SOUTHERN

MEDICAL AND SURGICAL JOURNAL,

EDITED BY

L. A. DUGAS, M. D.,
PROFESSOR OF SURGERY IN THE MEDICAL COLLEGE OF GEORGIA.

AND

HENRY ROSSIGNOL, M. D.

MEDICAL COLLEGE OF GEORGIA.

VOL. XII.—1856.—NEW SERIES.

AUGUSTA, GA:
McCafferty's Office—J. MORRIS, PRINTER.
1856.
An Essay upon the Relation of Bilious and Yellow Fever—prepared at the request of, and read before, the Medical Society of the State of Georgia, at its session held at Macon on the 9th April, 1856. By Richard D. Arnold, M. D., Professor of the Theory and Practice of Medicine in the Savannah Medical College.

The subject about to be discussed by me, in compliance with the appointment of the Society, is—The Relation of Bilious and Yellow Fever.

At first sight, this may appear to be a question of very little practical utility, and one upon which there is little contrariety of opinion. But when I recollect the change which years and experience have wrought in my own opinions, and when I see taught in medical text-books views so totally dissimilar to those I now entertain on the subject, I have not been unwilling to endeavor to convey to the society what I conceive to be the true relations between Bilious and Yellow fever. In doing so, I shall not pretend to give a history of either Bilious or Yellow fever, nor shall I bring any bibliographical array to support my views: not that I underrate the value of books, for without a proper knowledge of what has been learned and taught before our day, we would be little better than blind mill-horses, constantly pursuing one narrow
circuit; but, because the subject is a strictly practical one, which must be decided by the weight of testimony.

Each observer must bring in his mite of observation, add his grain to the mound of true knowledge; "and, as the laborious ant—"

Trahit quodcumque potest, atque addit acervo
Quem struit, hanc ignara aev non incanta futuri."

While Yellow fever has never been known to prevail in climates and localities where Bilious fever was not endemic—Bilious fever, in its most malignant form, is known to prevail where Yellow fever has never been seen. Having practised for more than twenty-five years in one of those localities where Bilious fever is annually endemic, and Yellow fever only an occasional visitor, I feel that I have been placed in a position which has given me some advantages in treating of this subject. Two opinions have prevailed amongst those who have judged from this fact: first, that Yellow fever, occurring as it does where Bilious fever is endemic, is but a higher grade of the same disease, produced by the same causes, acting in greater intensity; second, that Yellow fever is a disease sui generis, having no analogy nor connection with Bilious fever, not produced by any local causes, but invariably imported from abroad, therefore to be kept away by quarantines and all their inhuman vexations and costly consequences. I shall proceed to consider these two opinions in their order.

1st. Is Yellow Fever only a higher grade of Bilious Fever?—The first time I ever saw a case of Yellow fever was, while a pupil of the late Dr. Wm. R. Waring, in the summer of 1827. No man in the southern country was better acquainted with our fevers than he was. He had seen them in various localities while a surgeon in the army; he had seen them in all their violence in our own city—before the dry culture system, by removing the culture of rice, with its concomitant evils, from under our very door sills, had so favorably modified the type of Bilious fever as met with in the city proper; he had been through our then recent epidemic of Yellow fever of 1820, from the beginning to the end; in company with the distinguished Chervin, he had conducted a series of post-mortem examinations of Yellow fever subjects, and he could thus "speak by the earld." During the fall of 1827, Yellow fever broke out in our city: it did not prevail very extensively; for, occurring late in the season, its mighty destroyer, frost, put an end to it before it had time to spread extensively.
It was my privilege to conduct all the post-mortem examinations made by my preceptor during that season. Is it any wonder that I should have considered him my medical Gamaliel, and have sat reverently at his footstool? Among the opinions held by my distinguished preceptor was this identical one, that Yellow fever was but a higher grade of Bilious fever. He had imbibed this from his preceptor, the celebrated Rush; it descended in a straight line to me, and many years rolled by before I dared to question its accuracy; and I did not do so, until repeated observations had given me data on which to base my belief. Such is the power of authority, which too often trammels us in our researches. In fact, in our views of general affairs in this world, of politics, of religion, &c., there are very few who can truly apply as a motto—

"Nulius addictus jurare in verba magistri."

If Yellow fever were only a higher grade of Bilious fever, we ought to see it "cropping out," whenever there was any unusual intensity; or any greater prevalence of the latter.

I have witnessed every epidemic in the city of Savannah, from the year 1830 up to the present time; I have often known and seen Bilious fever of a malignant congestive type; for fifteen consecutive summers I was the attending physician of the city hospital, whither the worst cases of our ordinary climate fever are conveyed.

When year after year, I met with malignant and fatal cases of Bilious fever, and yet with not a single one of Yellow fever, I began to doubt whether or not I was right in my opinion. Occasionally a few cases of Yellow fever would occur, at intervals of years; these I studied with intense interest. From 1830 up to 1839, I never saw a case of Yellow fever in the city. Its characters were indelibly imprinted on my memory from the experience of 1827. In 1839, the city of Augusta was ravaged by this scourge: it was denied at the time that Yellow fever prevailed there. In the last of August, a patient, fresh from Augusta, entered the city hospital, and died in a couple of days. My then colleague, Dr. P. M. Kollock, and myself examined the body, and found the unmistakable post-mortem appearances of genuine Yellow fever. A short time afterwards, a patient from Charleston entered, and died, and after death presented the same appearances. It is worthy of recollection, that although these cases were placed in the wards of a hospital filled with Bilious fever patients, there
was no propagation of the disease. I still look back upon the year
1839, as the sickliest season I have ever experienced in Savannah,
with the exception of our terrible epidemic of 1854. Old inhabi-
tants will recollect it as the driest summer on record, when turnips
were planted in the bed of the Savannah river opposite Augusta.
It was also a hot summer. Bilious fever prevailed over the whole
country, and in a malignant form. Contrary to what would seem
the fact at first view, such a season was peculiarly calculated to
generate the malaria which is the generally acknowledged cause
of Bilious fever. It is conceded that mere moisture will not pro-
duce malaria; but mix vegetable matter with water, and subject
it to heat, and the most malignant malaria will be generated.
That year, swamps and ponds which had been covered with water
since they had been known to the white man, were dried up, and
the vegetable debris which had been precipitating to their bottom
for years and years, were exposed to the action of the sun and air,
and consequently were decomposed, and generated malaria. Now,
Bilious fever prevailed with great violence in our city from early
in July. I cannot imagine more favorable circumstances for the
spread of Yellow fever than accompanied the introduction of those
two cases in our city. Later in the season, I did meet with seve-
reral cases of Yellow fever, but they were so few in number that I
did not consider them as entitled to be considered epidemic. They
were isolated, occurred in different parts of the city, and had not
the slightest connection with the cases of the hospital. I consid-
ered them sporadic, and they most undoubtedly originated on the
spot.

I met with one solitary case of Yellow fever, with black vomit,
in the fall of 1840. She was an unacclimated foreign lady, who
had not stirred out of the city during the whole summer, nor had
she even peeled a banana from Havana.

In March, 1841, a case was brought to the hospital from Dem-
rara, and in October of the same year a case occurred in my private
practice, both of which were reported by me in the American
Journal of the Medical Sciences for October, 1842.

A few cases occurred at the hospital late in the fall of that year.
I found the post-mortem appearances so similar in all the cases I
had examined, from 1827 up to this time, that I was convinced
that Yellow fever must be a disease sui generis. It was with in-
creased interest, that during the summers of 1843, 4, 5, 6, 7, 8, 9,
I examined every case of fever which died at the hospital. Neither
during life, while attending them, nor after death, did I find any
signs to make me ever suspect that Yellow fever had existed.
With the exception of a sporadic case in June, 1852, I met
with no Yellow fever until the fall of 1852. Late in September,
an unacclimated painter was attacked with it in the north-eastern
portion of the city; he had been working here all summer,
and had had no connection with Charleston or Havana. He was
removed to the hospital after he had thrown up black vomit, and
he died. I had resigned my post as physician there in 1850, and
was not attending. An autopsy was made, at which I was pre-
sent. Before it was done, I stated to the attendants what morbid
appearances I expected to see; and they turned out exactly as
described beforehand. The fever began to show itself in several
places about the middle of October; but, fortunately, a frost early
in November cut it short. I examined several subjects who died
of it, and found the same peculiar morbid appearances. In 1854,
it was my lot again, as in 1852, to have the first Yellow fever pa-
tient. I was called in on the night of the 3d August, and he died
on the morning of the 5th, after having discharged quarts of genu-
ine black vomit. My last case of Yellow fever, with black vomit,
died on the 27th November. In the intermediate time, I had seen
hundreds of cases of genuine Yellow fever.

I had made post-mortem examinations in the beginning, the
middle, and the end of our epidemic, under the broiling sun of
August, the more temperate atmosphere of the latter end of Sep-
tember and October, and the almost cool temperature of Novem-
ber, and I found nothing new. From the beginning to the end,
I found the same morbid appearances. Of course I do not mean
that each case was an exact copy of the other; but just as all cases
of genuine Typhoid fever present the same morbid appearances,
although the patches of Peyer may be more ulcerated in one case
than in the other, or in some cases they may be enlarged without
being ulcerated.

Now, the morbid appearances after Bilious fever have never, in
my experience, approximated those after Yellow fever; and the
symptoms during life have presented wide and marked differences.
Let us devote a little attention to these two conditions.

It is well known that all fevers have many symptoms in com-
mon in their beginning, such as headache, lassitude, pain in the
limbs, &c.; and that merely from such symptoms it would be impossible for the most experienced and skillful practitioner to diagnosticate the particular kind of fever presented to him. He must wait the progress of the case, and the development of the characteristic symptoms, before he can decide. Of course I speak of the inception of the disease. Certain fevers come on, as a general rule, more suddenly than others; but the rule is not invariable, and we would be at a loss to make a correct decision if we depended merely on the first phenomena of febrile disturbance.

Moreover, some cases of well-known specific exanthematos diseases are developed so imperfectly that we are at a loss to decide, positively, whether or not the patient has had the genuine disease. Every practitioner of any experience must have met with such cases of Scarlet fever and Measles. Yet no one has ever, in latter days, denied that they are distinct and peculiar diseases, although a little more than a century ago Measles was confounded with Small-pox.

I do not deny that, when no suspicion is aroused, sometimes the first notice the physician has that he is treating a case of Yellow fever, is the appearance of the fatal black vomit. But even in epidemics of Yellow fever, black vomit often supervenes when the patient has apparently passed the point of danger and offered no untoward symptoms.

Nor must it be supposed that all cases of genuine Yellow fever appeared in one stereotyped edition. There was every variety of grade and intensity, from the ephemeral attack of twelve hours of fever, followed by speedy convalescence, to the more prolonged paroxysm of seventy-two hours, ushering in a malignant or a fatal case. Yellow fever is essentially a fever of one paroxysm; but that paroxysm is of very unequal duration, as just intimated. Now, if the access of fever should not be very marked, it could not be distinguished at first. Again: there are some cases which are ushered in with such marked symptoms that your suspicions would be at once aroused. The first case with which I met, in 1854, was one of this nature. There had been no unusual severity in the fevers which had occurred up to that time. The summer had been the very hottest I had ever experienced, and what is very rare many fatal cases of coup de soleil had occurred. I was called to see my patient at night (3d of August); he had taken comp. blue pill: he offered the usual symptoms of fever—pain in the
head, in the loins, over the upper part of the sacrum, down the thighs; a hot, dry skin, and accelerated pulse. I directed a demulcent drink, and that a dose of castor oil should be administered the next morning early. On my visit the next morning, I found him with a raging fever; intense headache; blood-shot, shining, watery, smoke affected eyes; a full, bounding, but not very frequent pulse; a constant retching, and quite delirious. The landlady said to me, "Doctor, what kind of a fever is this?" I replied, "It is first-cousin to Yellow fever." I bled him, and applied a blister to the epigastrium, and directed cold demulcent drinks. The fever continued unabated all that day and the ensuing night. On the morning of the 5th of August, on my visit, they showed me a large wash-hand basin filled with matter, which I pronounced black vomit. He continued to eject large quantities of it, and at noon he died. This man was a carpenter by trade, a northerner; it was his first summer south; he had been working on the roof of a house which was just finishing, and before he had moved to the place where he was then boarding, had lived in Curry town, the extreme south-western portion of the city, and had walked nearly a mile two or three times daily to and from his work, which was in the north-eastern portion of the city, through the broiling sun. It is again to be noticed that not a single other death occurred in that house during the season. After the epidemic became a fixed fact, and cases had occurred all over that section of the city, two of the inmates had the fever, but it was in a mild form. There was not any loop-hole wherein to hang even a suspicion that this man's disease had been contracted any where out of the limits of the city—no "low, long, black-hulled schooner" had just arrived from the West Indies to afford an easy solution of how Yellow fever had attacked a denizen of Savannah. The house was a mechanics' boarding-house. A great panic ensued; but I am yet to learn that any boarder contracted the disease from this case.

The next afternoon, the 6th August, I was called by Dr. Jas. B. Read to see a case on the extreme eastern edge of the city, many squares distant from the first case. The patient was a young German girl, entirely unacclimated; the house where she lay was on the eastern bluff of the city, overlooking the low swampy grounds in that direction; it was amply ventilated, standing isolated in a large lot. She had been engaged sewing decorations at the theatre, distant about three-quarters of a mile,
and had walked to and from it in the hot sun, during the whole season. Unfortunately there was no doubt about the case; she was moribund, with black vomit thrown up all about her.

On the afternoon of the 5th, I was called to see a patient in Congress-street, about a hundred yards to the north-east of my first patient, but in a different street. His fever did not attract my attention particularly. He was of a lymphatic temperament, a northerner who had resided several years in the city, and whom I had attended some summers before in a very severe attack of bilious fever. His fever was not very high; he complained of pain and languor. I gave a dose of blue pill, and directed oil the ensuing morning. On the 6th his medicine purged him and he was better. On the 7th, when I called to see him, he had left the house and gone to his business. On the evening of the 8th I was called to him; I found him with a slow pulse, a cool skin, and a constant retching, ejecting glairy matter, and no bile.

For the first time, I suspected Yellow fever, and that the cessation of the fever had been the calm which follows the single paroxysm of that fever. I ordered a blister to epigastrium, ice to suck, and iced gum water for a drink, and my alterative powders, (two grains of calomel, and one sixth of a grain of opium,) every two hours. On the 9th, he was much the same, except that the prostration was greater, so as to seem to threaten death from sheer exhaustion. Towards night, I discovered flocculi of black vomit in his vomit. He continued to throw up the black vomit mixed with a good deal of mucus, all that night, and all the next day, and died on the night of the eleventh. The quantity ejected was not very great, and it was thrown up with a great deal of straining and mixed with mucus. He sank away gradually and gently, like one yielding to the effects of a depressing poison, without the power of reaction.

Let us contrast the first case and this: The first case occurred in an unacclimated subject—it was violent from beginning to end. The last occurred in an acclimated subject—it was slow in its progress, less marked in the first stage, but running its stage of calm and secondary fever as is most generally seen where death does not occur during or just after the paroxysm of the fever.

Now no fact is more notorious than that acclimation to a warm latitude diminishes the susceptibility to yellow fever, and that it is far milder in those who have constantly resided south, summer
and winter, than in those who have not; and who are consequently unacclimated. By this time, the eleventh of August, I had been called to many cases, all in the north-eastern part of the city, but in separate houses and different streets—not in any ways connected with each other, and I could not doubt that we had a different fever to contend with than a bilious remittent fever, and I will now proceed to state the symptoms which brought me to that conclusion. Of course I will state what was the general type.

The invasion of fever was more sudden than in ordinary Bilious fever, and although all fevers will be found to have a cold stage of some kind, it was not well marked in these. There was intense pain in the back, over the last lumbar vertebra and upper part of the sacrum and extending down the thighs along the sciatic nerve. The pain over the eyes, in the frontal region, was excruciating; the eyes were watery, shining, sometimes injected, sometimes not, with the upper lid partially drooping, like one whose eyes were watering from a quantity of smoke. The skin was intensely hot and very dry; the stomach was very irritable; the ejecta were either a serous fluid, bluish green, as if blue vitriol had been dissolved in water; or a glairy, viscid, tenacious mucus. But the pulse was not disturbed in accordance with the general perturbation, it seldom being over a hundred beats to the minute, and very often not more than eighty or ninety. This symptom I have been disposed to look upon as very characteristic. This febrile state lasted from twelve to seventy-two hours, on an average about thirty-six hours, and was succeeded by a cessation of these symptoms, and an apyrexia, but without any critical evacuation whatsoever. After this, in fatal cases, black vomit came on immediately, or it was ushered in by increased irritability of the stomach, it becoming intolerant of the mildest ingesta, by a constant empty straining, and by the most acute sensibility of the epigastrium to any pressure. With this, in most cases, there was the most remarkable depression of strength; in some cases, several hours of a most perfect calm succeeded the paroxysm, and there was nothing to rouse suspicion of danger but a slow pulse, it generally sinking to forty or sixty beats in the minute. When black vomit supervened, a few hours terminated the case. With all this manifest affection of the stomach, the brain, as a general rule, did not sympathise. The intellect was not affected, until the last
closing scenes of life, when the brain gave away in common with the rest of the organism.

