SOUTHERN
MEDICAL AND SURGICAL JOURNAL.

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VOL. XII.—1856.—NEW SERIES.

AUGUSTA, GA:
McCafferty's Office—J. Morris, Printer.
1856.
A Thesis on the Detection of Arsenic: presented to the Faculty of the Medical College of Georgia, Feb'ry, 1856. By H. W. Dessy Ford, one of the successful candidates for the Doctorate.

It may be both instructive and interesting to glance at the marked difference in the position which chemistry now occupies, relative to this particular subject of critical analysis, and that it held at former periods. Instructive, because showing the wide difference between ancient and modern chemistry, and also how rapidly it has developed itself into the form and stature of a perfect science, repudiated many of its theoretical speculations, and fixed itself upon a firm foundation, which the cavils of the skeptic cannot shake. Interesting, because in perusing its literature of the sixteenth and seventeenth centuries, we are forcibly struck by the fact, that when secret and slow poison was first introduced into England, a few true stories of poisoning formed the nucleus of a thousand more, that were the creation of fancy, terror or malignity.

A writer, in speaking of the infant science, says: "It was only natural that the wonders of the new science should excite the terror of the ignorant, and be the subject of numerous exaggerations. The destructive powers of arsenic, furnished a readier theme for the marvel loving, than its powers of healing."

This arsenic was used to so great an extent, as the means for poisoning, that many eminent, learned men, were victimized by...
their contemporaries; "no reckoning made, but sent to their account;" suddenly by its administration at the hands of their most intimate, ambitious friends.

The science of chemistry was then in such a state, that, although it was called in to aid legal investigations, yet, the world had little confidence in its powers. Then, indeed, the demands upon chemistry asserted themselves, and society called out for some new light, and scientific men were morally bound to work energetically, and faithfully to answer these demands, or else, see their infant, though advancing science, sink into disrepute. These devotees, then, did begin their noble work, with minds which would not flag, though frames would yield. They studied the properties of this newly discovered metal more thoroughly; noticed the different phenomena it presented, under numerous circumstances; perceived its specific effects upon the viscera of living man—in short, all its properties and different phases came to be known. This knowledge, with such minds as Campbell, Gordon, Magendie, and others, was soon acquired, by their diligent research and numberless experiments. Then, if a person supposed to have been poisoned was examined, another all important question arose: What was the specific article?—for there are numerous poisons, animal, mineral and vegetable, each capable of causing death, and marked by its peculiar specific effects.

We perceive, then, that as rapid as the discoveries in the properties and actions of arsenic advanced, the learned Profession of the Law put forth some new questions in legal investigations, which tended to bewilder the physician and chemist, thereby showing that, though he supposed himself in possession of all knowledge of arsenic, in his post-mortem examinations and experiments, yet there were some items wanting to make his testimony complete. The chemist and his science doubtless owe much to the urgent demands which the Law has always pressed upon them. These demands of the Bench have forced the chemist to a rapid advance in methods of detection—as if, unwilling that the rational demands of one of the learned professions should remain unanswered by the other, to its reproach. Instead of being offended by the cavils and objections of a rival profession, they have always used these as arguments rather for more diligent study and research. The result has been the development of methods, by which the most stringent demands of the Law have been fulfilled,
so that this poison can not only be extracted from the organs of the person poisoned, but actually reproduced, free from any trace of organic matter, in its original state of arsenious acid, and of metallic arsenic.

This art of detecting arsenic, we find, has been advanced and perfected by another cause, the strict criticism of the proposed method of one chemist, by many rivals for fame, in the same arena; we must recognize this as one of the causes of progress, even whilst deprecating the extreme rancor and bitterness, which this rivalry has, in many instances, engendered; even the factious objections suggested by this spirit of rivalry, thus degenerated into coarse personality, may not have been without their good fruits. A striking instance of this is furnished by the history of the Laffarge case, in 1840.

The body of M. Laffarge had been examined by three different commissions of chemists, each of which failed to discover evidences of arsenic, to the satisfaction of the tribunal of justice. A fourth commission was conducted by Orfila, and others, and metallic arsenic produced from the organs.

M. Raspail was called by the defence to contest the results obtained by Orfila. Subsequently, in the medical journals, M. Raspail commented on these results, to his disgrace, charging M. Orfila with using materials, known by him to contain arsenic; and with a predetermination to find arsenic: and raising the most frivolous objections—only to give one example—that the arsenic, if found, might have been furnished by the arsenite of copper, coloring a small piece of green paper, found on the top of the grave. In answering these frivolous objections, the whole subject of the reactions of arsénical preparations was most fully again gone over. Most triumphantly did Orfila vindicate himself from all objections, leaving his objectors to the scorn of science, for this base attempt to invalidate his testimony, by ridiculous objections, and to stigmatize the name of such an one—a man endowed with such clean thoughts: “his only talisman being Truth—his only spell-word” Humanity.

This same cause of rivalry, (though more honorable than in the foregoing case,) has furnished us with very many methods of detecting arsenic unequivocally in the dead body, which may contain it.

Having thus cursorily and hastily glanced at some points in the
history of this subject, I shall detail some experiments undertaken, to determine for myself, the relative value of some of the many of these methods. This I was induced to do by the interest awakened in the subject, whilst assisting my father, Professor Ford, in examining the organs of a child, suspected to have been poisoned by arsenic, during the past summer. In these experiments the animal matter used was coarse glue.

First, was prepared a solution of arsenic, of the strength of 1 gr. to $\frac{3}{8}$ viii. of water.

A. Cold water $\frac{3}{3}$ xvi. in a capsule, 3 j. of arsenical solution, equal to $\frac{1}{6}$ gr. arsenic, four small pieces of copper wire, half inch in length, thoroughly polished, and 3 j. of muriatic acid. This was boiled for one hour, when the copper wires were taken out, placed in filtering paper, and laid aside for further examination.

B. Glue $\frac{3}{3}$ ss. in a capsule, with 5 j. of arsenical solution equal to $\frac{1}{6}$ gr. arsenic, and 5 j ss. sulphuric acid. This was boiled nearly to dryness, when nitro. mur. acid (nitric acid 3 parts, mur. acid 1 part) enough to moisten it, was added. Then dried by heat. When cool, $\frac{3}{8}$ vi. of water added—boiled and filtered. Also $\frac{3}{4}$ iv. water again added to remaining charcoal—boiled and filtered, and added to the above. This was done in order fully to dissolve any arsenical preparation.

C. Glue $\frac{3}{3}$ ss. in a capsule with water, add 3 j. of arsenical solution, which was boiled to dryness and scraped from capsule. Nitric acid $\times$ x. in a capsule was heated and the above gradually added, and as soon as dissolved, the mass charred instantaneously, without flame, leaving a dry, light charcoal. To this $\frac{3}{4}$ iv. of water added, boiled and filtered. Charcoal remained in capsule. $\frac{3}{4}$ iv. of water again added, boiled and filtered. Charcoal still remaining, was boiled in previously filtered solution and filtered, and filtered solutions mixed together.

D. Glue $\frac{3}{3}$ ss. placed in capsule, with $\frac{3}{3}$ xvi. water, 3 j. arsenical solution and 3 j ss. muriatic acid; this was boiled a few minutes to dissolve the glue, and 4 pieces of copper wire, half inch in length, put in, and this boiled for an hour and a half. The wires were placed in filtering paper and laid aside.

$\frac{3}{3}$ xii. water put in Marsh's apparatus, 3 j. arsenical solution, 3 j ss. sulphuric acid. Metallic spots obtained, but very equivocal. Solution of C, $\frac{3}{3}$ vii. put in Marsh's apparatus and $\frac{3}{3}$ j. sulphuric acid. Four or five spots were obtained in about thirty minutes,
having a metallic lustre, though but two were unequivocal. Foam not embarrassing.

After long use of the apparatus, as above, a glass globe, two inches in diameter, having a perforated stem one-eighth of an inch in diameter and two inches long, was perforated at the point opposite the stem; the stem and the half of the globe, to which the stem was attached, were covered with several folds of cotton cloth. The gas from the apparatus was then inflamed, and the nozzle introduced through the opening in the globe, so that the flame burned about the centre of the globe. This combustion was continued for an hour and a half, the cloth on the outside being kept saturated with water. At the end of the process about 5 ss. of fluid was found condensed in the lower part of the globe. To one half of this condensed fluid was added a drop of ammo. sulphate of copper. The result was a decided green precipitate—Scheele’s green.

Solution of B, § vii. was put in Marsh’s apparatus, and § j. sulphuric acid added. Four or five spots, having a metallic lustre, were obtained, but very faint. Foam was very embarrassing. The above was taken from apparatus, and boiled to disencumber it of sulphate of zinc; again placed in the apparatus, with more of same solution added. The foam was still so embarrassing as to induce abandonment.

The copper wires from A were put into a glass tube, and heated to redness. The film of arsenic, in crystals, was chased by heat into the smaller end of the tube, which part of the tube was crushed. A small quantity of water was added, and this boiled in a small capsule. This process was very unsatisfactory. The copper wires from D were treated as the former; this was as unsuccessful.

The solution of B was boiled in muriatic acid, to get rid of the foam; after which, it was put into a small apparatus, of domestic manufacture. Foam was still embarrassing. It was then put into a capsule, boiled to dryness, and then charred with nitric acid. Water was added, and resulting solution was put into apparatus. From this were procured unequivocal arsenical spots. After obtaining these spots, the flame was burnt in a glass globe, as above, for one hour. Took the condensed liquid and tested it with ammo. sulphate of copper. The result was an unequivocal precipitate, though not so decided as of solution C.
Having undertaken these experiments for the purpose of determining the relative value of the methods used, I shall draw my conclusions from these; not presuming to insist upon them, in opposition to any one of expertness in such chemical manipulations, who may have a different opinion, remembering that so much depends, in such investigations, upon that expertness, which practice can alone give. Although many writers and experts have given the preference to the process with sulphuric acid, yet I am constrained to believe, that the process with nitric acid is the better of the two, and the most reliable of them all. The manner of conducting the experiment is simple, yet exquisitely delicate. Although that part of the glue, treated with sulphuric acid, gave a similar result, in the glass globe, yet it had been treated by nitric acid, to dissolve all the animal matter, and with all was the result not so palpable. Another very essential difference between the two experiments was evident, showing more fully, that the nitric acid is more reliable. The foam was so embarrassing with that treated by sulphuric acid, as to induce abandonment, while that by nitric acid showed, at no time, the slightest indications of this foam. In my notes, where the experiments are enumerated and commented upon, it will be seen that the process with copper wire proved unsatisfactory. This, doubtless, was owing to the coarse manner in which the experiment was conducted—from which circumstance, I am unable to compare that delicate process with the others.

The process with the bulb has certainly much more to recommend it as the most reliable of all processes for detecting, and which doubtless will be considered by all intelligent experts as paramount. In my experiments (even after a long and continued use of the solution in the apparatus, which circumstance tended somewhat to exhaust the arsenic), by burning the flame through the glass globe, and testing the condensed liquid with ammo. sulphate of copper, I procured the decided green precipitate, which fact seems to show, that arsenic may be detected, by this method, in very minute quantities. I did not make the test with ammo. nitrate of silver, which doubtless would have given the peculiar characteristic yellow precipitate, because there was none at hand properly prepared. It may be well to add, that, whenever these two precipitates are obtained from the same liquid condensed, the semblance of a doubt of the presence of arsenic is immediately
dissipated; for the uncertainty which attaches to these liquid tests, under ordinary circumstances, will not attach to them in this instance, inasmuch as this liquid being condensed from a burning flame, all animal and vegetable matter must necessarily have been destroyed by that flame. In the process of metallic spots, often they are so feeble, that although the chemist himself may be satisfied with the results of his tests, yet his testimony may often be objected to, either by the counsel for the defence, or the Bench, whereas this process with the bulb would come in as a most perfectly satisfactory one.

From the intricacy of this kind of critical analytic investigation, it may be said, that one of the most difficult and responsible offices of a physician is called in question, when he is required to institute a post-mortem examination, to detect the presence of arsenic in the viscera of the body. Difficult, because it requires the deepest, indeed exacts a most thorough knowledge of Chemistry, without which he must submit to the mortification of displaying his ignorance of that branch of medical science, which is actually necessary that the accomplished and educated physician should possess. Responsible, because in legal investigations, where there is a suspicion of poisoning with arsenic, it often happens that justice will depend on the decision of the expert. From this last fact, then, how noble is that science which can arrive at these conclusions—and in these days with such a degree of certainty;—and what an enviable man he, who can embrace this knowledge to arrest and detect the villainous steps and practices of the homicide. Besides a perfect acquaintance with the laws of Chemistry, and with the difference between the normal and diseased states of the body, a precise and delicate manipulation is also requisite. The precaution of the systematic arrangement of every article used, should always be taken, so as to avoid even the possibility of any accident happening from their misplacement.
LETTERS FROM SAML. D. HOLT, M. D., UPON SOME POINTS OF GENERAL PATHOLOGY.
LETTER NO. 10.
MONTGOMERY, ALA., Feb. 25th, 1856.

Messrs. Editors—To say that Calomel is one of the most valuable of the mercurial preparations, and one of the most valuable articles of the materia medica, is but to re-assert a truth, which all candid and unprejudiced physicians must acknowledge as having been long since established, upon the accumulated evidence of experience and observation, notwithstanding the unceasing and unmeaning hostility and obloquy which it has encountered, not only from those out of the profession, whose ignorance of its properties, and uses, is a sufficient justification for their prejudice and hostility; but from those of the profession, who have less justifiable and less creditable grounds for theirs. And though we may never expect to see the time arrive, when ignorance and selfishness will cease to inveigh against its use, we do hope to see the time come when honest and intelligent practitioners everywhere, will rise up as one man, and assert and maintain the honor, dignity, and the independence of the profession, and throw off the ban of proscription, which has been placed upon them by popular prejudice, fostered and encouraged, as it has been, by designing Charlatans. For it is not only disgraceful to the profession at large, but it is exceedingly humiliating to the honest and conscientious members of the profession, to be compelled, in the present enlightened age of the profession, to become apologists to popular opinion or professional sentiment, for avowing an advocacy of the use of Calomel, or any other well known and valuable article of the materia medica.

Now, as I am addressing myself to the honest, candid, and intelligent portion of the profession, whether the advocates for the use of calomel, or not, I will propound a few questions for their consideration, and ask: What are the properties and uses of Calomel?—what its injurious effects? Are not its injurious effects, more often the result of its rash or injudicious use? Do its powers of injury, even in the hands of recklessness and ignorance, so far outweigh the good which it possesses, when in the hands of the prudent and skillful, as to require that it should be discarded? or, does the possible injury which may sometimes attend its judicious
administration, justify or require an abandonment of its use? And, are there not hundreds of other remedies, which are capable of inflicting far more serious and deadly injury, such as the lancet, drastic and hydragogue cathartics; emetics, mineral and vegetable; lobelia, tobacco, opium, and other narcotics and poisons, against which no hue and cry has been raised or condemnation uttered? To answer these questions properly, and others which would be equally pertinent to the subject, it will be necessary for me to draw upon such authority as will be less likely to be called in question; but before doing so, I will take the liberty of offering my own views, in order that they may have the benefit of the testimony which I shall introduce.

The therapeutic property of Calomel, upon which its great value depends, and indeed, the only one which I have been able to recognize, is its stimulant effect upon the secretory, and secerning vessels generally, having a special elective affinity for the salivary glands and the liver, but which may be extended to the other secretory organs, less impressible, by its combination with other remedies having such a tendency and direction. Calomel is unquestionably purgative, in the common acceptation of the term; but as such, its purgative effects are consequential to its stimulant action upon the secretory and excretory vessels, and ducts of the liver, and as the "sine qua non" to such action, it must first be taken into the general circulation, and as this result must depend in a great measure upon the state and condition of the absorbent vessels, so its purgative effects will depend upon the quantity and quality of the secretions which it causes the liver to throw out, which, sometimes being abundant and acrid, give rise to griping pains, and active vomiting and purging, which have too often been ascribed to the direct purgative and irritating effects of calomel. But my experience is, that calomel in the stomach and bowels, unabsorbed, is just about as inert and as unoffending as so much chalk; and that it is not only unirritating, but is one of the mildest and most soothing applications to an inflamed or irritable surface; and acting upon such a belief, I have often succeeded in obtaining the very best effects of calomel, by putting a sudden and effectual stop to obstinate vomiting, which had resisted the action of all other remedies.

In some of my former letters, I have endeavored to show the influence of atmospheric heat, and other causes, tending to render
the liver torpid, inactive, and liable to congestion; and I have
also shown, or called attention, at least, to the anatomical and
physiological relations which the liver sustains to all the other
abdominal viscera through the portal system of veins, which it is
necessary should be kept in view, in order that we may have a
better understanding of the action of calomel upon the liver, and
its value and importance as a remedy for the deranged conditions
of that organ, and all the organs connected with, and dependent
upon it.

I will first notice some of the peculiarities of calomel, in what is
recognized as its purgative action, and then its constitutional and
alterative effects.

