

PROCEEDINGS
OF THE
TWENTY-SECOND ANNUAL MEETING.

TWENTY-SECOND ANNUAL MEETING

— OF THE —

Oregon State Medical Society,

— HELD IN —

PORTLAND OREGON.

June 11 and 12, 1895.

HOTEL IMPERIAL, June 11, 10 A. M.

The president, Dr. J. A. Fulton of Astoria, called the meeting to order.

Minutes of last meeting read and approved.

A supplementary programme was then announced.

Dr. J. D. Fenton of Portland delivered an address of welcome.

A motion was made by Dr. G. M. Wells that ten minutes be allowed for opening and closing and five each for regular discussion. Carried.

Motion made by Dr. Cauthorn that the reader of a paper be allowed to call on some one to open the discussion. Carried.

Dr. C. H. Hall then read a paper on "Heredity."

Discussed by Drs. Doane, Bailey and Tucker.

Adjourned to 2 P. M.

AFTERNOON SESSION.

Meeting convened at 2 P. M.

Dr. Richmond Kelly of Portland read a paper on "Obstetric Complications."

Discussion by Drs. Josephi, Bailey and Thos. Parker.

Dr. E. F. Tucker of Portland read a paper on "Retroadjustments of the Uterus; Their Treatment by a New Method."

Discussion by Drs. Giesy, Holmes and Josephi.

Dr. H. R. Holmes of Portland read a paper on "Latent Gonorrhoea."

Discussion by Drs. Tucker, Thomas Parker, Bailey, Cauthorn and Wells.

Dr. T. C. Humphrey of Portland read a paper on "Alkaloidal Medication."

Discussion by Drs. Robinson, Parker, Doane, Wells and Cauthorn.

Drs. E. H. Chambers and E. H. Thornton of Portland, and O. H. Beckman of Astoria were elected members of the society.

Motion made that 2 o'clock, June 12, be set as hour for election of officers.

Adjourned till 8 P. M.

EVENING SESSION.—8 P. M.

Dr. G. M. Wells of Portland read a paper on "Combined Taxis."

Discussion by Drs. Cauthorn, Holmes and Doane.

Dr. F. Cauthorn of Portland read a paper on "Pathology of Some Unusual Conditions in Strangulated Hernia."

Discussion by Drs. Smith and Carll.

Dr. O. D. Doane of The Dalles read a paper on "Membranous Enteritis," with presentation of a specimen.

Discussion by Drs. Hall, Coe, Holmes, Walls, Mackenzie and Wells.

Meeting adjourned until 10 A. M., June 12.

JUNE 12—10 A. M.

Meeting was called to order by President Fulton.

In the absence of Dr. Cauthorn the secretary, Dr. Tucker was elected to fill his place.

The regular order of business was taken up.

Dr. J. T. Walls of Portland read a paper on "Neuropathology."

Discussion by Drs. Coe, Parker and Belknap.

Dr. F. M. Robinson of Beaverton read a paper on "Immediate Repair of Lacerations of Cervix."

Discussion by Drs. Moore, Parker, Tucker, Mackenzie, Giesy, Binswanger and Doane.

Adjourned.

AFTERNOON SESSION—2 P. M.

Special order; election of officers.

Nominations of President—Drs. Linklater of Hillsboro, Doane of The Dalles and Tucker of Portland.

Dr. Doane was elected president on second ballot.

Dr. F. M. Robinson of Silverton was elected vice president by ballot of secretary.

Dr. E. F. Tucker of Portland was elected secretary by ballot of secretary.

Dr. Mae H. Cardwell of Portland was elected treasurer by ballot of secretary.

Dr. A. C. Smith of Portland was elected a member of the executive committee.

Portland was selected as the next place of meeting.

At this time the question of Dr. A. E. Rocky being ap-

pointed by the American Medical Association at Baltimore as a delegate to the congress of physicians to be held at Berlin, whereby he appeared to go as the representative of the State of Oregon was brought up and discussed at considerable length which finally terminated in the adoption of the following resolution offered by Dr. Wells:

Resolved, That Dr. A. E. Rockey does not represent the profession of Oregon; and the Oregon State Medical Society hereby repudiates any action accrediting Dr. Rockey as a representative of the profession in this state.

The motion was seconded by Dr. Eaton.

Dr. Holmes thought the resolution was a little too broad and suggested some amendment. Dr. Strong thereupon moved that the words, "That he is not a member of the Oregon State Medical Society," be inserted in order to make the resolution more specific; and Dr. Josephi moved that the words also be added, "Or any other reputable medical society in this state."

The resolution as amended was then read by Dr. Wells as follows:

Resolved, That Dr. A. E. Rockey does not represent the medical profession of Oregon; and the Oregon State Medical Society hereby repudiates any action accrediting Dr. Rockey as our representative or as any representative of the profession in the State of Oregon as he has no membership in this Society or any other society in correspondence with this body.

After some further discussion the resolution was adopted.

Moved by Dr. Amos that the installation of officers be postponed until after the reading of all papers and until after all other regular business of this session has been finished. Carried.

Dr. C. C. Strong of Portland read his paper entitled "Can Female Diseases be Cured?"

Discussion by Drs. Wm. Jones, A. C. Smith, Tucker, Linklater, Holmes and Robinson.

Dr. William Jones of Portland read a paper on "Dangers of Intra-Uterine Instrumentation."

Discussion by Drs. Smith and Moore.

In the absence of Dr. Fulton, Dr. Coe occupied the president's chair.

Dr. Mae H. Cardwell of Portland read a paper on "Diseases of Children."

Drs. Lewis J. Belknap and B. J. MacDonald of Portland were elected members of the society.

Dr. W. F. Amos of Portland read a paper on "Anesthesia."

Discussion by Dr. Cauthorn.

Dr. M. H. Ellis of Albany was elected a member of the society.

Dr. J. C. Perry, of the U. S. M. H. elected honorary member of society.

Dr. Geo. F. Koehler of Portland read a paper on "Principles Involved in the Study and Treatment of Disease."

Papers by Dr. Henry W. Coe of Portland on "Locomotor Ataxia," and Dr. W. E. Maxwell of Portland on "Eczema," upon request of writer were read by title.

A vote of thanks was given to M. Guinean, of the Imperial Hotel, and to the retiring officers.

Dr. J. A. Fulton of Astoria read his "President's Address."

The new president and secretary were inducted into office.

Society adjourned on motion.

ORIGINAL COMMUNICATIONS.

HEREDITY.

BY C. H. HALL, M. D. SALEM, ORE.

Hippocrates on page 216 of the first volume of his great work says, respecting dropsy, consumption, gout and epilepsy that, "if they are hereditary, they are difficult to remove." This is all that the reputed father of medicine says, concerning this obscure dogma. Moses speaks of the iniquity of the fathers being visited upon the children unto the third and fourth generation. From these ancient authors, teachers and writers on medicine have generally accepted as axiomatic the obscure and unexplained doctrine of heredity. Now, almost all text books on the practice of medicine attribute many pathological states to the influence of this cause. This error has probably been intensified by many pathologists in a failure to distinguish between a post hoc and a propterea hoc. A man dies from the effect of cardiac dropsy induced by habits of intemperance, his son becomes a drunkard and dies from cardiac dropsy which all admit is a post hoc; but to attribute the son's cachexia to the father's sin would be evidently straining the *propterea hoc*. For if one son of a drunken father dies from drunkenness, does this argue for heredity, while all the other sons and daughters born of the same parents and begotten under the same laws, are entirely free from the father's malady.

C. D. M. asks: "Is it sure that a calf whose mother was suffering from tuberculosis will be afflicted with that disease?"

Reply.—"No. There has been a test made in Saxony when 16.5 per cent. of the cows had the disease, but 2 per cent of their calves were diseased. At Lyons, out of 400,000 calves that were slaughtered only five were diseased."

I think this proposition is quite capable of demonstration.

Mutilations of the body cannot be transmitted. All pathological conditions are mutilations. By the term mutilations I mean all and every kind of pathological conditions, whether acquired or congenital.

When I was a student of medicine, I remember with what laconic eloquence our professor of obstetrics, told us of an accouchment case he attended in Ohio, where the baby was born with a perfect body, head, arms and trunk, and the legs and tail of a black Newfoundland dog. He told us he was where he could observe the growth of this girl until she was 18 years old. He saw her often during these years, and when she had grown to be a handsome young lady in long dresses she wore the tail and hind legs of the enormous dog which had frightened the mother during her period of gestation. How do we account for this wonderful phenomenon? Just in the same way we do for the little trick that Esau played on his brother Jacob, when he was to have all the ringed-streaked and speckled calves produced by the cows of the flock being exposed to the influence of striped hazel twigs thrown around in the pasture when the cows grazed. It was a myth. I believe it demonstrable that the actual existence of the transmission of acquired mutilations cannot be proved.

There are a small number of observations made upon man, and the higher animals, which seem to prove that injuries or mutilations of the body can under certain circumstances be transmitted to the offspring. A cow which had accidentally lost a horn, produced a calf with a rudimentary horn; a bull which had accidentally lost its tail, begat tailless calves; a woman whose thumb had been crushed and malformed in youth, afterwards had a daughter with a malformed

thumb. While these individual examples may confront us, we must on the other hand, remember the countless instances of mutilations of bodily organs which are not transmitted. There are no observations which prove the transmission of functional hypertrophy or atrophy, and it is to be hardly expected that we shall obtain such proof in future, for cases are not of a kind which lend themselves to an experimental investigation. The hypothesis that acquired *characters* can be transmitted, is therefore only indirectly supported by these individual instances of the transmission of mutilations. It can hardly be doubted that mutilations are acquired characters; they do not arise from any tendency contained in the germ, but are merely the reaction of the body under external influences. They are purely somatogenic characters—characters which emanate from the soma or body only, as opposed to the germ-cells; they are therefore characters which do not arise from the germ itself.

If mutilations must necessarily be transmitted, or even if they might occasionally be transmitted, a powerful support would be given to the somatogenic principle, and the transmission of functional hypertrophy or atrophy, would become highly probable.

At a meeting of the Association of German Naturalists, at Wiesbaden, in 1887, there was reported a generation of tailless cats. These cats were said to have inherited their rudimentary tails from a mother cat, *which was said to have lost her tail by the wheel of a cart having passed over it.* Not only did the owner of the cats, one Dr. Zacharias, consider them as a proof of the transmission of mutilations, but in a recently published work on the Origin of Species, based on the transmission of acquired characters, by Prof. Eimer, the author speaks of the cats as a valuable instance of the transmission of mutilations.

Cats have often been born with rudimentary tails whose mothers had perfectly formed tails, and whose fathers could not be identified. These are innate monstrosities which have

arisen from unknown changes in the germ, as often occurs with children born with hair lips and imperfectly formed limbs. A few months ago I confined a lady with her first child, a perfectly formed boy of eleven pounds whose ears were firmly pressed forward, and gave to the child the appearance of having no ears at all. The grandmother of the child at once was horrified, and sang out in piping tones and said to the father of the child, "I told you not to cut off that dogs ears until after the baby was born." But after a few days with the application of adhesive plaster, the ears assumed their normal condition and remain permanently so.

The rudimentary tails in cats and dogs as far as they can be submitted to scientific investigation, do not depend on transmission of artificial mutilations, but upon the spontaneous appearance of degeneration in the vertebral column of the tail.

Prof. Weisman of the University of Freiburg has conducted a series of experiments on the tails of white mice, begun in October, 1887, with seven females and five males. On October 17th, all their tails were cut off and on November 16, the two first families were born. These two families were together, eighteen in number, and every one had a perfectly formed tail 11-12 mm. in length. These young mice were removed from the cage and their tails amputated. Twelve of them were placed in a separate cage and 333 young were born in fourteen months, up to Jan. 16, 1889, and not one of these had a rudimentary tail. Thirteen mice of the fifth generation were again isolated and their tails amputated. By Jan. 16, 1889, 41 young had been born with perfectly normal tails. Nine hundred and one mice had been produced in five generations of mutilated parents and not a single example of a rudimentary tail.

I have been familiar with the sheep farming of the Hon. John Minto of Marion county, Oregon, for the last 38 years. Every spring he cut off the tails of all the lambs of his flock of Merino sheep, and yet in all these years not a lamb has

been born with a rudimentary tail. For 4,000 years the descendants of Abraham have protected the sacred rite of circumcision and in all these 40 centuries not a child has been born with an abbreviated prepuce. If therefore, mutilations really act on the germ plasms as the causes of variation, the possibility or even the probability of the ultimate appearance of heredity effects could not be denied.

Settergast tells us, that the various species of crows possess stiff, bristle like feathers around the opening of the nostrils and base of the beak. These are absent only in the rook. The rook, however, possesses them when young, but soon after it has left the nest they are lost and never reappear. The rook digs deep into the earth in search of food, and in this way the feathers at the base of the beak are rubbed off and can never grow again because of constant digging. Nevertheless this peculiarity which has been acquired since the first pair of rooks left the hand of the Creator, has never led to the appearance of a newly hatched rook with a bare face.

If in all these visible mutilations, transmissions are not traceable, so we may conclude that pathological conditions are not hereditary. Children may be born with a cachexia which may render them susceptible to scrofula or phthisis, or gout, but no child is born with the diseases of its parents, or inherits the mutilations of its ancestors. If Hippocrates and Moses hinted at the dogma of heredity, it would seem that Paul had eloquently espoused the converse theory when he said: "For God hath made of one blood all nations of men to dwell on all the face of the earth." If heredity were a law, and all the colors and races and species of the animal and vegetable kingdoms sprang from a single pair of creative power, how could we account for the present individual diversity. *Environments evidently produce variations in species.* There is no reason for the assumption that amputating the tails of mice for a thousand or ten thousand generations would produce a race of tailless mice.

The supposition of the accumulative effect of mutilations is entirely visionary, and cannot be supported except by the fact that accumulative transformation of the germ-plasm occur; but of course this fact does not imply that mutilations belong to those influences which are capable of changing the germ-plasms. All the ascertained facts point to the conclusion that they have not this effect. The more striking the mutilation the more improbable the transmission in all cases relied on for evidence.

A veteran soldier with an empty sleeve, does not beget sons and daughters with abbreviated arms, but because the physician cannot demonstrate any other cause for phthisis or scrofula, he recklessly attributes its appearance to heredity. But it seems to me that a mutilation of the lungs broken down by tuberculosis would be just as impossible to transmit as an amputated arm or a scrofulous scar. The opinion has often been expressed that transmissions need not occur in every case, but may happen now and then under quite exceptional conditions, with which we are unacquainted; for this reason it might be urged that all negative experiments and every repetition of "proofs" of transmission of mutilations are not conclusive. Only recently a very clever young zoologist said in reference to Kant's statements upon the subject, that perhaps the most decided opponent of the transmission of mutilations would not venture nowadays to maintain his views with such security, it must be admitted that transmissions may take place as a rare exception. But if heredity were a law, then it should certainly affect every thing alike, which is produced or begotten under the influence or impetus of like condition. If one six-fingered child is born in a family why are they not all six-fingered. If one hair-lipped child is born in a family why are they not all hair-lipped. The exceptions to the law are more than the examples, and destroy the force of the law.

Similar opinions are often expressed especially in conversation, and yet they can mean nothing except that transmis-

sion of mutilations has been proved; for if such transmissions can take place at all, it exists and it does not make any difference theoretically whether it occurs in rare cases or more frequently. Sometimes heredity has been called capricious, and in some sense this is true. Heredity appears to be capricious because we cannot penetrate its depths; we cannot predict whether any peculiar character in the father will reappear in the child, and still less whether it will appear in the first, second or third or fourth generation: we cannot predict whether the child will be born with a nose resembling the father or mother or one of the remote ancestors. *Characters are made and not born*, and physical conformation is the result of culture or the lack of it. These are not results due to chance. No one has the right to doubt that everything is brought about by the operation of certain and definite laws, and that with the fertilization of the egg, the shape of the nose of the future child has been determined. The co-operation of the two tendencies of development contained in the two conjugate germ-cells produces of necessity a certain form of nose. The observed facts enable us to know something of the laws under which such events take place. Among a large number of children of the same parents some will always have the form of the nose of the father, others that of the mother, but this is congenital and not heredity, and differs from the transmission of mutilations and pathological conditions. *All organic life exists either in a physiological or pathological state.* Pathological functions are incapable of physical action. Mutilations of the body cannot participate in the physiologic act of reproduction. The physiological ovum is fertilized by the somatogenetic cell, and whatever mutilations occur afterwards must be the result of environment.

DISCUSSION.

DR. HALL asked Dr. Doane to open the discussion upon the paper.

DR. DOANE: This is a subject I have not paid any particular attention to. The thoughts presented, while not entirely new, are certainly deserving of some study before attempting to further discuss the subject. Whatever may be the individual opinion on this question of heredity, I think we will all agree with the opinions that were first announced years ago, and that the doctrines that have been heretofore taught must be in the light of present developments very much modified. The germ theory as it is generally accepted has upset a great many of our preconceived notions, and probably it has overturned many ideas in connection with this theory of heredity. I was impressed not very long ago with an editorial in the *Medical News*, which doubtless many of you have read, upon this subject. I can only present one or two thoughts that were presented there—not at all original with myself. The idea was advanced that diseases such as consumption (tuberculosis, to use the truer term), rheumatism and gout, and kindred pathological conditions were not inherited, but were produced solely, or principally at least, or the tendency to them, by environment. It is a common saying to-day that we no longer inherit consumption, but that we inherit the tendency to consumption, or that weak faulty condition of the body which makes one so unfortunate as to inherit that, prone to take on consumption on very slight cause. We are all more or less exposed to the germs of tuberculosis; we are all more or less exposed to the germs of other diseases, and if we were all equally susceptible to the effects of those germs we would all probably fall victims to disease. I doubt in my mind whether even the tendency is inherited, but is not the tendency a product of environment and influences brought to bear after birth rather than before.

DR. TUCKER: I think I understood Dr. Hall to say that

no pathological condition existing in the father or mother could be transmitted by the physiological act of reproduction to the child. I would like to ask him how he accounts for the transmission of syphilis?

DR. HALL: Mr. President, I may have been carried to an extreme measure upon this subject. During the ten or twelve years past I have taken every occasion to study the question in every way and I most emphatically deny the possibility of the transmission of syphilitic disease to posterity; for the simple fact that the organs of the body which are affected by syphilis could not by any possibility transmit a somatogenetic cell that could be restored into life, for the disease itself would nullify any reproductive power that might exist in any cell which could become a factor in reproduction.