This, considering the violence of the febrile paroxysm, must be considered one of the characteristics of Yellow fever. The thirst was great, but the tongue was not generally parched. At this period of the epidemic, the fatal cases terminated quickly; black vomit came on within three or four days, and the patient seldom survived beyond the fifth day. When he did so, the chances were greatly in his favor.

I have sketched only the prominent symptoms. Do they differ from those presented in a case of Bilious fever? Let us take a well-developed, well-marked case, with which to make our comparison.

A Bilious fever is almost always ushered in with a pretty distinctly marked chill. There are certain symptoms, as stated before, which are common to the various forms of fever, such as weariness, head ache, back ache, &c.; but the invasion of these pains is not so sudden in a Bilious fever. After the cold stage, there is an evident reaction of the system, and a hot stage ensues. This, again, is followed by a third stage, viz., one of sweating; the fever then abates, with a distinct critical evacuation, either by urine or by perspiration; and in a few hours it again begins to increase, and having attained its height a sweating stage again follows; and so the fever goes on, the remissions becoming shorter, the stages less marked, until the system sinks under it; or the paroxysms becomes lighter, the remissions so marked and distinct that they slide into intermissions, and the patient recovers.

Marked Periodicity is the distinctive characteristic of Bilious fever, as far as it has come under my observation.

But there are certain symptoms attending the paroxysms which are quite distinctive. The pulse becomes more accelerated in Bilious fever, ranging far above one hundred, increasing as the fever increases, becoming slower as it abates, presenting, as verified by me, in scores of cases, a variation of forty beats to the minute between my morning and evening visit.

There is headache in Bilious fever, but is not of that intense supra-orbital character as in Yellow fever, and is more diffused over the anterior portions of the brain. There is very frequently great irritability of the stomach in Bilious fevers, and the stomach ejects great quantities of bile. Sometimes the bile may assume a
greenish color. Very often, a severe attack of Bilious fever may present its paroxysms so marked, and the remissions so distinct, that you could with propriety class it in the intermittent variety; still the three stages, of cold, heat, and sweating, can be easily traced and marked in the form of Bilious fever. Now, if Yellow fever was but the highest grade of Bilious or Climate fever, we ought to find the worst cases of the latter closely approximating, if not running into the former. But what are the facts? The congestive type of Bilious fever (as witnessed by me in hundreds of cases) is unquestionably the very worst type of that fever. Every year, however healthy, affords cases of it in those individuals who have been exposed to swamp miasma in their avocations. Watchmen are required at the wharves under the bluff of the city. Savannah lies on a high bluff, forty feet above the level of the tide, and fronts to the north. Northward is a low alluvium extending in a direct line due north, for fully four miles before the high ground of South-Carolina is reached. Hutchinson's Island, immediately opposite to, and north of the city, is under the dry culture contract, which prohibits the planting of rice within a mile of the Exchange; but beyond the back river and on the Carolina side, are vast bodies of land, fully from two to four miles through in a northward direction, and extending east and west for about twelve miles, which are cultivated in rice with all its concomitant moisture. To the northeast of the city, these lands extend to the limit where the water becomes brackish and unfit for the culture of rice. To the direct east of the city, and beyond the limits of the dry culture contract, and on the Georgia mainland are many hundreds of acres of land cultivated in rice. To the north-west of the city, the alluvium takes a bend to the north, affording in that quarter some of the finest rice plantations in South-Carolina. What constitutes the defence of Savannah against the malaria of these low grounds? I answer; that, fortunately, almost the whole northern front of the city is defended by a high row of brick storehouses rising some twenty or thirty feet above the level of the plain on which Savannah stands; which storehouses are not inhabited, and thus afford a *material* bulwark against the introduction of malaria into the city. To the northeast of the city, this protection is not afforded, because the storehouses have not been built up in compact mass above the level of the plain of the city, as they have been at the portion of the front
more westwardly. In this portion of the city there are many
dwelling houses on Bay-street which are not protected; whenever
the winds prevail from the north or north-east, those houses have
invariably, and I speak advisedly from many years' experience,
afforded the first cases of Bilious fever, and the most malignant
types of it every year. I have a patient who lives in this locality.
About six years since he moved into his house, and he and his
whole family (a wife and three children,) were desperately ill of
Bilious fever. I advised him, nay, insisted upon it, giving my
reasons, that in the sickly season, when malaria was generated,
(say from the first of June until a frost in the fall,) he should keep
all the windows on the north side of his house closely shut by the
sashes, from early in the evening until the sun was high up in the
morning. He has done so, to the exemption of his family from
fever, and the great curtailment of my professional fees. Such
has been my advice to all persons inhabiting houses exposed in a
similar manner, and I distinctly aver, that where the advice has
been followed, the same result has obtained.

Those individuals whose liberty is the practical one so much
sighed after by the pseudo-philanthropists of the North, of work-
ing or starving, are the ones who take the perilous occupation of
watching at night under the bluff, and who are thus exposed to
the malaria which may be blown from the north-east, the north,
or the north-west, just as the wind may set.

The summer of 1855, was the healthiest I have ever known in
the city. Fevers did not rise above the grade of intermittent, as a
general rule, yet I met with two cases of congestive fever, both of
which were fatal within four days from my first visit, and each
individual had contracted his fever, from exposure at night and
early in the morning, in the very locality I have pointed out.
They were the fac simile of cases occurring more or less frequent-
ly every year; they were malignant, and they were fatal; but they
offered not the slightest resemblance to Yellow fever. The fever
was high, the pulse was accelerated up to 120 to 140 in the min-
ute; while the skin was hot to the touch, it was covered at times
with moisture, standing out in great beads of sweat. The brain
was affected with stupor from the very commencement. When the
fever remitted, which it did in the morning, and notably on the
morning of the alternate day, the brain would become relieved in
a measure, but as the fever exacerbated it would again become op-
pressed. These cases terminated in a stupor many hours before death. Perspiration in the very height of the fever, I consider as a very common symptom of the congestive form of Bilious fever, and I always consider those cases most dangerous which show this symptom, while there are stupor, an accelerated pulse, and an intensely hot skin. Whilst the cases which terminated favorably have the paroxysm of fever resolved by a critical sweat, with an abatement or cessation of the other febrile symptoms; I have time and again seen a man *in articulo mortis*, with a pulse so accelerated that it could not well be timed; in a profound stupor, and with the sweat standing out on his skin in great drops; and this condition of affairs had not supervened just before the patient became in *extremis*, but had gradually come on in the last exacerbation of fever. There are other cases which come under the category of pernicious or malignant intermittents, or congestive chills. I have known a patient in a state of perfect apyrexia in the morning, to die in the afternoon. These are the most malignant forms of climate fever met with in this city. I may fail to convey an idea of their real character, but it is from want of power in my pen, not from want of their total dissimilarity to Yellow fever.

Late last fall, but before a frost, a watchman on the Charleston wharf, at the northwestern portion of the city, was found early in the morning lying in a state of complete insensibility. The night had been a stormy one; he had had intermittent fever, but had persisted in going to his work in spite of the remonstrances of his wife. He was carried to his house on the brow of the bluff, and I was sent for. I found him in a complete stupor, with his pulse nearly gone, his extremities icy cold, his whole periphery cool, skin mottled, purplish, or rather in some parts bluish, with a clammy sweat, hurried respiration, and in short, in what I considered a dying condition. I had him stripped and rubbed dry, had dry heat applied to the surface, sinapisms to every available point, and a large blister to the epigastrium and one to each leg. Friction was applied continuously for some hours; after awhile he was enabled to swallow: I gave him hot brandy toddy every half hour, and calomel two grains, and opium one-sixth every two hours. Reaction gradually took place and the next day, about twenty-four hours after he had been brought home, he spoke. From that time he began to mend, and is now at this present writing "earning his bread by the sweat of his brow."
The rationale of this case is simple. The exposure to cold and moisture in an undue degree, converted what would have been, without such exposure, a mild paroxysm of simple intermittent into a malignant congestive chill, oppressing and depressing all the vital powers so as to prevent proper reaction, thus giving a fair representation of the congestive form of Bilious fever. If it were not so, I am at a loss to comprehend what that type of fever is, and must come down from the witness stand as never having seen a genuine case of it. One more prominent symptom remains to be noticed. While a jaundiced hue often follows an attack of bilious fever, another colour is its most frequent concomitant. There is in the worst types of it, a peculiar pallid anemic hue. This hue can be seen in those cases which have not fully recovered from attacks of intermittent fever, or where, as is often the case, a severe attack of bilious remittent fever has been succeeded by attacks of irregular intermittent fever prolonged late in the fall or even after a frost.

In my clinical lectures at the Savannah Hospital, I have frequently diagnosticated malarial fever subjects from merely seeing them, before, I had asked a single question of the patient. In enumerating the peculiar signs of Yellow fever, I did not speak of the yellow colour of the skin because I wished to reserve that point up to this period. Now, as a general rule, fatal cases of that disease presented that discoloration; and an unfavorable prognosis was almost always to be formed when the skin began to assume that colour; yet in the commencement of the epidemic, during our intensely hot and dry weather, when the cases were more acute and terminated more rapidly, I saw many dead bodies whose skin could not have afforded any index to the disease of which they had died, although black vomit had been freely thrown up before death. Of the cases which did recover, although many had been very severe, very few presented any morbid discoloration of the skin, and it was a subject of frequent remark by those who returned to us after the pestilence had left us, that they were astonished to see the survivors looking so well and free from any marks of previous diseases. A gentleman, his wife and child, had all had very severe attacks of the fever in September 1854. He visited the north late in October for a change. He has often told me that persons there would hardly believe that they had just come from what was then an infected city, and that they had been
sufferers from the scourge, so little did they bear any traces of it with them. Let me then sum up what are the prominent symp-
toms during life of each disease, before I go into the signs presented after death. I speak of the average of symptoms without noticing the varieties which occur in this, as in every other disease. In Yellow fever, the access of the disease is generally sudden; a person may be about in the morning and quite ill in the evening, or may be well in the evening and attending to business and be prostrated in the morning following. The sympathetic pains are much greater; the pain is over frontal region, over sacrum and down the thighs; the skin is hot and dry, and does not pour out perspiration as in Bilious fever; the pulse, never mind how high the febrile symptoms, seldom ranges over a hundred; the tongue is not coated, on the contrary offers no index of the state of the stomach. The paroxysm of fever subsides without any critical evacuation and a state of calm succeeds which lasts from a few hours to forty eight hours. The pulse at this stage generally falls as low as fifty or sixty. In bad cases, the stomach invariably shows great tenderness upon pressure, or there is an uneasy sensation in the epigastrium, and an intolerance of food. With this, there is also a remarkable prostration of strength. Many cases seem to be threatened with death from sheer exhaustion; nor is this at all dependent on any previous evacuations from the system, nor is it always in a direct ratio to the severity of the febrile para-
xysm, for it would occur where there had been no evacuation, and would follow a very slight paroxysm. If the case continued to grow worse, the retching is followed by the vomiting of the black vomit, the occurrence of which at the season of the year when alone Yellow fever prevails in this climate, leaves no doubt as to the nature of the disease or the fate of the case; or hemorrhage would occur from the mouth, the lips, the tongue, the gums, a scorbutic oozing. In an epidemic of Bilious fever, many of the cases have their periodicity so well marked that no one could doubt as to their true nature. Other cases have their remissions more obscurely marked, and without close watching, would seem to be continued fever, but a close observation will generally detect marked remissions, and decided exacerbations. The remissions almost always occur in the forenoon, the exacerbation in the latter part of the day and at night. Bilious fever seldom attains its height at one bound as does Yellow fever. Questioning will frequently
reveal the fact that there has been a distinct intermission between
the first and second paroxysm. The pulse is most certainly more
accelerated in Bilious fever, reaching often in a paroxysm up to
120 to 140. It will also vary many beats in the course of the day.
A paroxysm seldom lasts longer than twenty-four hours when it
either terminates, or there is a marked remission accompanied by
sweats more or less profuse, and a sensible abatement of all the
febrile symptoms. After this the fever again rises, again runs the
same round. If the case is to terminate favorably, the paroxysms
become lighter and lighter, the remissions more marked; very fre-
quently they may be considered perfect intermissions, and you
will see that the great peculiarity of Bilious fever is its periodicity.
In the paroxysms there are headache, and backache, and pain
down the sciatic nerve, but they are not so marked, as a general
rule; not of such a neuralgic character as is so often seen in Yellow
fever. There are nausea and vomiting, but bile continues to be
thrown up to the last stage of Bilious fever, should it be a case
marked by great irritability of the stomach. Now when a person
is attacked by Yellow fever, of course there is some bile in the
system, and it may sometimes be thrown up at the very commenc-
ment of the attack, but certainly it is never seen in the advanced
stages of the disease.

In October 1842, I published the article on Black Vomit in the
American Journal of the Medical Sciences alluded to before. My
experience was then limited, but I adhere without qualification to
the opinion then announced by me, viz: "Perhaps there may be
bile in the incipiency of the attack, before a physician is called,
but in every case that has ever come under my notice, that has
terminated in black vomit, the absence of bile in the excretions has
been the distinctive characteristic of the disease." The head is decided-
ly more affected in Bilious fever, than in Yellow fever. It
is a common thing for patients to remain in their senses long
after they have reached the stage of black vomit. Just before
death, the brain gives way as the other organs do. But in a
bad case of Bilious fever there is almost always oppression of
the brain, and cases lie in a stupor for two or three days before
death. I think the anatomical lesions discovered in Bilious
fever after death, which, I will detail further on, sufficiently ac-
count for this.

I think I have furnished sufficient points of contrast for the
symptoms during life. Let us follow out the diseases, and see what anatomical lesions are left on the dead body by them.

In Bilious fever we find marks of disease on the mucous coat of the stomach, the upper part of the duodenum and the liver. These I may state as invariable. In a large number of cases, and particularly in the worst types of Bilious fever, there are traces of the disease on and under the meninges of the brain.

In Yellow fever we find the same organs affected, except that, as the brain is involved during life in but comparatively few cases, it does not exhibit the same uniform alteration as do the stomach and liver. Now here is a point of relation, and to what does it amount?—to no more than does the relation of scarlet fever to measles, in each of which the skin and air passages are affected. In Bilious fever we find what I consider undoubted marks of an inflamed stomach.

The mucous membrane is often red and injected, either punctated or arborescent, it is often softened, so as it can be easily scraped off with the handle of the scalpel; it is very often of a slate colour in protracted cases, but invariably traces of bile can be detected in the stomach or in the intestines. In Yellow fever, we also find the mucous membrane injected, but certainly much more generally and much more intensely than in Bilious fever. You do not always find black vomit in the stomach, because it may have been ejected just before death, but most generally you will. I have opened, in my day, several subjects dead from Yellow fever, in whose stomach black vomit was found, although not a particle had been thrown up. The patient, from Augusta, who entered the hospital in 1839, alluded to before, was one of these cases. Without a post-mortem examination, it might have remained in doubt as to what his fever was. Another case, amongst the very last of 1854, was examined after death by Dr. J. B. Read and myself, and black vomit was found in the stomach, although none had been ejected during life.

