favourably, with little or no assistance from drugs. Still, however, we must not give up the importance and utility of our art. The previous discipline of the warm bath and shaving the head, to which all our patients are subjected, I consider of no small avail for mitigating the symptoms, and still more the removal from their own filthy, and ill aired abodes, to a clean bed, and a well ventilated, and comfortable apartment. Neither must we forget the maxim, that there is often more skill displayed in withholding than in prescribing a medicine.

During the 16th, and 17th, centuries, the most violent disputes took place in France between the rival sects of the Chemists, and the Galenists, and they furnished a perpetual fund of jest, and merriment, to the wits of Paris during

But the course of typhus does not always run so smooth, or with so happy a termination, and occasions arise when your interference will become absolutely necessary. As every organ, and function, is implicated in fever, so symptoms occur in all, requiring assistance; as in the circulatory, or cerebral, systems; in the skin, or surface; in various viscera of the abdomen; occasionally, in those of the thorax. These aggravations are observed to occur, sometimes separately, more frequently, combined.

That they may be the more distinctly understood, it will be best to consider them in detail. To begin with the skin, or surface. Preternatural, or morbid, heat of the skin is so usual an attendant that it has been, generally, reckoned one of the pathognomonic signs of fever; it is one also, often, very troublesome, and distressing, robbing the patient of rest, by day, and of sleep, by night, and even accelerating the pulse. It is, luckily, however, not a very urgent, or frequent, evil, in our common low typhus. When it does occur, it is easily obviated by sponging with cold water, and vinegar, an expedient of which the benefit is so generally known, that it is often ordered by nurses themselves, without waiting for the physician. Nothing can exceed the refreshment, and relief,

the latter period, or the age of Louis XIV., more especially two of them, Moliere and Le Sage. The author of *Gil Blas* has consigned the Galenists to everlasting ridicule, in the person of Doctor Sangrado, who is made to cure all diseases by a deluge of tepid diluents, under the shape of warm water, together with unbounded blood-letting, and, in this last respect, the character might be studied, not without profit, by some of our modern inflammationists. Another more sly, though not less effectual, hit, against physic and physicians, in general, will be found in the following passage of the same admirable romance: "Nous poussâmes gaiement jusqu' a Bunol, ou par malheur il fallut nous arrêter. Dom Alphonse tomba malade. Se lui prit une grosse fièvre avec des redoublemens, qui me firent craindre pour sa vie. *Heureusement il n'y avoit point—la les medecins, et j'en fus quitte pour la peur.* Il se trouva hors de danger au bout de trois jours, et mes soins acheverent de le retablir." It is amusing to observe the vicissitudes of practice. Sangrado's plans we have seen in our own days revived, for the cure of Cholera, with this difference, however, that he and his pupils used to throw their warm liquor into the stomach, and without any admixture of salt, whereas the Cholera doctors first let out the red blood by the lancet, and then by means of a syringe inserted into some of the veins, fill up the vacancy with brine.

of the practice. It spreads an agreeable coolness over the whole surface. Under its soothing influence, restlessness ceases, sleep returns; and even the pulse itself abates of its irritation, and velocity.

A more constant attendant of our low typhus, one, indeed, I may safely say, never wanting, consists of certain uneasy feelings in the superficial muscles of the body, more especially of the back, and limbs, simulating, or apeing, rheumatism. In the phraseology of patients, they are commonly styled pains of the bones. Diaphoretics are a sovereign remedy. The one I commonly employ is Dover's powder, in the dose of fifteen grains, at bedtime, which has the farther advantage of conciliating sleep. Next day, should there be any remain of the symptom, it may be speedily dissipated by increasing the dose of the medicine to ʒi , and preceding it by pediluvium. Instead of Dover's powder, you may order with similar benefit, a draught containing thirty drops of laudanum, with fifteen of antimonial wine. The success of either method will be found, nearly, infallible. I speak greatly within bounds, when I say I have used it with success in more than a thousand instances. Sometimes we have pain of the back alone to contend with, more especially in the primary stages. The best remedy I have found to be, repeated friction with oil of turpentine, and then covering up the part with a piece of doubled flannel.

I have already mentioned the baneful effects produced by typhus poison on the nervous tissue, more especially acting on its grand centre the brain, and on these the physician must ever keep a most watchful eye.

There are two symptoms, from this source chiefly, we are called to encounter, headach, and delirium. These, when conjoined, as they sometimes are, require exactly the same treatment with delirium by itself. As the two affections, however, generally exist separately, it will be more convenient to consider them apart.

Headach is of every degree of intensity, from slight uneasiness, to the most excruciating pain, but it partakes more of the former, than of the latter, character. Generally speaking,

it is not a very obstinate symptom. Our common remedy is an embrocation of vinegar, and water, and it will succeed eight times, out of ten. In more stubborn cases, you must substitute some lotion still more cooling, or more quickly evaporating, as tinct. of camphor, or sulphuric ether, in which pieces of cloth are to be drenched, and then, assiduously, applied to the head. If the symptom still resist, take a towel as folded up for the napery press, and after soaking it well in the coldest water you can procure, or in ice water, lay it on the scalp, resting one extremity on the nape of the neck, while the other terminates on the forehead, just above the eyes.* If all this fail, which seldom happens, apply leeches, and follow them up by a blister. You must beware here of a notion often inculcated, that headach in typhus always implies inflammation, either of the brain, or of its membranes. No opinion can be more unfounded, and I have repeatedly seen the practice resulting from it attended by the most pernicious consequences.

Delirium, as it is a rarer, so it is a more dangerous, occurrence. Like headach, it is of all degrees of constancy, and intenseness, as stated formerly, and its hallucinations admit every sort of variety. The period of the disease when it occurs, the attendant symptoms, more especially the state of the pulse, will show the proper mode of treatment. If met with early in the distemper, with injected conjunctiva, contracted pupil, and firm arterial beat, general blood-letting must be resorted to, and the blood will be most advantageously taken from the temporal artery, or jugular vein. If on the contrary the circulation be found feeble, leeches must be substituted for the lancet. In more advanced stages of the fever this last mode of detracting blood will be always preferable, and it may be assisted by the same cooling, and evaporating, lotions, recommended for headach. These failing, the partial affusion called Douche by the French may be tried, that is pouring cold water from a height on the vertex, and increasing its power by confining the stream within a tube of greater,

* It is needless to say, that this application, to be effectual, must be continued for some time, at least a quarter of an hour, or twenty minutes.

or lesser, diameter, or bladders filled with ice or snow may be applied over the whole head.* In the mean time internal medicines must not be neglected. A great variety has been tried. Those that have succeeded best with me are camphor and prussic acid. By large doses of each, I have repeatedly put a check to delirium. In cases where the transport is furious, and the patient insists on quitting his bed, every mode of soothing should be first tried, ere we resort to coercion. If this be neglected, and the strait waistcoat, all at once, forcibly put on, anger is added to the already irritated state of the feelings—the sum of excitement is greatly aggravated, and the danger of the succeeding collapse will be augmented in proportion. Binding the sick down in the bed, therefore, is always to be avoided, at least whenever it can be omitted with safety. Here I must warn you, as before concerning head-ach, against the opinion set up by various authors, that delirium in fever necessarily proceeds from an inflamed condition of the brain.† That such a disease as violent or acute phrenitis or meningitis exists, there can be no doubt, but it is seldom that either attaches itself to our low fever. The delirium we are called to treat partakes more of the typhoid, than synochoid character, and is the offspring rather of an asthenic, than a sthenic, state, of cerebral circulation. This opinion receives strong confirmation from what has been observed of the same symptom, as affecting patients, in hospitals for the insane. The practitioners there find their account in abstaining from all profuse evacuations of blood, which they tell us not only fail in relieving, but absolutely aggravate the disease, sinking the patient into incurable idiocy, or even, not unfrequently, leading to a fatal termination. This they assure us happens even in what are called the *high* cases, where the delirium is furious, and accompanied apparently with great corporeal strength. The nature of the disease is farther confirmed

* To these external applications may be added pediluvium, or if you do not choose to bring your patient out of bed, fomenting his feet, and legs, with warm water. This measure has often a soothing effect, both in headach and delirium.

† See Clinical Remarks on Blood-letting in Injuries of the head, by Mr. Cæsar Hawkins of St. George's Hospital.—Lond. Med. Gaz. for Aug. 25th, 1832.

by the description of the regimen they find most advantageous for the patients liable to such attacks; the diet enjoined instead of being low, is, on the contrary, always generous and invigorating.

To show how little aberration of mind is connected with inflammation within the cranium, we have another example in the curious species of delirium, called delirium tremens. Every body who has seen this disease, knows how ill it agrees with large depletions of blood. Nothing is more dangerous than such practice. I knew two instances where the patients sunk under it, in consequence of their cases having been mistaken for apoplexy. The opinion which maintains delirium in fevers to be always the product of inflamed brain, has been founded chiefly on appearances after dissection. The fallacy of the evidence from this source will be distinctly shown to you afterwards.*

* Numerous instances of asthenic delirium might be given, where of course the lancet is improper, as that from starvation, that among puerperal women from flooding, and other sources of exhaustion, and which last one of our most eminent accoucheurs, Gooch, tells us he is accustomed to cure by tonics and nourishing diet.

In the Croonian Lecture read before the Royal College of Physicians, London, May, 1831, by Dr. Seymour, Physician to St. George's Hospital, that practitioner, on the subject of delirium, observes, "In the great majority of cases the functions of the brain in mental derangement are increased in force, while the circulation is depressed, extremely quick, and feeble, and the action of the heart giving way at the smallest extraction of blood, and yet these are often attended with raving delirium, great increase of muscular force, and are in fact what are termed high cases. The consequence, I am informed, of such practice, is either the more frequent return of the high stage, or the patient sinks into one approaching fatuity." *Med. Gaz.* No. 204, p. 111. In like circumstances blood-letting was reprobated by Pinel, (from his experience in the Bicetre,) and by Haslam in Bethlehm. Similar testimony is afforded by Mr. Beverly, Superintendent, and Mr. Phillips, resident Surgeon in Mr. Warburton's Asylum called the White House in Bethnal Green: these gentlemen state, "we seldom or ever use the lancet in cases of excitement, if there is no evident effect upon the brain from increased cerebral action so as to lead us to fear an approaching attack of apoplexy or paralysis. The reason why we do not use the lancet in cases without any such symptoms existing of disease going on in the brain, is that we have done so in several instances, and the result was not favourable: the patient became reduced from the loss of blood, and the excitement not abated, the powers of the constitution gave way, the tongue became typhoid, and the patient sunk into a state of collapse and died." *Ib.* See farther at bottom of

Among other symptoms originating from febrile disorder of the common sensory, none of the least is the privation, or disturbance, of sleep, and we are now to consider what remedies are available against this common evil of fever. For the nocturnal repose we are accustomed to enjoy, we seem chiefly indebted to that state of exhausted excitability, or torpor, which is produced, during the day, by the various and unceasing impressions of the external senses, the fatigue of our voluntary muscles, and the exhaustion produced by the ordinary affairs, or occupations, of life, joined to habit, and exemption from any particular pain or uneasiness; but of these the influence is either lost, or suspended, during typhus, and hence wakefulness continues, when rest should supervene. We fly for aid to the medicines called narcotic; of all these, by far the principal is opium, a drug at once anodyne, and hypnotic, or comprising in itself the two requisites we desire, the power of abating pain, as well as inducing that state of exhausted excitability, or torpor, which may be said to constitute, if not the essence, at least the prelude, of sleep. The utility of such a medicine cannot but be apparent. It is, indeed, a remedy of the highest importance, and, from the most ample experience, I am now so thoroughly convinced of its benefit, that, generally speaking, I would no more omit a dose of it at bedtime, than I would neglect opening the bowels of my patient by a cathartic. I say this advisedly, gentlemen, and I say it strongly, because I have observed some late writers on fever too much disposed to under-rate the virtues of this valuable article. Those who have prescribed it on a great scale, as in hospitals, will form a juster estimate of its powers, and I have often been struck by the testimony borne in its favour, by the poor inmates themselves of our fever wards, though in

p. 118. Were farther authority requisite to discountenance blood-letting as an ordinary remedy in delirium, I might cite that of Dr. Carmichael Smith, who says he has seen losses of blood bring on the affection, both in cynanche maligna and typhus: Description of Jail Distemper among Spanish Prisoners; and that of Dr. Perceval of Dublin, who quotes Sir John Pringle as being of the same opinion. Irish Med. Trans. Vol. i. pp. 325, 326.

their own homely phrase, as well as pleased by the thankfulness they expressed for the relief it had afforded.*

As to the form of the medicine, it is of little consequence. When want of sleep is complicated with pains of the limbs, I prefer Dover's powder, or a mixture of laudanum with antimonial wine—if no such complication exist, I order laudanum by itself. Should there be any particular squeamishness, I prescribe opium in a solid form, say one grain, well imbued with some cajeput, or other aromatic, oil. If harassing, or troublesome, dreams occur, a plentiful dose either of opium, or laudanum, by soothing the irritated state of the imagination, and memory, so common a phenomenon in the sleep of fever patients, will often, as I have found by experience, remove the annoyance.

It is a common accusation against opium, that, under its more ordinary forms, it is apt to disagree with various individuals: in such instances, you may try the black drop, Battley's sedative liquor, or solutions of the drug made directly in the citric, or acetic, acids. In all this, however, I believe there is often a great deal of fancy. Many persons consider opium, in every shape, to be no better than a poison, and conceive whatever new feelings they experience from it as of the direst import: but if you persuade them to swallow it under a different name, so that they do not know what they are taking, they will then suffer no disturbance, and will be found ready enough to admit its benefit. Instances of this prejudice, or perversion of judgment, you will learn from every practitioner. If, in reality, the drug disagree, we must then resort to other narcotics, as henbane, belladonna, stramonium; but, for the most part, these substitutes seldom answer our purpose. I have, occasionally, conciliated sleep in fever, by large doses of hop, both in tincture, and extract,

* I cannot convey a better idea of this gratitude, than by repeating what often passed, when I questioned them, particularly on the day after admission, how they rested the preceding night; the answer has, innumerable times, been, "never better, Sir; it is the only night I have had any sleep since I took it,"—the fever.

but, then, I have failed with the same medicines far oftener, than I have succeeded. Should rest be prevented by any local affection, recourse must be had to local remedies. Should it be impeded by mental suffering, the mind of the sick, so far as possible, is to be soothed. The warm bath often exerts a powerful effect in producing repose, when the patient is strong enough to bear it; at any rate, pediluvium, or fomentations to the lower limbs, are almost always admissible.

Affections of the thoracic viscera are for the most part rare in typhus, provided they have been sound previous to the attack of the disease. Sometimes in cold weather we observe slight catarrh, with the usual discharge from the mucous membrane of the air tubes, nose, and fauces, but they yield readily to the common remedies. If there be pain of the chest, a few leeches will generally subdue it, or if more obstinate, it will be removed by a subsequent sinapism, or blister. To quiet the cough, I generally order the mucilaginous mixture of the House, containing the usual proportion of syrup of squill, and laudanum. Provided the lungs be not seriously affected, or have not been previously diseased, these measures, in most cases, will be found sufficient. It undoubtedly happens, however, that more serious pectoral disease is occasionally blended with typhus, in the same person, and of all others most frequently perhaps peripneumony and phthisis; and the question occurs how far the additional maladies can be said to be affected, or modified by the union. With respect to peripneumony it will be afterwards shown you that it is completely altered or neutralized by this conjunction, losing more or less of the inflammatory, and putting on more or less of the typhoid character, so that the complex disorder by no means requires the same extent of depletion, more especially of blood that would be absolutely necessary did the pectoral distemper exist isolated, or alone. This is now the settled opinion of the most experienced physicians. Even should peripneumony occur alone in a fever hospital, uncomplicated with typhus, of which we had some instances in our own wards, there must be some limit to venesection.

In the mind of the prudent practitioner, the apprehension

will always occur, and will have more or less of weight, that were the patient too much reduced, it is possible that by long exposure to the contagious hospital atmosphere, there might be at last conversion of disease, and that the typhus poison by weakening the powers of life, might materially accelerate that pulmonic disorganization and effusion well known so often to constitute the fatal termination of peripneumony.*

In the list of pectoral maladies joined to typhus, we had two of disorganized heart, and three of hydrothorax, with lungs more or less hepatized, or otherwise obstructed. Of the heart cases, one was of hypertrophy and dilatation joined of right ventricle, accompanied by such diminished size of the aortic valves as to render them incapable of performing their function. The other was of a more singular nature. We found the pericardium adhering to the heart all round,

* We have among our deaths 3 cases of pneumonia in union with typhus, the particulars of which will be given afterwards.—Grattan relates 7 instances of pulmonary effusion. His pathology is the following: "This state of lungs," he says, "though commonly the consequence of inflammation, is not essentially inflammatory: it does not possess any decided and strongly marked character of inflammation, nor can it be successfully treated by those means which are known to be the most powerful in subduing active inflammation. In this state of the lungs the bronchial cells are filled with mucus or phlegm, poured into them by the secretory and exhalant vessels of the inner membrane of the trachea, throughout its various ramifications: or this state may, and does sometimes depend on an engorgement, or tumid condition of the sanguineous capillaries, which from paralysis become inactive, and hence unable to propel the blood, are distended so as to compress the bronchial cells, thus preventing the admission of air so necessary to perfect the process of sanguification." General blood-letting he considers useless, nay, if large, will make the effusion go on more rapidly; active purges are alike inadmissible. The remedies he chiefly recommends are expectorants, with the inhalation of the vapour of vinegar, of water, or sulphuric ether; of course blisters were not omitted. He tried also calomel and ipecacuanha, mixed together in minute quantity. *Irish Med. Trans.* Vol. iii. pp. 420, 421, 422. Another Irish physician, O'Brien, regards effusion to be a rapid sequela of pneumonia in weakly people, and in such dissuades from blood-letting. *Ib.* Vol. ii. p. 465. Among our deaths we have 3 instances of hydrothorax in complication with typhus; in our cases of pneumonia and phthisis it was almost constantly a concomitant occurrence. Thoracic effusion in typhus, is by no means easy of diagnosis, because it is never, so far as I have seen, accompanied by any other species of dropsy, as ascites or anasarca, which so frequently attend it in cases unconnected with contagious fever.

so that the cavity was totally obliterated. The bronchial tubes were loaded with mucus, and the membrane more vascular than natural. At the time of admission, the pulse of this patient, a female, was almost extinct, and she died on the second day. In one of the cases of hydrothorax, the heart was enlarged, and flabby.

In the conjunction of fever with phthisis few new remedies are requisite except such as may serve to alleviate the cough. How far the two diseases are capable of being blended together, I am not prepared to say, but should a phthisical person be much and long exposed to the contagion of typhus, I know of no provision of nature by which he is to be shielded against the disorder more than any other individual. The new distemper here must evidently tend to aggravate the old. Again, should fever attack a person with tubercles of the lungs, but not yet developed, there is great risk that the disturbance will force them into activity, so as to produce decided phthisis. I have seen various instances of this occurrence, so far as could be judged from the previous history of patients, so that in this point of view typhus fever might be ranked among the numerous causes that give origin to pulmonary consumption. Among our deaths are seven cases of phthisis, incorporated with typhus.

But the abdominal viscera suffer more frequently from fever than the lungs. In one of the epidemics that prevailed some years ago, there was hardly a patient admitted during the autumn, and winter, months, who did not complain of severe gastrodyne, or pain of the stomach. It generally yielded, however, to a few leeches, a sinapism, or a blister. Instead of pain, sometimes a sense of cold was experienced in the epigastric region, and this I considered the more dangerous symptom of the two. The best remedy was external heat to the part, and it was easily conveyed by means of a bladder half filled with warm water. Small doses of heated wine, or spirit, were found useful auxiliaries. Vomiting, and nausea, are more common occurrences. The remedy I usually employ, and seldom without success, is the effervescing draught, assisted, if need be, by a little laudanum, or what is better,

if the stomach be very ticklish, half a grain, or a grain of opium, both so small in bulk as hardly to communicate any mechanical irritation to the viscus. But these symptoms seldom occur, or prove troublesome, if due attention have been paid to the bowels. A much more unmanageable, though rarer, symptom, is hiccup. Against this spasm almost a host of medicines have been prescribed, a clear proof of its intractability, such as the whole tribe of antispasmodics, the mineral acids, vinegar in large doses, &c., and I have tried most of them in vain. If I suspect acrimonious matter in the first passages, I begin with a gentle emetic. Besides the evacuation, the preceding nausea may be of service in resolving the spasm, and upon the same principle I would recommend small, and repeated, doses, of tartar emetic. Considering how much hiccup resembles retching, and how beneficial the effervescing draught has been found in this last, I have often prescribed it in the former affection, but seldom with complete success. The remedies that succeeded best with me have been frequently repeated small doses of sulphuric ether, aided by a sinapism, or blister, to the pit of the stomach.

Of the intestinal ailments that occur in fever, the most common is diarrhœa, or looseness. If you suspect acrimony, a slight purge will be beneficial; if after due evacuation, the disorder continue, it is to be checked by astringents and opium.

There is to be met with in fever, also, another species of diarrhœa of far more serious import than the common one; indeed, that sooner, or later, seldom fails to prove fatal. It is of a chronic nature, and appears by dissection, to take its origin from a number of small ulcers in the mucous membrane of the intestines. It is no less insidious than dangerous. We do not know when it commences, nor have we any certain mark of its existence, so far at least as pain of the abdomen is concerned, for though you apply considerable pressure to the belly, the patient neither shrinks, nor utters complaint, nor exhibits any other sign of uneasiness. We can only guess at its presence by the length of its duration, and

its resistance to ordinary remedies. Should you detect it at its commencement, I would advise leeches followed up by large doses of opium. During its latter stages, almost the only period when we recognise it, you must not suppose that it has any thing to do with inflammation. During the complete breaking down of all the powers of life that accompanies the closing scenes of phthisis, I have repeatedly seen the same species of diarrhœa prevail, and have repeatedly by dissection traced it to the same cause, extensive ulceration in the villous coat of the intestines. No man, surely, would employ blood-letting here, unless he wished to precipitate the fate of his patient. For the origin of this affection in fever, I am unable to assign a cause. I have met with it chiefly in neglected cases, or where from poverty, or other circumstances, the accommodations suited to the prosperous issue of the disease have been nearly, or entirely, wanting. I have sometimes thought it might have derived its source from the excessive use of acrid purgatives.*

The last affection of the intestines in fever, I am to notice, is hæmorrhage. I have already explained to you the danger of this symptom, when it occurs in advanced stages of the disease. It may proceed either from rupture of vessels, or exudation through their sides. The last I would regard as the worse evil of the two, since it shows so total a loss of tone in the coats of the arteries and veins—a circumstance I consider more to be dreaded than mere loss of blood. Our remedies must be the same as in other hæmorrhages, the application of cold, absolute rest, a condition, indeed, the weakened state of the sick, for the most part, effectually secures, astringents joined to opium, and the cautious use of wine. Unhappily, however, in most instances, all remedial means fail us, and the patient sinks under the symptom, which luckily is not very common in our ordinary typhus.

* Sometimes these ulcers eat through the whole of the intestinal coats, so that their contents find their way into the cavity of the abdomen. This, it is needless to say, is always a fatal occurrence. We had one case of this description.— Sometimes again we have typhus superadded to an existing peritonitis. In our dissections we met with two examples of this conjunction.

Occasionally we find typhus superadded to pre-existing affections of the liver and spleen, and there is no reason why persons labouring under such affections, should be exempted from the influence of the poison. So far as my experience goes, the union of low fever, with liver disease, is always fatal. As for the spleen, we know so little of its functions, that it is difficult to say what share it may have in the patient's death. The usual mode of its disorganization is excessive softness; we find it often no firmer than as much coagulated blood.