DR. BAILEY: Mr. President, I was impressed with the scientific aspect of Dr. Hall's paper. There are some conclusions reached, however, to which I should dissent, and especially with regard to syphilis. I think it is pretty well established from scientific researches and from common observation that syphilis is transmissible. And I might ask Dr. Hall how it would be in the case of some of the exanthemata that occur in the female in gestation; instances in which variola and rubeola, all affect the foetus in utero and render it immune to attacks subsequently in after life. The tendency to inherit diseases is an expression that is rather vague; but it is taking the place of statements to the effect that diseases are inherited. There are individuals with certain form of constitution, build, physiognomy and conformation that are more liable to some forms of disease than others, and that is just about all that they inherited from their ancestors of certain diseases that are produced, for instance, by a particular germ. A part of the paper I hardly know whether to take seriously or not—in which the doctor flies in the face of Holy Writ to question in some degree the accuracy of the statement as to what happened with the

striped cattle that we all learned about in our Sunday school lessons, and those beliefs that we perhaps inherited from our ancestors. But with regard to tailless rats and non-mewing cats, and all that kind of thing, the short space of time man has been on the earth as we now believe is a very limited period of time. Under Darwin's observations, if we could go back and trace a few millions of years we might say that we could develop generations in animals by breeding, that lost certain organs that became useless. Fish, for instance, in the caves that never see the light from generation to generation are without eyes. Some writer, I can not call to mind who it was, describes the cattle in remote islands where flies had never been known, that had nothing but a rudimentary tail, having no use for it. I merely throw out these ideas and observations. The paper is one of interest.

DR. STRONG: Mr. President, I have listened with a good deal of pleasure to that paper, because it is one that contains ideas very strongly embodied in the medical minds that certain characteristics are transmitted from our ancestors. We cannot see any particular relation between tailless mice and the subject of heredity, because that is more in the line of mutilation, while the law of nature intends as a whole to transmit a perfect animal, and therefore the general trend would not be to transmit a mutilated animal. I can see no reason to illustrate the one with the other. I can conceive also of the possibility under the present germ theory and its processes of development, the possibility of germs being transmitted as a part and parcel of the elements which are transmitted and go to form the new creature. I can see no reason why germs that produce tuberculosis should not be transmitted. I think it is a fact the profession would very slowly give up that syphilis is not transmitted, and therefore I think the illustrations are not appropriate between mutilation and the transmission of such diseases as we have mentioned. Dr. Hall's paper abounds in facts and statements which are convincing from the writer's study of the

question, but I must recognize the fact, if I heard the paper right, that if his quotations are all as his quotations from Holy writ, then I think there are some errors; for if I remember Holy Writ correctly the ring-streaked and striped cattle were not the issue between Jacob and Esau but between Jacob and his father-in-law. (Laughter)

DR. HALL: The question was not about the names of the people, but about the cattle. I may be mistaken in the genealogy of the people but I am sure I am not mistaken in the fact.

DR. BELKNAP: So far as my experience and education goes it seems to me it would rather knock the underpinning out of some of our previous ideas to entirely do away with the theory of heredity. We find what we call the neurotic tendency or nervous type; then we find the phlegmatic type. We find these different types and also find in different families or races, weak lungs or a tendency to weak lungs. Now, in testing with the spirometer, which I have used a great deal in practice in the last few years, I find about 220 to be the average, and there are some cases that will go much beyond this and some much below. Some even below five feet will go but a little above 100 or 150 after repeated tests and we find that tendency to weakness in certain persons. Now, the role that the germ plays in most of these patients is this: of course we are all subject to breathe atmosphere impregnated with various germs, why do we not all succumb or have these affections? Simply because as I explain it, of degrees of health, etc. If a person is in good healthy condition he has a tendency to resist disease. If a person has weak lungs and he is taking cold frequently as he may along through life, his lungs become still weaker and if he then in certain conditions breathes these germs of tuberculosis into his lungs they get a footing and the system is not able to throw them off and then he has the disease.

There being no further remarks offered, the Chair called upon Dr. Hall to close the discussion.

DR. HALL: So far as the illustration is concerned which Dr. Strong spoke of, I mentioned those illustrations which were visible and acknowledged by all, for the visible mutilations of the body are not hereditary. I suppose that is capable of demonstration, and those invisible mutilations which we call pathological conditions, while they are in a great measure inscrutable and undemonstrable, they are thought or said to be hereditary by those persons who will not admit that any mutilations of the body are transmissible.

So far as Dr. Bailey's question is concerned as to the transmission of syphilitic virus, as he is a theological gentleman, I want to remind him that there is another place in the Bible where it says, "Whatsoever a man soweth that shall he also reap." So that if he sows syphilis he may reap it, but it would be impossible to sow syphilis and produce children; they do not grow from that kind of germ. If a florist expects to get a variety of flowers or shrubs in his collection he only selects the purest buds for germination or inoculation or for grafting; and those buds do not produce trees or shrubs that are grown over with caterpillars and black aphid, but they take on those things after they produce full grown trees. The aphid is not in the bud, but it is taken on in the growth of the tree. While the doctor wonders about the possibility of syphilis being hereditary, the very fact, as I take it, that the syphilitic germ or syphilitic tendency affects the somatogenetic germ, it destroys its power of reproduction. You can not make a syphilitic germ produce a child; you can not make an aphid seed produce a rose. But the healthy, wholesome bud taken from its healthy stalk and transmitted to a healthy parent will produce that kind of germ which is in the bud growth itself. So it is with the syphilitic tendency or tuberculosis tendency, or any other. The diseased germs themselves do not reproduce beings, but if they produce anything it is disease. The disease-germ itself is not capable of reproducing one of its own species.

DR. BAILEY was granted permission to ask Dr. Hall this

question: If I understand the Doctor right, he takes the position then that as the syphilitic germ does not reproduce itself, if a syphilitic father reproduces his kind he reproduces as healthy offspring as if he had not syphilis.

DR. HALL: Yes.

DR. BAILEY: That is what I object to in the Doctor's theory.

DR. STRONG: I would like to ask where the syphilis comes from when the child is born with well marked syphilis? Where did the child get it?

DR. HALL: The question of inoculability of the foetus may be as possible as the inoculability of mature persons with independent life. The foetus in utero is not less subject to the attacks of disease or deformities or accident on account of the environment than is a grown person who has assumed an independent life; and many of these diseases that we call hereditary are contracted during the stage of utero-gestation.

OBSTETRIC COMPLICATIONS.—A CASE FROM PRACTICE.

BY RICHMOND KELLY, A. M., M. D., PORTLAND, ORE.

Dean and Professor of Obstetrics in the Medical Department of the Willamette University; Obstetrician to Portland Hospital, Etc.

Mrs. B., aet. 28, multipara; fairly developed, poorly nourished, had a delivery of twins at seven and one-half months of uterogestation with a protracted convalescence; had an abortion at close of second month of gestation in the spring of 1894, with retention for weeks of the secundines and profuse, protracted hemorrhages. So excessive had been the vascular loss that the normal color of the skin had never been restored. Personal history otherwise and family history negative, as bearing upon this case. Negative, with a possible exception of the mental state which, from domestic infelicity and financial reverses, had grown into such a condition that she had become wholly indifferent as to results in her case.

Patient ceased menstruating in September, 1894. The first half of pregnancy was attended with unusual disturbances of the digestive system, with marked dyspnoea on exertion, the latter due to irregular action of the heart and a hydroemic state of the blood. During the next six months of pregnancy she observed some blood in the excessive leucorrhœa with which she was at the time suffering. The color continued periodically for one month, when at one time she lost several ounces of partly fluid and partly clotted blood. Thereafter the color was never absent.

On April 13, 1895, pain was sufficient to determine a call of the physician. The general condition was not reassuring; surface pallid, subcutaneous tissue inclined to puffiness, appetite poor, bowels irregular; mental state depressed;

heart slightly dilated, hypertrophied and irregular in rhythm; urinalysis negative; pulmonary sounds normal; abdominal tumor unusually prominent, particularly in its transverse measurement; parities unusually sensitive to touch.

Digital examination revealed a dorso-anterior transverse presentation, the head directed to the left. The dorsum of the child inclined slightly downward. The left cervical zone was occupied by the placenta which extended to the os interum and rendered futile tentative efforts at cephalic version. The hemorrhage had at no time been alarming and the expectant plan of treatment was pursued with instructions as to the avoidance of active exercise.

Shortly after midnight of April 30th the membranes ruptured, there previously having been considerable pain, lancinating in character, through the abdomen and thighs. The escape of liquor amnii was attended with freedom from pain which immunity continued to the last. The patient was seen at 1 A. M., May 1st. Practically the same local and general condition prevailed as recorded at the former visit. Hemorrhage had continued throughout the month in a desultory manner. Efforts at correction of the malpresentation by cephalic version were again made without practical results, because: First, the presence of the placenta in advance of the head; second, extreme sensitiveness of the abdominal walls; third, moderate retraction of the uterus about the child. At 9 A. M., there had been no dilation of the os, no uterine contractions, no hemorrhage. At 11 A. M., the same observations were made. At 5 P. M. the maternal condition still remained the same with no uterine action except more marked retraction. The fetal heart, however, had become distinctly less vigorous and had increased in frequency from 140 to 180.

The exigencies of the situation were presented to the husband with the conviction that operative interference would be demanded, and that very soon, in the interest of the child. The advisability of council was presented and declined. At 6 P. M., after a thorough preparation, the patient was anaes-

thetized by the nurse, chloroform being used. The hand was with difficulty passed through the undilated cervix, the right foot grasped and drawn into the vagina, the head being at the same time dislodged. A tentative effort at reaching the left foot proved ineffectual, on account of the undilated condition of the cervix, and was abandoned. Traction was renewed through the medium of the right leg, and without excessive effort the breech descended into the vagina and a partly asphyxiated child was delivered at 6:25 P. M. Attempts at resuscitation of the child were speedily successful. The uterus did not contract vigorously and was held by supra pubic pressure until a drachm of fl. ext. of ergot produced some effect.

The presence of a small stream of blood escaping from the genitalia prompted a digital examination, when a laceration of the cervix upon the left side was discovered extending from the oral margin through the vaginal junction, and to, if not quite into, the uterine body. The laceration was produced at some stage in the delivery between the delivery of the right leg and the escape of the head. The stream was slightly pulsating, indicating an arterial lesion. Attempts at arrest of the hemorrhage were rapidly made in addition to the use of ergot, first, with the instrumental appliances at hand; second, with cotton tamponades crowded into the uterus cervical canal and vagina with firm counter pressure over the fundus. Notwithstanding these efforts, the patient was perceptibly failing.

The same red ominous stream from the genitalia continued. Messengers were dispatched for assistance, but it would be not less than an hour and a half before help could arrive. In the meantime pressure was continued, but it was becoming more irksome on account of the relaxation of the uterus due to failure of the reflex activity of the cord. The patient complained of the pressure, begged to be allowed to die, became restless, tossed about and was with difficulty restrained. The moments passed like ages under the trying

circumstances. Each moment the radials were becoming weaker, the movements spasmodic. The hour and a half came and went—three hours—and at 10 P. M. the unequal struggle was over, and assistance had not arrived.

The source of the hemorrhage was two-fold—from the divided arteries in the cervix, enlarged because of the proximity of the placenta, and from the uterine sinuses, which the anatomical relationship of the walls would not permit to close in the usual way. With skilled assistance sutures might have been passed through the vaginal vault so as to have included the divided tissues. Such a procedure might have been successful. Uterine and vaginal packing were only temporary expedients. Ergot was useless.

I find, in the literature at hand upon the subject, no exactly parallel case. The abdominal implantation of the placenta was probably due to the lowered vitality of the endometrium, pregnancy resulting as it did so soon after neglected abortion with its concomitant evils. The problem of prevention was a delicate one for satisfactory solution. Could the death of the mother have been more than problematically determined, time, which certainly would have decided the death of the child, might have been less harsh with the mother by either securing a more dilatable condition of the cervical tissues, or have secured assistance, or both. The friability of the tissues, with the consequent hemorrhages, could not have been certainly determined.

In cleansing the child, which was a well developed, well nourished female of eight and one-half pounds weight, the nurse discovered a laceration of the perineum which, upon inspection, was found to extend from the posterior margin of the fourchette to the anterior anal margin, in the median line, measuring one inch in length and three-eighths of an inch in its greatest depth. There is in mind no satisfactory hypothesis to account for the laceration. Literature is silent on the subject. Traction was not extreme and not made in an unfavorable direction.

Dr. Cauthorn, who arrived shortly after 10 o'clock, drew the margins of the laceration together with a single suture and the subsequent recovery was satisfactory and uneventful.

DISCUSSION.

By request of Dr. Kelly the discussion was opened by,

DR. JOSEPHI: Mr. President and Gentlemen, the paper just read is a very interesting one, and I think we will all agree with Dr. Kelly in his statement that it is the duty of the physician or the surgeon to report his bad cases as well as his good ones. This case he has reported is certainly from the stand point of results a very bad one, and from the stand point of interest to the profession a very good one. I did not understand Dr. Kelly to say what the condition of the placenta was or to say anything as to the separation of the placenta; so that I am unable to say whether this hemorrhage was assisted greatly by the uncontracted condition of the uterus by reason of the non-expulsion of the placenta. It seems to me it would have been wise to have resorted to saline injections; a copious rectal injection of salt solution, which as we all know, has often resulted in keeping up the flagging powers until hemorrhage has been staid or until the system could recover itself. We all know how these after-thoughts come in these and similar cases, so that I do not mean to impute any blame to Dr. Kelly in not having thought of this expedient, even if he did not think of it, of which I am not convinced.

There is one other point of which Dr. Kelly spoke that struck me as being a little peculiar, and that is that supra-pubic pressure alone was made until he discovered that there was a pulsating stream of blood flowing from the genitalia. He then made an effort to determine the source of this stream and found a lacerated cervix. I think I may have misunderstood his meaning, but it seems to me that in the treatment of such a case it is wise to use the combined method of in-

ternal and external pressure, placing the whole hand if necessary in the vagina doubling the fist behind the cervix and pressing it forward and using the supra-pubic pressure as counter pressure in order to hold the vessels of the uterus as much as possible under control. Of course the remedy for this condition lay primarily in the changing of the position of the child. Dr. Kelly says in his paper that he tried faithfully to produce cephalic version. I suppose he means by the external method in the first place and attributes his failure to the position of implantation of the placenta, as well as the contracted position of the uterus upon the child. This, of course, would not apply before the rupture of the membranes if there was a goodly quantity of liquor amnii, nor would it apply particularly to the condition of placenta prævia, but I presume in this case there was a small quantity of liquor amnii which accounted for the close pressure of the uterine walls upon the child, or some condition of that kind which prevented the success of the effort at turning. I do not well see under all the conditions—the condition of the patient and the lack of assistance—how Dr. Kelly could have succeeded any better unless he had resorted to the saline injections which I have already mentioned and which probably occurred to him at the time, but which under the peculiar circumstances of the case he could not have access to. I need scarcely mention that if the rectal injections failed, the transfusion of saline solution into the veins is also to be strongly commended.

DR. PARKER: I would like to ask Dr. Kelly, if he had had another case like it whether he would not resort to the Cæsarean operation?

DR. BAILEY: I don't know whether I heard distinctly as to the extent to which ergot was tried. I understood Dr. Kelly to say that a drachm of the fluid extract of ergot was used, but I did not understand whether it was used hypodermically or given by the mouth. I should have depended pretty largely upon the use of ergot as an assistant, but of

course any lacerated arteries of any considerable size could not be controlled in that way. The suggestion of Dr. Josephi of the use of saline injections is good, but it may be questioned if it should have been employed in a case attended with such difficulties as the one detailed. I merely make the inquiry relative to ergot as I think it is most generally understood that its efficiency, the rapidity of its action among other remedies, is greater when employed hypodermically.

DR. KELLY: I would like to hear from Dr. Giesy.

DR. GIESY: In these cases of hemorrhage (I don't know much about them, never having had any) I have always made it a rule to release or expel the after-birth just as quickly as it is possible after I get the child. I don't know whether the after-birth was expelled or not. May I ask that question?

DR. KELLY: Yes, sir, it was expelled almost immediately following the child.

DR. GIESY: Well, I do not know that I could say anything then, because the ergot was given, the manipulation was made, and it must have been one of these cases where the blood was so thin and the parts so relaxed that there was no contraction.

DR. KELLY: The only answer I have to offer with reference to the comments that have been made is that they are almost too friendly. I had not really hoped but I had feared there would be some adverse criticism offered. With reference to the placenta, it was spread as I think over pretty much all of the dilatable zone; it was spread over it to the extent of two inches. The patient had no hemorrhage from the ruptured membranes until delivery, and the delivery was distinctly in the interest of the child and not of the mother, because the mother's condition was good. After the delivery of the placenta, which occurred within ten minutes after the birth of the child, it could be readily noticed that the margin of it showed the presence of clots that were of some days and possibly weeks standing, days at any rate, from the character

of that area of the placenta. Otherwise the placenta was normal and was speedily delivered.

Now, with reference to saline injections; that was thought of, but the house was in an out-of-the-way district some distance from skillful assistance, and there was no intelligent help to be had in the house, and furthermore the saline injections, just as the use of ergot, would only afford temporary relief. No doubt the use of enteroclysis or hypodermic clysis would have tided the patient over for a period; but the hemorrhage continuing, the same condition would sooner or later have resulted which did result, unless the hemorrhage could have been arrested at the proper point. There was no one to give injections and I thought it questionable as to the propriety of them from the standpoint that the mouths of the vessels were open and exposed, not only the divided arteries, but the sinuses, which could not contract by any condensation the uterus could produce. The uterus did contract originally and was fairly good until the spinal cord was interfered with in its functions and then it again relaxed. Cephalic version was a failure originally, because of the extreme sensitiveness of the abdominal walls. The patient was almost hysterical. She would scream at the slightest touch, and of course I took a hopeful view and thought it could be turned at the proper time. If it had not been for that abnormal condition of the abdominal walls cephalic version could have been performed at the first visit.

I think, knowing what I do at the present time, the dangers of Cæsarean section would not have been greater than those the patient ran. The cervix was never dilatable; it seemed to be very rigid and maintained that condition from first to last, and therefore possibly the section might have given her a better opportunity for her life.

Ergot was given in drachm doses. The vessels were of such a character and so situated anatomically, that ergot would not have reached the place and had the desired effect.

Hemorrhage began immediately after the expulsion of the child's head, and I mean by expulsion or by counter pressure over the pubis, the counter pressure which followed the child as it escaped from the genitalia. So soon as the child and the placenta were delivered then pressure was made from the vagina as well as through the fundus uteri. Hemorrhage began immediately after the expulsion of the child and was identical in character to that which followed the expulsion of the placenta.

I had hoped that some comments would be made or the experience of some present would have been given to us on the laceration of the cervix. That is something that is entirely new to me; I never heard of an accident of that description before as the result of contraction and I should have enjoyed hearing the experiences of other obstetricians along that line.

RETRO-DISPLACEMENTS OF THE UTERUS; THEIR TREATMENT BY A NEW METHOD.

BY ERNEST F. TUCKER, M. D.

Lecturer on Clinical Gynecology in the Medical Department of the Oregon State University, Portland, Oregon.

Although the method I am about to bring before you is only applicable to adherent retro-displacements, I would in a very few words, like to say something about retro-displacements in general and their treatment.

In the first place, I think you will all readily agree without question, that the natural tendency of every healthy uterus is to remain in a normal state of anteflexion of its own accord. Although it may be temporarily displaced, either by an overdistended bladder, the position of the individual, or by any artificial means, as soon as the forces that hold it out of place are removed, it will return to its normal position in the pelvis. This statement, I think, hardly admits of discussion, and yet it has an important bearing on what I have to say.