Black vomit is however generally found in the stomach; but it is found free in almost all instances, lying on the surface of the mucous membrane; but there are cases in which the peculiar flocculi of black vomit can be detected in the very mouths of the patulous vessels of the mucous membrane; and in some cases, I have seen a dark black arborescent injection running under or in the mucous membrane, exactly like the red arborescent injection so
frequently met with. Now black vomit is a hemorrhage. I expressed in 1842, (loc. cit.) my belief, that it was blood altered; in 1852, I detected blood corpuscles existing in it; I exhibited them under the microscope to Dr. Wragg, Dr. West, Dr. Read, Dr. Bulloch, and the late Dr. Ladd. Here is proof positive that the mucous membrane is the seat of a peculiar hemorrhage. Dr. Copland has some grounds in wishing to designate Yellow fever as the haemogastric pestilence. Whatever may be the real poison, it undoubtedly has a peculiar tendency to produce hemorrhage from the stomach; but there is a great deal of acid in the stomach, and it produces a peculiar effect on the blood, coagulating it into the floeculi of black vomit. The ejecta in Yellow fever, tested by litmus paper, always show strong acidity; it is the acid which turns the blood, and prevents the hemorrhage from being a mere hematemesis. Occasionally the hemorrhagic tendency of the disease is shown by its action on the bowels, and blood is passed downwards. Such cases, as far as my experience goes, are always fatal, are genuine Yellow fever; but must be distinguished from those in which the black vomit is passed per anum; in the latter case, recovery is more apt to follow than when the unaltered blood is passed. Now, if Yellow fever were but the highest grade of Bilious fever, we ought every season to meet with occasional cases in which black vomit would be found in the stomach after death, even if it were not ejected during life. Such cases have never occurred in my experience. But it is when we examine the liver that we find unmistakable evidences of the peculiar nature of Yellow fever. In Bilious fever we find the liver of various shades, dark brown, umber, bronze, but always gorged with blood. In Yellow fever it is always altered in colour, being pale and destitute of blood. The best colour to which I can compare it is boxwood. Some boxwood is of a dirty yellow, some of a brighter yellow; so of the liver, some arc of a light pale boxwood, almost a dirty ash white, some of a more pronounced yellow color. In a Bilious fever liver, by pressing a piece of white paper on the cut acini, you will stain it yellow, showing the secretion of bile still having existed up to the time of death; but this cannot be done with a Yellow fever liver. It sometimes contains a thin bloody serum, most generally it is almost dry. In 1827, Dr. Waring pointed out to me this state of the liver as the exact state presented in our fatal epidemic of 1820. In every case that I have had
the opportunity of examining from that time to the present, I have found the identical appearances. I examined cases at all times of our epidemic of 1854, and I found no variation of any account. I consider this conclusive proof of the identity of the disease, from 1820 to 1854.

One case early in the season, presented a mottled liver, that is, there were spots in it which had undergone the peculiar change incident to Yellow fever, and there were other spots in which the liver presented the natural Spanish brown color. I attribute this to the patient having died before the change in the circulating fluids had been sufficiently great to effect the alteration of the entire parenchyma. In some cases of Yellow fever, I have seen the gall bladder contain only a dirty, thick, viscid bile. In Bilious fever, it is always filled with bile.

In Yellow fever, the absence of bile is not confined to the stomach: you may search from the cardiac orifice to the anus without finding a trace of it; often have I done so, and never have I succeeded. I do not say, that a person who has the opportunity of examining every fatal case which may occur in a large hospital may not succeed better, but when a man is harrassed with constant demands on his time, as a physician in full private practice will be in great epidemics, he cannot examine every case. It was my object in 1854, to procure a record of the post-mortem appearances during the various periods of the epidemic, and I examined cases which died in August, September, October, and November, and I found a great uniformity in all of them—the discolored liver and the total absence of biliary secretion in the prime vae.

I was the attending physician of the Savannah Hospital for every summer, from that of 1835 to that of 1849, inclusive. I rarely allowed a fatal case of Bilious fever to escape without an autopsy. I state most distinctly, that in every case I found an abundance of bile in the intestines, if I did not find it in the stomach.

My examinations of the head in Yellow fever have been very few. In the great majority of cases, the cerebral symptoms did not induce me to do so. I have seen cases in which the head was involved from the commencement, and, doubtless, those cases would have furnished evidences of cerebral engorgement; but, as a general rule, the local manifestations of Yellow fever are in the stomach and liver. The existence of long continued stupor in Bilious fever made me examine the brain very frequently; indeed,
in the great majority of cases, and as I stated before, enough was found to account satisfactorily for the stupor;—serum was generally effused under the coverings of the brain, in the ventricles, and under the arachnoid, and the latter membrane was frequently opalescent. I have always considered the brain as a special organ for the local manifestation of Bilious fever, both from the decided symptoms presented during life, and from the post-mortem appearances.

I present for the inspection of the Society, drawings in oil colors taken from nature, of the appearances of the liver in the two diseases. An inspection will be better than any description. The Society cannot fail to see, at a glance, the vast difference from the brown and bronze of the four copies of the Bilious fever livers, and the light yellow boxwood color of the Yellow fever liver. It has been my object to sketch the prominent characteristics of the two diseases, presented during life and after death. If they be so widely and so uniformly different, how can we class them as the same disease, modified only by intensity?

The second opinion entertained, that Yellow fever is a disease, *sui generis*, special, distinct, has been sufficiently discussed in the consideration devoted to the opinion, whether it is only a higher grade of Bilious fever. It will be seen that I entertain this belief.

The question of its origin and propagation would, itself, afford scope for an essay; and mine has already occupied so much time, that I could not go into it now. I can bear my decided testimony that in no instance has there ever been the shadow of the shade of proof, that it ever was imported into Savannah from abroad. On the contrary, the proof is positive that its first victims had had no communication, direct or indirect, with any source of infection. Moreover, when the British steamer Conway, which ran to the West-Indies, touched at this port, I attended two cases of Yellow fever from her, both of which died in the city, and yet no disease was propagated from them. In March, 1841, (as will be seen by my article quoted before,) I brought a case from a ship from Demerara, and placed it in the hospital where the patient died. It is said that it can be propagated from abroad in a city, although most give up the point as to its contagion in the country. The whole experience of 1820 and 1854, when our citizens fled by hundreds into the country, and into neighboring villages, towns, and cities, does not afford a single instance where the disease was
spread by the fugitives. If then, it is not propagated into the country, and into other cities by land routes, why is it supposed to be so fatal when it comes by sea? If one case can originate in a place, why not ten or twenty? Case upon case occurred in 1854, in which the patients had not been near a deceased subject. Isolation was no protection. The poison, whatever it may be, spread like a pall over the whole city, and covered in its embrace all who staid, or entered its precincts; but a quarter of a mile beyond its limits the poison became innocuous. Such is fact. Let those who appeal to fancy, disprove it, or theorise upon it.

Again: facts prove that Yellow fever is a city disease. Exposure to swamp malaria, staying on a rice plantation in the summer, and in the fall before a frost, will produce a malignant and most fatal Congestive-bilious fever; but never, no never Yellow fever. Such cases of Bilious fever, as I stated before, I meet with every year; but, thank God, very seldom have I encountered cases of Yellow fever.

Yellow fever, in this locality, has this in common with Bilious fever, it never prevails except in the summer and fall months, and is most effectually cut short by a frost. As a general epidemic, it ceased to prevail in Savannah about the second week in October in 1854; yet the poison continued in the atmosphere until a frost, and attacked those strangers who imprudently returned into the city. The last resident whom I attended was attacked on the 25th of October. The cases which occurred afterwards were, without exception, strangers and unacclimated.

Since my connexion with the Savannah Medical College, I again attend the hospital, and it was there, and amongst seamen that I met with my last cases. They lay promiscuously amongst patients with other diseases, but in no single instance did any body catch the disease. I stated towards the close of that ever memorable season, that I would expect to meet with Yellow fever for a fortnight after a frost. I had taken up the belief that ten days, or a fortnight, was the period of incubation of the poison. My last case died at the hospital on the 27th of November; frost had occurred on the 13th of that month. The unfortunate subject had reached our city before the mighty destroyer of the poison had withered and destroyed its noxious powers.

Such, gentlemen, is my experience of the relation of Yellow and Bilious fever.

Savannah, April, 1856.
ARTICLE XXIV.

LETTERS FROM SAML. D. HOLT, M. D., UPON SOME POINTS OF GENERAL PATHOLOGY.

LETTER NO. 14.

MONTGOMERY, ALA., July 22nd, 1856.

Messrs. Editors—Having expressed the belief, that the liver is primarily affected, and largely concerned in the production of these fevers, (intermittents and remittents,) it is but proper that I should give some of the reasons which induce that belief.

The liver is a large central organ, having more intimate and extensive anatomical relations with other important organs and systems, and a greater number of offices and functions to perform, than any other organ in the body. It receives the blood from other organs, which is distributed through its substance, from which the bile is secreted; it receives and elaborates a large portion of nutritious matter furnished by the process of digestion, to which the bile largely contributes; it prepares the carbonaceous products for more easy and rapid combustion in the lungs, and thus aids in the production and evolution of animal heat; it eliminates the impure and effete matters from the blood which it receives, and thus acts the part of one of the principal depurating organs of the system. The liver, being an excitable organ, is liable to frequent interruptions in the performance of its functions, by causes which tend to increase or diminish its activity, either of which results must be felt in a greater or less degree throughout the system, but more particularly by those organs in immediate connection with, and dependent upon it. To say nothing of other causes, the stimulant effects of atmospheric heat (which, by-the-by, is essential to the production of these fevers) upon the liver, which it highly excites, often causes an increased, and sometimes very excessive, secretion of bile; and the bile thus secreted is generally imperfectly formed and depraved in its character, and in place of subserving its natural purposes, it acts as an irritant to the stomach and intestines, giving rise to bilious vomiting and purging, (than which nothing more rapidly prostrates the nervous or vital powers,) suspends the function of digestion, interrupts the process of elimination, and depuration, and, in a word, suspends or deranges all its functions, as well as those of dependent organs. But if these effects follow excessive action, from over excitement, let-
us see some of those which result from deficient action, from depression. The non-secretion of bile, which is essential to the digestive process—the non-elaboration of the materials of nutrition—the non-elimination of the impure and noxious substances from the blood, and all the consequences which grow out of these suspended functions, to say nothing of the effects resulting from bilious and sanguineous congestion of the organ, are the legitimate and natural results of torpor and acidity of the liver.

The question then arises—what cause is most likely to produce this condition of the liver? Without calling in the aid of other causes, which no doubt exert an influence in the production of these fevers—malaria, for instance—which may have the effect of increasing the bile matter in the blood, and thus increasing work for the liver, or which may have the effect of impairing the process of digestion and nutrition, thus impoverishing the blood and rendering it more difficult of depuration, we can find a cause (and the true one in my opinion) for this condition of the liver, in the loss of excitability from over action, consequent upon long continued and high ranges of atmospheric heat. While atmospheric heat is thus employed in the production of these fevers, at least so far as the liver is concerned, it exerts a large influence in determining their general character, with respect to the degrees of excitement, as it tends in the same manner to impair the tone and vigour of the whole nervous system, as it does the liver. When the febrile or reactionary movement commences in these fevers, in the manner which has been indicated, by the redistribution of the accumulated excitability to the organs whence it was derived in the formation of the cold stage, or stage of nervous depression, it will proceed with a rapidity, and progress, to an extent or degree of excitement proportioned always to the condition of the general system, in respect to the amount of vigour and excitability which it possesses, as determined by the operation of the individual and general predisposing causes, and by the presence, or not, of epidemic influences—thus giving rise to the modifications which these fevers assume with respect to the degrees of excitement, which I have attempted, accordingly, to classify. So far, then, as the febrile movement, and the essential type and character of these fevers are concerned, it matters not whether the excitability of the system is plus, or minus, or whether the excitement rises above, or falls below the line of a healthy excitement. If the system possess vigour
and tonicity, the depression will generally be slight and of short duration, and the reaction will be prompt, rapid and complete. If the system possess less vigour, and more elasticity and mobility, the reaction may be rapid, but will be less certain, complete, or permanent; and if the system is enfeebled and depressed, the reaction will be proportionately slow, feeble and imperfect, or incomplete—thus furnishing us with the inflammatory, the irritant, and the congestive forms or grades of these fevers, with such intermediate grades or degrees as have already been noticed.

Before I attempt an explanation of the pathological condition of the principal organs, as they exist in the second or febrile stage of these fevers, it is proper that I should first notice the condition of the skin, and the remote systematic capillaries, as they not only play an important part in the production of these opposite conditions of the system, but furnish the most prominent signs of the actual condition with respect to the degrees of excitement or depression. I have stated that in the formation of the cold stage, these vessels became deprived of their accustomed or proportionate share of excitability, and in consequence became relaxed, allowing of the free escape of the blood sent to them by the heart, which continues its action to some extent independent of the general loss of excitability; and the flow of blood being retarded in the pulmonary extremity, from a feeble respiratory movement, necessarily accumulates upon the right side of the heart, including the pulmonary artery and its ramifications. While this state of congestion gives rise to plethora, or engorgement in the organic capillaries, in the manner which has been described, the remote capillaries remain free from such engorgement, except in very extreme cases of depression, as in collapse and asphyxia, and this is owing to the valvular structure of the veins, and the remoteness of their radicles in the extremities from the seat of congestion, which has been shown to be in the lungs and heart, (congestion of the liver having no agency, so far as the remote capillaries are concerned). The cold, flaccid and shrunken condition of the skin and extremities, and the feeble action of the heart, indicate the feebleness of the respiratory movement, and the sluggishness of the pulmonary circulation, which is sometimes so great, that not only do the extremities become cold, but the whole surface of the body, and even the tongue and breath become so. This condition may exist in various degrees, from the slightest chill perceptible, to a complete
collapse, depending of course upon the degrees of nervous depression; and so may the febrile or hot stage vary in degrees of excitement, from a simple flush to the most intense febrile action. Now, as it is our duty to enquire what takes place in these conditions, it is equally our duty to enquire into their why and wherefore. It would seem, then, that the two extremities of the circulation were mainly employed in the formation of the cold and hot stages of intermittent and remittent fevers; that while nervous depression retards the flow of blood through the pulmonary extremity, the same cause, by relaxing, allows it to flow readily and freely through the remote capillaries, and the natural consequence must be an accumulation of blood on the right side of the circulation, as it exists in the cold, or congestive stage, and which must continue to exist, with the attendant phenomena, as long as the depression continues; and that, while excitement accelerates the flow of blood through the pulmonary extremity, it retards it in the remote capillaries, and the accumulation necessarily takes place on the left side of the circulation, as it exists in the hot or febrile stage. It is true, that the circulation of the blood depends upon the action of the heart, but the heart has not as much agency in the production of these conditions as we might be disposed to believe from first sight. The retardation of the blood in the lungs cannot be charged to a want of action in the heart, for in fact, the blood is thrown in excess upon the pulmonary capillaries by the heart's action: nor is the emptiness of the remote capillaries, in the cold stage, to be laid to the feeble action of the heart, as we see that the action of the heart is always sufficient to empty the arteries.

With this explanation of the manner in which the cold stage of an intermittent is formed, from a want of nervous power and action in the two capillary extremities of the circulation, in consequence of its accumulation in the liver, or other weak and laboring organ, which I have shown to be the first step or link, in the chain of morbid actions which follows, with the blood accumulated upon the right side of the circulation, with its attendant consequences and its phenomena, I will proceed to examine the consequences of a return or restoration of nervous power to the capillaries of the two extremities, being the commencement of the reactionary or febrile movement.

Upon an increase of the respiratory movement, and a freer flow of blood through the pulmonary extremity, the heart, in conse-
quence of an increased supply of blood, which has been revivified in its passage through the lungs, becomes increased in action and sends the blood out to all parts, as fast as it is received from the lungs. If the heart failed in this, and was not responsive to the stimulus of the fresh arterial blood, an increased flow of blood through the pulmonary capillaries would necessarily produce congestion in the pulmonary veins—a thing, I dare say, which rarely occurs. So far, the action of the heart is instrumental in carrying on the febrile movement, though not instrumental in starting it. The next event in the regular sequence is an increase of the general excitement, from the stimulus of the blood upon the nervous centres, and an increase in the activity or power of action, in all parts which share in the general excitement. If the remote or systematic capillaries receive their full share of excitement, and regain their contractile power, they will be enabled to retain their blood sufficiently long for the arteries to become refilled, which will be the sooner, in proportion to the flow of blood through the pulmonary extremity of the circulation. In this condition of things, we have a full development of the febrile stage, with all the phenomena dependent upon it, such as a hot, dry skin—full, strong pulse, &c., &c. But this prompt reaction, and high excitement, presupposes or indicates a full share of vigour and excitability in the system, which does not always exist in our latitude.

From the influence of causes, which have been sufficiently pointed out, it is generally the case in our climate that owing to less vigour, and greater elasticity and mobility of the system, independent of the immediate depressing influences which would retard reaction, notwithstanding there may be a free flow of blood through the pulmonary extremity, duly exciting the heart to action, and the nervous centres respond to the stimulant influence of the blood, promising a full and uniform, or perfect, reaction, the unequal distribution of nervous excitement is almost always certain to prevent it. Thus, if the remote capillaries fail to receive their due share of the nervous power, or excitability in its re-distribution, or of the new excitement kindled up by the reaction, through the nervous centres, the consequence will be that they will allow the blood to pass so rapidly out of them as to prevent the perfect filling up of the arteries, which is the case in the irritant forms of these fevers, and which are characterized by a hot, but generally moist skin, and a frequent pulse—the action of the
heart being spent in frequency of action on account of a diminished column of arterial blood, the nervous centres, in such cases, being often the seat of accumulated excitement, giving rise to delirium, convulsions, &c. A greater degree of depression, or less of reactionary power, with loss of tone and contractility in the systemic or remote capillaries, generally determines the grade of these fevers, which will be known by the feebleness of and frequency of the pulse, and the coldness and flaccidity of the skin, during the reactionary or febrile stage of the disease, or paroxysm. In all the grades of these fevers below the irritant, the febrile excitement, though generally manifest, never rises so high as to create or establish a general arterial plethora, or a preponderance of the blood upon the left side of the circulation, except in organs which have been engorged by previous congestion, as the brain, stomach, liver, &c., in the manner which has been described, or in such parts as may become the point of accumulated excitability, giving rise to irritation or inflammation, which conditions are usually manifested by increased heat of the head and body, while the extremities remain cold, or preternaturally cool during the paroxysm, indicating a want of tone or power of action in the remote capillaries.

