Veratrum Viride, or American Hellebore. By W. C. Norwood, M. D., of Cokesbury, South Carolina.

We trust that the profession will pardon our appearing so frequently under the above caption. The great incredulity of many in reference to the powers of Veratrum Viride, and the apprehension by others of its poisonous properties, as well as the imperfection of our previous communications upon the subject, must plead our apology for now giving a more minute account of our experience, and urging the great importance of the subject, perhaps, for the last time. We intend to state facts and events that have repeatedly come under our immediate observation. We shall not pretend to account for the modus operandi nor the wonderful and magical effects of veratrum viride upon the system, in overcoming and subduing disease, nor shall we attempt to specify what or which are its primary or secondary, its direct or indirect effects. But we will add the opinions of a number of medical gentlemen, with the effects witnessed by them from time to time. By this course the medical world will be put in a condition to judge how far our observations and experience have been sustained by those of others who are not so personally interested as we are. Veratrum viride, as a therapeutical agent, had excited comparatively little interest previous to June, 1850; and it was noticed for a
time after that date, more on account of the extravagance of the claims set up for it as a remedial agent of superior powers, than because of any belief that it was possessed of peculiar and valuable properties. We could, if necessary, in this place, name several medical journals which noticed the article or articles written by us and spoke of the extravagance of our statements. If we recollect correctly, it was about the year 1835 that Dr. Charles Osgood's interesting article on the powers and properties of veratrum viride made its appearance. The only additional information he conveyed was that it is destitute of cathartic powers, which give it a superiority over the Veratrum Album or European Hellebore, in the treatment of cases where active cathartics are inadmissible. Be this as it may, it is certain, and cannot be successfully controverted, that prior to June, 1850, it was not known positively to possess any superiority over veratrum album; indeed the one was supposed to answer the same purposes as the other.

Without wasting time in the proof, or in noticing the remarks that others have made up to the period stated, we will simply observe, that from all we find in works upon materia medica or therapeutics, and in the standard works on the practice of medicine, there are no claims set up for its superiority to its "European congener." It stands unnoticed, is not even named as a remedy, and still less as a leading and valuable remedy, by any of the standard authors of the day in their treatises on the practice of medicine. Dr. Wood, of Philadelphia, in his very able, extensive and superior practical work on Medicine, even as late as 1849, does not mention or recommend it in the treatment of a single disease. It does not form a portion of the work upon New Remedies, by Dr. Dunglison, a man of untiring industry and application, and who never slumbers until all that is known is placed in his valuable works, nor has it a place in his work on the Practice of Medicine.

Now, if the peculiar powers and properties we claim for the veratrum viride had been known and established—if its peculiar adaptation to the successful treatment of typhoid pneumonia, typhoid fever, scarlet fever, puerperal fever, &c., &c., had been known, these very learned and deservedly distinguished authors would not have failed to notice it. We have entered
into the above brief notice to show how little was known of the value of veratrum viride prior to the notice given of it in June, 1850. When we furnish the testimony in our possession, it will be seen what a valuable remedy it is, and what a wide spread reputation it has acquired in the short time it has been before the public. Why Dr. Osgood ceased to give further notice of its powers we are not prepared to say: whether his silence grew out of a want of confidence in its remedial powers, or from death, we are wholly ignorant. We do not wonder at the violent and drastic effects he witnessed; but we rather wonder, from the large doses given, that he obtained any beneficial effects. Be this as it may, if it possesses the powers and properties we attributed to it, and is adapted to the treatment of the symptoms and diseases indicated by us, the discovery must be eminently valuable. Greatly enlarged experience and observation have strongly confirmed us in the belief of the correctness of what we stated on a former occasion, namely, that when its powers and properties are fully known and understood, it will constitute a new era in the treatment of disease.

In July, 1844, we first used it in the case of Mrs L. She had been laboring under a severe attack of pneumonia typhoides for several days. Calomel, blisters, Dover's powders, &c., failed to afford relief. We had been informed by a friend and an eminent physician, that veratrum viride "was analogous in its powers and properties to blood-root—more narcotic and less emetic, and that a fluid drachm, or a tea-spoonful of the tincture was a dose." We had often used blood-root with marked success in pneumonia, and especially in the cases of a bilious character. This case having annoyed us by its severity and obstinacy, and opium producing unpleasant effects, without relief to the pain, we determined to make a trial of the tincture of veratrum viride. We withdrew all other remedies, and put her on tea-spoonful doses of the tincture, to be repeated every three hours. Instead of making it by using (as was directed) "one pint of undiluted alcohol, of the strength of the shops, to eight ounces of the green root, or diluted alcohol, if the root was dry," we added one pint of undiluted alcohol to eight ounces of the dried root. This is the formula we gave to the public, and is the one we now use. It has been said by some,
that it was an extravagant formula, that we used an unnecessary quantity of the root. We always like to have our tinctures, of very active agents, of the same or uniform strength, and would much prefer the waste of a little root to the uncertainty of an untried tincture. But, if any one will read Dr. Robert's valuable article on Veratrum Viride, in this Journal, for June, 1852, it will be seen that he added double our proportion of alcohol, and that he had to use double the quantity of the tincture; thus demonstrating that we were as correct in estimating the quantity necessary to saturate the alcohol, as we were in our representations of its remedial effects on the system. We would not pretend to deny that less might do, if the root were dug at the right season, (between the 1st of September and frost,) and properly assorted and put up. But in the form in which it comes to us, it is rare that less can be sufficient.

But, to return to our case: We gave her a tea-spoonful at 11 A. M. About 1 P. M. we were sent for in haste, as the medicine, or something else, was acting drastically. We found the patient vomiting every few minutes; skin cold and covered with perspiration; great paleness, nausea distressing; complained of a sense of sinking and exhaustion. After the vomiting had ceased, the pulse was found not more than 60 per minute, full and distinct.

In a few cases, in which nausea was great and the vomiting frequent, we have found the pulse very slow, small, and almost imperceptible at the wrist; but as soon as the vomiting and consequent exhaustion subside, the pulse will be found slow, full and distinct. The nausea or vomiting, when in excess, can be readily and certainly relieved by one or two full portions of syrup of morphine and tincture of ginger, or laudanum and brandy.

In this case, before administering the tincture of veratrum viride, the skin was hot and dry; pulse 130, small and soft; circumscribed flush on the cheeks; pain severe; breathing hurried and difficult; cough frequent; expectoration scanty. The very striking effects of the medicine, the great reduction in the frequency of the pulse, and the sudden breaking up or arrest of the disease, in this and another case, profoundly enlisted our
attention, and led us from that period to observe more particularly its powers.

The second case in which we used the veratrum viride was that of Mrs. M., who was also laboring under a severe attack of pneumonia. Pulse from 130 to 140 beats per minute; pain violent, and extending from the right side, near the spine, to, and under the sternum; tongue red on the edges and tip, and covered in the centre with a thin, dark, dry fur; bright scarlet circumscribed flush appearing first on one cheek, and then on the other, rarely on both at the same time; the end or tip of the nose and chin frequently red; very pale around the mouth; expectoration scanty; mucus streaked with blood; cough frequent and very harrassing; great increase of pain under the sternum during a paroxysm of coughing; decubitus on the back; breathing labored and difficult. Did not see her till the fourth day: she had been bled, and otherwise treated, with little or no relief. Applied a blister, and gave a camphorated powder to allay the cough and violent pain, and to excite diaphoresis. At the expiration of three hours, to commence with the tincture of veratrum viride.

The first portion excited intense nausea, violent emesis, great paleness, coolness and a sense of sinking or of exhaustion. The patient and friends becoming alarmed, another physician who lived much nearer than myself, was sent for in great haste, but when he arrived the nausea and emesis had ceased; the patient was comfortable, pain and febrile symptoms subdued, pulse sixty-five, full and distinct. The doctor was surprised to find the condition of the patient so different from the representation given by the messenger. The disease was really broken up and a crisis and resolution brought about. Our friend, the doctor, ordered a little paregoric and quinine, in which we fully concurred on our arrival, as there was entire relief of all active febrile and inflammatory symptoms.

We would here observe that notwithstanding the complete reduction of the inflammatory symptoms and pulse, the peculiar circumscribed redness on the cheek or cheeks continued for three days. In another case of pneumonia, we observed the same peculiarity, with the additional fact, that although there had been more tendency to coma or uninterrupted and deep sleep.
the patient then became preternaturally wakeful, somewhat flighty and disposed to get out of bed. This continued about forty-eight hours. We do not know how to account for this continuance of the red cheeks under such circumstances. A case occurred in Georgia, in which a corpse was kept a week in consequence of the persistence of this redness.

Deeply impressed with the peculiar effects of verat. viride, we determined to make farther and conscientious trial of it in pneumonitis. The third case in which we administered it was that of Mr. T., who was taken sick when on a visit to his friend in this section of the country. We ordered the tincture given every three hours, beginning with eight drops, to be increased one drop at each dose until nausea, vomiting or some other visible effect was produced. On the dose reaching twelve drops it induced vomiting with but little nausea. The pulse was reduced from 135 to 78 beats per minute; the surface, from being hot and dry, became cool; and the severe pain was now but slightly felt on taking a deep inspiration. The interval between the doses was extended from three to five hours; but as twelve drops induced too frequent vomiting, the quantity was reduced to seven drops and continued three days without any return of the symptoms, when the case was dismissed and the patient was soon able to return home. This case was one full of interest on account of the success and promptness with which the violent symptoms were removed and the disease cured.

We might report any number of cases, but as many of them have already been given by others, we will confine ourselves to such facts only as may tend to illustrate particular points. We continued our experimental trials with various doses from three to twelve drops, increasing or diminishing them according to circumstances, until we acquired a perfect knowledge of its effects, and could graduate them at will.

We ascertained that in cases which had run on for sometime or in which emetics and cathartics had been freely used, a very small quantity was necessary. Where tartar emetic has been given, it is almost sure to act harshly and drastically. Where tartar emetic had been taken, we would therefore always give a full portion of syrup of morphine, at least one hour before
entering on the use of the veratrum viride, and in such cases would not commence with more than six drops for a male adult. Free venesection increases very materially its activity, especially its unfavorable or drastic effects. No one should think of following a large bleeding with the veratrum viride, unless with the greatest caution. The depressing influence of the loss of blood upon the brain and nervous system generally, cannot fail to render the use of so potent a sedative as veratrum viride exceedingly hazardous. The administration under such circumstances of an agent capable of reducing the pulse from 130 or 140 down to 75, 70, or even 50 beats in the course of a few hours, cannot be too carefully watched.

Intending to resume and to conclude in the next number of this Journal the account of our experience in the use of veratum viride, we beg leave now to append the testimony of some of those who have kindly written to us upon the subject.

Roswell, Ga., May 1st, 1852.

Dear Sir—I find, in experimenting with your veratrum viride, it is all in all things you have represented it, and is certainly the only arterial sedative on which we may at all times rely with certainty, and the most invaluable agent of this class in the whole materia medica.

Very truly,
WM. NEPHEW KING, M. D.

Columbia, Nov. 17th, 1851.

Dear Sir—In experimenting with the tincture handed me, (veratrum viride,) I have been very much pleased with its controlling powers over the heart and arteries. I have only given it in typhus fever, and one or two cases of pneumonia. It certainly reduces the pulse without any of those immediately prostrating and alarming symptoms which take place after the continued use of digitalis; neither does it irritate the mucous membrane of the bowels, as the salts of antimony do, when continued for days. I have given it in several cases of typhus, in which there was dry red tongue, great thirst, delirium, frequent dejections from the bowels, with soreness and distension of the abdomen, without the least aggravation of any of those disagreeable symptoms. I have not found it immediately to arrest the disease, or cut it short at once; after fully formed, but certainly to make it assume so mild a form as to require very little in the future treatment. I have, in several cases, broken up the forming stage of the disease, by keeping the heart below a natural and normal action for two or three days. In fact, I regard your tincture of every importance in the above diseases, and fully meeting the expectations of its warmest advocates. It certainly is the very article to fill the place
(a thousand times better and safer) of the tart. emetic in the contra-
stimulant treatment of the "Italian school." * * *

I remain yours, most truly,

SAMUEL FAIR, M. D.

Newberry Court House, Nov. 16, 1851.

Dear Doctor—I have given the medicine you sent me, (veratrum 
viride,) to two patients labouring under typhoid fever, with the best 
effect. In both cases the pulse was reduced from 120 and 140 to 70 
and 74 beats in the minute, by giving from three to four doses, there 
was no return of fever afterwards. The medicine was continued five 
days in one case and seven or eight in the other. I was sent for 
two weeks ago to visit a patient in consultation with an eminent 
physician, labouring under pneumonitis. I saw her on the ninth day 
from her attack—her physician had used all the remedies usual in such 
cases—she seemed to grow worse. When I was called in, he said he 
had no hope of her recovery—all the symptoms were unfavourable. I 
proposed giving Dr. Norwood's medicine, as I called it; he smiled, and 
said he was afraid it was a humbug, but consented, as he considered 
the case hopeless. We gave her (a young lady fifteen or sixteen 
years old) five drops; increased one drop each dose until we gave eight 
drops to the dose. It produced nausea of the stomach by this time; 
her pulse was reduced from 120 to 88 beats in the minute. Her 
physician remained with her during the night; he stopped giving the 
medicine. The next morning I saw her again and found her with a 
pulse of 110 beats in the minute. I asked the doctor if he had discon-
tinued the medicine; he said he had. We commenced giving it again, 
in eight drop doses; by the third dose her pulse was reduced to 74 
beats in the minute; said she felt much better. The doctor discontinued 
the medicine again for eight or ten hours to see the effect. The pulse 
rose again to 108 or 110 beats in the minute. We resumed the 
medicine again—about the third dose the pulse was reduced to 70. 
We kept it from 70 to 74 beats for several days, some six or seven. 
She is now convalescent. I will say to you, however, that the doctor 
has sent to me a second time for a small vial of the medicine, as he is 
giving it to some two or three cases of typhoid fever, and says he is 
very much pleased with its effects.

Your friend,

J. B. RUFF, M. D.

Bainbridge, Ga., June 5th, 1851.

Dear Sir—Since receiving the veratrum viride, I regret that I have 
had but one favorable opportunity of giving it a trial; in that, however, 
it succeeded beyond my most sanguine expectations. The case was 
one of Pneumonia, complicated very decidedly with typhoid symptoms. 
The patient being four years old and the pulse 130, I proceeded after 
trying all other modes of treatment unavailing for ten days to give 
the tincture in common doses. The first was ejected as soon as swal-
lowed, but was repeated instanter and was retained. The little patient 
now becoming tranquil and not anticipating any very sudden change,
I suffered myself to engage in common fireside conversation for some thirty minutes, when my attention was attracted to my patient by the extreme palor of his countenance, and upon examination found his pulse reduced to about 80, the skin bathed in perspiration, and, as far as one could judge, the disease gone, and the patient sleeping sweetly. But in order to assure myself that these results were produced by the medicine and nothing else, I withheld the second dose and the result was that the fever rose in five hours. The dose was then repeated and the same results followed as in the first instance. The portion was again withheld, whereupon the fever rose again in eight hours. But a repetition of the remedy subdued it as promptly as before, and by continuing it at intervals of six hours, there was no return of the symptoms: thus conclusively showing that the favourable results obtained could not be ascribed to the agency of any other article.

Yours, very respectfully,

E. R. RIDLEY, M. D.

Waynesboro', Burke Co., Ga., August 4th, 1852.