The absorption of calomel when taken into the stomach, is ef-
fected chiefly by the radicles of the gastric branch and the superior
mesenteric branch of the portal vein, and this function is inter-
rupted always, and even suspended, when these vessels become
extensively congested, and in cases of high gastro-duodenal irrita-
tion, which is often the result of such congestion. In such a con-
dition of things, a certain amount only will be absorbed, before it
passes the reach of the principal absorbents, however much may
be administered; hence it is, that, in certain diseases and conditions,
large doses of calomel will pass through the entire alimentary
canal, without making the slightest perceptible impression, or un-
dergoing any change whatever; and hence it is, that large doses
do not purge as much, in proportion to the dose, as smaller
ones, even when the absorbents are more active, for as soon as
sufficient has been absorbed to excite the liver to throw off its
secretions, the residue will be soon carried beyond the reach of the
absorbents, and become inert: hence the necessity, even in large
doses, of combining with it opium, to prevent its too rapid pas-
sage, and thus secure its action upon the liver; or what, perhaps,
is sometimes better, a repetition of the dose.* The usual effect of

* A few years ago I was called to see a man, some twenty-five or thirty years
of age, who had been treated some short time before for what his physician called
Rheumatic fever, which was accompanied with an obstinate diarrhea, and which,
though he had recovered so far as to be able to go about, had never been arrested.
When I visited him he had been in bed two or three days, and was in the following
condition:—His countenance sallow, and hyperastic; his eyes wandering, with a
vacant expression; delirium; picking at the bed-clothes, and catching at imaginary
objects; jerking of the tendons. The skin hot and dry, and the pulse 140 to 150,
when he was at all excited, small and wiry; the tongue dry, with a dark fur in the
a full purgative, or a large dose of calomel, (by which I mean from twenty-five to thirty grains and upwards,) is to produce, after a few hours, one, two, and sometimes three, full and free fecal and biliary evacuations, when its purgative effects will cease for some hours, when it will re-commence in small and frequent and more biliary evacuations, which, though they evince the best action upon the secretory function of the liver, require to be checked at once by the use of opium, in order to prevent hypercatharsis and the consequences attending it, even though it should be necessary to re-establish its action by its re-administration, which is sometimes the case; but more often the action will return upon the subsidence

centre, and the edges red and shining. The urine scant, and frequent serous discharges from the bowels.

A review of all the circumstances of the case, led me to the conclusion that his safety depended upon the favorable action of calomel; and if I had then been acquainted with Dr. Jno. Reid's method of treating typhoid fevers with small and oft-repeated doses, "so as to change (as he says) the existing deranged condition of the biliary organs, and gastro-enteric mucous membrane," I perhaps might have adopted it; but as it was, being desirous of making an early impression, I chose twenty grain doses, to two grains, and accordingly portioned four powders of twenty grains each of calomel, and directed one to be given every four hours, which was strictly carried out. On the second day, perceiving little or no change, I left four other powders of twenty grains each, to be given as before, and so on for six days, making in all 480 grains, or just \( \frac{4}{3} \) of calomel. On the third day there was very little change—the pulse was less frequent, and had some more volume; the discharges less frequent and a little more consistent, or less liquid. On the fourth day there were evident signs of improvement—the pulse 130; tongue not so dry; less purging and less delirium. On the fifth day the pulse was from 115 to 120, fuller and softer; tongue and skin more moist; discharges reduced to two or three, leaden or slate color; can be aroused to consciousness, but still slight delirium. Sixth day: had one discharge, slate color, but more consistent; pulse about 100; almost, or quite rational, and other symptoms in proportion. Ordered a dose of castor oil to be given four hours after the last powder, which was given. Seventh day: pulse full and soft, at about 90; all traces of delirium and subsultus tendinum gone; tongue moist, and cleaning off; skin moist, pleasant and natural; countenance clear and cheerful; has had one large black or tar-colored biliary discharge from the bowels, and one smaller, which was less so; had some desire to take food, which I allowed, and directed, in case of any more purging, to give a teaspoonful or two of paregoric. Eighth day: called to see what damage had been done—found him fully convalescent, without the slightest trace of salivation, and I turned him over to the cook.

In about ten days he called at my office and introduced himself, (I did not know him,) paid his bill, thanked me for my services, and said that he never had a dose of medicine to do him as much good as the one I had given him, as it had cured him of a very troublesome bowel complaint, and he felt as well as ever he had done in his life.
of the restraining effects of the opium, with less tendency to hyper-
catharsis. For the cathartic, or rather the purgative action of
calomel on the liver, the larger are the safer doses, for the reason
already stated, that a certain amount only will be taken up, and
when the impression is more suddenly made, is more apt to excite
the exeretory vessels and duets, and cause them to be relieved at
once of their biliary engorgement; hence the large, but few, bilious
evacuations which follow its action in full purgative doses. By
using calomel in this mode, a double object is obtained, namely,
relieving the liver of biliary engorgement, and establishing the
biliary secretion, thus obviating the necessity for a continuance of
its use, and diminishing the risk of producing salivation, an ocurrence
which is generally a matter of temporary inconvenience and
discomfort, and of trivial consequence and importance, compared
with its great value in other respects.

There can be no doubt that mercury, slowly and gradually intro-
duced into the system, has a tendency more directly to the salivary
glands, and that endermically, and otherwise, slowly introduced,
it may excite salivation, without materially or perceptibly exciting
the secretory functions of the liver. Owing to this tendency, the
introduction of calomel by small and repeated doses, often fails in
producing its proper purgative effect; that is, it fails to disgorge
the liver of its bilious contents, unless—by the slow, tedious, disa-
greecable, painful, and dangerous process of washing them out by
a bilious diarrhea, and often fails to establish perfectly the secreto-
ry function of the liver, notwithstanding the existence of salivation,
which often occurs as the consequence of such a mode of adminis-
tration. In such a condition of things, when salivation has been
induced by repeated small doses of calomel, and the liver remains
unimpressed, which is not unfrequently the ease, the very best
effects of calomel may be obtained by its administration in a full
purgative dose, which will have the effect of relieving the liver of
its engorgement, establishing the biliary secretion, and relieving
the salivation. This idea may perhaps be horrifying to the uninini-
tiated, but it is nevertheless true, as I shall show by the relation
of cases illustrative of this, and other important points connected
with the subject.

With respect to salivation, I must say, that without being insen-
sible or indifferent to its occurrence, I have never attached
much importance to it, either for its good or bad consequences, for
the reason, that the salivary glands are not implicated and do not require the action of calomel in those malignant affections in which it is most serviceable on account of its action on the liver, and other dependent and important organs; and the existence of salivation does not, by any means, furnish evidence, conclusive, that those organs have been, or will be, impressed by it. And as to its injurious effects, my seeming indifference, no doubt, grows out of confidence, as I can truly say, that for the quantity of calomel which I have used during thirty years of practice, (which the reader may suppose has been no little,) I have salivated comparatively very few patients, and have never had one to suffer permanent injury, or any thing more than temporary inconvenience; and these favorable results I must ascribe, in a great measure, to the liberal manner in which I have used it, from having been early impressed with a similar idea respecting the action of calomel, that—

"Tender handed, stroke a nettle,
And it stings you for your pains;
Grasp it like a man of nettle,
And it soft as silk remains."

And having acted upon it, I can boast of having generally obtained the best effects of calomel, without the bad; and all the regrets which I have ever had in connection with its use have been from the restraints imposed upon me, by the unreasonable opposition and unfounded objection to its use, by the patient or his friends, in cases in which it was absolutely and imperatively demanded.

Having said sufficient for the present upon my own authority, I will now introduce such testimony as will corroborate my views, at least as to the value of calomel; and the first is the U. S. Dispensatory, by Wood & Bache, from which I will select such passages and make such comments as may seem best suited to my purposes, (referring the reader to the entire article for perusal.) Dr. Wood, who seems to have penned the article upon the properties and uses of calomel, says: "Whether the object is to bring the system under the general influence of mercury, or to produce its alterative action upon the hepatic or other secretory functions, calomel, on account both of its certainty and mildness is preferred to all other preparations, with the single exception of blue pill."

"As a purgative, calomel owes its chief value to its tendency to
the liver, the secretory function of which it powerfully stimulates. It is usually slow and somewhat uncertain in its cathartic effect, and though itself but slightly irritating, sometimes occasions severe gripping pain, with bilious vomiting, attributable to the acrid character of the bile which it causes the liver to secrete. It is peculiarly useful in the commencement of bilious fevers, in hepatitis, jaundice, bilious and painter's colic, dysentery, especially that of tropical climates, and all other affections attended with congestion of the portal system, or torpidity of the hepatic vessels.” Now, it does appear, to my mind, that this evidence is sufficiently clear and pointed in favor of the uses of calomel (particularly as a purgative) in the diseases of our climate, and the South generally, whose chief (if not essential) characteristic features are biliary derangements and complications, to convince any one of its value, who is not entirely carried away by his prejudices. But while I am laboring to prove the uses and value of calomel in such affections, it seems to be equally necessary and incumbent upon me to prove, that “congestion of the portal system, or torpidity of the hepatic vessels,” really constitutes any part or parcel of our malignant fevers, and other diseases of like character, which it has become fashionable for the opponents of the use of calomel to deny, which I can regard in no other light, than as a hollow subterfuge and a flimsy pretext for their opposition. Of this, I shall say something more hereafter, and so will proceed with my extracts.

“In large doses, calomel is supposed, by some, to act directly as a sedative, and with this view has been given in yellow and malignant bilious fevers, violent dysentery, malignant cholera, &c. The quantities which have been administered in such affections, with asserted impunity, and even advantage, are almost incredible. A common dose is one or two scruples, repeated every half hour or hour, or less frequently, according to the circumstances of the case. We have had no experience in this mode of administering calomel.” So says our author, but I have had considerable, as the reader may suppose, by reference to the case reported in a note to this letter.

But this case, though it presents several important and valuable points connected with the use of calomel, is not to be taken as an example of my ordinary mode of using it, as in this case, three or four times the quantity was used, to what I have had to use in any other case; for though my common dose is from one to two scru-
Holt's *Letters upon General Pathology.*

1856.

... and sometimes drachms, I seldom have them to repeat often. With regard to the sedative action of calomel in large doses, I can say this, that if the term sedative is used in the sense of some writers, *to allay irritation,* as a local remedy, I can testify most positively; but if used in the common acceptation of the term, to reduce the vital forces of the general system, and diminish the action of the heart and arteries, I have never observed any such action. In proof of the non-irritant properties of calomel, whether sedative or not, the following testimony, furnished by a note to Dr. Williams' Principles of Medicine, is very satisfactory; it is this: “He who is familiar with the effects of mercury, calomel, for instance, in common disordered digestion, with some febrile irritation, and in gastritis, cholera and dysentery, can hardly deny its soothing and sedative operation, fully as manifest as, and in some of these cases, more procurable than from opium. Its constitutional effects, short of *ptyalism,* are often sedative and enfeebling in the extreme.” Now, the constitutional effects spoken of in the last sentence, which I have often observed to an extent requiring the use of opium and brandy, I have been in the habit of regarding as an evidence of the best effects of calomel upon the functions of the liver. That these sedative and enfeebling effects are the result of the passage of the pent-up products of the liver, through the intestines, which being thrown off, these effects subside, generally leaving the system under the constitutional effects, “short of *ptyalism.*” And Dr. Williams himself says, “This remedy (calomel) has no further sedative effects than those which proceed from its action on the intestinal canal,” which can have reference only to its local effects, or its sedative effects as a purgative, in the manner before stated. But let us go on with the Dispensatory: “When the stomach or bowels are very irritable, as in cholera and diarrhoea, from an eighth to a quarter of a grain may be given every hour or two.” With a view to salivation, the dose is from half a grain to a grain three or four times a day, &c. “As a purgative, from five to fifteen grains, *or more* may be given. Calomel has the peculiarity that its cathartic action is not increased in proportion to the dose, and enormous quantities have sometimes been given with impunity. In yellow fever, tropical dysentery, &c., from twenty grains to a drachm have been given, and repeated at short intervals, without producing hypercatharsis (see note); but this practice is justifiable only in cases of extreme urgency, in which the constitutional ac-
tion of mercury, as well as purgation, is indicated. For children, larger doses are generally required, in proportion, than for adults.”

Now, my experience is, that in cases of irritable stomach and bowels, and it is necessary to use calomel, the large doses are preferable, for the reasons already given. As an alterative, in chronic cases, where the action of mercury is desirable, there can be no question that its gradual introduction by small and frequently repeated doses, is the most certain (if not the most speedy) method for exciting salivation; or, if it is desirable to procure its antiphlogistic effects, by the power which it is believed to possess of diminishing the quantity of fibrin in the blood, and of removing the deposits of lymph and other products of inflammation, for which properties it is held in most repute in Northern latitudes, the objects will no doubt be obtained by such a mode of its administration. But it is a very different affair with the acute and malignant forms of disease in the South, in which there is supposed to be rather a deficiency of fibrin than otherwise, and consequently no such action of mercury is required, or if it was, they are often so rapid in their progress, that the patient dies before the inflammation has time to kill him, an expression which will be explained, if it is not now understood.

Although such cases are uncontrollable by calomel, in what are termed alterative and salivating doses, from the want of time, if nothing else, I have generally found the most malignant of them amenable to large doses of calomel, when it was used before the nervous system had become too much depressed, or before the healthy or sound constitution of the blood had been broken down, not, however, from the power of calomel directly to raise excitement and increase the general vital forces, or to change suddenly the constitution of the blood, but from its certain tendency to the liver, the secretory function of which it powerfully stimulates, whereby “the congestion of the portal system, and the torpidity of the hepatic vessels” are removed, dependent and congested, irritated and inflamed organs, relieved and restored to the performance of their functions of absorption, secretion, excretion, &c., whereby the effete and poisonous matters which have accumulated in the system in consequence of those suspended functions, are thrown off, and continue to be, as fast as they are eliminated, thus preventing the depravity and breaking down the constitution of the blood, and consequent prostration of nervous power, which gives such
malignancy to the diseases in question, and often, as I have said, destroys life, before any process of inflammation could accomplish it.

In confirmation of my views respecting the part which the liver plays in the malignant diseases of warm climates on account of the congestion and suspended functions to which it is liable, as well as the value which attaches to calomel in the treatment of those diseases, on account of the certainty with which it acts upon that organ, and others that stand in intimate relation, nothing could be more satisfactory or conclusive than is to be found in a few sentences in "Wood's Practice of Medicine," where this distinguished author, speaking of the treatment of dysentery, says: "But another important object in the use of purgatives is to unload the portal veins. The capillary circulation in the liver is often sluggish, and in many instances the secretion of bile appears to be suspended. Blood, therefore, accumulates in the veins, proceeding from the abdominal viscera, and must press injuriously upon the capillaries of the bowels. By stimulating the hepatic circulation and secretion, we remove this evil. Hence one great advantage of calomel." Here I would stop, if the next sentence did not contain an important fact for which I have contended respecting the action of calomel, which applies not more to dysentery than to other diseases: "This is indeed one of the most useful cathartics in dysentery, having the advantage of mildness in its action on the mucous membrane, while it excites the liver." But the consequences of such a condition of the liver are not less injurious, or is calomel less efficient in diseases of the stomach and smaller bowels, the spleen and pancreas, and the liver itself, all of which are in closer proximity and as likely to be effected by pressure from the distended portal veins, which it is important to unload as in dysentery. It is not, however, the congested condition alone of the liver which gives to our diseases the character of their greatest malignancy, but it is the consequences of the suspended secretions which usually attend that condition, but which may exist without the congestion, which generally gives to our diseases their dangerous and too often fatal character, by poisoning and breaking down the constitution of the blood from the non-elimination of the impurities generated in the system, as is the case in our worse forms of bilious remittent fever, yellow fever, dysentery, pneumonia, &c.—and in all of which calomel is not less prompt and certain (if properly used) in
restoring the suspended secretions, than in *unloading the portal veins*. But the beneficial effects of calomel upon the liver are not confined to the chylipoietic viscera, especially so far as concerns the restoration of the secretions. Dr. Chas. I. B. Williams tells us that (p. 243)—"The congested state of most organs, which occurs when the respiratory process is imperfect, renders necessary remedies suited to remove this state. The lungs, the brain, and the liver, suffer most. *The best remedies in these cases are mercurial, and other remedies which act freely on the secretions. Probably these remedies act in part by making the liver assist the lungs in the office of decarbonizing the blood. The speedy relief afforded to dyspnea by a bilious diarrhea, has several times seemed to me to countenance this notion.*" Now, what Dr. Williams here states as "probable," I know to be positively true; for having myself been afflicted for more than twenty years with asthma, I have never failed of being relieved very soon by the occurrence of a bilious diarrhea, which often comes to my relief, and puts a stop to a violent paroxysm. And who is there, who has had any experience in the treatment of bilious or congestive pneumonia, that has not seen the most prompt and decided relief from free, bilious purging, from the use of calomel? Indeed I can say, to the best of my recollection, I have never lost a patient with pneumonia, where such a result was early obtained; and, in this disease (in cases attended with torpor and congestion of the liver), I have always regarded calomel in the light of a life-preserving, if not a disease-curing remedy.

As the space allotted to my letters will not allow me to go on, having already reached the usual length, and having a desire to say something more upon the subject, and to present a few cases illustrative and confirmatory of my views respecting the uses of calomel, I will stop for the present, and resume the subject in my next letter.

Remaining yours, as usual,

SAML. D. HOLT.

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**ARTICLE XIII.**

*Lithotrity in the Female.* By L. A. Dugas, M. D., &c.