Having given what I conceive to be the "rationale," or the physiological and pathological process by which the cold and hot stages of intermittent and remittent fevers are produced, which, so far as the two extremities of the general circulation are concerned, is applicable to the production of every other form, grade, or type of fever, whether the attack be preceded by depression and congestion, or whether (if such a thing be possible,) the system rise into a state of general febrile excitement without it, I feel it due to myself in order to prevent misunderstanding, to repeat, that the capillaries of the pulmonary extremity of the general circulation, under the influence of general nervous depression, fails to transmit the blood through them as fast as it is sent to them by the heart, and consequently the blood accumulates behind them; and that the remote capillaries, under the same influence, transmit the blood faster through them (except where it is opposed by physical obstruction,) than it is sent to them by the heart; and that a reversed condition and result occurs when these vessels are under the influence of general nervous excitement,—the heart in the meantime performing its part in the circulation of the blood, but not
controlling the action of the capillaries.* It may be asked, why re-
sort to a local origin, and to the doctrine of accumulated excitabil-
ity for the production of the cold stage, and its diffusion or re-
distribution for the production of the hot stage of these fevers,
when an easier and shorter way would have been to assign their
production at once, to the direct influence of the causes of depres-
sion and excitement upon these vessels? In my opinion, I have
given the true method of their production. In my last letter, I
stated that a condition of general depression and congestion could
be produced from a sudden and violent shock to the great nervous
centres; and so may a high state of general excitement be pro-
duced from some powerful cause, but in each case they would be
but ephemeral and transitory, while in these fevers these conditions
are permanent, or of frequent occurrence according to their type,
form, or modification.

The importance of these points of pathology, respecting the degrees
of nervous excitement, and the consequent unequal and irregular dis-
tribution of the blood, involving, as they do, the most serious conse-
quences, attendant upon these and, indeed, all other fevers, is my
apology for dwelling upon them with so much earnestness and per-
tinacity; for I candidly believe, that if they were looked to more

* I would not question the fact that the circulation in the capillaries is assisted,
as has been maintained by the influence of affinities, that is, that the arterial blood
in exchanging oxygen for carbon in the remote or systemic capillaries produces a
forward movement, and that a deficiency of oxygen in the blood, has the effect to
retard its movement in the remote capillaries, while in the pulmonary extremity
the affinity of venous or carbonized blood for oxygen produces the same result, and
that an excess of carbon retards it in the pulmonary capillaries. But this doctrine
does not serve to account for the broken balance which sometimes suddenly takes
place in the formation of the cold and hot stages of the fevers in question, and cer-
tainly does not serve to explain the causes of their production, if we leave out of
view the condition of the capillaries, with respect to the degrees of nervous excite-
ment and depression, upon which I maintain these disturbances in the circulation
mainly depend. And if we attempt to explain the production of these phenomena,
according to the doctrine of affinity, we will find that the condition of the capilla-
ries have no agency in their production, but that they depend upon the condition
of the blood with respect to its amount of carbon and oxygen; and, we will be com-
pelled to look further for the causes which produce that condition, which will be
found as I have maintained, in defective and imperfect aeration from an enfeebled res-
piratory movement, whereby the quantity of oxygen is diminished; or from defect-
tive and imperfect depuration from suspended functions of the liver, whereby the
quantity of carbon is increased in the blood, which after all are but the effects or
results of deficient nervous excitability.
closely by the profession at large, our cardinal remedies would receive the credit of their true "behest," and these diseases would be treated with more uniformity and success than they generally have been. It is not in the spirit of arrogance, but of self-gratulation, (which, by-the-by, is about my only reward.) that I say, that the principles which I am endeavoring to advocate have carried me through a practice of nearly thirty years, with, perhaps, as few disappointments, or causes of regret, as the most favoured of my cotemporaries. It is not very delicate, I know, for a professional man to blazon his success, even in the ears of his colleagues; but, as an argument in support of this principle which I advocate, I will state that, in the management of certain formidable diseases, scarlet fever, for instance, though I have encountered it in all its forms and varieties, through several epidemics, besides many individual or sporadic cases, I have had the good fortune never to lose a case. It is but justice, however, to myself and the profession, to say, that I have pursued no plan, and used no remedies, which are not recognized and prescribed by the best authors, with this exception, that viewing the disease as self-limiting, having a definite course to run, according to its essential and specific nature, I have generally avoided as much medication as the books prescribe, but have left it to its course, watching, however, for such symptoms as did not belong to it in its essential character, and treating such upon the general principles which I am now advocating, to which, next to peculiar good fortune, I must ascribe my success.

In the management of yellow fever my success has been little less than in scarlet fever; and so of typhoid fever, and various forms of congestive diseases, the details of which will hereafter be given—among others, a severe epidemic congestive pneumonia, over sixty cases of which I treated one season, without the loss of a patient. It is not my privilege to say as much of some other epidemics which I have encountered, such as cholera, and cerebro-spinal meningitis. But in these, the want of equal success depended rather upon the want of a timely and proper application of the principles of which I speak, than from error or defect in the principles themselves, as, after all, I have not been able to ascertain that any other plan of treatment was more successful. I am not so vain and arrogant as to suppose that all cases of those malignant diseases which I have named are amenable to treatment, and that a great many of them would not terminate fatally under any known plan or system of
treatment; but I do say, that hundreds and thousands of cases terminate fatally, which under a timely and proper course of treatment would recover; and that in all such cases, where the physician has been called in time, the responsibility is upon him, and he cannot, upon the plea of their malignancy, or the impotency of the remedies at his command, evade, or shrink from it. Nor should he be too ready to claim the honor of success, as there are hundreds and thousands of cases which would recover without his aid or assistance. Such, no doubt, was the case with the largest number of the cases of scarlatina and typhoid fever which have fallen into my hands, which ("an honest confession being good for the soul") I am willing to confess, though I could not say so much with respect to the other diseases named.

Upon a redistribution of nervous power, or excitability, those organs will receive the largest share, whose capillary vessels have been the seat of engorgement from venous congestion. These have been shown to be, the brain, stomach and intestines—the liver and spleen; and these, in consequence of irritation thus kindled up in them, become the points of determination and accumulation of blood, as it finds its way, more or less rapidly, through the pulmonary extremity of the circulation. From the direct action of the causes of irritation, the capillaries of these and other organs may become the seat of engorgement, (which may or may not excite a febrile movement in the system,) which is the antecedent condition to inflammation in these organs. Thus, we see that the capillaries of these organs are doubly liable to engorgement, and, liable to double engorgement; for, if the reactionary powers of the system are insufficient to effect a full and complete reaction, and entirely remove the congestion, which is generally the case in our worse forms of fever, these organs, especially those which are connected with, and are dependent upon, the liver, (which often remains in a state of congestion after the lungs have been set free,) must necessarily remain to a greater or less extent, in a state of engorgement, to be increased from an increase of blood thrown upon them in consequence of the febrile reaction; and whether the reaction be partial or complete, these organs will always claim, and will generally receive, more than their full share of the circulating fluid. Hence it is that so many of these fevers are characterised during the febrile stage or exacerbation, by heat of the body and head, but preternatural, cool extremities, with a variable, but generally a feeble or frequent
pulse; and this is the class of fevers which I have designated as congesto-inflamatory and congesto-irritant. Should there exist a greater amount of excitability in the system, with an active pulmonary circulation, the degrees of animal heat will be proportionably exalted, and the action of the heart increased, giving great celerity to the blood which is in active circulation, and diffusing heat throughout the system. In this form of fever, which we recognize as the irritant, and by far the most common in young subjects—notwithstanding the reaction is often violent, as evinced by the increased action of the heart, and the great increase of heat which extends to the whole surface and the extremities—the reaction is never complete or permanent, for the reason that the enfeebled and relaxed condition of the remote capillaries permits such a free escape of the blood, that the arteries fail to be filled up, in consequence of which the pulse never requires much strength, but is always frequent, and the skin, though hot, is generally moist, a state of things peculiarly characteristic of this form of fever. Now, while the remote capillaries participate for a short time, and to a limited extent, in the general febrile excitement, the organic capillaries always come in for their full share, if not more, as evinced by the delirium and convulsions, and the copious bilious vomiting and purging which are almost the constant attendants upon the exacerbations of this form of fever, abundant secretion being one of the diagnostics between this and the inflammatory form. The irritation excited in the brain by the stimulus of highly oxygenized blood, consequent upon the active pulmonary circulation, constitutes a new and altogether different morbid action from that which existed in consequence of mere physical obstruction and engorgement of its capillaries from congestion in the lungs, manifested by a change of the dull, inactive and comatose state of the brain into a state of delirium, or phrenzy, and convulsions. This condition is sometimes the result of sympathetic action, in which case, though the manifestation may be nearly the same, the irritation will be more transitory and evanescent, and seldom passes into a higher or inflammatory grade of action, which a long-continued or frequent renewal of the condition, from the recurrence of paroxysms, is apt to do.

Another source of disturbance in the functions of the brain, and other nervous centres, is to be found in the changed character and depraved condition of the blood; but cerebral disturbance from this cause seldom takes place in the early stages, or in the milder forms
of these fevers, and is only found in the protracted and violent cases, in which the excitement has been so high as to suspend for a length of time the functions of secretion and excretion, allowing of the accumulation of poisonous matters in the blood, which should have been thrown out by the liver and other deputating organs; and the blood thus depraved, being thrown upon the brain and other nervous centres, unlike the freshly oxygenized blood, instead of excitement, produces depression, with low delirium and coma, the characteristics of the typhoid condition. It seems to be scarcely necessary to say that the inflammatory form of these fevers is distinguished from the irritant, by the more equally diffused and permanent character of the excitement—by the greater strength and less frequency of the pulse—by the more uniform and often less intense heat—by the dryness of the skin, and the suspension of the secretion generally—by the more protracted febrile exacerbation, and generally by less cerebral disturbance in the earlier stages of the disease.

Notwithstanding the frequent occurrence, and the important consequences of disturbance in the functions of the brain and other nervous centres in these fevers, there are other organs not less frequently involved—the derangement of whose functions are no less important and serious in their consequences than those of the brain—these are the liver, stomach and intestines. The condition of the latter, in connection with congestion of the former, in the cold stage of these fevers, I have already considered, and as these organs are all subject to the same states of morbid action, and the same consequences and results, from exalted excitement, it will be unnecessary to notice them separately, and I shall therefore confine my remarks, chiefly, to the functional derangements of the liver. These fevers have generally been called bilious, but whether they are entitled to the appellation from an excess, or deficient secretion of bile would be a difficult matter to determine. A certain degree of excitement is necessary for the performance of the functions of secretion, and whether it is too high, or too low, it becomes alike suspended; hence we notice that in the cold stage of these fevers, and particularly in those more decidedly congestive in their character, the vomiting and purging, which is a common occurrence, consists in the discharge of a glareous, mucus, or serous matter, having more or less acrimony, without any of the properties of bile. But upon the return of excitement, and the establishment of a febrile reaction, large quantities of bile are often poured out, giving rise to
violent and obstinate bilious vomiting and purging. This excited, and active secretory function of the liver, often occurs as an idio-pathic affection, but when it occurs as one of the phenomena, or as a concomitant of these fevers, it is usually in connection with the milder and more simple forms of intermittents, attended with a lively reaction, or with the earlier stages of remittents having the irri-tant form or character, of which it may almost be said to consti-tute a characteristic feature. But in the more advanced stages of these fevers, and in those of higher grades of excitement, as well as those of a lower, the suspension of the biliary secretion is one of the most common occurrences, and constitutes one of the most serious difficulties with which we have to contend. It is important, there-fore, to enquire into the causes, and then into the consequences, of the suppression or suspension of the secretion. We have stated that over action from excessive stimulus, induced debility and ex-haustion in the nervous tissues and have ascribed to this condition of the liver, (a state of depression from excessive action from the stimulus of atmospheric heat,) the origin of these fevers, and have shown the consequences which resulted, not merely in the suspension of the biliary secretion, but the formation of a portal congestion, and the consequent obstruction and interruption of the functions of other organs, and the changes thereby effected in the constitution of the blood. Now it must be borne in mind that the liver is liable to congestion and engorgement from several different causes—one from congestion of the lungs, from local debility or depression in its vessels from excessive excitement—or arterial and capillary engorgement and engorgement of the biliary ducts. The first, re-sults from the general causes of depression; the second, from general or local causes of depression in the capillaries of the portal veins; the third, from excitement and the stimulus of freshly oxygenized blood accumulated in the hepatic artery and its capillaries—mani-festing the signs of irritation, and perchance, the signs of inflamma-tion—but I must say, that in my whole practice, I never met with a case which I could recognize as inflammation of the liver; the fourth, engorgement of the biliary ducts and gall bladder, from the accumulation of bile consequent upon its previously abundant or excessive secretion, which, from its retention in these vessels, and having parted with its thinner portions, it often becomes black and inspissated. Hence, it is, that we often see harrassing vomiting, and purging of thin bilious matter of various color and consistence,
On Scarlatina. [September,

followed by the discharge of large quantities of black or deep green and consistent bilious matter, which is always hailed as the harbinger of safety to the patient, especially, if it has been the work of a substantial dose of calomel.

Being admonished that I am transgressing the usual limits allotted to my letters, I must close for the present, and resume the subject in my next letter. Hoping that you will not tire from reading, before I do from writing about these things, I remain, as usual,


On Scarlatina, and its Relations to Rheumatism, Carditis, and Albuminuria. By Dr. W. Hughes Willshire, Assistant Physician to the Charing-Cross Hospital.

[The complications of scarlet fever are very numerous, and will be found amply treated of in works on the subject; but rheumatism is one of those which, though common, has been but very cursorily noticed, excepting within the last few years. In 1851, Betz, of Heilbronn, drew particular attention to it, and went so far as to hint at an essential relation between acute rheumatism in the child and scarlet fever.]

He implies, at least, that an arthritic affection in young children often could not be told from the exanthem in question. That he had seen albuminuria and desquamation of the skin in acute rheumatism in children who had shown no exanthematosus eruption; and, on the other hand, that the eruption was not an essential symptom of scarlet fever, leaving it, therefore, a very difficult, if not impossible, case in which to arrive at a differential diagnosis. In 1853, M. Troussseau stated that he had very frequently met with the complication we are speaking of; that the arthritic disorder sometimes, though but rarely, became generalised, and attained a high degree of intensity, accompanied by delirium and other nervous symptoms terminating in death. In 1854, I myself read a paper before the Medical Society in London on Rheumatism in Children, in which I adverted to its union with scarlatina (reported in 'The Lancet,' 1854, vol. i., p. 138); and a few months afterwards, Mr. Haydon, of Bovey Tracy, communicated some very interesting facts in 'The Lancet,' in reply to a commentary, as it were, upon my paper. You will find Mr. Haydon's communication in the first volume of that journal for last year. This gentleman has had some very corroborative experience, and states, that although he cannot coincide in the views I have detailed of Betz, he admits there is a remarkable analogy between the phenomena, especially the secondary ones of the two diseases; and that so closely does he consider the two related that he makes it a rule, on the decline of the rash in scarlet
fever, to administer lemon juice. I strongly advise you to read Mr. Haydon's remarks, and then to turn to Betz's paper, which you will find in the eighteenth volume of the 'Journal für Kinderkrankheiten.' In both essays you will find how easily certain cases of the two affections—I say certain cases, mind—might be confounded together.