Dear Sir—I had intended, as a matter of great gratitude, at an early day to write you an acknowledgment of your prompt kindness in sending me a specimen of your tincture of American Hellebore, as well as to congratulate you upon your discovery of the controlling powers of that article over abnormal organic reaction. * * * I am satisfied with the display of its magical powers, as presented for my consideration. I am satisfied that a great desideratum has been accomplished. I am proud of it as an achievement of American medicine—I am proud of it, particularly, as a triumph of Southern experiment and observation, and believe that it will weave for the brow of the discoverer a chaplet of green, and with the lancet, win a partition of empire in the domain of practical medicine. * * * I will further, and more familiarly, say, that price shall be no bar to my keeping a supply in my office. I will never be without it, if money can get it. Deprive me of it, and I verily believe I should "throw physic to the dogs." I still have a small portion of the specimen you sent me. I intend to keep it until I am satisfied I can obtain a supply of equal purity and power. * * * Dr. Montgomery requests me to say that he is every way satisfied with the article—that it has furnished him with a number of beautiful cases and subject matter for a communication for the Journal; but he must plead laziness in extenuation of the omission. To use his emphatic language: "Take it from me, sir, and I'd quit the practice of physic." Before you dispose of what you have on hand, root or tincture, I must get some. I must be sure it comes from your hand—I don't care what the price is.

I remain,

E. L'ROY ANTONY, M. D.

[To be Continued.]
Hammond, on Calculi in the Urethra. [November,

Root of Veratum Viride, in order to supply the many calls upon him by physicians who were unable to obtain it. He still has some on hand which he would like to dispose of, so as to be refunded, in part, at least. The Doctor certainly deserves the thanks of the profession for his exertions to supply the article, until it is generally kept by the druggists. He will keep it no longer.—Editor.

—Editor.

ARTICLE XLI.

Professor Pancoast, in treating of strictures of the urethra, has most truly observed that, "no class of surgical diseases demands more attentive study on the part of the practitioner than that which involves, as one of its consequences, a retention of urine." My remarks, on this occasion, will be entirely confined to retention from impacted calculi in the canal of the urethra, and requiring the knife for their removal. It may be proper here to observe that, in each of the following cases, the ordinary means were used for the removal of the calculi, prior to resorting to the knife.

The operation is usually performed in the following manner: The median line is opened directly over the urethra, which is deepened until the foreign body is reached and extracted; a catheter is then introduced into the bladder, and the external wound closed with stitch and adhesive plaster—this is the plan now recommended by Dr. Mütter. There are many objections to this operation, which will be pointed out in the sequel.

In my first case, I performed the operation as recommended by this distinguished surgeon, and in consequence of a fistulous opening remaining in the urethra for some considerable time, and which required much trouble and pain for its eradication, I determined, should ever another similar case occur in my practice, that I would perform the operation in a different manner.

Case I. In the fall of 1829, my associate in the practice of medicine, Dr. Richard Banks, the distinguished surgeon now
of Gainesville, was summoned in haste to see the son of Mr. Wm. Sodlers, of Elbert county, about 5 years of age. He being absent, I was requested to visit the case. The messenger having apprised me of the condition of the patient, I fortified myself with the necessary instruments. I found the little boy in the most intense agony from retention of urine; the bladder was very much distended, reaching nearly or quite to the umbilicus. On making an exploration of the urethra, I detected a calculus lodged in the canal, just anterior to the bulb. Making some abortive efforts to remove it, I cut down upon it, and readily extracted it through the opening; the urine gushed from the wound with considerable force, to the great relief of the little sufferer. A catheter was introduced, and the wound approximated, and confined by stitch and adhesive plaster. From the constant stillicidium of urine through the opening, the plasters were soon washed away, and the pain and irritation produced from the presence of the catheter becoming so insufferable that I was compelled to withdraw it. Considerable tumefaction of the penis and scrotum ensued; this, however, subsided in a few days by appropriate treatment. The patient recovered from the operation, with the exception of a fistulous opening, as before stated; this was, in the course of five or six weeks, closed by frequent applications of the nitrate of silver.

Case II. In the year 1831, (as well as I now recollect, for I kept no notes of the case,) my co-partner and myself were requested to visit the son of Mr. Wm. Alexander, a child about three years of age, near Ruckersville, Elbert county. He had retention of urine, from the lodgment of a stone in the membranous portion of the urethra. The operation was performed in the following manner:—The patient was secured as for the operation of lithotomy; an assistant, placed on the right of the patient, with the fingers of the right hand forcibly drew the skin across the perineum from left to right, and held it firmly against the ramus of the ischium—by this procedure the raphe or median line of the perineum was from a half to three-fourths of an inch from its natural position, and to the right of the urethra; an incision was now made in an oblique direction down to the stone, which was readily removed by a small pair
of forceps. The assistant removing his hold upon the skin, it at once assumed its normal state. The orifice through the skin was now found from a half to three-fourths of an inch from the median line, and on the left side of the urethra. The wound healed by the first intention.

Case III. On the 15th of August last, I was requested to see the child of Mr. Orlando Holland, about one mile from Culloden. The little patient had been laboring under a difficulty of micturition for several days, for which he had taken diuretics and mucilaginous drinks, without relief. On the introduction of a silver probe, a small calculus was found lodged just behind the glans penis, about midway between the glans and front of the scrotum. Making some effort to force it out of the urethra, and failing to accomplish it, I proceeded to remove it by the knife. The penis was grasped by the left hand, twisting the skin from the left to the right side, and holding it firmly—a scalpel held in the third position completed the operation. The penis being released, the skin at once retracted, throwing the orifice made by the scalpel on the left side of the penis, and one-half inch from the median line beneath, presenting the appearance of the operation having been performed through the corpus-cavernosum instead of the corpus-spongiosum urethrae. In this case, union took place by the first intention; not a drop of urine ever passed through the wound.

Remarks.—In the first case, we had a troublesome fistulous opening to contend with for several successive weeks, caused, no doubt, by the manner in which the operation was performed. When the opening is made through the integuments, directly over the urethra, the wound has a natural tendency to gape, from the traction constantly kept up by a contraction of the skin in a lateral direction—superadded to this, a portion of the corpus-spongiosum is almost certain to protrude through the lips of the wound; the presence of the catheter in the urethra causes great pain and irritation, and at the same time distends the calibre of the canal beyond its natural size, which keeps open the rent in the urethra; and, in addition, the stitch recommended to close the wound, develops, in a majority of cases, still more irritation, and not unfrequently ulceration is the con-
sequence. Many other objections might be urged, but I think it would be an unnecessary consumption of time to do so at present.

In the plan I propose, as detailed in the second and third cases, neither of the foregoing objections obtains. The opening through the skin is some distance from that in the corpus-spongiosum urethre, consequently the external integuments immediately over the wound are entire. After the operation is performed, all that is necessary to complete the cure is proper pressure. The slight tumefaction which always succeeds the operation, produces a rigidity of the integuments—this thickening and immobility of the parts institutes a steady and constant pressure, and this, too, being so accurately applied, and steadily maintained, that there cannot be any impediment to union by the first intention; whereas, in the old plan of performing the operation this seldom ever takes place, for reasons already stated.

ARTICLE XLII.

Cases of Typhoid Fever. By J. A. Williams, M. D., of Pike county, Ga.

Case I. Called 7th June, 1851, to see Mr. P., aged 25 years, who had been complaining some ten days, the last three or four of which he was confined to his bed with a dull headache, indisposition to exercise, indolent stupid feeling, no appetite, and a slight diarrhoea. I found him somewhat emaciated, skin hot and dry, pulse from 100 to 110, some deafness, tip and edges of tongue red, centre covered with a white fur, bowels tender to pressure, with the usual gurgling noise and other less prominent symptoms, characterizing a simple case of typhoid fever.

Treatment.—A blister to the nape of the neck, a pepper poultice to the bowels, gum or elm water to be drank constantly; no other food allowed; laudanum when necessary, to control the bowels.

June 11th. Patient in the same condition—the symptoms a little aggravated: ordered treatment continued, laudanum increased.

June 20th. Patient doing well, no pain, pulse 90, skin moist,
slight diarrhoea: ordered small portions soup or gruel. Patient dismissed with instructions to keep within doors, guard the appetite and continue the laudanum if necessary.

Case II. Mrs. W., aged about 40, the mother of eight children, a woman who leads quite an active life, of stout and robust habit, somewhat inclined to corpulency, was attacked about the 1st of July, 1851, with a dull headache, a disinclination to exercise, no appetite, unpleasant taste, in which situation she remained until the 4th, when I saw her; emaciated, skin hot and dry, tip and edges of tongue red and parched, a brownish fur on the centre with distinct papillae appearing through the coating, pulse ranging from 120 to 140 rather full, bowels tender, a little distended, slight diarrhoea. In connection with the above, Mrs. W., had a slight cough, and upon exploring the chest detected some irritation in the right lung.

Treatment.—A small blister over the occipito-atloid articulation to combat tenderness, a pepper poultice to the bowels, gum or elm water drunk constantly, squills occasionally, a Dover's powder at night, laudanum if necessary.

July 10th. Little or no alteration in patient; treatment continued, squills increased.

July 17th. Sent for in haste to see patient. No abatement of fever, bowels distended and very irritable, tongue red: applied a blister plaster to the abdomen, former treatment continued, with instructions, should strangury ensue.

July 20th. Blister acted like a charm, swelling in the bowels subsided, some abatement of fever; treatment continued. Ordered a little rice water, in a day or two with instructions to increase the diet as the stomach could bear.

July 25th. Patient convalescent; skin moist and cool, pulse nearly natural, tongue cleaning off, sharp appetite. Dismissed.

Case III. January 6th, 1852. Called to see Archer, a negro boy, the property of Mr. R., who had been sick about a week. Mr. R., had given him salts once or twice, and a dose of Cook's pills; all ineffectual. Archer presented the usual symptoms of typhoid fever with a slight attack of pneumonia.

About this time, Dr. Norwood's preparation of hellebore made its appearance; thinking I had a proper subject for its use commenced by giving five drops every three hours until nausea
was induced, which occurred in ten hours, with vomiting, at which time I left, ordering the veratrum viride again in five hours, to be repeated as before. This practice was continued until the 10th, with the effect of reducing the force and frequency of the pulse during its administration, but upon its discontinuance that symptom would resume its former station. Being satisfied of the inefficacy of veratrum viride in this case, my patient was put upon the palliative plan and recovered in a few weeks, the fever having run its course uninterrupted by any therapeutic agent.

Cases IV and V. April, 1852. Called to the family of Judge E.; found his son, a youth of 15 or 16 years old, with headache, hot and dry skin, tongue red and pointed, bowels regular, with other characteristic symptoms of typhoid fever.

Treatment.—Elm water, a Dover’s powder at night, laudanum if necessary. While attending the above, my attention was directed to a negro woman in the same family, who presented the same symptoms as above. Treatment the same; both were relieved in four or five weeks.

Remarks.—The object of reporting the above cases is to induce some one more competent than myself to experiment, note and publish the result of the palliative treatment in typhoid fever. In looking over the Journals of the day, we see that some rely on mercury as the sheet anchor, others on veratrum viride, and a few on stimulation; all of which (according to my experience) are useless, if not detrimental. In general, mercury only serves to debilitate, and in some instances aggravates the already irritated bowels; veratrum viride merely exercises a temporary control over the vascular system, and of course leaves the alimentary canal in an irritated state, and more subject to colliquative diarrhoea; stimulants cause increased nervous and arterial excitement about the brain, inducing nervous derangement, subsultus tendinum delirium, coma, &c. So, upon the whole, our main dependence in typhoid fever are, palliatives, good nursing, and the vis medicatrix naturæ.
ARTICLE XLIII.


Mr. Editor—The following case may, perchance, interest some of your readers, or, at least, it will go to prove our textbooks correct, when they assert that an evacuation may take place at various parts of the body, vicarious of the normal uterine periodical secretion.

On the 8th of February of the present year, I was called to visit Mrs. W——, of Columbia county, who was much alarmed at a hemorrhage from the left eye: she is about 30 years of age, and the mother of six children. Reported to me, that for four or five nights past, she had perceived that her left eye had been bleeding about a table-spoonful during the night, as near as she could judge, from the stain upon the pillow, and that the eye was suffused with blood during the day. During the same period, she had a slight hemorrhage from the nose, and had discovered traces of blood in the expectoration. Feeling no pain in the eye, and having never received any injury from blow or otherwise, she was at a loss how to account for it.

Making particular inquiries into the case, I learned that she had menstruated in the usual manner only three times in twelve years, and that this was the proper time for the menses to appear. Her general health, during this time, has not been good, and her appearance, at the time of my visit, was decidedly anaemic, with some tendency to anasarca.

On careful examination of the eye-lid, I could detect some slight congestion of the minute blood-vessels; otherwise not different from the other. With a view to the improvement of her general health and to the re-establishment of the uterine function, I prescribed mineral tonics, chiefly the preparations of iron, for the interval, and special emmenagogues for the week on which the menses ought to return. Up to the present month she has had no return of the vicarious menstruation.

She followed my prescription for about one month; but the normal discharge not taking place, she became discouraged, and discontinued the medicine. Having understood that, some years since, her menses appeared after a few days bathing in the
sulphur spring at this place, and as there has been no return of the regular menstruation, I have advised her to try the bathing again.

A few weeks since, I was called to treat her for intermittent fever, and was struck with a remark of her's—that the side on which the vicarious discharge appeared "felt numb when the fever was on." Does this throw any light upon the state of the nervous system in "intermittents?" Could the peculiar condition of the uterine system cause any loss of sensation in the nerves of one side more than the other? Here is a point worthy of investigation by older practitioners than myself.

The "intermittent" yielded readily to treatment; and once since the convalescence she says that symptoms have induced her to think, that the vicarious discharge may again return from the eye.

August 24th, 1852.

ARTICLE XLIV.

Case of Extensive Sloughing of the Foot from Frost Bite; Amputation below the Knee. By B. M. Thompson, M. D., of Danielsville, Ga.

It is not uncommon to see cases of frost bite of more or less severity in this climate; but it is rare to see a whole limb destroyed by congelation alone. Such accidents are of much more frequent occurrence in the higher latitudes. The following case is interesting on account of the extent of the injury sustained and the consequences resulting from it—the loss of the entire foot and leg. My object in reporting it for the Journal is merely to record it, to furnish a link for the great chain of medical statistics.

The patient, a Negress, about 25 years of age, a native of Virginia, the property of Mr. J. W. H., of this county, ran away from her master about the 18th January last, and was out during the very cold weather experienced here about that time. She was out some three weeks, and when found near her master's plantation, was unable to walk from the swollen and painful condition of the left foot and leg. The foot, however, was
neither much swollen nor very painful, but a considerable portion of its skin was off, and there was a good deal of redness in the denuded part. This redness, as I was informed by her owner, was soon succeeded by a bluish colour; there was very little sensibility in the part. There was a good deal of soreness about the ankle, which was greatly swollen. There was no sloughing of the soft parts, except of the integuments, as before mentioned, when she was brought home. The foot was dressed with poultices wet occasionally with spirits of turpentine. This treatment had but little effect other than to stimulate the efforts of nature to cast off the lifeless foot. In the course of a few days the soft parts around the ankle joint began to slough, and when I was called to the case on the 29th of February, I found the sphacelated foot almost ready to drop off at the ankle joint. It was black, dry and hard, not unlike dried beef in appearance. The sloughing was extensive, especially on the inner side of the articulation, the tarsal bones were more or less exposed. The foot was only prevented from dropping off by the tendo-achilles, a small portion of the skin and some of the ligaments on the outer side of the joint. The other tendons, the nerves and bloodvessels were completely severed, and the arteries occluded. The leg for some distance above the ankle presented a very unhealthy appearance—it was nearly twice as large as the other, oedematos, of a bluish or livid hue, the integuments appeared to be thickened, and it resembled elephantiasis. The lower ends of the tibia and fibula could not be seen on account of the swollen condition of the tissues which covered them. A considerable quantity of very offensive pus was discharged from about the joint; there was a number of pale and exceedingly unhealthy looking granulations shooting out from the stump, which would slough upon the least handling. There appeared to have been very little constitutional disturbance amid all this local disorder. Her constitution was good, she was greatly emaciated, but was free from symptoms of organic disease of any vital organ.