But of all the functions that suffer from typhus, none are more deeply, or severely implicated than that of the circulation, and there is none, accordingly, that calls more seriously for the attention, and aid, of the practitioner; of the state of this function we judge, of course, by the pulse. It was formerly stated how essential an element it formed of prognosis, it would be useless to remark that it must be of still greater importance as a guide to practice, nor is there any circumstance about our patient we are more anxious to explore than the beat of his artery at the wrist.

It does not follow, however, that on all occasions, interference is necessary. There will, no doubt, in every instance, be more or less disturbance of the heart and arteries, but if it do not exceed in amount what seems inseparable from the febrile state, we hold it sufficient simply to watch the patient, trusting that the disorder of the circulation may gradually subside, or wear itself out, like the other symptoms. But we are not always to look for such mild cases. In too many instances the pulse flags, becomes either immoderately quick, or alarmingly feeble, for the most part both at once, and we must then endeavour not only to restore its energy for the present, but to counteract its weakness, or tendency to sink, for the future. While on this subject, I must not forget again to warn you against the common opinion that every case of contagious typhus must always necessarily commence as a synochus, and must, on that account, be treated, at first, as an inflammatory disease. That such contingency may occasionally exist, I do not deny, but I hold it of rare occurrence,

and unless there appear some unequivocal sign of inflammation, either in the head, chest, or abdomen, whatever the pulse may be, you must carefully beware of all inordinate depletion, more especially of blood. Only have a little patience, and you will soon see the benefit of this caution. Merely watch your patient for a day or two, and you will find the hard feeling of the artery, you so much dreaded, gradually disappear, and to be succeeded by the usual typhoid beat.

The leaning of the disease, in fact, is all on the other side, and the danger we apprehend, except in very rare instances, is not from morbid excitement, or inflammatory vigour, but from collapse, or excessive weakness. It is against this last condition accordingly that we direct our efforts. The means thus resorted to by practitioners, for counteracting the debility of fevers, have been very various, at different times. Among the old Boerhaavians who had a prejudice against the bark, *contrayerva*, and *serpentary*, were the principal remedies. Others administered *cinchona*, at first by itself, afterwards with wine, but both the compound, and its first ingredient, fell into disuse, though the employment of bark has again been revived, of late years, under the more effectual, and convenient, form, of sulphate of quinine. Camphor has been a favourite of many. *Cascarilla*, *angustura* and *canella alba*, have been occasionally prescribed, under the impression, that from their union of bitter, and aroma, they might be of the same efficacy with the old *serpentary*, and *contrayerva*. I have, repeatedly, tried them all, and have found them of sufficient utility. Indeed, I have no doubt, that any one of them will often conduct the disease to a prosperous termination. For many years back, however, I have laid them all aside, and trusted entirely, or almost entirely, to the use of wine. This remedy, in fact, possesses exactly the two powers necessary for correcting the pulse in typhus; restoring its strength in the mean time, and as a prophylactic, preventing it from sinking afterwards. It, besides, possesses conveniences, and advantages, that other febrifuges want. Thus, it hardly ever disagrees with the stomach, a circumstance of

great moment in a disease like fever, where the organ is so apt to be ticklish, or irritable. But its grand qualification consists in this, that you can derive from it every degree of stimulation you please, from the highest, to the lowest, so as to command every grade of excitement you may deem requisite for your patient, and that, merely, by adjusting the doses, and abridging, or prolonging, the intervals of its exhibition. We have thus put into our hands not only a most powerful, but, at the same time, a most manageable instrument against fever. As to the mode of administration, I shall merely mention the plan I have long followed myself, and which, by experience, I have found the most advantageous. Unless great debility occur early in the disease I rarely prescribe it till the eighth, ninth, or tenth day, and then order it only in small quantities, and at considerable intervals, as half a glass or one ounce, every three hours; and I may here remark, once for all, that it must be managed in such a manner, that the succeeding may always support the influence, or impression, of the preceding, dose. Should the artery assume a firmer beat, though still feebler than that of health, I make no augmentation, but proceed with the same allowance. If the pulse, on the contrary, continue too feeble, I increase the quantity, or shorten the interval of exhibition, ordering a glass every two hours, or half a glass every hour, or a whole glass every hour. For the most part, however, I rarely exceed twelve glasses, or an ordinary bottle, during the day and night. I must here warn you against a circumstance that might otherwise mislead you, it is this: it not unfrequently happens that when wine is taken in these large doses, though it invigorates the pulse to a certain extent, yet it does not raise it enough, and you might be apt to suppose that the medicine is given in vain. This, however, is not the case. It is keeping the disease at bay, and you have only to persevere, when, in a day or two, you will reap every benefit you desire. Occasionally, however, wine even in considerable quantities, affords too weak a stimulant, and its power must be assisted by the addition of alcohol, under a stronger, or purer, form, as of our ordinary spirits, rum, whiskey, or brandy.

Half a glass of either may be added to each, or every second dose, of port, sherry, or madeira. If still the pulse flag, join sulphuric ether, as ten or fifteen drops, or more, to each exhibition of the other remedies; or interpose this powerful diffusible during the intervals. Should wine produce acidity, or otherwise disagree, spirits must be substituted in its room, and it is needless to add, that every expedient must be tried to render whatever liquor you use agreeable to the stomach and palate. If possible too, or unless nausea forbid, some light aliment should be joined to your remedies, as beef tea, calf foot jelly, or panada, with the addition of aromatics.

Such, generally, will be found the most advantageous method of employing the powerful diffusibles, vinous, or spirituous, in the cure of typhus. I am aware that they have often been administered in far larger doses than those mentioned, but such are extreme cases, and I here restrict myself to their more ordinary modes of exhibition.

Before I quit this subject, I must not forget to tell you, that there are certain symptoms in low fever, according to many practitioners, which forbid the use of wine, and render it either useless, or hurtful. What are esteemed the principal among these are a hot skin and dry tongue. There will be found here much fallacious reasoning among authors. If it be merely asserted that a fever is more difficult of cure with these symptoms than without them, the fact may be readily admitted, but they do not necessarily contra-indicate the employment of wine. If the surface be moist as well as hot, the remedy is perfectly admissible, as I can assert from frequent trials, and if it be hot without being moist, the obstacle may be easily removed by sponging. But, in fact, a hot skin is rarely an accompaniment of our low typhus. With respect to dry tongue, I would rather it were moist, yet I give wine every day of my life notwithstanding the circumstance, should the pulse demand it, not only without evil or aggravation of the disease, but with the most decided benefit to the sick. Cases often occur, indeed, in which there is no choice. If you find your patient living supine, passing his stools and urine in bed, and the pulse all but extinguished at the wrist,

you will not do your duty unless you endeavour to restore, and support, him, by the plentiful use of the most powerful diffusibles, be the state of the tongue, and the skin, what it may.

Among the London practitioners, ammonia, and the camphor mixture, separate, or conjoined, are favourite medicines, and without doubt they are of considerable efficacy. The first named article has this advantage, that it determines to the skin, an influence always salutary in fever, but its stimulation is less permanent than that of wine. The best forms, I think, are either the simple or alcoholic solution, or that joined to some of the volatile aromatic oils, both of course to be administered in small, but repeated, doses. Though in the above remarks a very high estimate has been given you of the febrifuge virtues of wine, you must not therefore suppose that I regard it as an absolute panacea, or that every person must recover, provided, it be used in sufficient quantity. I trust I need not warn you against so gross an absurdity. Though much, no doubt, may be done for typhus by early and judicious management, yet universal immunity from its ravages can never be expected. Like every other epidemic, it must always have more, or fewer, victims. Such is the baneful potency of typhus poison, over the vital organs, more especially, it is probable, over the universal nervous tissue, that in many instances, prescribe as you may, no remedial means will avert the fatal event. Hence an invidious charge against wine, by saying that many of those die who have used it the most liberally. The fact is without question true, but the inference does not follow from the fact. No argument, indeed, can be more futile, since the same accusation might be brought, with equal strength, against every remedy our art can boast, even those of the most tried and acknowledged efficacy. But is a medicine to be reckoned useless, merely, because it is not omnipotent? Are copious blood-lettings, for example, with other depletions, and the antiphlogistic regimen, to be stigmatized as of no avail in inflammation, because every sthenic disease does not immediately fly before them, or because many persons are known to die under their

administration? The same reasoning precisely is applicable to wine in typhus. The extent to which we are obliged to carry the stimulant plan in the one instance, and the depleting system in the other, merely shows the violence, and danger, of the disorder against which we have to contend. The amount of the whole is what may be readily conceded, that the best directed efforts of our art are too often nugatory, and that diseases are not unfrequently, stronger than our remedies.

By far the most dangerous symptom in typhus, as I conceive, gentlemen, is delirium and other brain affections, and it is here you will find our ordinary remedies most frequently to fail, among the rest wine. There is no degree of feebleness in fever where I would despair with this medicine for my aid, provided the sensory were free, or only slightly implicated. In opposite circumstances, I am never without my fears. Nevertheless you have seen the success of wine in many instances where feeble pulse and the best marked cerebral symptoms were conjoined, not only common delirium, but the deeper lesion indicated by passing the urine and stools in bed. In counting the cases of this description we treated, and treated successfully with this remedy, not all of them certainly presenting the last named appearance, I find them amount to no less than 119, a fair enough sample of what is to be accomplished in these cases by the diffusible stimulants. At the same time you cannot but have observed that in other instances apparently exactly similar, this method failed utterly; and to this circumstance I wish to call your attention, because it is of great importance. If you ask me why there should be such different results in cases so like in their symptoms as not to be distinguishable from each other, I frankly confess that I cannot tell. I have often tried to solve the difficulty, and to discover the reason why the same means with which I had saved so many patients, should be entirely unavailing with others, but I have always failed in the research. In fact, I have nothing here to offer you but conjecture. It may be supposed that there is something in the poison of typhus that produces the same effect on the brain as what results from

what we call concussion of the same viscus, from external injury, a change the fatality of which is neither indicated by precise symptoms during life, nor by dissection after death. Some more fortunate practitioner, it is to be hoped, may, some day, detect the cause, and find out the remedy.

I am quite aware of what has been said for the efficacy of general blood-letting for delirium and other cerebral affection in fevers. It is only serviceable, however, at the beginning, a period of the disease which we have not had many opportunities of seeing, and one of its strongest advocates, Dr. Clutterbuck, who considers typhus as nothing more than inflammation of the brain and its membranes, yet allows that if the disorder be far advanced, so that the cerebral affection has been fully developed and matured, the most profuse evacuations of blood are entirely useless—a conclusion in which, so far as my experience goes, I completely concur.

I have now finished, gentlemen, the directions that seemed requisite for the remedial management of typhus. They apply chiefly to the collapse, or latter stages, of the disease. Should the same symptoms occur earlier, it is superfluous to remark, that a similar plan of treatment must be pursued.

As to the food, or drink, of patients in fever, or what may be called the dietetic regimen, little need be said. Every body knows that thirst is an almost inseparable companion of this disease, and the fluids we allow, are various, as water gruel, milk and water, small beer, butter-milk, imperial, &c. &c., and unless some particular circumstance forbid, the choice may be safely left to the patient. Ripe fruits afford an agreeable variety among the liquids the sick swallow so greedily. If the tongue, or mouth be chapped, currant jelly, particularly black, or rose gargle, will be at once grateful, and beneficial.

With respect to food, still fewer directions are necessary. So totally is the appetite extinguished that little for the most part is desired. Panada, or beef tea, may be occasionally offered, and except in cases of delirium, there is hardly any risk that they will be taken in such quantities as to prove injurious. Greater danger occurs after convalescence, more

especially in the houses of the opulent, where dainty viands may tempt to excess. There will then be produced what is called *crapula*, which, however, will readily yield to a vomit or a purge, and must be guarded against by future temperance. In these cases the patient is often said to have relapsed again into fever, but this is a mistake; the disease is not fever, but *dyspepsia*. In hospitals there is little dread of such accidents. The scantiness, and plainness, of the fare, will, for the most part, effectually exempt the convalescent from surfeit, or over-eating.

In our fever wards, accordingly, I have seldom thought it worth while to enjoin any dietetic restrictions, and among the whole number of patients I have treated, from first to last, in the Infirmary, I do not recollect to have met with above three or four instances of *crapula*.

With respect to other rules of convalescence it is needless to detain you, your own good sense will suggest every thing requisite. Convalescents it is obvious should live in a well warmed, and airy chamber, and enjoy the same comforts as during the disease. Almost the only precaution necessary is to avoid premature bodily exertion, and too early an exposure to cold.

II. We come now to the second, or controversial, branch of our subject, and the first topic that presents itself for discussion is the dogma of *crises*, and *critical days*.

I presume every gentleman here understands the meaning of these terms. It has been, from the most remote era, the almost universal opinion of physicians, that, in all fevers, may be discerned certain especial signs, or symptoms, which not only bring along with them material relief, or aggravation, of the distemper, but point out its issue, whether fatal, or salutary, and such symptoms, as they thus form a guide to our judgment, have been termed *crises*, to which is added the belief that these signs appear only on particular days, and such days, accordingly, have been distinguished by the title of *critical*.

It is not difficult to conceive the origin of this famous old

dogma. Fever is a disease so terrifying, and dangerous, as at all times to have excited the most lively apprehensions, alike in the patient, and his friends. Hence its whole course would be diligently watched, every change it presented would be carefully noted, as well as its import weighed, to ascertain, if possible, how it might secure relief for the present, or promise security for the future; or, on the contrary, might indicate immediate, or eventual, danger. By degrees it might be discovered that, for all these ends, some symptoms were to be regarded as more important, or more to be trusted to than others, and thus a foundation might be laid for the dogma of *crises*. That of critical days might spring up by a similar process of reasoning. Suppose a person seized with fever—common sense would dictate that he must sooner, or later, become better, or worse—he cannot stand still—he must either advance, or retrograde, he must either recover, or die. But the disease was observed to be of limited duration, lasting only a certain number of days, and within that period his fate must be decided, either favourably, or unfavourably. Should he survive the whole term, his life, in most cases, would be secured, and the longer the interval he passed over without material aggravation of the distemper, the nearer he would approach the end of the malady, or his haven of safety. Every day of the disease accordingly would be most anxiously counted; those on which prominent symptoms occurred would be carefully marked, and registered, till, in the course of repeated observations, it might be at length discovered, or supposed to be discovered, that certain days were more important than others, or exhibited more decided signs, either for good, or evil, than their fellows, and these days, at last would come to be distinguished by the title of *critical*. This dogma of crises and critical days, as already remarked, is one of the most ancient in physic. If we throw a rapid glance over its history, we find it to have existed during the most remote era, in Egypt, and the East—to have been taught by the hierarchy of the former country, who at the same time were its physicians, to those Grecian sages who first resorted, for instruction, to the banks of the Nile;—by them, to have been trans-

mitted to their countrymen, the Greeks, and by that ingenious people to have been farther drest up, and perfected, till it finally appears, under its most complete, and matured form, in the writings of Hippocrates. From Greece, with other medical dogmas, it passed of course to Rome, and continued to flourish till the end of the Empire.

In more modern times, again, after the revival of letters, being found a standard doctrine in the writings of Galen, it was received with the same implicit reverence as his other tenets. Fortified by his sovereign name and sanction, it spread universally, to the east and to the west; was admitted alike by Christian and Saracen,* and has at length reached the pre-

* During the splendid era of the Caliphate, medicine was cultivated with peculiar distinction among the Arabians, or Saracens, and received the most liberal patronage from their different princes. Our art, in fact, owes many obligations to this people. Thus they early rendered into their own tongue the principal medical writings of the Greeks, and, as is well known, it was from versions of these translations into the languages of Europe, that the Christians of the west derived their first acquaintance with the physic of the ancient world. They were the first likewise to establish regular schools of medicine, with ample endowments for the teachers, and, what was of more importance, they first built hospitals for the sick, an institution, of all others, the most conducive to the progress of the science of healing. They farther enriched the *Materia Medica* by the addition of new articles, more especially a milder set of purgatives than had been known, or at least employed, among the Greek physicians, such as rhubarb, senna, and the pulp of cassia; and they augmented the list of aromatics by joining to it, camphor, cinnamon, and nutmeg, spices which their familiar intercourse with India had thrown into their possession. From the same quarter they derived sugar, a purer sweet than honey, the only one, at least in any abundance, known to the Greeks and Romans, and that substance furnished them with the two new pharmaceutical forms of the syrup and confection. In addition to all this they are the reputed fathers of chemistry, and there can be no doubt that they were very early acquainted with the art of distillation, as well as various forms of mercury, more especially corrosive sublimate. These last they employed in diseases of the skin, and when syphilis first appeared in Europe, towards the end of the 15th century, it was from imitation of the Arabians that the physicians of the time first bethought themselves of using mercury for its cure.

Among the pupils of the Arabic schools were numerous Jews, a race of men, from community of origin, and strict monotheism, less hated and persecuted by Mahometan than Christian nations. The fame of Avenzoar and Avicenna survived in the persons of these Hebrew Doctors as late as the middle of the 16th century. We have a remarkable proof in the History of Francis the 1st of

sent era, but little shorn of its original authority. Divested of its theoretical appendages, the Pythagorean, or Egyptian, dogma of the mystic property of numbers, more especially, the number 7, with its multiples, and its corollary of climacteric years; the intelligent, and conservative, powers of Nature, with her concoction, expulsion and metastasis, of humours; and taken merely as a simple exposition of facts, without any overstrained deductions, and restricted to those febrile diseases only to which it was originally applied by Hippocrates, and the ancients, it will be found sufficiently consonant to observation, and experience. The maladies on whose phenomena it was founded were not only the intermittents, and remittents, of Greece, and the East, but also many of the Phlegmasiæ, and it cannot be denied that in these disorders the changes are often so sudden, violent, and striking, as to afford abundant elements of crises, in other words, a decided abatement, or aggravation, of the disorder, and a hardly less certain prognostic of a happy, or unfortunate event. The regularity of these changes is no less remarkable, witness the almost clockwork punctuality in the periodical returns of quotidians, tertians, and quartans, furnishing so probable, or feasible, a ground for the dogma of critical days. The same doctrine has been found no less applicable to the disorders that prevail in tropical climates, the seats of our European colonies, as the East and West Indies, than in those regions where its original founders, the fathers of physic, resided. Of this we have abundant proof from the records of disease in these equatorial countries, and

France. That Prince when labouring under a fit of sickness was so convinced of the superior skill of the Jewish physicians, that he applied for one to his celebrated rival the Emperor Charles the Vth, who accordingly sent him a practitioner of that nation from his dominions in Spain. This person, unluckily for his professional credit in the eyes of his royal patient, happened to have abjured Judaism, and Francis having no confidence except in a real unchristianized disciple of the eastern school, refused to consult him after his arrival in France. Under the same prejudice he despatched a messenger to his ally the Grand Signior, who furnished him with a genuine unconverted Jew of Constantinople. The disease we are told yielded to the stranger. The remedy prescribed was simple enough: being nothing more than a course of asses' milk.

the authors of these records will be found among its most strenuous defenders in modern times. It is suitable too to our own intermittents and remittents, when these last, though rarely, appear among us, and also to some of our Phlegmasiæ.

Taking all this for granted, the question occurs how far does the doctrine apply to the disease that forms the more immediate object of our present attention, the contagious typhus? Before entering into this discussion, however, it becomes necessary to remark, that though crises and critical days have been almost uniformly mixed up into one, and the same dogma, yet there exists, in fact, no necessary, or inseparable, connexion between them; in other words, there may be diseases in which, though we observe crises, yet there can be found no critical days. We have an example of this very distinction, as I conceive, in our low fever. Indeed were I to speak out my sentiments on this subject, and I certainly speak not without hesitation, and deference, against almost universal medical opinion, I would say at once, that for crises in this disease, taking the phrase in its ordinary sense, to denote some sudden, violent, and decided change, either for good, or evil, the evidence is exceedingly ambiguous; while, for critical days, it is more defective, if not entirely wanting. To begin with critical days.

To those who maintain the existence of such diurnal periods in typhus, the even tenor of its course must always prove a stumblingblock, it being by no means the character of this disease to undergo sudden exacerbation, or decline, like the fevers described by Hippocrates, or to advance by fits, or starts, either towards death, or recovery, but, on the contrary, to proceed with a pace, slow, creeping, stealthy, and uniform.