If we were to take extreme forms of the two diseases—viz., on the one hand, an almost universally bright-red child, with sore throat, strawberry tongue, who had been known to have been with other children similarly affected, and on the other hand, a pallid, scrofulous child, without any cynanche, with a white-coated, moist tongue, and swollen and painful joints, there would certainly arise, I should say, no difficulty about the matter. But you must recollect scarlet fever is not always of this characteristic appearance; the sore-throat may be next to nothing, or absent; the rash only evident about the joints, which may be a little puffy and painful; the tongue but moderately and simply coated, and the child not known to have been directly exposed to the zymotic virus. On the other hand, if what is stated be true, albuminuria may be temporarily present in rheumatism, as also, according to Betz, desquamation of the cuticle. Now, in a case presenting such equivocal signs as I have hinted at as possible, I do not know that from these signs alone, and from these only, the differential diagnosis could be perfected. But this I must believe, from my own experience up to the present, that the further course of the malady, together with the commemorative history of the patient, would not fully help me out of the difficulty. The difficulty, in truth, has not yet occurred to me; but let me tell you that in India and in America a somewhat analogous one has happened to others, and from this, and what you will glean from Dr. Betz and Mr. Haydon, it is not impossible we might hereafter have a case occur amongst us, or an epidemic disorder, which should have such a mixture of the symptoms of rheumatism and scarlatina that we should be much puzzled to give it a satisfactory name.

A late patient of mine had been surrounded by the scarlatinal poison; she had sore-throat, diffused red efflorescence; the tongue and even breath were those of scarlet fever. The rash subsided, desquamation just began around the neck, when pains of the ankle and wrist joints came on; the parts were swollen and shining, and a kind of secondary fever was lighted up. Now, whatever might be thought of this latter affection, the former was clearly not rheumatism. That there is some obscurity about the exact nature of a disease occasionally attacking not only children, but adults, and marked by arthritic pain and tension often of a severe character, accompanied by a scarlet rash, is plain from the accounts given by some American and East and West India practitioners. For instance, in 1824, 1825, and 1847, outbreaks of a disease like the one I have just mentioned occurred in the East Indies. In the summer
of 1828, the malady appeared epidemically in some of the southern cities of the American Union, after having previously prevailed in some of the islands of the Gulf of Mexico. In Charleston it spread with great rapidity, ultimately attacking almost the entire population. In this latter city the last occurrence of it that I have found recorded was in 1850, and is alluded to by Dr. Dickson, of South Carolina. In Calcutta, again, during the hot and rainy season of 1853, a variety of the same affection prevailed, and has been well discussed by Dr. Goodeve in the first volume of the 'Indian Annals of Medical Science' for that year, and to which I must refer you. Now, this disease has been called scarlatina rheumatica by Cocke and Copland, exanthesis arthrospia by Nicholson, and the strange cognomen of dengue has been popularly bestowed upon it. I have said that the first account of this disease goes not further back than 1824; but it is proper I should mention that Dr. Dickson, of Charleston, writes as follows:—"I recognize Rush's 'break-bone fever' of 1780 in Philadelphia as the first notice of a malady such as I have called dengue," and Dr. Waring, of Savannah, alludes also to dengue under the title of "eruptive breakbone."

I cannot go into the details of this peculiar disorder: indeed, I have only alluded to it to show you that there occurs a fever, accompanied by arthritic pains and a red exanthem, whose true relations to scarlet fever yet require clearing up. However, I may just remark, that Dr. Dickson regards dengue as a distinct contagious disease, giving immunity from second attacks, and that it is not scarlatina; whilst Mr. Goodeve states that most of the symptoms characteristic of the latter affection found expression in some of the cases occurring in Calcutta. He says, "the fever eruption, reddened mucous membrane and tonsils, desquamation of cuticle, swelling of hands and feet, state of the tongue, albuminous urine, and all such as go to make up scarlatina; but it would be premature to assert that the cause is identical with that of scarlatina. I should say that it would require a longer series of observations of several epidemics, with careful examination of all the attendant circumstances, before we could pronounce the diseases to be identical." Again, Dr. Copland affirms, that the disease called dengue "was not a form of scarlatina is shown by the severity of the rheumatic or neuralgic symptoms," &c.; and "that it was not a rheumatic fever was shown by the undoubted propagation of it by infection," &c. I see that in some later observations by Dr. Mackinnon, in the third number of the 'Indian Annals,' he doubts whether any of the epidemics of India described as attended by red efflorescence of the skin can be identified with any of the varieties of the scarlatina of Europe, denies the disease we have referred to to be contagious, and for which, he says, the name of "the red fever" is as good a name as any other.

On reviewing the different accounts given by Indian and American writers of this puzzling affection, the disease appears in certain
places, and epidemics, to have had more the characters of rheumatism; whilst in others it has had more those of scarlatina. The case mentioned may be said to illustrate, so far as my own experience extends in respect to the relations between scarlatina and rheumatism, one form of the arthritic complication—namely, that occurring early in the course of the exanthematosus disease. When it so occurs, the specific inflammation about the joints terminates, like primary or idiopathic rheumatism, in resolution or delitescence. But it may take place at a later period of the disease, when all has been thought to be over. It occurs after desquamation has made some progress, and then may terminate in the supplicative crisis. Now, this is peculiar about the rheumatic inflammation generally of young children; unlike in adults or older children, it may lead to purulent effusions into joints, and also about them. I have known this to occur in several instances, and it is more liable, I believe, to ensue in the secondary rheumatism of scarlatina. This is a fact well known to authors. If I recollect aright, Dr. Kennedy, of Dublin, has published something upon it, and Trousseau, I know, states that scarlatinal rheumatism is often more dangerous when localized to a single joint, than when attacking several articulations at one time, as it is then more inclined to terminate in suppuration, and even eventually to result in caries of the articulating surfaces. This circumstance, however, of the arthritic inflammations of infants and young children, not unfrequently terminating in the formation of matter, has led some pathologists to deny the true rheumatic nature of the disease in question. I must confess, too, it does seem to me not improbable that the results of pyohæmia, or purulent infection, of phlebitis, umbilical or otherwise, may, along with other forms of abnormal action, have been occasionally placed to the credit of ordinary rheumatic inflammation. But Mr. Henry Lee, who has paid great attention to the subject of purulent infection, states that he has seen, as the result of the absorption of sero-purulent fluid from an ill-conditioned abscess, very severe rheumatism, affecting, in some instances, the pericardium and dura mater. In these cases there was occasionally more fluid secreted than in ordinary rheumatism, but in milder examples there was no such distinctive mark; they are said to have been "in every sign and symptom apparently identical."

I have undoubtedly seen cases in children, which I should not have known from rheumatism, terminate in the supplicative crisis. If it be said, then such cases therefore could not have been rheumatism, the argument is of course settled; but it appears to me only so by a petitio principii. In adults, again, rheumatism does occasionally, however rarely, terminate in the formation of matter. In this case of mine some mischief ensued certainly to the mitral, if not also to the aortic valves. It may be but slight, still there it is, and may lay the foundation for changes in after years connected with the walls of the heart's chambers. The abnormal sound (which
some of you have listened to) at the apex is very loud, but this is not proof of the amount of structural mischief at the mitral, for there may be much noise and slight change, and but little noise and great alterations. The former, I hope, is the case here; but from the pulse being so small at the wrists, my hope is not to be too much trusted too. A German pathologist affirms it to be a general thing in scarlatina for the first sound of the heart to be aspirated or blowing, which is a result of the altered state of the blood and not of the valves, and is a proof that scarlatina and all its complications arise from one common blood disorder. I presume he must mean to refer only to the sound at the base, and not at the apex (where our worst one is), of the cardiac region. Sounds at the base may be of hemic origin, but at the apex they are, I believe, always of structural derivation.

The liability of the heart to become affected during scarlatina has been known for some time. Roux in 1819, and Krukenbergius in 1820, are said by Rilliet and Barthez to have referred to the occurrence of pericarditis in connection with scarlatina and measles. Trousseau says Bouillaud pointed it out; but certain it is Mr. O'Ferrall, in 1835, detailed to Dr. Graves, of Dublin, how he was obliged to have recourse to leeching, calomel, and James' powder, to overcome acute pericarditis in connexion with scarlatina. Several later writers, as Burrows, Willis, Joy, &c., have noticed this complication; and not long ago, M. Trousseau stated it as his belief, that "many cases of organic disease of the heart, which only become evident at an after period, have had their origin in scarlet fever." But, so far as I know, we are most indebted to Dr. Scott Alison for prominently bringing this matter before our notice, though it has been said that the frequency of the complication has been somewhat exaggerated. You will find Dr. Allison's original paper in the 'Medical Gazette' for 1845. The case I have mentioned makes the third one of heart complication during scarlatina that I have seen, and here it has occurred in connexion with an arthritic affection. One might a priori, suppose the sequences of the phenomena would always thus present themselves, but it seems that it is not so, as the involvement of the cardiac organ is alluded to by writers, as well as seen by myself, without the arthritic affection having preceded or accompanied it. I find it remarked that in the Stuttgard collection of papers on "Children's Diseases," the occurrence of purulent collections in the pericardium during scarlet fever is mentioned by Von Ammon, whilst effusion of pus beneath the periosteum and in the substance of muscles has been recorded by others. It is, therefore, possible that the rheumatism in this case had, in one sense, nothing in itself to do with the cardiac affection, but that both were the effects of one same and common cause, and that this cause was the same condition of the blood which gave rise to every other manifestation of the scarlet fever.

But it is said that the rheumatism occurs, like the dropsy, from
unnecessary or incautious exposure of the child to cold; whereas, in the latter case, we find the disease will come on where no such exposure was known to have taken place. We frequently find this secondary rheumatism occur without any cardiac involvement arising, while the primary form, in children, is very far more apt to include the central organ of the circulation within its grasp than at other periods. Had it been my intention to treat of scarlatina or rheumatism substantively, I must, when speaking of the nature of these maladies and their complications, have said much about uric and lactic acids, of urea in the blood, of a specific poison, of the necessity of that poison being eliminated from the system, of the peculiar irritative action of the urea or some other poison in the blood upon the serous membranes, of an elective affinity in scarlatina for the kidneys, and many other topics of the like nature. But I should have felt it my duty to have impressed upon you at the same time, that these would but have helped us to hypothetical and very obscure explanations of points which it must be confessed an exact and valid pathology is not as yet able to include within its grasp. All that I now lay stress upon, therefore, is a certain observed sequence of phenomena—Scarlatina, Rheumatism, and Heart-disease; a sequence observed sufficiently often, and under such intimate relations of time and place, to warrant us in associating these phenomena in casual connexion, however the true relations of this connexion may be afterwards definitely fixed. I would also, in conclusion, recall to your memory, that whilst we have had recorded experiments on animals, in which the kidneys have been removed, or the ureters tied, &c., and cases related where the secretion of urine has been suppressed from stoppage of the ureters by calculi and new formations, and which cases and experiments have been followed by vomiting, dryness of the tongue and mouth, &c., intense thirst, altered or husky voice, serous profluvia, great perspiration, convulsions, deep stupor, and death. I am not aware that dropsy, affections of the synovial membranes, of the pleura, or of the pericardium have followed, as in scarlet fever.—[Lancet.

On Dilution as a Principle in Therapeutics. By Benj. Bell, Esq., Edinburgh. (Read before the Medico-Chirurgical Society.)

In the administration of remedies of a metallic nature, we may err in not having them sufficiently diluted. Our purpose should be to gain ready entrance for them into the blood: take for example iron. We know that the entire blood of an adult does not contain more than 30 grains, and that this is deficient in anaemia. But what use can there be in giving such large quantities when so little is required? Our doses, then, ought to be small in quantity, and amply diluted. We may learn the same lesson by examination of medicines prepared for our use in the great laboratories of Nature. The
famous spring at Pyrmont contains about half a grain in the pint of water; that at Tunbridge Wells only about one-third of a grain; the same also in the bromine and iodine springs of Kreuznach; they exist under the same condition of remarkable dilution. But we cannot doubt their virtue in glandular swellings and scrofulous diseases. May we not, then, take a hint from nature, and prescribe our metallic preparations in smaller doses, and largely diluted?]

Of late years, I have seen reason to believe that all the benefit derivable from certain metals, as remedial agents, may be secured by comparatively small doses, provided that they are sufficiently diluted. I have seldom trusted to what could be called very minute doses; but so far as my observations go, I have a strong impression that, within certain limits, we do not increase the efficacy of such medicines by adding to the quantity prescribed. For example, eight or ten drops of the muriated tincture of iron in a large glassful of water, will, if useful at all, do as much good to our patient, as double or treble the quantity. And a similar remark applies, in my opinion, to the iodide or bromide of potassium, a single grain, or at most two of either, in a glass of water, being capable of effecting all the benefit of larger doses. We have already seen that the natural springs of world-wide celebrity present these ingredients in the proportion of one-third of a grain to sixteen ounces, and therefore we shall probably find, upon more extended trial, that corresponding doses of our pharmaceutical preparations, similarly diluted, will answer every purpose.

It may be observed in passing, that although no strictly analogous argument, furnished by the chemical composition of natural springs, can be urged in regard to the administration of antimony, the principle of dilution is probably no less applicable to that important remedy. Without venturing to speak very dogmatically on the subject, I have a pretty strong conviction that we both increase its remedial power and lessen its tendency to cause sickness by diluting our antimonial solution. Many practitioners suppose, that without the production of nausea, we miss our object in subduing pulmonary inflammation, the principal disease for which that metal is prescribed; but I doubt the soundness of this view, and am inclined to think that every object may be gained without sickening our patient, just as iron and mercury can produce their curative effects without the former causing headache, or the latter salivation. Our aim should be to bring the antimonial into contact with the capillaries of the inflamed part, and this, probably, will be best promoted by presenting it in a form fit for immediate absorption into the circulating fluid. When this is attended to I find it seldom necessary to give more than the sixteenth or twenty-fourth part of a grain of tartrate of antimony in successive doses.

Another à priori argument, in favor of this practice, may be adduced. Unless a metallic medicine be administered in a state of proper dilution, that condition will be secured by the secretion of
more or less fluid within the stomach itself, in order that the foreign matter may be in a fit state for being taken up and appropriated by the blood. But it is evidently desirable that this secreting process should be kept within as narrow bounds as possible, in every case standing in need of tonic remedies, and therefore, if it can be rendered unnecessary, even in part, by previous dilution ab extra, we secure an advantage for our patient. If, however, we still adhere to large doses of the active ingredients, it is obvious that, in order to effect the necessary dilution, we must increase the entire quantity of liquid so enormously as to oppress the stomach, and derange the digestive process.

Other advantages of moderate doses, largely diluted, taking their efficacy for granted, will be readily admitted; for besides being less hurtful, it may be, to the teeth, they are also less unpalatable and better adapted on that very account for being taken and digested with the food, a point, the importance of which has been long fully recognised by practical men. That this last advantage is not imaginary, may be gathered from our familiar experience regarding the chloride of sodium. Mixed with the food at meals, although useful in helping digestion, it has no aperient action; but the same quantity dissolved in water, and swallowed when the stomach is empty, before breakfast, will almost certainly open the bowels. In the former case, it is absorbed gradually in company with the food; in the latter, being presented too abruptly and strongly to the fastidious capillaries of the portal system, it is rejected by them, and hurried onwards through the canal, from convolution to convolution, and hence its qualifications as a purgative. But, if the solution be a concentrated one, it will probably never reach the bowels, but act as an emetic, and be rejected at once from the stomach. And cases are met with, in the annals of toxicology, where death itself was occasioned by large and irritating doses of common salt. Supposing it, however, to act as a purgative, the explanation of its action given seems preferable to the endosmosis theory, which appears to be too mechanical, and ignores entirely the vital affinities and endowments so conspicuous in the living body. We have supposed the solution of the chloride of sodium to be moderately concentrated, like seawater, or in the proportion of about a drachm to four ounces; but, if we double the allowance of water, we shall probably find that the action is no longer purgative, but diuretic. The salt has entered the circulation and will be eliminated by the kidney.

It may be objected, perhaps, to the principle we are advocating, that it takes for granted that the whole of the medicine swallowed is absorbed and received into the blood, whereas, allowance should be made for a great part of it passing through the alimentary canal unchanged, and more, therefore, ought to be administered than we can expect the system to appropriate. That this superfluity does exist, when large doses are given, we have already suggested, and also that it is probably a source of injury to the patient; but the
likelihood of such being the case, is obviously much less when the
dose is small and well diluted. Moreover, we are very apt, I sus-
pect, to over-estimate the quantity of any foreign substance which
is needed, in order to act upon the human body.

I have repeatedly seen so small a dose as the eighth of a grain of
extract of belladonna taken into the stomach of a young person,
cause full dilation of the pupil. Now, if we only consider how ex-
tremely minute must be the portion even of this very small dose,
when circulating through the blood-vessels, which could come into
contact with the nerves of the iris, we may form some conception
of the susceptibility and delicacy of our frame. To take another
illustration, let us consider the remarkable efficacy of the muriated
tincture of iron in erysipelas, or in acute desquamation of the *tubuli
uriniferi* following scarlet fever. An adult is labouring under the
former of these complaints, with much febrile disturbance, as indi-
cated both by the hot dry skin and the frequent bounding pulse;
we administer ten drops of the tincture every two hours, and after
two or three doses, as I have often seen, the symptoms give way, the
skin becoming moist and cool, and the pulse slow and soft. A few
grains of iron, at the very most, have mingled with the circulating
blood, and of these a mere fractional portion has reached the congest-
ed capillaries of the inflamed region, and occasioned the favourable
change, and in this way, a comparatively small dose of a substance
confessedly foreign to the system, comes to fulfil the purpose we
have in view with all the efficiency of a larger quantity.