Amputation of the diseased member seemed the only mode of relief and the circular operation was performed on the 5th day of March, by Dr. Benjamin V. Willingham, of Lexington, who at my request kindly consented to operate, assisted by his
brother and associate, Dr. Willis Willingham, of the same place, Dr. William Johnson and Mr. Howard, medical student, of Oglethorpe county. The limb was taken off about three finger's breadths below the tubercle of the tibia, the patient being under the influence of chloroform. It was performed in the usual manner, and with much skill and dexterity by my friend, Dr. Willingham. I had some apprehensions as to the final result of the operation, as the patient was at times stubborn and refractory, but she convalesced rapidly and satisfactorily and is now (June 1st) entirely well, with a good looking stump. It may not be amiss here to state that this patient had her right foot frost bitten in the early part of last winter, (being runaway at the time,) which was followed by considerable sloughing of the skin and subcutaneous tissue of various parts of the foot; these ulcers healed slowly and had not entirely cured up when she went off in January: they are now, however, well—there is slight contraction of the flexor tendons of that foot together with a little stiffness of the ankle joint.

The chloroform had the happiest effect in this case; about three drachms of this article poured on a piece of sponge and enveloped in a handkerchief folded into the shape of a hollow cone, were applied to the patient's nose, which produced complete suspension of sensation in less than two minutes. She remained under the influence of this wonderful "pain killer" in all about twenty minutes, perfectly unconscious of what was going on during this time, and awoke after the operation was over as from a pleasant slumber. It produced no unpleasant symptoms in this case, the pulse was as full, soft and regular, and the breathing as easy and natural as that of a person in the most perfect health.

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ARTICLE XLV.

A Simple Instrument for Operating upon Hare-lip. Described in a letter to the Editor, by E. F. Starr, M. D., of Rome, Georgia.

Dear Sir—Enclosed I send you a small plate made of pewter, with eyes, or loop-holes, on the back, which, if you will adjust upon one prong of your long straight dressing forceps,
will make (should it fit well) a convenient instrument for operating for hare-lip. To have the lip firmly clasped the entire distance it is to be cut—to have it well supported by a substance which will not destroy the edge of the knife when cut upon, and to have a guide for the knife in making the cut—are objects to be desired. These may all be obtained by this instrument, thus arranged, in one cut. It might be well, perhaps, to have a thin piece of leather pasted upon the face of the plate. The opposite blade of the forceps should be rough like a file, longitudinally, to prevent slipping. An instrument might be made for the purpose with plate attached, and the edges or sides of the opposite prong should be made straight, as guides for the knife. In using it, the operator should take hold of just so much of the lip as he wishes to remove, then cut along the edge of the outer prong, next the body of the lip, down to the plate on each side. You will readily perceive its application, however. The plate may be made wider or narrower, to suit individual cases. I used this one not long since in an operation, and found it quite a help.

ARTICLE XLVI.

Observations upon the use of certain New Remedies. By L. A. Dugas, M. D., Professor, &c.

Believing it to be the duty of members of associations of this kind to make known such results of their observation as may be useful, I will beg leave to offer a few remarks upon the use of some of the remedial agents recently brought into notice.

Chloride of Sodium or common salt has been, at various periods, proposed as a valuable remedy, but has attracted more than usual attention during the last twelve months.

I have long been in the habit of prescribing it alone, or in combination with the Bi-carbonate of soda, in certain forms of dyspepsia and general debility. From 10 to 30 grains of salt in a tumblerful of cold water, taken every morning on rising from bed, is highly promotive of appetite and digestion when the dyspepsia is unattended with organic lesion of the gastric surface and seems to be rather dependent upon a state of atony. The soda, in an equal quantity, constitutes an useful addition.
where there is a tendency to acidity, and also when the kidneys do not secrete freely and properly. The remedy appears to be especially applicable to the cases of general debility and nervous irritability so common among our ladies of sedentary habits. It may be sometimes necessary to commence with a smaller dose than just mentioned; but I have stated that suited to the majority of cases.

In Acute Dysentery, I have found common salt often of striking usefulness, but it must in general be given early in order to realize its value. It is now ten years since I first witnessed its efficacy in this disease. I had been attending a mulatto boy, eight years of age, for four or five days without being able to ameliorate his condition. The onset of his attack was attended with high febrile action, which, however, gradually subsided as he became exhausted by the continuance of the countless, bloody, mucous stools, and painful tenesmus. The rectum rejected anodyne enemata as fast as they could be administered. Nothing I could suggest seemed to bring any relief; and I left him one night, thinking his case hopeless. On the next morning I found him wonderfully relieved, and was congratulating myself with the idea that my last prescription had "done the deed," when the boy's father announced that he had taken the liberty to omit my prescription and to give his son last night, in lieu of it, a cupful of strong fish brine, which afforded so much relief that he had just again repeated it. The stools became watery and less frequent, the tenesmus was entirely relieved and the patient convalesced rapidly.

I have ever since that time resorted to salt and water; when the high febrile excitement conter-indicated the immediate resort to anodyne enemata; and when the administration of these failed to give relief. In the early and high febrile stage, it will often subdue the fever and tenesmus most admirably, and the patient will rapidly recover.

The efficacy of this and perhaps of other saline purgatives in dysentery, probably depends upon its combined depletory and revulsive operation. The depletion is derived from the small intestines in the form of serous stools, and this action must also relieve, by revulsion, the morbid condition of the large intestines. The case loses the painful and alarming character
of dysentery and assumes that of ordinary diarrhœa, which will either gradually subside or yield to vegetable astringents combined with opiates.

In the spring and in autumn, when the causes of periodical or paroxysmal fevers combine with the vicissitudes of temperature to produce diseases in which the phlegmasiae and the neuroses are so commingled as to re-act injuriously upon each other, we often find dysentery prevailing to so great an extent as to constitute an epidemic. If the febrile excitement be then closely observed, it will often be found to present daily exacerbations, which must be prevented by the use of quinine during the remissions, in order that the remedies directed to the local affection may have time to prove beneficial. If the quinine be withheld each febrile exacerbation will aggravate the intestinal inflammation, and this, in its turn, will make the next paroxysm more alarming—until it be too late to avert the fatal result. The same may be said of the so-called epidemics of pneumonia, which are most prevalent in mild winters and in remittent fever districts.

In cases unattended with fever, I am in the habit of resorting, as soon as the bowels are thoroughly emptied, to enemata consisting of a teaspoonful of laudanum, or half a grain morphia suspended in half a gill of thin starch or mucilage, repeated until retained long enough to give complete relief. This plan will be found to succeed in the great majority of such cases. Yet, we occasionally encounter one in which the anodyne will not be retained, and it is then that I resort to the salt and water.

The dose I administer is a teaspoonful of salt in a cupful of water, to be repeated every three or four hours until the stools pass off freely and without tenesmus. If the tenesmus returns, the salt should be again given, but at longer intervals.

I am aware that the sulphate of soda in similar doses has been highly recommended, and I have sometimes used it, as also the sulphate of magnesia, with decided advantage. Common salt, however, is less disagreeable, more convenient, and, I think, more efficacious.

Common salt has been of late urged in France, especially by M. Piorry, as a valuable substitute for quinine in Intermittent
Fever; and if it can be thus used advantageously, the discovery would be one of great importance to those who cannot afford to purchase the more costly article. I have prescribed it in only two cases of Intermittent fever; in one of which it proved successful, the patient having had but one paroxysm after its use was commenced. In the other, which was complicated with tubercular disease of the lungs and intestines, it increased the diarrhœa so much as to cause its discontinuance after the second dose. In these cases a teaspoonful in a tumblerful of cold water was ordered three times a day. It is to be hoped that this application will be fully tested by the Profession.

_Lemon Juice in Rheumatism._—There is perhaps no disease, for the relief of which more remedies have been and are still being continually suggested, than Rheumatism. The last in the list is lemon juice, of whose efficacy the British Journals are full. It may be known to some of those present, that I have for many years advocated the theory (first suggested by Prof. J. K. Mitchell, of Philadelphia,) of the Spinal origin of Rheumatic affections, and consequently the use of counter-irritants in the vicinity of the origin of the nerves leading to the seat of pain. My views upon this subject were published in the Southern Medical and Surgical Journal in 1837, and I have seen no reason to change them since. We however, not unfrequently, meet with cases of acute Rheumatism of great intensity, in which the spinal treatment cannot be energetically carried out on account of the inability of the patient to rest in any other position than upon his back. In these cases it is difficult to cup the spine often, and very painful to apply blisters to this region. We are then compelled to resort to other means. I should also add that the spinal treatment is by no means so speedily effectual when large joints are much affected with acute inflammation, as in cases of less violence. This is of itself an additional reason for the use of internal medication, than which I have found nothing more useful than repeated emetics of tartarized antimony, followed in the evening by full doses of opium or morphine.

Within the last few months, however, I have been induced to try the Lemon Juice in a number of cases of acute rheumatism and in the exacerbations of the chronic form of the dis-
ease, and always with most decided advantage. The remedy is very grateful to the palate, and the patients own that they feel better as soon as they begin its use, and worse when it is omitted. I usually order a tablespoonful of the lemon juice of the shops to be taken every four, three, or two hours, according to the violence of the case and the toleration with which it is received by the stomach and bowels. It seems to promote the action of the kidneys, to keep the bowels solvent, to lessen general excitement, and to diminish pain.

So simple and pleasant a remedy in so formidable an affection is well worthy of farther and systematic trial.

Collodion and its kindred preparations, the solution of Gutta Percha in Chloroform and Gum Shellac in Alcohol, are agents which promise to be useful. Collodion has been much lauded as an application to Erysipelas, and it is, therefore, proper to hear the testimony against it as well as that adduced in favor of its efficacy. Having recently had charge of a case of this disease, I applied the collodion very early to the part affected and a little beyond the inflamed surface. But the disease extended rapidly from the ear to the face and scalp, and in a few days invaded the entire head and a portion of the neck. The collodion was persevered in to the last without appearing to exercise any controlling influence whatever. My patient recovered, it is true, but I cannot attribute this result to the local application. This is the only case of erysipelas in which I have used it.

Solution of Gutta Percha in Chloroform.—This is made by dropping into a vial containing chloroform small fragments of pure gutta percha until the solution acquires the consistence of thick mucilage. It is then applied with a camel hair pencil, which should afterwards be repeatedly dipped in pure chloroform and carefully wiped with paper or old linen so as to prevent its becoming stiff and unfit for farther use.

I will now relate the result of its application in two cases of cancerous affection.

Mr. L. had been troubled with an epithelial cancer of the lower lip which had resisted all applications for eighteen months. There existed, upon the right side of the median line and at the junction of the skin and mucous surface, a small and thin scale
or scab, which would occasionally fall or be rubbed off, leaving
a raw surface of exquisite sensibility exposed to irritation until
another scab would be formed. Beneath this surface there was
an induration about the size of a common pea, or rather a little
larger, in which the patient frequently felt a very annoying
sense of burning, and sometimes darting pain.

At this stage of the case, as the patient was averse to the
knife, he was advised to try the application of collodion, which
he diligently persevered in for about six months, applying it
three or four times daily. This arrested the farther growth of
the disease, relieved its itching and burning, protected its
surface from ordinary irritants, but did not heal the denuded
surface. He then substituted the solution of gutta percha in
chloroform in lieu of the collodion. In a letter to me he thus
describes its effects:—"In a few days I saw and felt a change
in the color of the sore and in the irritation; in a week or ten
days, the lump disappeared and the irritation subsided, and in
three weeks it was almost entirely healed over; in less than a
month it was well, leaving an indentation on the lip."

In a note dated the 1st of this month, (April,) my patient
writes me: "I begin to feel a return of it in the same place the
cure was made eighteen months ago. Recently a lump has
appeared in the lip; it is hard and sometimes a little sore—it
gives me no trouble yet, but I am afraid of it." I will advise
him to use the gutta percha and chloroform again.

The next case was that of Maria, a negress, about 50 years
of age, who was sent to me from the country on the 3d of No-

vember last, with a cancerous ulceration of the mamma of sev-
eral months standing. Both mammary glands were very much
atrophied, but the affected one was the smaller of the two,
presented nothing but a mass of schirrous induration which
seemed adherent to the thoracic walls, and in the depressed
centre of which the remains of a nipple were to be seen drawn
back and ulcerated. The ulcer covered a surface equal to the
areola. The axillary glands were much enlarged, and the pa-
tient a prey to continual pain, especially at night, which de-
prived her of sleep.

Feeling satisfied that the knife promised no relief under such
circumstances, yet unwilling to send her off without trying
something, I put her upon the use of the gutta percha and chloroform, thoroughly coating the whole breast daily with it. The discharge from the ulcer would at first cause the pellicle of gutta percha to become loosened in twenty-four hours, so that the surface had to be cleansed before the re-application of the remedy. The suppuration, however, gradually lessened until the coating would remain a week—the painting still being made each morning. Under this treatment, the patient was gradually relieved of all pain about the breast and even in the axilla. She slept quietly at night, enjoyed her meals and felt quite well. Her general health improved, and she left at the end of one month, with instructions to continue the treatment perseveringly, and to get her master to inform me of the result. I have had no report from her since, but have learned incidentally that she never applied the remedy after she left here, and placed herself under the charge of some one who professed to be able to cure cancers—with what result, I know not.

These two cases are narrated with the simple purpose of directing attention to an application which may stay, if it does not cure, so formidable an affection as cancer.

Solution of Shellac.—The costliness of the solutions of gun cotton and of gutta percha renders it desirable to have a cheaper article that may be used as a substitute in cases which require the consumption of a large quantity of such plastic materials. A solution of shellac in alcohol has therefore been proposed for this purpose. This may be prepared by adding successively small bits of shellac to the alcohol of commerce until enough be dissolved to make a mucilaginous solution.

Some of the French practitioners having attributed to collodion extraordinary antiphlogistic properties when applied over affected joints and other inflammatory affections, even more deeply seated, I determined to try, during last winter, the shellac solution in an old case of Rheumatism, in which most of the joints of the extremities were being successively invaded. The toes, ankles, knees, fingers, wrists and elbows were nearly all alternately implicated—becoming very painful and rapidly swelling, so as to be almost doubled in size in a day or two. I furnished the patient a bottle of the shellac solution and ordered it to be painted over and around the joint as soon as it
would commence to be painful, and to repeat the application several times a day until a thick coating remained, after which it might be applied only once a day. Under this treatment I was gratified to find that the patient could, in a few hours, arrest the pain and prevent the swelling of the joints to which he made the application. He stated that he never had any thing to give him such prompt and effectual relief, although he had been suffering such attacks every winter for the last ten years. One joint or another continued to annoy him for a month, during all of which time he resorted to the shellac with the same success.

This is the only case in which I have tried this solution.

[Trans. of the Med. Soc. of the State of Georgia.]

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. Ricord. Translated from the French by D. D. Slade, M. D.

[Continued from Page 618.]

THIRD LETTER.

My Dear Friend,—The conclusion which terminates my last letter,—The blennorrhagia of which the muco-pus being inoculated gives rise to no result, does not recognize the syphilitic virus as cause,—this conclusion, deduced from undeniable facts, again places the history of blennorrhagia at the same point from which it has been transmitted to us in the book of Leviticus. Old as man, older than he, for animals created before him are subject to blenorrhagia, and not to the verole, this disease has nothing in common with the syphilitic infection.

In spite of those, who, since Paracelsus, Bethencourt and Fallopian, have wished to make of blennorrhagia, not symptomatic of chancre, a new disease identical with syphilis, the researches that I have made, corroborating the descriptions so exact of Alexander Benedictus and of Cataneus, have given to the doctrines of Balfour, of Todus, and of Duncan, the value and the solidity that Bell would have given them himself, if he could have explained the facts supposed to be exceptional, as we can explain them at the present day.