In order to evade this difficulty, two of our principal writers, Cullen and Fordyce, have had recourse to a singular expedient. They assume, as an axiom, that there exists no such disease in nature as a continued fever—that the malady so called is at bottom nothing more than a mere intermittent, only with this difference, that the fits, instead of keeping separate, as they ought to do, mix themselves up with one another; that

is, one paroxysm commences ere its predecessor has had time to terminate, so as to give the whole the false semblance of continuity. Hence you see, gentlemen, whatever we may think, the typhus of which we have treated so many cases, if we believe Cullen, is nothing more than an *ague in disguise*, like the armies of the two kings of Brentford, in Buckingham's Rehearsal; so that we have entirely mistaken the character of the disorder, all the while. Nor is this writer satisfied with one such transformation only; he insists upon three, affirming that ere it runs its course, every typhus assumes consecutively the whole principal types of his favourite form of ague, being till the seventh day a quotidian, from the seventh to the eleventh a tertian, and from that to the end a quartan. Fordyce, again, is satisfied with one model of fever, a quotidian, or what he terms an ephemera, a fever consisting only of one paroxysm, and, as the name imports, lasting but for a single day, though sometimes running out to thirty-six hours; and he argues, that a typhus is nothing more than a succession of such ephemeræ, only one ephemera commencing before its precursor had finished, so as to assume the pretended aspect of continued fever. Such reasoning cannot but appear more fanciful, than solid, and with respect to the supposed identity of typhus, and ague, on which it is founded, there is no doctrine in physic that will be found more untenable. These two distempers, in fact, differ essentially from each other, no less in their source, than their symptoms, agreeing in little else than their common name of fever. The ague takes its origin from malaria, or marsh miasm, the produce of inanimate matter in some particular state of decomposition; typhus, (I speak here of contagious typhus only,) from an infection generated by a living human body. This last is just one of the morbid poisons, as they have been called, like that of small-pox, scarlatina, or measles, each of which, as is well known, produces its own specific disease, or its own peculiar series of symptoms. Precisely the same property belongs to the poison of typhus, which may be observed to engender phenomena no less appropriate, and characteristic, of itself, than those that distinguish the others. How little they resemble those of ague

will appear from the following brief comparison of the two diseases. In ague, after the collapse of the cold stage, we have regular, and violent, reaction, as evinced by excited pulse, heat, and sweating; in low typhus, whatever may be said by authors, we have little else but collapse, and if any thing like reaction occur at all, in most cases, it is slight and trifling.* In the former malady there are regular, and frequent, truces, during which, the patient enjoys, if not health, at least an approximation to health, so as to be exempted from confinement in bed, and, sometimes, to be even capable of pursuing his usual avocations; in the latter, after the first few days, he sinks prostrate beneath the distemper, is chained to the recumbent posture, experiences no material respite, but is borne along, as it were, in one continued stream of disease. Ague, besides, often terminates all at once, or abruptly, either by a process of nature, or the efficacy of medicine, as of sulphate of quinine: typhus fairly begun, runs out its course (a few instances excepted) without check, or stop, either from nature, or art, and when it ceases, ceases only by degrees. No opinion, then, can be more unfounded than that which maintains the identity of typhus, and ague, or asserts that the former is a mere modification of the latter; it would be just as reasonable to affirm that our low fever was a mere modification of small-pox, or measles. One of the principal arguments found in books for the identity of typhus, and ague, is their alleged mutual conversion into each other; authors telling us, that a continued fever, during its course, is often changed into an intermittent, an intermittent into a continued fever. As to the last of these transmutations, I am

* Dr. Cheyne, *Dub. Hosp. Rep.* Vol. ii. p. 14, and other Irish physicians, whose opportunities of treating typhus have been so ample, often carefully measured the temperature of the patients, and in many instances found it below, not above the natural standard, yet increased heat is always regarded as the chief symptom to denote the presence of reaction. Another mark is augmented vigour of pulse, yet you have had all an opportunity of perceiving how seldom this takes place in typhus, and how evanescent it is when it does occur. In advanced stages of the disease, Cheyne considers heat of skin as no impediment to the use of wine, should the accompanying symptoms demand it—an opinion in which long experience enables me to concur.

hardly qualified to speak from personal experience, having treated but few cases of ague, not above forty, or fifty, during my whole life, all of them of extraneous origin, for this form of fever is not a native of Glasgow, or its vicinity, but, of these few, I never saw so much as one changed into continued fever. Concerning the last named transformation, that of typhus into ague, I can speak with more confidence: of the thousands of the former disease I have treated, I never met with the metamorphosis even in a single instance.* The argument of Cullen and Fordyce from this source, therefore, or that because critical days are to be met with in intermittents, they are also to be found in typhus, must be considered as utterly untenable. But besides all this, there is a peculiarity attaching to typhus, which, till the error to which it naturally leads, be guarded against, must ever prevent the settlement of the controversy, I mean the obscurity that hangs over the first symptoms of this disease, so as often to conceal the time of its actual commencement, the point of course where we must all begin to count in our calculation of critical days. You will be more particularly convinced of this truth, if you ever happen to be the inmate of a house, where any of the inhabitants in whom you take an interest, and with whom you hold daily intercourse, may have contracted the contagion. If you watch the progress of the disease, in its commencement, you will find it advance only, for the most part, step by step, or with the most stealthy pace. At first, the patient merely observes himself a little languid, or out of order;—the next day he will say that he is chilly, and you will find him creeping over the fire—on the next, he will tell you that he has no appetite for dinner—on the next, he will seek for sympathy by complaining that he has slept none over night—on the next

* It may be proper to remark here that the denial by Cullen and Fordyce of the existence of continued fever, involves a doctrine that may be traced back to Hippocrates, and seems founded on the phenomena of the diseases under this name described in his works, more especially a famous analysis of these fevers by De Haen. See Good's Study of Med. Vol. ii. pp. 88—90. The authority is of little avail. It may be doubted whether the Father of Medicine ever had an opportunity of treating a single case, or at least many cases, of our genuine contagious typhus.

he will have some vomiting, and headach—on the next he will be very querulous about a pain in his back—and finally, he will be seized with uneasy feelings all over, and will find himself so weak as to be obliged to retire to bed. Here the doctor finds him, or perhaps not till some time afterwards, and if he be an advocate for critical days, he considers it as the commencement of his disorder, and begins to count accordingly. But it is evident that the disease had existed long before. The symptoms narrated are no less those of genuine typhus than those that await him afterwards, differing only in degree and violence, but the same in kind. Such, however little adverted to, will be found the usual progress of contagious typhus, as I have ascertained, by questioning the sick, in many hundred instances. There were very few among them in whom I could not detect manifest traces of the malady, long before they were confined to bed. One instance was so remarkable as to deserve particular mention. The patient was a small farmer, who, in the intervals of agricultural labour, eked out his subsistence, by driving coals with his farm horses. His whole family, as the phrase is, was down in fever. He himself caught the infection, but he did not relinquish his employment. Being unwilling to forego his usual earnings, he struggled to the last, at least for ten days, till his limbs finally refused their office, and he was compelled to lay himself up with his wife, and children. It is obvious that this man as certainly laboured under fever, during the ten days he was driving his carts, as at any period afterwards, and it is no less obvious, that by the bulk of medical attendants, he would not be considered as a typhus patient till the time he quitted his labour and was actually confined to bed. Here then we have a formidable impediment to the settlement of our present dispute. It must be quite evident, that it is impossible to determine whether critical days exist, or exist not, in low fever, till it be decided at what particular period the disease commences, and till all observers agree to use this period, and this period alone, as a common startingpost, in their career of computation. But has any such convention been established among practitioners, or has any such epoch

of commencement been agreed upon? Unquestionably not. Till these two requisites of certainty then have been attained, and acted upon, are we not entitled to say that for the occurrence of critical days, no adequate proof has hitherto been laid before the public? With regard to my own experience, and it is not small, it is entirely on the negative side of the question: I have sought for these critical diurnal revolutions long, and diligently, but have ever sought in vain—and I have at length wholly relinquished the search. That important changes in typhus often occur on the days called critical, even granting them, notwithstanding the above causes of uncertainty, to be really such, I am willing to admit, but may not something be due here to the operation of chance? The duration of fever has been reckoned twenty or twenty-one days, and of these eleven have been termed critical. Take a pair of dice, and throw them from the box a thousand times, and it is well known that certain numbers will turn up more frequently than others. Let, in like manner, a thousand cases of typhus fever be taken, and let those days be carefully counted, and marked, on which supervene crises, or important symptoms, may it not happen that more of them accrue on the eleven than the other nine, merely from the superior number of the former, or by the chapter of accidents, and not from any new law imposed on the animal economy by the poison of typhus? It is indeed only when the disease has been, as it were, strangled at its birth, as by the timely exhibition of an emetic, followed up by a sudorific, that I have ever seen any thing in typhus that deserves the name of crisis. Here, indeed, we have a complete, and simultaneous, disappearance of the symptoms, or to use the Hippocratic phrase, the disease is actually and decidedly, finished, or *judged* (*judicatur morbus*). Thus, after a single night's time, from this treatment, it often happens that hardly any thing morbid remains, neither headach, confusion of mind, anorexia, nausea, vomiting, pains of back, and limbs, the sole remnants being slight whiteness of tongue, with a little languor, both which vanish in a day, or two. But do we see any thing like this in ordinary cases, after the disease has fairly begun, and establish-

ed itself? Certainly not. In instances of favourable issue, the symptoms in place of disappearing suddenly, or conjointly, recede only one by one, slowly, and reluctantly, as if loath to quit their victim, so as not unfrequently to exhibit a very odd, and incongruous, mixture of disease, and recovery. Thus if the tongue become clean, the appetite does not return; or if the appetite revive, the tongue continues foul; if headach cease, delirium, or coma, will supervene; if the pains of the back and the limbs vanish, the pulse becomes accelerated, or subsultus commences. If all convulsive motions cease, petechiæ or vibices will spread themselves over the skin. If the pulse return to its natural state, anorexia, and feebleness, may still continue to harass the patient. In cases again that terminate fatally, the exacerbations, in like manner, are observed not to occur all at once, but separately, and individually. That such, gentlemen, is the real process of nature in low fever, a few instances excepted, I appeal for evidence to our journals, and to your own reiterated observation. Upon the whole, with respect to crises, and critical days, in typhus, I cannot help thinking that we have been misled by a double source of error, one, an imaginary analogy, or identity, set up between this and other febrile diseases, particularly intermittents; another, too implicit a veneration for the general doctrines of fever transmitted to us from the ancients.

The pathology here, as is well known, was, for many ages, entirely humoral. The original Hippocratic doctrine was, that fever arose from a certain fomes mixed up with the blood, but which, after a certain time, (the period of concoction,) might be expelled along with different excretions, as alvine fæces, urine, sweat, and perspiration, or might be transferred to some part of the body where it would be rendered harmless, as expressed by the terms metastasis, and phlegmon. With respect to the practice here, though we were told anciently, that nature must be let alone, or not interfered with in these her salutary operations, yet in modern times, though we do not altogether disregard the rule, we seldom hesitate to moderate these evacuations, or to check them altogether, whenever we think them too profuse. I

confess I have some leaning towards one of them, namely the last, or the discharge by sweat, and perspiration. I cannot help thinking that of all the parts of the body implicated in fever, the great periphery of the skin, or surface, must always be accounted one of the principal. So highly did the celebrated Cullen rate its importance, that he placed, as is well known, in one single condition of it, the spasm of its extreme vessels, the proximate cause of the disease. Without subscribing to this tenet in its extreme sense, it cannot be denied, that a temperate, and pervious state of the skin, is a circumstance always highly desirable in fever, and the best proof of its existence is a moderate flow of sweat, and perspiration. The benefits ascribed to antimonials in former, and to calomel, in more modern times, might perhaps be explained by their power of operating this salutary condition of the surface. During my younger days, I knew several practitioners who used to treat fever, through its whole course, with little else than diaphoretics, and there are, certainly, worse modes of practice. When I find a patient sweating, except in some particular circumstances, I rarely interfere, but on the contrary, order some tepid drink to aid the operation, not unfrequently, I am convinced, with advantage. You must not suppose with the humoralists, however, that it is the mere evacuation of sweat, and perspiration, that is of service here: the benefit lies deeper. The discharge of these excretions in low fever, only shows, either, that the heart has so far recovered its vigour, as to be able to push the due quantity of fluids to all parts, and among the rest, the skin, so as to overcome the spasm of the extreme vessels, granting such to exist; or that by some internal rally of the constitution, which we cannot very well explain, the skin has become open, or has been restored to its former functions. The mere evacuation of the sweat and perspiration here, is no more the cause of the amendment in fever, than the discharge of saliva is the cause of the amendment in syphilis, the latter merely indicating that mercury is in the system, and doing its work. The fever is not relieved because the cutaneous secretion flows freely, but the cutaneous secretion

flows freely, merely because the fever is relieved. Another old and favourite crisis was epistaxis, or bleeding by the nose. How the fomes of typhus should escape by the vessels of the nostrils, more than any other part, the ancient writers do not condescend to tell us. It is, nevertheless, within due bounds, a favourable or advantageous symptom. The pathology is somewhat obscure, since the loss of an ounce or two of blood, and the quantity is seldom more, seems quite inadequate for explanation. Perhaps some restorative effort of the constitution may impart such unwonted impetus to the circulation, that, though sustained in other parts, without injury, may be sufficient to overcome the frail texture of the capillaries of the nose.

I come now, gentlemen, to the last, and most important, of our speculations, that which regards the nature, and essence, of our contagious typhus, and involves the two questions, whether it be a sthenic, or asthenic, and whether it be a topical, or idiopathic, distemper. You must have already anticipated my opinion on both points. It appears to me, that so far from having any thing sthenic in its composition, this disorder must be regarded, as in almost every respect, the reverse. I certainly do not deny that inflammatory symptoms occasionally occur, but I hold them to be merely extrinsic, and accidental, and by no means necessary to the essence, and character, of the disease. The danger to be dreaded, almost in all instances, is not from violent excitement, or exaltation of the powers of life, but from their abatement, or depression, either immediate or eventual.

As for the idiopathic nature of our typhus, the proof appears no less satisfactory. All its phenomena, instead of arising from any thing topical—any affection of a particular part propagated to the rest—seem evidently to flow from the general impression of the poison on the whole system, an impression in which every portion shares, and in which, of course, every organ and function, sooner or later, is more or less implicated. For the above two positions, gentlemen, it is possible you may now desire more formal evidence: and the following brief notices respecting the progress of medical

opinion on the subject of fever, may not be uninteresting, and may not be without use, in our present inquiry.

From the earliest records of our art, till about the middle of the last century, the pathology of this, and other diseases, in all the schools, a few excepted, is observed to have been strictly humoral. Every malady, it was said, had its seat in the blood, and other fluids, and was the offspring either of their exuberance in quantity, or their vitiation in quality, and could only, of course, be cured in one of two ways, either by reclaiming them from their depraved condition, or by expelling them from the body. At a period somewhat prior to that last named, when the celebrated Boerhaave formed his eclectic system out of the tenets of so many preceding sects, this ancient Hippocratic, and Galenical dogma, which, as it attributed every thing to the fluids, and left nothing to the solids, has been usually styled the *humoral pathology* was adopted by him in its fullest extent, and constituted one of the most prominent parts of his doctrine. With what universal favour this new edition of the ancient medical code was received, is known to every person acquainted with the history of our art. Nor is it difficult to perceive the cause of its popularity. Combining, with infinite talent and address, the tenets of all the rival sects then existing, into one homogeneous whole, it conciliated the applause of each, and it received no less powerful support from the persuasive eloquence, the profound learning, the extensive practice, and irreproachable character of its founder, while taught by him, for so many years, and to such crowds of pupils, from the practical chair of Leyden. You need not be surprised, therefore, when I tell you that, for a considerable period, it banished all other systems from the schools. What it taught of other diseases, it inculcated also with respect to fever, and as the Boerhaavians found it easier to evacuate the blood by the lancet, than to correct its supposed cacochymy, blood-letting came to be regarded as the great, if not the sole remedy, in every disorder that bore the name of febrile, equally in the lowest typhus, as in the most acute phlegmasia. That in the former malady the depleting plan was carried to the most enormous extent, we have the

testimony of Dr. George Fordyce, one of our best writers on fever, and who began practice at a time when the Leyden tenets still enjoyed their highest degree of authority. In the sixth and last, dissertation of that practitioner, we find the following serious, and severe, animadversion, as the result of long experience, passed on the Boerhaavian treatment of typhus: "The author cannot help lamenting, on this occasion, the terrible proposition laid down in an aphorism of Dr. Boerhaave, that it is difficult to diminish the strength at the beginning of fever, but easy to support it towards the end: the contrary of which he has found by long experience to be true. This proposition of Boerhaave, from the year 1730 to the year 1760, *destroyed more men than fell in battle in the whole of that period during two dreadful wars in Europe.*"—pp. 13, 14.

But the Boerhaavian errors were not long to survive during an enlightened age, and a new system had been already promulgated by a rival teacher in the University of Halle, the celebrated Hoffman which, though obscured, at first, by the superior lustre of the Leyden tenets, was destined finally to destroy them. To understand this great revolution, it is necessary here to remark that hitherto but little attention had been paid by any sect in physic to the really vital parts of our frame, the nervous and muscular tissues. Some dissections, no doubt, we had of the brain and nerves, by Willis, and Vieussens, as well as some experiments instituted by them relative to their functions, and these had been still farther illustrated by the speculations of Baglivi, but they led to no results of importance, nor to any general change of opinion. That honour was reserved for Hoffman. Throwing aside that slavish veneration for antiquity in which the age was yet entrammelled, the Halle Professor was the first to perceive the futility of the existing doctrines, and their inadequacy to explain the phenomena of life, and he had the boldness to propose a new theory of his own, founded on the only sure basis, the laws of irritability, and sensibility, or the properties of the muscular, and nervous fibre. The proposed doctrine, it may be observed, however, was, at first, but coldly received, and long remained a dead letter in the statute book of medicine. It

was not till many years afterwards that it came to be fairly appreciated, when aided by the powerful efforts of the Edinburgh school, particularly Cullen and Whytt, and farther assisted by Haller's splendid discovery of the irritability of the muscular fibre, with which it perfectly assimilated, it came, at length, to form that complete system of solidism, which, almost without exception, constitutes the creed of the modern physician.

Delivered thus from the incumbrance of the humoral pathology, physic, in all its branches, began to assume a new aspect. In particular, of many diseases a totally different view was taken from that which had been suggested by the tenets of Boerhaave. Various among them were found in which could be traced neither cacochymy, nor plethora, in which instead of redundancy there appeared deficiency of blood, so that we would add to the vital current, if we had it in our power; and in which, instead of lowering, we would invigorate, if we could, the sinking functions of life. In short, it was discovered that there existed a new set of distempers, of a nature hitherto but little recognised, the reverse of inflammatory, and on the contrary, justly to be styled asthenic, because atony, laxity, or, in one word, debility, constituted their principal, or leading feature. For the new diseases, new means of cure were requisite, and these consisted not in emptying, but in filling, the vessels, not in wasting, but in preserving strength, so as to enable the patient to meet with safety the contingencies of his morbid condition. In this list of maladies our contagious typhus soon found a place, and was treated accordingly; and the same plan was speedily extended to a variety of other distempers.

It must not be supposed, however, that this change of sentiment was universal, on the contrary, many retained their old opinions, both writers and practitioners, and there remained a large party of dissentients. It was alleged, besides, that the new system had been pushed too far; hence a reaction was predicted, and such reaction, accordingly, took place. Thus, some twenty, or thirty, years ago, we find an attempt made to revive the depletory plan of Boerhaave, not

certainly in form, or theory, but in essence, and in practice. The phraseology was merely changed. Instead of the old plethora, we had the more modern phrase *inflammation*, pushed in its meaning to the most extravagant extent, but with entirely the same scope, to countenance, or defend the most profuse depletions, particularly of blood.*

For this return to the old practice reasonings the most far fetched, and inconclusive, were deemed sufficient warrant. Thus it was said there existed fevers in America, and the West Indies, which could be cured only by the most copious blood-lettings, as illustrated by the practice of Rush in the yellow remittent of Philadelphia, and by that of Jackson in the same, and other similar diseases of our sugar islands, and hence it was argued that a corresponding treatment was equally applicable to the low contagious typhus of Great Britain and Ireland, as if it were identical in any thing but the name of fever, and not wholly different in origin, symptoms, and character. To support the same views, the old error of the locality of fever was again revived, of which the history of Medicine furnishes so many instances, both in ancient and modern times, and of which I may now mention a few examples from the latter only.

Thus, in the 17th century, we find Baglivi deducing all febrile affections from the mesentery; Sylvius De le Boe, the immediate predecessor of Boerhaave in the practical chair of Leyden, deriving them all from the pancreas. The mechanical physicians of the same era, who considered the whole human frame as a mere bundle of pipes, and strainers, governed by the common laws of hydraulics, on the other hand, believed that the source of the disease was simply too quick

* To show the prevalence of the opinion, both in the medical world, and in general society, that typhus was a mere phlegmasia, or inflammatory disease, see an article entitled, "On the Causes, Cure, and Prevention of Contagious Fever," in that popular work the Edinburgh Review, Vol. xxxi. for 1818-19. The author sums up his doctrine in the following words: "For our own parts we look upon typhus as to all practical intents and purposes, an *inflammatory* disease; and are satisfied that in by far the majority of cases ending in death there has been inflammation acute, or subacute, of some vital organ."—p. 430.

a passage of the blood through the larger, coupled with too slow a transit through the smaller, vessels of the body: while Boerhaave, again, with his commentator Van Swieten, imputed fever exclusively to a morbid condition of the heart. The same stream of hypothesis has descended to the present times, but coupled with a new error, and the conjoint opinion may be thus expressed, that all fever is a mere topical affection, and that the topical affection is, in all cases, one, and the same, namely, an inflammation in some particular part, or organ. In other words, that our low contagious typhus is nothing more than a phlegmasia. Such is the doctrine that has been maintained by writers, some of them not without eminence, in different parts of Europe, as by Ploucquet in Germany, Broussais in France, Clutterbuck, and with some qualification, Beddoes in England, and by Mills in Ireland. The advocates of the new etiology, however, do not all agree as to the particular part of the body, the inflammation of which thus gives rise to fever. Ploucquet prefers the head, but does not altogether despair if he can find a proper sthenic affection in some other quarter. Broussais, again, insists that the true seat of the disease is the mucous membrane of the alimentary canal, while Clutterbuck, with hardly less zeal, assigns it solely to the brain, and its membranes. Mills is the most indifferent of the whole as to site, and is quite satisfied, provided he can procure some focus of inflammation any where within the body. Here, then, gentlemen, we have a double doctrine, to be supported by the same proofs, and to be refuted by the same arguments. Objections fatal to both its parts are derivable alike from the dead, and the living, subject. Thus innumerable dissections of typhus patients have been performed in whom no trace of local disease could be detected, and this is true perhaps of the majority of those who have sunk under the disease, provided the different viscera have been previously sound. But the truth of this assertion will be more fully demonstrated to you afterwards. Evidence still more conclusive from the living body, has been daily presented to you, for many months back. When I say that typhus does not arise from a local

source, I must not be understood to mean that no parts of our frame are more severely tried by it than others, the contrary of this I have shown you to be the truth, both when detailing the prognosis, and method of cure; what I maintain is, that the affection of no one tissue, or organ, exclusively, is the source of the disease in other parts. All the functions of life are attacked, the cerebral, circulatory, digestive, and assimilatory, together with secretion and excretion, but the fever does not necessarily commence in any one of these as a focus or centre, and then proceed to the rest; but on the contrary, the symptoms run in one undivided circle, and all equally originate from one and the same common impression of the typhus poison. No doubt we see every day various really local affections accompany typhus, as cynanche, catarrh, rheumatism, &c., but these do not constitute any portion of the essence, or main body, of the malady; they are merely accidental, or accessory, for the fever holds on its own course in spite of them, and attains its usual termination, whether they be present, or absent. This remark will be found equally true, however severe the collateral disorder may be, as will appear by the following examples, and innumerable others might be quoted, if necessary. The first instances are from Dr. O'Brien, Physician to the Cork Street Fever Hospital, Dublin, and appear to have been two cases of very acute pneumonia. This writer observes: "The two worst cases just now mentioned were marked by peculiar distress, at their commencement, and were bled for three successive days to the amount of fourteen ounces, each day, within the first six days, after the attack. *The fever however went on*, attended with that formidable train of symptoms which is always the precursor of ulceration of the hips and sacrum, viz., delirium, subsultus tendinum, involuntary discharges of fæces, and urine," &c. Irish Trans. Vol. iii. p. 493.* O'Brien is of opinion that these bed sores, as we are accustomed to call them in our hospital,

* The success here certainly holds out no temptation to the practice of blood-letting in typhus. It will be afterwards seen that the judicious physician who narrates these cases came to the same conclusion himself at last.

take their origin from debility, or loss of vitality, in the superficial capillary arteries.

The example from Armstrong is still more conclusive. "Not long since I attended a case of typhus from the beginning, where the patient was twice bled to syncope, for an inflammation of the brain, and a third time to syncope, for a subsequent inflammation of the bowels; but though all appearances of inflammation subsided, *the typhus itself maintained a determinate duration, the increased heat, quick pulse, and other symptoms of fever continuing for many days afterwards.* Now, had I taken, in this case, the continuance of the fever as a certain sign of the continuance of the inflammation, the patient instead of having recovered, would have been inevitably lost; for after the third bleeding, the prostration of strength was so great that it required the utmost care in regulating the bowels properly, on the one hand, and in allaying irritation, on the other. Here bleeding was employed under the most favourable circumstances, in the beginning of the disease: it was carried far enough to subdue the inflammation, but much milder measures were afterwards necessary for the removal of the *remaining typhus.*"* Armstrong on Typhus, 3d edit. p. 172.

Upon the whole, it may be remarked, that to the fallacy of considering typhus a local disease, two circumstances have chiefly contributed. One is, that some of its symptoms are occasionally so prominent and violent, for example, delirium, vomiting, purging, as to obscure the rest, and give a faint semblance of locality. If you watch your patient, however, you will soon detect the mistake, for so far from these symptoms being the cause of the others, after they are removed, the disease just proceeds as before. The other source of error here, is the accidental addition of some extraneous ailment, such as catarrh, cynanche, rheumatism to an existing typhus. Such ailments, however, as already said,

* That in this case there was severe topical affection of the head and intestines, cannot be doubted, that the affection was strictly inflammatory, is not so clear. At any rate the patient seems barely to have escaped with life from these syncope blood-lettings.

are mere excrescences, and form no part or parcel of the fever itself. They are like the tassels, embroidery, or fringes, of a garment, of which the cloth or fabric, still continues the same, whether these appendages be present or absent.