In the second place, any uterus that has a tendency to remain in a state of backward displacement without the presence of adhesions binding it down, does so on account of a pathological condition existing either in itself, its appendages or the pelvic floor. If you accept my first statement, you must necessarily accept my second one, and it must follow from this, that if one or all the pathological conditions that lead to this tendency on the part of the uterus to lie backward in the pelvis can be cured, the uterus can be easily returned to its normal position and will tend to remain there of its own accord. We know that a freely moveable uterus can be maintained in position by a properly applied tampon

or a properly fitted pessary, but these methods are seldom curative of the conditions that brought about the displacement, and consequently the results are not permanent.

Without going deeply at all into the etiology of displacements, I think I might safely state in a general way, that they are usually the result of a chronic metritis or of a sub-involution following a laceration of the cervix, and indirectly sometimes the result of a rupture of the pelvic floor; not because the pelvic floor offers any support to the uterus itself, but because it does to the rectum, and when the rectum is deprived of its natural support, straining at stool drags down and depresses the posterior vaginal wall, which in turn drags on the uterus, and when this organ is already enlarged, and consequently has a natural tendency to prolapse, this becomes a very important factor in producing retro-displacement, as the uterus cannot prolapse until it is more or less retroverted, and the acute angle formed by it and the vaginal axis under normal circumstances obliterated.

Prolapse and retro-displacements are, in the consideration of the treatment of these two conditions, almost inseparable, and that which applies to the treatment of one applies equally to the treatment of the other. In fact, I believe that it is rather the prolapse than the retro-displacement that gives rise to most of the symptoms; for if we examine the blood-supply of the uterus, we find that the blood vessels which supply the fundus, the freely moveable portion of the uterus, are very tortuous in their entire course through the upper border of the broad ligaments thereby, admitting considerable stretching and even twisting of these ligaments without bringing about any interference in the circulation, while on the other hand, those that supply the uterus at the vaginal junction, the fixed portion of the uterus, are short and straight so that when this part of the uterus is depressed or raised above its normal level in the pelvis, the circulation is at once interfered with and a chronic congestion is the result.

We see this illustrated clinically, in that the more the uterus is prolapsed, the greater is the increase in the size of the uterus; if, however, such a uterus be artificially raised and maintained at its normal level, its size will rapidly diminish, although this result will not be permanent, unless the conditions that caused the displacements are remedied.

Again, we often see adherent retroverted uteri that seem to cause absolutely no symptoms of discomfort, probably owing to the fact that the adhesions occurred before the uterus had time to prolapse to any great extent, and consequently the cervix has been kept at very nearly its normal level in the pelvis. Where there are no adhesions, I believe very firmly that the uterus can be replaced and maintained in its normal position by appropriate local treatment followed by the necessary plastic operations on the cervix and vaginal walls. Where this plan is not followed by success, I believe there is only one way to account for it, and that is by lack of skill on the part of the operator, for there is no other branch of surgery in which the personal factor forms such an important condition to success as in the very simple plastic operations performed every day for the relief of a lacerated cervix or a lacerated pelvic floor. Whenever I make a failure I know that the fault lies in myself, and I simply do the operation over again if I can get the chance.

When we come to deal with a uterus bound down by adhesions, we have exactly the same condition of affairs plus the adhesions, and if we could get rid of these, the same treatment would be curative as applied to the earlier condition. The oldfashioned treatment of using pressure by means of tampons to cause a gradual absorption of these adhesions and bring about a replacement of the uterus is no doubt still of much value in certain cases where time and expense are of no consideration, and much benefit can be derived from this treatment alone if conscientiously persevered in, with the least amount of risk to the patient, but there are many cases which have not the time or the patience to undergo such a

long course of treatment as this method requires. No operation is applicable to these cases that has not for its object the breaking up of these adhesions.

The operation of forcibly breaking up the adhesions under an anæsthetic by bimanual manipulations through the vagina and abdominal walls, seems to be rather a dangerous procedure, as few are gifted with that amount of diagnostic skill that would tell the size or the amount of adhesions present or the exact condition of the appendages.

The other operations, such as shortening the round ligaments within the abdominal cavity after having broken up the adhesions, or that of ventro-fixation, necessitate the performance of a laparotomy which entails a certain amount of danger even in the hands of the most skilled, and this without relieving the conditions which brought about the displacement, for the uterus drawn up out of the pelvis and made fast to the belly wall, as I have always seen the operation done, is just as much a displacement as the condition for which the operation was performed; the circulation is interfered with by drawing the uterus up in the same manner as it is by drawing it downward, as the blood vessels in the neighborhood of the vaginal junction are stretched just as much, providing the uterus remains in its new position any length of time.

Many cures, however, are reported to follow this procedure and by very conscientious operators, whose word we cannot possibly doubt; but I believe where a cure follows this operation it is when it has been performed on a uterus otherwise healthy and where the ventro-fixation part has failed. That is to say that the good result comes from having brought the uterus forward after having broken up all the adhesions, and maintaining it there for a short period by fastening it to the anterior abdominal wall; after this has been accomplished the sooner the uterus breaks away from its attachment, the sooner will the patient be relieved of her symptoms, owing to the natural tendency of the uterus to remain forward when

it is once in a healthy condition and has been freed from its adhesions. Nor do I see any reason why these adhesions should recur, providing the uterus remains healthy.

The method of procedure I wish to call your attention to, is the following: After all the good that can be accomplished by local treatment has been obtained, such as curing any endometritis, cellulitis or pelvic peritonitis, or any inflammatory condition of the appendages that can be cured without their removal, the patient is prepared for operation as carefully as if she were to undergo a vaginal hysterectomy, placed upon her back under an anaesthetic, an opening is made into the peritoneal cavity through the posterior vaginal vault, through which the finger is then introduced and the adhesions between the uterus and rectum are broken up; when these are entirely separated the uterus can be readily pushed forward; any bleeding points can be readily ligated; at the same time the condition of the appendages can be easily made out and if necessary any adhesions binding these organs down can be torn away and the ovaries and tubes freed. The opening into the peritoneal cavity is then closed, and the cervix may be operated on if necessary; after this a tampon of iodoform gauze is placed in the vagina to hold the uterus in position, and the operation is completed.

The patient, after the operation, is made to lie on her side as much as possible to aid in maintaining the uterus in position although in the second case on which I operated this precaution was omitted, the uterus maintaining its position perfectly and has since. At a later date, as soon as convenient, the perineum should be restored. In the meantime the bowels should be kept loose, in order that there may be no straining at stool, which would tend to draw the uterus back again into its abnormal position.

The length of time the supporting tampon should be left in the vagina after the operation will vary with the condition of the uterus, and must remain greatly a question of judgment with the operator, although I believe that if the

uterus is in a pretty healthy condition, a few days will be all that is necessary. In one of my cases the tampon was removed on the third day, and yet the uterus has remained in good position ever since. I know that in performing a laparotomy for the removal of diseased appendages, where a retro-displaced uterus is present, if these adhesions are broken up and the uterus is made to resume its normal position, the result is not permanent, because the removal of the appendages defeats the object in view, as far as the position of the uterus is concerned, on account of the contracture of the broad ligaments which follows the removal of the ovaries and tubes, and which will have a tendency to displace the uterus anew; besides after removal of the ovaries and tubes, the uterus has to depend on its blood-supply from the uterine arteries alone, the circulation in which is much more readily interfered with than in the ovarian arteries, a fact which makes displacements in such patients much more difficult to treat.

As I have performed this operation but a few times, and these are of recent date, I will hardly be able to convince you of its value from any clinical proofs, although my cases have so far been successful; and I do not see any theoretical reason why they should not continue so, or why the operation should not prove of value in other similar cases. If so, I think this method of procedure has certain advantages over other operations for the same condition, from the fact of its extreme simplicity, and that it has for its object the removal of all pathological conditions without substituting any new ones, but on the contrary placing all the organs in nearly as normal a condition as possible.

DISCUSSION.

By request of Dr. Tucker the discussion was opened by

DR. GIESY: Mr. President, this is a paper in which I have been very much interested, and particularly with the clearness and conciseness with which the author has delineated the causes of displacements. I fully agree with him

that in order to cure a displacement it is necessary to remove the cause. I have in several cases noticed that after repairing the lacerated cervix as well as the lacerated floor of the vagina, thinking that I would use a pessary afterwards, that I have found that the uterus had gone back to its normal condition without any effort on my part whatever. I noticed that in quite a number of cases, so much so that it began to attract my attention, and now I always replace the uterus after completing an operation. I have not seen the operation that Dr. Tucker suggests nor have I performed it, but I can see nothing that would prevent it from being a feasible one and one very readily done and which certainly should be successful. I should like to hear from the Doctor after his cases have grown old enough, as it were, so that he could tell something about the result.

DR. JOSEPHI: I want to ask Dr. Tucker one question publicly which I have already asked him privately. After he breaks down these adhesions I would like to know how he prevents a recurrence of them. He must produce a wounded surface between the posterior walls of the uterus or the posterior peritoneal coating of the uterus and the anterior rectal coating, and it seems to me there would be a very great tendency, in fact almost unavoidable, unless there was some material interposed, to have the adhesions recur, which would make the condition of the patient certainly as bad as at first.

DR. FULTON: I would like to hear this question discussed pretty thoroughly; being more or less interested in it myself. I have been doing Alexander's operation in connection with repair of lacerations of the cervix and also of the perineum, doing the combined operation. I have just made up my mind to stop doing the combined operation, for the simple reason that I think the sub-involutions are too great. It draws too much on the round ligaments. I am glad to know that Dr. Tucker has demonstrated the operation so far as he has. I would like to hear from others on the subject.

DR. TUCKER: In reply to Dr. Josephi's question about how to prevent adhesions recurring, I tried to state in my paper that adhesions only occur between opposed surfaces of a diseased peritoneum. That is, the adhesions occur in the first place between the uterus and the rectum, probably by inflammation from the uterus, or appendages; an endometritis might spread through the tissues of the uterus and through its covering peritoneum and produce adhesions between the uterus and rectum. But after all, inflammatory conditions have been cured before operating, when the uterus is then torn away from the rectum, there is no reason why adhesions should recur, as they are only produced by diseased peritoneum. Even if both these surfaces were still diseased, the surface of the rectum and the posterior surface of the uterus are widely removed from each other after the uterus has been pushed forward. After breaking up the adhesions I do not leave the uterus in a retroverted position; on the contrary, antivert it. The intestines naturally fall down between the rectum and the uterus and intra-abdominal pressure is brought to bear, and the natural tendency for the uterus to remain in its natural position is thus aided, as well as by the tampon to keep it there.

I would like to say in reply to Dr. Josephi's remark, "theoretically it is all right but practically it is all wrong," that I am willing to admit that the paper is theoretical. All these things begin in theory. I conceived the notion within the last month and I felt so sure of it that I operated on two cases and expect to operate on another one in two or three days, and in the two cases I operated on there was no difficulty. Even before I placed the tampon in the vagina I examined the patient bi-manually and the uterus remained forward before I placed the tampon in. In both cases I operated on, while recent, every indication points to permanent improvement and the uterus has remained forward. I do not see any reason why retroversion should recur. If there is

any reason theoretical or otherwise I would like to hear it, because I propose to do the operation again.

In reply to Dr. Fulton, I did not refer to Alexander's operation because this paper discusses only versions with adhesions, and it is not applicable to these conditions. I am very glad to hear, however, that he is about to give it up, as it is an operation I have never thought very well of. In the first place I would like to quote a remark of Dr. Thomas Addis Emmet, who has probably had more experience than any other man living in this country in cases of this kind, a statement he made about a year ago I think at the meeting of the American Gynecological Society, saying that he had never failed to keep the uterus in the vagina by plastic methods alone. Alexander's operation is a subterfuge. If any of you have examined the cadaver of a woman you will find that the round ligaments, when the uterus is in its natural position, do not come directly forward, but when they leave the uterus they go first in a posterior direction then outward and finally forward. So that if you drag on the round ligaments in the normal uterus you retrovert it or push it back. Of course if it is retroverted and you pull on its ligaments you do pull it forward some, but you do not pull it to its normal position, and if the ligaments are going to hold, you hold it out of its normal position.

GONORRHOEA AS AN ETIOLOGICAL FACTOR IN DISEASES OF WOMEN.

DR. H. R. HOLMES, M. D., PORTLAND, ORE.

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Under this title gonorrhœa is considered to be an affection of the female genitals, due most probably to a micro-organism, known as the gonococcus of Neisser.

To be sure, the title of my paper would permit a reference to, at least, a specific urethritis of the male, and I desire to submit the question of non-sterility of the male sometimes after a gonorrhœa, which he may have considered for many years to have been completely and permanently cured. And again, I wish to call attention to the seeming impossibility at times, and the extreme probability at other times of such a person conveying the infection to another. An undoubted pathogenesis of gonorrhœa may not be accepted by the entire profession soon, and by some the different phases of vaginitis, urethritis, endometritis, salpingitis, etc., will be looked upon as conditions of degree rather than being due to a particular causative agent which bears a certain sameness through all time.

I am not at all inclined to doubt the specific nature of gonorrhœa and am willing to give all the credit to the gonococcus of Neisser as a cause of the disease.

The first proposition that I offer is that gonorrhœa may be communicated to the wife years after marriage by a hus-

band whose only attack antedates with its apparent cure, the matrimonial venture.

The second proposition is that such a husband may impregnate the wife, and months after a child, healthy perhaps, is born to her, he may without a second exposure himself infect her.

The third and most important proposition is that there is a latent gonorrhœal endometritis owing to a dormant or inactive state of the gonococci, which under circumstances favorable to their reinvigoration are capable of precipitating apparently the most acute gonorrhœa with all of its active symptoms and graver sequelæ, pyosalpinx, etc.

The most convenient and perhaps as fair and profitable manner of dealing with the subject, is in a clinical sense; wheretore, I submit the following cases in point:

CASE 1 (as bearing on first proposition).—

Mrs. B., age 24; married four years; nullipara. Consulted me in May, 1893. Always enjoyed good health until two weeks before, when heat, dryness and pain about the vulva and vagina were noticed as well as a decided febrile reaction. These symptoms continued for two or three days, and were followed by a profuse vaginal discharge of a greenish yellow color. When patient was first seen there was an abscess of one of Bartholin's glands. The husband's statement is believed, he having solemnly asserted his loyalty to his wife and that he had had a gonorrhœa, which had been treated by a respectable physician and pronounced cured two years before marriage. The husband had la grippe shortly before the wife began her suffering.

Other cases could be cited so similar to this as to require no separate history. In a conversation with Dr. Everett Mingus, professor of pathology in the Willamette Medical College, regarding such cases, he said: "I have many times recognized such. I account for the phenomena as follows: After a man has had gonorrhœa, the acute state having passed,

gonococci may linger about some part of the urethra an unlimited period of time, unable to produce a considerable poisonous effect while a man is pretty well, for his vitality is sufficient to continually overpower them. But, if the subject's vitality suffers from some depressing influence, the gonococci, as if taking a cowardly advantage of the situation, 'brace up' and assert their specific nature."

CASE 2 (as bearing on second proposition).—

Mrs. W., age 25; married seven years; one child (healthy) 5 years of age. Consulted me in May, 1894. Had always had good health until two years ago. Symptoms were, first, dryness of vulva with burning sensation and fever lasting two days, and followed by profuse vaginal discharge of a yellow color. After several weeks of simple treatment the discharge got to be of a bloody character, and continued so for several months, when she consulted a physician who curetted and perhaps applied some antiseptic. Patient seemed well for several weeks, but directly after intercourse with her husband, who was just recovering from a fever, her old trouble reappeared. When examining, and suspecting a gonorrhœal endometritis, a specimen of the discharge was sent to Dr. McKay, lecturer on pathology in the Oregon State Medical College, and reported back by him as "just swarming with gonococci." Dr. McKay examined the husband and found him with a "gleet" and several urethral strictures. He was willing to share with me, belief in the man's statement to the effect that he had never gone astray since marriage; and that he had had but one attack of gonorrhœa, which he supposed cured long before his courtship. Other such cases, apparently, could be cited. Dr. McKay told me he had known of many such.

While there are those standing high in the profession who deny such probability, I must candidly submit that if the lobuli of one testis are in a healthy state; if the vasa recta are patulous; if the rete testis, the vasa efferentia, epididymis, the vas deferens, the vesicula seminalis with the

ejaculatory ducts are all in good condition; in other words, if one testicle and the other structures alluded to, have nothing the matter with them, and the urethra does not contain an abnormal secretion capable of devitalizing the spermatozoa, I can see no anatomical or other reason why a man cannot impregnate his wife one day and infect her with gonorrhœa the next.

CASE 3 (as bearing on third proposition).—

Mrs. M., age 34. Consulted me in May, 1894. Married at 18 years of age; three months after had what the doctor called gonorrhœa. Some abscesses about vulva were lanced; husband had swelled testicle; separation and divorce followed. The usual health seemed almost, but not quite, restored within a few weeks. There lingered a usually slight though varying leucorrhœa with some backache, and sometimes a pruritus vulvæ. At the age of 21 Mrs. M. again married. The husband was a minister, who died of pulmonary tuberculosis a little less than two years later, leaving to the disconsolate widow the legacy of a puny child and puerperal fever. Within about four months Mrs. M. was able to walk around the house. About this time the child expired. The mother insisted upon attending the funeral, had a chill at the grave and was then confined to her bed with a high fever and tender abdomen for several weeks, when she grew better, though her physician insisted on calling a specialist, who opened the abdomen and removed a sac filled with pus, undoubtedly a pyosalpinx. The husband, a year before death, had a urethral discharge and a swelled testicle, cleverly called tubercular by his physician. After the coeliotomy, Mrs. M. had an ordinary convalescence, and for more than a year remained quite well. Soon then, however, an itching above the vulva began, urethral irritation, too, and with these annoyances a muco-sanguineous flow which had lasted most of the time for four years, although two curettages had been done.

When four years had gone by the discharge had ceased,

and our heroine married ten months later. Three months later still the bloody discharge returned. One day, a little later yet, her husband, who had been limping about for a few days, said to her, "Good-bye; I am going for a walk and don't know just when I may return. If you should marry another man, I hope the stupid fellow may enjoy your society as much as I have," and he limped away. I suspect that limp bore some relation to a swelled testicle. Patient was now prostrate for a time with grief. When she rallied, philosophising that marriage was a failure, she took up her abode with a spinster aunt who lived by the side of the sea. The salubrious atmosphere and the society of her congenial relative soon restored her shattered nervous system, and for more than two years she enjoyed better health than she had since she had last seen her first husband.

When I first saw this historical patient, she had but recently recovered from a continued fever. She looked a very "monument of human misery," and complained that the ghost of her early misfortune was haunting her still. The bloody uterine discharge was quite profuse. The uterus was curetted carefully, irrigated with 1-5000 bichloride solution, followed by sterilized water and packed with iodoform gauze. Patient for six months remained well, and so far as I know is well yet.

The periods during the time she was under observation were regular and she was free of intermenstrual bloody flow. The breath of suspicion, I have been credibly informed, never visited this poor woman, except it were blown by the limping husband No. 3, and I regard as an acceptable theory the following: That patient contracted gonorrhœa from the first husband; that after an apparent recovery the disease remained in a latent form, and nearly three years later she communicated it to her second husband; that the circumstances of her labor were favorable to the reinvigoration of gonococci, the production of pyosalpinx, endometritis and puerperal fever;

that the third husband was probably infected by an honest, innocent woman who was ignorant of her condition.