But blennorrhagia, as I understand it, absolutely different
from syphilis in its causes, in its form, and in its consequences, does it depend upon a special virus?

There would be nothing repugnant in admitting a special cause having the power specifically and constantly of producing blennorrhagia and its consequences. Nothing is more apt, in fact, to determine a blennorrhagia, than the muco-pus furnished by certain inflamed mucous surfaces. But when we go back in the strictest manner, and with the most rigid observation, to the causes determining the best characterized blennorrhagia, we are forced to see and to confess that the blennorrhagic virus ordinarily has no share in it.

Nothing is more common than to find women who have communicated blennorrhagia the most severe and the most obstinate, with the most varied and the most serious blennorrhagic consequences, and who were only affected with uterine catarrhs, sometimes scarcely purulent. Quite often the menstrual flow appears to have been the only cause of the communicated disease. In a great number of cases, in fact, we do not find anything, or only some errors in diet, fatigue, excess in sexual relations, the use of certain drinks, such as beer, or of certain articles of food, such as asparagus. From these circumstances spring that frequency of belief very often correct, that a gonorrhœa has been caught from a woman perfectly healthy.

Upon this point I am certainly aware of all the causes of error and I pretend to say that no one is more careful to guard against the frauds of every kind sown upon the steps of the observer than I am. It is, therefore, with full knowledge of the causes that I sustain this proposition.

PROPOSITION.

Women frequently give blennorrhagia without having it.

Blennorrhagia, such as some individuals persist in understanding it, that is to say, as the consequence of a contagion, is as rare in women as it is common among men. I do not believe that I advance too much when I say that women give twenty gonorrhœas where they contract one. And this is easy to understand, for women so subject to discharges not syphilitic from the genital organs, are the most frequent source of discharges which in the man cannot be considered as an effect of contagion.

It has been impossible for me to consider as serious the doctrine of my learned colleague, M. Cazenave, who admits very readily that many women under the influence of chronic uterovaginal catarrhs, can have sexual relations without communicating anything, provided that they are not "echauffées" to the
degree of virulence, or that they are not raised, so to speak, to a red heat. Is it not more simple to understand and more rational to say, that with a less degree of excitement, the secretions are less irritating, and that the being habituated to these secretions, would produce an immunity for some persons, and a sort of acclimation. It is thus, as I have frequently seen, that a married woman can cohabit with her husband without communicating any thing; but should a lover come, this last contracts a blennorrhagia. The husband was acclimated, the lover was not. When one studies blennorrhagia without prejudice, without preconceived ideas, he is forced to acknowledge that it is often produced under the influence of most of the causes which determine the inflammation of other mucous surfaces.

The experience of Swediaur is here to prove this. This observer injected volatile alkali into the urethra, and produced a blennorrhagia. Does this experience show that a blennorrhagia can be always produced, and at will, by irritating injections? No, certainly not, no more than a coryza or an ophthalmia could be produced by the same means. For a blennorrhagia, as for every other inflammation, the pre-existence of predisposition, that great unknown influence which dominates over all pathology, is necessary. This is proved by the fact that a blennorrhagia is not always taken in those same conditions where it is the most evidently communicable. Without this fortunate immunity which the absence of predisposition gives, blennorrhagia, already very common, would be still more so.

An experience of twenty years has taught me, and permits me to affirm, that excepting blennorrhagic discharges symptomatic of chancre, it is often perfectly impossible to recognize the cause of a blennorrhagia.

I know that many of my colleagues obstinately refuse to admit this opinion; every blennorrhagia awakens in them the idea of syphilis, and their therapeutic prescriptions are but the logical result of their prejudices.

Here, my dear friend, I ought to make to you a confession, and I shall make it publically. This persistence of some of my honored and learned colleagues, to always consider and to treat blennorrhagia as an accident of a syphilitic nature, has often astonished me. Thus it has many times happened to me, not to satisfy a frivolous curiosity, much less to yield to a culpable, slanderous motive, but to enlighten and re-assure my mind, to have recourse to a stratagem of which I wish to make the avowal with all the reserve and the delicacy that I owe my honorable brethren.

It was under the following circumstances:—A man presented himself at my consultation with well-marked blennorrhagia.
He stated to me that he had had relations with but one woman, and that this woman was his wife or his mistress. This man was uneasy or alarmed. He brought with him the woman the cause of his trouble, and the latter, protesting her innocence, along with the patient, supplicated me to submit her to the most rigorous examination. This examination, made with all the attention and care of which I was capable, showed me the sexual organs of this woman in a perfectly healthy state. There was nothing, absolutely nothing, in the most profound folds of those organs which could explain the blennorrhagia of that man. I begged the woman to pass into a neighboring room, and alone with the patient, I made use of all the means possible, of which I spare you the details, to arrive at this certainty, that the patient had had no sexual connections but with this woman; it was in these alone that he could have contracted the disease which he had. I reassured the husband or lover; I acquitted the wife or the mistress; but I begged them both to be accomplices in a little stratagem, which it remains for me to indicate. I sent them both and separately, let it be well understood, to such of my learned colleagues whom I know to be direct antagonists to me upon the question of blennorrhagia. I said to the patient, demand clearly this question: is my blennorrhagia syphilitic? I said to the woman, demand distinctly, could I give a blennorrhagia to a man? The couple returned to me, the man with a diagnosis thus written—"syphilitic blennorrhagia; the treatment followed ad hoc. The woman returned with this—"the perfectly healthy state of the organs permits me to affirm that madam could not communicate a malady which she has not."

It is not an isolated fact that I point out to you, my dear friend; this experiment I have often renewed, and sufficiently so, with some variations, to corroborate my convictions, and to re-establish my ideas.*

What do these facts signify? That the cause of a blennorrhagia cannot be always known; that this disease can be pro-

* There are some facts more curious still than those relating to blennorrhagia contracted with healthy women. A case analogous to the following has not been presented, perhaps, to the notice of M. Ricord, but of its authenticity it is not possible for me to raise the least doubt.

A man of thirty years of age, a physician, had been continent for more than six weeks, and his last sexual relations were not of a suspicious character. A fortuitous circumstance permitted him to pass almost an entire day in company with a young woman whom he loved. From ten o'clock in the morning until seven o'clock in the evening, he made vain efforts to overcome the resistance of this woman, whose virtue did not yield. But during all this time, this physician remained in a constant state of excitement. Three days after, he was taken with a blennorrhagia of the most violent and painful kind, which lasted forty days. Most assuredly here is the form of a blennorrhagia not syphilitic.—

Note of the Editor.
duced by causes common to all inflammation, if there is a pre-
disposition; but that the most special agent of blennorrhagia
is the mucous-pus furnished by the inflamed genito-urinary sur-
faces.

This manner of regarding it appears to me more rational
than that which would attribute the blennorrhagia called vena-
real to a sort of half virus imagined by our very learned brother
and ingenious writer on syphilis, M. Baumès. To this practi-
tioner, blennorrhagia is a degenerated kind of chancre; it can
give rise to a constitutional syphilitic infection, more feeble
however, than that produced by chancre, but without being able,
nevertheless, to reproduce this latter by means of contagion or
inoculation. "One can then foresee," adds M. Baumès, "the
greatest similarity between the constitutional symptoms which
are the consequence of the one and of the other of these disea-
ses; and in fact experience proves that the difference between
these symptoms lies not in their nature, but only in their degree
of intensity, in their gravity, and in their situation, which after
blennorrhagia extends generally to fewer tissues, and to a small-
er number of organs, than after chancre."—Baumès, Précis
théorique et pratique sur les maladies vénériennes, tom. i., page
259.

Here is a true half-way doctrine. This mere theory is neither
justified by facts, nor by observation or experience; one con-
dition is alone wanted to it—the proofs.

Hitherto, then, and it is certainly my present opinion, that
simple blennorrhagia is completely stranger to syphilis as to the
causes which can produce it.

But it has been objected to this, that the pus of chancre, that
is to say the syphilitic virus, can produce blennorrhagia. This
opinion is very old; it has been sustained since the appearance
of the verole in Europe, and it can be very legitimately still
sustained. But what does this mean? Are the observations
of the ancients to be relied upon? They are incomplete and
insufficient; it is impossible with these to proceed scientifically
from the effect to the cause. Would you appeal to experi-
ments similar to those of Harrison, who drew his conclusions
from the production of a blennorrhagia by the introduction into
the urethra of pus furnished by a chancre, without knowing
what it had physically determined. No, we shall arrive more
simply and more logically at the conclusion of the possibility of
the production of a non-virulent blennorrhagia, by the pus of a
chancre, in considering this pus as having the power to act in
the manner of simple irritants. A woman having chancres at
the inoculable period, could thus produce a blennorrhagia in a
man which could not inoculate. We can thus understand the
observations of Swediaur and others, in supposing that they had not committed some error in diagnosis, inasmuch as these observers made use of neither speculum, nor inoculation—observations which prove that men affected with chancre, have communicated blennorrhagia to women.

Here is what clinical observation teaches, and that which experiment can demonstrate. It is not rare to see patients with a chancre of the glans or of the prepuce, successively taken with balanitis or with balanoposthitis, determined by the irritating action of the pus of the chancre. But while the chancre furnishes pus inoculable, the pus furnished by the balano-posthitis is not so. (We shall see later that in order that the virulent pus should act specifically, some conditions are necessary which are not always met with.)

Faithful to my first conclusion, reducing to their just value these first objections, I affirm that when Harrison produced blennorrhagia with the pus of the chancre, either this pus acted after the manner of simple irritants or it produced an urethral chancre which was not ascertained. We shall see also later, than when Hunter produced a chancre with some pus supposed to be blennorrhagic, it was with the product of a true urethral chancre that he had operated.

But if inoculation has proved that the cause or the causes of blennorrhagia, whatever may be its seat in the two sexes, differ from the specific principle, from the virus which chancre fatally produces, the consequences of blennorrhagia ought always to differ from those of chancre; and yet many constitutional veroles are attributed to blennorrhagia. These are the questions which will make the subject of my next letter. We shall see, also, if it is possible to establish a differential diagnosis between two affections which some wish systematically to confound. You will permit me first to speak a word upon the inoculation of blennorrhagia.

Yours, Ricord.

FOURTH LETTER.

My Dear Friend,—As I promised, I shall say a few words upon the incubation of blennorrhagia. Incubation has been made a condition of virulence. Every virulent disease ought to present a period of incubation. Thus those who admit that blennorrhagia is a product of a virus, admit equally that this virus does not produce its first effects till after a time of incubation more or less long.

I say more or less long, and it is not without reason. The authors, in fact, as well for the incubation of syphilis properly called, have admitted for that of blennorrhagia a period the most convenient. The term of the incubation has been fixed
between some hours (Hunter and others) and fifty and some days (Bell.) What shall I say? MM. Cullerier and Ratier have reported the history of an incubation which lasted during five months. Assuredly a very elastic incubation. You know that matters are far from passing thus in the virulent diseases where the incubation is incontestable. The limits of the period of incubation can be more accurately fixed in the variola, in vaccinia, in scarlatina, in the measles, and in hydrophobia. The fine works of M. Aubert Roche have ever told us the certain limits of the incubation of the plague, which never exceeds eight days. For blennorrhagia, it is a far different thing, as you will see; here there are no certain limits.

What is, then, this incubation of blennorrhagia, which they have made me again very recently deny? We must understand this matter; it is a pure question of words. I do not deny the evidence; and consequently I do not deny that between the action of the cause, and the appearance of the first phenomena of blennorrhagia, there is a period more or less long; but is there present an incubation properly called, an incubation similar to that of the variolic or vaccine virus? I contest this, and I explain that time, more or less long, which exists between the action of the cause and the appearance of the phenomena, by the disposition and by the particular susceptibility of the tissues which have undergone the influence of the cause. There is no more incubation present in this case, than there is between the action of an exposure of the feet to cold, and the appearance of a coryza. One does not blow mucopus immediately from the nose after such exposure to cold; there exists a certain period between these two actions. Do you call this period the incubation of the coryza? Why then make use of a similar expression for blennorrhagia?

In those cases where blennorrhagia does not appear till long time after one is exposed to the suspected cause which produced it, is it not more rational to admit another cause which remains unknown, than that pretended incubation which nothing explains, nothing justifies? Is it not so in almost all inflammations? Can you always go back to the direct cause of a pneumonia, of an arthritis, of a phlegmon? Without doubt, in man the sexual relations are the most direct cause of blennorrhagia; but we should fall into strange errors, if we wish to refer all blennorrhagias to a virulent cause. I could give you some very singular examples which prove the contrary, but I refer the reader to the interesting note with which you have accompanied my preceding letter.

From this exclusive manner of considering the etiology of blennorrhagia, there results often, in practice, a singular manner
of interpreting facts. A man affected with blennorrhagia has had connection with several women; he hastens to make a sort of moral choice among these women, and by means of elimination he happens to fall often upon the most innocent. This sort of application of the law of suspicion has caused strange errors to be committed, of which I have often been witness.

Let us then conclude upon this point that the effects of blennorrhagia can follow at some distance from the cause which produces them, but that nothing proves that the period which exists between the action of the cause and the appearance of the morbid phenomena, is the result of a true virulent incubation.

I should prefer, my dear friend, not to make too frequent digressions from my programme, but how can I avoid deciding incidental questions when they present themselves beneath my pen? Such is that of the specific seat of blennorrhagia. You know that the question of this seat has been much agitated. In man it has been made to travel from behind forward, from forward backward; to advance or to retreat, at the will of the fertile imagination of writers upon syphilis. From the spermatic passages, in passing successively by the glands of Cowper, the fossa navicularis and the follicles of Morgagni, the seat of blennorrhagia has travelled a good deal. It is true that Bell, in establishing different degrees in blennorrhagia, has made its seat retrograde from before backwards. But it is not with these questions, so well known, that I wish to detain you. I will call your attention, however, to a singular prepossession of Hunter. This great observer admitted, as you know, a virulent blennorrhagia to be identical to chancre; he placed the seat of it in the fossa navicularis; but he inquires if the inflammation which propagates itself by degrees towards the posterior portions of the urethra, continues to be virulent beyond the fossa navicularis. We must confess that the genius of Hunter yielded to the spirit of system. Besides, in studying Hunter, we see his observing genius constantly in contest with his theory of blennorrhagia. He started with a false idea; facts come constantly to prove it to him, but theory is there to obscure his intellect, and in place of dismantling his theory by facts, he endeavours, on the contrary, to make facts agree with his theory—an excellent example of the dangers of pre-conceived and systematic ideas in the cultivation of the sciences of observation.

In the female, Graff placed the seat of the virulent blennorrhagia in the follicles in the neighborhood of the urethra. One of our brother physicians of Bordeaux, who died a few years since, Moulinié, thought he had seen in the glands of the vulva (so well described by Bartholin, and of which Boerhaave has traced
the pathological history, resumed and completed in our day by M. Hugenier) a sort of organ of virulence in a blennorrhagic point of view.

In the midst of all these opinions, strict observation shows that those portions of mucous surfaces the most exposed, are those which are the most easily affected. Nevertheless, we must allow that the mucous surface of the urethra in the two sexes is more often affected after sexual intercourse than the other mucous surfaces of the genital organs. This fact is an argument for the partisans of the virulent contagion. I will corroborate it, if they wish, by this proposition, which appears incontestible, that a woman attacked by blennorrhagia of the urethra can be considered as having the most commonly contracted it from a man suffering from blennorrhagia; and you see that this proposition could have its importance in legal medicine. Thus, for me, I should be ready to admit that a woman in whom I discovered a blennorrhagia of the urethra had taken it from a man. But does this fact come in aid of the existence of a virulent contagion? No, and I explain it by this other fact, alone true and incontestable, that pus furnished by the urethra is the most irritating of all pus for certain mucous surfaces.