It was a somewhat fanciful, but true, remark of John Peter Frank, that fever is less a disease itself than a picture, or emblem, of all other diseases, "*umbra morborum omnium, potius quam ipse morbus,*" and proceeding on this idea, considering that it attacks every organ or function, of the body, nothing is more easy than to split it into almost as many disorders as we please, as well as to assign for each, "a local habitation and a name." For this purpose we have only to fix our eyes steadfastly on any one single affection, and then shut them carefully on all the rest.

Say we wish to make it apoplexy, we advert solely to the coma it so often exhibits; if we prefer epilepsy, or convulsions, we quote the tremour of the cold fit of agues, and the *subsultus tendinum*, with other muscular twitches, of continued fevers; if we would convert it into dyspepsia, we allege the totally prostrate state of appetite, and digestion; if into insanity, we offer the proof of such frequent delirium; if into an exanthem, we talk of its *vibices*, and *petechiæ*, &c., &c. There is no end to such speculations, and they are equally useless, and puerile. The truth is, our contagious typhus, and I purposely exclude from consideration here, every other species of fever, is just a congeries or concourse of symptoms, simultaneous, or successive, joined together by the hand of nature, and known from repeated and indisputable, experience, to be produced by a particular morbid poison. Such is the substratum, or essence, of the disease. Its presence however does not exempt the body from the attack of other maladies, as catarrh, bronchitis, cynanche, pneumonia, &c., which thus become apparently complicated with it, though in reality merely accessory, or accidental, affections. Take them away, the fever remains behind, proceeds in its ordinary course, and pushes forward to its own usual, and individual, termination. Having thus shown you, gentlemen, that our typhus is an idiopathic, not topical distemper,

we are next to inquire more closely, whether it be one based in inflammation, and it is but fair now to cite the arguments that have been advanced on the affirmative side of the question. The principal I think may be reduced to four: the quickness of the pulse; the necroscopic appearances; the inflammatory affections said to occur in the course of the disease; the alleged benefits of blood-letting, and purging.

1. Rapidity of pulse.

It is a curious circumstance that so many writers have confounded rapidity, with strength, of pulse, considering the two necessarily conjoined, and both alike proceeding from a morbid vigour of circulation. No doctrine in pathology is more unfounded than this. It is very true that in inflammatory diseases the arterial beat is unduly accelerated, but it is at the same time rendered full, hard, or strong, while in low typhus, and other asthenic maladies, it is equally certain that besides its quickness, it is no less characterized by debility. Look at our journals, and you will find this assertion invariably true without, perhaps, a single exception in a hundred instances. There is no necessary connexion, then, between celerity, and strength, of pulse. On the contrary, when the former condition exceeds a certain limit, it always implies great debility, and the feebleness augments with the quickness. Let an animal be bled to death, for instance, and the pulsations will be found more and more rapid, till they can be no longer counted, or till the heart ceases to beat, or beats only at intervals. Yet surely it will not be alleged here, that if you withdraw from a living body the whole of its vital fluid, you produce in that body a state of active excitement, or inflammation. Accordingly, whenever you find the pulse in typhus very quick, you will also find it very feeble, as when it ranges much beyond 120 in the minute, and so far from ordering blood-letting on account of that quickness, you must, for that very reason, abstain from the lancet, and must substitute, in its room, wine, and cordials, or whatever can strengthen your patient. By these last means, times without number, I have reduced the morbid velocity of the arterial beat, the first effect of these remedies being to improve its

vigour, while as its force increased, the rapidity is observed sooner, or later, to diminish. You must not suppose that this rule holds in extreme cases, only a moderately quickened pulse is also often a weak one, in the same manner as when it is found excessively rapid. In fact, accelerated movement of the heart, and arteries, is a concomitant of asthenic, no less than of sthenic disease, and the use of the lancet must be determined not by the quickness, but the comparative force, of their contractions.—This argument of the inflammationists, therefore, as founded on erroneous pathology, must fall to the ground; indeed, instead of aiding their cause, it is directly hostile to it, or recoils against themselves.

2. Necroscopic appearances.

The principal evidence of the inflammationists has been derived from this source; when closely examined, it will be found wholly fallacious, and illusory.

As morbid anatomy now constitutes so important a branch of medical education, I hold it my duty here to offer you a few rules, by attending to which, while you secure every advantage from this mode of inquiry, you will also avoid many errors into which it might otherwise lead you. These rules are chiefly three.

a. That you omit no opportunity of seeing the bodies of patients dissected, but, at the same time, be exceedingly cautious as to the conclusions you draw from the phenomena they present. Above all, you must be particularly careful not to confound those changes that occur only in the dying or the dead subject, and which therefore have been styled the *cadaveric* appearances, with such as have actually existed in the living. This has been a fertile source of mistake, both in physiology and pathology. To give a single example in the former: when subjects are opened, the arteries are found empty, the veins loaded with blood, and it was hastily concluded that the same condition existed while the frame enjoyed vitality. And hence arose the most celebrated physiological error recorded in the history of physic, that which asserted the circulation to be performed by the veins alone, or exclusive of the arteries, which, as their name denotes, were supposed to be destined merely for

the transmission of air : an error known to have prevailed in all the schools above two thousand years, or from the age of Hippocrates to that of Servetus and Harvey. Some of the reasonings of the inflammationists will be found to be built on premises hardly less fallacious.

b. That you repair to every dissection with a mind entirely unbiassed—not determined, as too often happens, to see something, though, in fact, you see nothing, nor to magnify the most trivial circumstances into serious indications of disease. No deception is more common than this in private practice. Rash opinions as to the nature, and seat of the malady, have unfortunately been given, and the desire of course springs up to verify them by dissection. Natural are converted into unnatural appearances; molehills are swelled into mountains. Any old practitioner, if you consult him, will furnish you with abundant instances in illustration of this species of deception. I think it, in particular, necessary here to add, that when you can find no morbid alteration whatsoever, you must not thence conclude that your labour has been thrown away; on the contrary, such dissections are of the highest importance by preventing false ideas concerning the nature of the previous disease. Had we a regular collection of such, each appended to its supposed malady, it would have prevented many errors in practice.

c. You must discriminate duly between the credit due to the examinations of the practised, and unpractised, morbid anatomist. It is the former only you can trust, since he alone can point out to you the real differences between the sound, and unsound, appearances.

These remarks premised, we must now examine, more particularly, the evidence pretended to be deduced from dissections to show that our contagious typhus is an inflammatory distemper. We have an abundant collection of such from two eminent inflammationists, Beddoes and Clutterbuck, but they are so mixed up with *post mortem* appearances from other febrile disorders, more especially phlegmasiæ, as to be quite irrelevant to the present controversy. The question here is what are the necroscopic changes from the contagious

typhus alone, and the more to simplify the inquiry, I must suppose that the patient had been otherwise sound, or subjected to no disease but such as arose from this particular fever. I know of no series of published dissections with exactly these qualifications, except the following, but it luckily happens that these possess every requisite we can desire—in-
 disputable anatomical skill, and the certainty that all those whose bodies were examined had been the victims of typhus. The individuals from whom we derive these invaluable documents are Dr. Macartney, Professor of Anatomy in Trinity College, Dublin, and Mr. Kirby, an eminent teacher of the same branch in the same city, but without the walls of the University. Dr. Macartney's testimony will be found in Irish Trans. Vol. ii. pp. 573—576, and is communicated by an excellent, and experienced, practitioner, Dr. Barker, of the Cork Street Fever Hospital. Dr. Barker observes, "On the morbid changes caused by this disease, information may, no doubt, be acquired by anatomical examination of the body after death. To supply the want of this information at Cork Street, I have obtained from my friend, and colleague, in Trinity College, Doctor Macartney, the Professor of Anatomy, a most satisfactory account of the appearances which he has observed in those that die of this disease. I had put to him the following query: Are the appearances after death from this fever those of genuine inflammation, or are they of any peculiar kind?—He has favoured me with the following answer, which I have much pleasure in publishing, convinced as I am, of the extent, and accuracy, of his observations. He informs me, that "having reviewed his notes on the anatomical examination of persons who have died of typhus fever, he can state as the result of his experience, that the morbid appearances in typhus fever are *not those of common visceral inflammation*. A great proportion of the subjects for anatomical lectures in Trinity College, during last winter, appeared to have been of the present epidemic, *as they had petechiæ on the surface*, and his late observations on these have enabled him to confirm the above conclusion, which he *had deduced many years ago*. The morbid appearances that strictly

belong to typhus are the following, accordingly as the head, lungs, or abdominal viscera are engaged in the disease. 1st. Fulness or distention of the vessels of the brain especially the veins, some water effused on the surface, and into the cavities of the brain. 2d. The same species of congestion in the lungs, and different degrees of effusion in the cavities of the pericardium, and pleura. 3d. Venous congestion in the liver, spleen, or alimentary canal, sometimes a blood-shot appearance, or spots of extravasation in the mucous coat, more particularly in the stomach, and first coils of the intestines. In some instances a more generally pulpy, or swollen, and discoloured, state of the mucous coat of the alimentary canal. These congestions were always of a purple or venous colour, and the blood throughout the body appeared to be accumulated in the venous system, and had little tendency to coagulate. Such were the appearances attendant on the congestions observable after typhus fever. The morbid appearances in real and pure inflammation are these: 1st. In the head, the minute branches of the arteries appear more numerous than usual from carrying florid red blood. The effusion which takes place is more consistent than in the former case, and appears like whey; or pus is secreted on the membranes—the arachnoid coat is thickened and opaque. 2d. In pleuritis and pericarditis, there is the same distribution of the arteries, and a wheyish-looking fluid, pus or lymph thrown out. In inflammation of the substance of the lungs there is always venous congestion, but the small arteries also are increased, and the lungs feel more firm than in typhus. 3d. In gastritis, and enteritis, the inflamed parts are denser, the redness is brighter than in typhus. The peritonæum is liable to be involved, and termination is slough, or ulcer, after a certain time. In cases where general fever is combined with real local inflammation, as sometimes occurs in dysentery, or when pneumonia is combined with typhus, or the latter with permanent and violent delirium, the peculiar morbid appearances of each disease are to be observed in combination.

“ Two facts deserve to be recollected, 1st. That the duration of general fever and visceral inflammation are not the same.

2d. That internal inflammations are very common in hot blooded animals; but idiopathic fever is peculiar to the human kind. It may be added, that processes of an inflammatory nature are fitted for repairing parts that have their functions interrupted, or their structure injured, but the effects of typhus have no such power."

Dr. Barker adds, "The coincidence between Dr. Macartney and some of the best modern writers, on the subjects, are such as to prove satisfactorily that the congestions observed after typhus fever differ from those of genuine inflammation, and the question of treatment, as founded on these appearances, remains pretty nearly as before, to be determined by experience only."

Mr. Kirby's dissections are communicated in the following letter to Dr. Stoker, Physician to the same hospital, and no less conversant, from ample experience, with the nature, and treatment, of typhus, than Dr. Barker :

"MY DEAR SIR,—Having seldom examined such bodies as are brought to my anatomical theatre with a view to determine the organs which seem to be principally affected in fever, I feel that my authority may not be sufficiently particular to afford you the satisfaction you look for, on a point of so much interest as that which engages your pen. In the midst of all the discussions relative to topical congestion, it was impossible, however, not to remark the singular want of accordance between the prevalent opinions, and the local appearances. The brain, so constantly supposed to be the seat of inflammation, *rarely exhibited the characters indicative of such a state*—in some instances, this organ was much paler than usual; in a very few, amongst a great number of dissections, was there any evidence of sanguineous, or serous, effusion; nor was the latter to any considerable extent. The veins on the surface of the brain often appeared in a state of unusual plenitude; I do not think, however, this circumstance affords any argument in favour of preceding inflammation, nor can we be certain to what degree this state of fulness existed before death. I should not omit to mention, that the vessels between the cranium and dura mater, sometimes contained an unusual quantity of blood; the contents of the thorax were generally exempt from any character of the recent disease, and rarely exhibited such marks as are

esteemed to denote preceding inflammation ; there was very seldom any evidence that the peritonæum, or abdominal viscera, had been the seat of local inflammatory action. In short, in a great majority of cases, *so little did any particular organ seem to suffer, that I have wondered what could have been the cause of death.*

“ I am, my dear Sir,

“ Harcourt Street, Sept. 11, 1818.”

“ J. KIRBY.

P. S. I may recall to your memory an interesting dissection of a boy at which you were present ; he died of fever, and although it was believed that the brain was the seat of inflammation, the appearances were such as to convince us, that a very opposite condition prevailed.*

To DR. WILLIAM STOKER, &c. &c.

It is with great pleasure I quote what follows, so much in accordance with my own opinion, concerning typhus, from a physician like Dr. Stoker, so capable of accurate observation, and who has enjoyed such ample opportunities of treating the disease : “ All the causes of typhus, whether predisposing, or exciting, seem to be debilitating : contagion probably is so in a high degree, so far as its first effects have been ascertained, and the marks of debility developed in the course of typhoid fevers, are unquestionable.”†

From the above dissections, it incontrovertibly follows, that typhus is to be regarded, neither as a topical, nor inflammatory, disease. In like manner, since patients die, in innumerable instances, without leaving behind any lesson discoverable by the knife—does not the circumstance inspire infinite scepticism as to the real import, and consequence, of those morbid appearances so ostentatiously brought forward, as the sole, or principal, cause, of destruction, in this malady, more especially effusion, and vascular congestion ? May not these be considered, in many instances at least, as mere adjuncts, or accessories, not essential to the fatal event, which would have equally taken place whether they were present, or absent ? Besides, I am prepared to show that, in a majority of cases, these pretended signs of inflammation did not exist at all during life, but are entirely cadaveric changes, arterial, or

* Irish Med. Trans. Vol. ii. p. 431.

† Ib. 432.

capillary, and venous congestion, most certainly, and with the utmost semblance of probability, likewise, sanguineous, and serous, effusion.

To begin with congestion. It is a universally admitted fact that the blood after death is found expelled from the arteries, and unless it be extravasated, whither can it betake itself, except to the continuous tubes, the capillaries, and the veins? These two sets of vessels, therefore, in addition to their own vital fluid, must receive, and contain, also that which formerly circulated in the arteries, so as to be preternaturally filled or distended. No wonder, therefore, that there should thus arise both arterial, and venous, congestion,* more or less diffused throughout the body. The degree, or place, may vary according to circumstances, as from texture, or the quickness, or slowness, with which different sets of vessels may be disposed to part with their vitality.

Both kinds of plethora are particularly observable in the brain, and hence besides evident plenitude of the veins, we see numerous red streaks running along the membranes, consisting of small capillaries florid with blood, and for the same reason we observe a multitude of red dots following the incision, when the viscus is divided by the knife.

Serous effusion, in like manner, is often no less a cadaveric sign, and it seems to arise from that loss of tone which always accompanies departing, or departed life. The cohesion of our solids, you are aware, is no less a vital, than a mechanical property. How much it owes to the former, you can easily convince yourselves by feeling and contrasting together, the muscles of the calf of the leg, in a living, and of a dead person. In the latter, a similar laxity prevails within, as we see exemplified in the walls of cavities, whether vascular, or serving the purpose of reservoirs. Among the last, we have a familiar example in the gall-bladder. All of you who have seen a dead body opened, cannot fail to have remarked the yellow tinge surrounding this bag, owing, without doubt, to a portion

* The inevitable congestion from this source that must ensue after death, is exceedingly well stated and illustrated by Dr. Grattan, in *Irish Med. Trans.* Vol. iii. pp. 394, 395.

of the contents oozing out through the sides, in consequence of cadaveric relaxation, an escape the tone of the living body prevents, and which indeed could not happen without producing the most violent inflammation.

The same reasoning is still more applicable to those vessels that carry the thinner portion of the blood. The fluid here is of less tenacity than the bile, and assisted by the laxity of fibre that death occasions, will readily make its way through the parietes, so as to constitute those serous effusions so often detected in the bodies of the dead.

Against that assumption of morbid anatomists, that effusion and vascular congestion, in the dead body, always indicate the certainty of previous inflammation, in the living, a still more conclusive argument, were it necessary, might be derived from experiments, and observations, instituted on purpose, as by those of Dr. Seeds, and Dr. Kellie.*

The first of these gentlemen bled a great number of animals to death, as well as observed what happened in the shambles, and regularly found not only the brain inundated with serum, both in the ventricles, and between the plies of the tunics, but also complete congestion, both venous, and arterial, the former indicated by turgescence of the veins, the latter by numerous small arteries injected, and florid, with blood, and meandering along the membranes. The same experiments and observations were repeated and confirmed by Kellie. Now it surely will not be argued here, as observed before, that bleeding an animal to death is a source of inflammatory diathesis, either general, or topical. The last named experimenter details, too, some curious cases of death from cold, where exactly the same appearances of the brain presented themselves, yet the cold here, as neither preceded, nor succeeded, by heat, could have acted in no other way than as a pure sedative.

I have often been surprised that the above facts, and considerations, should have been so little attended to by morbid

* For those of Dr. Seeds, see *Med. Chir. Journ. and Review*, Vol. i. for January and May, 1816. For those of Dr. Kellie, see *Trans. of Med. Chir. Society of Edin.*, Vol. i.

anatomists, and should have so little influenced their conclusions. I give an example in Dr. Bright. I select Dr. Bright on purpose, because he is particularly eminent in this branch of research, and because we owe to him some valuable observations concerning a particular disorganization of the kidney productive of dropsy, though our obligations would have been greater, had he taught us how to prevent this structural lesion, or to remove it, after it had occurred. The following is the dissection of the head of a typhus patient, taken at random, from the cases of that gentleman: "The vessels on the surface of the brain were turgid, and, on the left side, there was found a slight deposition of opaque yellow fibrin running along the side of some of the chief branches of the vessels, in little patches of the size of grains of rice. The ventricles contained a considerable quantity of serum, and though it was impossible to calculate it exactly, yet there were evidently several drams. The corpus callosum, and the neighbouring parts, were rendered soft by the contact of the serum. This case terminated fatally, about the eighth day, *apparently from the severe inflammatory affection of the arachnoid membrane: for the traces of disease in the head were of a very unequivocal character.*"*

I am sorry that I cannot agree with Dr. Bright in the inferences to be drawn from this dissection. To say the least, there are many points of uncertainty. Who shall assure us that the turgescence of vessels, and effusion, were not mere cadaveric appearances, and, of course, not the source of death? Why may not that have ensued from the hidden cause that we know so often destroys patients in typhus, yet leaves behind no lesion cognizable by the knife? As for what is termed the severe inflammatory affection of the arachnoid, I own I cannot understand the acuteness of an inflammation not marked by any thickening of the membrane which was its seat, and merely indicated by a few dots of coagulable lymph, of the size of grains of rice, and I hold this change much too trifling to explain the fatal event.

It is hoped Dr. Bright, in case he happen to see this pub-

* Reports of Medical Cases, with a View of Illustrating the Symptoms, and Cure, of Diseases, by a reference to Morbid Anatomy: Lond. 1827, pp. 187, 188.

lication, will excuse the freedom of these suggestions. They are urged for no invidious purpose, but merely to prevent the authority of his name from being used as a pretext for an injurious practice, in typhus.

With respect to sanguineous effusion, in the dead body, where there is no rupture of vessels, it is to be explained on the same principle as serous, namely, cadaveric laxity. A similar effect, from the like atony, we observe, often, in the living subject. Hence the dropsies of relaxation, hence the greater number of passive hæmorrhages, of which last affection we have a good example in sea scurvy, where the blotches are found to be nothing more than coagulated blood that had escaped through the sides of the vessels. In like manner, we often find deposits of fluid merely from depending posture after death; and if blood be effused, it will render the surrounding parts red, simply by imbibition, so as to give them the false semblance of inflammation. But these appearances, whether in the living, or dead body, afford no argument for the depletory system, on the contrary, they require a totally different mode of management. You must not here suppose, gentlemen, that it is meant to be asserted, that after effusion, whether serous or sanguineous, has actually supervened, it will not continue after death. In almost every case it actually does. All that I here maintain, is, that it is frequently only a cadaveric appearance, and for its existence previously, that we require farther evidence besides what is merely afforded by dissection. It is likely, here, gentlemen, you may be disposed to put the question, are there, then, no marks left in the dead, from which we can conclude to a certainty that inflammation has antecedently existed in the living body? Undoubtedly there are. Inflammation produces certain sequelæ, that may, and generally remain after death, as ulceration, suppuration, and gangrene. It does not follow, however, that blood-letting is the proper remedy for such states, granting them to exist in a typhus patient. In gangrene, for the most part, the contrary plan is indicated, and surgeons find many cases, of ulceration, and suppuration, where all depletory measures are improper. In the chronic ulceration, for instance, that sometimes besets the intestines in low fever, and,

still more frequently, during the closing scenes of phtthisis, no practitioner would order large evacuations of blood, unless he wished to precipitate the fate of his patient.

Before concluding this subject, I must notice another appearance frequently met with in dead bodies, and always assumed, though I think on somewhat doubtful grounds, as an infallible mark of preceding inflammation. This is an exudation of coagulable lymph, or fibrin, often glueing membranes to one another, or to neighbouring parts. That this often accompanies inflammation, there can be no doubt, but it seems to exist too, independent of that cause, or at least, where the sthenic affection, if it occur at all, is exceedingly slight. We have a familiar example in the adhesion between the pulmonary and costal pleuræ, so frequently, or rather, almost so invariably, met with in dead bodies, where, however, no inflammation of these membranes had ever been known to precede, or, indeed, any other pectoral disease whatever. There is therefore still some ambiguity respecting this appearance, as a sign of inflammation, at any rate, did we know it to exist, at least by itself, it is very questionable, how far blood-letting would act as a remedy. Disagreeable consequences, as is well known, unless guarded against, often arise from this process of nature, in the healing of burns, as in uniting the arm to the forearm, the chin to the breast, but the preventive here, is to avoid contact of parts, not institute venesection, which last without the first, would to a certainty prove entirely unavailing.

So much for the necroscopic appearances, to show that typhus is an inflammatory disease, and must be treated with frequent blood-lettings, and other depletions. Some of this error has arisen from the loose and indefinite manner in which the term inflammation is used, and there is no set of persons so careless in this respect as morbid anatomists. With many of these gentlemen, there is no appearance that the scalpel meets with, that is not set down to inflammation. If a part be harder, or softer, than natural, it is alike inflammation; if it be opaque or diaphanous, hypertrophied, or atrophied; if it be altered in colour, if white, black, grey, or yellow, it

acknowledges no other cause, but is still inflammation. It is needless to say that all this is merely a mask for our ignorance. No doubt, it saves a world of trouble, and is a species of reasoning admirably adapted to the habitual indolence of medical inquiry. The fact is, the pathology of organic disease is yet in its infancy, and it may be truly said, that there are numberless causes daily operating such changes in the human frame, besides what are "dreamt of in our philosophy."