The chronic muco-sanguineous uterine discharge with its peculiar behavior is too well and commonly known to require remark. In view of the fact that a large per cent of males contract gonorrhœa before they assume the responsibility of married life, and since the observation of Næggerath and even more recent experience have convinced the profession of the correlation of these gonorrhœas, even though apparently cured years before, to many of the obstinate chronic female pelvic diseases, I believe it to be the duty of the profession to impose this knowledge upon the laity, and I believe too, that the man who contemplates marriage, having once had gonorrhœa, should know the importance of consulting a specialist on venereal diseases beforehand, as well as he knows the importance of vaccination when expecting exposure to smallpox.

DISCUSSION.

By request of Dr. Holmes, the discussion was opened by

DR. TUCKER: I can best endorse what Dr. Holmes has said by relating a case I had in Portland over a year since and which I treated for about a year. The patient, a woman, came to me first with leucorrhœa, without very much apparent cause so far as the uterus was concerned, but there was a good deal of endocervicitis. I treated her for awhile and would get her well and send her home. In three weeks or a month she would return with the same trouble. After having done this a number of times, and apparently cured the woman, as soon as she went home to her husband she would be back again, sick. Finally, I asked her if she would please ask her husband to come and see me. Her husband came, and I asked him very frankly if he had ever had any gonorrhœa as a young man. He was very indignant, and denied any such cause for his wife's troubles; said he had never had any such disease. The next time I saw his wife she said "Why didn't you tell me what you wanted

to see my husband about?" I asked her what she meant; she said she knew he had had a disease about the time her first child was born; he was using a syringe, and she said that was not the only time either; that she had been visiting her mother at one time, and on coming back he was still using the syringe. On her asking him what was the matter, he told her that he had contracted a discharge from having had connection with her while she was pregnant the first time; and the second time it had returned owing to the absence of his wife! I hardly knew what to do, but told her to tell him to come in and see me again. In about a week husband and wife both came in, the husband bringing me a letter from another physician saying that any man that would accuse him of having any disease whatever was a falsifier and libeller, that he was the finest specimen of physical manhood to be seen anywhere and had never suffered from any such trouble as his wife mentioned, and abused me, and accused me of trying to interfere with their marital happiness. I think these cases occur very often, and it is very hard to get at the real facts, and about the only way to create any real improvement is by teaching the laity, the people at large, the danger that exists in gonorrhœa. I believe, so far as diseases of women are concerned, gonorrhœa is a more dangerous disease than syphilis. A man who has syphilis is much more apt to take care of himself and see that he is cured before entering into marital relations, or even to abstain from it altogether, as I have known men to do, than if he has gonorrhœa. The custom too frequently is to laugh at gonorrhœa as a cold in the head. Indeed, lots of men say, "I have had it a dozen times," and think nothing of it; they go to the nearest drug store, get a prescription, the discharge ceases, and they think they are cured. But they are not, and I feel sure the more I see of diseases of women that many of those diseases are started in the first place by latent gonorrhœa.

DR. STRONG: I do not know of any subject that is of

deeper interest and of more importance to us and to the people who are affected than female diseases. My own impression is that latent gonorrhœa has only one rival as a cause of female diseases. That rival—I do not know which I would put in the vanguard—is the non-returning of the uterus to its normal condition, the condition known as involution, after the puerperal state. I am convinced after a careful observation extending over a number of years that latent gonorrhœa is responsible for a very large per cent of diseases of which women are victims. I think many men who have had it are honest in the belief that they are cured when they are not; and one of the dangers lies in that very fact. The other danger is of course in getting the disease, and the feeling that exists not only in the laity but I am sorry to say amongst the members of the profession, that gonorrhœa is a simple thing, that it is a little inflammation of the mucous membrane of the urethra, and can be cured, and the only thing requisite in a case of gonorrhœa is the fee. I think great stress ought to be laid by the profession upon the importance of curing gonorrhœa positively, and not a mere suppression of the discharge even after a number of years; but the entire destruction of the germs of gonorrhœa, and the condition of the parts so restored as to prevent their development in their places of concealment. The strongest factor, in my judgment, in cases of female diseases, is their prevention rather than their cure; and then this dispute between those who believe in ventrofixation and those who believe in posterior packing, etc., will be to a great extent eliminated from the debates of the profession.

DR. HOLMES: May we not hear from Dr. Bailey?

DR. BAILEY: You call me out on a subject that is out of my line and in which I have had but little experience. But I have lately been impressed with the importance of gonorrhœa as a factor in causing female diseases; and the profession is generally beginning to understand it. We have studied the human coccus and the various cocci, but the gono-

cocci is able to hold the fort, it would seem, when he establishes himself in the urethra. It has no doubt occurred to many members of the profession long before this question was raised, as to what latent gonorrhœa could accomplish, and it is surprising, indeed, to find one who has long been supposed to be cured of gonorrhœa, after an attack of some disease, or after imbibing too much beer or tangle-foot, showing a gonorrhœal discharge which, as Dr. Holmes says, would infect either his wife or some other woman. This is easily explainable, if we believe as has been shown that the gonococci may remain latent, so to speak, and ready to attack the individual when his health is under par. It really seems almost improbable that they would remain so long in the urethra or in the vesiculæ seminalis or in the epididymis—without giving rise to trouble. We know they remain for years, but must meet with favorable conditions to develop. There is one thing, however, in some of these cases that we ought to think about. There is always an element of uncertainty which is well demonstrated, in the case that was specially emphasized by Dr. Holmes, of the woman who infected her husband. This element of uncertainty is undoubtedly often a factor in the physician's diagnosis; and he probably says to himself, "If I understood all that, I could easily account for the manner of the infection." I think sufficient is known as to the dangers of latent gonorrhœa to warrant us in emphasizing the fact among the laity. If we did so, it would soon be looked upon as a disease far more dangerous to the human family in its ultimate results than syphilis itself. It is a question which runs along parallel to and we might say touches the work of the gynæcologist. One of the members of our society, recently a delegate to the American Medical Association, contributed a paper touching upon this point from which it might be inferred that he was about to give up the field of the gynæcologist for that of a specialist on venereal diseases. Evidently it is well for the successful gynæcol-

ogist to have a thorough knowledge of venereal diseases, especially that of gonorrhœa.

DR. CAUTHORN: I do not know much about gonorrhœa as affecting the female, but taking all the evidence that our specialists are bringing to our observation, I have, some time ago, come to the conclusion these other gentlemen have, that gonorrhœa is the cause, perhaps, of a large proportion of the ills of women. While this discussion has been progressing it has occurred to me that the medical profession does not fully realize the relation in which it stands to sociological questions. There are a few places wherein a man may express himself honestly and candidly about all questions, and one of those places is in discussions of a medical society. I have long been of the opinion as a physician, as the result of observation, that houses of prostitution should be licensed, and should be put under proper police surveillance. When we say this to the laity or the outside world, we are immediately met with accusations of laxity of morals; they have a fine-spun theory about church relationship to a man's life and the religious idea which they immediately meet you with, and say your position would encourage licentiousness. On the contrary, I doubt it. I believe that as physicians we bear a more intimate relation to this sociological proposition than the people would give us credit for. I believe that to a large degree moral law is preceded by physical law, and that moral laws, if they are based upon a proper consideration of physiological relationships and physiological phenomena, would be better observed, and would better subserve the interests of society in general. I do not believe any man was ever kept out of a house of prostitution by a dread of catching disease; they will go there anyhow. The propagative instinct, we know, is the strongest in the whole economy of physiological processes. Men are driven by these imperious instincts to the gratification of the sexual desire, and they will take all chances and all risks that are concomitant with it in order to obtain the gratification. Therefore the moral question should be

laid aside when we come to consider this subject; that is, from the standpoint of the religionist; and the only proper view from which to consider it is, that of the physician who knows something of physiological law, who realizes the serious and deleterious effects of the violation of those laws upon the human race at large. It is our duty as physicians to look beyond the present gratification, beyond the moral effect upon the individual, to its effect upon his descendants and the succeeding generations. I believe that we can do more ultimate good by regulating prostitution by proper laws, than by talking individual morality to the race from now till doomsday.

DR. HUMPHREY: I think a great many physicians are to blame for not impressing upon the minds of patients the troubles that arise from gonorrhœa. A patient came to me not long ago and said he had just come from a prominent physician in Portland. He said, "so far as gonorrhœa is concerned it doesn't amount to anything; I have had that at least fifty times; it doesn't amount to any more than a cold." I think there is too much of that lax way of dealing with gonorrhœa. It ought to be impressed upon the mind that it is not the easiest thing to get rid of.

DR. TUCKER: May I add a word to what I said a few minutes ago? When I first came to Portland, like a great many other physicians, I had women from houses of ill-fame to be examined. I examined them but always refused to give them a certificate as to their condition, for the reason that I was not able to make a diagnosis at a single examination. A woman may have gonorrhœa, quite a profuse one, take a douche immediately before coming to the physicians office, and if it was an old chronic latent one, he would find no signs or symptoms of that disease. Upon such an examination he might give her a certificate of cleanliness and health, and yet she could immediately infect any one, and the physician get the blame he would deserve for having given her the certificate. I say this advisedly, although I know many of the physicians that stand highest in the profession in Portland are in

the habit of giving such certificates to women. I would like to express my sentiments as thoroughly opposed to it because it is not possible to make a sufficient diagnosis without taking very much more time than is usually given to it.

DR. WELLS: The longer I practice medicine and the more extensive my observation has been, the more I am convinced of the correctness of the position taken by the author of this paper, that gonorrhœa stands as a cause of probably the largest majority of diseases of the pelvic organs in woman among those who are exposed to its dangers. I rise more particularly to speak in opposition to the sentiment expressed by my friend Dr. Cauthorn. I do not think this society wants to put itself on record as flying in the face of the religious sentiment of the community. I do not think Dr. Cauthorn intended to place himself in that position. I want to call his attention to the Higher Law which says not only that "Thou shalt not commit adultery," but "Thou shalt not steal, thou shalt not bear false witness against thy neighbor," and that the same restrictions are placed upon their violation of the code as there is upon the other. And why should the State fly in the face of the Decalogue and permit men to commit adultery under certain conditions any more than it would sanction murder under certain conditions? I do not like to let that statement go before the society unchallenged.

DR. THOMAS PARKER: I believe statistics show in all those countries where Dr. Cauthorn's ideas have been adopted in the past that there is less gonorrhœa than in countries where strict laws of morality are relied on, and sanitary conditions are bad.

DR. SAYLOR: One point I think has not been sufficiently dwelt on in this discussion, namely, to know when an individual is cured after having had gonorrhœa. Until that can be determined satisfactorily it will be impossible to know when an individual can marry without infecting his helpmate. Having had some little experience in the treatment

of this disease, especially in the male, I realize how difficult it is for one to establish absolutely that the patient has been cured. We well know that the gonococcus is very hard to dislodge, and that after one has been infected for some considerable time the acute symptoms subside, and we have what is termed gleet. To my mind, until this damaged patch or stricture has been entirely cured, that person is capable of propagating the disease. My theory with regard to the ultimate cure is this: that if on examination of the urethra with the bulb sound I find there is no resistance (after having used the proper means for cure), and there is no discharge, I begin to think my patient is well. These damaged patches and strictures, are of course treated in different ways by different physicians; but if you can introduce a normal bulb sound and pass it down to the membranous urethra and there is no discharge, then in the majority of cases the patient is cured.

DR. WHEELER: I regret very much that I did not hear Dr. Holmes' paper. It is a subject I have been interested in very much, I have treated gonorrhœa a good deal, but I don't think I can tell you anything new about it. The doctor's paper evidently takes up the Noeggerath theory as given years ago. He said "Once gonorrhœa, always gonorrhœa." It would seem sometimes he was correct; yet that strikes me as a very exaggerated statement. To be candid, I do not believe it. Gonorrhœa is curable. These latent gonorrhœas are probably due to stricture. If a stricture is present holding the gonococci or making a bed for it to thrive in, that of course should be obliterated. Yet strictures are not always the cause of reproducing or keeping the gonococci in existence. I should rather take, not the extreme ground in these cases, but the medium ground, and hold that, notwithstanding you may have a gonorrhœal pyosalpinx, a gonorrhœal tubal trouble, it will cure itself, or that the gonococci may die. I have seen women who I knew had gonorrhœa, and I know they have borne children afterwards; and Emmet makes the broad statement in one instance that a woman hav-

ing had gonorrhœa may get well and conceive. As to the return of gonorrhœa in the male, I do not take much stock in that theory. Gonorrhœa may of course return until it is cured; but after the gonococci have ceased I do not see why a new generation of gonococci should come, unless from a new exposure. Men are always liable to these exposures. I always take their statements with allowance. I have known, as I said before, an instance where the male is suffering from stricture and his wife has not and never had gonorrhœa; but if the stricture was filled with gonococci she would necessarily be infected, whereas she has not been. I would rather go on record as taking the medium ground, with the exception that I agree with Dr. Cauthorn, and state from positive statistics that countries which regulate and control prostitution by law and police surveillance are, notwithstanding contagious and infectious diseases, gonorrhœa and syphilitic troubles, the most healthful place in the world.

DR. WHEELER (continuing): Calcutta has had for years a law governing this question with reference to houses of prostitution. While that law was in existence gonorrhœa and syphilitic troubles in that country were to a great extent controlled. Within the past seven or eight years that law has been repealed; and the history is that diseases peculiar to the genital organs have increased to a frightful and alarming extent.

DR. CAUTHORN: I do not wish to be misunderstood in this matter, and if you will allow Dr. Wells and myself to talk we will soon come to an understanding, but if you keep us apart there is no telling how long we shall disagree. (Laughter) I think the discussion is right in point, and would not for a moment entertain the idea of being out of order. The very intent and purpose of this paper of Dr. Holmes' is to prevent these ill effects upon women. I am free to confess that perhaps 99 out of 100 originate with the man; that is, so far as the respectable women are concerned, whose interests we are looking after and have at heart. I do not want to fly

in the face of the religious sentiment, for which I have a high respect, although I do not admit an idea is right simply because it is a religious idea and believed in by a large number of people. I think it is a duty of science to step in and modify those opinions, and therefore I suggest that from the standpoint of the physician one of the most certain means of preventing disease is to prevent its dissemination.

DR. WELLS: I do not antagonize any law or measure the advocacy of which would lessen any disease which is destructive to the human family, as small pox was before vaccination became practiced the world over; but when a measure is advocated which flies in the face of the Decalogue and tramples upon everything which religion regards as sacred—the very foundation stones of society—then I say we had better adopt some other measure.

DR. STRONG: I do not wish to discuss this subject from its religious aspect; but I think there is something to be said on the subject, and I wish to speak of it in two ways. In the first place, I wish to call attention to the fact that we know it is almost impossible to determine when a person is cured of gonorrhœa, male or female. You may examine the woman as carefully as you can, and take as much time as you wish; but you cannot tell whether she has latent gonorrhœa or is cured.

DR. HOLMES: Do you consider the use of the microscope?

DR. STRONG: Yes. Then another point I wish to raise is that, in cities like Paris, which is under the most magnificent system of police observation and control, statistics show that there was as much or more gonorrhœa than at any other time. Another fact which cannot be denied is that a very large per cent of gonorrhœas are contracted not in houses of prostitution but from your servant girls, and from "private snaps" men have. That is where gonorrhœa comes from, gentlemen, and we all know it. A very large per cent of our

young men nowadays keep their mistresses; it is an admitted fact that cannot be denied. What is the use of controlling a small factor—the disseminating centres—when you cannot control nine-tenths of it? I am perfectly willing those houses shall be licensed if it will control it; but I think the strongest influence upon it is the weight of public sentiment. If public sentiment was such as to condemn in a man that which it condemns in a woman, it would have some influence over it. We do not allow a woman to have the least breath of suspicion on her, but if she has, we damn her from then to the end of time; but we take into our houses a young man that comes from those places and who we know keeps his mistress. There is no power on earth that can withstand public sentiment, and if it is brought out strongly enough to bear on this question something will have been accomplished for the cure of diseases among women, for I claim gonorrhœa is one of the greatest causes of diseases of women.

DR. WELLS: I want to say a word in compliment to Dr. Holmes for bringing out this discussion in behalf of the women of the country. I think he has struck the key-note; and it has been a mystery to me why this subject has not been brought before the people years ago. To whom does the public look to be educated on these subjects except to the physician who studies these things. Why do we stand here and want to stop science in its progress? Nor is it necessary to go too far into the moral side of the subject; but we as physicians know too well the misery that is being wrought when we see the beautiful girl with the flush of disease in her cheek, and we know the cause of all this, but dare not tell.

DR. HOLMES being called to close the discussion, said: I have very little to add except to thank the members for the cordial manner in which they have received my paper. I would have it known as emphasized by Dr. Cauthorn, that the purpose of my paper was the importance of prophylaxis. That is what suggested my closing remark, that the man who once has had gonorrhœa and was contemplating matri-

mony, should know the importance of consulting a specialist before doing so, as much as he should know the importance of vaccination before exposure to small-pox. It is a pity indeed that honest innocent women are so often in their ignorance exposed to the consequences of gonorrhœa. It is deplorable, too, that men when contemplating marriage do not understand that if they have ever had gonorrhœa they are liable to communicate it years afterward to the wife. Dr. Wheeler and Dr. Saylor are regarded in this part of the country as authorities, to be sure, and yet I want to urge upon them the importance (since they are both specialists in the venereal line) of taking every precaution possible to make sure that their patients are thoroughly cured, before entering into the married state.

ALKALOIDAL MEDICATION.

BY J. C. HUMDHREY, M. D., PORTLAND, ORE.

Mr. President, and members of the Oregon State Medical Association: About fifty years ago Prof Adolph Burggrieve, of the University of Ghent, conceived the idea of administering in disease, according to certain simple rules, the active principles of plants prepared in granules. Because the medicines were "mathematically measured" in small doses, the name Dosimetry was applied, to distinguish this method of prescribing from others then in vogue.

It is not claimed that Dosimetry is a new system or that it is a complete practice. The active principles of plants, which have been used for ages in the crude form, are prepared in granules. This is done for the purpose of convenient dispensing and of assuring accurate dosage. That granules contain accurate doses is evidenced by the fact that uniform results are obtained and fatal effects have never been observed.

These granules represent the minimum adult dose and in acute diseases they may be administered every fifteen minutes, every half hour or every hour, according to the severity of the attack, until some improvement is manifested. The medicine should then be given at greater intervals. By pursuing this method closely it is simply impossible to overdose the patient. On this account the use of alkaloids and other active principles are prepared free from danger.

Another advantage of the Alkaloidal method is that in acute diseases active treatment may be begun immediately, even before a positive diagnosis can be made. Physicians who have tried this method, testify to its wonderful efficacy

in obviating many acute febrile diseases, a thing comparatively easy to do at the beginning of the attack.

There is no recognized dose, for no one can positively say how much of a given remedy will be required to relieve a symptom, therefore minimum doses should always be given and repeated frequently until the desired effect is obtained. In diphtheria and various other diseases, paralysis frequently occurs. Now we do not wait until there are evidences of paralysis, but we apply our remedy before paralysis makes its appearance, and if strychnia is a proper remedy to use to cure paralysis, it is also a proper remedy to use to prevent it.