While certain writers on syphilis contest the existence of blennorrhagia of the urethra in the female, others do not admit in her of a blennorrhagia except when it has its seat in the urethra. These two extreme opinions are erroneous. Observation has led me to admit all the varieties of blennorrhagia upon all mucous surfaces.

Whilst I am here, will you permit me to disemarrass myself of some other incidental questions relative to blennorrhagia? I shall proceed more freely and more rapidly afterwards, on the great questions which remain for me to treat of. If I examine the lesions of tissue which blennorrhagia produces, whatever may be the mucous coat affected, I do not find anything that simple inflammation cannot produce. There is sometimes a slight erythematous condition without secretion. It is the dry blennorrhagia of some writers, a denomination ridiculous and absurd, introduced into the writings upon syphilis, and in view of which we can admire the persevering efforts of M. Piorry to bring about a reform in the nomenclature. Sometimes we have to do with a mucous clement, catarrhal, and with all its products at different degrees, mucous, mucoso-purulent; in fine there are some true phlegmonous complications which we meet with, from which result in man for the urethra, the blennorrhagia accompanied with chordée, and the quite frequent production of abscess upon the course of the urethra.

But neither in the state of the tissues nor in the nature of the
products do we find anything which can be compared to the accidents of syphilis properly called.

Are the consequences of blennorrhagia comparable to those of syphilis? It has been said so, but it has not been proved. There are some analogies, without doubt, but some notable differences also. Thus one of the first accidents which blennorrhagia can produce, and which resembles one of those produced by syphilis, is bubo. But in the first place, enlarged glands are infinitely more rare as the consequence of blennorrhagia, than of chancre. In the next place, the bubo is never met with except in blennorrhagia of the urethra, in the two sexes, the other varieties never giving rise to enlarged glands. I well know that one of our fellow medical men of Belgium speaks of buboes peri-auriculaires, which ought to manifest themselves in blennorrhagia of the eye, but I must confess that I have yet to look for an example. In fine, the blennorrhagic bubo has this peculiarity, that purely inflammatory, it has very little tendency to suppuration, and when this happens it is never inoculable.

Do you wish to follow out that which blennorrhagia can produce ordinarily upon the two sexes? Take blennorrhagic ophthalmia, which never manifests itself but during a blennorrhagia of the urethra; in good faith, is it possible, unless we wish to confound everything, to establish the least comparison between this ophthalmia and syphilitic iritis?

With regard to blennorrhagic rheumatism, is it reasonable to establish the least difference between this affection and the accidents produced by syphilis upon the osseous system? Is there anything in the world more unlike than blennorrhagic arthritis and exostosis, for example?

What should I say of the cutaneous affections, except that I am profoundly astonished that some physicians have wished to discover a resemblance between the cutaneous affections produced by certain remedies employed in the treatment of blennorrhagia, and the special affections of the derma that syphilis produces. The previous holding of a false doctrine has here produced some very strange confusions. Blennorrhagia, it has been said, produces cutaneous affections like the chancre; and the roseola which succeed the use of copaiba and of cubebs have been cited as examples. I assure you that these roseola do not appear but when these resins are given. They answer me—but they do not appear except when there is a blennorrhagia existing. I answer, in my turn, that copaiba and cubebs are not given, but when there is a blennorrhagia. I add, and this is important, that I have administered copaiba in cases of vesical catarrh, and I have often seen these exanthenmata make their appearance.
But these resinous exanthemata have characteristics so marked, that with the strongest disposition in the world, it is impossible to confound them with genuine syphilitic exanthemata. They are developed generally with great rapidity; they are very acute, of rubecolic form, or often connected with lichen urticarius; if they are not very confluent, they are grouped preferably in the neighborhood of the articulations, and in the sense of extension, such as about the wrist, elbow, knee, instep, and around the ears; they are commonly accompanied with much itching, which is the contrary of syphilites, and a most important condition; so that we can say of them—sublatà causà tollitur effectus. In fact, they rarely survive a week the cause that produced them.

These exanthemata bring to mind a curious fact, which I ask you to permit me to relate in the form of an episode; it has also its instruction. Two or three years since, one of our most distinguished brother physicians presented himself at my house very much frightened. Until now, said he to me, I have had faith in your doctrine, but I find it at fault, and in my own case, that is truly hard. So saying he took off his clothes and said, "What is this?" showing me his chest and back. I examined and said, "That is a beautiful syphilitic roseola," "Syphilitic, do you say; and are you very sure of it?" "Perfectly sure!" "Ah, well, you convict yourself. I have never had in my life any other venereal accident than a blennorrhagia, and that was twelve years ago." "On your side are you very sure of that?" "Just as sure as of my existence. I examined my friend from head to foot, and having done so, I said to him gravely, and with a certain air of solemnity, "Friend, you have recently had a chancre upon the right hand, and the chancre was situated neither upon the thumb, nor upon the index finger, but upon one of the three last fingers." "You are joking," said he. "I am joking so little," I added, "that you still carry a bubo,"—and I made him feel, in fact, an axillary gland still enlarged. Then my friend, recalling his thoughts, told me that some months before he had attended and dressed a woman who had chancres; that an ulceration had come upon the middle finger, that he had not taken care of it, and that this ulceration had cicatrized. There is the source of your roseola, said I, and act accordingly.

Finally, what physician at the present day could confound the blennorrhagic epididymitis with the syphilitic sarcocele? It is no longer possible, since the time of Bell, still less possible since the works of Astley Cooper, and since what I myself have done in regard to this subject.

You will permit me to pass in silence the pretended tuberculous diathesis invented in Germany as a consequence of the blen-
Elephantiasis. [November,

norrhagic virulence. The question of tubercles in general is already sufficiently obscure, without adding to it any new darkness.

You see, dear friend, that I approach at last the programme that I had traced out for myself. In my next letter I shall enter upon it resolutely.—[Boston Med. and Sur. Journal.

Elephantiasis Arabum of the right inferior extremity successfully treated by Ligature of the Femoral Artery. By J. M. Carnochan, M. D., Professor of the Principles and Operations of Surgery in the New-York Medical College, Surgeon to the State Emigrants' Hospital, Ophthalmic Surgeon to the same Institution, &c., &c.

Case.—Charles Roller, of lymphatic temperament, and short stature, aged 27, born in Aix-la-Chapelle—occupation, merchant, left his home in December, 1849, landed in New-York in February, 1851, went thence to Connecticut, where for eight months he worked in a factory, standing during his hours of labor; thence went to Virginia, where he worked on a farm for about six months, at the expiration of which period he was taken with fever, of an intermittent character. Up to that time, he had always been in good health.

During the fever, the inguinal glands became swollen and painful; the swelling and pain extending in the course of the femoral vessels as far as the knee. The pain was followed by swelling and redness of the thigh down to the knee. From the knee, the pain and swelling continued to extend downwards as far as the toes; being, at this time confined chiefly to the portions of the limb along the course of the saphena vein, and also of the posterior tibial vessels. The redness and tumefaction here, as in the thigh, was preceded by deep-seated pain. The tumefaction of the limb continued to increase; while, at the same time, febrile exacerbations occurred at intervals, varying from two to six days. After a period of about six weeks from the commencement of the disease, the fever entirely disappeared, and by this time, also, the pain and redness had entirely ceased; the limb, however, remaining hard, swollen and rough, and presenting, in a marked degree, the peculiar characteristics of elephantiasis Arabum, in the chronic period of the disease. From this time forward, the hardness and intumescence gradually increased, and the limb became so cumbersome, that the patient was obliged to give up all business, and confine himself chiefly to a recumbent posture. In this condition, the patient left Virginia for the purpose of seeking medical relief at the New-York Emigrants' Hospital, into which he was admitted
the fifteenth of January, 1851. The appearance of the patient upon entering the Hospital was somewhat emaciated. He had no febrile symptoms, and the chief difficulty, under which he labored, arose from the enlarged and hypertrophied condition of the right inferior extremity.

The limb was enlarged from the toes to within a short distance below Poupart's ligament. The thigh, although enlarged, was not much indurated; while, from a short distance above the patella, downwards, the limb presented a dense, hypertrophied, hard, scaley, shapeless mass, the appearance of which will be best apprehended by referring to accompanying plate. the morbid condition of the tissues pervaded the foot and toes, there presenting groups of tuberculated growths. The circumference of the limb around the ankle, was nearly as large as that of the calf; measuring fifteen and one-half inches, while the circumference of the calf measured nineteen and one-half inches.

The patient was put under treatment upon entering the Hospital. The recumbent posture was enjoined, and for some time various discutient lotions were used. Bandaging was resorted to, with frictions of ung. Potass. Iod.; the Iodide of Potassium being also prescribed internally.

At times, also, the limb was painted with strong tincture of Iodine; local and general baths were used, regular bandaging of the limb, from the toes upward, being the while carefully observed.

This plan of treatment was perseveringly adhered to from the fifteenth of January to the twenty-second of March, a period of a little over two months, without any amelioration. Having thus tried, without success, the method of treatment most approved of, I proposed to place a ligature upon the femoral artery, with a view of changing the morbid condition of the structures supplied by the branches of this arterial trunk. A consultation was held, and my proposition was acceded to as preferable to amputation, the usual alternative resorted to in this stage and extent of the disease. Accordingly, on the twenty-second of March, 1851, I secured the femoral artery, a short distance below the origin of the arteria profunda. Upon exposing the femoral artery, this arterial tube was found to be changed, so as to present an appearance somewhat like the color of the aorta of the ox, and to be larger than the common iliac of the human subject. In consequence of this appearance of the artery, after some hesitation, I applied the ligature, preferring to do this, rather than to expose the external iliac, of the soundness of which I could not be certain.

The ligature came away from the femoral artery on the
eleventh day, accompanied by secondary hemorrhage, the occurrence of which I had expected as probable. For the purpose of arresting the hemorrhage, the external iliac artery was secured by ligature, by Dr. A. E. Hosack, who happened to be on duty at the time in the Hospital. The external iliac was found to be about the size of the brachial artery. This, for a time, apparently had some influence upon the hemorrhage; but on the following day, bleeding was again renewed from the orifice, in the femoral artery, with as much profusion as ever.

The hemorrhage was now restrained by the prompt application of a tourniquet, on the cardiac side of the bleeding orifice, by the house surgeons, Drs. Thompson and A. K. Smith.

This even failed to stop permanently the hemorrhage, and the blood recommenced oozing copiously at intervals. The patient was now sinking fast, and the ligature of the common iliac, or amputation at the hip-joint, appeared to be the only resources left. But the hemorrhage now being evidently reflux, it was suggested to apply the tourniquet, so as to produce compression on the distal side of the bleeding orifice: this was done, and was followed by a complete cessation of the bleeding.

From this time, (April fourth, 1851), the house surgeon kept an instructive record of the case, which record I have now before me. For several days, the pulse ranged from 115 to 108; the dressings were carefully attended to, and light diet prescribed. On the twelfth, the leg was found to be considerably reduced in size, and the ligature of the external iliac, came away. On the seventeenth, brandy and quinine, with good nourishment, were ordered. On May the first, finding the leg still more reduced and the lower wound healed, I ordered tincture of iodine to be painted on the leg, and the bandage to be continued; I also ordered a solution of chloride of soda to be used as a wash on the upper wound, which continued to discharge freely.

The patient now went on gradually improving in strength and appearance, and left the Hospital in the latter part of June, completely cured of his malady. At this date, sixteen months after the ligature of the femoral artery, the patient is in robust health, and presents no indications that the disease will return.—[New York Journal of Medicine.


Contusions, Ecchymosis, Sanguineous Tumors, etc.—The efficacy of arnica in these affections was first discovered in Germany; and gained for it the name of Fallkraut, or herba
lapsorum. It was formerly applied as a topical remedy; but at present it is more usual to trust to nature, and to employ rather camphorated spirits and saline lotions. It is not unreasonable, however, to give arnica internally after a fall, if the patient has, as often happens, undergone violent concussion amounting to stupor; but when reaction has set in, medicines of another class must be used.

Pulmonary Diseases.—Murray extols the power of arnica in a host of various diseases; and, in the first place, we have pulmonary affections, of which the names have in the present day changed: such as pain in the side with dyspnœa, false pleurisy, humeral cough removing in the summer and autumn; cachectic and œdematous cough; asthma arising from sudden chill, with rheumatism of the chest and back; asthma following delivery, with alteration of the voice and pain in the nape of the neck; peripneumonia. The tonic and stimulant properties of the arnica would evidently be far from always serviceable in these diseases, some of which correspond to pneumonia and pleurisy, others to catarrhs, or to convulsive coughs. But, according to Dr. Roques, the nauseating properties of arnica have several times triumphed over obstinate catarrhs. In these cases, the arnica should be combined with pectoral medicines, and its use must be persevered in for some time. Arnica is no less useful, according to the same author, in cases of pneumonia of an atactic character. In these cases, the arnica is given with extract of cinchona; this combination excites the powers of the system, reanimates the action of the lungs, and favors expectoration.

M. Martin Lauzer remarks that M. Guitrac gives large doses of tartar emetic in chronic catarrh; and Professor Brous-sonet and Cruveilhier give large doses of ipecacuanha in the same affection and in the pneumonia of old persons; and suggests that arnica might produce similar effects, in such cases, to those of ipecacuanha.

Arnica has been recommended by various authors in inflammation of the liver with petechia, suppression of the menses or lochia, uterine hæmorrhage, congestion of the spleen, nodosities of the breast, general atony, hectic fever, atrophy, calculous nephritis, and contractions of organs.

Paralytic Affections.—The writers of the last century have chiefly pointed out the effects of arnica in cases of paralysis. Juncker states, that he cured more paralyzed and contracted limbs with arnica alone, than with combinations of remedies. Collin states, that he cured tremblings, convulsions, palsies, and other nervous affections. Under the influence of this remedy, the patients had pain in the eyes, creeping and tick-
ling sensations in the limbs, and a sense of heat; and these phenomena almost always were prognostic of benefit. These effects are precisely those produced by strychnine; hence arnica would be a succedaneum for this formidable agent; and why should it not be employed before having recourse to the preparations of nux vomica? Collin says that, in cerebral affections, arnica is contra-indicated until the fever has ceased, or has diminished; and then nitre must be added to it. Kornbeck extols the action of arnica in mercurial paralysis: it would evidently be only useful in asthenic palsies, in whatever situation. Dr. Roger relates the case of a woman who, after fever, had a sense of weight and loss of power in the lower limbs. Under the use of powder of arnica flowers, she experienced creeping sensations and pain, followed by complete restoration of motion and sensation. According to Collin, Murray, and Conradi, arnica has also cured cases of asthenic amaurosis; but M. Martin Lauzer believes, with Schumucker, that it always fails in cases of amaurosis which have slowly gained their highest degree of intensity.

Spasms and convulsions: convulsive cough.—Arnica, according to Murray, has cured tremblings of the limbs or tongue, opisthotones, convulsive movements of the head, and twitchings of the limbs. If these affections are nervous, of asthenic origin, and consequently calling for the employment of tonics and stimulants, arnica may be of use.

Intermittent Fever.—Collin, Stoll, Aaskow, Deiman, and Voltelen, praise arnica as a remedy in ague. Stoll was very successful in the treatment of a quartan, which resisted cinchona. He made an electuary with flowers of arnica and syrup of orange peel; and gave a piece as large as a nutmeg four times a day. This dose caused severe pain in the stomach, and cold clammy sweats, with a large, full, and very slow pulse; but opium calmed these symptoms, which, however, were the forerunners of a rapid cure of the fever.