The practical rules for blood-letting, are, among many practitioners, no less vague and indefinite. If in disease, the pulse be found hard, firm, full, bounding, throbbing, we all agree that detraction of blood will be frequently, advantageous. But your zealous phlebotomist is not satisfied with any such limitation. Although fully concurring in the last precept, he also insists, that in case the pulse should be of the most opposite description, that is, should it be small, weak, and thready, we must nevertheless open a large vein, and here if you demur, he will look up in your face, and with great gravity tell you, that it will infallibly rise under the lancet; in other words, his doctrine is, that, when the arterial beat is too strong, we are to bleed it *down*, and when it is too weak, we are to bleed it *up* to the standard of health; in plain English, we are to practise venesection in every possible state of the pulse whatsoever. If it be simply asserted here, that in some species of inflammation, as that of the alimentary tube, and its membranes, the artery of the wrist does not give to the finger the usual inflammatory impression, yet we ought, notwithstanding, to take away blood, the position is perfectly intelligible, and one, in the justice of which, I perfectly concur, but from this fact, to argue, *as a general rule*, that where the constitution is unduly debilitated, we are to abstract the vital fluid by way of restoring its vigour, is a conclusion no less dangerous in practice, than in theory it appears to be unfounded and absurd.

As for those blood-lettings said to be practised among the less scrupulous part of our profession, either from charlatanism, so as to inspire the patient with a formidable notion of his

disease, to ensure the *eclat* of what is called *active* practice, or from the meaner motive of obtaining the conventional fee for a surgical operation, I have nothing to observe. I trust there is no gentleman here that will be swayed by such unworthy motives. The life and health of man are sacred deposits in our hands, and must not be tampered with either from vanity, or at the sordid call of avarice.*

3. Another proof alleged for the sthenic nature of typhus is taken from the inflammatory affections observed, sometimes, to accompany its progress. That such affections occasionally attach to it, there can be no question, and hence this might appear the strongest argument for the depletionists, but in reality, if carefully considered, it is not only the weakest, but even, in its actual bearings, will be found wholly to upset their doctrine. When I speak of fever here, I mean that disease solely which arises from the specific poison of typhus, and as it acts upon a person previously in the enjoyment of health, and of sound viscera. That of such unmixed malady, true inflammation is ever the attribute, there is no proof, rather the contrary, but you will observe, as before inculcated, that the presence of typhus does not exempt the sufferer from the attack of other diseases, and among the rest various of the phlegmasiæ. This invasion may take place in two ways—either, when the phlegmasia patient is exposed to the contagion of typhus, so as to catch the new distemper; or when the victim of typhus has been unwarily subjected to the influence of inflammatory causes, as cold and moisture, intemperance, &c., so as to have some sthenic disorder ingrafted on his fever. That such complications occur not unfrequently, there can be no question—you have seen various examples in our hospital. Let us now advert to the nature of this mixed malady, which, for illustration's sake, we shall suppose to consist of typhus and

* The celebrated Dean Swift, lamenting, on occasion of a fit of sickness, the absence of his usual medical adviser, as well as intimate friend, Dr. Arbuthnot, thus happily characterizes the skilful, and, at the same time, conscientious physician :

Removed from kind Arbuthnot's aid,
Who knows his *art*, but not his *trade*.

pneumonia, and let us consider how it is best to be treated. If the inflammationists be right, the road of practice is smooth, and easy. All concur in the belief that pneumonia is a sthenic disease, and these gentlemen assure us, that typhus is no less so; nothing remains then but to break down this complicated inflammation by the most active measures, the most copious blood-lettings, and every form of depletion. But is this actually found to be the most beneficial mode of treatment? Certainly not. Every practitioner of experience knows that pneumonia by itself, and pneumonia mixed up with typhus, are two very different diseases, and require a very different management. He constantly finds that the addition of the low fever acts as an unfailing drag, or restraint, on the career of inflammation, and that one-half, or even, often, one-fourth, of the blood-letting, and other evacuations, that would be demanded by the pectoral malady in an isolated state, is quite sufficient for subduing it when in conjunction with contagious typhus. This modifying, or neutralizing, power of the last named malady, when it happens to be joined to any disease of excitement, has been long known, and observed, and notwithstanding all his prejudices in favour of profuse blood-letting, did not escape the sagacity of Sydenham. It is fully recognised in the following paragraph. Talking of pleurisy as the adjunct of typhus, he pronounces his favourite remedy venesection to be useless, nay noxious, in such combination. “At vero si febris repetitam venæsectionem, respuat, *neque juvebit ista imo et nocebit in pleuritide, quæ cum febre stabit, cadetve.*”* The same sentiment is repeated, but in more comprehensive terms, by another old English author, though of later date, Sir Richard Manningham, who formally lays it down as a general maxim that fever is endowed with a converting or neutralizing power, reducing, or assimilating, every disorder with which it may happen to be combined, into its own nature. This doctrine founded on long experience is now admitted by all practitioners, except, perhaps, some bigotted inflammationists; you have repeatedly seen it acted upon, and you will recollect it

* p. 210, edit. Test.

was illustrated at some length in the former, and practical part of the lectures. Did it require farther confirmation, I might quote here in its aid, the authority of the Irish physicians, whose experience in typhus, under all its modes, and combinations, has been so frequent, and ample. Thus Dr. Grattan tells us, that, after long, and attentive, observation, he had at last come to the conclusion that in pneumonia joined to typhus, no bleeding should ever exceed eight ounces.* In a subsequent report, when instructed by farther trials, he informs us he found it expedient to reduce this quantity, to little more than one-half. Thus, of 316 patients affected with pectoral inflammation, he says, 116 were blooded, but the average measure taken was no more than five ounces and a half. He adds that of 268 head cases, the temporal artery was opened in 98, while the evacuation was only five ounces, which however he found quite sufficient for his purpose.†

Now what is the fair, and legitimate, conclusion to be drawn from these premises? Suppose in place of typhus, had been joined to pneumonia some other equally, or more, violent, phlegmasia, say phrenitis, what would be the nature of this new complication, and how ought it to be treated? You will no doubt answer, of a nature the most inflammatory, and to be encountered by the most active, and unsparing depletion. But when typhus is joined to pneumonia in place of phrenitis, what is the quality of the distemper, and the appropriate mode of practice? Your reply will be equally ready, that though there exist sthenia, here, yet in consequence of its union with low fever, it is reduced to an inferior grade, and by no means calling for the copious blood-letting requisite in the other instance, while if venesection be employed at all, it must be on the most reduced and moderate scale—instead of thirty ounces, five ounces, or five ounces and a half, as practised by the experienced Grattan. Now, does not this circumstance throw as it were a blaze of light on the true nature of typhus, when we find it thus bridling the well known sthenic disposition of pneumonia, as it will invariably do that of every

* Dub. Hosp. Rep. Vol. i. p. 477. † Ib. Vol. iii. p. 408.

other phlegmasia with which it may be casually conjoined? I have already shown you that these inflammatory affections thus occasionally attached to typhus, are mere adjuncts, not constituting any part or portion of its nature or essence, but altogether extrinsic and accessory. Take them away, therefore, what remains? I answer an asthenic disease, or one almost of pure debility, abounding in signs of irritation, some of them violent enough, but rarely under its pure form, exhibiting symptoms of real, decided, or genuine inflammation. Such, gentlemen, seems a fair deduction from the facts above stated. This argument of the inflammationists then will avail them but little; instead of supporting, it invalidates their doctrine, while on the other hand, it strongly confirms the view I have all along given you of the real character of contagious typhus.

4. Alleged efficacy of blood-letting, and purging. By blood-letting, here, I do not mean those slight, or partial, evacuations of blood, as by cupping, or leeches, which all practitioners employ in typhus, but those copious, or profuse, depletions from a large artery, or vein, that suit a phlegmasia such as the inflammationists consider typhus to be, and which are found indispensable in that order of diseases. On this subject, I shall begin with citing some of those who disapprove, or reprobate the depletory practice, and then advert to what has been said on the other side, by a few of the principal authors who recommend the opposite plan of treatment. At the head of the first may be mentioned Fordyce. Already I have laid before you the general doctrine of this celebrated author; I shall now mention in addition, one, or two, of his more particular tenets. In his Third Dissertation he observes, "If the disease which the author has endeavoured to define as fever be only meant, the taking blood from a large vein in any part of the body indiscriminately, *never diminished, shortened, nor carried off a fever in any case he has seen*, nor has he found any on record in which it has had that effect." He adds, "Taking away blood from the arm, or from any large vein, neither increases nor diminishes a fever, nor alters its course, so far as he has seen." In another place a most

important caution is subjoined—"The further debility arising from emptying the vessels, by taking away a quantity of blood, is often such as to destroy the patient in the remaining part of the disease. Patients in consequence have been very often cut off, where blood has been taken indiscriminately from any large vein, at the beginning of the disease, *as the author has seen in a great many cases.*"

On the same side I adduce with pleasure the testimony of various Irish physicians, a class of men whose experience in this disease, from its unhappy prevalence in Ireland, is known so much to transcend not only our own, but that of the practitioners of any other country. Thus Dr. Grattan, physician to the Cork Street Fever Hospital, Dublin, and to whom we owe the valuable dissections of Dr. Macartney, has the following observations: "It frequently happens when a patient has been under the care of an injudicious practitioner, and has been blooded, or purged, or blistered, to excess, that when his strength begins to fail, and his case seems hopeless, he is then, and only then, advised to apply to the hospital for relief, and forced upon us for the purpose of releasing his former attendant from all share of responsibility. Under such circumstances, what can we effect for accomplishing a cure? We may indeed prolong the patient's existence, and by the use of wine, and invigorating cordials, support his strength for a time, but it is in vain to look for his ultimate recovery; the principle of life, which was unnecessarily wasted in the commencement, becomes unequal to the struggle of a protracted illness, so that at length he sinks into the grave, from mere exhaustion."* From the above quotation, gentlemen, you will easily see the averseness of this experienced physician from the depletory system, and it besides exhibits a lively picture of a species of suffering that we ourselves are so frequently fated to endure. Thus how many cases are forced upon us weekly, that are entirely hopeless, and which seem sent up merely that they may die in the hospital, and be buried in its ground. In saying this, I impute not the slightest blame to

* Irish Med. Trans. Vol. i. pp. 461, 462.

their former medical attendants : the evil lies deeper, and far beyond their control. In fact, it has been the curse of that part of our population who are the usual victims of typhus, to have fallen, during our different epidemics, under the superintendence of a set of persons, who from ignorance, or some other cause, have never taken the proper measures for checking the contagion, but who, by their municipal authority, have carefully excluded all others of their fellow-citizens who are competent to the task. Now, however, that Parliamentary Reform has been achieved, it is hoped, this enormous nuisance will be abolished, along with the other abuses of the self-elective system that gave it birth.—Dr. Grattan states farther, that in two cases where pneumonic symptoms occurred, he ordered bleeding to the extent of sixteen ounces, but only one of the patients survived the evacuation. In the other, the fever soon became malignant, and proved fatal. Emboldened by farther experience, the same distinguished physician, in a subsequent Report, speaks out more freely, more especially regarding those marks, both in the dead, and living, bodies, of typhus patients, that have so confidently been pronounced to be undeniable proofs of inflammation : “ Even in all those cases in which the capillaries of any organ are distended previously to death, it may be doubted whether such distension, or engorgement, was identical with inflammation, or in any way connected with it. I have seen instances of patients who, a few hours previous to their death, exhibited marks of apparent inflammation, tending rapidly to gangrene, and which state clearly depended on a deficiency of nervous energy, and constituted in fact the commencing dissolution of the part : this occurring on the surface of the body, is evident to the sight, and cannot be questioned. May not then the same process equally take place in the brain, and thus an appearance which is only the effect, come to be considered the cause, of the disease ? This is I think extremely probable, and it is likely that, in this way, very erroneous opinions may be adopted by the mere anatomist, with respect to the immediate cause of fever.” What follows approximates though not entirely, yet so nearly, to my own notion of typhus, that,

of course, I quote it with the greatest degree of pleasure. "The doctrine which teaches that fever is a disease of the nervous system, and at the same time admits that this diseased action does often occasion local inflammation, seems to be nearer to the truth than if we were to ascribe fever to either of these causes exclusively. It embraces the advantages of both theories, and, in a practical point of view, comprehends every case of fever, and every variety of treatment, which can be employed in its management."*

Dr. Stoker, colleague to Dr. Grattan in the Cork Street Hospital, every where in his Report, reprobates, from experience, the depletory system. Among many other passages of similar tendency, I give the following extract, taken at random: "I have frequently observed how insidious this seeming relief from blood-letting is, in the commencement of our common fevers, both where I have been necessarily compelled to employ it, either for local inflammation, or apoplectic threatenings, which co-existed, or when I supposed that remedy was fully indicated by the symptoms of excessive excitement: the succeeding crisis being imperfect, prostration of strength coming on more rapidly, and recovery more protracted, or death taking place more suddenly, and without those struggles by which it is generally opposed."†

The authority of Dr. Cheyne, Physician to the Hardwicke Fever Hospital, Dublin, I adduce here on several accounts, with peculiar satisfaction. This gentleman, in consequence of his office of Physician General to the army, having been in the habit of treating soldiers, a class of men, for obvious reasons, the subjects of diseases highly inflammatory, and therefore requiring the most copious evacuations, could hardly fail of being strongly impressed in favour of the depletory system. Of such predilections we have abundant proof in his Report, where we find him often lamenting the injurious tendency of the old method of treating fever, and praising, in equal proportion, the superior advantages of the new. In the course of the Report, however, we perceive his zeal gradually abat-

* Irish Med. Trans. Vol. iii. pp. 394, 395.

† Report of Dr. Stoker in Irish Med. Trans. Vol. ii. pp. 460, 461.

ing, till at last, we observe him, if not recanting, at least largely qualifying, his former opinions, and what is infinitely to his credit, with admirable candour, frankly avowing the change that had taken place in his mind. Among other proofs, he tells us, that he used to bleed by the arm in swellings, and pains, of the epigastrium, but found afterwards that he was more successful by leeches. In one case of delirium, he adds, he opened the temporal artery, when the patient became sensible, but he died the next day. Speaking generally of bleeding in fevers, he concludes in these words, "It is my duty, however, more especially as I have the name of being an advocate for blood-letting in fever, to state that several cases have come to my knowledge in which blood-letting, practised when the disease was confirmed, proved injurious; great prostration followed, and although the local determination which probably demanded a cautious use of the lancet, was subdued, yet the struggle was more dubious than it otherwise would have been. In two instances, I had reason to think that blood-letting was productive of fatal effects: one of these cases was characterized by vigilance, a tongue scarcely affected, great quickness of the pulse, and dun-coloured petechiæ; both cases were atactic. But these were instances of the abuse of blood-letting. There are many cases of fever in which blood-letting is inadmissible in any stage of the disease; and there are many cases in which early blood-letting would be salutary, while late bleeding would be ruinous." Some parts of this last sentence show the reluctance of a man to abandon, even after evidence of its unsoundness, a favourite and preconceived opinion. In another part of his Report, this eminent physician states that when he ordered blood-letting to the extent of twelve ounces, he always superintended the operation himself, a clear proof how much his mind misgave him when he resorted to this mode of practice.

Dr. Cheyne subjoins a list of his fatal cases, with remarks and dissections, and in perusing this it is impossible not to admire the extreme spirit of fairness in which every thing that comes from him is narrated. Thus he every where details with the same freedom, and fulness, the occasional want

of success in his treatment, as its most happy results, more especially the effects of blood-letting, an example highly worthy of imitation by every physician who sincerely desires the advancement of his art.*

The next two, and last testimonies, those of Drs. Hagan and Macdonnell, though short, are not the less conclusive. Dr. Hagan was another of Dr. Grattan's colleagues in the Cork Street Hospital, and he gives us the following statement: "In last August, (1813,) the sick under my care in the hospital, were 104, of whom four died. Of the 100 dismissed cured, two only were bled. I am disposed to think that more than nine-tenths of the 98 cured without being bled, might have lost a few ounces of blood without injury, but how unfairly should I have referred their recovery to blood-letting?"† There is a great deal of truth as well as shrewdness in this remark. An internal medicine, however powerful, unless perhaps an evacuant, has often little about it to attract the attention of an hospital pupil. Blood-letting is in a different predicament. All the details are striking, the tying up the arm, the rising of the vein, its perforation by the lancet, the spout of blood, the rattling of the cups, the bloodless lips, and perhaps fainting of the patient, circumstances all of them calculated to make a deep impression on the senses, and imagination. Hence, blood-letting, though perhaps altogether useless, if employed with other remedial means, too often throws the rest into shade, and obtains the entire merit of the cure.

Dr. Macdonnell, of Belfast, my old fellow-student in the University of Edinburgh, and whose long experience, and high eminence, in his profession, require no eulogy from me, along with his colleague, Dr. Stephenson, is equally brief and decisive. In the annual report of the Belfast Hospital, to which these two gentlemen are physicians, we read the following statement: "With regard to the plan of treatment, we may observe, that of 3527 admissions in three years, not more than *two or three have been bled during the fever.*"‡

* Dub. Hosp. Rep. Vol. i. pp. 60, 61.

† Ib. 462, 463.

‡ Annual Rep. of Belfast Fev. Hosp. for year 1819, p. 8.

To this list of testimonies against general blood-letting, as a regular, or standard practice, in fever, I may be permitted to join my own. From first to last, I am sure, I have treated at least five thousand cases of this disease, yet the number of times I have ordered the opening of a large vein, or artery, I am confident, does not exceed thirty. Many of these blood-lettings, indeed, were not practised against contagious typhus at all, but rather against different really inflammatory distempers, more especially of the chest, that had been sent to the hospital, by mistake, instead of fever.

But it is time now, gentlemen, that I should offer you some remarks on the writings, and opinions, of those who have enlisted themselves on the opposite side of this controversy, and who maintain that typhus being nothing more than a phlegmasia itself, must be treated just like the rest of the order, that is, by the most profuse blood-lettings, and every mode of the most unspairing depletion. Many writers have advanced this doctrine; I shall limit my observations to four only who may be considered, if not leaders, at least fair representatives, of their sect or party: Armstrong, Bateman, Clutterbuck, and Mills. Of this last, however, it is but justice to say, that his blood-lettings were far more moderate than those recommended by his brother inflammationists. I begin with Armstrong, as the most celebrated.

Dr. Armstrong was originally a physician of Sunderland. With the very excusable view of introducing himself into practice, it may be conjectured he had early conceived the design of writing a dashing book, and the time he chose was admirably adapted to his purpose. The reaction of the depletory system was then at its height, and it was the opinion not only of practitioners, but of many of the public at large, that the human frame was hardly susceptible of any disease that was not essentially based in inflammation, or in some species of sthenic excitement. As yet, this doctrine, in Great Britain at least, had not been extended to continued fever. Here then was an opening for a new book not to be neglected, and as in religion and politics men assume a show of ultra zeal to please their sect, or party, the same plan was adopted by

Armstrong, in medicine. All opinions that militated against the new light were to be put down, and trampled under foot, and all those who supported them, even the most celebrated names, were to be consigned to derision, and obloquy. As the tone might seem too high for so young, and obscure, a practitioner, it was to be moderated and softened by an assumed veil of modesty, and candour.

I had read this book long ago, gentlemen, and I looked it over again lately, certainly not without amusement. The whole is dictatorial, dogmatic, and oracular. One would think that no man had ever seen a case of typhus, or formed the least conception of its nature, till Armstrong came into the world. Among other pretensions, new forms of the disease were to be detected, and described, which had hitherto eluded the observation of others, and, as examples, may be quoted what he has christened by the names of his *Inflammatory* and *Congestive* Fever. Of the first the principal symptoms assigned are the uneasy muscular feelings apeing Rheumatism that so usually harass patients at the beginning of typhus, and which he attributes to a no less serious cause than inflammation of the vertebral cord, or its membranes. No pathological position seems more untenable, not to say absurd. Among all the signs of fever, the flying pains alluded to, appear the least important, which certainly would not be their character did they actually arise from lesion of so vital an organ as the spinal brain; and if there be inflammation here at all, gentlemen, which I very much doubt, it is certainly the most docile and tractable inflammation one would ever wish to meet with, for it takes its departure at the first bidding of the practitioner; in other words, it yields invariably, as you have seen in a hundred instances, to the most ordinary and simple diaphoretics. As for the famous congestive typhus of Armstrong, it is another instance of humbug. Stripped of the mystification in which it is involved, and so far as it is not a mere creature of theory, it is neither more nor less than the typhus gravior of nosologists, a form described by every author who treats of fever, but which is now obtruded on the world as quite a new modification of the disease, first noticed

by Dr. Clarke of Newcastle, and Dr. Jackson, the army physician, but of which he himself was to give the fullest, and most satisfactory, elucidation, as well as to point out the most proper method of cure.*

As for the practice recommended by Armstrong, it was such as might be expected from the new light—unmeasured, and endless, depletion, particularly of blood. One of his precepts seems peculiarly objectionable, that which enjoins us to take the typhus patient out of his bed, set him upright on his legs, and then bleed him by the arm till he falls down in a swoon. Among other arguments for this plan is its alleged benefit in plague, as if plague and typhus were identical diseases; and one of the authorities in its favour is so utterly ridiculous that it is difficult to conceive how it could have found a place in any treatise except as matter of burlesque. The authority here alluded to is a strange character of the name of Dover, half-doctor, half-Buccaneer, and who towards the end of the 17th century, seems, with a few ships, to have infested, in the last capacity, the maritime cities of Spanish America. Having learned that 180 of his men had been seized with plague, this pirate-physician, or physician-pirate, thought proper to issue the following mandate, which I give in his own words: “I ordered the surgeons to bleed them in both arms, and to go round to them all, with command to leave them bleeding till all were blooded, and then come and tie them up in their turns. Thus they lay bleeding and fainting, so long, that I could not conceive they could lose less than an hundred ounces each man.” So signal was the success of this detraction of blood, according to Dover, about eight pounds at once from each individual, or nearly one-third of the whole vital fluid, that only seven or eight of the men sunk under the pestilence. That seven or eight of the sailors lost their lives on this occasion, we may readily believe, that they died of plague is not so clear; I am more disposed to think that they perished by the blood-letting. Among the ancient Romans, as every body knows, opening the large veins was a familiar

* Clutterbuck, in his treatise on fever, though himself a brother inflammationist, is observed to scout, with proper scorn, this pretended new species of typhus.

mode of suicide, as well as for inflicting death on others, and when the blood did not flow fast enough, it was assisted by the warm bath, or the heated air of a stove, or bagnio. Of the former expedient no use was made here, but the burning atmosphere of the West Indies might have perhaps supplied in some sort, a substitute for the latter.*

The other authority cited on this occasion by Armstrong, is a certain nameless surgeon, who, as related by Sydenham, served in the King's army during the civil war of Charles I. The substance of his narrative is the following: "He bled them all standing, (that is the patients with plague,) and in the open air, and had no vessels to measure the blood, which falling on the ground, the quantity each person lost could not of course be known. The operation being over, he ordered them to lie in their tents, and though he gave no kind of remedy after bleeding, yet of the number that were thus treated, not a single person died, which is surprising." Very surprising certainly, so surprising as to be altogether incredible, for who ever heard of the plague raging any where without inflicting death at least on some of its victims? Of all practitioners, indeed, Sydenham is the last to be quoted on plague, for it was a disease he never saw. It is a well known blot upon his name, that when the distemper first broke out in London, he immediately shrunk from the encounter, left his patients to their fate, and buried himself in the country.

Such are some of the far fetched, and inconclusive, reasonings of this prince of depletionists, to defend his *addeliquium* blood-lettings, and to show the inflammatory nature of Typhus.

In looking over Armstrong's work, we perceive some

* I have looked at Dover's book, which bears for title, "The Ancient Physicians' Legacy, by Thomas Dover, M.B." The following analysis of the human fluids which he spared so little, I find given by this learned Buccaneer: "Now the fluids consist of animal, mineral, and vegetable beings, alkalis and acids, which may be thus accounted spirits, sulphur, salt, earth, and water: there is no description to be given of the first of these, unless a negative one—the second two are minerals—the other two the joint cause of vegetation."

occasional ebbings of his zeal for depletion, and detestation of strengthening medicines, and accordingly he admits that it is possible wine may be given in certain cases without positive disadvantage; and in what manner, and quantity, do you think, gentlemen, he permits it to be administered? why, in tea-spoonfuls, mixed with water. Had he ordered the tea-spoonfuls of water by themselves, or without the wine, it would have been more consonant to his doctrine, and unquestionably the practice would have been equally effectual.