A boy of ten years, we shall suppose, who has recently passed
through an attack of scarlet fever, is observed to have oedema of the
eyelids, a furred tongue, and a full, febrile pulse. His urine is of a
dingy colour; it deposits a dark sediment, and is scanty and albu-
mious. Five drops of the tincture of iron in a glass of water, are
administered every four hours; a speedy improvement commences,
and in two or perhaps three days, the symptoms are entirely gone.

Such facts, besides enforcing the principle we have been incul-
cating, should teach us to deal tenderly at all times with an organ-
ization so finely constituted, and to administer our remedies under
a more settled conviction that, if potent for good at the right time,
they may also be potent for evil, when the case and the occasion
are not favourable.

One may express himself in this manner, without verging in the
very least towards the credulity of those persons who profess their
faith in the power of infinitesimal doses. It is one thing to recom-
mand half a grain in preference to five, and a very different thing to
stand up, with unabashed countenance, for the millionth or billionth
of a grain. The human mind is baffled in forming even a concep-
tion of such infinitesimal quantities; and, therefore, they may be
placed in a category by themselves, for the use of those who take
pleasure in believing everything that ordinary people are compelled
to doubt. But is there no truth in the allegation, that some of our
large doses of remedies, inteneed to enter the circulation and mingle with the blood, must startle and stagger many reverential students of the human frame and functions, and so create, it may be, a hurtful reaction in favour of homœopathy, and other kindred eccentricities?

I would venture to suggest, that some topical appliances—those especially of a discutient nature—may be rendered more efficacious and useful, by attending to the same principle of ample dilution. For example, the most efficient mode by far in my experience, of employing iodine locally, is a solution consisting of two grains of iodide of potassium and one of iodine, in an English pint of water. It must be used warm, as a fomentation to the affected part, by means of a sponge, twice or thrice a day, for five or ten minutes at a time. In cases of glandular tumour and of fibrinous exudation into the cellular tissue; in chronic enlargement of the testicle or epididymis; in short, in all that class of disorders where iodine or mercurial ointment would seem to be indicated, I have often found it remarkably efficacious. The warmth of the application renders it more agreeable to most patients than if it were cold or unctuous, and at the same time probably increases its power by relaxing the pores, and so favouring absorption. It seems to be much preferable to the tincture of iodine painted upon the surface—a favourite remedy with many. The latter certainly does good in some cases, but we may doubt, I think, if it produces its beneficial effects in the manner usually attributed to iodine and its compounds. It is a strong stimulating fluid, and sometimes occasions a good deal of local suffering; so that, when attended by favourable results, it probably resembles in its action a common blister, or the croton-oil ointment, and relieves inward congestion or chronic inflammation indirectly, by its power as a counter-irritant, and not directly, by hastening the process of absorption. The distinction is not unimportant; because some swellings, of inflammatory origin, may reasonably be expected to give way under a system of counter-irritation; while other growths, of a slower and colder constitution, will be more amenable to a treatment less active and heroic.

Another illustration of the same principle seems to be afforded by the outward uses of croton oil. When employed in a form tolerably concentrated, it induces, as we all know, an eruption of minute vesicles; but when mixed with a large proportion of olive oil, and rubbed over an extensive surface, such as the abdomen, it sometimes shows its power as a purgative, in the most unequivocal manner. One case in particular, that I attended with the late Dr. Thatcher, occurs to me, in which this treatment was eminently successful. A young gentleman seemed to be dying of obstruction in the bowels. As all the ordinary means of relief had been exhausted, and circumstances forbade the farther use of internal remedies, a liniment, composed of one part of croton oil and twenty-four of mingled olive and camphorated oil, was used in an embroca-
tion to the belly; and very speedily, and in consequence, as I believed at the time, the peristaltic action downwards was restored, and the obstruction overcome. Of course, we may have mistaken in this, as in other cases, the post for the propter hoc; but such facts seem to suggest the propriety and desirableness of new experiments in the same direction.

The spiritus terebinthinae is another local remedy which we may probably employ with advantage, in a less stimulating shape than we are in the habit of doing. This has been suggested to my mind by witnessing the effects of small doses of turpentine in certain forms of iritis, and in rheumatic ophthalmia. In exercising these curative effects upon textures so remote from the stomach, the medicine must be supposed to enter the vascular system, and to modify the blood by stimulating one or more of the organs of excretion; and it is reasonable to infer, therefore, that the well-known remedial power and efficacy of terebinthinate embrocations in certain chronic forms of muscular rheumatism and neuralgia, may depend very much upon a similar therapeutic action. And on this supposition, we should take care that the local remedy is not presented in a form too stimulating, our object being, not so much to cause counter-irritation by it, as to secure its absorption by the integuments covering the seat of pain.—[Edinburg. Med. Journal.


The author passed in review the actual state of obstetric science and practice in relation to the pathology and treatment of placenta prævia. He showed that the prevailing belief was, that so long as the delivery of the child was not effected there was no security against hæmorrhage, and that hence the rule in practice of proceeding to forced delivery as early as practicable was almost universally inculcated, the only exception consisting in the more or less general substitution of the plan of totally detaching the placenta. The author thus showed that, while the practitioner was anxiously waiting for the moment when the dilatability of the cervix uteri would permit the passage of the hand, for the purpose of turning, the patient might perish of flooding; and that, therefore, in the most severe class of cases, those of central placenta, some other resource, some means of placing the patient in security against renewed flooding, before the full dilatation of the os, was eminently desirable. Dr. Barnes then explained the physiological course of a labour with placenta prævia, and the mode in which Nature sometimes arrests the hæmorrhage before the expulsion of the child. He illustrated, by the help of an ingenious and interesting diagram, that a stage of labour arrives when the recurrent contractions of the womb do not entail any further flooding; that the pains return in their usual course, with the usual effect of fur-
other dilating the os uteri, and forwarding the labour, but without causing any further flooding; that the labour was in fact resolved into a natural one, and would be safely concluded by the natural powers. Reflection upon these cases had led Dr. Barnes to doubt the truth of the obstetric dogma, which declares that there is no security against haemorrhage, so long as the presence of the liquor amnii or the child in the womb prevents full contraction. Dr. Barnes then unfolded the anatomical, physiological, and clinical facts which led him to the conclusion that, under proper restrictions, Nature might in many cases be trusted with more confidence than was generally believed. The clinical facts, he said, had come to him first, and led him to examine into the anatomical and physiological bearings of the case. He related cases in illustration, and quoted a commentary upon one of these cases from the memoir he had published ('Lancet,' 1847), in order to establish his priority in the enunciation of the views he now laid before the society; and adverted to the fact, that he had since the publication of that memoir, constantly taught, in his lectures on Midwifery, the same doctrine; and stated that even the original of the diagram now exhibited, rudely sketched, was also figured in the memoir referred to. He had, therefore, believed his views to be original, and was surprised to find, in several numbers of the 'Berlin Monatschrift für Guburtskunde' for the present year, a controversy between Dr. Cohen, Dr. Credé, and Professor Hohl, in which Dr. Cohen for the first time expounded similar views to his own, whilst Dr. Credé, referring to writings of 1853-54, also claimed them; and Professor Hohl assigned them to Dr. Zeitfüchs so far back as 1843. Dr. Barnes acknowledged, with pleasure, that Dr. Cohen's views of 1855 fully confirm those put forth by himself in 1847; but he found, in the writings of Credé and Zeitfüchs, nothing whatever to show that either had, in the remotest degree, possessed himself of the points in question. But Cohen had gone beyond the author in proposing a new operation based upon the physiological and clinical facts expounded. This operation consists in—1st. Determining the side of the uterus to which the smaller flap of the placenta is attached; 2. In rupturing the membranes and detaching the placenta from this half of its circumference; 3. Exciting uterine contraction; 4. Hooking finger over edge of placenta, tearing membranes from the freed border of the placenta; and 5. In separating the placenta in a circumference of 190° to 200°. The greater half of the placenta, now freed from the dragging of the lesser half and membranes, is now drawn back, just as in placenta lateralis, with the uterus. From this moment there is no further danger. Cohen insists, like the author, on the impropriety of hastening labour unless urgent complications arise. Dr. Cohen refers to his experience to prove the efficacy of this method, but does not recite any cases in illustration. Dr. Barnes pointed out that the difference between his memoir and
that of Dr. Cohen consisted simply in this:—Cohen had, in 1833, carried forward the principle Dr. Barnes had enunciated, in 1847, by proposing the artificial partial detachment of the placenta, instead of trusting, as Dr. Barnes had recommended in certain cases, the execution of this operation to the powers of Nature. The author then quoted from Sir Charles Bell passages showing that the anatomical distribution and physiological action of the muscles of the uterus accorded with and explained the clinical facts observed in the course of intermissions and cessations of hemorrhage from placenta praevia as set forth by Dr. Barnes. The author then explained the mode and mechanism by which the hemorrhage in placenta praevia is arrested. The opening of the mouth of the womb, and the detachment of the placenta adhering to this part, are effected by the active contraction of the longitudinal muscles of the uterus; this active contraction shortens the cervix, when it intermits a passive contraction goes on, which maintains or even increases the shortening of the cervix. This shortening necessarily compresses the torn mouths of the vessels, and checks the flooding caused by each successive detachment of fresh placenta, until the detachment has gone to the boundary line, beyond which point the further expansion of the cervix has no effect, and when all fear of flooding is at an end. It was not therefore necessary that the uterus should be empty in order to arrest the flooding. The arrest depends upon the contraction of the cervix, which went on, although the fundus and walls were prevented from contracting. Dr. Barnes submitted the following as some of the conclusions deducible from his researches:—

1. In cases of placenta praevia, the hemorrhage is sometimes arrested spontaneously before the complete detachment of the placenta, before the discharge of the liquor amnii, and consequently before the expulsion of the child or the pressure of its head against the cervix.

2. That this spontaneous arrest of the flooding is owing to the sealing up of the vessels torn by successive detachments of placenta, and the attainment of a stage of labour when no further detachment can take place until after delivery.

3. That dangerous and even fatal flooding sometimes occurs while the os uteri is still closed, and so undilatable as to render it impossible or expedient to have recourse to forced delivery.

4. That in such cases it is eminently desirable to possess some means of diminishing the hemorrhage until the hand can be passed through the os uteri.

5. The spontaneous or artificial detachment of the cervical portion of the placenta competes with two most formidable operations, dangerous to mother and child—forced delivery, and the total separation of the placenta. The new principle of treatment may, in many cases, supersede forced delivery altogether; since the patient being secured against further flooding to resort to turning
when the flood has ceased is an unnecessary proceeding, although
the os uteri may admit of it. In many more cases this principle
will be the means of gaining the necessary time to admit of turn-
ing or other modes of forced delivery being performed with safety.
In almost every case it may supersede the practice of wholly de-
taching the placenta, since the end in view being the arrest of the
flooding, it is better to detach only just so much of the placenta as
will effect this end, than by detaching all, to destroy the child.

Medical Properties of Charcoal. By JAMES BIRD, Esq.

Pure charcoal is known to possess two singular properties, first,
the power of absorbing gases to an almost incredible extent, and
secondly, the capability of rapidly oxydizing any dead animal or
vegetable substance placed in contact with it, as explained by the
experiments of Dr. Stenhouse and others. As charcoal is not
chemically affected by either acids or alkalies, and is quite insolu-
ble, its admixture with other substances is not incompatible, so
that it may be combined with any other kind of medicine, ad libi-
tum.

It is a powerful absorbent of fluids, whether aqueous or aceriform;
and as it unergoes no change in the human stomach, it may with
great truth be described as the only pure absorbent we possess, for
the same cannot be said of aether, lime, magnesia, bismuth, or any
other mineral absorbent, all of which form salts with the acids
they meet with, sometimes to the complete subversion of all their
original properties, while the vegetable or farinaceous absorbents,
such as flour starch, gum, &c., are changed by digestion, giving
off frequently offensive gaseous emanations as the results of mal-
assimilation.

Charcoal is, therefore, recommended as a pure and effective ab-
sorbent, applicable either alone, or in combination with other
remedies, to a cure of a large number of the acute disorders of the
mucous surfaces of the alimentary canal, and also of those of the
uterine passages, and particularly in those instances where the
secretions are inordinate in quantity, or offensive in condition.

It may, perhaps, induce a more extended trial of charcoal pow-
ders if a few instances were named in which the remedy has proved
eminently serviceable. In the exquisitely painful small ulcers
within the mouth, on the inner surface of the lips or cheeks, which,
at irregular intervals prevail to some extent, and are extremely
troublesome, the following wash will be found an excellent reme-
dy:—

B. Pulv. carb. ligni pur. ij.; mel. rosæ ʒ i.; decoe. cydoniae
ʒ iiij.; aq. dest. ʒ iv. M. Fit. lotio sepe adhibenda.

This lotion is also useful in excessive ptyalism; it speedily mod-
irates the discharge, and instantly corrects its fetor, and its appli-
cation is extremely soothing and agreeable. The same may be said of it when used as a gargle in ulcerated sore-throat, the sloughs separating rapidly and easily, leaving a healthy surface underneath.

In the earlier stages of diarrhoea, a mixture composed somewhat as follows speedily gives relief:—

B. Pulv. carb. ligni pur. 5 j. ad 3iv.; mucil. acaciae 3ij.; syr. auranti, tr. cardam. co. aa. 3iv.; aq. dest. 3ij. M. Sumat 4 drachms 3ia, vel 4ta q.q. hora.

In the bowel affections of children, accompanied with worms, powdered charcoal, in doses of 10 to 15 grains, with one grain of ipecacuanha powder, and from 3 to 5 grains of rhubarb, taken at bed-time, acts like a charm, correcting disordered secretions with certainty and comfort, and promoting a healthy tone and action. In gastralgia and gastrodynia, charcoal powder, in doses of 30 or 40 grains, three times a day, in water, seldom fails to give relief; and in cases of severe tenesmus, accompanied by bloody or mucous stools, an injection into the lower bowel of one drachm of charcoal powder in a small quantity of thin arrow-root or gruel has been found to give almost instant relief.—[Ibid.

On Chronic Ulcers of the Legs. By F. C. Skey, Esq., F.R.S.

Abundant have been the examples of treatment of chronic ulcers of the legs by means of the internal administration of opium. I know of no treatment at all comparable to this, so rapid or so efficient. These cases have had many observers, who can bear testimony to its value. Exactly in proportion to the amount administered is the regeneration of the defective structure. Years will probably yet elapse before this principle will be universally acknowledged, before the medical community will admit that in opium we have an agent far more valuable than that derived from its comparatively worthless power as a sedative. It is notorious, that the chronic ulcer—the disease of ten or even of twenty years' duration—is unattended with pain. To suppose that opium effects its local marvels by any sedative property in the drug, appears to me worthy only the advocacy of a senile-female, or of an un instructed youth. If we administer five grains of the soap and opium pill, night and morning, to a man advancing in life, who has been carrying about with him for ten years a large callous ulcer, with an ash-coloured base, surrounded by high walls of organized lymph, and in which there remains not a vestige of activity, good or bad, advancing or receding, the moisture from which is a hot ichor, becoming sanious under provocation,—if we examine carefully this surface at the expiration of forty-eight hours, we shall find it speckled with red points; these are future granulations, and, in ten days, the whole of the base of the wound will be carpeted with them.—[Lancet.
Practical Deductions from a Clinical Record of Twenty-six Cases of Strangulated Femoral Hernia.

Mr. Birkett, in a paper read before the Medical Society of London, (April 26th, 1856,) commenced by stating that the object of the paper was, first, to bring prominently into the foreground the causes of death; 2d. The circumstances by which those causes are brought about; and, 3d, The means by which they may be avoided. It was shown, by means of a table of the cases, that a certain number of unfavorable circumstances occurred in each case, and that, in proportion to the aggregate, as a general rule, the case was cured, or terminated fatally. But in some of the cases only two, three, or four unfavorable circumstances existed, and yet the patients died; and in these, as well as others with a larger number, the causes of death were sought for and demonstrated. Of the twenty-six cases, all of which were operated upon by the author, one-half terminated fatally. In the fatal cases, death resulted from causes over which the operation could have but little influence; and it was undertaken only with the view to place the patient in a condition more favourable to recovery. The causes inducing the fatal result may be thus enumerated:

1. The consequences of a journey performed while the patient was suffering with strangulated femoral hernia.
2. The defective constitutional nutrition of the patients generally.
3. Irrecoverable prostration, the result of long continued vomiting and strangulation of the bowel in aged women.
4. Violence inflicted on the hernia. To this cause, the death of not less than five out of the thirteen is to be attributed.
5. The administration of purgatives before the operation.