There are various objections raised against this method of treatment, because it is assumed by some to be a new system of medicine, but it is not a new system. It is simply a method of administering active principles in a perfectly safe and effective manner.

Some claim that Dosimetry borders on Homeopathy, because little pills are used and because the physician can dispense his own medicines in a similar manner to that of the Homeopathist, if he wishes to do so. Others affirm that alkaloids are dangerous medicines; so they are in the hands of the unenlightened, and so is any medicine except homeopathic medicines. The danger is removed by knowledge sufficient for a proper diagnosis and by an understanding of the physiological action of medicines. Those physicians who object to alkaloidal granules which contain certain known quantities, do not object to giving infusions, tinctures, fluid and solid extracts, all of which must contain alkaloids in unknown quantities. A physician cannot take a bottle of the tincture of aconite root ordinarily found in drug stores, and tell how much there is in a single dose. It must be that he does not wish to know how much active principle he is using and therefore gives the crude preparation. I see no advantage in giving the crude drugs in tablespoonful doses when we can obtain better results with a sugar-coated pellet which is not disgusting to the patient.

I have been using the alkaloid system to a limited extent for a few months past, and I must say that I have been very much pleased with the results obtained and feel confident that it is a step in the right direction.

First—It gives us the opportunity of carrying a stock of medicines in so small a space that we are able to meet the immediate demands of almost any case without having to send a long distance to receive only a poor substitute for what we have prescribed.

Second—It is equally as pleasant and palatable as homeopathic pills, and at the same time we know that we are giving just what the patient should have and we also feel that we are prepared at all times to cope immediately with acute attacks which demand active treatment.

DISCUSSION.

By request of Dr. Humphrey the discussion was opened by

DR. ROBINSON: Alkaloidal medication is something I have been using for over six years. I first began the use of alkaloidal medication on account of having prescriptions filled by incompetent druggists. The difficulty I often found in prescribing certain kinds of medicines, as aconite, strychnia, arsenite of strychnia, and such as that in the usual combinations got from the drug store was that every little while I knew of some trouble created by it. You soon fall into the idea because these things are very handy, especially for little children. You take a few granules of different colors and put them into a little dish on the table and write the directions and put along side. In my practice if I think the people can not count with exactness or that there might be some trouble of that kind I take a few colored ones and put in, requiring a certain number of doses to be used in a certain length of time, and so many red pills thereafter. It would only be necessary to drop the granule in the child's mouth, give it a sip of water if a small babe and leave it until the

next dose. I went on from that until I began to use it more generally. The only thing I found requisite to become successful with alkaloidal medication was that one has to make his diagnosis as accurately as the medicines are apportioned. If you are going to use a rifle and hit the center of the target you have got to have a definite center to hit. As Prof. Walla said, we are beginning to use the rifle instead of the shot gun; and in alkaloidal medication we began to use the rifle and we had to make a closer diagnosis. I notice we never jump right into practice and begin to dispense alkaloids at once. I had to go very slowly with it and I have been working along with it for six years, and now I am far more pleased with it than ever before.

DR. BOWEN: I would like to call your attention to one other advantage that has not been touched upon. I do not understand that the paper contemplates a restriction of the use of what has been called the dosimetric system altogether. I understood the title of the paper to be alkaloidal medication and I understood it to advocate simply the use of alkaloids or the active principles of the old time drugs rather than resorting to the crude mysteries our forefathers used for so many centuries. I should be heartily in favor of any means, and I think every physician should be, by which we can render our remedies more palatable, less bulky and at the same time more accurate in doses. Certainly these are three things that can often, or almost always be accomplished by the use of alkaloids or active principles. We may not only gauge very accurately the dose we want to use, but we may protect ourselves against crude drugs, and that is one of the important things. We go to a drug store and get a drug which has possibly lain upon the druggist's shelf for many years, and it would be a very fine point in diagnosis to tell whether that drug was reliable or not and often times we are disappointed; but we are seldom or never disappointed in the use of alkaloids; whether we prescribe them in the manner suggested or whether we make them up in the solutions. I think the use

of alkaloids in prescriptions is increasing daily and principally for these reasons. And one point I wanted to emphasize was the fact that in using them we know our medicines are more reliable. Moreover, we all know that the effect of medicine depends often times upon the dose in which it is given. We know that a tenth of a grain or half a grain of ipecac has a much different effect from that which will be produced by ten, fifteen or twenty grains. We know, too, that what is true of this is true of a vast number. I recollect a lecture I listened to once in Bellevue Medical College by a professor there on small doses often repeated; I have often thought, in reviewing the whole college course, that there was no one lecture or no one subject that was to me of as much value as that one. We may laugh and make a great deal of sport of homœopathy and the doctrine of infinitely small doses; while I am as far from being a believer in homœopathy as any one, yet there is something in small doses frequently repeated. Let any one take a large dose of colocynth and I fancy he will very soon experience the pains of colic; but I can testify that in my hands there is no surer remedy for colic than small doses of that same medicine. What is true of that is true of others. The point I wish to make is that aside from using remedies of greater reliability and palatability and accuracy of dose by the means of those alkaloidal remedies, using smaller doses frequently repeated, we may accomplish results that we can accomplish in no other way; and we can use doses sufficiently small unless we make use of the active principles.

DR. THOMAS PARKER: I agree with Dr. Humphrey in every respect. It is the palatability and accuracy of the dose and the ease with which it can be transported. I agree with the Doctor, too, that the only medicine that can be handled carelessly by any one is homœopathic medicine; and of course the only trouble about it is that if some homœopath loses his case of medicines in the broad waters of the Columbia it would poison all the fishes. However, such an accident has

not yet happened. The palatability and smallness of the dose and accuracy of the dose of those little minute things will eventually knock out the homœopath entirely. I saw within the last few weeks an article written by an eminent homœopath wherein he advises the members of that school to be careful to find out in every case whether there is any germ at the bottom of it, and if there is, it is no use, homœopathy won't work. In fact, they have found out and candidly admitted that homœopathy won't work in any disease that is at all known and only in something about which nothing is known, and in that case homœopathic pills are of wonderful efficacy. (Laughter) There is another thing about these granules, these alkaloidal remedies, that is very fine. We shall eventually ruin the whole drug trade for every druggist is practicing medicine, *sub rosa*, you know; and every druggist is really, *sub rosa*, telling people what a wonderful man he is and a natural born genius and that he can take him in the back part of the drug store and cure him in three shakes of a lamb's tail. (Laughter).

DR. WELLS: Not that I want to blow anybody's horn, but P. D. & Co., have gotten out what is known as Diurnal Tablet Triturates, which is actually the finest medicine it has ever been my pleasure to give. They are divided into four parts and you can give the smallest to the youngest child you have occasion to treat. One tablet represents one-twelfth of the dose for 24 hours. More particularly with the use of aconite, with those I have been able to produce the physiological effects of aconite very distinctly, and saw the beautiful work of it in myalgia. That muscular pain vanished like a cloud before a summer's sun under the use of P. D. & Co's. triturate of aconite.

DR. CAUTHORN: It is always well to interject a little objection to everything that seems to be going too smoothly one way and it seems to me everything that has been said heretofore about alkaloidal medication has been so much in

its favor that it may not be amiss to raise some objection against it. It has occurred to me that the preparation of pellets of a certain size to make a dose tends to make the physician a "rule of thumb" man, and that he will cease to know the effects of medicines. He may be an accurate prescriber, as Dr. Robinson has suggested, for certain indications, and that is all right; but the combination of effects of medicines I do not think he will very fully appreciate and I think he is too apt to get into that way of prescribing, number one for certain symptoms, number three for another, etc. I will take my chances right along with the man with a prescription book who has a thorough knowledge of the *materia medica*.

The discussion was thereupon closed by,

DR. HUMPHREY: Of course Dr. Cauthorn has a perfect right to drink infusions and tinctures of crude drugs if he wishes to, but I would prefer to take my drugs in some other form. I never had any physiological effect from aconite in anything I have bought from any drug store and I carried on a drug store for three years myself. I never got any physiological effect from the tincture of aconite that I could see any benefit from until I adopted the aconition of the alkaloid.

COMBINED TAXIS IN THE REDUCTION OF INGUINAL HERNIA.

BY G. M. WELLS, M. D.

Prof. of Pediatrics, Medical Dep't. of the Oregon State University.

In the pursuit of scientific investigation and the alleviation of human suffering, we as physicians and surgeons are ever reminded that "there is nothing new under the sun."

Hypodermatic injections were used in 1664, but not until 1843 did Dr. Wood of Edinburgh develop it as an original method, when it became universally adopted. Herodotus boiled water in 550 B. C. to destroy its infectiousness, but not until Van Loenwenhoek in 1683 gazed through a lense, and for the first time in history saw bacteria which Koch and others have proved to be the cause of the infectiousness in water, was this simple measure universally resorted to. We could thus enumerate many of the established facts of medicine and surgery which have been elaborated and developed by men who, to say the least, had no previous knowledge of their existence.

I bring before the society to-day the subject of combined taxis, not simply rectal taxis. Probably no subject in the whole domain of surgery has been so written and re-written about as hernia. There is no variety of ailment that affords a more lucrative field for quacks than that of rupture, and at the same time I may say, no branch of surgery that requires more technical skill than the operation for the radical cure for hernia. It would take volumes of written matter to enumerate the thousands of cures—so-called—from the charlatan *Emplastra contra Rupturum* of Cambriere in 1680, for which Louis XIV gave many thousand pounds, to

the scientific radical cure of Bassini, McBurney, MacEwen and others of to-day, but it is not my purpose to burden you with radical cures, it is those emergency cases we meet in general practice where immediate relief is possible by reduction of the hernia, and leave the more difficult surgical treatment to a later and more opportune time.

Called hurriedly to see a patient, we find him suffering from an irreducible hernia—symptoms of strangulation loom up like a dread spectre before our mind. What are we to do? Taxis has been tried and failed. Two courses then are open for us to follow, viz.: combined taxis (if you please) and the radical operation. It is the importance of combined taxis I wish to refer to to-day, and the following two cases will make plain what I mean by combined taxis.

CASE 1.—On Sept. 25, 1893, A. S., a German, age 35 years, weight 160 lbs, previous history good, was brought to my office by Dr. J. H. Hickman, of Clackamas, suffering from a right inguinal hernia of 24 hours duration. The Doctor had resorted to the usual taxis and had made a prolonged application of cold, with the patient on an inclined plane; all without avail. After a careful examination, the diagnosis was confirmed and the patient sent to the St. Vincent's Hospital, where he was informed that if the rupture could not be reduced under anæsthesia, the radical operation would be performed. After complete anæsthesia, the patient's lower limbs were placed in suspension rings (the hips raised and the pelvis flexed upon the abdomen) and well directed efforts at ordinary taxis were carried out with no favorable results. Before resorting to the knife the thought came to me that I might succeed by rectal taxis combined with the usual method; up to this time I had never heard or read of rectal manipulations in hernia. Lubricating my fingers well with vaseline I inserted the middle and index fingers far up into the rectum pushing toward the right internal ring, and by pressing upon the abdomen over the ring with the left hand, I could feel the knuckle of the intestine,

then, by gentle manipulation the protruding gut perceptibly gurgled and the hernia was reduced. A spica bandage was applied for a few days when a truss was fitted and the patient returned home.

CASE 2.—In October of the same year I was called to the outskirts of the city to see D. H.—a laborer aet. 60 years, who had worn a truss for right inguinal hernia for several years; that morning he got up and split wood without putting on his truss and on attempting to replace the rupture he found he was unable to do so. You all know that a man who has been ruptured for years is pretty good at taxis, in fact they are adepts at it, and when we find they have failed we know we have a bad case to deal with. On arriving I placed him on an inclined plane and resorted to ordinary taxis, but with no better result than his own. Administering chloroform and with assistance, I introduced my fingers into the rectum and manipulating as in the above case, I failed to reach the parts involved. Recognizing the fact that in this case I had to deal with a man of over 200 lbs. weight and that I could not reach the parts with my fingers unaided, I secured a metallic case knife, and protecting the blade with gauze I passed the handle, guided by the fingers, into the rectum and toward the internal ring; after careful bi-manual manipulation the gut returned into the abdomen.

I am well aware that two cases are not sufficient to establish the value of any method, but the facility with which these hernias were reduced, after resisting ordinary taxis, induces me to believe that the profession has failed to properly estimate the value of combined taxis in inguinal hernia.

Some months ago I spoke to several surgeons about this method and reported the cases to the Portland Medical Society, when I laid some claim to originality, but since that time my attention was called by my friend Dr. Cauthorn, to an article in the Medical News by Dr. G. S. Brown, in which he resorted to rectal taxis in a child successfully. Since the

above report I find the following in the Handbook of Medical Reference: "Traction may sometimes be efficiently applied through the rectum—the fingers and in rare cases the entire hand may be introduced into the rectum and traction applied to the lower part of the ileum which rests upon the pelvic basin, and possibly to the colon itself. The influence of traction thus obtained is an important factor in determining success."

In order to further test the practicability of combined taxis in any available degree upon the structures involved in inguinal hernia, I visited, through the courtesy of Dr. Cauthorn, the dissecting room of the Willamette Medical School, where on May 18th he placed at our disposal a well-formed male cadaver. After thoroughly emptying the rectum, we ascertained that the ends of the middle and index fingers of the right hand passed deeply into it, can be easily felt by the left hand depressing the abdominal wall two inches above the internal ring, also that by passing a large male sound or other similar appliance into the rectum it will serve to bring parts into reach that would otherwise be inaccessible, and also serve as a point of resistance to the yielding abdominal walls. We also ascertained to our surprise that the right and left rings were equally accessible by fingers or sounds per rectum by bi-manual manipulation. Thus it was demonstrated to our perfect satisfaction that by flexing the pelvis toward the chest, compressing the abdominal wall with the left hand and the fingers deeply in the rectum, the internal rings can be brought within reach of the surgeon. This is what I term combined taxis in the reduction of inguinal hernia.

NOTE.—On June 21st, since writing the foregoing, I had a case of inguinal hernia in a child fifteen months old, where all the ordinary methods of taxis, the patient under profound anæsthesia, utterly failed to reduce the hernia. Under combined taxis the gut was easily and quickly returned.

DISCUSSION.

At the request of Dr. Wells, the discussion was opened by

DR. CAUTHORN: The subject of hernia, from the special standpoint of Dr. Wells was brought to our attention last year incidentally, but not in the manner that it should have been brought. As the doctor suggests, it was again brought to the attention of the medical profession at a meeting of the Portland Medical Society, where some considerable discussion was evoked, and where I remember to have myself argued that the result from the doctor's two cases was a coincidence, and where I also remember that I argued, upon anatomical grounds, against its possibility. So that when I had an opportunity last month, as the result of some anatomical work I was going to do myself, to get a cadaver, I called Dr. Wells' attention to the fact that I would be glad for him to solve the problem, and that I would be pleased to assist him, as I was very much interested in finding out how far the inner abdominal ring was from the anus. So we made some examinations. I did not believe that the rectum was so distensible that a finger placed upon the anterior wall of it could be brought in contact with the anterior wall of the abdomen; in other words, that the rectum could be made to stretch entirely across the true pelvis; but, as the doctor has said such is the fact; and although the finger cannot be made to reach to the site of the internal ring absolutely while the ring is in position, it could be brought to within three-quarters of an inch or an inch of the ring; but with pressure on the ring, the fingers may be approximately brought into position by this combined method at either abdominal ring, so that it seems to me the doctor has demonstrated that there is something in this method, and that by this means you may exert a little pressure, a little traction, from the inside and thus assist efforts at reduction. I wish to compliment him on the interest he has taken in this matter, in solving, I think per-

haps for the first time, the anatomical relations of this process, and I am highly gratified that I was associated with him in this case. As a matter of personal interest, I wished to know how far distensible the rectum was, and I am coming to the conclusion that there is practically no limit to the powers of research through the rectum; for if it will stretch entirely across the true pelvis, and allow one to reach through the abdominal ring the inguinal ring, it certainly is a very distensible organ; and the method of research possesses considerable value.

DR. FULTON: I think this is one of the most important papers we have had so far. I remember a year ago that Dr. Wells mentioned this case, wherein he had used that case-knife, wrapped, and succeeded with it, and at the time I rather thought it was an accident, but I am glad to know that it has been demonstrated to be possible. I think there is a good deal in it.

DR. CAUTHORN: If you will allow me to say one word more Mr. President. There is a slight objection to this method. Dr. Wells only claims for it that it is or may be an adjunct to ordinary taxis. The same objection may be urged against it of course that may be applied to ordinary taxis; and it could be only used in those cases in which ordinary taxis is at all justifiable. For my part, I am extremely doubtful about applying taxis, unless it is applied very early. The application of taxis after twenty-four hours, or even within a less time than that, if the symptoms of strangulation be severe, I consider one of the most dangerous things in the world, so much more dangerous than the open incision that they are not to be mentioned in the same connection. Therefore, with this restriction, the method is of value and may be used only in connection with those cases of very early strangulation, where the symptoms are not so acute.

DR. HOLMES: Mr. President, I was present last year when this method of taxis was mentioned. I can see how an

instrument introduced through the anus, making pressure in the neighborhood of the abdominal rings, may exert a direct traction upon a herniated bowel and help to reduce it; but it seems to me that the method might be accompanied by some danger. If one could reach into the bowel with the hand and gently manipulate, taking hold of the bowel, as Tait does, when having opened the abdomen, pulling back the intestine, I can see it would be quite safe; but to grope about with an instrument with which one is not entirely familiar—a case-knife or any other instrument—it seems to me that one might do damage; and rather than to grope about in the intestine with an instrument that one is not entirely familiar with, it seems to me that the operation which Tait proposes, of opening the abdomen, reaching into it and grasping the intestine and gently manipulating it, would be more safe. When the doctor comes to close the discussion, I would be glad if he would just lend us the benefit of his experience and judgment, by telling how much force might be safely applied. I think there is something in this method of taxis, too, of considerable value; but without more experience than I have had—I should be afraid that, if inflammatory processes had gone on, possibly a softening had occurred that, by manipulating with an instrument, which is not entirely under one's control, some damage to the part might be caused.

DR. DOANE: Mr. President, one thought has occurred to me. In the investigation that Dr. Wells and Dr. Cauthorn have made upon that cadaver, by demonstrating the fact that on that cadaver the internal rings could be reached with manipulation through the rectum, it seems to me that that does not necessarily demonstrate that the same thing can be accomplished on the living subject. I know nothing of this matter experimentally. I do not know what the difference would be in distensibility in the tissues in the living subject and in the cadaver; but I apprehend that if that cadaver had been dead a long time and the tissues had lost all their vital powers of resistance, that something could be accomplished

very easily, although it would be extremely difficult in the living subject.

DR. CAUTHORN: The man died the day before.

DR. DOANE: With that explanation the difference, of course, would not be so great. Still, granting the possibility of accomplishing that in some subjects, there are, evidently, a good many cases in which this would not be available, and I apprehend that it will be in perhaps a very small minority of cases, that this method would be applicable. We sometimes find, when apparently all hope of reducing a hernia is gone, that is, all hope of reducing it by the ordinary methods of taxis, that our efforts, perhaps when we least expect it, are suddenly successful. I recall in one instance when another physician and myself were engaged in attempting to reduce a hernia and we had given up the case and were discussing what to do next; we had abandoned all operation and nothing had been done for the patient for the space of nearly half an hour, and while we were getting ready for the next step, through some change of position, which the patient made himself, or some other favorable circumstance, the bowels spontaneously returned.