Notwithstanding these and all other objections, however, Armstrong's book was highly successful. It was received with unbounded applause by the numerous friends of the depletory system, was soon reckoned a standard work, and passed through several editions. It was, without doubt, fluently and plausibly written, and every thing was advanced with such an air of confidence and candour, as for a long while to elude detection. What was of more importance, it completely answered the writer's purpose, and on the *eclat* it procured him he settled as a physician in London, where he soon arrived at a very lucrative practice. Among other pieces of good fortune, he was in a short time appointed to the London Fever Institution, and it is to his conduct in this situation, gentlemen, that I wish more particularly to call your attention. You would naturally expect that there would be now no end to his blood-lettings, and as little limit to his doses of calomel and opium, the anti-inflammatory effects of which, on the authority of Dr. Hamilton of King's Lynn, he had so highly extolled in his book: what then will be your surprise when I tell you that his practice, before so active, vigorous, and flourishing, after a brief space became wholly inert; that his bleedings rarely exceeded a few ounces, that his panacea of calomel and opium was dropped, and that almost the only remedy he administered was an occasional dose of cold drawn castor oil. From these facts, which rest on the undisputed testimony of a cotemporary London physician, one of two consequences inevitably follows, either that Armstrong was insincere in the praises bestowed on his former mode of

practice, or from a wider acquaintance with the real nature of typhus than he had been able to attain in Sunderland, he had discovered that practice to be futile or dangerous. The last I believe to be the true conclusion. The fact is, Armstrong inverted the natural order of things: he wrote his book first, and obtained his experience afterwards.—Taught by that experience, however, this great leader of the inflammationists, at last relinquished the cause of his party, and by his desertion afforded an experimental proof of the fallacy of their doctrines.

It is possible, gentlemen, you may conceive that I have treated this writer with too great severity, but if you advert to the supreme arrogance and contempt with which he is wont to speak of all those who had not become proselytes to the new light, you will not think him entitled to much indulgence. One instance will be sufficient. It is when he talks of the Edinburgh school, more especially the celebrated Cullen, to whom medicine, in its different branches, owes so many obligations, both as an author, and a teacher. The passage is as follows: “During the rise and progress of the fatal doctrines of debility, and putridity, the lancet was condemned in many fevers, and by authors, who, with a singular inconsistency, continued to commend the sagacity of Sydenham. But the speculations of Cullen, and other men of genius, which have so long obscured our pathological views, are, at length, passing away, like clouds before the spreading light of more favoured times, and we may reasonably hope will soon entirely disappear from the horizon of the medical world.”*

Such is the style of arrogance and conceit in which Armstrong and his school indulge when speaking of the most celebrated practitioners. As for the oblivion with which he here threatens Cullen, it is almost unnecessary to remark that so far as his book on typhus is concerned, it is now fast closing over himself. At one time indeed he was of high authority, was universally read, and by none more widely, and eagerly, than by the students of this University.

Many of these I know to have been misled by him on the subject of fever, and I have a heavier charge against him in this, that I believe several of my young friends, when labouring under this disease, to have had their fate precipitated by the erroneous notions of practice they had imbibed from his book.

Notwithstanding all this censure, gentlemen, you must be careful how you suppose Armstrong to be without merit as a writer on medicine. No opinion would be farther from the truth. He has in fact composed valuable treatises on various diseases, particularly such as were of an inflammatory nature. Here he was in his element, and he has not only given us an excellent description of them, but pointed out a most judicious mode of treatment. The great defect of Armstrong, and his sect, is the narrowness of their views. They put us in mind of a curious instrument sometimes met with in Russian bands of military music, which is so contrived as to emit only a single note. The minds of these gentlemen resemble that instrument. Of disease they can conceive no source but one, something sthenic: in the vocabulary of their pathology there is but one sound, *inflammation*.

With the other advocates of the depletory system, I shall not detain you so long; the next I am to mention is Clutterbuck. From this author we have two treatises, one on the seat, the other on the prevention, and cure, of typhus. The first, though I do not concur in its tenets, I have read with great pleasure. It is a work of extensive erudition and research; the argument is ably and skilfully put, and above all, it is impossible not to admire the spirit of perfect candour and fairness that runs through the whole performance.* Of

* One instance of these qualities in Dr. Clutterbuck that came, more immediately, under my own personal observation, I have great pleasure in mentioning. Many years ago, when the Doctor did me the honour to attend my lectures, I happened to notice, in his presence, a small publication of his in which it was stated that he had found calomel to act as an antidote against the poison of lead, by the benefit it produced in the peculiar colic that metal is known to excite. On the dismissal of the class, he immediately came round, and, with the utmost frankness, informed me, that he had discovered his opinion in this matter, to have been erroneous, and that, from subsequent trials, he had

the other performance, I am sorry I cannot speak in the same terms. We have unfortunately to lament here, much of that violence against old, and predilection for new, opinions, that so peculiarly characterize the sect of the depletionists. Indeed, between Clutterbuck's sentiments and mine there exists a gulf so broad and deep, that it is impossible we can ever approximate. I shall therefore content myself, simply by stating his opinions, and any comment I may offer shall be the briefest possible. According to this author, then, fever being neither more nor less than a violent phrenitis, or inflammation of the brain and its membranes, our only hope of safety, as in other desperate phlegmasiæ, must lie in the unrestrained, and early, use of the lancet. With this view, the moment that any head symptoms appear, we are to bleed our patient to the extent of twenty, or thirty ounces at once, or if lesser evacuations be preferred, these are to be repeated till from eighty to an hundred ounces be speedily taken away. So imminent is the danger, that no impediment ought to deter us from this course, neither a feeble pulse, the most complete prostration of strength, nor even amendment in the sick, the last circumstance merely showing the necessity of the farther detraction of blood.

Such is a brief, but faithful summary of the plan of treatment recommended by Clutterbuck. To those who consider it with attention, the following objections, or queries, will hardly fail to suggest themselves.

1. As there occur many cases of fever in which there exists no head affection, at least none of any moment, in all such cases must not these profuse evacuations be accounted entirely useless, if not hurtful, or dangerous?

2. Since it often happens that typhus patients can neither digest, nor assimilate, food, during a long period, so as to furnish a supply of fresh blood to the vessels, while secretion going on, more or less plentifully, must act as a constant drain, is there not risk that detraction, at the beginning, of so enormous a quantity of the vital fluid, nearly one-third of the

become convinced that mercury exercised no specific power here whatever, and was serviceable merely by emptying the bowels, like any other cathartic.

whole, may exercise a baneful, if not fatal, influence over the future course of the disease?

3. Since it is acknowledged by the author that the cerebral affection in which he places the basis of fever is remediable only in its commencement, but when fully developed is wholly incurable by the most profuse evacuations; and considering that so much evidence can be brought to show that this affection in many instances is not actually based in real or genuine inflammation, why may not this intelligent physician be induced to try an altogether opposite course, and put to the test of experience the efficacy of cordials, and stimulants? That these have often succeeded there can be no question, and I can vouch for their success from my own practice. Not, gentlemen, that I consider them, by any means, infallible: on the contrary, if they have answered in numerous cases, they have also failed in many others. Whoever has seen much of this disease must have come to the conclusion that, every day, examples of it are to be met with, such as the neglected or protracted cases of which you have seen so many in this hospital, where all treatment is hopeless—where on the one hand, the pulse is so weak that we cannot bleed, and on the other, the vitality of the nervous tissue, in its great centres, the brain and spinal cord, is so thoroughly overwhelmed, or exhausted, as to be no longer reanimated, or restored, by the most powerful stimulants. If you ask me what is to be done in such emergencies, I frankly confess my ignorance. Medicine has as yet devised no remedy: or, if such be devised, it is wholly unknown to me.

The next of the inflammationists is Bateman. In his work, entitled "A Succinct Account of Contagious Fever," this author begins with, first, abjuring his old creed, and then proceeds to profess his new faith, with all the zeal of a neophyte, and not a little of the intolerance. Former practices, and former remedies are to be reprobated, of course, among the last, particularly, cinchona, and opium. Nearly a whole page is consumed in the condemnation of the last named medicine. Among other strictures he observes, "During the early and middle stages of fever, I think the necessity of absolutely

rejecting them (opiates) cannot be called in question." Further on, he adds, "In a word, they are decidedly injurious, aggravating, instead of relieving, the very symptoms which they are intended to remedy."* With respect to opium, gentlemen, it is a medicine of which you every day see the advantages, no defence therefore seems here necessary. Regarding cinchona, again, his opinions may be reckoned peculiar to himself, for he pronounces it a solecism in practice to prescribe it in the same disease along with wine,† a combination, though now unfashionable, sufficiently justified by old experience, and from these specimens we are not led to form a very high idea of this author's attainments in the *Materia Medica*.

Bateman's book seldom stoops to argument. It deals rather in general positions, which it promulgates under the shape of so many aphorisms, to be received with respect, and gratitude, by the medical world, but at the same time, without discussion.

Among the few scanty instances of detail, is the following case of a young lady. It is unfortunate, however, for the cause of the author, for instead of the benefits, it rather shows the evils of the depletory system. I shall give it in his own words:—

"I lately visited, in consultation, a young lady, in the 11th day of a relapse, but in the fifth week of fever, as she had been but a few days in the interim convalescent. At this period she was lying in a state of languor, and depression, articulating feebly, and, though then collected, had been rambling, and sleepless, during the two preceding nights, and complained much of a disturbance in her head. The pulse was nearly 130, small, and compressible, but with a smooth evenness in its beat; and the tongue was parched, and rather brown. A little weak wine had been given. Before my visit a free epistaxis had spontaneously taken place, which the nurse had busily checked with all expedition: it was remarked

* Pages 110, 111.

† Page 129.

by the medical attendant, that previous to this hæmorrhage, the tongue was tremulous when protruded, but was ever afterwards shown perfectly steady. Seven ounces of blood were taken from the arm at noon, the wine omitted, and a rhubarb draught, with saline medicines ordered.* At night she was found in a state of free perspiration, after a quiet sleep of four hours, manifestly improved in strength, speaking in a firmer tone, without any remains of the disturbance of the head, the pulse a few beats slower, but more soft, and full, and the tongue moist,—a condition in which it subsequently continued during the slow, but regular improvement of a fortnight more, before sufficient strength returned to allow the patient to quit her bed even for a short time.”

This patient who, with the exception of a short interval, had languished seven weeks under fever, at last was relieved by epistaxis, a crisis which, however we may explain it, is found to be one of the most favourable in the disease. Every symptom accordingly had subsided, so that all farther practice seemed superfluous, except in the eyes of a depletionist. But the young lady was not to escape without the usual routine of evacuation, and the antiphlogistic regimen; so accordingly the little wine, which had before been allowed her, was withdrawn, at the very period when it promised to be the most serviceable, and she was, at the same time, bled at the arm, and purged with salts, and rhubarb. The consequences were such as might have been expected. Instead of recruiting in a few days, as she might otherwise have done, we find her so thoroughly debilitated as not to be able to stir from her bed even for a single moment, during a whole fortnight afterwards.

In looking over the treatises of Armstrong and Bateman, we find these two authors mutually commending or eulogizing each other, and certainly, so far as typhus is concerned, their merits, or pretensions, must be considered as precisely equal.

* I take it for granted that the saline medicines here given along with the rhubarb, were purgatives.

Qui Bavium non odit, amet tua carmina, Mævi,

Et tu vitula dignus, et hic.

You will please to observe here, gentlemen, that what I say of Bateman, refers solely to his dogmatism, and opinions, on the nature, and treatment, of typhus. On other subjects he is a valuable writer, and well merits your attention. In particular, he is the able continuator of the system of skin diseases by Willan, which that author left unfinished, though unfortunately, he has perplexed us, by the same interminable multitude of genera, species, and varieties, that form so great an objection to the original work. Even his treatise on fever I recommend strongly to your perusal. After what I have said, his depletory doctrines will not mislead you, and you will find in it a fund of extensive, and useful, information, more especially, a curious history of the origin, and progress, of epidemic fever in the metropolis, and some interesting tables of the comparative rate of mortality from the disease, in the different London hospitals.

We come now to the last of the inflammationists, Dr. Mills, and he will not detain us long. Whatever may be said of his other pretensions, there can be no doubt that this gentleman was the father of a new practice in typhus, that of treating the disease by frequent, but very small, blood-lettings. Mills' publication was, by no means, without its importance. Though it advocates the inflammatory essence of typhus, and of course the necessity of blood-letting as one of its principal remedies, yet it dissuaded from the merciless venesections to which the sick had been before exposed from the ultra inflammationists, and the results of the practice it recommends serve to throw additional light on the actual nature of fever.

In Smollett's Romance of Roderic Random, we meet with a personage, by name Captain Whiffle, who being of a delicate constitution, when he was let blood, never suffered more to be taken at a time, than an ounce, and three drams, and, that there might be no mistake, he had the weight carefully ascertained by an accurate pair of scales. Were prophecy

allowed in medicine, Whiffle might be considered as a type, or precursor, of Mills. The method of both was almost identical. The Captain's quantity of blood was an ounce, and three drams, but, to make the reckoning even, we shall call it an ounce, and a half; the Doctor's quantity was five ounces, no material difference, since with many patients, I am disposed to think, it would be matter of very little moment to which of the two evacuations they had been subjected.

Mills' book is entitled "An Essay on the Utility of Blood-letting in Fever," and in looking over the list of venesections set down in his tables, I find the quantity of blood taken to vary from six to four ounces. Such is the ordinary measure, in by far the majority of instances. The number of such blood-lettings enumerated is about 431, 250 where six ounces, and 181, where four ounces, were drawn off. From these data, I concluded, that five ounces, or five ounces and a half, might be esteemed a fair average of Mills' depletions. In different cases, we find the proportion both below, and above, this standard, but the exceptions are comparatively few.

Mills is an equally zealous inflammationist with the rest of his sect, maintaining like them, that typhus is nothing more than a phlegmasia, and his subdivisions of the disease are founded on this principle, as into cephalic, pulmonic, hepatic, gastric, &c., &c., according as the brain, liver, lungs, stomach, might happen to be the organ inflamed. He apologizes to his brother depletionists, for the very small or trifling bloodshed his practice enjoins, and he founds his excuse on the following aphorismal remarks:—

That the advantages to be derived from blood-letting do not depend on the quantity taken away.

That the removal of pain, heat, or uneasiness, in the organ, or organs, diseased, is the effect to be produced.

That if this can be accomplished by the abstraction of a few ounces of blood, any further depletion, at that time, will be injurious.*

I have all along inculcated, gentlemen, that our low typhus,

* There will be found no little good sense in this last observation.

in its nature, and essence, is an asthenic disease, and that, from this inherent quality, when it happens to be casually joined with an inflammatory distemper, it robs the inflammation of more than half its activity, so that the complication produced requires for cure, far more moderate depletions than would be necessary, did the same sthenic malady exist by itself, or apart from the fever. Mills' practice may be considered as a practical commentary on this text, affording an additional proof of its truth, and accuracy. Thus numerous cases are adduced by him, to show that various sthenic affections, when joined to fever, yielded readily to his small blood-lettings. But were we to encounter any decidedly acute phlegmasia, separately existing, say a violent pleurisy, or phrenitis, would any rational person trust for cure to the Whiffle practice, or expect to subdue the disease by taking away, at a time, five ounces, or five ounces and a half, of blood? No rational person would look for such result.* The justice of this reasoning, as founded on the asthenic nature of typhus, is farther confirmed by the practice of the experienced Grattan. That able practitioner, in 116 pectoral, and 98 head, cases, ordered, in the first set, only five ounces and a half, of blood, to be taken from a vein, and in the second, still less, namely, five ounces, to be abstracted from the temporal artery, yet with every benefit that could be desired. There can no doubt remain, therefore, that since typhus is thus found to exercise, so forcibly, a restraining, or neutralizing, power over the most serious inflammation, it must, in its own nature, and essence, be asthenic, so that this new practice of Mills, instead of assisting the partisans of depletion, is in direct hostility to their cause, and furnishes an additional argument against it. Nay, upon this subject, gentlemen, I venture a step further, by saying, that we sometimes appreciate at too low a rate, the sedative power of typhus when thus joined to a phlegmasia, so as to resort needlessly to the lancet. To countenance this

* It is very surprising that Mills, in his book, should be so exceedingly anxious to prove typhus an inflammatory disease: were it actually such, it must evidently reduce his own practice to a mere farce.

idea, we learn from the same eminent physician I have so often quoted, Dr. Grattan, that though he had under his care, 316 pectoral, and 268 head, cases, yet he did not think it necessary to order blood-letting for more than 116 of the former, and 98 of the latter. No less than 200, therefore, the balance of the first class of patients, and 170, the residue of the second, were not bled at all, yet recovered in as great proportion as those who had actually undergone the operation, at least, the practitioner says nothing to the contrary. This is a circumstance of no little practical importance, since, were its truth established, it cannot but throw some shade of doubt on the necessity of phlebotomy in the other instances where it was practised. The sagacious Dr. Hagan informs us, that of 100 typhus patients he dismissed cured, of whom only two were bled, each of the other ninety-eight would have borne, without material detriment, the loss of five, or six, ounces of blood, and that had such operation been performed, the cure would have been imputed to that depletion alone. It was formerly stated that 516 inmates had been discharged this hospital perfectly restored to health, though they had experienced no treatment but an occasional laxative, with an opiate, at night; now I have no hesitation in saying, that each of these, with very little, or no injury, might have been bled to the same extent, and I need not tell you, that every inflammationist in the kingdom would have immediately insisted that it was to this evacuation solely they owed their recovery. You must endeavour to guard your minds, therefore, against fallacies of this sort, by which we are all too apt to be misled. In treating a disease, you must ever keep in view its natural course and tendency, and must always recollect that remedies and recoveries do not always exist in the relation of cause, and effect.

So much, then, for the argument of the inflammationists, founded on the supposed benefit of blood-letting in fever. You must long ago have perceived that so far from supporting, it invalidates their cause, and in fact turns against themselves. Let us now consider what assistance they derive from

their other mode of evacuation, that of purging. Here they will be found equally unfortunate, for their whole reasoning may be shown to rest on a mere error in therapeutics.

Thus it is taken for granted, as a general principle, that the action of this class of medicines is necessarily and invariably, debilitating, than which no position is more untenable in the whole materia medica. No doubt, if so administered as to cause profuse, and reiterated, discharges of watery fluid from the intestines, they will prove weakening, but never, if properly managed, or as they ought to be given, in ordinary cases of typhus. On the contrary, when so prescribed as merely to unload the bowels, and perhaps in some measure to correct their secretions, instead of debilitating, they are highly invigorating. We see this, every day, in various diseases. I shall merely mention one where their benefit is universally acknowledged, I mean amenorrhœa, or chlorosis. No disorder exhibits more unequivocal marks of debility than this, as exemplified by the whole train of symptoms, as the bloated, and greenish-pale, countenance, the œdematous limbs, the quick, and feeble, pulse, alternating with frequent palpitations, and occasional syncope, on the slightest exertions, the total prostration of appetite, and digestion, the obstinately constipated state of the bowels giving rise to the prodigious accumulation of fœces within—this last reckoned the more immediate source of the entire distemper. Mark what happens when purgatives are so exhibited as merely to remove the load, prevent the future retention of excrementitious matter, and perhaps amend the intestinal secretion. All the symptoms yield by degrees—appetite, digestion and strength return—the pulse becomes natural—the œdema vanishes—the countenance recovers its wonted hue, and general health is restored. It is exactly on the same principle that purgatives become serviceable in typhus, that is, by unloading the bowels, and by preventing the accumulation of those vitiated intestinal secretions so apt to occur during this disease. Such is the view taken by the great authority on this subject, Dr. James Hamilton of Edinburgh, and those who have imputed to him any other doctrine, have either

misunderstood, or misrepresented, his opinions. To prevent all possibility of dispute here, I shall subjoin his own words. Speaking of purgatives as a remedy for typhus, he observes, "It is now some years since I have relinquished almost entirely the use of emetics, and glysters, in fever; I trust to a purgative medicine to ensure a regular alvine evacuation, although the *daily exhibition of a purgative for this purpose is not always required.*"* Farther, "The complete, and regular, evacuation of the bowels in the course of fever, is the object to be attained, and he adds, that so managed, instead of aggravating, they lessen the debilitating effects of the disease. *Within this limit,* he proceeds, I have had much satisfaction in prosecuting the practice; nor have I, in a single instance, had occasion to regret any injury proceeding from it; *for I am not an advocate for exciting unusual secretion into the cavity of the intestines, and for procuring copious watery stools:* these while they are not necessary, might increase the debility so much dreaded."†

It thus appears, gentlemen, that the action of purgatives, unless when so pushed as to cause profuse discharges of watery fluid, is invigorating, not debilitating, and consequently the argument of the inflammationists to show that typhus must be a sthenic malady, because such remedies are serviceable in its treatment, must necessarily fall to the ground. So far as regards the real essence, or nature, of the disease, the fair inference is just the contrary.

III. The only other speculative points that remain for discussion, are the two following: one, how far is fever subjected for its production, and phenomena, to the different seasons of the year; the other, how far is it ruled in its course, by the influences of the sun and moon, and other heavenly bodies. The theoretical portion of the lectures, however, has run out to such an enormous, and unexpected, length, that I must waive at present entering into either controversy. With respect to the last, it will be sufficient to remark, whatever may

* Hamilton on Purgatives, p. 34.

† Ib. pp. 35, 36.

be said of other fevers, that a real contagious typhus, after it has fairly begun, is not likely to be altered much, either in its course or termination, by the influences of the sun and moon, whether exerted separately, or combined.

I proceed now to the last branch of our subject, the general results of the practice pursued, and the prophylaxis. I shall begin with the former.

The first topic that claims attention here, is the rate of mortality. All of you who have looked over the Reports of different Hospitals, must be struck by the discrepancies they present in this respect, though under the most skilful, and often, the same physicians, and in a disease the different epidemics of which are believed to be essentially the same. The extracts that follow from some of the principal among the English and Irish Hospitals, will place this fact in a sufficiently clear point of view.

LONDON HOSPITALS.

London House of Recovery. Physicians, Bateman and Armstrong. From the opening of this Institution, in 1802, till 1816, the proportion of deaths, according to Bateman, was from 1 in 6 to 1 in 12: according to Armstrong from 1 in $3\frac{5}{4}$ to 1 in $12\frac{1}{2}$;* from this period till end of 1818, it was from 1 in $12\frac{1}{2}$, to 1 in $13\frac{1}{2}$.

London Public Dispensary. Physician, Dr. Willan. During autumnal months of 1799, nearly 1 in 2, or one-half.

Guy's Hospital. Physician, Dr. Marcet. From May 1816, till April 1817, 1 in 4; from May 1817 till April 1818, only 1 in 15.

London Hospital. Physician, Dr. Yelloly. From 1812 till 1817, 1 in 5—in 1817, 1 in $7\frac{1}{2}$ —the first three months of 1818, 1 in $17\frac{1}{2}$.

Westminster Hospital. Attending physician, Dr. Tuthil. In four years, average 1 in 10. In 1818, 1 in 19.

Middlesex Hospital. Attending physician, Dr. Southey.

* Armstrong on Fever, p. 226.