Typhoid Fever.—It was in the treatment of this disease that arnica enjoyed its best days; but here its fame also suffered dire shipwreck, thanks to its abuse by the Brownians, who almost used it as a specific in continued fevers. Collin said that arnica, by its cardiac power, removes stupor, somnolence and delirium, in putrid fevers, brings back the eruption of suppressed exanthemata, and resolves metastatic swellings. But Murray judiciously adds that, in order that the desired result may be produced, we must take account of the season and of general conditions;—words full of justice, a reflection on which would have prevented the abuse of using a single remedy in a disease so variable in its form. Stoll, who introduced the use
of Arnica in putrid fevers, gave it only in those cases in which, the pulse remaining nearly quiet, the patient was feeble, stupified, prostrate, in a state of somnolence, or muttering delirium. Arnica is also particularly indicated where the pulse is small, weak, and fluttering, with torpor and prostration of the muscular system. Dr. Roques says that it should be used especially in the enervating diarrhoea, in the obstinate dysenteric flux which, in the third stage of typhus, threatens to entirely destroy the vital powers. Murray recommends it, combined with camphor, when gangrene has supervened on other adynamic symptoms.

In the campaigns of the empire, the good effects of Arnica in the army-typhus have been observed and recorded. Dr. Cazin, who used it successfully in the army hospitals during the campaign in Germany in 1809, has since that time frequently employed it in the adynamic stage of typhoid fevers, combining it with the roots of valerian and angelica, which dilute its emetic and cardialgic properties.

Gout.—Barthez has recommended Arnica in the treatment of gout. Dr. Roques was of opinion that the best treatment of gout is to torment the patient as little as possible with medicine.

Summary.—Arnica is an energetic excitant, which is far from meriting the oblivion into which it has fallen. Its tonic, excitant, and emetic action, allays it to ipecacuanha; and the nervous symptoms which it secondarily excites, give it some relation to the preparations of Nux Vomica. Gilibert considered it tonic and aperient in small doses: and in large doses emetic, purgative, diuretic, sudorific, and emmenagogue. When given alone, it is liable to produce more or less severe pain in the stomach. This may be prevented or assuaged by combining the Arnica with a small dose of opium or some aromatic, such as angelica, canella, or ginger. But if its nauseating effects be desired, it must be given alone.

Doses and mode of administration.—The infusion, according to the formulary of the hospitals of Paris, is made by infusing for an hour, a drachm of the flowers in a quart of water, and straining. The decoction is made with the same proportions: it is more powerful. The dose of the infusion may be as much as an ounce, sweetened with syrup.

The powdered flowers may be given in doses of from ten grains to three or four drachms, in electuary or bolus; this form is preferable in cases of paralysis. The powdered root may be given in the same manner.

The dose of the distilled water is from an ounce and a half to three ounces of the alcoholic tincture (one part of the root to
eight of alcohol) from fifteen minims to five drachms; of the ethereal tincture (one part of flowers to four of ether) from fifteen minims to two and a half drachms; of the aqueous extract (one part to five of water) from seven grains to a drachm, in a draught or in pills; of the alcoholic extract (one part of flowers to eight of alcohol and one of water), the same quantities.

For External Use, the leaves and flowers may be used as a poultice: and the powder may be employed as a sternutatory.

Compound infusion of arnica flowers. (Roques.)—Take of arnica flowers, valerian root, each two drachms; infuse them in a closed vessel with half a pint of boiling water; then strain, and add, of peppermint water two ounces, simple syrup one ounce, ether a drachm, tincture of opium from fifteen to twenty drops; to be given by spoonfuls in the adynamic period of typhoid fever.

Compound preparations of arnica.—The above-named preparations are advantageously combined with wine, cinchona, camphor, and valerian.

Compound powder of arnica root. (Roques.) Take of arnica root in powder fifteen grains; camphor three grains; to be given every three hours in dysenteric typhus, to combat the prostration of the vital powers.

Stimulant bolus.—Take of camphor, arnica flowers, and treacle, each fifteen grains; divide into twelve boli, and give one every hour.

Stimulant electuary.—Take of powdered arnica root an ounce and a half, crude opium three-quarters of a grain, syrup a sufficient quantity. Divide it into six doses; one to be given every two hours in case of purulent absorption.

Aromatic tincture of arnica.—Take of arnica flowers an ounce and a half; cloves, canella and ginger, each two and a half drachms; anise, three ounces; alcohol, a quarter; macerate for eight days, and strain. A spoonful in water two or three times a day in contusions.—[London Jour. of Med. Ibid.]


In our former paper, the case of Williamson was presented with some remarks in relation to the use of sulphuric ether for producing anaesthesia in operations in the General Hospital at Vera Cruz, in 1847. In the summer of that year, an amputation of the thigh was performed, the patient having been put under the influence of ether, in which the hemorrhage was almost uncontrolable. The blood spouted in all directions, and I have
never seen an operation where it was necessary to secure so many bleeding vessels. Even after every small vessel that could be got at was secured, it was necessary to use cold water freely to suppress the general oozing of blood. At the time, I imputed the obstinate hemorrhage to the pernicious influence of the ether. In gunshot wounds anesthetic agents are almost universally unnecessary, and are almost universally injurious. It was for this reason that they were entirely given up in the hospital at Vera Cruz.

It may be well questioned whether anæsthetics are not calculated to produce injurious effects in all important amputations; but they certainly do so in operations performed for gunshot wounds. M. Velpeau says: “Chloroform evidently depresses the nervous system, and as great prostration always exists in patients who have received gunshot wounds, it is advisable to refrain from any anæsthetic means.”—Ranking’s Abstract, 1848. Mr. Alcock refers to the cases of soldiers wounded in battle, where the excitement is such as to carry them through almost any operation. I regret that Mr. Alcock’s paper is not before me. These are the cases spoken of by Mr. Guthrie: “Soldiers in general are anxious to undergo an operation when they find it inevitable, and frequently press it before the proper time; that is, before they have sufficiently recovered the shock of the injury.”—Gunshot Wounds, p. 232. These are the cases which require a little more time, some “encouraging words,” and perhaps a little wine or brandy and water; but no anæsthetics, for the patients are already sufficiently depressed.

There are two sets of cases; in one (Velpeau’s), the shock to the nervous system is great, from which the patient may not recover, and the use of anæsthetics would be awfully destructive; in the other class, they are unnecessary, and would prove useless and injurious. In the flap operation they must prove more injurious than in the circular; from the fact that muscle forms almost the entire covering for the stump; and the contractility of the muscular tissue is for a time almost annihilated, to be recovered irregularly at irregular intervals. Further, after the use of these agents wounds do not heal so readily by the first intention.

M. Jobert, on the use of ether, states that the local inflammation has proved less, and that union by the first intention has been prevented. I am able to bear testimony to the correctness of M. Jobert’s statement.

I must be permitted to refer to the Transactions of the American Medical Association for 1851, pp. 271, 272, 315, 323. In the Massachusetts General Hospital:
"It does not appear that the fatal results of amputation have at all diminished by the introduction of anaesthetic agents."

New-York Hospital:—

"The general mortality has been for three years and a quarter forty per centum. As regards the method of operating, we observe that the amputations of the thigh, in which the fatality was as high as thirteen in seventeen, were all flap operations. Eleven of the leg were removed by the circular, one died; while of four by the flap, two died."

That is, nine per cent. in one set of cases, and fifty per cent. in the other.

"In almost every case chloroform or ether was employed; but while it is admitted that anaesthetics may have had some influence in the increased mortality in the New-York Hospital over preceding years, since union by the first intention was now much less frequently observed; still it is to be remembered that hospital gangrene, entirely unknown before and purulent cachexia and erysipelas, extensively prevailed there during the past three years."

Could the anaesthetics have had an influence in producing the "hospital gangrene, entirely unknown before," and the "purulent cachexia and erysipelas," as well as prevent union by the first intention?

Dr. Lent, Resident Surgeon of the New-York Hospital, says:

"In almost every case, however, either chloroform or ether was employed; generally the former until the occurrence of a fatal case from it in this hospital; afterwards the latter, from which we have never had any bad consequences, and which has never failed to prove effectual. ** * * Anaesthetics came into general use about the period of the commencement of these statistics. May not the employment of these have had its influence upon the mortality? This is a very important question. We do not deny that it may have had some influence in augmenting the facility of operations; but we have seen no reason to infer that it has, except perhaps the fact that union by adhesion seems to have been much less frequent since the introduction of anaesthetics into this hospital than before. Whether the two are in the relation of cause and effect, it is, we fear, impossible to determine at present."

In an unhealthy atmosphere or climate, the healing of wounds by adhesive union is doubly important for obvious reasons; and I have often regretted that etherization was so much resorted to in capital operations at Vera Cruz during a portion of 1847; nor can I avoid congratulating both the patients and myself that before the summer had passed away, its employment was wholly abandoned. Anaesthetics poison the blood and depress the ner-
Wound of the Liver—Excision of a large portion of the right Lobe. By J. C. Massey, M. D., of Houston, Texas.

Some three weeks since I was summoned, in great haste, to visit a son of Mr. Simmons, at a distance of some thirty miles from this city. A brother of the unfortunate youth wounded, had a gun lying across his lap, picking the flint; it went off, the contents of the whole load passed into the right hypochondrium, and mostly out about the region of the epigastrium. The youth, who is about seven years of age, was standing close to the gun, and it was charged with large shot; a portion of the liver protruded through the external wound. A physician in the neighborhood was sent for, who reached the case about four hours after the accident. After examination, he viewed the case as hopeless, and consequently declined doing anything; he visited the case, however, on the next day, and advised that I should be sent for. On the fourth day after the accident I visited the patient, accompanied by my friend Dr. Black. We found him in a very deplorable situation; the anterior margin of the right lobe of the liver was protruding through the cavity on the right and a few lines above the umbilicus; it was in a gangrenous condition, with a portion of the omentum attached; the substance of both was so much altered, that it was really difficult to tell what the protruding portion was; the abdomen was very tense and hard, the least pressure giving severe pain; there was great arterial excitement, accompanied by a high inflammatory fever. This is a brief and very succinct account of the condition of the little patient, and my friend Dr. Black, as well as myself, regarded the case in a hopeless condition. I informed his friends, after making known to them the danger of the operation, that I would operate, remove the gangrenous portion of the liver, and give him all the possible chance there could be left for his life. From the external character and appearance of the wound, I was fearful gangrene had extended within the abdominal parieties.

I commenced the operation by enlarging the orifice about four inches; on examining the substance of the liver, I found two shot had passed at least two and a quarter inches from its inferior border, penetrating through it; the substance of the liver which was in juxtaposition to the wounds had a thick, grumous appearance, with sphacelated portions. Under the circumstances, I determined to excise every portion of the liver which had the appearance here described.
Blanchard, in his *Anatomica Practica Rationalis*, says, "A small portion of the substance of the liver may be removed without necessarily inducing a fatal result; and Dr. Henen, (Mil. Surg., p. 439) says, "A deep wound of the liver is as fatal as if the heart itself was engaged."

I felt great apprehension in excising the amount I was necessarily compelled to do, and when I inform you that I excised quite one half of the right lobe, equal to twice the amount of the left, you will then see how easy it is for persons high in the profession to make statements without proper data.

When the operation was finished, I passed a strong suture through the abdominal parietes, closed the wound, and subsequently a vigorous antiphlogistic treatment was adopted. I will not encumber your pages with a long detail of the daily treatment of this case. Nothing very remarkable, except for about ten days his discharges were passive, and he could exert no control whatever; at the present time he is able to exercise in his room, secretions natural, wound nearly healed up, and I consider him entirely out of danger.

This is an instance among many which may occur, and which may serve to prove to the profession, that a case, however desperate it may appear, should never be given up without an effort; and I do deem it very reprehensible, when professional men retreat, if I may use the term, in desperate cases. An operation once undertaken, should always be concluded *secundem artem*—according to the circumstances of the case, however desperate may be the supervening results, or the obstacles that may seem to render the operation unavoidable. Sometimes he will find in spite of all opinions, the patient recovers. I had a patient to lie apparently lifeless, in Grimes County, which is well recollected, under my own Scalpel, and under this embarrassing situation I finished the operation, and my doing so is the means of his present enjoyment of health, and his friendship to me. I was kindly assisted by my friend Dr. Black.


**On the Jaundice of Infants. By M. Duclos.**

Although this frequent affection of early infancy does not, in the great majority of instances, present any danger, it occasionally gives rise to important occurrences, and indeed, when complicated with other affections, may sometimes prove fatal.

Besides the yellow colour, the icterus of infants may be attended with fever, somnolence, tension of the belly, and colic, with constipation or diarrhoea. Its causes may be ranged under five different heads, which it is of importance to distinguish.
1. Retention of the meconium is the most frequent of all. If it be not evacuated within twenty-six hours, colicky pains are set up, and the skin becomes yellow. The colostrum is in this case the best purgative. When the child cannot or will not suck, a tea-spoonful or two of the syrup of rhubarb, chicory, and peach-flowers, equal parts, may be given. When, after the meconium has been passed, a considerable degree of tympanitis remains, together with what is called “windy colic,” preventing sleep, M. Duclos administers small doses of rhubarb and calcined magnesia. 2. The next in frequency is spasm of the digestive organs. The child suffers from cardialgia and colic, is in a state of fever, is constantly trying to suck, and has few or greenish stools. Sometimes convulsions occur. Purging and vomiting aggravate in place of relieving the condition. As retained meconium is usually the origin and cause of the symptoms, that must first be obviated, and then recourse had to emolient baths, mild anti-spasmodics, linseed poultices, friction with camphorated oil, and mild lavements. If the milk is too old, the nurse should be changed; and when an anodyne is required to relieve the violent colic; a little lettuce-water should be added to some sugared water. This description of medicine, however, requires care, and opiates in any form are inadmissible. Narcotism, which induced death in one child and was nearly fatal in another, was brought on by a clyster containing ten drops of laudanum. 3. Engorgement of the liver is another cause, and one especially acting after compression of that organ by the uterine contraction in foot and breech presentations. When this condition is present, purging the child is not sufficient. It must be kept warm, and its skin rubbed with hot flannel; with gentleness, however, lest erysipelas be induced. When the skin is rough and hot, emolient tepid baths are useful adjuvants. 4. Bad nourishment is a frequent cause of icterus, the milk disagreeing with the child, or improper food being given to it when brought up by hand. 5. Cold and humidity: young infants are very susceptible to changes of temperature—too great heat or cold being alike injurious to them; but as regards the present affection, cold is especially mischievous.—[Rev. Med. Chir. Med. Chir. Rev.

Burns and Scalds.

There is no practical subject in our profession, in which the disastrous and fatal effects of mal-treatment by medical men, as well as the mischiefs of popular ignorance are more apparent, than in the remedies resorted to in the cases of scalds and burns, now unhappily so frequent in our country, by reason of the
murderous recklessness of human life in the men entrusted with our public conveyances, in which steam is employed.

So long ago as 1830, in the first American edition of Cooper's Surgical Dictionary, published by the Harpers, of this city, we took occasion to urge upon the profession and upon the public the importance of a better philosophy and practice in the medical management of the mischiefs resulting from such accidents, than that usually in vogue. We then stated the results of our experience for ten years in the treatment of scalds and burns by the instant application of wheat flour, an article always at hand, and the perseverance in this application alone until all the acute inflammation had subsided. Our theory and practice thus promulgated, was approved and recommended in the then forthcoming edition in London, by Mr. Samuel Cooper himself, and has since found its way without credit into numerous publications at home and abroad. Even in the late Therapeutical work of Dr. T. D. Mitchell, of Philadelphia, this identical practice is ascribed to Dr. John Thomas of England! who in 1832 called the attention of the profession thereto, as we are told, in the Ohio Medical Lyceum; two years after our publication as aforesaid, and twelve years after our testimony to its efficacy had been published.

But waiving the unimportant subject of priority, we are grieved to learn from the public press that such multitudes are annually perishing by scalds in steam-boats, and from burns by camphene, spirit gas, and otherwise; nearly all of whom, however severely burned, we do not hesitate to say might be preserved from a fatal result if this simple practice were adopted immediately after such accidents. Instead of this, however, we hear of the application of cold water, lead water, molasses, oils, cotton, "pain extractors," &c. &c. accompanied almost uniformly by the death of the sufferer, and often "after lingering in excruciating torture" for days or hours.