Now, I apprehend that in the case detailed by Dr. Wells, wherever the finger can be made to reach the tumor and perform that motion—which the doctor reporting the case in the *Medical News* likened unto the working of the cork in the bottle—wherever the finger or any instrument can be used which is as much under the control of the surgeon using it as the finger, then the method can be made available perhaps and would be justifiable, and would be wholly in the surgeon's control; but, except in very rare cases, it strikes me that the chances of doing damage by the introduction of an instrument which is not under the surgeon's control, would certainly be an operation of danger. And yet even the most dangerous expedients are sometimes resorted to successfully. I can recall a circumstance which took place several years

ago, in which we succeeded in reducing a hernia by a device which would not be called combined taxis. It would not merit the title which the doctor has given to his method, yet at the time it was to me an unheard of procedure.

I was called to attend a case of right inguinal hernia of long standing which had been reduced and restrained by a truss. The patient was a laboring man, weighing 190 or 200 pounds, I should judge; we found him with the hernia down, and he was suffering great pain. First we put him under the influence of an opiate to relax the parts and restrain the pain, and then attempted to put him under the influence of an anæsthetic. Neither chloroform nor ether could be successfully given, as the moment that either agent was given he began to vomit, and the vomiting was so intense and prolonged and utterly uncontrollable, we feared that the hernia would only be contracted more tightly. In his despair, he insisted that the hernia or the hernial tumor or sac should be punctured. He made the statement to us that that expedient had been resorted to by another physician several years before and that it had succeeded. This was in the middle of the night, and it was at a place where help was impossible and we were not prepared for an operation. Finally at the urgent solicitation of the patient himself, we introduced a small needle. The gas was withdrawn from the bowel and immediately, or within a very few moments, the bowel went back. What impressed it more upon my mind was the fact that, although we gave him strict orders to remain in bed, by no means to go out until he was permitted, on calling again in the morning to see him, I was surprised to find that he had got up and gone away to his work. I had a great dread that we should have some bad results, and symptoms of peritonitis or of possible extravasations from the bowel were in my mind for some time. The man resumed his work and we never heard of him except that he recovered entirely and had no trouble at all.

Now, while this is not an expedient that I would recommend or that I think would be safe to use in many cases, still,

it simply illustrates the idea that we may sometimes succeed by a desperate expedient, which perhaps is not advisable.

DR. FULTON: While I am personally very much opposed to taxis to any extent or for any length of time, no matter if you see a hernia within a half an hour, taxis beyond ten or fifteen minutes, it seems to me, is very dangerous. But this case illustrates a principle which is advocated by some at least, that by making tense the abdominal wall, instead of relaxing it, it sometimes assists in the reduction of a hernia. I think, perhaps, by making counter-pressure there, it gives a support which perhaps steadies the ring. There might be something in that.

THE CHAIR: Is there anything further on the subject? If not, Dr. Wells will close.

DR. G. M. WELLS: Mr. President, as I stated in this communication, I had not seen anything on the subject prior to reading up for this paper. Then my friend Dr. Cauthorn called my attention to this article in the *Medical News*, in which the author had claimed priority and thought also that he had discovered it. In the Hand Book of Medical Reference, the author of the article on Hernia speaks very plainly of this, that not only two fingers, but that the whole hand may be introduced in adult subjects by persons having hands suitable. A report of the case by Dr. Brown of Birmingham, demonstrates to my mind, conclusively, the value of this method in all young subjects. There is no question, if you take his statement as true, that you can reach the internal ring by one or two fingers in the rectum in children; and, if that is the case, no surgeon is justifiable in placing his patient upon the operating table until he exhausts this method of reduction. Neither is it so in the case of an adult. If you can exert enough influence by taxis to unbutton the hernia, it is your duty to do so before you put your patient under the knife; and I hope any of you that will try the method before

the operation will be satisfied that in a good proportion of cases it will be available.

My first patient was ready for an operation at St. Vincent's Hospital, and Dr. Mackenzie, who is on the floor, can verify the condition that the patient was in and the preparations that we made and the facility with which the hernia was reduced by the rectal method which had utterly failed in the hands of Dr. Hickman by the ordinary method.

In answer to Dr. Holmes, as to the amount of force to be used, and the danger of traumatism produced by a wound from a blunt instrument, or the shock for instance from an electrode or metallic substance of wood, indeed a substance of any kind that will aid your fingers in reaching the ring, I think is timidity. Dr. Doane would puncture a hernial sac with an aspirating needle or with a hypodermic needle and reduce the hernia rather than subject the patient to a knife. I myself reduced a hernia some years ago under circumstances similar to those stated by Dr. Doane, in a young subject, by emptying the sac of its liquid contents by the hypodermic method. You would fear introducing traumatism into the sac, but you would not fear introducing traumatism by opening the abdominal walls. It is a contradiction. Then as to the amount of force, I would use the necessary force to reduce the rupture; I would run the risk of rupturing the rectum, rather than subject the patient to an operation for hernia, if I could not do it without.

In our demonstration of the case on the cadaver, Dr. Cauthorn and I ascertained that we could pass the two fingers by impinging them closely on the perinæum, to about the depth of three inches, in the cadaver the resistance of the perinæum being entirely overcome as it would be with a patient under the influence of an anæsthetic. I believe really there is more relaxation of the sphincter under chloroform than there is in articulo mortis. I do not think the sphincter relaxes more in articulo mortis than it does under chloroform, because I demonstrated it fully in that subject; the doctor

and I tested it fully. You can get further into the rectum by your fingers or by an electrode under anæsthesia than you can on a cadaver, and you can exert more internal effect there. If you empty the rectum out thoroughly, as the doctor and I did in this case, then you have an empty rectum, and if you have a small enough hand, you can carry your hand in there and almost unbutton the hernia.

I am fully persuaded that the profession stands in its own light if it refuses to test this method before subjecting a patient to an operation, and in all cases of children I exhort you to adopt this method of combined taxis. (Applause).

THE PATHOLOGY OF SOME UNUSUAL CONDITIONS IN ACUTE
STRANGULATED HERNIA.

BY F. CAUTHORN, A. M. M. D.

Visiting Surgeon to St. Vincent's Hospital, Surgeon N. P. R. R., Ex-President of Oregon State Medical Society, etc.

It is not my intention, in presenting this paper for your consideration, to enter into a discussion of the subject of hernia in general, however important the subject may be from a surgical stand-point; but rather, to call attention to some special conditions that I have met with in practice that are not fully considered in any work on surgery with which I am familiar.

As the result of this experience I have been led to draw certain conclusions as to the pathology, diagnosis and management of these conditions, that I have for some time wished to bring to your attention, that we may thus compare notes to our mutual advantage.

I am sure that there would be but little difference of opinion between us, as to the management of a case of strangulated hernia, presenting all the cardinal symptoms as represented by an increase in size of the hernial tumor, pain, irreducibility and absence of impulse, associated with evidence of shock and vomiting, going through its various degrees up to the stercoraceous. We are seldom at a loss as to what to do in typical cases of any disease. It is the atypical case, the one that in many essential particulars conforms to the rule, but in many others does not, that puzzles the judgment and causes too often a fatal delay in instituting the proper procedures for its relief. It has been my peculiar fortune to meet with several such cases of strangulated hernia; in fact,

with such a number that I am inclined to believe these conditions prevail to a larger degree than is commonly thought. In the first place there are various degrees of strangulation, an essential point to be borne in mind. In its simplest degree it is merely a constriction sufficient to occlude the calibre of the intestine, without marked disturbance of the blood supply. In its most exaggerated form it is the complete arrest of the circulation in addition. Perhaps in those cases of acute hernia brought on by violent efforts in a person not before subject to a hernia, the second condition prevails almost from the beginning. Of cases of acute strangulation of previously existing hernia I believe the great majority go through all these degrees if left to themselves.

Before proceeding further, I desire to say something of the pathology of strangulation in general.

I think we may set it down as a fact that it is never due to an actual contraction primarily of the neck of the sac or surrounding tissues, a theory that was so long held. There is a *relative* narrowing of the neck of the sac by reason of an increase *absolutely* of the contained parts. Let us consider for a moment how this comes about: A man suffering from a reducible rupture undergoes some severe exertion and finds that the hernia is no longer reducible. A larger amount of the intestine than usual is forced into the neck of the sac, which we may suppose, has yielded of its own resiliency however slightly, and now makes constriction at this point by just this amount of resiliency. The return of venous blood is impeded, not stopped entirely and a passive congestion of the gut ensues with swelling. The constriction is increased *pari passu* with this increase in size of the tumor, *relatively* but not *absolutely*, until the caliber of the gut is occluded. The congestion being within the sac, more intestine may be forced through the neck by coughing or bearing down and thus the hernial tumor may be in some cases indefinitely increased in size. But when the taxis is employed, we find we can only reduce this super-added and uncongested portion. As

the congested part approaches the neck there is not room to accommodate it and the contents of the intestines, faeces or gas, are stripped back, thus forming an inseparable obstacle to complete reduction.

This is the explanation I have to offer of those cases in which there is impulse on coughing with possible increase in size of tumor and in which from lack of complete obstruction to the blood supply there is absence of the more severe symptoms of shock and vomiting, etc., in the early stages.

The condition is only remediable by operation and is a very deceptive one and too often causes a delay by the absence of the typical signs of strangulation. Its recognition is not a matter of so much importance to the surgeon who would operate early in any case of hernia that could not be reduced. But unfortunately this view is not held by many general practitioners and thus they are deluded into temporizing with hot and cold application, with opium, and repeated trials of the taxis until the last degree of strangulation is reached and irretrievable damage has been done. The general outlines of the following case will illustrate what I have tried to say in regard to these cases of partial strangulation.

I was called to see Mr. P. aged 66, who had suffered for years from a rupture. Six days previously he had, while at work, suddenly felt his rupture come down, and experienced a severe sharp pain at the same time. His own efforts at taxis failed and a physician was called who arrived several hours afterwards and who also likewise failed at reduction. However, after a few hours the local pain and soreness subsided. There was occasional vomiting, bilious in character, four or five times in the twenty-four hours, until the day upon which I saw him, when it became suspiciously stercoraceous. He was comfortable, but weak. A local examination showed a moderate hernia, which gave distinct impulse on coughing and which could be increased in size indefinitely by bearing down. Efforts at taxis were followed by easy reduction of the larger part of the mass, but at the internal ring there al-

ways remained a perceptible amount that could not be reduced. His medical attendant stated that this had been the history of the case since its beginning. An operation was performed and the condition described above was found. The gut was congested and thickened, but not to such an extent as to endanger its vitality. The patient lived about twenty-four hours and died of exhaustion incident to inanition and the prolongation of the mild degree of the shock. There had been, during the whole time no really alarming symptom, except the inability to completely reduce the tumor and the very gradual progressive exhaustion. The moral I would have drawn from this case is this: No matter how mild the constitutional symptoms; no matter how nearly the rupture may be reduced, if it cannot be entirely reduced, do not wait more than a few hours before resorting to operation.

The other class of cases to which I wish to call your attention is represented by the so-called "Reduction en masse" or "en block" or the "properitoneal hernia" of Kronlem, and I wish to introduce what I have to say on this subject by giving an outline of two cases that I have met with.

CASE 1. A steamboat man about thirty years of age, had suffered for years from a rupture for which he had worn a truss. While trucking upon the boat, his hernia came down and was reduced by a physician and the truss reapplied. After a day or two's rest in hospital he again went to work; but complained that things did not feel right at the site of the hernia. An examination failed to show the presence of the hernia. The rings were very much enlarged and lay directly over one another as is often the case in old herniæ, thus leaving a direct opening into the abdomen. The finger could be passed into the abdominal cavity to its full length and twisted around, without difficulty. He presented some symptoms of strangulation, however, in the way of vomiting etc., and was sent to the hospital and came under the care of Dr. G. B. Young, formerly U. S. Medical Surgeon at this station. By the following day his general symptoms were all worse

and Dr. Young asked me to see him again. The local condition was the same as the day before. Percussion over the ring gave distinct dullness, which, at the time, it was difficult to account for, but it suggested that something was wrong at this point, and this together with his general symptoms caused us to operate. The sac was found entirely within the abdominal cavity, the neck, the point of constriction, having been separated from its attachments at the ring, lying fully four inches away from the abdominal wall. An amount of intestine represented approximately by the size of the fist was strangulated and very much congested and thickened. The neck of the sac was pulled up and cut. The blood supply soon re-established itself and the intestine was returned. The patient made a good recovery.

CASE 2. A man about fifty-five years old had suffered for years from a hernia; had worn a truss which had kept it in place. Six days before I saw him his hernia had come down and been reduced; but he complained of pain within the abdomen on the same side. The canal was patulous and the finger could be pressed up to and within the internal ring meeting at this point with an elastic tumor. General condition was extremely bad; there was profound prostration of the nervous system, stercoraceous vomiting, etc. Percussion showed dullness over the internal ring which extended toward the median line, well behind the rectus muscle diagnosis made of strangulation within the abdomen and although well nigh hopeless an operation was advised and done. A free incision was made with the upper limit well above the internal ring and all the structures cut down to this point. The various components of the cord were recognized; but I confess to have been sorely puzzled at the entire absence of anything that looked like a hernial sac. The internal ring was enlarged and just within presented this elastic cyst-like tumor. I determined to find out just what condition of things was present and very carefully cut into this tumor which proved to be the intensely distended hernial sac. There was an immediate es-

cape of a large amount of bloody, foul smelling serum and a mass of intestine the size of the double fist, black and gangrenous presented. The neck of the sac was found to lie well behind the rectus muscle and nearly in the median line. It could now be lifted up and brought forward which was done, and the constriction cut; some ten inches of the spachelated gut was removed and on account of the condition of the patient the two ends were stitched to the external wound and the operation hurriedly completed.

The progressive sepsis that was present at the time, and profound prostration continued and the patient died in about thirty-six hours.

An earlier recognition of the condition would undoubtedly have been successful, as in the other case.

The only difference between the two cases consisted of the greater amount of serum in the latter case, the sac being distended by it to its full capacity.

PATHOLOGY: The two cases present the following peculiarities: In both cases the sac was entirely within the abdomen, the neck being removed from the internal ring by several inches. The question arises how is such a condition brought about. These cases were both cases of long standing and both had worn trusses for years. It seems to me that in this fact lies the solution of the problem. The element of time combined with the pressure of the truss supply the essential conditions. Upon the first appearance of the rupture and while the sac is small the truss is applied. This keeps the hernial mass within the abdominal cavity. Let us suppose a certain degree of laxity of attachment of the neck of the sac to the internal ring. A small amount of intestine may then find its way into the sac, but is prevented from coming down by the presence of the truss. The pressure thus exerted presses the neck back from its attachment to the ring, and thus as it recedes the hernial sac increases in size and comes to lie entirely within the abdominal cavity. The conditions determining strangulation are of course the same as

in any other case of rupture, except that the constricting tissues are solely the puckered folds of thickened peritoneum at the neck of the sac.

I wish to emphasize the importance of percussion in recognizing this condition, as it is the point of most importance and one which I have never seen referred to in this connection. It has been resorted to to determine the contents of a hernial mass externally; but not to determine the presence of internal strangulation. If percussion over the internal ring gives dullness or flatness it indicates the presence of something abnormal; in this case the presence of congested intestine with more or less serum in the sac and if associated with other symptoms of strangulation indicates the site of the trouble. Again it should be of value after reduction as indicating whether the reduction is complete or merely the return of the sac with its contents into the abdominal cavity.

It seems to me that these two cases strongly indicate that all cases of reduction *en masse* are simply the return of the intestine into a preformed sac which under ordinary circumstances and under the pressure of the truss normally lies within the abdominal cavity. I cannot believe that taxis ever strips up the neck of a sac primarily as our text books would have us believe.

DISCUSSION.

At the request of Dr. Cauthorn, the discussion upon his paper was opened by

DR. A. C. SMITH: I have not had any such complications as Dr. Cauthorn has enumerated, but I can readily see how they would have existed. The only complications that I have seen are a couple that I have given in my report of hernial operations heretofore. One was a hernia of the coecum, and evidently the appendix had taken on inflammation after the hernia had become irreducible; it had been irreducible for several weeks; the hernial mass was very tender, and

the appendicitis probably accounted for this condition. Now, in this case there was an enormous thickening of the sac, so much so, that, after having cut through several inflammatory layers, I lost my bearings and thought I had gone through the sac entirely, and I finally cut very cautiously, thinking that I was going to enter the bowl itself, but I found it was only a thickened sac and within it was this inflamed, suppurating appendix. The constriction was apparently not in the ring itself, resembling the condition of things that Dr. Cauthorn mentioned, where, if I understood him correctly, the constriction is within the sac and not within the ring. Is that it?

DR. CAUTHORN: No.

DR. A. C. SMITH: The sac itself constitutes the constriction?

DR. CAUTHORN: No, it is the hernial opening, in which the sac has been shoved back from the attachment to the ring.

DR. A. C. SMITH: There were in this case what I considered inflammatory bands surrounding the neck of the hernia, and they constituted a constriction within the constriction, within the ring. I first incised the ring and I found that I could not reduce the descended mass—there was still a mass; then I made a slight incision into these constricting bands and they gave way, and I massaged the contents of the immensely distended coecum back into the intestine, and removed the inflamed appendix, and finished the case as an ordinary radical cure.

By the way, to follow up this case, which I never reported before, about eight months later this man came under my observation (this bears more upon appendicitis than upon hernia). This man came back under my observation with extreme tenderness and dullness in the right iliac region and with a slight temperature, only a temperature of 99 to

99½. I did not grasp the situation at once, remembering that I had removed his appendix, did not consider that it was appendicitis, and I treated him expectantly for a few days. A few days later I was called hastily and I found him in a state of collapse; in fact he died while I was there. I held an autopsy and I found a perforation of the stump of the appendix had sloughed away, an escape had taken place of the intestinal contents, and general peritonitis resulted. That case is a strong argument I think in favor of using the Lembert suture, inverting the stump after amputating the appendix. In this case I simply used the single circumscribing suture and it had evidently sloughed off at this late date, nearly eight months.

The other case that I was going to mention as a complication, was simply an inguinal hernia in which a half descended testicle was densely adherent to the bowel. The only thing I thought I could do was to remove the testicle, which I did, and reduce the hernia. These are the only cases of any complication worth mentioning, that I can recall I have seen in my hernia experience.

DR. CAUTHORN: I would like to hear from Dr. Carll.

DR. CARLL: The only remarks that I could make, gentlemen, would be in commendation of Dr. Cauthorn's paper. I had brought to my attention a short time ago a rather interesting case of so-called hernia. Diagnosis was made by percussion. Percussion, by the way, would be very good, but in this case I do not think it substantiated the diagnosis of hernia. The case came down from Idaho. The patient was an official of one of our transcontinental lines, was taken ill at a station in Idaho, called in the company's surgeon, and the surgeon made a diagnosis of double inguinal hernia, said it was the worst case he ever saw, or the worst case *she* ever saw—I think it was a woman physician who had been appointed local surgeon of the line. She sent the gentleman to the company's hospital, and from there the

surgeon in charge was about to operate, when the patient declared that he would not stop there. Being an old friend and patient of mine, he came to me, and without entering further into the case, I will say that I put the patient on mercurials and specific treatment, and he has been doing quite well ever since. That double inguinal hernia proved to be something else. But percussion on both sides demonstrated very marked dullness!