In whole of 1818, average 1 in 13; but during last six months of same year, only 1 in 16.*

DUBLIN HOSPITALS.

Cork Street Fever Hospital. Attending physicians, Drs. O'Brien, Grattan, Stoker, Barker. In 1804 1 in $13\frac{2}{13}$, 1805 1 in $10\frac{1}{7}$, 1806 1 in $12\frac{27}{81}$, 1807 1 in $11\frac{30}{31}$, 1808 1 in $11\frac{9}{8}$, 1809 1 in $13\frac{3}{80}$, 1810 1 in $11\frac{2}{9}$, 1811 1 in $12\frac{95}{113}$, 1812 1 in $13\frac{4}{18}$, 1813 1 in $16\frac{52}{63}$, 1814 1 in $16\frac{15}{41}$, 1815 1 in $19\frac{40}{91}$, 1816 1 in $16\frac{05}{73}$; from January 5, 1817, till April 30th, 1819—deaths of males, 1 in 16 and a fraction—of females, 1 in 20 and a fraction.†

Hardwicke Fever Hospital. Attending physicians, Drs. Perceval and Cheyne. From November, 1813, till November, 1814, treated by Dr. Perceval, 289 cases of typhus, deaths 24; 362 cases of synochus, deaths 6; 122 cases of febricula, deaths 2; total admissions, 773, total deaths, 32, or 1 in 24.‡ In same hospital, under Dr. Cheyne, from April, 1816, till March, 1817, admissions 780, deaths 53, 1 in 14 and a fraction.¶ In same hospital, from April, 1817, till March, 1818, admissions 701, deaths 43, or 1 in 18 and a fraction.§ From Dr. Grattan of Dublin, we have the mean rate of mortality in four different receptacles for fever in that metropolis, and also the average of deaths in the whole hospitals for typhus in the city, during 7 immediately consecutive periods of three months each. The four receptacles are the House of Industry, where the deaths were 1 in 21; Cork Street Hospital, 1 in 30; Steeven's Hospital, 1 in 50; Sir Patrick Dunn's, 1 in 28. The average of fatal casualty among the whole hospital fever patients in Dublin, was during the first period of three months, ending 30th November, 1817, 1 in 16;

* See Bateman on Fever.

† Irish Med. Trans. Vol. i. Report by Drs. O'Brien and Grattan, pp. 409, 461—by Dr. Stoker, pp. 82, 401—Rep. by Dr. Barker, ib. Vol. ii. pp. 568, 569.

‡ Ib. Vol. i. p. 316. ¶ Cheyne in Dub. Hosp. Rep. Vol. i. p. 64.

§ Ib. Vol. ii. p. 74.

during the second period, below 1 in 15; during the third period, above 1 in 24; during the fourth period, below 1 in 32; during the fifth period, 1 in 22, nearly; during the sixth period, 1 in 19; during the seventh period, less than 1 in 18. Taking all these periods into one aggregate, the proportion of deaths might be estimated about 1 in 22.*

PROVINCIAL IRISH HOSPITALS.

Besides the Belfast Receptacle for Fever already mentioned, the only others in which I think it necessary to notice the rate of mortality are those of Cork. Into what is termed South Fever Asylum of that city, attending physician Dr. Pickells, there were admitted, from December, 1817, till February, 1819, 3018 patients; the deaths, deducting 9 who died immediately after admission, were 126, or 1 in 20 and a fraction.† Another Cork physician, Dr. Reid, in counting the number of fever patients admitted into this and other asylums, reckons every case a fever, from the slightest febricula, to the most malignant typhus.‡ Under Dr. Barry, another Cork physician, Grattan tells us that the deaths were 1 in 26, and he adds that in his own hospital, during the year ending 4th January, 1819, they were only 1 in 30.

These examples, gentlemen, will be quite sufficient to show you the very different rates of mortality that occur in different fever hospitals. The most striking discrepancies are those that exist between the English and Irish establishments. This will be best seen by taking the extremes. Thus, in Guy's Hospital, London, under the attendance of Dr. Marcet, the deaths in 1816 are said to have been 1 in 4, and in the London Public Dispensary, under the charge of the celebrated Willan, during the autumn months of 1799, no less than 1 in 2, or a half; while in Dublin, during 1817, according to Perceval, the average of the whole receptacles was only

* Irish Med. Trans. Vol. iii. pp. 337, 338, 339, 399, 400.

† Ib. p. 229.

‡ Ib. p. 9.

1 in 22, and in Steeven's Hospital, taken separately, if the printing be correct, only 1 in 50. In all this there is something difficult to understand. Without the slightest insinuation of misrepresentation or unfairness, there can be no question that the Irish physicians use the term fever, in a much laxer sense than we do in Great Britain. Thus, Dr. Perceval tells us that in 1813 he had under his care 122 instances of febricula, deaths only 2, (1 in 61); and Dr. Reid of Cork informs us that in the asylums of that city they counted every thing fever, from the slightest febricula to the most malignant typhus. In Dr. Perceval's list, too, of the above-mentioned year, I see 362 cases of what is termed synochus, the deaths only 6 (1 in 60). The extreme pauperism known to prevail in Ireland during seasons of peculiar distress, may serve in some measure to explain the above circumstance, as they must relax or supersede, all the usual barriers to hospital admission. Such is the destitution on these occasions, as already stated, that many persons, braving all risk of infection or death, feign themselves ill of typhus, that by getting within an infirmary, they may obtain a morsel of food.

After these details, which may be considered, at once, curious, and important, it is time now we attend to our own rate of casualties. These, unfortunately, have been very great.

By the Journals it appears that from 1st January, 1831, till 1st July, 1832, a period of eighteen months, there have been received into our wards 1171 patients, viz., 798 males, and 373 females. The deaths among the first have been 96, among the second 36, amounting in all to no less than 132. You must not here suppose, however, that this large mortality has been owing entirely, either to the untractable malignity of the disease, or to the inadequacy of the remedies employed. This would be leading to a double error. On the contrary, a large proportion of our deaths, perhaps one-half, has arisen from causes quite unconnected with either circumstance. In fact, gentlemen, besides the usual danger of the malady, we have had to encounter no less than *four extra* sources of

fatality, some of them common to other fever hospitals, others peculiar to ourselves. Of these it will be proper now to give some account, as well as to explain their effects.

1. The first is the intrusion into our wards of cases not typhus at all, but of disorders worse than typhus. Our deaths from this source have been 19; 13 of males, 6 of females. The disorders themselves, among the first, were pneumonia, 2 cases; phthisis pulmonalis, 5; ulcerated intestines, 1; organic affection of spleen, 1; organic affection of liver, 2; hydrocephalus internus, 1; sanguineous apoplexy, 1; while among the second they were phthisis pulmonalis, 2 cases; hydrothorax, or pectoral effusion, 3; bronchitis, 1.

2. The second source of extra fatality was the complication of other maladies with typhus, producing a compound of a more deadly nature than the fever by itself. Our lethal terminations from this cause were also 19, viz., 14 among the males, 5 among the females. The diseases thus in alliance with the epidemic, among the first, were, pneumonia, 2 instances; phthisis pulmonalis, 5; hydrothorax, or pectoral effusion, 2; ulcerated intestines, 1; peritonitis, 2; organic disease of heart, 1; meningitis, 1: among the second, phthisis pulmonalis in 2 instances; bronchitis, with diseased heart, 1; pneumonia, 1; hydrothorax, 1.

3. A third cause of fatal issue was the advanced stage of the disease at the time of admission, or its protraction beyond the curable period. This in fact was the grand source of our casualties, and it never any where operated with more deadly sway than in the Glasgow fever hospital. The exact amount of extra deaths from this cause it is impossible to calculate; some approximation to the truth may be gathered from the following facts. Thus it appears by the Journals, that among the males, such was the hopeless state from long continuance of the fever, 2 expired almost instantly after entering the house, 6 the next day, 16 the day following, and 10 the day subsequent—in all 34; while among the females, 2 died on the first day after admission, and 7 on the second; thus forming an aggregate of 43 who were either moribund, or next to moribund, at the time they were committed to our charge.

In fact, gentlemen, it may be some satisfaction for you to know that we lost very few, comparatively speaking, whom we had an opportunity of treating during the early, or curable stage of the disease. Thus, out of our 132 deaths only 18 occurred in persons who had been admitted to our wards previous to the seventh day of the fever, according to the usual mode of reckoning, or to the tenth day, if we include the latent period, while the remaining 114 fatal terminations were all of patients who had not been received till a later, generally a much later epoch, of the distemper. This I consider as a most important fact, because it not only shows the power of our art in typhus, when its resources are timely administered, but the danger of allowing the malady to linger on without medical assistance. Neither must it be supposed that the above 43 patients who, from the protracted term of the malady, sunk so soon after admission, were the only sufferers from this cause. A multitude of others who survived longer, and were merely kept alive, for a while, by dint of wine, and spirits, were no less the assured victims of the same fatal circumstance, the force of medicine here only postponing, not averting, their fate. Of the dreadful extent to which this source of mortality has been allowed to operate, during the present epidemic, you will be able to form a judgment when I show you the very late period at which the majority of the sick were received into our hospital. Thus of our 798 males none, except 196, were brought to us sooner than the seventh day of the disease. When I say the seventh day here, I speak within too narrow limits, counting, as is wont in our Journals, from the time when the patient was first confined to bed. But I have already shown you that the disorder almost always commences much earlier, and if we estimate what may be called the latent period at three days, a calculation rather below, than above, the truth, it will thus appear that six-sevenths of our male patients did not, in fact, come under our management sooner than the tenth day, a period after which, as every practitioner knows, but little can be done in typhus. This was the minimum epoch: but it will be found by the Table below that no less than 167 were not received till the eleventh,

nominally the eighth day ; 77 not till the twelfth, nominally the ninth day ; 94 not till the thirteenth, nominally the tenth day ; and that no fewer than 27 were detained till the twenty-fourth, nominally the twenty-first day, and, by remaining in their own houses, were thus suffered, all the time, to disseminate contagion every where around them, for the long period of more than three weeks.

The same tardiness of admission is no less conspicuous among our females. Four-and-twenty of them were detained at home, spreading contagion all the while, till after the twenty-fourth, nominally the twenty-first day, while of the whole number 373, only 56 reached the hospital previously to the seventh, but in reality the tenth day of the fever. Upon the whole, throwing both males and females into one aggregate, it will be found from the Table, that of the sum total 1171, no less than 1010, instead of being sent to the Infirmary in time to receive any benefit, were withheld from us till they had already lingered at least ten days under the distemper, and till it had taken such deep root as to be but little amenable to medical prescription.

4. The last source of extra mortality I am to notice, is one peculiar to ourselves, and quite unknown to other hospitals : it is the interposition of a new set of practitioners, who, in consequence of their appointment, by public authority, to treat typhus patients in their own houses, have thus acquired the virtual right of putting a veto, in the first instance, on their transmission to the Infirmary. The effect of such regulation is abundantly manifest. If the cases be mild and promising, they will be retained at home, nor can any blame be attached to the proceeding ; while, on the contrary, if they be desperate, or dangerous, they will find their way to the hospital. How much this circumstance must have swelled our bill of mortality, requires no illustration. Instead of getting the usual proportion of good and bad cases that occur in every epidemic, so far as the bounds of the Royalty extend, the latter only fall to our lot—the curable ones, at least a large proportion of them, are regularly intercepted in their road to this establishment—the refuse only remains—and we

are thus left to practise as we may, on what might be justly enough termed the mere offals, or garbage, of the distemper. What proportion of our deaths is attributable to this source, it may be impossible to calculate; every person must perceive, it must have been no slight source of mortality.*

Such, gentlemen, are the powerful obstacles that have impeded our efforts for restoring to health the patients you have seen treated in our fever wards. A number of them were not typhus at all, but of a more dangerous nature; others had the disease complicated with some additional malady, well known to be incurable, as pulmonary consumption; while a third set, though wholly fever, yet from causes over which we had no control, more especially the long protraction of the malady, were of the worst possible description. How much this last circumstance contributed to the fatal event, a fact already stated affords the clearest evidence, namely, that out of our 1171 patients only 18 died whom we had an opportunity of treating during the early, or curable, stage, that is, previous to the seventh, or more correctly speaking, and taking in the latent period, antecedent to the tenth day of the distemper. Now, gentlemen, if you consider all these unfortunate coincidences, you will probably neither be surprised, nor shocked, at the amount of our lethal casualties, but rather with me think it matter of pride, and thankfulness, that our deaths have not been more numerous than they have actually turned out. At any rate, it was proper to lay before you every information possible relative to the condition of our patients,

* In the Glasgow Medical Journal will be found a regular account, by the District Surgeons themselves, of all the cases of typhus patients treated in their own houses, within the currency of the last five years, that is, from 1st August, 1827, till 31st July, 1832. The success of these gentlemen, as appears by the rate of mortality printed in their report, is of the most striking description, and far exceeds any thing we can boast of in the Infirmary: thus in 1828-29, their deaths were only 1 in 55, an amount of fortunate issue, I believe, quite unprecedented in the history of typhus. It shows clearly, however, when contrasted with our own melancholy results, that all, or nearly all, the mild, and favourable, cases occurring within the bounds of the Royalty, must have been regularly intercepted in their road to our wards, and that those sent us must have been merely the off-scourings, or off-strainings, of the distemper.

as well as the practice pursued, and with respect to the last, as I have no concealments to make, and no professional purposes to serve, I was equally anxious to show you its unsuccessful, as its successful, results.

Before I conclude, you may probably expect that I should exhibit formally to you our rate of mortality, as in other hospitals. In forming this, you will easily see what large allowance must be made for the untoward circumstances in which we have been placed, many of them serving to baffle every effort of medical skill. Thus among our deaths we had 19, not from typhus itself, but other diseases admitted by mistake for typhus, and other 19 from its complication with additional maladies, so as to form a compound of a more deadly nature than fever by itself. The first of these, it is evident, cannot be imputed at all, and the second only in part, to our epidemic simply considered. Here then we might perhaps make a deduction of 38 from the fatal casualties, of our low fever. Farther, we had 43 moribund cases few of whom could have been saved, unless we had possessed the power of working miracles, besides an additional number whom we kept alive, a short space, by dint of wine, and whiskey, but who finally sunk, because, in fact, they were equally moribund with the others, at the time they were committed to our charge. With all these deductions, our deaths, you will probably agree with me might in all fairness be reduced, at least one-half, or from 132 to 66, so far as curable cases were concerned, so that our rate of mortality might be stated at about 1 in 17 and a fraction. But really, gentlemen, estimates of this sort are of very little consequence, they are so liable to be influenced by a thousand circumstances over which the practitioner has no control, and many of which no medical skill can surmount: I have, however, laid the whole facts and data before you, without disguise or reservation, so that each of you is in full capacity to draw his own conclusions.

I shall now submit to you a brief summary, or analysis, of some of our principal fatal cases, with dissections.

TABLE OF ADMISSIONS.

MALE PATIENTS.			FEMALE PATIENTS.		
Nominal Day of Admission.	Actual Day, or after addition of Latent Period.	Number admitted.	Nominal Days' Admission.	Actual Day of admission, or after addition of Latent Period.	Number admitted.
2	5	1	2	5	2
3	6	13	3	6	6
4	7	33	4	7	24
5	8	65	5	8	24
6	9	84	6	9	28
7	10	74	7	10	39
8	11	167	8	11	69
9	12	77	9	12	30
10	13	94	10	13	36
11	14	31	12	14	19
12	15	31	12	15	14
13	16	7	13	16	5
14	17	68	14	17	34
15	18	2	15	18	5
16	19	3	16	19	4
17	20	2	17	20	0
18	21	1	18	21	2
19	22	2	19	22	2
20	23	1	20	23	0
21	24	27	21	24	24
28	31	6	28	31	2
		789			369
		9			4
		798			373

9 added to the Males, and 4 to the Females, whose day of admission could not be ascertained, and therefore left blank in Tables.

I. *Cases of Extraneous Diseases, or not of Typhus at all.*

Pneumonia. *David Gunn*, æt. 32, Vol. xxiv. p. 60, admitted 8th day after seizure, when we found him quite delirious, he died early on the third day after that of admission. The right

lung was completely hepatized, and adhered firmly both to the ribs all around, and to the diaphragm.—*James M'Alister*, æt. 52, Vol. xxvii. p. 214, admission on 12th day—was completely delirious, lay supine in bed, and died on 2d day—right lung greatly increased in weight and density, and infiltrated with bloody serosity. Strong adhesions all round—left lung completely hepatized, and poured out pus on incision—seemed case of old pneumonia.

Phthisis pulmonalis. *John M'Donald*, æt. 35, Vol. xxv. p. 114, admitted on 7th day.—Dissect. In left lung numerous tubercles, and large abscess.—*Alex. Muirhead*, æt. 43, Vol. xxv. p. 136, admitted on 6th day, numerous tubercles and pus effused between pleuræ on right side.—*James M'Gonegal*, æt. 26, Vol. xxvi. p. 204, no inspection, but decidedly phthisical on admission, which was on 8th day.—*John Gibson*, æt. 29, Vol. xxvii. p. 20, admitted a fortnight after seizure, with cough, expectoration, delirium, and fluttering pulse—in left lung suppurating tubercles with hepatization around—some small ulcers besides in arch of colon—in fact, a case of phthisis in last stage.—*John Millar*, æt. 36, Vol. xxvii. p. 53, admitted on 8th day, with great dyspnœa, purulent expectoration, and pulse all but gone, died on second day after that of admission—effusion into right side of thorax, lung of same side entirely hepatized, and large abscess in inferior portion. This man sent to hospital in closing scene of phthisis.

Ulcerated intestines. *Neil M'Gregor*, æt. 31, Vol. xxv. p. 31, admitted 8th day, died at 3 o'clock morning of 3d day, numerous small ulcerated patches in intestines. During short time he survived, no purging.—*James M'Kenzie*, æt. 26, Vol. xxvi. p. 63, admitted 16th day—intestines adhering together by coagulable lymph, soft and blackish, and perforated by numerous ulcerations, into cavity of abdomen—mesenteric glands hard and enlarged.

Softened spleen. Two cases where this the only morbid appearance, one connected, other unconnected with contagion, last case was of *John Allan*, æt. 38, Vol. xxvii. p. 100, admission 10th day: what influence this condition of spleen had on fatal event, not easy to appreciate.

Diseased liver. Two cases, one of them *James Harvey*, æt. 26, Vol. xxvii. p. 166, admission 7th day, when jaundiced all over. Dura mater yellowish, and yellow fluid between convolutions of brain—liver much enlarged, brittle, and friable, gall-bladder distended with dark, viscid, bile, not properly examined as ought to have been whether duct patent. In addition, right lung completely hepatized, and adhering all round—spleen, grey, pulpy, and double of natural size.—The other case was that of *James Pedie*, æt. 38, p. 181, admission 14th day. This case complicated with cough, cynanche, and violent headach, pulse hard, was bled both from arm and temporal artery. In history of case could not trace contagion, but after last bleeding, petechiæ appeared, and patient speedily sunk, so that likely to have been contagious typhus from first. Besides usual marks of congestion in brain, liver large and friable.

Hydrocephalus internus. One case *Wm. Boyd*, æt. 13, Vol. xxvii. p. 178, admission 8th day.—Inspect. Head large, and mis-shapen, dura mater adhering to bone, large quantity of fluid in two lateral and third ventricles, the lining arachnoid thickened—veins on surface of brain, and longitudinal sinus engorged; much serous fluid lying between convolutions—on pons varolii and at decussation of optic nerves, arachnoid greatly thickened with gelatinous deposition below. Decided hydrocephalus with usual symptoms, and treatment, and usual fate. No evidence of contagion, and indeed no connexion with typhus.—*Hugh Cleland*, æt. 30, Vol. xxvii. p. 66, admission 6th day, died suddenly 2d day after, no treatment but castor-oil and opiate—on posterior lobe of brain large mass of blood, partly coagulated, between arachnoid and pia mater, brain below deep red colour, effusion into ventricles.

II. *Fatal Cases in which other Diseases complicated with Typhus.*

Pneumonia. *Thomas Angus*, æt. 46, admission not till 3 weeks after seizure, Vol. xxv. p. 69—hepatization of left lung with hydrothorax, complicated in this case with remarkable

change of spleen, which was found of no firmer consistence than coagulated blood.

Charles Gallocher, æt. 55, Vol. xxv. p. 128—Hydrothorax, with remarkable softening of lungs, liver, and spleen—have styled this pneumonia for want of a better name—diseased state of liver and spleen, at least of former, may have contributed to the fatal event as much as that of lungs.

Phthisis pulmonalis. *John Mackie*, æt. 16, Vol. xxv. p. 85, admitted 6th day, scrofula, swelling of neck. Right lung partially hepatized, and both lungs filled with tubercles in different stages of advancement—effusion into both sides of chest—typhus engrafted on phthisis.—*John Picken*, æt. 45, Vol. xxv. p. 41, admitted 8th day, singular state of right lung—on being cut into, broke down into substance like coffee grounds, mixed up with pus and blood, had besides large ulcer of four years' standing on one of legs.—*Alexander Mason*, æt. 22, Vol. xxvi. p. 179, admitted 8th day, died on 3d day after that of admission. Tubercles in second stage—covered with petechiæ, so that little doubt that typhus here engrafted on phthisis.—*Robert Edwards*, æt. 40, Vol. xxvii. p. 70, admitted 9th day, with cough and purulent expectoration, died on morning of 4th day after that of admission, so that had reached all but end of his career before he was sent to the hospital—strong adhesions between pleuræ of left side, and left lung in its lower portion full of suppurating tubercles communicating with bronchiæ—typhus engrafted on phthisis.—*Malcolm M'Bryde*, æt. 37, Vol. xxvii. p. 79, admitted 9th day—much reddish brown effusion into left side of thorax; lung itself completely hepatized, and in upper portion a large abscess—may be considered either as phthisis or pneumonia—contagion distinctly traced, so that whatever it may be esteemed, had typhus for companion.

Hydrothorax. *John Cassidy*, æt. 41, Vol. xxiv. p. 147, admitted on 11th day, unusual congestion of brain with effusion into ventricles—4 lbs. reddish fluids in chest, and oozing of similar fluid in great quantity from lungs when cut into—heart enlarged and flaccid, death on 5th day after that of admission—typhus here apparently engrafted on hydrothorax—

no pectoral symptoms except slight catch at bottom of sternum, which soon subsided, and no anasarca or ascites—diuretics not tried; indeed hardly time or opportunity.

Peritonitis. *George Campbell*, æt. 58, Vol. xxiv. p. 115— not admitted till 21st day. Sero-purulent fluid, and flakes of coagulable lymph found on surface of peritonæum, but intestines sound—though pain of abdomen on 4th day after admission, that is, on 25th day of disease, pulse too feeble for bleeding.

John Kelly, æt. 61, Vol. xxv. p. 18, admitted 14th day— principal symptoms, bilious vomiting, hiccup, but no purging—intestines matted together, and adhering to peritonæum by layers of coagulable lymph, small abscesses discovered on separating folds—small ulcer discovered as cardiac orifice of stomach, hiccup in all probability owing to this last—only instance have met with of this source of hiccup in all my dissections of fever.

MALES.—*Results.*

Softened spleen. *Wm. Miller*, æt. 50, Vol. xxvi. p. 213, admitted 8th day—on inspection, nothing remarkable, except state of spleen, which at once much larger and softer than natural.