Since the perfection of the instruments for crushing stones in the bladder, and the extension of their use, there must be but few surgeons who would consider themselves excusable for resorting to the knife in calculous affections of females, unless circumstances
of a peculiar character rendered lithotritv impracticable, which is very rare. Indeed, the shortness and great dilatahility of the female urethra, very materially facilitate the introduction of the lithotrite and the manipulations subsequently necessary to the seizure and crushing of the stone; so that the cases in which this operation cannot be performed are infinitely more rare in females than in the other sex.

In May last, (1855) Miss E. H., about 8 years of age, was placed under my charge. She had been a sufferer with stone from her infancy, and was in a truly deplorable state when she arrived here. Very much emaciated, sallow and hectic, she suffered excruciating paroxysms of pain almost every half hour, during which her screams were heart-rending to her friends. Incontinence of urine and prolapsus ani added to her discomforts. Upon endeavoring to sound her, the slightest contact of the instrument with the orifice of the urethra would bring on a paroxysm of pain. Under these circumstances, she was placed under the influence of concentrated chloric ether, and a careful examination was made without difficulty. This confirmed her own belief, by revealing the existence in the bladder of two calculi, which could be felt by placing the instrument between them so that they could be struck alternately by inclining the blades to one side and to the other.

A week was consumed in quieting and preparing her for the operation, and on the 29th of May, in the presence of several professional brethren, having secured the limbs with bonds, as for lithotomy, and administered the chloric ether by inhalation, a calculus of one inch and a quarter in diameter was seized and crushed. The crushing was done five times at this sitting.

7th June. Has had no severe paroxysm of pain since the operation. Has passed a large number of fragments, say about a teaspoonful. Has had cholera-morbus twice from improper food, which reduced her very much. The crushing was this day repeated six times, the largest stone seized measuring one inch and the smallest half an inch in diameter. The etherization was only partial, as she conversed during the operation, but felt no pain.

14th June. Has passed a quantity of fragments, most of which were lost in the alvine discharges, which became frequent and dysenteric, attended with a great deal of fever and some cough. An attempt was made to operate to-day, but she was so much exhausted that it was not completed; yet the bladder did not partake of
the general disturbance, and her paroxysms of pain had pretty much ceased.

21st June. Finding her health pretty much improved, the crushing was done to-day six times without inconvenience; the fragments seized were all small.

26th June. Passed fragments and detritus for two or three days after the last operation, but none for a few days past. Introduced the lithotrite to-day, but could find no calculus.

29th June. Explored the bladder carefully with the lithotrite—and finding no vestige of stone, discharged the case. The patient returned home in a few days—and has ever since enjoyed fine health, as I have been recently informed by the family physician.

The history of this case illustrates the happy influence of anaesthesia in such operations, for the apprehensions of the little patient and her excessive nervous susceptibilities were such that it would have been utterly impossible to operate without it. The chloric ether was therefore used at each sitting. Bonds were used to prevent accident.

The calculi were composed of ammonio-magnesian phosphate of lime.

-Augusta, Ga., 1st March, 1856.

Remarks on Uterine Affections, based on the Discussions of 1849 and 1854 at the Academy of Medicine of Paris. By Camille Melchior Gibert, Physician to the Hospital St. Louis, Secretary of the Academy of Medicine, etc., etc. (Translated from La Revue Médicale.)

Twenty years ago, Lisfranc adopted and extended a theory already abandoned by his master Dupuytren, by attributing to Chronic Metritis every symptom peculiar to woman almost, and making this morbid state the starting-point and origin of cancerous affections of the womb. It was an easy matter, consequently, for Lisfranc, to prevent malignant uterine disease, or even to arrest it in its early stages.

The popular applause with which his lectures were greeted, and the crowd of terrified women which sought at his hand the cure of diseases that others had rightly pronounced incurable, turned Lisfranc's head. He carried a list of his achievements to the Institute. He had the temerity to ask for the chair of Dupuytren, and the Monthyon Prize. He declared that amputation of the neck of the uterus, as the prophylactic or curative treatment of cancer, was one of the most brilliant conquests of modern surgery.
Two years had scarcely elapsed, after the presentation of this academic memoir,* when another surgeon, a pupil of Lisfranc, and an assiduous and impartial witness of his practice, struck down the pretensions of the great operator, in a book which made a great sensation in its day. I will leave the ashes of the dead in peace, and refer the curious reader to M. Pauly's work † for details of that scandalous episode in modern surgical history. I will only mention here, that Lisfranc himself abandoned his brilliant operation soon afterwards, that he withdrew his memoir to the Institute, and that he never replied to Dr. Pauly's work.

Still the surgeon of La Pitie continued, in his lectures and practice, to base the special pathology of women on Chronic Metritis and its results: granulations, engorgements, ulcerations of the neck, congestions of the body of the uterus.

In 1837, when I first endeavoured to convince practitioners by a multitude of clinical facts, ‡ of the abuses which had been brought about by a theory based on an application to uterine diseases of the system of Broussais, attention was especially directed to ulcerations of the cervix uteri, and it was this lesion more particularly that was supposed to be prevented or cured by various topical remedies, especially by repeated cauterizations.

I do not hesitate to reproduce here the plain language which a just indignation then extorted from me; for these are all matters of history:

"It is sad to reflect that in the era of industrialism and moral degradation in which we live, some men of real attainments, who occupy stations which command the public confidence, do not fear to descend to the level of those healers of ulcers and the whites, whose putrescent advertisements sully the columns of the public journals, or offend the public morality in street-corner placards."

"There are to-day, in this capital, a multitude of lazy or hysterical women confined to bed, subjected to a useless regimen, and wrongfully harassed by explorations, caustic-applications, or operations designed to cure diseases which have no existence!"

I will recall also as a starting-point in any discussion as to the value of the uterine lesions revealed by speculum-examinations, two assertions that I advanced in the Academy without a word of contradiction.

1. The different alterations and variations in colour, size, form, texture, and position of the neck of the uterus, which certain surgeons have incorrectly regarded as the origin and exclusive cause of a multitude of morbid symptoms, may exist, and be clearly ascertained to exist by the touch and

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* In 1834.
‡ Maladies de l'utérus, d'après les leçons cliniques de M. Lisfranc, faites à l'hôpital de la Pitie. Par H. Pauly, ex premier prosector de M. Lisfranc, etc. Paris, 1836, 1 vol. 8vo. Germer Baillière.
speculum-examination, without giving rise to any of those symptoms which have been ascribed to them.

2. That the symptoms which Lisfranc, in particular considered the necessary concomitants of uterine engorgement (as others, at the present day attribute them to uterine deviations) are observed in cases in which nothing is found of those local alterations that are supposed to be their source.*

So that it is demonstrated, to my satisfaction at least, that on the one hand, these material lesions are far less important than has been asserted; that sometimes, indeed, their existence produces no inconvenience; and, on the other hand, that the various pains, the local discomfort, the nervous sufferings, observed in many women condemned to Lisfranc's treatment, should be referred to a very different cause from the one which was thought to indicate the necessity for that sort of practice.

In Dr. Pauly's book may be learned the extent to which errors in diagnosis and abuses in practice were carried in those days. The neck of the uterus was not unfrequently amputated when perfectly healthy, and the appearances due to incipient pregnancy were mistaken for morbid lesions!

And it must not be fondly thought that in our day, when Lisfranc's treatment is no longer popular, that these errors are not committed. Although the majority of surgeons have been brought to confess that ulcerations of the cervix are commonly mere temporary alterations, connected with uterine catarrh, and of little importance in themselves, they still treat these unimportant lesions by applications of caustic, and not unfrequently some of the most eminent of their number have pronounced women affected with ulcers of the womb, (and what alarm this phrase excites in patients' minds!) whose wombs were not ulcerated at all, but simply presented at the os tincæ a redness caused by an uterine discharge and following its phases, or even a permanent scar, or spot, such as is left occasionally after the process of parturition.

It cannot be repeated too often that the uterine lesions revealed by the speculum depend on two principal causes: 1. Syphilis (primary and consecutive venereal ulcers, depressed and circumscribed granulations of the cervix, the lesions resulting from blennorrhagia); 2. Uterine catarrh or leucorrhœa, which may accidentally be accompanied by redness, irregular granulations, superficial erosions, passive congestions of the neck, engorgements, etc. In the latter group there are two important divisions; sometimes the leucorrhœa is only the symptom of a general condition or diathesis, (whether strumous, gouty, or dartrous,) that demands the physician's chief attention†; sometimes on the contrary, the local catarrh assumes

* Bulletin de l'Académie de Médecine, Tome XV. p. 147.
† Many of our patients at St. Louis with eczema and impetigo are subjected to discharges which have their periodical exacerbations and remissions corresponding to dartrous (psoric) crises. Oftentimes redness and excoriations of the cervix uteri, which are but temporary, supervene on these exacerbations.—Note of the Author.
a predominant importance, and becomes the source of general and local nervous symptoms which require topical treatment from the first, treatment consisting chiefly in free injections of cold water (douches ascendantes) and astringent washes.*

While opposing the exaggerations of the theory and practice of his master, and especially the useless amputations of the uterine neck, Dr. Pauly retained the teachings of La Pitié in relation to the local treatment of engorgements, which Lisfranc regarded as the climax of uterine pathology.

It was reserved for another pupil of Lisfranc to reduce to their real value the assertions which the surgeon of La Pitié advanced on this subject, with so much confidence.

Dr. Baud undertook in a memoir addressed to the Academy of Medicine in 1849, to establish that the uterine affections on which Lisfranc particularly fixed his attention had their source, almost all of them, in a constitutional affection, of which the local lesions were simple consequences. This author maintained that Lisfranc beguiled by the errors of the Physiological doctrine, had made inflammation the origin of all the chronic diseases of the uterus, and engorgement their constant result. (And many a surgeon in our day has got no farther.)

On the other hand, Dr. Baud, attaching great importance to certain deviations of the womb, regarding them as the causes of most cases of dysmenorrhoea and sterility, discovered in one particular displacement (anteversion, which he proposed to remedy by a special instrument) the source of a multitude of disorders, of engorgements among others. Engorgements were thus made a secondary local lesion, altogether refractory to the treatment for chronic metritis.

M. Hervez de Chégoir regarded the opinions of M. Baud as too general and too exclusive. He attached as much weight to retroversion as the author of the essay did to anteversion.

The conclusions of Dr. Baud's memoir are remarkable, and they agree so closely to some of my own assertions, that I shall quote them literally. They are summed up in the three following general propositions:

1. All treatment which ameliorates the condition of the functions in general is one step towards the cure of uterine diseases.
2. All treatment the effect of which is to benefit the local disorders at the expense of the general economy aggravates these diseases.
3. The treatment should be in accordance with the idea that the condition of the uterus is passive and mechanical.

This last proposition might serve for an epigraph to M. Valleix' recent cases; but let me not anticipate. The memoir of Dr. Baud

*See my Memoir on the Treatment of Leucorrhoea (Rev. Méd. April, 1845;) and remarks on the Therapeutics of Diseases of Women, (Bulletin de Thérapeutique, January, 1848.)—Note of the Author.
and the remarkable report of M. Hervez de Chégoîn, excited a discussion in which many distinguished surgeons expressed their views.

The day the report was read, I could not forbear remarking that I had opposed for twelve years past the practice which some modern surgeons adopted, in consequence of ascribing an undue importance to the uterus, and attending solely to local symptoms, a practice pregnant with errors and abuses. How much wiser was the theory and practice of the enlightened and conscientious savants of the last two centuries, who considered that the non-gravid uterus played a subordinate part in the economy, and regarded that organ as a sort of emunctory, whose fluxes and congestions usually depended on some cause affecting the system at large! Long since, I proposed to substitute for caustic and the antiphlogistic treatment in vogue, the use of astringents, cold, injections of cold water, sitzbaths, etc., combined with such means of support or readjustment as the mechanical displacements of the uterus might indicate.*

At the following meeting, I had occasion, in reply to different speakers, to review some of the assertions made in the discussion, many of which were contradictory, and could prove were it necessary, how anatomical diagnosis may lead the prejudiced into error, notwithstanding the perfection to which it has been brought, and that its advocates are pleased to term it the only true foundation for rational practice! Thus while Lisfranc proclaimed engorgements the mother-lesion of the disorders peculiar to women, his disciple Dr. Baud considered them a secondary result, and M. Velpeau went so far as to doubt their existence. But, by way of compensation, the professor of La Charité announced the frequency of an inflexion of the neck, the source of a great number of symptoms, a morbid state which many eminent surgeons, MM. Moreau, Malgaigne, and Roux, among others, declared they had never seen. Then came M. Robert, seeking to extricate ulcerations of the cervix from the kind of discredit into which they had gradually declined, and ascribing to granulations of the uterine meatus all those symptoms which Lisfranc referred to engorgements. Lastly, while the author of the memoir considered anteversion as the commonest and most serious deviation, the author of the report maintained that retroversion was the predominant lesion. If we consider, also, the discrepancies of opinion in regard to anteflexion and retroflexion of the cervix, we shall form some idea of the want of unanimity not only in regard to the value of the diagnostic signs of uterine disease, but in respect to the very existence of these signs.

For my part, I am quite disposed to admit, with MM. Lisfranc, Jobert, Malgaigne, Roux, and Huguier, that chronic engorgements of the cervix uteri exist; but I regard them almost invariably as the effects of a lymphatic diathesis, the consequences of parturition, or

the complications of leucorrhoea. When, under such circumstances, local and general morbid processes are developed, these processes being nervous and congestive rather than inflammatory, will not yield, as a general rule, to the antiphlogistic treatment advised by Lisfranc, nor to cauterizations of the neck of the uterus. In addition to the sedative and astringent local treatment I have mentioned, I insist on the necessity of general treatment adapted to the lymphatic, herpetic, gouty, or syphilitic diathesis, which frequently keep up discharges from the uterus, and prevent the cure of the engorgements, granulations, and ulcerations revealed by the speculum.

On the other hand, I am altogether inclined to recognize, with MM. Velpeau, Moreau, Huguier, Hervez de Chégoïn, and Baud, uterine deviations, as the source of local symptoms, provided they are sufficiently grave and lasting to constitute real diseases. But I also admit, with MM. Jobert, Huguier, and Moreau, that there are abnormal states that are quite harmless, which are sometimes improperly treated as morbid conditions.

As to M. Amussat's opinion, which makes no account of the notions our predecessors have left us, and dates the commencement of the science of uterine affections from the application of the speculum, I reply that he forgets two things: 1. That the errors in diagnosis exposed in the discussion also date from the invention of that instrument; 2. That the principal object of that debate was to pass judgment on the special pathology and therapeutics which Lisfranc and his imitators substituted for the history of Leucorrhoea or uterine catarrh, as described by the physicians of the last century and the commencement of the present.—I leave it to the profession to decide whether that judgement has been advantageous of the modern theory and practice. As to those moral influences, which were so little regarded in the surgical discussion, which nevertheless present such curious phenomena in all the diseases of woman, and especially in those women whose imaginations are excited by the fear of what they term an ulcer, I have already described their effects, and presented them to the attention of practitioners; * and I purpose hereafter to return to this interesting subject.

I come now to the late discussion, excited by a report of M. Depaul on various documents relating to the treatment of uterine displacements by Professor Simpson's intra-uterine pessary, which M. Valleix adopted and introduced in France.

The extended and judicious report of M. Depaul terminates with a series of conclusions, some of which I will quote in substance.

"1. The influence of uterine deviations on the health of women has been considerably exaggerated."

I extend this proposition, and apply to it all the other local lesions.

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* See my article on the neuroses in the March number of the Revue Médicale, for 1840; the report of M. Jolly, in the number for September, 1843. Lastly, the number for January, 1848, of the Bulletin de Thérapeutique.—Note by M. Gibert.
which have been successively brought forward as the causes of nearly all the discomforts experienced by women, (ulcerations engorgements, granulations.)

"2. In many cases, the symptoms referred to these deviations, are produced by some other pathological state of the uterus."

I suppress the words: of the uterus; for in many women treated as if affected with some lesion of that organ, the uterus presents no morbid alteration that can be appreciated by the senses; in some cases even, moral causes alone produce the symptoms.

"3. Deviations (within a certain limit and under certain conditions) constitute only a simple unimportant deformity, which has no deleterious influence on the health."

"4. There are some deviations, however, though they are infrequent, which appear to induce serious disorders, and to require the intervention of art."

"5. Those who have attributed the ameliorations and cures of a large number of women to the use of the intra-uterine pessary, have overlooked the fact that, in these cases, other means were employed, whose well known action (in chronic phlegmasia, or a neuralgic condition) explain the results which were obtained."

"6. The cases communicated by MM. Broca and Cruveilhier to the Academy, and other cases observed in France and England, demonstrate only too clearly the danger attending the employment of the intra-uterine pessary."

"7. We therefore think that instruments of this kind (from which we carefully distinguish the hysterometer, or uterine sound, an exploring instrument applicable to certain exceptional cases) should be condemned and proscribed, for they are useless, being incapable of producing the effects expected of them, and exposing patients to the most serious dangers."

Many of the orators who participated in the first debate, engaged in this, sustaining, with some modifications, the opinions which they advanced in 1849, in regard to the general question, and examining the points which were more especially the object of the report. One of them, M. Malgaigne, who had previously argued against M. Velpeau's views on inlusions of the cervix, admitted that a more attentive study of the subject and an extended experience, had changed his opinions in this particular.