Now it ought to be promulgated to the profession, and for humanity sake to be known to the whole people, that in any case of burn or scald however extensive, all the acute suffering of the patient may be at once and permanently relieved, and that in a moment of time, by sprinkling over the injured surface a thick layer of wheat flour by the hand, or what is better, by a dredging box. Every vestige of pain produced by such injuries is instantly removed, and the sufferer not only escapes the shock to the nervous system accompanying such torture, but will generally fall into a quiet sleep the moment the atmospheric temperature is thus excluded from the wounds.

Why then should persons thus injured be allowed to die with intense agony, occasioned by burns and scalds, as they often do,
if not without treatment by the applications so often made, many of which augment their sufferings, and render such injuries irreparable? Even in the late explosion on board the Reindeer, it is said that many of the scalded lived for hours, suffering all the time from their external injuries, and then treated with raw cotton, lime water, and linseed oil, &c. &c. until they were dead. Not a pang need have been endured beyond the time necessary to apply the flour, which must have been at hand, if the ignorance of their friends, and the antiquated prejudices of their medical advisers, had not led them to rely upon the miserable substitutes which superstition has canonized for centuries. And so, we affirm of every case of burn and scald, even if the entire surface has suffered.

In the New York and Bellevue Hospitals this mode of treating burns has been long in use; until recently, as we learn, the same object has been effected at the former institution by the analagous method of covering the injured parts with a mucilage of Gum Arabic, so as to protect the denuded surface from the atmosphere, and which the surgeons there prefer to the flour in some cases, where the weight of the latter becomes an inconvenience. To this method we make no objection, but having for so many years employed the flour alone, to the exclusion of all other agents, and in every variety and extent of injuries by fire, we have thus reiterated our testimony, and as this agent is found in every house, and can be instantly procured with more readiness than any of the other articles named, we give it the preference over all others. And we repeat our full persuasion that not one in a hundred of those perishing by burns and scalds, need succumb under their injuries if they were at once, or as soon as may be, covered with wheat flour. We have applied it successfully, after numerous other remedies had been unsuccessful, and when many hours had elapsed after the accident. To give this suggestion to the people, and scatter it broadcast over the land, will save a multitude of lives in a single year.

[New York Medical Gazette.]

Poisonous Chloroform.

To the Editor of the Boston Medical and Surgical Journal.

Sir.—The numerous deaths which have recently taken place from the inhalation of chloroform, seem to require that I should state what I know upon this subject, without waiting for more extended researches which I have now in progress; for a word in time may save human life, and I shall therefore present my views, even though some may think that I ought to wait until my work is completed to its full extent before publication. I have
formerly been charged with dilatoriness in presenting my discoveries to the public, and wish to avoid a repetition of this accusation, even though my work, in its present state, is not so complete as would be required for scientific purposes.

I have long had a strong suspicion that the very sudden deaths resulting from the inhalation of chloroform, must have been produced by the presence of some poisonous compound of amyle, the hypothetical radical of Fusel oil or the oil of whiskey; and I began a series of researches upon this subject several years ago, but was called off from my work by unexpected persecutions. This work I have resumed, and I will now state what facts and inductions I am able to lay before the public.

1st. When chloroform, and the alcoholic solution of it called chloric ether, was made from pure alcohol diluted with water, no fatal accidents took place from its judicious administration.

2d. When chloroform was made, as it now too frequently is, from common corn, rye, and potato whiskey, deaths began to occur, even when the utmost care was taken in its administration.

3d. In the Chelsea case, where this kind of chloroform was probably contained in the alcoholic solution incorrectly called chloric ether, death took place in a very sudden manner, and the post-mortem appearances of the subject indicated the usual effects of poisoning by chloroform.

From these data, it might justly be inferred that some poisonous matter exists in the cheap chloroform of commerce, and I suspected that it arose from the Fusel oil which exists in whiskey. This opinion, at my suggestion, was published by two of my friends, to put the public on their guard, and those gentlemen urgently advised that physicians and surgeons should return to the use of pure sulphuric ether (oxide of ethyle,) as originally prescribed by me.

It is well known that I have always preferred my original anaesthetic agent to all the substitutes that have been proposed since; but still I have always been willing to give the proposed substitutes a fair trial, and did try them all, first upon myself, and then upon such of my pupils as felt willing to allow the experiment to be made upon them. I also in a measure compromised with that powerful anaesthetic agent chloroform, by mixing small proportions of it, about one fourth or fifth part, with sulphuric ether, so as to concentrate the anaesthetic agent into a smaller bulk, and I have extensively used this preparation in the production of anaesthesia, and without producing any dangerous or even unpleasant symptoms in any case, but I always took care to ascertain that the chloroform used by me was pure.

Having, during the last month, succeeded in procuring some
very pure Fusel oil (of whiskey), I undertook the researches which have resulted in the conviction that it is this amyle compound that produces the poisonous matter of certain kinds of chloroform. When mixed with hyperchlorite of lime (bleaching powder) and water, in the same way as we prepare alcohol for the production and distillation of chloroform, I found that the mixture in the retort, after agitation and standing some time, became warm, indicating that a re-action was taking place between the Fusel oil and the hyperchlorite of lime.

After some hours the retort was placed in a water-bath and distillation was effected, the volatilized liquid being condensed by means of one of Liebig's condensers. A clear colorless liquid came over, which was at once recognized as having the peculiar odor of bad chloroform. It is perhaps a ter chloride of amyle, but has not yet been submitted to analysis. It is so powerful that merely smelling of it makes one dizzy, and working over it made me so sick that I was obliged to go out of doors for fresh air several times during my operations on it. In order to make sure that the Fusel oil was all decomposed, I again mixed the product of the distillation above mentioned with a new lot of bleaching powder, and water; and after three hours, with frequent agitation, it was again distilled, and gave what I regard as the pure unmixed poison. This I am now to test on such animals as have proved good ether subjects, and shall make report of my results in this Journal.

If my views are correct, it follows:—

1st. That all chloroform intended for inhalation as an anaesthetic agent should be prepared from pure rectified alcohol, to be diluted with water when used for distillation from hyperchlorite of lime.

2d. That no druggist should sell for anaesthetic uses any chloroform which is not known to have been properly prepared as above suggested.

3d. That the mixture of chloroform and alcohol, commercially known under the name of strong chloric ether, must be made with the same precautions as chloroform.

There is less danger of the existence of Fusel oil in sulphuric ether, which is always made from strong rectified alcohol.

There is more danger of the existence of sulphurous acid in this liquid, and that is a dangerous poison, but it is one readily detected; and persons will object to inhaling ether containing it, on account of its wellknown disagreeable odor of burning sulphur.

Fusel oil itself, according to the microscopic researches of my friend Dr. Henry C. Perkins, of Newburyport, appears to act as a poison. His experiments were suggested by an article
published by Mr. Henry A. Hildreth, imputing the poisonous qualities of some kinds of chloroform to Fusel oil contained in it.

It is important now that this Fusel oil has been introduced into medicine as a remedy in phthisis, that the profession should know that when it is inhaled it may produce fatal results, and that great caution is necessary in the use of so powerful an agent. Administered, a few drops at a dose, by the stomach, it does no harm, but is undoubtedly useful in some forms of disease. Experience will soon show how far it is remedial in tuberculous diseases; and this remedy is in good hands at present—Dr. Morrill Wyman and Dr. Perkins having engaged in the researches as to its medicinal use.

I annex a letter which I have just received from Dr. Perkins, deeming it an interesting contribution to physiological science.

Respectfully your ob't serv't, C. T. Jackson, M. D.

Assayer to the State of Mass. and to the City of Boston.

Boston, Sept. 1, 1852.

Newburyport, Aug. 27, 1852.

My Dear Friend,—Noticing the other day, a paragraph in one of the papers, which attributed the evil effects of chloroform to the Fusel oil it contained, I tried an experiment upon a frog with a few drops of this oil dissolved in ether, and found that after inhaling it for a short time the same effects were observable under the microscope as appear when chloroform is used, viz., an almost entire suspension of the circulation in all the bloodvessels ramifying upon the web of his foot; there was in fact, only a very slight backward and forward motion to be seen in one single vessel; in all the others the blood was perfectly stagnant. The frog was insensible for a much longer period than when the ether alone is used. He is now bright and ready for another experiment—to which I proceed.

I exposed him to the vapor of a few drops of Fusel oil dissolved in about a drachm of New England rum, for about six minutes, when he closed his eyelids and seemed under its influence. He was then placed upon the stand of the microscope, but not the slightest appearance of circulation was to be found in any of the vessels of the web; it was unusually pale and exsanguinous. He removed his foot twice or thrice from the stand, and gasped several times. I was now called away, and was absent about half an hour. Upon my return, the frog was found dead.

Several queries suggest themselves, which you will allow me to propose:—
1st. Is there any Fusel oil in sulphuric ether?
2d. Can the Fusel oil be removed from the chloroform?
3d. Would the vapor of New England rum, rot-gut whiskey (which contains this oil) produce anaesthetic effects?
4th. In what other liquors is this oil found?
5th. Does it in small doses, as administered by our friend, Dr. M. Wyman, and as I am now trying it upon his recommendation, diminish the pulse and act as a direct sedative?

To the third and fifth queries I shall direct my attention. The others I leave for your investigation.

Very truly your sincere friend, H. C. Perkins.


At a recent examination of the pensioners at the Hotel des Invalides, M. Hutin found that among the entire population of 3177 pensioners, there were 670 who had hernia. These were distributed as follow:

- 631 Inguinal (213 double, 418 single.)
- 6 Femoral (5 left, 1 right.)
- 18 Umbilical.
- 11 Superumbilical.
- 2 Subumbilical.
- 2 Near spine of ilium.


Miscellany.

Should the Use of White Lead as a Paint be forbidden by Public Authority?—This question is exciting considerable interest in France, one of the few countries in Europe where a due regard to the public health is part of the business of government. In England, and this country, we are too jealous of individual rights, too independent, if you please, to allow our rulers to watch over the well-being of the community.

We shall therefore merely present facts, without comment, as given to us in a memoir of Dr. H. De Castelnau:

"In his remarkable memoir on Painting with White Zinc, Dr. Bouchut advised the government, if it had due regard for the health of workmen, to forbid the use of white lead as a paint on all the public buildings, and that an example should be presented for imitation by the substitution of an article less deleterious. The favorable manner in which this proposition was received by the Academy of Medicine, at its session on the 4th of November, 1837, indicates their full accord-
ance in the idea, although they were necessarily restrained from enter-
tering into the merits of the question of economics, and we derive a
similar indication of opinion in the large premium bestowed by the
Institute, in 1849, on M. Le Claire, for his essay on the means of
rendering occupations less unhealthy.

All these circumstances have doubtless tended to aid in diffusing a
report that government is about suppressing the manufacture of white
lead. To aid such a measure, a few details on the point of sickness
and mortality will be of use.

In accordance with a requisition from the prefect of police, the ad-
ministration of hospitals demanded an annual return of all cases
admitted into them of diseases from lead. It thus appears, that during
ten years (1838 to 1847,) 3142 were admitted, and that 112 of these
died, being an annual mean of 314 sick and 11 dying. There can,
however, be scarcely a doubt but that the first number is too low.
There is very frequently a doubt as to the nature of the complaint on
admission—indeed lead affections take some time to develop them-
thes and thus cases are frequently referred to other diseases. It is highly
probable that at least 400 cases are annually admitted, and that fifteen
deaths occur.

Of the gross number (3142,) three-fifths (1898) were cases of work-
men engaged in the manufacture of white or red lead, and the remain-
ing two fifths were persons employed in using these products, as paint-
ers, grinders, makers of porcelain cards (so called,) &c.

Then again, there are many cases treated at their own dwellings,
but unfortunately we have no data exactly to estimate their number.
It is quite probable that they are at least equal to those treated in hos-
pitals, and if this be conceded, we have annually 400 cases of lead
disease in those who are strictly manufacturers of the preparations of
lead, and of which 14 die. It would be too extravagant to carry this
proportion throughout France. Reducing it ninetenths, and with a
due regard to the statistics of provincial hospitals, we are certainly
safe in stating the total annual result at 2000 cases of disease and 80
deaths. These would be at an end with the suppression of the manu-
facture.

But there is another matter to be also considered. The average
sojourn of a patient with saturnine disease in a hospital is 16 days.
Add to this, the illness and loss of time that precedes, and the debility,
broken health, and loss of business that follows so many of the work-
men. Even if we do not estimate this last, still, the hospitals will be
relieved annually of sixteen or seventeen thousand days of sick persons,
not to take into account the permanent residence of many incurables.

Can there, then, be a doubt that the public health will be greatly
improved by the suppression of these manufactories? Still, however
powerful may be the arguments in favour, it would hardly answer to
attempt their suppression, unless we could find a proper substitute,
both in the healthiness of its manufacture, and its value in the arts.
Can both of these objects be accomplished by the employment of the
white oxide of zinc (le blanc de zinc?)
As to the first, Dr. Bouchut, just at the time of concluding his memoir, in July, 1851, received the following return from the company manufacturing zinc at Asnieres. Up to the date named, they had employed 151 workmen, who together had performed labour during 31,585 days and had been in the factory 36,156 days. In other words the average was 209 labour days, and 344 days of residence for each person.

It is scarcely possible to present a more favourable bill of health. Who ever heard of a manufacturer of white lead remaining in its manufacture during 344 successive days? Besides, most of the above workmen still remain, and are able to count upwards of 1000 labour days.

Dr. Bouchut has carefully studied what should be called the effects rather than the diseases caused by this species of manufacture. They are as follows:—

1. Pains in the throat and slight cough occur during the first days of labour, until the mucous membrane becomes accustomed to the exhalations from the white zinc. But they disappear very soon, and the workmen there are no more subject to cough or throat affections than the same given number of any other persons.

2. Many of the workmen are at various periods subject to a curious species of innervation shown by febrile or non-febrile restlessness at night. But this does not affect the general health, and they return in the morning to their labour. Occasionally, there is a species of excitement, temporary, such as Delaroche and Barbier ascribe to the oxide of zinc, but with most it is the short feverish feeling just described. It is always of short duration, never dangerous, and disappears after the system has been accustomed to the employment.

3. Occasionally eruptions appear on the skin, of reddish papulae, which readily disappear with proper treatment.

Having thus noticed the effects, Dr Bouchut proceeds to mention three cases of slight disease, ascribed to this cause. But a careful analysis proves that they were not owing to it.

Here, then, we have results which are frequently produced by emanations from the most harmless substances, when inhaled in the form of powder. The difficulty only extends thus far. But while white lead as powder causes its severe results also, we must recollect that it is equally noxious when manipulated in the humid form. From this, however, white zinc is totally free. It is only the powder of it that affects the workmen.

We should also remember the large doses that have for many years been administered of the white oxide as a medicine, without causing any accident. M. Orfila, the highest authority in toxicology, gave 20 grammes ( ) to small and feeble dogs, with only the result of gentle vomiting, and a subsequent perfect recovery. How very different are the consequences of administering white lead.

As to the economic value of white zinc. It can be manufactured for exactly the same price as white lead, and being much lighter, a larger quantity can be sold for the same sum of money. It cannot be
adulterated. This, indeed, has been made a formidable objection to it. White lead is very commonly mixed with sulphate of barytes, and not unfrequently with chalk. White zinc can be used with equal facility as a paint. It does not dry as readily as white lead, but the difference in time is small. It has been objected that it does not set well as a paint, but this is altogether a mistake. Two coats cover wood very nearly as well as white lead, and there is this further advantage, the vapours of sulphuretted hydrogen do not affect it, whilst all the preparations of lead turn black from them.

M. Leclaire, an eminent house-painter, and others, have verified its use, on more than two thousand buildings, some of them public ones, to the satisfaction of the community.