Diseased heart, followed by slight hydrothorax and obstructed lungs. *Henry M'Lean*, æt. 25, Vol. xxvi. p. 198, admitted 6th day—symptoms, throbbing of pulse, with much headach and delirium—treatment, leeches, blisters, saline diaphoretics—right ventricle both dilated and hypertrophied, but principal disease in aortic valves, which so small as unable to perform function—slight effusion into both sides of chest, and inferior third of each lung hepatized—this of course an old disease, and as contagion clearly traced, evidently typhus ingrafted on organic disorder of heart.

Meningitis, 1 case. *Thomas Liddle*, æt. 28, Vol. xxvii. p. 228, admitted 8th day, when delirium, with full and throbbing pulse, ʒv. blood from temporal artery—delirium ceased, but recurred with more violence next day, followed by coma, under which sunk on 3d day after that of admis-

sion, notwithstanding blister to head and soles of feet. Complete meningitis, for all membranes of excephalon thickened, arachnoid at base of brain as thick as dura mater, with effusion below, in ventricles much serous fluid, substance of brain softened. Did not get patient till 8th day, and impossible to say when mischief began, contagion clearly traced, and most probable opinion seems to be that case of typhus ingrafted on meningitis.

Such are the principal instances among the males, where other diseases had been introduced into the hospital in place of typhus, or where typhus had been complicated with other diseases. In one example besides, *Daniel Brown*, æt. 43, Vol. xxiv. p. 144, admitted 14th day, head said to have been previously injured by a fall: and in another, that of *Thomas Gallocher*, Vol. xxiv. p. 170, pectoral symptoms, it was reported, had existed during a considerable period before his reception into the Infirmary, but in neither case was the information sufficiently full and complete, nor had we an opportunity of verifying its accuracy by dissection.

FEMALES.

a. Extraneous diseases unconnected with typhus.

Phthisis pulmonalis. Ann Cameron, æt. 30, Vol. i. p. 44.—This patient had been a month confined to bed with phthisis, terminating as usual in diarrhœa—at this time she was put out of her lodgings, and sent to the Police Office—here her case was certified as one of fever by the surgeon, and she was sent to the Infirmary. She survived three days—on inspection, we found her lungs full of tubercles, many of them in a state of suppuration, and communicating with the bronchiæ, the surrounding lung hepatized—liver enlarged—a large dark ulcer near caput coli—the diarrhœa we found had been of 3 months' standing.—Ann Grant, æt. 31, Vol. ii. p. 162, admitted 10th day, and died in 4th day after admission—right lung contained tubercles on all the different stages, and there was effusion into same side of chest—uterus enlarged, with tumour about size of walnut adhering to parietes.

Hydrothorax or *pectoral effusion*. Mary M'Nicol, æt. 20, Vol. i. p. 95, admission 7th day—no pectoral or dropsical symptoms till day of death, which 10th after admission, when breathing became oppressed, lips livid, with anasarca of left hand and arm—on inspection, ζxvi. of fluid in cavity of chest, ζii. in that of pericardium, and a pint in that of abdomen—considerable effusion between arachnoid and pia mater, and in ventricles—there was slight redness of posterior pleura costalis—how long before death these effusions supervened, not easy to tell—diuretics were not tried.—Mary Pettigrew, æt. 15, Vol. ii. p. 153, admission 14th day—considerable effusion into both sides of chest—lungs œdematous, and upper portion of left hepatized—mucous membrane of intestines florid, and softened.—Have set down this case among those unconnected with typhus, perhaps erroneously, for purple vibices on different parts, and large one on left foot.

Mary Picken, æt. 52. p. 192, admission 9th day—considerable effusion into both sides of thorax, with appearance of hepatization on top of both lungs, liver preternaturally hard—no disease of membranes, no pectoral symptoms.

Bronchitis. Jas. Kilpatrick, æt. 21, p. 124, admitted 8th day—pectoral symptoms, and very feeble pulse. On inspection, lungs œdematous, and loaded with mucus, with some effusion into thorax—could not trace contagion here, but some petechiæ on skin—spleen softened and enlarged.

b. Extraneous diseases in union with typhus.

Phthisis pulmonalis. Mrs. M'Donald, æt. 46, p. 22, admitted 21st day—right lung crowded with tubercles in all stages: left reduced to size of man's hand, and thoracic cavity of same side filled with sero-purulent fluid.—Mrs. Allen, æt. 43, p. 139, admitted 8th day—left lung filled with tubercles in different stages, and hepatized—spleen so soft as to resemble coagulated blood—some ulcerated points in intestines.

Hydrothorax or *pectoral effusion*. Mary Perry, æt. 25, p. 87, admitted 8th day—on third day after admission, began to breathe laboriously, and to pass stools and urine in bed,

next day died—considerable effusion into both sides of thorax, and lungs loaded with mucus—usual appearance of red points, and serous depositions between membranes, and into ventricles of brain.

Pneumonia. Eliza Morrison, æt. 22, p. 214, admitted 4th day—pain of right side and cough—was received on 25th June, and died suddenly on 29th—on inspection right lung adhering to parietes by old and strong membranes, universal hepatization, and on top abscess of size of hen's egg, filled with foetid pus, and communicating with bronchia—left lung partially adhering and partially hepatized. Seems case of pneumonia, terminating in abscess, contagious typhus here superadded.

Bronchitis, with Diseased Heart. Agnes O'Conner, æt. 30, Vol. i. p. 234, admitted 8th day—cough and expectoration—and pulse all but extinct—died on 2d day—appears to have had severe cough for two preceding years—on inspection, pericardium universally adhering to heart, so that cavity totally obliterated—bronchial tubes loaded with mucus, and their membrane more vascular than natural. Contagion traced, so that typhus here ingrafted on bronchitis and diseased heart.

OTHER CASES.—*Admitted after 7th day of Disease, or later.*

V. I.

1. *Mrs. Brown*, æt. 30, p. 9, admission 14th day—in state of utmost exhaustion, died day after admission.

2. *Catherine Connell*, æt. 20, p. 34, admission 11th day—delirious, and unable to articulate, died on 2d day after admission. Cerebral effusion both into ventricles, and between arachnoid and pia mater.

3. *Isabella Kinningburgh*, æt. 20, p. 46, admission 8th day—delirious, and exhausted, died 2d day after admission. Slight effusion between arachnoid and pia mater.

4. *Nancy Lochrie*, æt. 40, p. 62, admission 8th day—no inspection.

5. *Mrs. Campbell*, æt. 28, p. 122—in state of total stupor, with pupils dilated, and not contracting to any light—breathing laborious, petechiæ, died 1st day after admission.

6. *Kate M'Donald*, æt. 16, p. 134, admission 14th day—no evidence of contagion—unable to articulate, died on 3d day after admission—on inspection, no reasonable cause of death.

7. *Janet Couper*, æt. 20, p. 181, admission 8th day, died 2d day after admission. Slight serous effusion between pia mater, and arachnoid, and ventricles.

8. *Mrs. M'Kinnon*, æt. 46, p. 182, admission 14th day, died on 2d day of admission—no adequate cause of death.

V. II.

9. *Mary Ross*, æt. 40, p. 8, admission 19th day—in extreme weakness, but no delirium, had wine, sp. ammon. aromat, and whiskey, admitted on 23d December, but died on 26th.

10. *Jane Paton*, æt. 17, p. 56, admission 9th day—violent cough, suspect phthisis from cleanness of tongue, but neglected to examine sputa, and no inspection.

11. *Anne Murdoch*, æt. 49, p. 59, admitted 10th day—cough and diarrhœa for 7 preceding weeks, admitted 25th, died on 29th January—suspect old phthisis, but no inspection allowed.

12. *Mrs. Morrison*, æt. 36, p. 84, admission 11th day—admitted with very feeble pulse, pain at epigastrium, hiccup, laborious respiration, survived from 11th till 14th day—on inspection, no morbid appearance.

13. *Kate M'Lauchlan*, æt. 30, p. 112, admission 8th day—ordinary symptoms, sunk gradually—on inspection, no morbid appearances.

14. *Catherine Ewing*, æt. 33, p. 56, admission 6th day—one of cases with preternatural sensibility of nerves of touch, for screamed out when attempted to feel pulse, or otherwise touch—admitted on 23d of January, 1832, and died on 27th.

15. *Elizabeth Wilson*, æt. 27, p. 201, admission 5th day—petechiæ on 5th day of admission, lay supine, with pulse exceedingly feeble, and died 3 days afterwards—no morbid appearances.

16. *Janet M'Alpine*, æt. 44, p. 208, admission 5th day, but as she was one of the night-nurses, might have been infected, and going about with disease, before she was confined to bed—no inspection.

17. *Mary Maxwell*, æt. 29, p. 184, admission 5th day—after 4 days began to revive, when large gangrenous spot occupying both buttocks and space between, sunk under discharge—foundation of this sore no doubt laid before admission.

18. *Margaret Shaw*, æt. 50, p. 191, admission 3 weeks—pulse totally disappeared night of admission, survived in this state till 3d day—no morbid appearances.

19. *Margaret Neil*, æt. 58, p. 199, admission 9th day—died on 2d day—no inspection.

20. *Margaret Warwick*, æt. 18, p. 222, admission 11th day—pulse nearly extinct—soon attacked by difficulty of dyantition, circumscribed and painful tumour occupying whole abdomen below navel, nature of which not discovered, as friends prevented inspection—no contagion, and not fever at all.

JOURN. N. 2.

21. *Mary Bryland*, p. 4—brought in state of stupor, unable to speak, pulse extinct, became perceptible from wine and whiskey, but died next day.

22. *Margaret Gardner*, æt. 56, p. 6—unable to articulate, pulse hardly to be felt, admitted 21st December, 1831, revived a little by wine and whiskey, but died on 24th.

23. *Jean M'Geohagen*, æt. 36, p. 175, admission fortnight, dismissed recently from Mile-End Hospital, 11 weeks pregnant, survived only 3 days—on inspection, no morbid appearances.

24. *Janet Thomson*, æt. 36, p. 176, admission 10th day—survived only 3 days—on inspection, usual brain appearances.

25. *Mary Dun*, æt. 45, p. 200, admission 10th day—died on 2d day after that of admission—more than ordinary fluid between arachnoid and pia mater, and in ventricles—cough, but thoracic contents healthy.

PROPHYLAXIS.

It only now remains, gentlemen, that I should subjoin a few remarks on the *prophylaxis* of typhus, or point out such measures as may best prevent its occurrence, as well as best check its progress after it has occurred. These measures may be said to be of a double description, those that lie within the scope, and power, of the population itself of infected cities, and districts, and those that require the concurrence of Government.

— Among the former, by far the first in point of importance, is the command of such hospital, or hospitals, as may be sufficient for the immediate reception of the first victims of the disease. Two essential purposes are thus attained; one, the more speedy cure of the sick, another, security to the public against the chance of their infecting others. For accomplishing the first of these ends, the facilities of a fever hospital are chiefly the following:—That the patients are instantly removed from the poisoned atmosphere of their own filthy, and airless, dwellings, where they were farther subjected to all the other miseries, and privations, of poverty—that after being purified, and refreshed, in the warm bath, furnished with clean body linen, and a clean bed, they enjoy the inestimable advantages of a large, comfortable, and well ventilated chamber, and that, in this favourable situation, they receive regular, and skilful, medical attendance, the most careful nursing, and every accommodation of medicine, and diet, that the sagacity of the physician can devise, or that money can purchase, and to crown and secure the value of all these benefits, that they are administered promptly, and at an early stage of the distemper, the only period, generally speaking, when the resources of our art become available.

The security to the public from a fever hospital, must be no less obvious, since the infected being secluded within its walls can no longer communicate the distemper, as they would infallibly do at home, to relatives, friends, acquaintances, or neighbours. For the same reason, free access being now

allowed to the vacated house, or chamber, opportunity is afforded for the most thorough cleansing, by the usual means of white-washing, fumigating, and ventilation, so far as the last is practicable. In this manner, each focus of contagion may be extinguished almost the moment it arises, and if the plan be diligently, and extensively, pursued, the disease, deprived of its fomes or pabulum, if not entirely put down, may be circumscribed within very narrow limits. I am aware it may be said, here, that in some epidemics the number of patients is so great that no house will hold them; this is true, but it arises solely from mismanagement, or from the disorder being neglected at its commencement, and the means of prevention being too long delayed. Were these early enough, and extensively enough resorted to, there is no question but the prophylaxis would, on almost every occasion, be completely successful.

In case it be objected that the public may be still in danger from patients after they have been dismissed from a fever hospital, in consequence of their carrying out the infection along with them, this too is carefully guarded against. In every well regulated establishment of this sort, the clothes of the infected person, the moment he is admitted, are stripped off, and immersed in a large tub of water, so as to be completely surmounted by the fluid, where, after steeping a due time, they are thoroughly washed, dried, and exposed to the atmosphere, till they lose every trace of infection. Meantime those who have overcome the disease are kept apart in a ward by themselves, till ready for dismissal, and as it is not yet settled at what period of convalescence the body ceases to emit typhus virus, I hold it matter of prudence that the term of seclusion be rather long than short, so that when the recovered person resumes his employment, he may, by no possibility, injure those with whom he associates. Besides, when patients leave the convalescent wards too soon, they are extremely apt to relapse, and we all know that a relapsed case is more dangerous, and worse to cure, than one proceeding from the primary, or original attack of the disease.

Such, gentlemen, are the invaluable advantages to be derived from fever hospitals, as instruments both for the more

speedy cure of the sick, or for checking the career of contagion. Several years ago, these advantages being explained to the public, a large subscription was begun, and completed, for furnishing such an asylum, as a necessary appendage to our Infirmary, in other words, for erecting a typhus Lazaretto to be preserved always open, and within which, at the attack of each new epidemic, the first infected might be secluded, so as either to nip the disorder in the bud, or at least keep it at bay, till such farther hospital accommodation could be obtained, as might be deemed requisite for the purpose: and such, gentlemen, was the origin of the supplementary building that now stands in the rear of the Infirmary, and at right angles to the original edifice. Its original destination, however, was soon forgotten, or neglected, and it was put to other uses. Thus part of it became allotted to the ordinary patients of the charity, surgical as well as medical—a large portion was converted into a dwellinghouse for the matron—another into a dormitory for the nurses—another into a dining parlour for the clerks, &c., &c. In all this it is evident there was complete breach of bargain with the benevolent founders of our Lazaretto, and it is no less clear that the managers exceeded their powers. The directors had no more right to divert this part of our hospital to any extraneous purpose, or to any other than to that for which it was intended, than they had a right to convert it into a cotton-mill, or steam-loom manufactory.

As might have been expected, at the next invasion of our epidemic, the guardians of our public health were found quite unprovided and defenceless. Instead of watching, as they ought to have done, the rise and progress of the contagion, (and every information requisite might easily have been obtained from their own district surgeons, or the physicians of the Infirmary,) so as to check it in its initiatory stage, the only time in which it can be checked, they chose rather to lay by, and look on. In fact, they had no place in readiness for receiving the first infected, so as to seclude them from the community, and a very small hospital would have been sufficient for the purpose; while, in the meantime, the pestilence not waiting on their

supineness, proceeded in its ordinary course, and with such rapid steps, as soon to defy control. Then, indeed, and not till then, new hospitals were provided, but it was too late, and so far as regarded arresting the progress of the contagion, these asylums had become now quite useless and nugatory. The effects of this procrastination, and negligence, were never more fatally illustrated than during our present epidemic. So widely had it been allowed to spread, that, as appears by the report of the district surgeons, in the course of the last 5 years, that is, from 1st August, 1827, till 31st July, 1832, no less than 5,822 persons were confined with it in their own houses, (of these only about a fourth were sent to any Hospital,) so that in the different wards and districts of our city, there were thus brought into full activity many thousand separate foci, or sources, of contagion, all in complete communication with the public, and all of them, of course, scattering the malady round about them in every possible direction.*

Providentially, gentlemen, this fearful epidemic seems at last drawing towards a close, but it will infallibly return, sooner or later, and the question occurs, how is it best to be resisted when it again attacks us? This is a serious inquiry, and must not be lightly discussed.

One measure irresistibly suggests itself, namely, that its management be instantly withdrawn from our self-elected rulers, and placed in other hands. In a matter that so nearly concerns the public, it is time that the public should at last interfere, and that the people should take charge of the epidemic themselves. Once rid of the incubus of the municipality, the task will be found smooth and easy.

The organization unhappily necessary, of late, for the Cholera, will answer admirably well for typhus. In every infected district, let a small board, or committee, be appointed to watch over the disease, and whose business shall be, as

* We have now, in our newly erected Fever Hospitals, such ample accommodation, that this dreadful contingency need never occur again, unless from the most complete and culpable negligence on the part of those who take charge of our infectious typhus.

soon as a case appears, to send it without delay either to the Fever Hospital of the Infirmary, or what is better, to some similar asylum that may be provided in the more immediate neighbourhood. In this manner our two grand objects will be accomplished, more speedy cure of the sick, by superior hospital accommodation, and a stoppage of the epidemic itself, by excluding, for a season, the infected, and completely separating them from the sound. It is needless to add here, that our district surgeons must be instantly freed from their duty of attending patients in their own houses, a service cruelly imposed, and of such danger, that I believe few of these gentlemen have escaped catching the disease themselves, while others, as is well known, have lamentably perished the victims of its malignity—a service too, to increase its hardship, in a manner gratuitous, since the remuneration is altogether inadequate to the risk, and labour. These practitioners, however, will be exceedingly useful in the less perilous office of *inspectors*, to point out such cases as are actually typhus, and for this new function, by no means without its danger, justice demands that they should have an additional salary. I am aware here of an objection frequently started, that many of the sick will refuse to remove from their own houses, but by proper management this obstacle may be easily surmounted. The working classes are the great sufferers by typhus, it is in fact their own disorder, and they are too well informed, and enlightened, not to perceive, and appreciate all the advantages of an hospital, not only as affording more effectual relief to the immediate sufferers, but as a shield against that infection, to which they themselves are so peculiarly exposed. They will willingly lend their aid, therefore, on this occasion, and will soon dissipate whatever prejudice may still remain against these establishments. In all the typhus local boards, for this, and other reasons, a large proportion of the members should always consist of operatives.*

* Some sixteen, or eighteen, years ago, when this pestilence first began to assail us, our magistrates, and their partizans, formed what they were pleased to call a Fever Committee, but though of this committee the avowed purpose was medi-

Besides hospitals, or lazarettos, other means for averting the evils of typhus, may be said to lie within the scope of the inhabitants of infected cities, and districts.—One is to open up to the atmosphere all their close courts, as well as all their confined lanes, and alleys. Various improvements with this view have often suggested themselves in Glasgow, as by demolishing old crowded buildings, and erecting airy streets in their room: one for instance, to run from Saltmarket westward to the Stockwell—another at right angles from the Trongate, southwards, till it reaches the Bridgegate, or river; and a third, as was proposed by an ingenious citizen, to extend diagonally eastward and northward from the cross, till it terminated about the end of Duke Street. Were these schemes carried into effect, there can be no doubt that such constant and powerful currents of air might be secured as would sweep away a large portion of the contagion, and might materially check the progress of any future epidemic.

As to the preventives of typhus, or those precautionary measures that can be accomplished only by Government, these

cal, and *nothing but medical*, that is, to relieve or put down a particular disease, yet in the whole association was to be found neither physician nor surgeon,—in other words, it did not contain a single member who knew an iota of the business they undertook to manage. Never was town-council measure more barefaced and ludicrous. Had one of these gentlemen been seized with fever himself, would he have put himself under the care of any one of his colleagues, or would he have trusted himself to the wisdom, or skill, of the whole confederacy, supposing it united into one mass? Certainly not. Yet did this band of unprofessional persons, coolly, and deliberately, assume to themselves the management of hundreds, nay of thousands of their fellow-citizens at this time labouring under the infliction of fever. The consequences were such as might have been expected, a series of blunders from first to last, and it is the rump of this precious committee that still takes charge of our epidemic, though as to their competence for the task, there can now be only one opinion among the citizens of Glasgow.

I may observe here, by the way, that a very large sum of money was placed at the disposal of this committee, but to this hour I have never seen their balance sheet; it is very possible, however, it may have been published, though it has escaped my notice. If they have already printed it in the newspapers, they have only done their duty: if it be still withheld, it is surely high time it should now be rendered.

are so numerous and various, as far to exceed our limits here. The aim and scope of the whole, however, must be one and the same—to *amend the condition of the working classes*, the great and almost exclusive sufferers by typhus, so as, by rendering the wages of labour commensurate to comfortable subsistence, effectually to prevent those periodical inroads of privation and poverty, to which, unhappily, they have of late years been so frequently exposed. To enumerate such measures as by lessening our national burdens, might directly, or indirectly lead to so happy a result, would require too copious details for this place: the correction of one abuse, however, would act so powerfully as a corrective, and preventive, of typhus, that it ought not to be passed over without particular notice. The abuse I mean here is the window duty. No impost was ever more oppressive. It is tyrannical, indeed, in a double sense; it taxes not only the free and common light of heaven, meant to be the boon of all, the poor as well as rich, but even levies an impost on the very air we breathe. It has already been shown you that unimpeded atmospherical circulation is at once the grand preventive, and destroyer, of contagion, but from this source of safety the poor man, in his humble dwelling, is completely debarred. Whatever aperture admits air, also admits light, and is counted a window, and though a dozen, or even twenty openings may be requisite for purifying his dwelling, yet he dare not avail himself of more than six, except at a price he is unable to pay, or at the certainty of having his goods distrained by the harpies of the excise. Hence it is that the houses of the indigent, being by this cruel law rendered incapable of ventilation, become thus the primary seats of contagion, where it concentrates into its utmost malignity, first of all seizing on the unhappy inmates, and from them spreading to the rest of the community, and were it for no other reason, this execrable tax should be instantly abolished. Upon the whole, were this and the other above recited measures of prophylaxis carefully enforced, there can be no doubt that the evils of typhus might be materially abridged, that our working classes might, in great measure, be rescued from its gripe,

and that in time, perhaps, the disease itself might be utterly banished from the land.

Before we part, gentlemen, I must say a word of the Infirmary itself. One of the allotted Managers is the Member of Parliament for Glasgow, but there are now two members, and it must be difficult to determine which of the two ought to hold the office. The truth is, we must have a new Charter altogether, the present is not suited to the times. I am old enough to remember when our regulations were first framed. The rule of the boroughmongers, and oligarchy, now happily overthrown, was then in its zenith; every thing new and liberal was viewed with scorn or detestation, every thing old and rotten was regarded with veneration and applause. The traces are manifest in our charter. The whole power almost is vested in nominees from different corporate bodies, infallibly carrying their own abuses and corruptions into the management of the charity. All this must be done away with, and the government placed on a liberal and popular basis. Already ten of the directors are named by those who support the expense of the establishment, let the whole twenty-five, in future, be appointed by the same authority. The elective franchise also must be altered: it is at present too high; let it be reduced to £5 given in perpetuity, or one guinea a-year. Many advantages would result from this change; of which none of the least would be the immediate delivery of the house from all municipal domination, to which it has been so long subjected, besides exciting an interest for the prosperity of the Institution in a much wider range of the community.*

* Our late worthy Lord Provost, as I am informed, laid it down as a maxim, in a meeting of the managers, that in choosing surgeons to the hospital, those who had acted in the city districts, other circumstances being equal, should always be preferred, of which the plain English is the following:—The Provost and Magistrates already name the district surgeons, and on this vantage ground the hope might be built, that they would thus virtually obtain the power of appointing the Infirmary surgeons also.

In an Institution purely medical, such as our Infirmary is, or one of which the sole purpose is to alleviate or cure disease, persons of the healing profession, physicians and surgeons, will no doubt be necessary in the management, but the number, as well as choice of the individuals, might be safely enough left to the good sense of the public.

THE END.

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