I shall briefly review the principal topics embraced in the discussion which followed M. Depaul's report, commencing with the speech of M. Malgaigne.

The orator referred, in the first place, to the discussion on the nature and treatment of uterine affections, which took place five years ago. "The difference of opinion which were then manifested still prevail; what some attribute to uterine displacements, others refer to engorgements, to metritis, uterine catarrh, or simple neuroses, while others again discover in some general condition of the organism, the source of every local disorder. What is now required according to M. Malgaigne, is exactitude of diagnosis, which may enable us to connect certain symptoms with some precise
species of alteration, and to get rid of the confusion which involves those who see the same kind of lesion in simple and complicated cases, and employ the same treatment in all. Having directed his studies with this view, M. Malgaigne had arrived at the conclusion that the uterine affections under discussion might be distributed in three principal divisions, viz:

1. Displacements, (prolapsus, anteversion, anteflexion, retroversion, retroflexion.) The principal diagnostic sign is found in the effects of the vertical and horizontal positions of the body, which suddenly and completely aggravate or relieve the patient’s sufferings.

2. Chronic Metritis of every variety, (engorgement, catarrh, granulations, etc.) The cervix is painful when touched, and the recumbent posture (which is necessary for a cure) only moderates the pain, without relieving it altogether.

3. Neuroses, or neuralgæ, which are observed under two forms: neuralgia of the cervix, and neuralgia of the appendages of the uterus.

M. Malgaigne considered M. Depaul’s condemnation of the intrauterine pessary too absolute. It was proper, in the first place, to make a distinction between simple catheterization of the uterus, which was almost exempt from danger and the employment of an intra-uterine pessary. It was also necessary to take into account, the rare and exceptional cases in which mechanical treatment, by the pessary, redressor, or hypogastric girdle, have produced sudden and instantaneous relief, and also those yet more remarkable examples, in which the simple contact (attouchement) of an instrument with the cervix uteri has sufficed to dissipate the symptoms.

M. Malgaigne’s general conclusion is, that the intra-uterine pessary should not be condemned until it has been thoroughly tested, by being used with prudence and precaution in those cases in which diagnosis clearly indicates the necessity of a redressing instrument.

It is obvious that this is an incomplete view of the subject. In his three grand divisions, the orator makes no mention of constitutional causes or diathesis. Does he not admit, with us, that syphilitic, strumous, gouty, and dartrous diathesis are the sources of catarrhs, erosions, and engorgements, which cannot be considered mere local lesions of the uterus? What value does he attach to the influence of the moral on the physical faculties, by which morbid symptoms are so readily engendered in women? What are we to understand by metritis, if congestions and fluxations, passive and inflammatory engorgements, discharges, ulcerations, and divers specific lesions are classed under this term.

M. Depaul replied to M. Malgaigne; he did not, however, approach the main question, but insisted on this fact, which, he maintained, was established by incontestible statistics, viz:—

That a great number of uterine displacements produce no symptom whatever; and that, in many other cases, when concomitant lesions are cured, the symptoms erroneously attributed to displacement disappear of themselves.

M. Huguier came next, and by a precise and logical argument attempted to prove, from anatomical, physiological, hygienic, and
pathological data, that some displacements (anteversion, retroversion, inflexion, anteflexion and retroflexion) were the direct causes of special disorders of the functions of the pelvic viscera, and that oftentimes they could be rationally treated and cured.

He believed that latero-flexions and latero-versions usually depended on irremediable anatomical anomalies, but that they usually produced no inconmodity; the same thing nearly was true of anteflexions, (the very deviation that M. Velpeau had pronounced, in the former discussion, the cause of the symptoms usually ascribed to so-called engorgement.) The retroflexions and retroversions, direct causes of sterility in some cases, involved difficulties, M. Huguier said, that it is not easy to remove. Oftentimes, however, he had succeeded with his fingers, aided by an uterine sound, and a bougie, or pledges of lint in the rectum, in reducing or partially rectifying such deviations. Of all displacements, anteversion could be treated most advantageously, by an hypogastric bandage, by astringents, and appropriate decubitus. Not unfrequently an abnormal mobility of the uterus produced unpleasant symptoms, and in these cases the hypogastric belt was particularly efficacious.

M. Hervez de Chégoïn followed M. Huguier, and, in his turn, maintained the practical importance of displacement as an element of uterine disease, and discussed its treatment by mechanical means.

M. Hervez established three principal classes of deviations: anteversion, retroversion, and prolapsus. He declared that he had often relieved distressing general symptoms, unaccompanied by local symptoms, (which practitioners would consequently hesitate to refer to their true source,) by replacing the uterus by means of a special extra-uterine pessary. He formerly denied that deviations were harmless, as has been maintained by some surgeons, who mistook the exceptions for the rule. He was positive that displacements produced sterility and a variety of serious symptoms, which could only be relieved by a mechanical treatment. He advocated his own pessary, which was innocuous, and pronounced the instrument of Simpson irrational and dangerous. In consequence of its communication with the peritoneal cavity, he considered all irritation of the cavity of the uterus unsafe.

M. Paul Duboïs replied to M. Hervez, expressing his disbelief in the efficacy of the method of reduction employed by the letter. He though that replacing the uterus by acting on its internal walls was a rational proceeding in certain cases. The question was, could it be modified so as to avoid the dangers that now attend its use. In 1849, M. Duboïs pronounced an extended discourse on the whole subject of uterine affections, in which, although he attached more importance to the etiology of chronic metritis than I do, yet, the distinctions he established and the reservations he made, approximated his views much nearer to mine than to those of any other of the speakers. It was at the meeting of the 10th of June that the judicious and eminent professor replied to M. Hervez de Chégoïn. On June 27th, he resumed, and took up the subject of uterine displacements and their treatment.

As in 1849, M. Duboïs attributed primary importance to chronic metritis,
and he ascribed to this complication, like the reporter, M. Depaul, the majority of those symptoms which others made to depend on displacement. He believed that most of the patients suffering from these symptoms, even those successfully treated by the Simpson Valleix method, were cured by the removal of the co-existing chronic uterine inflammation. By temporarily replacing the womb, and keeping it in a state of immobility, time was afforded for the resolution of the concomitant metritis. The orator admitted also, that an intra-uterine instrument might favorably modify a state of local nervous irritability which existed in many women. He believed that the intra-uterine instrument was applicable in the exceptional cases in which the gentler means in common use, antiphlogistics, sedatives, astringents, cauterization, pessaries, etc., have failed. He observed that in consequence of disordered menstruation depending on uterine lesions, a state of chloro-anæmia was often induced, which only yielded to general treatment. He had two grievous faults with which to reproach the intra-uterine redressor, regarded as a special method of treatment: 1. It caused pain and inflammation which might become fatal; 2. It did not procure a durable reduction or replacement of the deviated or displaced womb. Operators were utterly deceived on this point.

M. Cazeau objected to ascribing to an hypothetical metritis the symptoms observed in women palpably affected with displacements or engorgements. He denied the existence of chronic metritis, as far as related to the parenchyma of the womb, (imitating the scandalous incredulity of M. Velpeau, who in 1849, denied engorgement, and substituted therefore, anteflexion.) M. Cazeaux admitted with MM. Huguir and Hervez de Chégoïn, that prolapus, retroversion, anteverision, and even anteflexion, were direct causes of symptoms remediable by mechanical means. He thought that the reporter, M. Depaul, had strikingly displayed the dangers of the intra-uterine pessary, but had not appreciated its advantages. M. Cazeau agreed with Professor Dubois in thinking that cures might be obtained, in exceptional cases, by the prudent and careful application of this instrument, which were not attainable by any other method.

I avow that MM. Dubois and Cazeau appeared to me too lenient towards the intra-uterine instrument, restricted as are the advantages they attribute to it. M. Gillebert d'Hercourt, of Lyons, has demonstrated in my opinion, by his experiments on the dead subject, the superiority of pessaries of vulcanized caoutchouc, which can be inflated after introduction, over all other mechanical means for the adjustment of uterine displacements.* At the meeting of July 4th, the discussion was continued by Professor Velpeau.

He opposed the opinions of his colleague M. Dubois. He believed that the uterus could be replaced by the Simpson-Valleix instrument, or by any analogous intra-uterine bougie carefully applied and properly superintended; and a radical cure was attained thereby, whereas pessaries and hypogastric belts are palliatives which must be habitually and indefinitely employed. He had often used similar instruments, and had acted on the internal surface of the uterus by catheterism, injections, cauterizations, etc.,

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* M. Gillebert's researches are published in a pamphlet (Etudes sur le mode d'action des pessaires, 8 vo. Lyons, 1854) which we have been unable to obtain.——O.
so frequently, that he was inclined to think the dangers of M. Valleix's
method were grossly exaggerated; the method, moreover, could be modified
and perfected. M. Valleix's results were remarkable and deserved at-
tention. The three fatal cases which have been so bruited abroad should be
compared with the very numerous cases in which intra-uterine redressement
had been successfully employed. The orator regretted that M. Depaul had
not discussed the fatal cases reported by MM. Broca and Cruveilhier,
instead of making a condemnation of the method of M. Valleix the special
object of his report.

M. Velpeau entertained doubts in regard to the neuroses and phlegma-
sie invoked by some of his colleagues in explanation of the obscure points
in this question. He referred to the frequent blunders to which Lisfranc's
engorgements had given rise, and insisted that the anteflexions and retro-
flexions, to which he had first called attention, were the sources of chara-
teristic local symptoms. These displacements were not always curable;
but hypogastric bandages, pessaries of different forms, and intra-uterine
sounds and redressors were resources of art that were sometimes employed
with complete success, after the failure of all other remedies.

This debate, like the one that preceded it, has proved that the
greatest diversity of opinions may exist among the most eminent
and enlightened men in regard to material lesions, which are acces-
sible to vision and to the touch.*

Uterine displacements, which were barely mentioned in the dis-
cussion of 1849, and which many surgeons at that epoch, contested
or altogether denied, at least as a pathological species, with a char-
acteristic array of symptoms; these uterine displacements were the
turning point of the discussion of 1854.

Now if we recall the predominant importance which surgeons
have, in the last 30 years, successively assigned to chronic metritis,
to ulcerations of the cervix, to engorgements of the neck, to granula-
tions of the internal surface, and lastly, to deviations, we may safely
predict that the latter lesions will not long enjoy their "bad emi-
nence" in uterine pathology. If, on the other hand, we reflect on
the serious and disastrous consequences which have resulted from
the practical application of these wild etiological theories, we shall
appreciate the value of words of caution promulgated by an esteem-
ed and enlightened Academy.

* It is curious to see how certain aristarchs, with the arrogance and imperturba-
ble impudence engendered by the habit of being eloquent in the closet, have under-
taken to lecture the orators of the Academy, and to assign subjects for their expres-
sions of opinion.

"No one [if we believe these gentry] has laid down the true basis of the discus-
sion... To make displacements of the womb and their treatment by the intra-
uterine pessary a simple practical question, is [in their judgment] the way to leave
the Academy as much in the dark as before the discussion."

O profound logicians, eminently positive philosophers! I think that you might
learn a lesson of modesty in these various appreciations and interpretations of mate-
rial and positive facts, elicited from the learned and skillful men who engaged in the
discussion which you regard as so unphilosophical.—Note of M. Gibert.

This indignant growl is intended for Dr. Amedée Latour, editor of the *Union
Médicale.—Translator.
When we see great surgeons so possessed by illusive hypotheses, as to propose and practice amputation of the uterine neck in women with no serious lesion, or no disease whatever of the uterus;—when, in other instances, incipient pregnancy has been mistaken for grave uterine disease requiring the most energetic treatment;—when, over and over again, we observe patients subjected to cauterizations, pessaries, bandages, protracted decubitus, and the remedies of a whole special therapeutical arsenal, who continue to suffer until they abandon their unfounded apprehensions, and abstain from all surgical treatment; it is impossible to deny that flagrant abuses have been introduced in the treatment of diseases of women, abuses which it is the right and duty of academic bodies to signalize.

To caution the learned and skilful against their own delusions; to enlighten the ignorant who accept, with blind confidence, the theories promulgated by the former, and practice whatever is advised by men of eminence;—lastly to admonish the public at large of the dangers to which too great credulity will expose it,—these are the objects which may be accomplished by a decided expression of opinion by the Academy. This may be attained by a slight, but important modification of the conclusions of M. Depaul’s report.

I cannot now refer to all the facts in favour of the version which I advocate. But I may be permitted to advert briefly to the fact that the nervous, strumous, dartrous, syphilitic, rheumatical, gouty, and hemorrhoidal diatheses, very frequently give rise to symptoms that are commonly ascribed to those uterine lesions which occupied the exclusive attention of Lisfranc and his pupils,—lesions that are purely secondary when they really exist.

I hasten to admit that local lesions not infrequently coexist with

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*"I have examined," Dr. Duparque writes, "portions of uterine tissue, in a perfectly normal state, which had been removed in consequence of supposed schirrous induration of the cervix."

"I can affirm," Dr. Deligny said to me, "that while Lisfranc was temporarily clinical surgeon at St. Côme, I saw him amputate the cervix in three cases in which only slight erosions existed."

A woman in the commencement of pregnancy was on M. Roux’s operating table, at Hôtel-Dieu, to undergo amputation of the cervix uteri. From some cause, the operation was postponed, when the patient’s condition was happily discovered.

An eminent surgeon examined a lady of twenty-six years by the touch, and gave an alarming prognosis; he thought that if she continued to neglect herself, in two months it would be necessary to have recourse to extreme measures. He had just amputated her elder sister’s cervix. Subsequently this young lady attributed every uneasiness to her ulcerated uterus. I had great difficulty in eradicating these notions; but after venesection, baths, rest, and mental discipline, her symptoms were ameliorated, and her cervix was in the healthiest possible condition. [Quotations from the work of Dr. Fandy.]

For my part, I have attended two young women, in whom incipient pregnancy was mistaken for uterine disease. It was proposed to amputate the cervix in one case, and, in the other, to burn it off with the actual cautery. In both, pregnancy was normal and regular.

I am also acquainted with two ladies of over sixty years, in whose cases a celebrated surgeon advised removal of the cervix thirty years ago, who now enjoy perfect health. —Note of M. Gilbert.
the general diathesis. The most common is uterine catarrh, of which, redness, tumefaction, superficial ulceration, and passive congestion of the cervix, are incidental accompaniments, which occur particularly in women who have borne several children.

But the degree of importance ascribable to these lesions is to be estimated from a multitude of local and general circumstances. In many cases, it depends entirely on the amount of mental disorder excited by apprehension of uterine disease, which disorder re-acts on the physical organization. It is thus that we are able to account for cures obtained by the most dissimilar and singular methods. Some local phenomenon ceases, which has kept up what I call the uterine monomania, and presently all the nervous symptoms excited by this monomania cease likewise.

I will adduce a single illustration of these strange recoveries, in which moral influences are chiefly effectual, although attention to the local condition is also requisite:

A weak and nervous lady became alarmed by the reports of several friends, who were undergoing speculum treatment for womb-disease. She presented an anteversion of the womb, the organ being very moveable in a large pelvis; there was a decided anteflexion of the cervix, and uterine catarrh. In addition to these local signs, there existed the courtége of symptoms that Lisfranc attributed to engorgement: gastralgia, hysterical spasms, bearing-down pains in the loins, sense of weight in the perineum, and pain in locomotion.

After divers ameliorations and relapses, a menorrhagic attack supervened, which was a finished stroke to the terrified lady, who took to her bed, and remained there for some months, the bleeding recurring from time to time. At last, I resolved to employ a kind of pessary recommended by old Dr. Devilliers, which is nothing else than an unripe orange. The soft and polished surface, and rounded form, of this fruit, allows it to remain in the vagina without causing discomfort, while its rind furnishes an astringent juice.

The green orange was introduced during an interval in the metrorrhagia; the next day, the lady left her bed; the pains ceased; no more blood appeared except at the regular monthly period, when its quantity was moderate. Two years have elapsed, during which this lady has enjoyed uninterrupted health.

The pessary escaped during an effort of defecation five days after it was applied.

In this case as in many others, I attribute the cessation of the symptoms to the moral reaction which took place when the imagination was calmed, and diverted from eternal contemplation of the uterus. It is useless to add, that in the case reported, the deviations of the womb continued notwithstanding the restoration of the patient's health.

We must not forget that, barely twenty years ago, the cervix uteri was amputated for simple ulcerations, and that if this operation had not almost immediately produced several disastrous results some surgeons would still practice it. Now there are innu-
mereable examples of women who refuse to submit to this operation, who are now in perfect health.

I trust that the operation of intra-uterine redressement will be abandoned in its turn. Meanwhile, there is both utility and propriety in a salutary announcement to the medical public, of the abuses in surgical practice in uterine affections. It was on this account, that I proposed to amend the conclusions of M. Depaul's report, which apply only to displacements, so that they should embrace diseases of the womb in general. Thus modified, the conclusions would read as follows:

1. There has been great exaggeration of late years in regard to the influence of displacements of the womb, and other uterine lesions, such as ulcerations, engorgements, etc., on the health of women.

2. In many cases, the symptoms ascribed to these lesions, are produced by pathological causes of an entirely different nature.

[Virginia Med. and Surg. Journal.]