The author unhesitatingly preferred to reduce the hernia without opening the peritoneal sac in those cases in which the surgeon would be justified in returning the protrusion by the taxis, if it could be accomplished.

In the twenty-six cases, the peritoneal sac was opened in twelve, and the causes which prevented the reduction of the hernia without so operating were the three following:

1. The contents of the sac.
2. The morbid condition of the contents of the sac.
3. The dimensions of the neck of the sac, and the unyielding state of its tissues.

Six cases were related in which the author had reduced the hernia by a simple division of the fibrous tissues about the neck of the sac, and external to that covering of the hernia known as the fascia propria. To this simple method of relieving the constriction around the bowel the author gave the name of the "Minimum Operation." The causes of death in the fatal cases were shown, by post-mortem examination, to be referable to peritonitis, injury of the bowel inflicted in the taxis, exhaustion after fecal fistula,
phlegmonous inflammation, collapse, acute bronchitis, and perforation of the bowel. Of the cured cases, the minimum of hours during which the bowel was strangulated was three hours; the maximum was seventy-seven hours. Of the fatal cases, the minimum period of strangulation of the bowel was eleven hours, the maximum seventy-nine hours. Of the cured cases, the average number of hours during which the bowel was strangulated amounted to twenty-three. Of the fatal cases, the average period of strangulation of the bowel was forty-six hours. The causes of death were primary and secondary: 1. Prostration; peritonitis; gangrene of the intestine; perforation. 2. Bronchitis; abscess behind the peritoneum; phlegmonous inflammation and suppuration. The circumstances by which they were brought about: Age; a journey; the defective constitutional nutrition of the patient; the morbid state of the canal above the strangulated piece of bowel; injury of the hernia caused by the constriiction of the ring, and by manual violence inflicted on it; the duration of the sufferings; the intensity of the constitutional sympathies; fecal fistula; neglect of the tumour; the administration of purgatives; the warm bath. The means by which they may be avoided are: By care in manipulation; the early relief of the bowel from constriction; the reduction of the hernia without opening the peritoneal sac; the exhibition of opium, and the avoidance of all causes likely to induce exhaustion.—[Med. Times and Gazette.

On Myeloid and Myelo-Cystic Tumors of Bones: their Structure, Pathology, and Mode of Diagnosis. By Henry Gray, F.R.S.

The author detailed the history of nine cases of tumors of this form removed during life, with a minute description of the results of his own microscopical examination of six of the tumors.—The results at which he arrived were as follows:—That these tumors were not of a malignant nature, although in several of the instances given they have been so regarded both previous to and after removal by operation; that on the contrary, their minute structure bore the closest analogy with the normal constituents of the marrow and other elements of bone in the early periods of life: that their growth is confined to the osseous texture, or its investing membranes, the periosteum and dura mater; that they occur at a period of life when the normal constituents of the medulla exist in the greatest amount, and are developed in those parts of the osseous system in which those structures exist in a most distinct and well-marked form, (all the cases given took their origin in the epiphysial ends of long bones;) that they are occasionally mixed with the other elements of bone in a rudimentary state, as fibrous tissue and cartilage, and even with bone itself; that they may probably occur in any bone; that since they are thus found to consist of an abnormal amount of some of the nor-
mal constituents of the medulla cells, the name "myeloid" given to them by Mr. Paget is most appropriate (the author proposes to add the term "cystic" to such of them as present a mixture of cysts with the structure above described, and regards their fibrous element as most probably derived from the organization of lymph effused as the result of chronic inflammatory action, or from some abnormality in the development and growth of the fibrous element of bone:) that they occur in all the cases at present recorded at an early period of life, and that their growth is generally much less rapid than malignant disease, both which facts afford important diagnostic marks to distinguish them from malignant growths; that the absence of the malignant cachexia, of glandular lymphatic enlargements, and of diseased internal organs, combined with the facts that, although these tumors attain occasionally a considerable size, yet they present no tendency to ulcerate or obtrude externally, and generally retain some surrounding shell of bone within which they have grown, serve as additional aids to the surgeon in forming a diagnosis between myeloid and malignant growths; that they do not return when entirely removed; and that for all these reasons they are to be regarded as innocent tumors.—[Association Med. Journal. New York Med. Journal.


Four forms of dropsy are observed in pregnant women, which are far from being of the same importance.

1. Mechanical Dropsies, perhaps the most common, are due to the pressure exerted by the gravid uterus, their production being favored by the lesser density of the blood in pregnant women, and the slight diminution of albumen that exists in its serum. These dropsies are confined to the lower extremities, are of no importance beyond their inconvenience, and disappear after delivery.

2. Dropsies due to changes in the Blood, but unaccompanied by Albuminuria.—The change in the blood which induces these dropsies, consists in a diminution in the amount of the albumen of the serum, a diminution that is sometimes considerable, and for which we can assign no other cause than the fact of the pregnancy, and its influence on the various immediate principles of the blood. This description of dropsy, like the two next, tends to become general. It is of importance to distinguish it from the two others, and especially the 4th, for it does not predispose to eclampsia. It is by analysis of the blood alone that we can establish its existence. It disappears also after pregnancy, but far more slowly. It has been observed that women suffering from it remain feeble for a long period, their "getting up" being slow and difficult.

3. Dropsies with Changes in the Blood, and Albuminuria, but without Bright's Disease, properly so called.—These dropsies are the
Operation for Pharaphimosis.

[September,

consequence of the diminution of the albumen of the blood, produced by its deperation through the kidney. Until lately it was supposed that such loss might take place without material lesion of the kidney; but from the investigations made by M. Robin and the author, it results that this albuminuria is due to a special modification taking place in the epithelial cells of the tubuli, a modification consisting in the infiltration of the cells and tubuli by numerous granules of a proteric nature. This infiltration is analogous to that which M. Robin had already found in choleraic albuminuria, and like it is susceptible of cure. The absolute diagnosis during life of this disease from Bright's affection is very difficult, and yet it is highly important, as the prognosis must be entirely based upon it. It is in women who are the subjects of these dropses that we have to fear eclampsia, and the predisposition to puerperal peritonitis. Eclampsia is not, however, a necessary consequence; and when we find general dropsy, change in the blood, and albuminuria co-existing, we still cannot affirm that this terrible accident will follow. On the other hand, whenever we find eclampsia we are certain of finding, not only dropsy, but albuminous urine, and change in the blood. In respect to the termination of this form of dropsy it may be observed, that if eclampsia does not supervene, a cure is almost certain, while, in the case of its occurring the result is dependent upon that of the eclampsia.

4. Dropses due to Bright's Disease.—It is very important to establish the diagnosis of this form. We may lay stress upon the somewhat larger quantity of albumen, the presence of fragments of tubuli, of fibrinous filaments, and fatty globules. When eclampsia complicates this form it is invariably fatal; and even when eclampsia does not occur, the disease is not arrested after delivery. The dropsey continues to increase, the termination proving, after a certain period, fatal.—[London Med. Times, from Medico-Chirurgicale.

Operation for Pharaphimosis.

At the session of the Academy of Sciences (Paris,) on the 21st of April, the following extract was read from a letter of M. Malgaigne:

"With this strangulation, as with strangulated hernia, we attempt at first to accomplish reduction, and usually succeed. But when reduction is impossible, it is advised, as with strangulated hernia, to divide the bridle which strangulates it, even if it is necessary to repeat this section at two or three points. The danger of strangulation is thus diminished, but the reduction still continues to be impossible. At least I have never seen it accomplished after such an operation. What is the reason of this want of success? It is that the preputial ring, in producing inflammation,
ulceration, sometimes even gangrene, of the parts strangulated, commences by thickening the subjacent cellular tissue, and by producing extensive adhesions between the integument and the cavernous bodies. Dividing the stricture, though repeated, does not destroy these adhesions, and does not suffice for the reduction, while destruction of these adhesions, even without division of the stricture, is sufficient to allow the return of the parts to their place.

"Thus the study of this affection has led me to distinguish a new element, hitherto left in the shade. The establishment of this element gave a new indication, and this is the way in which I have met this indication.

"A young man came under my care the 11th of this month, for a paraphimosis of five days' duration, and already there was seen upon the back of the penis a superficial ulceration, embracing more than half of the circumference of the organ. The internes tried to reduce it without success. The next day, at the visit, I was no more fortunate; the adhesions of the integument to the cavernous bodies presented an insurmountable obstacle to it. I slipped a narrow bistoury flatways between the integuments and the cavernous bodies, by means of which I divided those adhesions to the extent of one centimetre (four-tenths of an inch). This did not suffice. I carried a probe-pointed bistoury into the incision, to complete the division of the adhesions throughout their whole extent, and the reduction was accomplished with the greatest facility. The next day, the engorgement of the prepuce had diminished, the third day the ulcerated surface had cicatrized, and the patient went out the 20th of April, having been well several days, and without experiencing any kind of accident."—Am. Med. Mon.

On the Action of Digitalis upon the Uterus. By W. H. Dickerson.

The writer commences his paper by stating that during the month of October, 1854, a patient in St. George's hospital, laboring under most severe menorrhagia, was cured by the infusion of digitalis, exhibited for the relief of cardiac affection, from which she also suffered. In consequence of this he had been induced to try the remedy, by the permission of Dr. Lee, in a series of cases of uterine hemorrhage which had occurred in the hospital. These cases, of which a table is given, were seventeen in number, and the general results of their treatment were as follows: In every case of uterine hemorrhage, unconnected with organic disease, requiring the employment of active remedies, admitted into the hospital after October, 1854, the administration of digitalis was had recourse to as the sole treatment, and the discharge was invariably arrested by it. The time which elapsed before the hemorrhage subsided varied with the dose in which the digitalis was exhibited. When large doses were given, as an ounce to an ounce
and a half of the infusion, the discharge never appeared after the second day; when smaller doses, it never continued beyond the fourth day. In uterine hemorrhage connected with organic disease, the remedy acted with less certainty; its exhibition was required for a longer time, and the effect was sometimes transient. The author then spoke of the mode in which the digitalis operated in controlling uterine hemorrhage; and after concluding that its effect could not depend on the sedative influence of the drug in the heart and arteries, he showed, by various experiments and observations, that the arrest of the hemorrhage was due to the action of the digitalis on the ganglia of the uterus, by which the organ was stimulated, and the muscular substance powerfully contracted.—[Dublin Hospital Gazette.

Eneuresis.

Two methods for treating this troublesome affection having been given in our May number, we present the following, which we adopted in our practice long ago, from the Medico-Chirurgical Review, of January, 1849.—Memphis Med. Recorder.

Ex. Belladonna.

Ex. Hyoseyami, aa. gr. xvj.

Saechari albi, 5j.

Aq. Camphorae, 3 jiss. Take a teaspoonful at bed-time.

In obstinate cases we have sometimes repeated the dose two or three times in the same night. We have rarely failed of success, though in one case we were obliged to give up the remedy before the cure was complete, in consequence of the excessive dilatation of the iris. We remember one case, where the disease had continued from early childhood till the age of seventeen, which was permanently cured by a week's use of the above prescription. The young man was unable to recollect a period at which he had not been constantly troubled with an attack on retiring to sleep. Where remedies so diverse eure the same disease, we are naturally led to the hypothesis that, though the symptoms are identical, the pathology is different; for example, we are disposed to think that where Dr. Merrill's remedy—the Iodine—was successful, irritation of the mucous lining of the bladder, was the cause of mischief, while in cases where the present treatment effected a cure, (Belladonna having a special tendency to the muscles of organic life,) the sphincter vesicae had been the seat of the disease. D. F. W.

On the Detection of Phosphorus in Cases of Poisoning.

Mitscherlich has published a very simple and satisfactory method of detecting phosphorus in forensic investigations. The matter to be tested for phosphorus is to be distilled in a flask with water and sulphuric acid, and the vapors conveyed through a
Bloodletting in Young Subjects.

Dr. Beck justly remarks that young subjects do not bear the loss of blood as well as adults. They fall into syncope more readily, and their lives are almost endangered by it. That their nervous systems are more powerfully affected, is considered by the fact that convulsions and coma more frequently occur after the loss of blood in children, than in adults. Nor will a repetition of the remedy be so well borne by the child as the adult, and if carried only a little too far, children sink under the loss of blood irretrievably. Leeching exerts greater power over children than adults, because of the greater vascularity of the skin, the effect resembling more nearly that produced by general bloodletting. Hence the so frequently fatal effects of leeching; the difficulties of which are further increased by the uncertainty as to the quantity of blood drawn. Too great caution cannot be exercised, therefore, in leeching children. The operation should be performed with the child in an erect posture, and as soon as paleness of the lips or face appears, the bleeding should be arrested, and the patient should be closely watched to prevent hemorrhage, which may prove quickly fatal. Dr. B. recommends matico as the best astringent remedy for ar-
On Sulphate of Cinchonia in Intermittent Fever. By Dr. Jno. S. Dukate, of Fredericsburgh, Ind.

In the "Medical Observer," for January, 1856, I find an article entitled, "Report of fifty-seven cases of Intermittent Fever treated at the City Dispensary by the Sulphate of Cinchonia, by Dr. J. C. Welles." I have always held, that in order to test the therapeutical virtues of any article of the Materia Medica, especially of the so-called specifics, we should administer it alone, if tolerated by the stomach without combination; for who can tell, for instance, whether cinchonia or quinia cured the patient, when both are given together or alternately? Dr. Welles' article is not quite satisfactory, although very nearly so. In the account of case first, he states that the patient had taken ten grains of quinia combined with opium and piperine. He had a chill on the 7th and 8th— took cinchonia and had no chill on the 9th and 10th; he was then put upon sub-carbonate of iron. On the 26th of September he had a severe chill, and was again put upon cinchonia, and convalesced. So far as this case is concerned the treatment was too complicated to be entirely satisfactory, but it is not so with nearly all the other cases.

My object, however, in communicating this article, is to call the attention of the profession to the remedial virtues of cinchonia in intermittent fever, and to offer to them my experience with the medicine. Perhaps I may not have used the article so extensively as some others, but still my experience will contribute something to the common stock, and assist in judging of the value of the remedy.

I had never administered a grain of cinchonia until I saw Dr. Welles' article, when I determined to give it a fair trial in intermittent fever—which I have had a fair opportunity to do, as it has prevailed extensively in my locality during the past spring. In order to test its remedial value I administered it alone in every case, or at least using no other antiperiodic. I preceded its exhibition, however, always by an active cathartic. I have now used three ounces of this article in intermittent fever, and have come to
the conclusion, from what I have observed in using the above amount, that it is superior to the sulphate of quinine in the treatment of this peculiar and tenacious disease. Patients, also, seem to take it more readily than they will quinine, having, most of them, from frequent use become tired of its repetition.

The following cases may possess some interest:

1st. Mrs. B., aged 24, contracted an intermittent fever last autumn, which was promptly arrested by quinine, but relapsed again and again; paroxysms continuing at intervals during the fall, winter and spring. She became anemic, with a tendency to drop-sical effusions. On the 1st of March I put her on the use of cinchonia—ordering her twenty grains divided into five grain doses, exhibited every four hours. It was of the tertian type, and on the day for the paroxysm she had some unpleasant coldness of the extremities, followed by slight febrile excitement.

After this was over she took eight grains more, and has not had another chill to this date, (June 15th,) and is in the enjoyment of tolerably good health. She is now using, for enlargement of the spleen, the proto-iodide of mercury internally, and iodine ointment externally.

2nd. Mr. S. was attacked last fall with intermittent fever, which became chronic. All the so-called specifics were used, but to no purpose, except to arrest the disease for a short time. He was put, by me, on the cinchonia without combination with any other medicine; after using thirty grains his disease was arrested, and has not since returned. A period of two months has now elapsed since he has taken any of the medicine, and previous to this his ague had returned about every two weeks.

3rd. This was a case of chronic ague, which had resisted quinine, and nearly all other remedies. He was put upon the cinchonia, and has now been free from the disease about one month.

This article might be extended by relating special cases, but it is unnecessary. It is sufficient to say that in every case of intermittent fever, whether recent or chronic, the sulphate of cinchonia has completely arrested the disease, and in but rare cases have there been any relapses. It may be thought by some that I have given the remedy more credit than it deserves, but I am of a different opinion. In every case I have given it alone for the purpose of testing its antiperiodic powers. No complication of effect has been produced by using quinine, piperine, salicine, strychnine, arsenic, or any other potent remedy. It is very clear that it is to the sulphate of cinchonia alone that the cures are to be attributed. I therefore feel justified in using it in all cases of simple intermittent in preference to quinine. I do not yet know enough of it to place that reliance upon it that I would on quinine in pernicious fevers, malarial neuralgia, &c., &c.