The results, then, of suppressing the use of white lead by public authority will be—to save annually the lives of eighty workmen—to prevent 2000 cases of disease, some of them, indeed, incurable—and to enable active industry to continue its labours uninterrupted.

[Abridged from *La Lancette Francaise* (Gazette des Hopitaux) *American Jour. Med. Sciences.*]

*The influence which Daguerreotyping exerts upon the Health of Daguerrean Artists, together with some Observations on Light and Actinism.* By Charles W. Wright, M. D., of Cincinnati.

It is stated by Chevalier, in the Annals of Public Hygiene, that the vapors of iodine exert no injurious effect upon the health of the workmen engaged in its preparation; but this observation does not appear to hold good in other pursuits, where persons are compelled to inhale the vapors of this substance. Thus, there are some phenomena presenting themselves in the art of daguerreotyping, that would seem to indicate that the vapors of iodine, when inhaled for a considerable length of time, produce all of the peculiar effects of that agent when administered by the mouth. It must, however, be borne in mind, that besides iodine, the chloride of iodine, bromine and hydrofluoric acid are employed in the photographic art, and that the vapors of these bodies are floating in the atmosphere along with that of iodine; and as these substances belong to the same class, and when administered produce similar effects on the system, may contribute also to the appearances observed.

*The influence which these vapors exert on the respiratory apparatus.*—The most striking effect is the continual clearing of the throat. This the operator is frequently not aware of himself, until his attention is called to it. This symptom appears not to result from irritation or inflammation of the respiratory passages, but seems to be caused by thickening of the bronchial mucous. The same symptom is frequently induced by inhaling chlorine. Occasionally there is considerable difficulty in dilating the chest, but where the rooms are well ventilated, this is rarely observed. Inflammation has never been observed as a result of the inhalation of these vapors, as they are ordinarily diffused in the operating room. Some operators mix their compounds by the sense of smell, and not by weight or measure.
Effects on the Brain and Nervous System.—These vapors frequently produce determination to the brain and vertigo. Sometimes a species of intoxication is observed, where the fumes are very strong, but this is not common. These effects speedily disappear by exercise in the open air.

Effects on the Sight.—Irritation and chronic inflammation of the conjunctiva are sometimes observed. This membrane is so sensitive to the vapor of iodine, that many operators can detect its presence in the operating room more readily by its effect on the eyes, than by the sense of smell. It exerts no influence on the sense of hearing and touch.

On the appetite.—It very frequently diminishes the appetite, and in no case have I ever known it to be increased.

On the bowels.—It exerts no perceptible influence on the condition of the bowels.

On nutrition.—It has not been observed to cause emaciation, nor any decided increase of flesh.

On the salivary secretion.—In no case was salivation observed, but very frequently dryness of the mouth and fauces was complained of; at the same time the secretion of the nose was diminished.

On the kidneys.—The urinary secretion did not appear to be either increased or diminished. The urine passed when the operator is actively engaged in coating plates, always contains iodine, and by operating on a considerable quantity at a time, I never failed to detect its presence. In some cases there is considerable irritability of the bladder, and in one instance the individual could not retain his urine for a longer period than two hours.

On the skin.—The skin does not appear to be much affected by these vapors. In one instance, however, the eruption which is sometimes observed to result from the use of iodine, made its appearance. This person was sent to the country, when in the course of three weeks the eruption disappeared, but in returning to the same pursuit, it again developed itself, and the individual is at the present time affected with it.

On the genital organs.—These vapors sometimes act as an excitant to the genital organs. In some instances this excitement was followed by an almost total extinction of the sexual appetite. In one case there was an absorption of one of the testicles, with atrophy of the other.

It will be observed that all of the foregoing appearances are those that can be produced by the administration of iodine alone, and it would seem to be the active agent in these cases; but the bromine and chloride of iodine may, and probably do, contribute to the same result.

The mercury which is employed to bring out the image, is so small in quantity as never to produce the symptoms of mercurial poisoning; at least I have never noticed it, or heard of its producing a bad effect.

In galvanizing the silver tablets, the fingers are sometimes plunged into the solution of cyanide of silver, which induces a painful ulcerat-
tion around the nails, which, however, speedily disappears by proper treatment.

The above is the result of three years investigation of this subject, and the facts are gathered from the history of forty-three daguerrean artists, who have been in the business for periods, varying from two to twelve years.

Before leaving this subject, I would call the attention of the profession to the subjects of Actinism and Light. as these promise, above all others, to give us a clearer insight in regard to the influence which the atmosphere exerts upon health and disease, than any other branch of the natural science.

It is found that the actinic force is influenced by the seasons, temperature, the quantity of light which is transmitted through the atmosphere, and other causes which are not well understood. Thus the quantity is greater in March and April than at any other period of the year; and Prof. Draper states that in his progress from New York to the Southern States, he found it to diminish; and this is in accordance with the experience of daguerrean artists. The extinction of actinism in the atmosphere is not the same as that of light. Thus on certain dark, close days in the spring, the actini force is much less than it is when the air presents a more hazy appearance. There is also a very marked difference in the atmosphere of the city compared to that of the country, in regard to extinctive power. When a given amount of change is produced on a sensitive surface in the country in two seconds, it will frequently require thirty seconds to produce the same effect in the city, under the same circumstances. The extinctive power of the atmosphere of Cincinnati is about twice that of Louisville; and the atmosphere in the vicinity of Harrodsburgh Springs, Ky., possesses less extinctive power than that of any other locality which has been examined in the West.

Electrical action is quickened by actinism, and I found by making a sensitive plate a part of the galvanic circuit, that the action of actinism was favored by the electrical current.

It is to be hoped that some one who has the proper apparatus, will investigate the magnetic properties of oxygen in its relation to actinism, as these subjects must have a very important physiological bearing.

The following is a summary of our knowledge of solar radiations, by Prof. Hunt:

1. The rays having different illuminating or colorific powers, exhibit different degrees and kinds of chemical action.
2. The most luminous rays exhibit the least chemical action upon all inorganic matter. The least luminous and non-luminous manifest very powerful chemical action on the same substances.
3. The most luminous rays influence all substances having an organic origin, particularly exciting vital power.
4. Thus, under modifications, chemical power is traced to every part of the prismatic spectrum; but in some cases this action in positive exciting, in others negative depressing.
5. The most luminous rays are proved to prevent all chemical
change upon inorganic bodies, exposed at the same time to the influence of the chemical rays.

6. Hence, actinism, regarded at present merely as a phenomenon different from light, stands in direct antagonism to light.

7. Heat radiations produce chemical change in virtue of some combined action not yet understood.

8. Actinism is necessary for the healthful germination of seed; light is required to excite the plant to decompose carbonic acid; caloric is required in developing and carrying on the reproductive functions of the plant.

9. Phosphorescence is due to actinism, and not to light.

10. Electrical phenomena are quickened by actinism, and retarded by light.—[Western Lancet.

Lucifer Match Making and Amorphous Phosphorus.—The announcement of Prof. Shrotter's discovery of the mode of preparing amorphous phosphorus, derived much of its practical interest from the supposition that the phosphorus in this state would be less dangerous and injurious to the persons engaged in the manufacture of lucifer matches. A medal was awarded to Mr. Albright for the introduction of the prepared phosphorus as an article of commerce, and it may now be obtained at a moderate price of Messrs. Sturge, of Birmingham. The dreadful disease to which the makers of lucifer matches are liable, from inhaling the fumes of ordinary phosphorus, having been described and brought under public notice, it might have been supposed that no time would have been lost in ascertaining the value of the above discovery as a means of alleviating so much human suffering. Matches prepared with the amorphous phosphorus were shown in the Great Exhibition, and it was stated at the time that in the manufacture of these matches, the evils arising from the inhalation of deleterious fumes were obviated, while the result of the experiment was satisfactory.

It is, however, difficult to introduce any innovation of this kind into an extensive branch of manufacture. A series of experiments must be made to test the efficacy of the new preparations, the most advantageous mode of employing it, the quality of the goods, and the economy of the process. In the mean time the several departments of the manufactory are progressing like clock-work. All hands are busily employed, the proprietor is fully occupied with superintending the operations and the accounts, and a large box of amorphous phosphorus remains in the office unpacked, waiting for a convenient opportunity to complete the experiments.

Such was the state of affairs at Mr. Dixon's manufactory at Newton Heath, near Manchester, on the occasion of a recent inspection. Outside the building large piles of timber were stored up ready for use. A machine worked by a steam-engine was reducing blocks into the form of matches. A block previously cut the length of the match, and pressed against the side of the machine, disappeared in a few seconds. The sticks being removed into the next room were tied into bundles
about eight inches in diameter, ready for dipping in sulphur. This was done in another room in an iron vessel over a furnace. Immediately after the dipping, the workmen give each bundle a slight pressure with a rotatory movement to separate the matches from each other at the moment of solidification, otherwise the sulphur would cohere into a solid mass. The matches are next transferred into a room where they are arranged, so as not to be in contact with each other, in frames about two feet by one foot, ready for the phosphorus dipping. The composition used for this purpose consists of chlorate of potash, phosphorus, and glue, and it is spread in a thin layer on a stone or marble slab, heated below by steam or hot water. The operator holds the frame lengthways, and dips the ends of the matches in the composition, taking care that all of them are coated. Sometimes the sticks are in the first instance cut twice the required length, dipped at both ends, and afterwards bisected. In the process of cutting they occasionally ignite, occasioning loss and also vitiating the atmosphere. When the dipping is completed, they are taken to the sorting room and packed in boxes. In another room the boxes are labelled and sent to the packing room. The boxes are made on the premises, the shavings cut, and the tops and bottoms stamped by machinery, cut to the proper size, glued, and fitted, which operations are performed in separate apartments. Each box of lucifer matches, price retail one halfpenny, passes through the hands of seventeen persons, chiefly children. The Factory Act is not applicable to these establishments, and the children, averaging from seven to twelve years of age, work twelve and sometimes thirteen hours in the day. They earn (by piece work) from 3s. to 5s a week, and the adults from 9s. to 12s.

The cases of disease occur chiefly in the phosphorus dipping room, sometimes in the room where the matches are sorted and packed in boxes, but seldom in other parts of the establishment. The nature of the disease is described in the Dublin Quarterly Journal of Medical Science, for August, page 10, by Mr. Harrison:

"An affection ensues which is so insidious in its nature that it is at first supposed to be common tooth-ache, and a most serious disease of the jaw is produced before the patient is fairly aware of his condition. The disease gradually creeps on until the sufferer becomes a miserable and loathsome object, spending the best period of his life in the wards of a public hospital. * * * * Many patients have died of the disease; many, unable to open their jaws, have lingered with carious and necrosed bones; others have suffered dreadful mutilations from surgical operations, considering themselves happy to escape with the loss of the greater portion of the lower jaw."

Mr. Harrison's paper contains much interesting information, with the medical reports of several cases.

It would be foreign to our purpose to enlarge upon this view of the subject; but the disease being of chemical origin, the modus operandi of the poison may involve a chemical enquiry. Does the phosphorus when inhaled destroy the vitality of the bone by chemical action on its
substance? or does it operate merely as an irritant on the tissues, causing inflammatory action? The bone in its diseased state has a spongy cellular appearance, with excrescences of a similar character adhering to it. The teeth generally continue sound and white, while the jaw which contains them is altered in texture, dead, and discolored. We believe the diseased bone has not been chemically examined. Whether such examination would throw any light upon the subject is a speculative question; but we think it not unworthy of consideration. There are at this time in the manufactory several persons who have suffered severely from the disease, and who, on recovery, immediately returned to their work—not however to the dipping department. In the Museum of the Manchester Infirmary is the lower jaw of a young woman who is now at work. Her face is much disfigured by the loss of her chin, and in looking into her mouth the root of the tongue is seen connected with her under lip, the space formerly occupied by the jaw being obliterated by the contraction of the cheek. A young man who has lost his jaw is also in the factory. They are not isolated cases.

It is stated in the factory that the workpeople have sometimes applied the phosphorus paste to decayed teeth, under the idea that it was a cure for the toothache, and to this imprudence some of the early cases of the disease are attributed. The frightful nature of the disorder is now sufficiently understood to serve as an incentive to greater precautions. Increased attention has been paid to ventilation and cleanliness, and the practice of taking meals on the premises is not allowed. It appears, however, from the statements of some of the workpeople who are engaged in the phosphorus dipping room, that their clothes become incandescent in the dark, and although the cases of the disease are less frequent than they have been formerly, a security against its recurrence is not attained. The proprietor of one factory states that he has had no cases in his establishment on account of a more careful method of dipping the matches, by which the face of the operator is further removed from the source of danger; but we are informed that some patients, from that factory have applied for medical relief in the neighborhood. Mr. Standring informs us that there is now in the Manchester workhouse a young woman suffering from a "phosphoric jaw." She worked three years in a match manufactory; she then went to a silk mill, where she had been about a year and a half before the disease first made its appearance. Eleven months since she was was admitted into the infirmary and remained there eighteen weeks, since which time she has been an inmate of the work-house. The disease at present affects only one side of the jaw—a portion of which is likely soon to be detached.

Various means of prevention have been tried, and others suggested. In a manufactory in Dublin, camphor is added to the composition, which masks the smell, and is said to act as a prophylactic. This latter opinion requires further proof. Mr. Taylor of Nottingham, suggests the use of a mask with a tube communicating with the outside of the building. Mr. Stanley of St. Bartholomew's Hospital, recommends
the exposure of oil of turpentine in saucers about the workrooms, as a solvent of the fumes of phosphorus. Dr. Baur recommends the use of a sponge or handkerchief moistened with a solution of soda or potash and applied to the mouth. The proprietor of the factory above referred to states that he has diminished the quantity of phosphorus to less than a third of that which he formerly used, and that by this and other precautions the prevalence of the disease has been greatly diminished. He has tried the amorphous phosphorus on a small scale, by way of experiment, and says that it is more expensive than the ordinary kind, as a larger quantity is required. But the chief objection appears to be that the composition now in use answers quite well. The matches never fail; the mode of preparing the composition is understood; the result is known, and the demand for the matches unceasing. The amorphous phosphorus requires further trial; the makers are not yet accustomed to it. If it should fail their trade would be injured; the experiment would interfere with the habits of the factory; therefore the operations are continued in the usual way, the box of Sturge's phosphorus remains unopened in the office, and the value of the discovery is not fairly put to the test.—[London Pharmaceutical Journal.

American Medical Association.—At a meeting of the Association held at Richmond, Va., May, 1852, the undersigned were appointed a committee to receive voluntary communications on medical subjects, and to award two prizes of $100 each to the authors of the best two essays.

Each communication must be accompanied by a sealed packet, containing the name of the author, which will be opened only in the case of the successful competitors. Unsuccessful communications will be returned on application after June 1st, 1853.

Communications must be addressed, post-paid, to the Chairman of the Committee, Dr. Joseph M. Smith, 56 Bleeker-st., New York, on or before the 20th of March, 1853.

Joseph M. Smith, M. D.
John A. Swett, M. D.
W. Parker, M. D.
Gurdon Buck, M. D.

New York, Sept. 17th, 1852.

Alfred C. Post, M. D.

Another Medical School.—We perceive by the newspapers that our Savannah friends are about to establish a Medical School in that city, and that the Faculty has already been organized, as follows:

R. D. Arnold, M. D., Professor of Practice.
P. M. Kolloch, M. D., Professor of Obstetrics and the Diseases of Women and Children.
W. G. Bulloch, M. D., Professor of Surgery.
C. W. West, M. D., Professor of Chemistry.
J. G. Howard, M. D., Professor of Anatomy.
H. L. Byrd, M. D., Professor of Materia Medica.
E. H. Martin, M. D., Professor of Physiology.
J. B. Reid, M. D., Professor of Pathological Anatomy and Demonstrator of Anatomy.