On the Etiology and Hygiene of Cancerous Tumours. By Professor Bouchardat, of the Faculty of Medicine of Paris.

Having devoted a good deal of attention to some of the points involved in the great discussion on cancerous tumours which has lately agitated the academy of medicine, I had resolved to take part in the debate. But in preparing my remarks for that occasion, I found myself wandering so much from those subjects to which the discussion was limited, that I have determined to arrange my ideas in this form, hoping that the facts which I am about to produce, may, one day, be of use in the investigation of those difficult and important questions which relate to the etiology, hygiene and prophylaxy of cancerous tumours.

Before entering upon the matter before us, I cannot resist the temptation of expressing my opinions, with regard to one of the parties to the late academic debate. I mean that party who denied the value of the microscope in clearing up the diagnosis of cancerous tumours. To some of these persons who have taken part in the debate, I am tempted to address a question, which I never fail to propose to those who talk with me about the alcoholic ferment, and who regard with doubt, the admirable discoveries of M. Cagniard Latour. "Have you studied the ferment with the microscope?—No.—Well, begin at once to study them conscientiously, and then we will discuss the subject."

I well know that the subject of cancer presents many serious difficulties, but by ascertaining precisely the definition of the term, we can for the most part, explain away the difference of opinion. It is very evident that if we retain the old definition of cancer, the microscope will furnish information of great value without doubt, but which will still not be pathognomonic.

Cancer, they say, is an accidental production, capable of being
transmitted hereditarily, but produced more frequently from unknown causes. It generally increases rapidly, invading the surrounding tissues; ulcerates in process of time, producing great loss of substance, and sometimes large and repeated hemorrhages; developing in the neighbouring glands and ganglions engorgements of the same nature as the primary tumour; returning most generally after ablation, either in the cicatrice or in the neighbouring glands, or in some other part of the economy; producing in its last stages a peculiar cachexia, and giving birth, in at least one-half the cases, to the production of many tumours either in the liver, lungs, or some other part of the system.

We comprehend without difficulty, that some one of these symptoms might be absent, and yet, that the existence of cancer would be not less certain. But does the manifestation of these various characters constitute a unique, fundamental disease, as for instance, the various manifestations of the syphilitic poison constitute a malady identical in its origin?

If we pursue this comparison carefully, it is plain that if from its causes, progress, and treatment, we should conclude that syphilis is a special disease, assuming various forms, we must conclude that the cancer of the old authors cannot be considered as a morbid unity. Diseases, certainly presenting many points of resemblance, yet differing in many important features, have been grouped together under the general name of cancer. I doubt not that the time is coming when these distinctions will be established, and I believe that we approximate the truth when we say, that cancer does not form a morbid species, with distinct varieties, but should be rather considered as a class of diseases with many symptoms in common.

Nevertheless, before determining this great question of the morbid unity of cancer, we should examine the objections to be urged against that large number of partisans who group cancerous affections together, just as they group syphilitic or tuberculous affections.

There is no doubt that from the point of view in which many authors have placed themselves, this morbid unity seems very apparent, but when we regard this difficult question in its other aspects; when we consider the important facts which can be obtained from its etiology and therapeutics, our faith in this morbid unity diminishes. We can understand how an illustrious surgeon can say "I appeal to my study of this affection at the bedside, and I divide cancer into three classes—those which always return, those which frequently return, and those which rarely return," (Velpeau, session of the Academy, 9th January.) The great surgical experience which has led to this opinion, deserves our highest respect, particularly when it emanates from one who has so ably and patiently examined this difficult subject. But to the physician, this problem can receive another solution. To him, the dis-
tinction of the micrographer leads to useful results which cannot be appreciated by the surgeon who views the question in a different aspect.

Before going farther with this discussion, I will reproduce various extracts from the *Monteur des Hôpitaux*, the work of M. Broca, a young surgeon, who at the same time is a micrographer and clinician. That he has studied the subject with great diligence is proved not only by the beautiful essay which was received with so much approbation by the academy of medicine, but also by many other remarkable articles which appeared in the journal edited by M. Castelneau.

We have the most profound conviction, that future investigations will show the precise distinctions which exist between the varieties of cancerous tumours. At this time, the majority of physicians confound the simple glandular hypertrophies and cancerous tumours properly so called. But although the science is not sufficiently advanced, to determine accurately the lines of difference, yet we cannot doubt that some progress has been made.

Let us now examine the *etiology* of cancerous tumours. We can take up successively the influence of hereditary causes and of age, before we discuss the important questions of diet, moral influence and external violence. We should sift to the bottom the chaos of malignity, and endeavour to shew by what rules we may either avoid cancers, or retard their progress when the evil has developed itself.

*Hereditary influences* play an important part in the pathogenesis of cancer. M. Leroy d'Etiolles estimates that there is an hereditary transmission in one-tenth of the cases; M. Lebert in one-seventh. Without disputing these statistics, I will remark, that the hereditary influence in the genesis of diseases, comprises not only a native predisposition, but also a group of habits formed by a similarity of education or even of organism, and that very often these *hygienic habits* lead many generations to the same end. Hereditary predisposition is not alone the fatal cause, but we can by rectifying the customs and bad habits of our predecessors, avoid the curse under which one or more generations have fallen. I am convinced that this view of the subject deserves serious attention.

*Functions of the skin in connexion with age.*—We see cancers at all ages. It has been observed in children that the eye is usually the part affected, but it is a well established etiological fact that the relative frequency of cancer is much greater in the decline of adult life or in the beginning of old age.

The appearance of the three principal species of cancer: *cancer properly so called, cancroids and fibroids*, coincide most generally with a degeneracy of the functions of the skin and especially in that important function (and yet one but little studied in its pathogenic effects), the production of the epidermis, with its appendages the nails and hair.
The important principles which are eliminated by the skin under the form of epidermic scales and hair, play an important part in the animal economy, and many diseases of the latter years of life appear at this period of giving way in the functions of the skin.

*External violences.*—We attribute in the etiology of cancer, much importance to the influence of external violence. I am far from denying this influence, but it is evidently indispensable that the other conditions of age and predisposition should be also present to give any value to this cause.

*Depressing moral influences and passions.*—The moral affections, and more especially grief, has a certain influence on the development and progress of cancerous affections. The effects of sorrow are depressing, they repress the energies. The skin is the first organ which feels the general languor. We can very well understand then, how these emotions lead to cancer, if the conditions of age and of nutrition are also present. We can also see how these depressing influences are involved in the word malignity, an extraordinary expression in scientific language, and which means something that we do not understand. However this may be, we have two principal interpretations of this word in connexion with cancer.

Cancer is called malignant either when it leads rapidly to death, or when it extends quickly into the neighboring glands or into different organs than those originally attacked. That both of these results are greatly encouraged by the influence of the emotions of grief, we well know, and hence the influence which these passions have over the question of the malignity of cancerous tumours.

We say that a cancer is malignant, when it extends very rapidly. This occurs when there is a change in the nutritive functions, in fact, when there is an anomalous action established in the organism. It continues, indeed, because the movement advances in the direction it first assumed. Stahl has adopted this maxim of Plato, which expresses the idea: *facilius est movere quietum quam quietare motum.*

Without doubt, the skin does not alone participate in this abnormal direction of the vital movements, but it plays a part worthy of attention. It is very probable that the three principal species of cancer are caused by different modifications of nutrition. So we will say, though with great reserve, that the true cancer arises from a diminution of the epidermoid productions, and is influenced by age, or by some modifications of nutrition in the glands and lymphatic ganglions; that the development of fibroid coincides with a diminution in the epidermoid production and a diminution or aberration in the production of fibre, and that cancr oid owes its origin principally to a perturbation in the epidermoid functions.

*Diets.*—Deducting the many glandular hypertrophies which are often confounded with cancer, I am convinced that diet has a great
influence in producing the three species of cancer. An exciting and largely predominating animal diet may do much in developing this disease.

The reasons I have for this opinion are drawn from the fact, that this affection is almost always met with in carnivorous animals; the remarkable immunity of the Trappists who never eat meat, which is attested by M. Debreyne, and the mode of destruction and elimination of the residue of animal matters.

Prophylaxy.—I will sum up, in conclusion, the rules I believe most useful to follow, to prevent cancer, or to arrest its progress when the malady has displayed itself:

1st. To rouse the functions of the skin by cold baths, by daily frictions with coarse flannel or hair gloves.

2d. To stimulate the muscles by daily and regular exercise, and by gymnastic exercises in proportion to the powers of the system.

3d. To prefer a vegetable diet, and to eat very moderately of meats.

4th. To avoid moral emotions particularly of a depressing character, and to keep the mind amused and agreeably occupied.

5th. To obtain either by regular habit or by some purgative, one or two regular operations from the bowels every day.—[Virginie Medical and Surgical Journal.

On Uraemic Eclampsia. By F. WIEGER.

Dr. Wieger proposes to defend that new view of eclampsia which regards it as a complication of uraemia, against the older views sustained by L'Huillier and Depaul. He divides his memoir into six parts:

1. Critique of the Negative Observations.—The author seeks to show that the cases of eclampsia without albuminuria, recorded by L'Huillier, Depaul, and Mascarel, must be eliminated, either because the albumen was not searched for with sufficient care, or because the absence of albumen does not prove the absence of uraemia.

2. On the Condition of the Kidneys.—He seeks to invalidate the assertion of Blot, that the kidneys in eclampsia are not always diseased, by the collection of as many cases as possible. He maintains that in albuminuria, fibrinous cylinder-casts are always found in the urine, and that these often increase after delivery, and are found, as Braun and Litzmann have shown, so long as there is albumen in the urine.

He gives a table, which shows that up to the tenth day of the puerperal state anatomical lesions in the kidneys are always found; and that profound alterations of the kidneys are more frequent than congestive conditions.

He concludes this section with the following deductions:
a. The kidneys may be diseased without albumen being separated in the urine in observable quantity.

b. The absence of albumen at a given time is no sure proof of the absence either of disease of the kidney or of uræmia. The presence of albumen at a given time stands in no relation to the stage of the disease.

c. The albuminuria increases at the approach of labor, during labor, and the fits of eclampsia.

d. The kidneys cannot pour out albumen in any considerable quantity, or during a certain time, without becoming clogged up and diseased.

e. The appearance of the kidney-disease is often complete, and effected in a short time; often it persists in a slight degree, and becomes aggravated in following pregnancies.

f. When it persists during the puerperal state, the disease of the kidney induces other attacks, or causes complications, or aggravates existing ones.

g. Albuminuria grows with the occurrence of complications.

3. Albuminuria and Edema.—a. On Albuminuria.—Is there albuminuria without nephritis? The author cites instances from Simon, Schmidt, Henoch, and Canstatt, to prove the affirmative. As to the curability of the nephritis which attends pregnancy, he shows that as it depends upon transitory conditions, it is not like Bright’s disease, which is commonly dependent upon persistent or recurrent external causes, as cold, &c.

By adducing the statements of Blot and Litzmann, which exhibit the presence of albuminuria in fifty-six primiparae out of a hundred and seventy-eight, and in twenty-two multiparae out of a hundred and fifty-nine, he confirms the opinion, that the first labor is a predisposing cause of albuminuria and eclampsia.

b. Of Edema and Anasarca.—The presence or absence of this symptom has no constant value in diagnosis. It is not always present in Bright’s disease,

4. On Uraemia and Uraemic Symptoms.—a. On Uraemia.—Uraemia, the result of nephritis, is characterized in its chemical relations by the retention of water and excremenntial matter in the blood, which in its turn is impoverished by the loss of albumen and sometimes of globules. The excremenntal matter is thus driven to the skin, stomach, or salivary glands, and even to the lungs, or accumulated in the serous cavities or cellular tissue, to be taken up again into the blood, to aggravate the uræmia. For this reason, a strong diuresis may often persist a long time without exercising a remarkable influence on the nervous system. The degree of intoxication can only be determined by simultaneous examination of the blood and of the urine. He cites two cases of Gegenbauer and Chaiari; in the latter, urea, and, by decomposition of this, ammonia, were found in considerable quantity in the blood, and
the prophecy that eclampsia would break out was verified. The woman died.

b. On Uraemic Convulsions.—In seventy-eight cases of Bright, Barlow, and Frerichs, there was amaurosis and amblyopia ten times; syncope, nineteen times; singing in the ears and deafness, ten times; convulsions, fourteen times. The cerebro-spinal symptoms which precede the attacks of eclampsia have the closest resemblance with those of Bright's disease. Out of a hundred and forty cases of eclampsia collected into a table, forty-three showed premonitory symptoms. Of the cases in which the eclampsia broke out before labor, there were forty per cent.; of those in which it began during labor, thirty per cent.; and of those in which it began after labor, twenty per cent., which were attended with premonitory symptoms. As premonitory symptoms, the author enumerates vomiting and diarrhoea, but principally headache, disturbance of intellect, and often delirium; cramps and amblyopia, sometimes followed by blindness, not unfrequently precede. The changes of the pulse and pupils were too uncertain to be considered.

As characteristic of the so-called uraemic eclampsia, the author mentions that no predisposition lies at the foundation of eclamptic convulsions; they cannot become habitual; as a rule, the fits are frequently repeated. The disease is never chronic; it makes no periodic relapses, and seldom returns in subsequent pregnancies. The symptoms are those of epilepsy, without the cry at the onset.

The question as to the relation between epilepsy and eclampsia the author regards as not settled, but inclines to the view that epilepsy predisposes to eclampsia. Apoplexy of the brain and membranes may cause fatal convulsions, and appears frequently as a complication of Bright's disease, and makes the diagnosis more serious.

5. Etiology and Prognosis.—External Predisposing Causes.—The bad method of living of the poor favors the disease, and is the reason why it is more frequent in lying-in hospitals than in private practice.

Individual Predisposing Causes.—First pregnancies, twin pregnancies, are mentioned. Dubois has pointed to distortion of the pelvis and rachitis, duration of labor, and mal-position of child, asphyxia, indigestion; mental excitations, as fright, anger.

The mortality is, according to Murphy, 24 per cent.; Blot, 35½ per cent.; Lever, 28 per cent.; the author, 30 per cent. The mortality among the children is given by Blot as 67 per cent., and by the author as 45 per cent. Many children died shortly after birth, without inspection revealing any tangible cause of death. Frerichs, Litzmann, and Braun ascribe this death to intoxication of the blood.

6. Treatment.—The author divides the prophylaxis into a remote (against the albuminuria), and a treatment against the uraemic pro-
dromata, shortly before the labor. The peculiar condition of the blood indicates the following fundamental rules of a rational treatment:

a. The blood must be improved by good nourishment, tonics, and iron; Miquel recommends a vegetable diet.
b. Exciting diaphoresis by baths, &c.
c. Gentle purgatives.
d. Maintenance of the urinary secretion by gentle diuretics.
e. Direct action upon the renal obstruction, by abstraction of blood from the region of the kidneys.

_Treatment of the Uremic Pre-current Symptoms before Labor._—Tartar emetic, vapor baths, and scarification of the edematous parts, are considered. Chailly recommends chloroform when there is great tenderness in the uterus. The author regards it only as a palliative against the convulsions, and not as against the fundamental evil, the uræmia. _General bleeding_ he regards as a precious means. _The expectative method,_ the author regards as admissible when the convulsions are not strong, or first appear during the expulsion of the child. _Opium,_ much praised by many authors, is, according to the author, chiefly useful after delivery. _Cold affusions,_ are recommended by Recamier and Booth.

Coma, after cessation of the fits, the author treats with diaphoretics, salines, and diuretics, since the condition of the brain is caused, for the most part, not by hyperæmia, but by serous infiltration. Of _revulsive measures,_ the author rejects cantharides. Of antispasmodics, musk has been useful after too great depletion.

The author then adduces several cases in which artificial delivery was resorted to, showing that often there is a rapid cessation of the attacks after the emptying of the uterus, and that the mortality is about the same.

_Abortion._—Convulsions which appear before the period of viability of the fetus, end for the most part with its expulsion.

In eclampsia before the beginning of labor, he enjoins excitation of the pains and hastening of delivery. He uses secale cornutum for this purpose, when the head cannot be reached by the forceps.—_[Schmidt's Jahrb, Med. Chir. Rev._

__On the Cesarean Operation, and its performance more than once on the same subject._—By Prof. Stoltz, of Strasbourg.

For upwards of fifty years, says M. Stoltz, this operation, although frequently resorted to in Paris, has not generally been followed by success, whilst towards the close of the last century it was, in numerous instances, attended with the best results, in the hands of the celebrated accoucheurs of that period, Lauversat, Deleurye, Coutouly, Millot, etc. This failure of the operation, in
the practice of modern surgeons, he considers as having led to its being regarded as much more hazardous than it really is, and to its consequent restriction to those cases in which it is found impossible to effect delivery by any other method. Thus, the accoucheurs of Paris have nearly all been induced to adopt the English practice, according to which, rather than risk the life of the mother, they sacrifice that of the child, wherever the difficulty appears surmountable by so doing; a proceeding which our author believes to be unjustifiable, on the grounds that one life is taken without any certainty of thus saving the other. M. Stoltz has performed the Cæsarean operation six times, with the result of saving both mother and infant in four of these instances, and the child in the remaining two. The latest of these cases is that which he communicates to the Academy of Sciences, as possessing additional interest in the fact of its being the second occasion in which the operation had been performed, with complete success, on the same individual.