It is hardly in line to discuss the paper of Dr. Wells, but I think the points that Dr. Wells raised are certainly very good, and certainly his recommendation to try them is good. The point with reference to speedy and immediate operation in hernia is of course acceptable. It is almost heresy to say anything in opposition to that; but still I believe that we all recognize the fact that it is safe to wait a reasonable length of time. The application of ice bags is often good. I had a case brought to my attention a short time ago where a physician had been trying taxis about fourteen or fifteen hours. I found that taxis for about fifteen minutes and the application of an ice bag afterwards, reduced the hernia. But it has been a mystery to me why he was not able to reduce it in the hours that he had tried it. Surely none of us would be justified in using forcible and energetic taxis for that number of hours, and I do not believe any of us would do it; but it occurred to me upon thinking the case over afterwards, I was not sure in my mind but what the physician had been successful in his taxis. I thought I heard the characteristic reduction of the mass, and therefore stopped my manipulation and applied the ice bag; but it is barely possible that the long manipulation he had subjected the patient to had reduced the inflammatory condition, and the ice was the thing that did the work after all. I know from experience that it is possible to have the sac, inside of the abdominal wall, and certainly after we reduce hernia, if the symptoms continue, then the operative method must be en-

gaged, and it would be most inexcusable for any of us that did not do it.

THE CHAIR: If there is no further discussion Dr. Cauthorn will close.

DR. CAUTHORN: Mr. President, I do not know that I have anything further to say. I had hoped that this would elicit some similar experience on the part of others. Dr. Jones, when speaking of this on a former occasion, called attention to the fact that he had met with one similar case. I believe there have been a good many cases of hernia reduced that have died with the idea that perhaps the hernia had been reduced, but that the gut had become spatulated, and that the cause of death might be placed in this category. If, however, in an assemblage like this we cannot dig up more than three cases of this kind, the condition is perhaps not so common as I thought it might be. I believe, however, that the report of this German physician that I mentioned, states that he had at that time only collected about fifteen cases of this kind. It seems to have been my rare misfortune to meet more than my proportion. I have not seen this question of percussion dwelt on anywhere. I do not mean, you understand, the question of percussion in the diagnosis of an ordinary scrotal inguinal hernia. That is resorted to, and has been from time immemorial, to ascertain whether the contents of a hernia are a distended intestine or not; but I mean the use of percussion over the ring to ascertain the condition within the abdominal cavity immediately under that sac. And that is the only particular point I dwell on, and I say I believe it is of value; it has proved to be thoroughly and absolutely reliable in both of those cases, and I can see no reason why, from the well known laws of natural philosophy, that it should have failed in that connection. If there be a solid and thickening congested bunch of

intestines underneath the ring at that point, percussion will certainly elicit dullness there, and I should imagine that in the case Dr. Carll spoke of, the treatment the patient had received would elicit a good deal of "cussing" without the "per."

MEMBRANOUS ENTERITIS.

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President Oregon State Medical Society.

(Read before the Oregon State Medical Society, June, 1895.)

Membranous Enteritis is a remarkable and somewhat rare disease, the characteristic feature of which is the passage from the bowels, at more or less frequent intervals, of large quantities of tenacious mucus, varying in form and consistence from irregular flakes, strings, or masses of feebly coagulated matter, to fully formed and seemingly organized moulds or casts of the bowel in which they are produced.

The pathology of this disease has not been definitely settled, but the source of its characteristic products is generally conceded to be the muciparous glands of the colon, which under some abnormal stimulus, pour out an abundant secretion which subsequently passes through a process of solidification and at times of an apparent organization.

The different forms observed probably depend upon the varying chemical conditions of the intestinal secretions and contents, as also upon the length of time which may elapse before the mucus exudation is separated from the wall of the intestine and finally extended from the bowel.

The affection has been described under various names by most writers on practice, the most complete presentation of it which I have found being the article on Pseudo-membranous Enteritis, by P. S. Wales, in the second volume of Pepper's System of Medicine.

I will not attempt to discuss at length its pathology or causes, further than to say that the disease is non febrile and is probably not of an inflammatory nature, although the pain-

ful and at times violent gastro-intestinal disturbances accompanying it have led to the employment of many designations ending in the termination itis, signifying inflammation. Though not confined to any age or sex, it occurs most frequently in young or middle aged women of nervous temperament, and is usually preceded or accompanied by hysteria, melancholia, or some other manifestation of the neurotic constitution. Quite frequently there has been antecedent dysmenorrhœa or some uterine or ovarian disease.

The prognosis is not encouraging, for, though seldom fatal, the disease usually becomes chronic, and as with other directly nervous affections, unless some radical change can be effected in the patients circumstances and surroundings, lifting her as it were, out of her morbid environment and awakening in her newer motives and interests in life, there is little prospect of ultimate cure.

The treatment consists in the use of anodynes and quieting measures to relieve the more painful symptoms, the administration of the various mineral and vegetable alterative tonics, with nourishing alimentation, and that wise and tactful administration of every detail of management, so difficult to attain but imperatively demanded in the care of every nervous disease.

The clinical history may perhaps be best presented by a report of the following case which has recently been under my care. Mrs. B. was first seen on January 14, 1895. The patient was a native of Wisconsin, aged 32, married 10 years, with one child five years of age, and had had one miscarriage about seven years ago. Her family history, so far as it could be traced, seemed clear of any hereditary disease or neurotic tendency. Her own health had been good up to the beginning of the present year, though I elicited information of some slight hysterical manifestations which occurred while a girl at school. Menstruation had been regular and painless, there was no history of any uterine trouble, and physical examination failed to reveal any pelvic disease. During the

past two or three years the patient had worried a great deal about financial matters, as well as over family affairs. Her husband was for a time employed as a night watchman and she brooded much over her loneliness during the long winter evenings and nights, and because of his constant refusal to go with her to places of amusement or social enjoyment. When first seen she was suffering from occasional paroxysms of severe abdominal pain, with nausea and vomiting. The bowels were moderately distended with gas, and there was slight tenderness over the ascending colon, in which I thought I recognized a mass of fecal matter which was acting as an obstruction. The tongue was lightly coated, and the temperature normal.

The painful symptoms were easily controlled with anodynes, but I had great difficulty in getting any action from the bowels. Various purgatives were faithfully given without effect, and on attempting to make use of enemata there was the greatest difficulty in introducing more than a few ounces of fluid, which however bland and unirritating, was at once ejected without effect. A rectal examination was made but there was no fecal impaction found. After three days of continuous effort there was a small passage of hardened scybalæ, followed later by another scanty in quantity but nearly normal in appearance. The patient seemed greatly relieved and thought she would have no more trouble, though I advised perseverance with enemata of glycerine and water until the bowels should be thoroughly cleared out.

On March 10th I was recalled to the case and found the same symptoms as before, except that the pains were more severe and cramp like in character, and the patient complained of a distressing lump in the abdomen, which seemed to be migratory, sometimes appearing in the right side, at others in the left, and again just above the umbilicus, as though due to spasmodic contractions or cramps of these various sections of the colon. Whenever the patient was so far under the influence of anodynes as to be free from pain the

lumps or tumors would vanish, only to reappear with the return of painful paroxysms. The same difficulty as before was experienced in clearing out the bowels, though from repeated use of enemata these could be more readily borne and retained in greater quantity.

About March 15th the patient passed a large number of translucent cylindrical bodies, resembling in shape, size and appearance, lumbricoid worms which, when first seen by a feeble lamplight, I supposed them to be. A subsequent closer examination, however, showed them to be, not worms, but masses of structureless matter of worm like shape; from three to seven inches long, smooth and translucent, with both extremities rounded and blunt, but lacking both the transverse striæ and the longitudinal markings of true lumbrici. In color they were a pinkish white, and through the center of each could be traced from end to end an irregular channel resembling an alimentary canal, studded in its course with numerous little sacculi or pouches, the whole filled with an opaque material which rendered the entire length of the canal dark and distant in outline, and added very materially to the worm like appearance of these bodies.

With the passing of these bodies the patient experienced some relief which, however, proved but temporary. Constipation soon became more obstinate, and the attacks of pain and vomiting more frequent and distressing. For relief, large doses of opiates were found necessary, and purgatives of every name and nature, in large doses and small, enemata of every conceivable character and description, various local applications, inunctions and frictions, with massage of the abdomen, were all tried without avail. Occasionally, at intervals of one or two weeks, the obstruction would seem to yield and the bowels move once or twice perhaps but never freely. At no time were there present the chain of extreme symptoms indicating acute strangulation or a complete obstruction and the vomiting was but once of a bilious character and never became stercoraceous.

A few days after the passage of the worm like bodies, there began to appear numerous irregular masses of semi-solid mucus resembling closely the white of an egg just beginning to coagulate under the influence of boiling water. These in turn gave place to large flakes of matter having a membranous appearance, ranging in color from pure white to a dingy yellowish gray, and from one to three lines in thickness. Sometimes the membranes would be smooth and dark in color looking like fragments of sausage casings. Occasionally there would be a few masses of the original vermiform appearance. All these varying forms continued to pass in considerable quantities during the whole of March and April. On May 19th and for several days thereafter there appeared large quantities of more fully formed membranes, which proved to be complete cylindrical molds or casts of the intestine, pure white when first passed, varying in length from one to three inches, and in thickness from the thinnest film to two or three lines. They were too fragile to bear much handling but when floated out in water, showed the outer surface thrown into shallow folds following for the most part the long axis of the intestinal wall.

During all this time the patients general condition grew gradually worse. Owing to frequent vomiting but little nourishment could be taken, and then was progressive emaciation and failure of bodily strength. There were no special nervous phenomena or disturbances of any of the special senses, though as would be expected from such a general condition she soon became nervously weak, irritable and despondent, with little enjoyment of the present and scarcely any hope for the future.

On June 3rd the patient passed out of my care and was taken to Portland, so that I am unable to follow her history any further except to say that she was seen by one or two physicians of this city, by whom she was cared for until relieved of her sufferings by death on the 8th of the present month.

DISCUSSION.

At the request of Dr. Doane the discussion was opened by

DR. HALL: This subject has recalled very vividly to my mind the case of a little child about six years old, that I was so unfortunate as to lose a few weeks ago, from very much the same condition as described by Dr. Doane. I don't know what more could have been done for the patient than Dr. Doane described, for it seems that after passing from his hands into the center of our medical operations it met with a speedy death. It reminds me of a little story that I read a few days ago in one of our medical journals. A patient was very anxious about himself, and sent for a doctor and said that he was very much afraid he was going to die, or be buried without dying, until he had fallen into the doctor's hands, and then he was satisfied he would be dead before he was buried.

In the few cases of this character that have fallen under my notice, treatment has been very unsatisfactory. At this time I have a teacher in one of the public schools in Salem who has been compelled to resign her position on account of the condition described by Dr. Doane, and whatever treatment I have been able to suggest has been very unsatisfactory. I do not know that I could add anything to the pathology or diagnosis or the treatment that Dr. Doane has offered and would be very glad to hear anything that any physician present would suggest that would result in a speedy cure.

DR. COE: About seven weeks ago a woman was brought to me from the State of Washington, and the diagnosis of her case by the physician and by the family, was nervousness, which is a very broad term and covers a very large field. Nervousness well describes it, because the woman was neurasthenic. She was in a state of melancholia, bordering on insanity; and among other troubles that she complained of

was this condition of the bowels. She had a number of troubles, among others, eye troubles; and I sent her to Dr. Eaton who fitted her eyes with glasses. She was able to be about, but suffered terribly; unable to sleep and unable to eat. She was placed upon a milk diet and for some four weeks she took nothing but milk. She passed large quantities of this mucus from her bowels; it was not in the shape of casts, as pointed out by the doctor, but consisted simply very large masses of mucus. She was treated with galvanism and mild laxatives used, and she is at home now and is very much improved in health. She has had no discharge of mucus from her bowels for some two or three weeks. She is still on milk diet. I would say, of course, she is not in the condition that Dr. Doane's patient was, because she was not as low as that. She has been troubled for some five or six years.

DR. HOLMES: I was hopeful that Dr. Coe would relate another case, one that I have recently seen with him, a case that has been very graphically described by Dr. Doane. If he had had the one to which I allude in his mind, he could not have described it more accurately than he did. I supposed Dr. Coe was going to speak of it; it was one that I saw with him not less than three weeks ago. The woman has had this catarrhal bowel trouble for a long while, had all of the nervousness that the doctor speaks of, and I was reminded of the case when Dr. Coe said that with such conditions as he was then dealing there were frequently associated dysmenorrhœa, ovarian and tubercular diseases. I will now relate some of the peculiar symptoms of this case. The patient was 46 years old. She consulted Dr. Coe six weeks ago. The case might be considered interesting from a medico-legal point of view, for she is just on the point of suing her husband for divorce; I think once he did bring some action against her. What was it for, Dr. Coe?

DR. COE: Insanity.

DR. HOLMES: It does not change the situation materi-

ally. She relates some things that are astounding. She began complaining of her husband's inattention to her something like a year after this catarrhal bowel trouble was noticed, and I mention it not for the purpose of jesting, but with a desire to throw some scientific light upon these peculiar cases. For the first seventeen years of their married life her husband was in the habit of having sexual intercourse with her, which she received as an indication of his loyalty and love, as many as eleven times every twenty-four hours; during the last year he had only been with her in that way as much as about ten times in the twenty-four hours, and during the last six months, had only had intercourse with her about five times a day. And figuring in that way she supposed that in two or three years he would care nothing for her at all. So now she declares that he is untrue to her. Dr. Coe explained to her, however, that as age, gray hair and wrinkles were creeping upon a man, he would change somewhat in his disposition. And yet she does not take that as any legal tender, and is about to institute suit for divorce, I believe. It seems to me that the case is unparalleled. Indeed I cannot conceive of the physical ability of individuals to carry on such practices to the extent that this woman relates; and I am sure she means all that she says.

DR. WHITE: I would like to say a few words about these cases, because within the last year it has been my misfortune to have some cases touching on this subject of enteritis. I have one case now I am treating. My cases have all been males; none of them have been among females, but I did find that neurotic condition—that does not speak of whether it had any bearing on the trouble or not; I am not prepared to say, but all cases I have had are distinctly neurotic, and the doctor has described my case as well as I can describe it. I will speak about my treatment. I have come to the conclusion from what experience I have had that these cases take their origin in imperfect digestion of food in the alimentary canal after it leaves the stomach. My patients

have not been troubled with vomiting; the stomach apparently is all right, but I have had all along intractable cases of constipation, mucus in profuse quantities from the opaque albuminous, in appearance, to that of blood; and along the ascending and descending and transverse colon you could distinctly mark out its impaction along the walls, with an apparent opening in the center; and you would only get this mucus and this membrane when a part of this impaction would seem to slip off the gut. There being such firm impaction, I conceived the idea of beginning at the lower end of the rectum and working from the bottom. So I put my patient on aloes, belladonna and nux vomica and I have succeeded tolerably in getting away quantities of fecal matter, of a dark, putty-like nature and of a fearful odor. The patient I have charge of now, I have had six weeks. To-night he was at my office just before I started here, and he tells me that he feels perfectly comfortable in the alimentary canal, and he thinks that the canal is entirely cleaned out. Heretofore he has had pains in the iliac region, both the right and left, and in the colon and especially in the left hypochondrial region and a characteristic gurgling sound that all these individuals laboring under spinal irritation have, and which you find in hysterical women and all other neurasthenic patients. That has entirely disappeared, and the gas that was found to accumulate before, has now gone.

As a stimulant and to aid the solitary glands of the alimentary canal in digesting I used pancreatin and ox gall, and so far I have succeeded well. How well I will get along with this case I cannot say.

The other cases have now been exempt from any symptoms of the mucus membrane, one for the past ten months, another for something over a year, and in this case is very slight mucus, a greater quantity always coming just before a large bolus, of this putty-like fecal impaction. I would advise those who have these cases, to begin at the lower end of the alimentary canal and clean it out, because if you

begin above you are liable to do some mischief. You take remedies that act on the upper bowel and they lose their force before they come to the lower bowel, and you do not get as satisfactory a passage as you otherwise would. I give a remedy that acts on the lower bowel, as we all know aloin does, and do not omit the pancreatin and ox gall, because I am sure they are valuable remedies in bringing about perfect digestion. It occurred to me in all these cases that nature never intended food to pass through the alimentary canal until it was perfectly digested. If it takes a week for nature to digest food, she will hold it in the alimentary canal that long, and if you want to get your bowels regulated the best thing you can do is to correct your digestion; therefore we give the ox gall and the pancreatin for that purpose, in view of predigesting the food, and then with nux vomica to stimulate the bowel, and by and by they will perform their functions in a normal way.

DR. BELKNAP: My attention was first called to this condition about twelve years ago when I was a medical student. From that time on I have had quite a number of cases and had my attention called to it in a number of different instances. My observation and study of the cases, most of them being of the nervous type, would not lead me to attribute this condition to be the cause of the nervous affection, but rather the contrary, the nervous affection bringing this about by the innervation. I have found it in a number of cases of melancholia, having had nearly fifty cases in my practice. I have found it in men as well as others, also in children. One case Dr. Mackenzie called my attention to some little time ago and I found no trouble, as in other cases, of easily removing the difficulty when the constipation was removed. In every case I found constipation. My method of treatment is that carried out by many others and is the electrical treatment. I also use in connection with it, massage of the bowels; and in all cases I strive to build up the general health of the patient, and by so doing improve every part of the body. My method

of applying the treatment is to use the rectal electrode and a large electrode over the abdomen and interrupt the current. I use sometimes the galvanic and sometimes the faradic, and a good many times combine the two and get a regular massage movement in that way by the current. In many cases, I have given them no medicine.

In using the galvanic current I use from eight to ten milliamperes, and in the faradic I use a current up to a degree that they can stand without any special pain. Muscular structures or fibres must be improved or increased, put into a healthy condition by the action of those agents, and therefore it takes a little time, and according to the best authorities in treatment of this kind they use no drugs and no medication whatsoever for that purpose, claiming better results than they get by using the medication, with the electrical treatment.