Effects on the system.—This article does not produce the same disagreeable effects upon the nervous system that quinine does. In
no case has my patient complained of tinnitus-aurium, vertigo, cephalalgia, or gastric disturbance. In short, patient's feel no inconvenience from its administration.

Doses.—I think from twenty to thirty grains of cinchonia are required to completely arrest an attack of intermittent fever. My mode of prescribing it is to weigh out twenty grains and divide it into five doses, one to be taken every three or four hours, between the paroxysms, and in the absence of fever. If this quantity does not completely arrest the disease, I then give from eight to ten more, and the object is certainly accomplished. The cinchonia I have used is prepared by Powers & Weightman, and costs me one dollar per ounce.

[In a letter to one of the editors, from E. C. Woolley, M. D., of Butler county, in this State, the following language is used:—

"I have used the sulphate of cinchonia exclusively, with very happy effects; succeeding in every case but one, in arresting the paroxysms of ague by the first administration of the medicine. I think it not at all inferior to quinine as an antiperiodic."—Eds.]

[Cincinnati Med. Observer.

Treatment of Typhus Fever. By Edwin R. Maxon, M. D., of Geneva, N. Y.

What, then, are the indications in the treatment of Typhus? Shall we bleed, purge, and starve our patients, and thus increase the debility which the morbific agent has produced? Or shall we strive to support the sinking energies of the system, and thus enable it to bear up under and throw off, with its various excretions, the debilitating morbific agent it has so unfortunately imbibed.

From my experience in the treatment of typhus fever, for the past few years, I am compelled to believe, that with a proper sustaining course of treatment, nearly every case of typhus may be arrested, and the patient convalescent, by the fifth or sixth day; and that very few need be kept in longer than the ninth day, if attended to in season, and before any serious local inflammation has taken place.

The indications then are, to remove from the alimentary canal, any irritating matter which it may contain; to equalize the circulation; to promote perspiration; and to support the sinking powers of the system by tonics and a due amount of proper nourishment.

I therefore usually give, at first, two or three blue pills, and follow with half an ounce of castor oil; use warm pediluvia, morning and evening, the first day or two; also, rubbing the whole length of the back with a tepid infusion of capsicum in vinegar; and, generally, give the sulphate of quinine and Dover's powders, of each, grs. jv, every six hours, at first, for a day or two. I then
discontinue the Dover's and give pulv. camp. gr. j, with quinine sulp., grs. ii, every six hours, and continue till the fever is arrested, giving crust coffee, with milk, at first freely, as nourishment, and as soon as the stomach will retain it, toast every six hours; and, by degrees, other nourishing and digestible food, as the appetite generally returns and calls for it.

Such are the means which I have found most effectual in arresting typhus fever; and even in cases in which, through neglect, or maltreatment, local inflammation has supervened, I have found this course to do well, in conjunction with sinapisms, dry cupping, or blistering, as the case may require.—[Buffalo Med. Journal.

Mode of Reducing Strangulated Hernia, after failure of the Taxis, by a Bloodless Operation.

M. Seutin, the eminent surgeon of Brussels, is endeavoring to establish, in a Belgian Medical Journal, the superiority of tearing either the inguinal or crural ring, over incising the same, for the reduction of strangulated hernia. He quotes experiments on the dead body, and several successful cases; and is confident that his method will soon supersede the operative measures generally resorted to. He places, first, great reliance on graduated taxis continued with due precautions for a considerable period; and when this fails, he endeavors to hook his index-finger round the margin of the ring, by passing it between the tumour and the abdomen; and by using a certain force, he causes the fibres of the external oblique to give way and crack to an extent sufficient for the reduction of the hernia. M. Seutin defends his practice with considerable ability, and hopes trials will be made.—[Lancet.

Alkaline Treatment of Rheumatism.

The (Burlington) Medical and Surgical Reporter gives a table of twenty-six cases of acute rheumatism treated by the alkaline method, in New York Hospital, under the care of Dr. John H. Griscom. The average time that the patients were under treatment was about thirteen days, and the whole duration of the attack twenty-one days. The treatment consisted in the administration of the supertartrate of potassa and soda, every hour, and the application of an alkaline and opiate lotion to the swelled joints. As the urine became less acid, corresponding improvement in the symptoms was noticed.—[Boston Medical and Surgical Journal.

Danger of Employing Iodine Injections for the Cure of Hydrocele.

M. Gosselin made an interesting communication to the Société de Biologie, on the 24th of May. He has ascertained that in three cases where, after the death of patients, he has examined the testi-
icles, there is a peculiar danger in employing iodine injections in the vaginal cavity as a means of curing hydrocele. This danger consists in the absence of the secretion of a sperma fit for fecundation. In these three cases, no spermatozoa were found in the seminal vesicle of the side, where a hydrocele had been treated by iodine injections. In experiments upon dogs, M. Gosselin has found, also, that after such injections, the production of spermatozoa does not take place, and that the testicle becomes pale and smaller than before.—[Med. Times and Gaz.

Etiology of Congenital Deafness.

In a paper read before the French Academy of Medicine, M. Ménière states that the intermarriage of relations has more effect than any other cause in producing deaf-mutism. This is shown by the fact that the disease is nowhere so common as in those isolated communities where almost all the inhabitants are related to each other, as the Canton of Berne, in Switzerland, where the degeneracy of the race is seen in all its deformity—cretinism, idiocy and congenital deaf-mutism.—[Gaz. des Hop., and Boston Med. Jl.

EDITORIAL AND MISCELLANEOUS.

American Contributions to Medical Knowledge.—The multiplicity of medical periodicals in the United States evinces a degree of activity in the medical mind of our countrymen that has no parallel elsewhere; for while we have between thirty-five and forty regular medical journals, besides the periodical issue of the transactions of a large number of Societies, to say nothing of the publications by the Eclectic, Botanic, Homeopathic, Hydropathic, and other quondam brethren, we believe that the British and French have only about a dozen each, and that the whole of Europe does not furnish as many as we do. And yet it is very questionable whether this state of things will ultimately prove beneficial to the profession at large, or even to its American branch.

Under existing circumstances, no one can become acquainted with the workings of the American medical mind without reading a number of periodicals well calculated to stagger any but an editor, and even editors are not always proof against the accumulated load of their table. It cannot be denied that by increasing the number of journals we correspondingly multiply that of contributors. Many a physician, the result of whose observations might be useful, will feel himself stimulated to write and to impart his experience to others, if a journal be started in his neighborhood, who would otherwise have remained silent. Others will be disposed to imitate his example, and unexpectedly, perhaps, realize the fact that they
also have views that ought to be made known, and that writing is not so
difficult as they thought, nor the exclusive privilege of the favored few.
Men are thus accidentally trained to the art of composition, and often be-
come accomplished writers. In this way journals are eminently useful
within the sphere of their influence, and we would be the last to approve
of any diminution of their number. We would, on the contrary, like to
see as many published as the profession can or will support. But we must
repeat that, in the present state of things, much of most valuable matter
intended to reach the general eye through such channels is never heard of
beyond the more or less restricted limits of their own circulation.

It cannot be expected that any practitioner in the United States will
subscribe to all the American medical periodicals, besides those from
abroad; and if any were found so liberally inclined, it is not presumable
that he would or could give them all even a hasty perusal. With the ex-
ception of the copies we send to Europe in exchange for their publications,
we may say that our periodicals are entirely unknown in foreign countries—
whereas several of their journals are reprinted in this country, and exten-
sively patronized. The writings of Europeans are not only brought direct-
ly to us, but our own journals cull most assiduously from them every para-
graph supposed to possess the least interest. While our local periodicals
are thus actively engaged in heralding the achievements of Europe, those
of domestic origin remain unnoticed. In addition to the extensive circula-
tion among us of European journals and reprints, the publications of Braith-
waite and Ranking give a degree of permanency to their papers which is
denied our own. I say, denied our own, because it is quite notorious that
these "retrospects" and "abstracts" are devoted to the propagation of
European views, and almost entirely silent with regard to what is said or
done in America. Take up these semi-annuals, number after number, and
you will look in vain for any evidence of the mental activity to which we
have referred. Can it be that we do or say nothing worthy of permanent
record? Foreigners are perhaps not so much to blame in this matter,
when some of our leading periodicals make an equally meagre showing for
us under their heads of "Domestic Summary," "American Intelligence,"
&c. There is indeed no journal issued in our country which contains even
the slightest notice of one-fourth of the valuable contributions to medical
knowledge by American writers. The petty jealousies of rival schools and
cities, and the more significant evil of sectionalism, tend materially to fetter
periodicals sustained by antagonistic interests, and consequently to restrict
their sphere of usefulness. We might mention some important discoveries
in diagnosis, and in the treatment of particular diseases, which have never
been alluded to in rival cities and sections of the Union, while some of the
verriest puerilities of foreign prints will be found going the round in every
journal of the land.
Now, we need a remedy for the evils pointed out, and the object of these remarks is, to urge upon the profession the adoption of one that may be effectual, without interfering with the interests already involved in local journalism. Let a work be published by subscription, semi-annually, bearing the title at the head of this article, or any other of similar import. Let it consist of three parts: the first, to be made up exclusively of such papers contained in the original departments of American Medical Journals, as may be deemed by competent supervisors, worthy of permanent record; the second, to be devoted to the review of American books on medicine and its collateral branches; and the third part, to contain abstracts of the contents of our medical journals, general medical intelligence, &c. Let the conductors of such a journal be men of industrious habits as well as of competent abilities, whose sole object will be to furnish us a complete and impartial reflex of the workings of the American medical mind during the six preceding months. Such a work, if well conducted, would enable us to become acquainted with the views of men of ability in every section of the country, and would at once become a standard book of reference in Europe, as well as in America. Writers might then enjoy the satisfaction of contributing to the literature of their own immediate neighborhood, and still feel that by so doing, their labor was not necessarily restricted to the limits of their local periodical. With the consciousness that merit would entitle their papers to a place in the proposed national work, they would be actuated by an incentive which they do not now experience, and would strive to do themselves credit abroad, as well as at home. Such a work would not conflict with the interest of existing periodicals, because it would publish no paper that had not already appeared in a local journal, with the exception of reviews. Physicians would naturally continue to support the journal of their own neighborhood, and take also the national work for more extended knowledge, as they do now the reprints of European retrospects.

We have no doubt that such a work, as we propose, would meet with the approbation and patronage of the American Medical Association, if the subject were brought before that enlightened body at their next meeting. We appeal to our editorial brethren, throughout the length and breadth of our great confederacy, to aid us in bringing about so desirable an undertaking. Surely there are many men in our large cities who have the requisite qualifications for such a task; and who, living where the facilities for printing and general dissemination are abundant, might find it advantageous to embark in the enterprise.

Escape of Great Vessels by their Elasticity, from Balls. By G. H. B. Macleod.—There is no circumstance in gunshot wounds which is more striking than the wonderful way in which the great vessels, by their elasti-
city, escape from the ball in its transit. Thus bullets innocuously traversed parts where one would suppose a pin’s head could not be placed, without wounding a vessel. True, the fact that such cases remain to be seen, results from the vessel not having been opened, and we do not know in how many cases the result was not so fortunate, but still, viewed merely as happy escapes, they are curious and interesting. In the course of the femoral vessels, this phenomenon is particularly common. Through the axilla, through the neck, out and in behind the angles of the jaw, between the bones of the forearm and leg, balls of every size often take their passage without harm to the vessels. Take the following cases as examples: A soldier was wounded at Inkerman, by a ball which entered through the right cheek and escaped behind the angle of the left jaw, so tearing the parts that the great vessels were plainly visible in the wound. Three weeks after he was discharged without having had a bad symptom. A soldier of the Buffs was struck in June last, when in the trenches, by a rifle ball, in the nape of the neck. It passed forwards round the right side of the neck, up under the angle of the inferior maxilla, fractured the superior maxillary and malar bones, destroyed the eye, and, escaping, killed another man who was sitting beside him. This man made a recovery without a bad symptom.

A French soldier at the Alma was struck obliquely by a rifle ball, near, but external to the right nipple; the ball passed seemingly right through the vessels and nerves in the axilla, and escaped behind. His cure was rapid and uninterrupted. Endless numbers of similar cases may be seen in any military hospital.—[Edinbourgh Med. Jour.

Treatment of Erysipelas.—M. Velpeau gives the results of his treatment of 1000 cases of Erysipelas. He places the greatest reliance in iron. He employs the proto-sulphate of iron in solution, about twelve grains to the ounce of water—or as an ointment, eight parts to thirty of lard. In forty cases in which this was exclusively used, the erysipelas yielded in from twenty-four to forty-eight hours. The ointment is more easily applied to some parts than the lotion, but is somewhat less efficacious. It should be used about three times a day. The lotion should be applied by soft compresses or cloths kept constantly moist.—[Bull. de Therap.—Boston Jour.

Removal of Milk in the Breast.—Mr. Gibbon states in the Lancet, that the application of belladonna to the mamma is an excellent means of checking the secretion of milk. With a lotion consisting of half a drachm of the extract of belladonna to half a pint of water, he has succeeded in arresting the secretion in three protracted cases, where a variety of expedients had failed.—[Boston Med. Journal.

Formula for the Internal Use of Chloroform.—M. Dannecy, pharmacien, of Bonleaux, recommends the following formula: Pure chloroform, half a drachm; oil of sweet almonds, two drachms; gum arabic, one drachm; syrup of orange flowers, one ounce; distilled water, two ounces; mix the chloroform with the oil, and make an ordinary oily draught. The author also gives a very ready mode of testing the purity of chloroform. Mix the latter with some oil; if the chloroform be quite pure, the limpidity of the oil will not be destroyed; whereas, any chemical impurity, however small, will give rise to a cloud.—[Lancet.
Rapid Detection of Sugar.—M. Botte has several times employed the method suggested by Liebig of quickly detecting sugar. A small quantity of ox-gall is dissolved in the suspected fluid in a test-glass, and a quantity of concentrated sulphuric acid equal in amount to that of the fluid in the glass is rapidly added, care being taken to pour it along the side of the glass. If sugar is present a beautiful purpurine is immediately produced. [Rev. Médicale—Med. Times and Gazette.

Vaccination in Relation to Blindness.—Statistical researches show us that, prior to Jenner's discovery, of 100 cases of blindness, 55 were due to small-pox; and Dr. Dunnot, physician to the Hospice for the Blind, has recently supplied an interesting account of the progressive decrease of that proportion. Among the blind of sixty years of age, he finds this variety of cause in 12 per cent.; in adults, it only exists as 8 per cent.; and, in children, only as 5 per cent. We may take as a mean, counting all ages, about 7 per cent., which, as at the commencement of the present century, the proportion was 35 per cent., exhibits a diminution of 28 per cent. [Med. Times and Gazette.

Solution of Gutta Percha.—Mr. Maisch, of New York, advises the following method: One part of the best commercial gutta percha is cut into small pieces, and by agitation is dissolved in twelve parts of chloroform. On standing for a day, all the coloring matter rises like a scum to the surface, leaving the solution clear. This may then be easily drawn off to the last drop. A wide glass tube, narrower at the bottom, and so arranged that both ends may be closed with corks, is the only instrument necessary. After the separation is complete, the upper cork must be removed, and the lower one loosened, so as to allow the fluid to run out slowly. The advantages of this solution over collodion consist in the absence of contractile power and gloss, and in its elasticity and greater resemblance to the skin in appearance.—[Memphis Med. Recorder.

Pectoral Syrup.—Take of ipecacuanha, one ounce; seneka, three ounces; refined sugar, two pounds; sulphate of muriate of morphia, sixteen grains; oil of sassafras, ten minims; make two pints of syrup. Macerate the ipecacuanha in coarse powder, for fourteen days, in one pint of diluted alcohol, express, filter, and evaporate to six fluid ounces. Next digest the seneka in coarse powder, with ten fluid ounces of water, and two fluid ounces of alcohol, at a heat not exceeding 104° F., for six hours, strain, express and filter, adding, if necessary, enough water to make ten fluid ounces. Mix with this the tincture first obtained, and dissolve in it the sugar, at a gentle heat; strain, and while yet warm, add the morphine and oil of sassafras, dissolved in a very little warm alcohol. The dose is from one to two teaspoonfuls. This is Jackson's formula as prepared by Stevens. (See Amer. Journal of Pharmacy, May, 1856.) The prescription is a favorite one with many Northern physicians, as an anodyne expectorant; and is, doubtless, a better remedy than any of the numerous compounds sold in the shops, all which have opium for their basis. Great mischief is undoubtedly done in pulmonary diseases, by the injudicious and extra-professional use of all these anodyne cough mixtures. It is far from being true that every case of cough requires or admits the use of anodyne remedies.—[Ibid.