The patient, Adele Fenninger, æt. 39, was the subject of general rachitic deformity, and had been operated on, three years previously, by Dr. Bach, of Strasbourg, who published the case in the Gazette Médicale, of that city, in 1846. On that occasion the operation was successful, although performed under very unfavourable circumstances, the patient being "radicalement" rachitic, in very delicate health, ill nourished, and suffering from considerable obliquity of the uterus. At the time of the operation, somewhat too large an incision in the abdominal and uterine walls was attended with troublesome protrusion of the intestines; and during the progress of the case, erysipelas showing of the points of suture, and phlebitis, followed by oedema of the left inferior extremity, were circumstances which more than once threatened the life of the patient; however, by careful attention, these dangers were averted, and a complete cure was obtained in the space of two and a half months.

The second operation, which presents no features of much importance, is minutely described by M. Stoltz, who remarks, that from the ordinary course of the second pregnancy, three years after the first operation, it may be concluded that the cicatrization of the uterus had been sufficiently perfect to resist with impunity a second dilatation of that organ; and that the second operation, like the first, had been complete in its success, mother and child being both saved. In conclusion, he refers to a number of other instances where the operation had been repeatedly performed on the same patient with successful results. The following published cases are enumerated by him, and the publication mentioned in which they are to be found:—

Dr. Mangold, of Bale, operated, first in 1797, and again in 1801, and M. Mautz, in 1807, upon the same patient, who died on the twenty-second day after the third operation.
Dr. Bacqua, of Nantes, operated successfully on one patient, first in 1797, and again in 1806.

Dr. Dariste, of Martinique, operated successfully on one patient in 1805 and 1807.

Dr. Lemaistre, d'Aix, operated three times on one patient, in 1805, 1807, and 1814. The patient died five days after the last operation.

Dr. Charmeil operated in 1813 (?) and another surgeon in 1814, on one patient, each successfully.

Chaussier communicates the successful performance of the operation a second time.

Merrrm of Cologne, operated successfully on one patient, first in 1812, and again in 1826. Dr. Zwanck operated for the first time on a patient named Adametz; Dr. Weideman operated a second time; Dr. Michaelis a third, and again a fourth time, with success.

Dr. Rouvin communicates a case where the operation was repeated with success. Dr. Bowen performed the operation twice on one patient, in 1833 and 1835, with success.

Professor Kilian, of Bonn, operated successfully on one patient, first in 1832, and then in 1833. In another case, he operated in 1837, and the same patient recovered from a repetition of the operation in 1843.

Dr. Mestenhaeuser operated successfully on one patient in 1840 and 1844.

Here then, adds M. Stoltz, are fourteen well authenticated instances of this operation having been successfully repeated on the same patients. In two of the cases mentioned, the operation was performed three different times, but, on the last occasions, followed by the death of the mother. In a third instance, however, it was performed no less than four times with complete success. This example would almost induce him to believe in those cases quoted by some authors, where women were said to have undergone the Caesarean section six or seven times.—[Gazette Médicale. N. Y. Jour. of Medicine.

On the Pathology of the Pancreas. By Dr. Eisenmann.

The interesting experiments of M. Cl. Bernard upon the uses of the pancreatic juice, have led the author of this article to consider whether or not the results obtained by that physiologist, are capable of being applied to the diagnosis of disease in this organ.

If the pancreatic juice assists the digestion, and consequently the absorption of fatty matters, the conclusion is, that when the functions of that gland are destroyed by disease, fat should pass with the dejections, and be found in them in an unaltered condition. It is interesting to observe how nearly science has anticipated what the observations at the bedside of patients laboring under such
disease have confirmed. The author has united the facts observed
by others with those presented in a case of his own.
Of seven cases adduced, six terminated in death; there had been
abundant fatty evacuations, and post mortem examination showed the
existence of induration, or other alteration in the pancreas. In the
seventh case, quoted from Lussana, the patient recovered. The
principal symptoms in this case were copious salivation, a
sense of weight in the epigastrium not increased upon pressure,
eructations, flatulence, coldness of the surface, and a small and
slow pulse. The expression indicated abdominal disease. Consti-
pation was present, but the faces contained yellowish particles
resembling fatty concretions, and which were in fact composed of
fatty materials. These occurred in increased quantity after purga-
tives. Mr. Eisenmann regards Lussana as the first who applied
to pathology the discovery of Bernard. In the instance observed
by himself, the author had diagnosed pancreatic disease, but had
not made out the presence of fat in the evacuations. This circum-
stance induced him to suppose that the pancreatic juice did not
exclusively produce the absorption of fatty matters, but was as-
sisted in this action by the bile. Budge and Wistinghausen have
made experiments which countenance this idea, and the author re-
calls a case cited by Pearson, of a woman who evacuated daily 3
oz. of a fatty substance, and did not present, upon post mortem
examination, any trace of pancreatic disease, but the liver was
pale, large, and destitute of bile. One remarkable circumstance,
is, that in many of the cases cited by M. Eisenmann, the oily eva-
cuations had ceased, while the pancreas was so indurated as to
render the performance of its functions impossible. The author
also alludes to another circumstance, not less worthy of remark,
viz, the large quantity of fat yielded, a quantity much greater
than that contained in the food, and which leads to the supposi-
tion that part of the fecal matters is transformed into fat. How-
ever that may be, the fact shows us that the presence of fat in the
intestinal evacuations indicates with probability, but not with cer-
tainty, derangement in the functions of the pancreas; while, on
the other hand, the absence of fatty matters does not authorize us
to conclude that there is no disease in this gland, if other symp-
toms exist indicative of such an affection.

The author has endeavored to employ the facts collected by him
in establishing a more correct symptomatology of disease of the
pancreas. He divides the symptoms into two classes, those ac-
companying degeneration of the gland, and those arising from its
acute and chronic inflammation. For the first of these, the symp-
toms are too various to afford any certain signs of the affection;
the presence of fat in the evacuations, and the examination of the
abdomen by the hand, suggest, but cannot positively determine,
its presence. It is otherwise, according to the author, with inflam-
mation of the pancreas. This is distinguished by a sense of weight
in the epigastrium, which is especially manifest about two hours after eating, extending towards the breast with a feeling of oppression; loss of appetite, inodorous eructations, malaise, retching or even vomiting, although the tongue may remain clean; constipation; small, diminished pulse; emaciation; expression of features indicative of an abdominal affection; melancholy and sometimes weariness of life.

The author recommends as a mode of treatment, the waters of Friedricks-hall, in small doses.—[Vierteljahrschrift für die Praktische Heilkunde. Buffalo Med. Jour.

On the Comparative Value of Laryngotomy and Tracheotomy in the Treatment of Croup. By J. P. Bachelder, M.D. (Read before the New York Medical Association.)

Among the remedial measures employed by surgeons for the cure of croup, that of making an opening into the trachea (tracheotomy) has been most frequently resorted to, while that of opening into the larynx through the crico-thyroid space, has been almost entirely overlooked and neglected. This preference of tracheotomy to laryngotomy, has never seemed to the writer to be predicated upon any sound reasoning; but, on the contrary, that the very nature of things was not only against it, but indicated most clearly that the other alternative should be preferred. With a view to a more correct appreciation of the comparative merits of the two operations, we have been induced to institute an examination into their respective claims to be adopted by the profession. We shall consider

I. The Objections to Tracheotomy.

These arise principally from the anatomical relations of important parts to that which is the immediate field of the operation:

1. The middle and inferior thyroidal veins, and the plexus or network which they form in front of the windpipe, some of which almost necessarily are wounded when tracheotomy is performed, give rise to a haemorrhage, which endangers and sometimes actually destroys life, either by draining the system, by blood getting into the trachea, or the delay occasioned by its flow.

2. The proximity of the carotids to the trachea, and the irregular distribution of blood vessels in this region.*

3. The thyroid gland is proportionally larger in the child than

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* The right carotid sometimes traverses the windpipe; also now and then a middle thyroid artery mounts upwards directly in front of the trachea to the thyroid. Dr. C. E. Isaacs, the Demonstrator of Anatomy in the University of New York, who is one of the most indefatigable and accurate anatomists in the country, says—"I have seen seven cases in which the middle thyroid artery arose from the innominata, and was distributed to the thyroid gland. In two subjects it was remarkably large, and its division in tracheotomy would have occasioned a very profuse hemorrhage."

"In two instances, no innominata existed; but the arch of the aorta gave off,—

1. the right subclavian; 2. the right carotid."
in the adult, while other parts (particularly the trachea) are smaller; and the cervical portion of the thymus gland rising, in some cases in childhood, considerably above the sternum, shortens very much the tracheal space between it and the thyroid.

4. The depth and small size of the trachea, especially in children, render it difficult to open, and sometimes, it is said, even to find that organ, and greatly enhance the danger of wounding important parts in close proximity.

5. The deficiency, at the posterior part of the trachea, of the fibro-cartilaginous rings, which therefore occupy only two-thirds of its circumference.

These, when entire, by their elasticity, keep the windpipe fully distended; but when divided in front, as in tracheotomy, this result no longer obtains, the divided extremities fall together, and not only close the opening made by the knife, but actually diminish the calibre of the tracheal tube. This state of things is also aggravated by the action of the sterno-hyoid, and sterno-thyroid muscles, which lie on the sides and partly in front of the trachea, where their inner margins are nearly in contact, at the very place in which the opening is made when tracheotomy is performed. These ribbon-like muscles, when acting forcibly, swell into rounded masses, in consequence of which, the sides of the trachea are compressed and forced together, and the opening, made by the division of its rings, closed. These muscles, when they act, also draw the larynx downwards, which, by compressing the rings into a smaller space, and forcing them backwards, still further diminishes the tracheal cavity. These circumstances,—the action of the muscles and perhaps the flow of blood,—render necessary the introduction of the metallic tube, through which respiration may be carried on, and matters which block up the air passages discharged.

Objections, growing out of the danger, difficulty, and inexpediency of this operation, will be noticed in another place.

II. Objections raised against Laryngotomy.

The objections most frequently urged against laryngotomy, and of course regarded as arguments in favor of tracheotomy, are that in the former the wound is made in the inflamed part, and that the tube (the necessity for which, in this operation, is only assumed) could not be tolerated; therefore, the opening should be made below the seat of the disease. These, I am aware, have been considered and adduced as the most cogent reasons against the opening being made in the crico-thyroid space. To the writer, however, this reasoning has always seemed strangely inconsistent. Let us examine its validity.

1. Cutting into the inflamed part. What is croup? Just what its technical name, tracheitis, implies,—an inflammation of the trachea. Dr. Watson, whose authority in this matter will not be questioned, tells us that "the essence of this complaint is a violent
inflammation affecting the mucous membrane of that portion of the air passages which lies between the laryngeal cartilages and the primary bronchi; in a word, of the trachea, or windpipe. That is the genuine seat of the disease, but the inflammation sometimes ascends into the larynx, and not unfrequently dives into the bronchi, and into their ramifications. Usually, the adventitious membrane commences just below the larynx, where it is thin and soft; about the middle of the windpipe (the tracheotomic region) "it is more dense and firm; lower down in the trachea, and in the bronchi, it is generally looser again, pulpy, and broken."

With due deference, and in all candor, we would ask gentlemen who adopt the mode of argumentation alluded to, to consider to which place the reasoning most legitimately points? But with us, the idea of cutting into the inflamed part has little weight, when we reflect how often it is done. What surgeon would hesitate to cut off an inflamed tonsil which endangered life? Or, who that has tried it, has failed to find that it is the most prompt and certain method of curing that disease? Who does not cut into carbuncles and inflamed parts to let out matter, and sometimes into parts affected with erysipelas for the same and perhaps other purposes? And who ever saw any injury result from the practice?

2. Intolerance of the tube. The other objection brought against laryngotomy is, that the tube cannot be borne when passed into the larynx. The fallacy of this objection will be presently pointed out, when it will be shown that the tube, should one be necessary, is equally well, and indeed better borne, than in tracheotomy, and is much more readily replaced after having been removed and cleansed, than when inserted into the trachea. After what has been said, we shall, of course, be free to admit the necessity of the tube when tracheotomy is resorted to; but in that very necessity is to be found a serious objection to the operation itself, which is instituted for a double purpose,—the admission of air, and the ejection of a superabundant morbid secretion. If, for the former only, the tube might not be so objectionable; but being made of inert materials, it cannot facilitate, but must necessarily impede and prevent, the discharge of matters, which—thickened, inspissated, tenacious, and adhesive—cling to its inner surface, and produce obstructions which require its frequent removal.

* To the remarks of Dr. Watson, we would add that the inflammation, which commences in the mucous membrane, not unfrequently, and in severe cases of croup generally, extends to all the other tissues and structures which enter into the constitution of the trachea. This involvement of the structures, somewhat peculiar in their organization, causes a contraction which not only diminishes the capacity of the trachea, but interferes with the movements of that part as a whole, and also of its constituents among themselves. This diminution of calibre and mobility, with its morbid irritability, renders this organ much less tolerant of the metallic tube. Another objection to the tube in the trachea, grows out of the difficulty of adapting its size to the exigencies of the case: if too small, neither respiration nor the expulsion of the morbid secretion can be carried on through it; if too large, it occasions great distress, by putting the trachea on the stretch.
The rima glottidis being the natural aperture through which matters within and below that orifice are discharged, the artificial opening, it would seem, should be as near it as circumstances will admit. If through the crico-thyroid space, and as large as that space will allow, the trachea—entire and all its parts in possession of their vital properties, powers, and actions—will contribute to the expulsion of those matters; which it would not do if crippled by the division of its rings in front, and also shackled and obstructed by an inert tube within its cavity. This tube, if inserted through an opening in the crico-thyroid space, may be short, and therefore less likely to interfere with the functions of the trachea than it would if long and passed far into that organ, at or near its middle portion; in which case the patient, although he has the tube through which to breathe, gets on badly.

Tracheotomy in comparison with Laryngotomy.

Tracheotomy is a dangerous operation, and under almost any circumstances difficult to perform, especially in the child. It is sometimes followed by or is the cause of immediate death, by haemorrhage, by the shock it gives to the system either by its violence or loss of blood, which the exhausted state of the patient cannot bear, or the delay occasioned by the bleeding or other circumstances.

Tracheotomy requires a skillful, practiced surgeon and anatomist, with divers instruments, for its performance.

Laryngotomy is an operation easily and quickly performed without haemorrhage or delay; is attended with no danger, there being no parts of importance in the way of the operator's knife. It is, indeed, very little more difficult or hazardous than venesection in the arm of a fat child.

Laryngotomy may be performed on the spur of the occasion, by almost any medical man, with the lancet only. The operator needs merely to pinch up a fold of integument directly over the thyroid cartilage, and an assistant to do the same over the cricoid, and then thrust a lancet, scalpel or bistoury, through, and divide the duplicature; this done, and the integuments being allowed to resume their natural condition, the operator, steadying the thyroid cartilage between his thumb and forefinger, pushes the lancet through the crico-thyroid membrane, close to the upper border of the cricoid cartilage, and makes a free opening into the larynx; which may, if required, be enlarged upward, two thirds the length of the thyroid, and downward, entirely through the cricoid cartil-
Tracheotomy, in every instance, requires the insertion of a metallic tube, through which respiration may be carried on, and the morbid secretions discharged.

In laryngotomy the insertion of the tube is seldom necessary. The membrane occupying the crico-thyroid space, being composed of the yellow elastic substance, contracts when divided, and prevents the closure of the opening; the more certainly if an incision be made upward so as to give the artificial opening the shape of an inverted J. If necessary, the angles with a portion of the thyroid cartilage, on each side of the perpendicular cut, may be clipped off; and also, as suggested, a portion of the cricoid cartilage in front may be removed. In this way, the artificial opening may be made considerably larger than the calibre of the trachea itself. Generally, we believe, it will be found that two strips of adhesive plaster applied, one on each side near the margin of the wound, (if longitudinally made,) and drawn backward and crossed behind the neck, will be all that is required to keep the wound patent.

Tracheotomy is so difficult to perform, that the administration of chloroform has been deemed necessary to its best performance, and there is reason to believe that in more instances than one, the influence of this agent, superadded to the exhaustion induced by the disease, the loss of blood, and the shock, notwithstanding the employment of this substance, has caused death or rendered it more speedy and certain.

In favor of tracheotomy, it is urged that almost all authority is on its side. This is true, most strangely so; for the nature of things, anatomically considered, and common sense based on them, and also on sound reasoning,—seem to be all in favor of laryngotomy.
Laryngotomy and Tracheotomy in Croup.

To this objection, as well as that already alluded to, the intolerance of the tube in the larynx, we beg leave to refer to the last June number of the London Lancet, in which will be found "Some cases of Laryngotomy, by John Erichsen, Esq.," which seem to be exactly in point. They derive additional value and interest from the fact that they were all cases of laryngitis. In case first, he made an opening through the crico-thyroid membrane, and introduced a large silver tube. On the third day, the tube was finally withdrawn, and the wound gradually closed; the patient recovered without a bad symptom. Case 2. Mr. Erichsen says, "I lost not a moment in opening the crico-thyroid membrane, and inserting a large silver tube, which she has never been able to lay aside. It has been worn for between six and seven years, and in close proximity with the vocal cords;" and, he continues, "the action of these has been in no way impaired." Pretty good evidence this, that the tube in the crico-thyroid space can be borne quite as well as in the trachea. Case 3. "A short, stout, thick-necked woman, about thirty years of age. There was not a moment to lose. I immediately proceeded to operate, but on making an incision through the integuments, the vessels of the neck, over-distended by asphyxial congestion, poured out so large a quantity of dark blood, that it was necessary to wait a few moments before opening the windpipe. During this delay the patient, who was seized with a severe spasm, ceased to breathe, and fell back to appearance dead. I immediately plunged the scalpel through the crico-thyroid membrane, cut this freely across, and put in a large silver tube. She made a good recovery."* These were cases of laryngitis: of course the opening and insertion of the tube were in the inflamed part, yet not only no inconvenience resulted, but the most perfect success followed. They annihilate the objections raised against cutting into the inflamed parts, and the intolerance of the tube in laryngotomy.

A few short extracts from cases reported in the London Lancet, (December, 1855,) by Henry Thompson, F.R.C.S., M.D., will close this paper: Case 1. "A man about forty years of age," with "an attack of acute laryngitis." "There was no time for delay. I proceeded at once to make an opening into the larynx, sufficient to admit a full-sized double-trachea tube. The relief was instantaneous." This occurred on the 27th of September; and on the "1st of October, the apparatus was removed, and the edges of the wound approximated;" which on the 8th "had entirely closed." "Case 2." "A little boy, three years of age, livid and almost insensible, struggling violently for breath. There was evidently no time to be lost. Opening first the crico-thyroid membrane, I found it necessary subsequently to divide the cricoid cartilage, and upper ring of the trachea, in order to introduce the tube. We had the satisfaction, however, of leaving him, in the course of half an

* The italicizing is ours.
Treatment of Pseudo-membranous Angina.*

By the Alkaline Carbonates.

(Translated from the "Gazette des Hôpitaux," April 7th, 1855, for the Boston Medical and Surgical Journal.)

The solvent action attributed to the alkalies, upon the fluids of the body, and particularly upon certain abnormal products of secretion, has already been taken advantage of in quite a large number of diseases. We are not sure whether pseudo-membranous angina figures among the great variety of affections to which this treatment has, of late years especially, been applied; our recollections on this point are not exact. Were this so, however, the interest which attaches to the case recently communicated to the Academy of Sciences, by M. Marchal (de Calvi,) would be none the less real. Its clinical and practical bearing gives it naturally a place in our pages.

Starting with the idea that the principle which is the cause of the disease known under the name of pseudo-membranous angina [angine couennense] is unknown to us, but manifests itself by a phenomenon, the formation of false membranes, indicating an excess of plasticity in the blood; and regarding this excess of plasticity, if not the immediate cause, the most striking phenomenon of the pathology of the malady, at least the fact which nearest approaches it, and to which, consequently, we must address ourselves, in order to attack the evil as nearly as possible to its source, M. Marchal had for a long time, he says, conceived the idea of resorting in such cases to the employment of alkalies, without, at

*The French expression, angine couennense, is one which it is difficult to render satisfactorily in English, but the literal translation, which we have adopted, will be sufficiently intelligible. The disease is also called—pseudo-membranous inflammation of the fauces, membranous angina, diphtheritis, &c. The latter synonym was introduced by Bretonneau, by whom the precise nature of the disease was first satisfactorily made known.—[Translator.]
the same time, neglecting the inflammatory indications. This he has done successfully in the following case, which we give in his own words, adding that we adopt the conclusions of our confrère, with the reservations which he has made.

"M.B., Chief Engineer of the Vincennes Railway, was attacked in the beginning of March, 1855, with a sore throat, which at first appeared slight, but which rapidly grew worse. When first called, at the beginning of the attack, I prescribed simple remedies. The next day, the inflammation was much more intense; the mucous membrane of the posterior fauces was very red and oedematous; the suffering very acute both in the posterior fauces, and in the sub-maxillary regions, and much increased by the act of deglutition. But what especially struck me, and from the first glance caused me the greatest uneasiness, were streaks of a pearl-white color, on the surface of the tongue, and more particularly on the palatine mucous membrane, and that of the tonsils (which were not much swollen,) forming by their agglomeration very evident patches, concerning which there could be no mistake. It was, in fact, the product of a plastic exudation; only, in the mucous mem-
brane of the throat, the product was interstitial, in other words it had not passed through the epithelium; whereas, on the tongue, the false membranes, one of which was of the size of the nail of the little finger, were superficial. I endeavored, for the sake of greater certainty, to scrape off one of these spots from the soft palate; I was unable to do so, and the effort occasioned in the patient a violent attack of vomiting. He complained of extreme distress in the posterior part of the nasal fossa, which attained its height during the action of deglutition. The pulse was 180, large and soft. On account of the greater number of cases of eruptive fever which existed at that time, the idea of commencing scarlet fever naturally presented itself to my mind; but, on the one hand, the mother of the patient had died (in 1845,) of an attack of pseudo-membranous angina, and it is well known that this form of angina may be called a family disease. On the other hand, the plastic infiltration of the pharyngeal mucous membrane, and the false membranes on the surface of the tongue, were unmistakable. Diphtheritis then existed, and in a man hereditarily predisposed, there was reason to fear that this affection, arresting the scarlatinous eruption, would pursue its course, the same as if it were idiopathic.

I therefore decided to apply leeches, in order to diminish the inflammation, and to give the bicarbonate of soda, in large and often-repeated doses, to counteract the excess of plasticity of the blood. I prescribed twelve leeches to the submaxillary regions (six on each side,) and 12 grammes (3 drachms) of bicarbonate of soda, in twelve powders, one to be taken every half hour, in a spoonful of sweetened water.

"This was at nine o'clock in the morning. At one o'clock, the
patient had taken 8 grammes (2 drachms) of the bicarbonate. The leeches had drawn a large quantity of blood, which still flowed abundantly, evidently less plastic than in the normal state. As to the throat, the appearance of things was astonishing, and afforded me as much surprise as pleasure. The false membranes on the tongue remained, surrounded by a pultaceous, dirty-grey layer, which also covered the gums, where it was white; but the plastic infiltration of the posterior fauces had completely disappeared; not a trace of it was left. In the space of four hours, a most alarming state of things, capable of inspiring the deepest anxiety, had wholly subsided. Was this owing to the influence of the bicarbonate of soda? Such is my opinion.

"In the evening, red points appearing upon the skin, announced the scarlatinous eruption, which was general and intense, and which had hardly begun to fade before it was followed by a military eruption of white, serous vesicles, very close together, on the neck and arms, accompanied by short paroxysms, during which the heart beat violently, as in the suette.

"I return now to the essential point in this communication, the disappearance of the diphtheritis in the throat, under the probable influence of an alkaline salt. In the first place, no conclusion can be established in therapeutics from a single case. Moreover, this instance is not so demonstrative as we could wish, since in my patient the diphtheritic angina was connected with scarlatina, and the pseudo-membranous angina of scarlatina is much less grave than the idiopathic variety. But, as I have already observed, there was one circumstance, its hereditary character, which gave to the angina, although scarlatinous, a peculiar gravity. Besides, when we reflect upon so sudden a disappearance of the diphtheritis, after the administration of the bicarbonate of soda, we can hardly fail to see in it an effect and a cause; and we may ask whether the same effect would not take place in idiopathic diphtheritis.

"I have said that the object of the alkaline salt was to counteract the excess of plasticity in the blood; it might also have another mode of action, a local or direct effect upon the diphtheritis. This did not escape the attention of M. Trousseau, to whom I communicated the case, which so much interested him that he desired to try the alkaline carbonates in the treatment of pseudo-membranous angina. The local effect which I have mentioned is easily understood, since a gramme (15 grains) of bicarbonate of soda, in a teaspoonful of water, is rather hard to swallow, and 'scrapes as it goes down,' according to the expression of the patient."

Blancard's Pill of the Iodide of Iron.

Five years ago, M. Blancard, a pharmacien of Paris, proposed an unchangeable pill of the iodide of iron, made directly from its elements, which was officially approved by the French Academy
of Medicine. The excellence of this preparation was generally acknowledged, and it is already, in France, the most common form for the administration of iodide of iron. Our pharmaceutical authorities at Philadelphia, however, adhere to the saccharine solution which Dr. Jackson introduced many years ago, and Prof. Bache declares that the solid iodide "might well be dispensed with." Practitioners will differ sometimes from the chemists, and so it has proved in this case. It is found that, notwithstanding the assurances of the self-constituted authorities, the syrupy solution of iodide of iron, does undergo change: that it often injures the teeth, disagrees with the stomach, and contains free iodine. Consequently, as our dispensatory-authors and colleges of pharmacy simply advise us, if we must have a pill, to evaporate their syrup, or to use the antiquated and unreliable process of Callond, practitioners have found it of advantage to import M. Blanchard's preparation, which is now very commonly prescribed, not only in New York and Boston, where there are agencies for the sale of it, but in many remote country towns. And here we may take the liberty of recommending to the gentlemen who have taken on themselves the direction of pharmaceutical matters in this country, that they should not be too dictatorial or dogmatic, if they expect to retain the authority which has been conceded to their talents and learning.

With these preliminaries, we give at length the process for preparing Blanchard's pills, which we take from the Bulletin de l'Academie de Medecine. It is founded on the volatility of ether, and the insolubility of the iodide of iron in this vehicle:

Take of iodine seventy-seven grains; Iron filings thirty-seven grains; Distilled water two and a half drachms; Honey one drachm and thirty-four grains; Absorbent powder (say powder of Althaea) a sufficient quantity. Make 100 pills.

Place the water, iodine, and iron in a Florence flask; shake the vessel as the reaction takes place; filter the green liquor that results, into a small iron capsule, the weight of which is known. Wash the flask, and filter with two and a half additional drachms of water, slightly sweetened with a portion of the honey to be used in making the pills. Pour both liquids into the capsule, and evaporate, at first rapidly, then at a gentler heat, until the weight of the mixture is equivalent to the combined weight of the iodine and the honey (171 grains, or 5 iij. nearly). Add a sufficient quantity of powdered althaea root, or, still better, equal parts of althaea and liquorice powder, about 5 iij. Divide the mass into four equal parts; roll each part in powdered iron. Make each mass into a cylinder on an iron slab; divide each cylinder into twenty-five pills, and roll each pill in powdered iron, to cover the iodide exposed by the spatula. Expose the pills to a gentle heat that they may contract no moisture, and proceed at once to the second part of the process—varnishing the pills.
Make a solution of balsam of Tolu in three parts of ether. Place the pills in a porcelain capsule, pour on them a portion of the ethereal tincture, and impress a rapid motion of rotation, that the pills may be moistened on every side, and that the ether may evaporate rapidly. As soon as the pills begin to stick together, throw them on a dry surface, separating those that are agglutinated and leave them exposed to the air for twenty-four hours; then dry them over a stove at a gentle heat.

It is well to give them a second coating of varnish. Blancard puts them in a bottle with a stopper covered with silver, which is at once tarnished by the vapor of free iodine.

Each pill contains about one grain of iodide of iron, and one-fifth of a grain of powdered iron on its surface. Two to four pills daily is the ordinary dose in chlorotic, scrofulous, tuberculous and syphilitic diseases.—C. E.—[Gazette Med. Sardin. Buffalo Med. Jour. and Month. Review.

The Simplest Operation for Uncomplicated, Congenital Phymosis.

By T. Furneaux Jordan, Esq., M. R. C. S.

Not only are Surgical authorities of opinion that circumcision is rarely, if ever, necessary; but those truly frightful slits, extending half-way up the penis, to be seen in the pretty engravings which adorn some (of our best, too) Surgical manuals are fast getting into chirurgical disfavor. The present mania, however, of attributing uncomplicated, congenital phymosis in every case to the unfortunate mucous lining of the prepuce alone, and the practice of heroically slitting up the same to the very point of its reflection from the penis, has arisen rather from the hypothesis of theorists than from the enlightened experience of acute observers.

The non-dilatability of the congenitally phymosed prepuce is confined to the margin of the preputial orifice and to the skin and mucous membrane in its immediate vicinity; such non-dilatability undoubtedly extending to a greater distance on the inner than on the outer aspect of the foreskin.

The received opinion, touching the non-elasticity of the preputial lining in its entire extent, is so far from being correct, that ordinarily such lining, for some distance anterior to its point of reflexion, is arranged in rugous folds, like all other mucous membranes that are too large for the organ they line, save when the peculiar function of that organ is being exercised.

The opinion that the skin is not implicated in phymosed stricture, is equally incorrect. In one patient, on whom I operated with complete success, by far the tightest portion of the prepuce, after recovery from the operation, was the skin for two lines behind the cicatrices.
From the above remarks, it will be inferred that any incisions, which extend further than the parts forming the margin of the prepuce, and for a short additional distance on the mucous surface, are unnecessary, and hence cruel. A single incision, however, as described, would fail to secure the retraction of the prepuce, not because the incision is too limited, but because a single incision cannot possibly relieve the whole circumference of the congenitally contracted preputial orifice; two, however, or at most three, of the small incisions in question would afford complete relief.

The mode of operating which I have adopted, and with signal success in its results, is this:—Having first induced local anaesthesia, by applying pounded ice to the penis for two minutes, I introduce one blade of a pair of scissors (blunt-pointed, yet cutting to the end) to the distance of ¼ an inch, between the glans penis and the prepuce, on one side of the penis, at a point midway between the frenum posteriorly, and the mesial line anteriorly. Both layers of the prepuce being divided to the extent mentioned, a similar incision is made at a similar point on the other side of the penis. The prepuce is now retracted to the extent allowed by the incisions, which by this proceeding are brought quite external, enclosing between their lips an uncut layer of lining membrane. This is divided on each side, by introducing one blade of the scissors, to the extent of, and immediately under, the original wound. The entire prepuce may then be retracted, a piece of wet lint wrapped round the penis, and the whole supported by a proper suspensory bandage. The patient need not lie in bed. Where three incisions seem preferable, they should be equidistant from each other, the third being at the mesial point anteriorly, the two lateral incisions should be a little nearer the frenum, than when two only are made.

The incisions may of course vary a line or two, one way or the other in extent, according as the constriction is more or less aggravated.

The recapitulatory points to which I would draw attention, are:—

1. That the skin is more, and
2. That the mucous membrane is less, involved, than is generally supposed.
3. That two, or at most three, comparatively small incisions will afford complete relief.
4. That no assistant is required, and
5. No instrument save a pair of scissors.
6. Two or three small incisions cause much less irritation, and heal much more quickly than one large one.
7. That the patient need not lie in bed.—[Med. Times and Gaz.]
Nitrate of Silver as a Remedy for Burns. By John Wiltbank, M. D., of Philadelphia.

I wish to call the attention of the readers of the Examiner to the value of the nitrate of silver as an application to burns and scalds. I have used it frequently both in deep and superficial burns, and I have been equally surprised and gratified by the results. The advantages of the caustic application are numerous. It furnishes a complete protection to the inflamed surface, subdues the pain, arrests the serous discharge, changes the character of the inflammation, promotes a speedy cure, and if I am not mistaken, prevents the formation of those ugly cicatrices and the irregular contractions of the skin which so often occur in the healing of burns.

The mode of application is simple. In superficial burns a strong solution—20 to 40 grains of the nitrate to the ounce of water—should be applied over the whole surface with a camel's hair pencil, vesciations should be opened and the surface carefully wiped dry before the solution is applied. If the burn is deep and the discharge of serum abundant, the entire surface of the ulcer should be touched lightly with the solid stick.—[Medical Examiner.

Formic Acid in the Blood of a Person, killed by the Inhalation of Chloroform.

The following note, received from a source which entitles it to credit, will be read with interest.

"In the Journal of January 17th, an account of a recent death from chloroform in this city, was given. A quantity of the blood, removed at the autopsy, was placed in the hands of Dr. C. T. Jackson for chemical examination. He has ascertained that the blood contained formic acid, and that it could readily be separated by distillation in the heat of a chloride of calcium bath.

"Chloroform consists of formyle and three equivalents of chlorine; formic acid of formyle and three equivalents of oxygen. The three atoms of chlorine leave the chloroform and unite with the blood, while three atoms of oxygen leave the blood and unite with the formyle of the chloroform, replacing the chlorine and producing formic acid. Thus the blood is not only deprived of its oxygen, but it is so altered as to be incapable of absorbing vital air and the patient dies from asphyxia. The production of formic acid under such circumstances has never before been known, and of course it is to be regarded as an important physiological fact of no small practical moment."—[Boston Med. and Surg. Journal.

Anæsthetics in the Austrian Army.

A circular has recently been issued, ordering that in future, the army medical officers shall always employ, for the purpose of in-
dancing anaesthesia, a mixture consisting of one part chloroform
and nine parts ether, this being the proportion long employed by
Dr. Weiger, a Vienna dentist. — [N. Y. Med. Times.

Pathology of Diabetes.

M. Andral stated at the meeting of the Academy of Sciences
(Paris) in July last, that in five cases of diabetes which he had
examined after death, since the publication of the researches of
M. Bernard, he had uniformly met with one alteration. He had
found in each instance all the anatomical characters of a very in-
tense hyperæmia, and different in its aspect from ordinary hyper-
æmia of that organ. The tissue of the liver of the diabetic patient,
is everywhere gorged with a large quantity of blood. M. Andral
regards this as being at the same time, a change peculiar to dia-
etes, and a proof of the sugarforming function of the liver. — [N. Y.
Med. Times.

EDITORIAL AND MISCELLANEOUS.

Another Bearded Woman.—Our city has been visited this month by
another of those anomalous beings heralded as bearded women. The sub-
ject of this notice is announced as the "Bear Woman," "a most extra-
ordinary nondescript," "a half-human half-beast creature," "otherwise called
Julia Pastrana, a hybrid root-digger Indian, from California"!! The pamph-
let sold at the door contains, as usual in such cases, certificates from "Dr.
Mott" and other "distinguished physicians." We do not envy the taste of
the "distinguished" members of our profession who would lend their
names to such purposes of humbuggery. We are happy to say, however,
that the valued and respected name of Valentine Mott is not among the
certifiers. But, lest we might do injustice to the "distinguished" gentle-
men, we beg leave to append their own language:

"Dr. Mott's Certificate.
"NEW YORK, December 3, 1854.

"Sir:—To naturalists alone we leave the task of solving the enigma con-
cerning the origin of Julia Pastrana, the 'Semi-Human Indian,' which would
have puzzled the Sphinx. From her uncouth gait, it may be conjectured
that the mysterious animal moves as if an elongation of the Spinal column
should have taken place, producing a tail, which in consequence of human-
ity predominating, has been denied.

"She is a perfect woman—a rational creature, endowed with speech,
which no monster has ever possessed. She is therefore a Hybrid, wherein
the nature of woman predominates over the brute—the Ourang Outang.
Altogether she is one of the most extraordinary beings of the day.

"I remain yours respectfully,
ALEX. B. MOTT, M. D."
"From Professor Brainerd.

"CLEVELAND, August 5, 1855.

"Sir:—In compliance with your request, I will state that I examined the hair of the specimen of the Gents Homo which you have in charge, and compared it with the hair of the African, under a high magnifying power, and from this comparison, have no hesitation in saying that the individual in question possesses, by this test, no trace of Negro blood. Her other peculiarities, the hair upon the body, its length and structure, the form of the mouth and nose, the size of her limbs, peculiarity of her breasts, &c., and various other features, entitle her, I think, to the rank of a distinct species. Yours, truly,

"S. BRAINERD, M. D."

We must leave it to others to say how "a perfect woman" can be "a Hybrid, wherein the nature of woman predominates over the brute—the ourang outang," to say nothing about the sapient conjecture of "the elongation of the spinal column" which "should have taken place, producing a tail." It is assuredly not surprising that whole communities should be duped, when men in high places, and therefore supposed to be well informed, certify to such nonsense. We do not know the meaning attached by the certifiers to the word Hybrid, and will not stop to discuss the value of this term as applied to varieties of the human family, or to mixed races; yet, notwithstanding their opinion as based upon an inspection of the hair, we think this woman's flattened nose, thick lips, projecting jaws, and swarthy complexion, testify unmistakably to the infusion of negro blood in her veins. Her straight hair is like that of our savage, or other branches of the red family, who probably furnished the other element of her origin. Be this as it may, Julia Pastrana is an exceedingly homely bearded woman, and nothing else. Her arms and shoulders are not more hairy than those of many men. The whole of her forehead, nose and face is covered with a downy beard about one-eighth of an inch in length, but which, beneath the chin, reaches two or three inches. There is nothing uncommon in her person nor in her gait.

Medical College of Georgia.—The Commencement exercises in this institution took place on the 3d day of March. The Dean reported that "there were one hundred and seventy students in attendance upon the course of Lectures just concluded; of whom 119 were from Georgia, 22 from Alabama, 18 from South Carolina, 5 from Mississippi, 2 from North Carolina, 2 from Florida, 1 from Texas, and 1 from Nova Scotia." The Degree of Doctor of Medicine was then conferred by Ex-Governor Wm. Schley, President of the Board of Trustees, upon seventy-three gentlemen, and the Honorary Degree of Doctor of Medicine upon Dr. John Harwood Burt, of South Carolina. An able and appropriate Address to the Graduates was then delivered by the Rev. Mr. Ryerson, which was followed by a chaste
and eloquent Valedictory by Dr. W. L. M. Harris, of the graduating class. These interesting proceedings were attended by a large and respectable concourse of ladies and gentlemen. The Graduates were:

FROM GEORGIA:

FROM SOUTH CAROLINA:

FROM ALABAMA:

FROM TEXAS:
J. W. Veazey, D. S. Watts.

FROM MISSISSIPPI:

Medical College of Savannah.—The following gentlemen received their diplomas at the commencement, on the 6th March:

Oglethorpe Medical College.—The first annual commencement of the Oglethorpe Medical College of Savannah, took place on the 8th March. The following are the names of graduates:
Savannah Spring School of Medicine.—An organization under the above name has been formed in our sister city. The term of Lectures is to be from the middle of March to the end of June. The lecturers are Holmes Steele, M.D., on Obstetrics, the Physiology of Generation, and Diseases of Women and Children; John M. Johnson, M.D., on Medical Chemistry and Materia Medica; Joseph J. West, M.D., on Anatomy, the Physiology of the Viscera, &c.; and Charles H. Colding, M.D., on Minor and Operative Surgery. Fee $50, in advance.

Medical Society of the State of Georgia.—The next annual meeting of this Society will take place on the 9th April, at Macon.

The American Medical Association will convene at Detroit (Michigan) on the 6th of May.

A protection against Puerperal Fever.—A case of labor recently occurred in the Boston Lying-in Hospital (reported in the Boston Medical and Surgical Journal,) which Dr. II. R. Storer was called to attend. Having that day seen a severe case of Erysipelas, and believing that he might impart the infection to the woman and thereby induce puerperal fever, he declined going. Compelled by circumstances to go, however, he determined to put the female upon the use, as a prophylactic, of 15 drops of mur. tr. of iron, every six hours. Although he had to extract the placenta with his hand, no bad results followed, and the iron was discontinued on the 5th day. Dr. S. regards the case as confirmatory of the efficacy of the preventive treatment, already recommended by others.

Although the production of Puerperal fever in the way just alluded to, is still a mooted question, there is a growing disposition on the part of the profession to admit the fact—and we should therefore not omit a resort to any measures of prevention that may be proposed, especially when these are in themselves harmless.

Works received.—We acknowledge the reception of the following works: Report of the Pennsylvania Hospital for the Insane, for the year 1855, by Thos. S. Kirkbride, M.D., Physician of the Institution. The American Journal of Science and Arts, (full of valuable matter as usual.) An Essay on Intermittent and Bilious Remittent Fevers, with their Pathological relation to Ozone, by E.S. Gaillard, M.D., of Charleston, S. C., (a very interesting and creditable production). Treatment of Displacements of the Uterus with the Abdominal Spring Pessary, by J. McF. Gaston, M.D., of Columbia, S. C., (an ingenious contrivance to obviate some of the many objections to such instruments). A Paper on the Effects of Lead on the Heart, by John W. Corson, M.D., of New York; (a valuable paper on a subject but little un-
derstood). A Tabular View of the Signs furnished by Auscultation and Percussion, and their application to the Diagnosis of Diseases of the Heart and great Vessels, 2d ed., enlarged and improved, by O. Billingh, M. D., (a great convenience to students). Also, a number of Introductory Addresses, Circulars, &c.

American Physicians in Russia.—There are at present twelve American doctors serving in the Russian army in the south of Russia. Eight of these doctors, Marshall of California, Smith of New Orleans, Weems, Hank and Johnson of Baltimore, Md., Hart of Memphis, Tenn., Parks of Illinois, and Clarke, of New York, are stationed at Simpheropol, in the Crimea. Drs. Bostwick, of New York city, Oliver, of Boston, Mass., Morton, of Nashville, Tenn., and Smith, of Vermont, are stationed at Odessa. Thirteen others have served in the Crimea, of whom five have died there; seven have returned, and one died at Berlin, on his way to America. Dr. Draper, of New York, died of typhus fever at Sebastopol, on the 19th of March, 1855. Dr. King, of Charleston, S. C., died of typhus fever at Kertch, on the 20th March, 1855. Dr. McMillan, of New Orleans, died of cholera at Sebastopol, in June, 1855. Dr. Jones, of Maryland, died of cholera at Simpheropol, on the 24th of October, 1855, and Dr. Deninger, of Keeding, Pa., died of cholera at Simpheropol on the 25th of October, 1855. Dr. Stoddard, of Baltimore, Md., died at Berlin, on the 21st January, 1856. Over the graves of Drs. Jones and Deninger, a very pretty monument has been erected by their comrades, and at the time of the taking of Kertch by the allies, a monument to Dr. King was in process of construction, the monument being erected by the city authorities. Drs. Harris, of New York, Turnipseed and Davega, of South Carolina, Henry, of Mobile, Ala., Eldridge, of Maryland, Read, of Norristown, Pa., and Holt, of Georgia, have retired from the Russian service.—[Cor. N. Y. Herald.

The March number of the New York Journal of Medicine, contains an able paper from the pen of Dr. John W. Corson, "On the Effects of Lead on the Heart," in which the author arrives at the following conclusions:

1.—That allowing a due excess of force to carry on the embarrassed circulation in organic affections of the heart, it appears that certain symptoms in slow poisoning from lead, as well as in cardiac disease proper, typhus fever, and apparent death from catalepsy or other causes, all tend to prove that, as a rule, the impulse may be termed the pulse of the heart; and that, its more careful study than heretofore, may aid us in the general diagnosis and treatment of disease.

2.—That the symptoms of weakening of the heart in lead poisoning, are confined to cases of partial paralysis, or general muscular debility, accompanied usually by the purple streak of the gums, indigestion, constipation, pains in the head, muscles, or joints, and sometimes by lead jaundice; and that commencing and emphasizing with the most frequent, these heart symptoms from lead are:—weakened or soft tapping impulse; faintness on unusual exertion; feeble and generally slow pulse; palpitation; cardiac uneasiness; and to these are occasionally added, great despondency
or morbid fear of death; suspicions of organic disease of the heart, fainting fits, night-mare, or troubled dreams.

3.—That these depressing heart symptoms are absent in the earlier and more acute stage of lead poisoning, known as "lead colic," when, on the contrary, the stimulus of pain generally renders the impulse of the heart and the pulse at the wrist more firm than natural.

4.—That skill in the detection of minute variations in the impulse of the heart, naturally requires a little careful attention and practice.

5.—That these debilitating effects of lead most commonly occur in hearts previously sound, but they sometimes complicate existing organic cardiac disease from rheumatism or other causes.

6.—That the agencies or causes of lead poisoning are very numerous, and often obscure; and that slighter cases supposed to be ordinary dyspepsia, constipation, debility, or bilious colic, are frequently undetected.

7.—That the above tests of the immediate influence of lead on the heart in disease, are further corroborated by experiments upon animals; showing that, more mildly and slowly, lead, like digitalis, oil of tobacco, upas antiar, the wooara, and some other poisons, tends specially to paralyse the central organ of the circulation, and, like these, ultimately to produce what Bichat termed "Death by the heart."

8.—That the remedies for the paralyzing influence of lead may be divided into two classes:—Disinfectants, such as the iodide of potassium, and preparations of sulphur; and Antiparalytics, such as strychnia and electricity; that the best treatment combines these two elements; and that, on the whole, the most convenient and efficacious are free doses of the iodide of potassium, and minute proportions of strychnia or nux vomica.

9.—That the above conclusions are founded mainly on the evidence of ten cases, principally among the badly-nourished and improvident poor finally resorting to public institutions; and they may possibly be somewhat modified in future by more extended observation in private and more favorable practice.

Vaccine Matter.—As it may be interesting to physicians to know where fresh vaccine matter can be obtained, we insert the following:

"Physicians wishing Vaccine virus, by addressing Dr. Wm. Hutson Ford, City Vaccinator, Charleston, S. C., and enclosing one dollar, will receive, by return mail, ten points, or a set of glasses charged with fresh virus; or, by particular desire, a recent seab. Seventy-five points, or seven sets of glasses, will be sent for five dollars."

Results of some Statistical and Physiological Researches on Twins.—At a meeting of the French Academy of Sciences, Nov. 26, Mr. Baillarger read a paper containing the results of some statistical and physiological researches on twins. We have thought the subject presents some features which might interest the readers of the Examiner.

"Numerical distribution and relative proportion of the sexes in twin births; hereditary influence.

1. The facts group themselves into three categories:
"The first, two boys at a birth.
"The second, two girls at a birth."
"The third, a boy and a girl.

"The result obtained in computing 256 double births shows:—

Two boys in 100 cases.
Two girls in 58 "
One boy and one girl in 98 "

"It would seem as if the presence of two boys in twin pregnancies is almost twice as frequent as that of two girls. And also that the third class, that of the presence of the two sexes, is almost equal to the first.

"II. The solution of the second question, viz: the relative proportion follows from the above figures.

In 512 twin children are found: Girls, 214; Boys, 298.

"The number of boys exceeds that of girls, therefore, by more than one third. This result will certainly seem remarkable if we bear in mind that the proportion of the sexes in the totality of ordinary births, is of 16 girls for 17 boys. So that the difference is in the one case more than a third, and in the other, only a sixteenth. The relative proportion of the two sexes is governed then in twin pregnancies by special laws, quite distinct from those which govern normal births. This fact, interesting in itself, becomes still more so when compared with the documents already collected by M. Flourens on the proportion of the sexes in animals, in which the predominance of males over females is one sixth instead of one sixteenth. I would connect the great predominance of the male sex in twin births with another fact, which is deduced from the general statistics of births, and which at first sight may seem strange. I refer to the far greater proportion of boys among still-born children. This amounts to 17 boys to 12 girls. This singular predominance of boys among still-born children can, in my opinion, be in part if not fully accounted for by the excess of the male sex in twin births, which furnish, as is well known, a pretty considerable contingent to the statistics of still-born children.

"III. Twin births are hereditary in certain families, but in different degrees and different conditions. A large number of facts show, that the daughters of mothers who have had twin pregnancies, have often themselves two children at a birth. This disposition occasionally passes over one generation, when the grand-daughter instead of the daughter has one or several double pregnancies.

"The facts which I have collected would seem to prove that this hereditary disposition is transmitted also through the male. Some men would thus have the faculty of procreating two children at once, although no such hereditary disposition existed in their wives. This fact would have a great physiological importance, and I admit that it should be based on indisputable proofs. I merely indicate it now, and will return to it in a future paper.

"Before closing, it may not be improper to call attention to the fact, that the hereditary disposition of which I have been treating, seems to have been taken advantage of to obtain among animals, species which procreate two young instead of one. Flocks of sheep have thus been formed, consisting of individuals which normally bear two lambs. Single birth among them become the exception instead of the rule. I have seen a flock composed of nearly one hundred head of sheep, of which each ewe annually brings forth two lambs."
Robert Collins, in his Practical Treatise on Midwifery, gives the result of 16,654 births, occurring in the Dublin Lying-in Hospital, during a period of seven years, from 1826 to 1833. Among these, as appears in a table, pages 164 and following, (Am. Ed.,) there were 240 twin births, in which the sex of the children is mentioned. By a computation of this table, we arrive at results relative to the numerical distribution and proportion of the sexes, materially different from those which Mr. Baillarger's facts would lead us to expect. Thus in 480 twin children, there were: Girls, 234; Boys, 246; distributed in the following manner:

Two boys in . . . . 73 cases.
Two girls in . . . . 67 "
One boy and one girl in . . 100 "

By these figures, the presence of two boys in twin pregnancies is only one-eleventh more frequent than that of two girls, and the presence of the two sexes is more than one-fourth more frequent than that of two boys. The number of boys exceeds that of girls by only one-fifteenth, a result but little larger than that obtained from the totality of ordinary births, viz: one-sixteenth.

On the other hand, if we turn to the statistics of the same Hospital, as reported by Alfred H. McClintock and Samuel L. Hardy, for the three years of their connection with the Institution, from Jan. 1st, 1842, to Jan. 1st, 1845, (Practical Observations on Midwifery, p. 329,) we find that during that period, there were 6,634 births, of which 95 were twin births. In these there were: Girls, 79; Boys, 111; distributed thus:

Two boys in . . . . 38 cases.
Two girls in . . . . 22 "
One boy and one girl in . . 35 "

Here we are struck with the similarity of the relations existing between the above figures and those which Mr. Baillarger found to exist between the facts collected by him.

The presence of two boys is eight-elevenths more frequent than that of two girls, while in Mr. B.'s cases, it is twenty-one twenty-ninths, or the same thing.

The presence of the two sexes is less than that of two boys, as with Mr. B., though not quite so near being equal.

The number of boys exceeds that of girls more than one-third, as in Mr. B.'s cases, the ratio differing but a unit.—[Medical Examiner.

Albany Medical College.—The donations to the Albany Medical College have amounted to $75,000. Of this sum, $50,000 have been expended in buildings, land, and for the support of the institution during the four years of its existence—leaving a surplus of $25,000. Thirty gentlemen of Albany contributed to its permanent endowment $1,000 each.—[N. Y. Med. Times.

Asylum for Inebriates.—A petition is before the Massachusetts Legislature, praying for the establishment of an Asylum for Inebriates, wherein they may receive such treatment as shall restore them to soundness of health and sanity of mind; and also to afford such facilities as shall render the asylum a self-supporting institution.—[Ib.