DR. MACKENZIE: Mr. President: It occurs to me that there has been some misapprehension on the part of some of the speakers who have succeeded Dr. Doane in their grasp of the subject that he dwelt upon. He treated of the subject of membranous enteritis, which I have always been led to regard as an exceedingly rare disease; if my inferences are correctly drawn from the subsequent speakers it is a very common disease. I have never seen a case myself of true membranous enteritis and I was very much interested in Dr. Doane's description of this disease. I have seen the parallel of this disease, however, in membranous bronchitis, in croupous bronchitis, or plastic bronchitis, as it is called. This disease I believe is equally rare. We find, however, in all mucus surfaces a parallel to croupous bronchitis, and I believe that membranous enteritis is a strict parallel to croupous bronchitis or plastic bronchitis. The disease in the lungs which I have seen is not only exceedingly rare, but exceedingly fatal, and where not fatal it is characterized by extreme rebelliousness to treatment. The symptoms produced by it are more severe, so far as the acute paroxysms are concerned, than that disease that affects intestines, and

it is necessarily more rapidly fatal. You must bear in mind the minute character of the finer bronchial tubes which are affected by this disease. We can readily understand how quickly this plastic material will fill them up and embarrass and finally prevent respiration entirely. I am rather inclined to think, with due deference to the frank statements that have been made by the different speakers, that they referred more particularly to different forms of catarrhal enteritis. I certainly am convinced that the case referred to by Dr. Belknap, was a case of catarrh of the large intestine, the presence of mucus in large quantities was present in that case, but there was nothing present at any time to my knowledge to evidence that such absolute casts of the intestines as were presented to us to-night in that bottle exhibited by Dr. Doane. I do not know whether Dr. Doane will bear me out in my position, but I feel convinced that the position I have taken is tenable and is correct.

DR. G. M. WELLS: I must place myself in the position that Dr. Mackenzie has taken in regard to this disease. In casting over my experience in all my practice I do not remember to have seen but one case that approximates the description Dr. Doane gives of this case of his, in which my patient passed similar products to this sample that he exhibits, and that patient had while under my observation very similar experiences to Dr. Doane's patient. The patient would not be persuaded by anything I could assert but what she had worms. She would declare that she felt the worms, or, as she sometimes called them, *tubes*, as they would rise up into her stomach and want something to eat, or want something to drink—she was so far insane on that point; and she would insist that I should give her a vermifuge occasionally. She told me that the only remedy she had ever had that did her any good before she came to me was from a man that was a worm doctor, that he gave her the best medicine she ever had; that whenever she took his medicine the worms came. That patient's mind became more and

more disturbed, became so much so that she determined to go to San Francisco. Finally she disappeared and went to San Francisco, and I think went the same way that Dr. Doane's patient did when she came to Portland.

DR. BELKNAP: I did not mean to talk at random about these cases. In some of them I have had under investigation I found the true casts many times in these conditions, and I think I understood Dr. Doane to say it was a non-inflammatory condition, although the title of the paper would be to the contrary; but however that be, I have seen both varieties, and I have seen both cases. Sometimes you would get the mucus, as he stated in the paper, and at other times the clear cast.

DR. WALLS: With all due respect to Dr. Mackenzie's statement, I have the casts in a bottle in my office and if he will come around to-morrow morning I will be glad to show them. I think that they are more common than Dr. Mackenzie thinks.

DR. MACKENZIE: I have had a hospital practice of fifteen years, and I have seen medical cases of all kinds in that time, and I would be glad to see the case.

DR. HOLMES: I want to bring forward that one interesting fact in regard to the relations existing between the alimentary canal and the genital organs. It is quite commonly known and accounted for on a definite anatomical and physiological basis, that diseases of the genital organs produce irritation of the intestinal mucus membranes. I suggest this case in point here too as bearing on the possibility of diseases of the alimentary canal setting up a remarkable irritation or excitability of the genital organs. I think that the case is particularly interesting on that point to Dr. Coe, he being a neurologist.

THE CHAIR: If there are no further remarks, we call on Dr. Doane to conclude the discussion.

DR. DOANE: It has not been my fortune to meet with a case of this kind, and while I may not have been so well informed on this subject, or perhaps have been lacking in the experience, yet when the case presented itself to me I confess that I was almost caught napping, but I desired to give it a thorough investigation, I examined all the literature that I had access to, and I am convinced after listening to the remarks of these gentlemen here, that a certain number of them at least have entirely missed the character of the case I attempted to detail. Now, I am well aware of the cases of fecal accumulation, in which the matter forms a tube, as it were, inside of the intestinal wall, in other words, forms a lining to the intestinal wall, with an aperture through the center through which liquid matter may still pass, may exist and go on for months, and even years, and that these may break down and at times pass away, and that from local irritation may be engendered an inflammation, a case of true enteritis, in which there may possibly be passed something which would at times approach to a resemblance of membranes. I am aware also that there are cases of membranous dysentery, as has been incidentally mentioned by Dr. Mackenzie, and I am aware too that there are cases of catarrhal inflammation of the intestines; and all these cases are perhaps sufficiently common. But this case was a little peculiar, in that it came on suddenly. The patient was in absolutely good health, so far as any investigation that I could make would disclose, up to the very day that she was first taken, and the symptoms in the first attack to which I was called were very severe; the course of the case was rapid; within five months she had met her death. Now, I searched the books thoroughly, and while I found very little in some books and in some nothing at all, and in some quite a long treatise, yet I found this point was concurred in by all, that it is a comparatively rare disease. Furthermore, that the books have recognized, as far as I have been able to find, that it is a nervous disease, at least, that seems to be the generally expressed opinion, and while the

pathology, as I have stated, is by no means settled, yet the general concensus of opinion seems to be that it is not inflammatory, but that there is some affection of the mucus-producing glands of the colon which under some abnormal stimulus, call it irritation or what you please, but they are so stimulated that they pour out an abnormal secretion; it is a mucus secretion and not a fibrous secretion. That point it seems has been thoroughly investigated and if you will investigate the contents of that bottle under the microscope I think you will become satisfied that it is not by any means fibrous but it is purely a mucus cast, and in this case the form varied. I think they ran through all of the forms of which I can find any account. The worm form seems to be rather rarer than all of the other. I only found two works in which that was mentioned at all. That happened to be the first form of this material which was called to my attention. The statement is also universally made that while not confined to any sex or age, yet the victims are mostly women of young or middle age and almost invariably that have presented some of the symptoms of nervous diseases before, and, as has been remarked by Dr. Holmes, it very frequently seems to sustain some relation to pelvic disorder or disease of the genital organs.

Now, there are perhaps some symptoms that I did not mention. There was a great deal of gurgling of the bowels; a great deal of distention of the bowels. I found from consultation of the books that these cases nearly always present diarrhoea, but in this case the converse was the case. I am satisfied from the investigation I have made that this was not a case of fecal accumulation, although at the time that this patient passed out of my hands I am satisfied there was then an accumulation, but I mean to say that it did not start or have its origin in fecal accumulation. When these tumors would form they could be felt through the abdominal wall, and it appeared to me that from the shape and form of the tumor, and the natural contour and feel of it, just as though there was a band of muscular tissue gripping the bowels, and

the sensation of the patient, the cramp-like pain, led to that belief; and at times it would be in the ascending colon, and perhaps in an hour that would be relieved, and it would be on the other side. This tumor, which I conceive to be caused by a gripping or spasmodic action of these muscles, was migratory in character, which again convinced me of its nervous origin. So far as remedial measures are concerned, I will say that nearly all the remedies, belladonna, nux vomica, pepsin, ox gall—all of those things that have been mentioned, with the exception of the electrical treatment, were used, and faithfully, as were also all the various forms of enemas that could be suggested. So far as diet is concerned, milk diet has been referred to. This patient unfortunately either would not or could not take milk in any form, and it is my experience in this case that the solid food agreed better than liquid; in fact liquids were almost always rejected; but I find on reference to some of the text books that they make some statement that liquids as a rule are not well borne.

DR. MACKENZIE: I want to ask you if you used any local treatment on the bowels at all?

DR. DOANE: There were various local applications, hot and cold unctions, massage.

DR. MACKENZIE: I mean injections or anything of that kind.

DR. DOANE: Injections were used almost from the beginning. There was some difficulty in using them at first, but they were used. One of the means of counteracting that tendency to immediate rejection of fluid was to put the woman in position. That was the only way in which they could be tolerated on the start. However, that difficulty was overcome. I have been informed to-day, I do not know whether it be true or not, that after the arrival of this woman in Portland an attempt was made, as the doctors express it, to flush

out the colon, and that soon after the patient died. Now, this was not detailed to me from any medical authority and I don't know whether it is at all true, but the thought suggested itself to my mind if any such attempt was made, any violent attempt, I can well conceive how the result would be bad.

NEURO-PATHOLOGY.

BY J. T. WALLS, M. D., PORTLAND, OR.

By a touch of the invisible hand, the dead inorganic atoms of carbon, hydrogen, oxygen and nitrogen, were fashioned in the Divine image and this tenement of clay "passed from death unto life" and man became a living soul with aptitude commensurate with his environments. Pathology is coeval with Adam's fall, and when he was given the grand bounce and unceremoniously chucked out of the Garden of Eden, amid the brambles and briars, not only to earn his bread in the sweat of his face, "but also to suffer the slings and arrows of outrageous fortune," at the hand of the deadly microbe, whose minions of destruction as "the shadow of death" have, through microscopic revelation materialized in modern pathology, and only through the instrumentality of bacteria can this mortal body return to the earth, as it was and the disembodied spirit of the God who gave it. Can such things be and exist for our special wonderment and overcome us like a summer's cloud? Death is the antithesis of life, disease that of health, hence a knowledge of pathology must be deducted from a comprehension of life, as elucidated by the teachings of physiology, that we may be enabled to intelligently classify and group together intangible propositions the multifarious phase of pathological phenomena.

In the "issues of life and death" we are confronted with many problems of pathology, none of which is of more engrossing interest to the progressive physician than the subdivision of Neuro-Pathology. When we consider the nervous

mechanism as an integral part of the organism, the anatomical distribution of its nervous matter, how it lies embedded among the different tissues, that its protoplasm is subject to the same laws common to all nutritional processes, yet subordinating and binding together in bonds of responsiveness all the diverse and complex functions of the organism into one harmonious whole, then may we properly comprehend the great relative importance of the nervous system, as indicated by the safe-guards with which it is hedged about and exemplified, as in starvation. In this classical process of denutrition, there results loss of adipose tissue by consumption of 97 per cent, which "folds its tent and silently steals away."

Next in extent to suffer are the skeletal muscles and glandular organs sustaining a loss of about 60 per cent. then the blood and unstriated muscles lose about 17 per cent, while the brain and spinal cord lose none. It is worthy of note that in this process of starvation or death by inanition that the textures of inorganic life next to nerve tissue suffer least. The deduction that may fairly be drawn from these data indicate unmistakably that the nervous tissue is the last to succumb to the physiological famine, then it "treads alone some banquet hall deserted." This leads us to the consideration of some morbid conditions of the blood involving the nutritional integrity of the nervous mechanism, for it is an axiomatic fact, that oxidation and its concomitant phenomena are under the control of the nerve centers, and when these fail for want of proper pabulum oxidation runs riot, as in hyper-pyrexia, or becomes subnormal for want of proper nervous innervation. The blood is always distinctly alkaline, having a wide range of limit compatible with health between its minimal and its maximal alkalinity, and as oxidation occurs most readily in alkaline media, its maintenance at the normal standard is essential to the preservation of that equilibrium between nutrition denutrition, upon which life depends.

The physiological fact, that during fasting blood attains

its minimum alkalinity and the renal secretion its maximum of acidity, and vice versa, as during digestion, affords a rational explanation of that singular phenomenon.

The physiological exemption of the digestive mechanism from self digestion. Thus, upon physiological grounds, the prolonged exhibition of alkalies is contra indicated, especially to the extent of inducing in the blood the pathological condition of superalkalinity which may not only deplete the brain till idiocy ensues, but beggar the organism to a condition of profound anemia by a destructive process of pathological oxidation—a resorption of fixed or native tissue, and entailing remotely cystic irritation, by a precipitation of the earthy phosphates in the urine.

A sustained superalkalinity of the blood, whether brought about by autogenesis or the ill directed resources of therapeutic art, engenders destructive oxidation and hastens destruction beyond the compensating powers of integration. The pathological oxidation resulting from superalkalinity of the blood, in all essential features, is identical with and equally as destructive as that of pyrexia, yet with this essential difference, that in the former there is no concomitant elevation of temperature, while in the latter the supervention of a lethal temperature is due to paralytic suspension of the thermolytic function. Thus it is not amiss to occasionally take our bearings anew from a physiological standpoint, in order to have accurate conceptions of the limits within which the pendulum of life may normally oscillate; to have a biological summation of vital aptitudes and activities before whose touch-stone of scientific exactitude all dogmatic theories dissolve into airy nothingness.

For the better comprehension of some phases of neuropathology it is important to keep in mind the office of the sympathetic organic nervous system as the bond of union, the connecting link, between animal and organic life, and as in the solar system, the "greater light rules the day" and the "lesser light rules the night," so when the nerve centers of

animal life functionally engaged, the sensorium dominates all the potential resources of the organism in fulfilling its behests, as manifested when "thoughts that breathe and words that burn" sway and move the soul in thrills of nervous vibration.

And only upon cessation of psychical activity does the lesser light of the organic system reign supreme within its sphere. Hence undue sensorial activity by exhaustion of organic energy may eventuate in psychical disturbance. The morbid phases of insomina, coma, vigil, etc., may be better understood by a consideration of the phenomena of sleep.

Physiological sleep is metaphysical or psychical death, the repose of animal life in contra distinction to that of organic life.

Trance, hibernation and allied conditions typically illustrate the organic life minus sensorial denomination.

Rest, mere bodily repose, does not meet all the demands of nature. It is psychical oblivion that gives the respite needful to exhausted energy and for the renewal of vital activity there must be a periodical suspension of psychical activity, a periodical rest for the nerve centers of animal life as it were a cerebral diastole as during sleep the vital expenditures are reduced to a minimum as exemplified in hibernation, reducing rapidity of the circulation the frequency of respiration, lessening the bodily temperature.

Normal sleep treads fast upon brain fag or sensorial exhaustion and "steals upon us unawares" enfolding our faculties in metaphysical oblivion, and in sleep alone are the organic nerve centers emancipated from the thralldom and domination of those of animal life from sensorial control. And as the organic processes are the substrata upon which functional activity rest, it is obvious that they should have periods of immunity, exempt from all associated labor, that the organic processes unembarrassed by sensorial mandates have to do alone with the organic processes of life, the physiological

necessity of which is exemplified in the unwonted cell nutrition of the glandular organs when functionally at rest.

Notwithstanding the intricacy and complexity of the morbid phenomena of the nervous system neuro-pathology has been so fruitfully explored in certain lines as to evoke from the profession more than ordinary interest.

From the realms of conjecture and mystery it has emerged into the domains of reality as a living entity.

Anatomical lesions have been traced with such precision that the resulting pathological phenomena stands forth with a distinctiveness of individuality and tangibility of form as to enable the physician by looking on this picture, then on that, to incite in one patient a hope that springs eternal in the human breast, and while in another he may not hold out the delusive phantom of hope as an antidote to all his ills, yet a consummation so devoutly to be wished, may be compassed by the obliteration of environments, and drawing round about him the mantle of obliviousness so that "coming events may not cast their shadows before."

To the alienist all phases of abnormal deviations have their corresponding anatomical lesions definitely ascertained and determined to him, as highly differentiated member of the medical body should be relegated as his special pabulum, all that class of patients whose organic derangements portend a culmination in psychical deviation.

DISCUSSION.

By request of Dr. Walls the discussion was opened by

DR. COE: This very scholarly paper, of course, does not pretend to cover the whole field of neurapathology. As stated by the writer it is one of the largest fields a physician can explore, and a paper of this kind can only touch the borderland of study in such a department. I was sorry the doctor did not go more into the pathology of nervous diseases; that is, that he did not arrange his paper to cover a certain

classification of discussions and go more into definite pathology of definite diseases; but of course his paper is in another direction. We cannot well understand the pathology of nervous diseases unless we understand the histology of the nerve structure itself. We must have a general knowledge of the intimate anatomy, a histological relation of the nerve cells themselves. I have seen a recent paper by Dr. Starr of New York, on the causation of nervous diseases, in which certain pathological points are touched upon. Dr. Starr well likens the nervous system to the financial condition of the individual. He says that the man who uses up such funds as he takes in in his business or profession as he goes along, is on the verge of financial bankruptcy, and that we should have a certain nervous reserve on hand at all times so that no matter if we do use a little more than the ordinary amount of nervous energy, we shall be like the merchant who has more money than he needs for a particular occasion, and, therefore, the one does not stand in dread of nervous bankruptcy as the other does not fear financial bankruptcy. I can only compliment the writer of the paper on the manner in which he has handled the subject.

DR. THOMAS PARKER: I do not know if I rightly understood the author of the paper, and I would like to have this point made clear, whether he intended to say that pathology began with the expulsion of Adam out of the Garden of Eden, or if that is the foundation of the whole thing. If those are his premises his reasons were very logical and his conclusions well derived.

DR. FULTON: Quite an important question, too.

DR. CAUTHORN: Speaking of this matter the other day with Dr. Coe, the thought came to me with reference to the relation between financial hard times and the nervous condition of individuals. I think there is a very direct and intimate relationship along this line as Dr. Coe has suggested. It occurred to me, that the average weight of this community

and perhaps every other community, is three pounds per man less than in good times. I think this society would weigh less per man by three pounds than it did three years ago. So that there is not only a comparison but a direct relationship between stringent times and the condition of man's health.

DR. BELKNAP: It is a fact that mental worry kills more people than mental work. When we consider so broad a subject as the one under discussion, it leaves one hardly in a position to know where he began or where he left off. The causes, of course, are many, and in considering this subject while our minds run back over the list of nervous diseases as put down by some authorities (the best authorities put down as many as one hundred and seventy-six different varieties), it is not expected that we are to carry all of the details and understand all of the different stages and conditions. However, there are about sixty-five different varieties that the specialist is supposed to understand pretty thoroughly. It is a broad subject, and in taking up any particular line we cannot go into details. The nervous system is susceptible, of course, to a vast number of influences, and we feel it in various ways. Anything that acts upon the mind, anything that depresses, has an influence on the nervous system; and the nervous system governing every other organ in the body, we many times get what we call reflex disturbances; we get their effects produced by these depressions, and vice versa. If the nervous system be the primary cause, then we will get affections in other ways. What I mean by vice versa is, we may get the nervous system secondarily affected by other affections all the way through. So, as I said, it is a question in which we have not even time to go into detail and take up any special line of study.

THE CHAIR: If there is no further discussion we will call on Dr. Walls to close it.

DR. WALLS: I believe I have but little to say except in regard to Dr. Parker's suggestion. I think it is possibly an

established fact that pathology must have had its origin when Adam left the Garden of Eden. I suppose it made an impression on one man that has been handed down through generations and it has lost none during its time. This, however, is not a paper that is calculated to bring out very active discussion. I am very well pleased with the way the society has received it.

IMMEDIATE REPAIR OF LACERATION OF THE CERVIX.

BY F. M. ROBINSON, M. D., BEAVERTON, ORE.

Mr. President and Members of the Association:

My reason for introducing a paper on this subject, is the bitter opposition usually manifested both within this society and outside of it. When this subject has been incidentally mentioned, many positive assertions have been made, such as "it cannot be done," "it is bad surgery," "improper, unphysiological and dangerous."

Another specious argument that has nearly always been advanced, is that the necessary contractions of the uterus after parturition would frequently so distort and twist the cervix that a good result could not be attained. The assertion that it cannot be done has been proven to be untenable many times by various obstetricians of prominence in the profession and their success cannot be gainsaid.

The second proposition is as groundless and would as little bear proof as the preceding one. The body of the uterus down to its junction with the cervix, will contract thoroughly, becoming hard and firm under proper conditions, and the cervix proper will be soft and patulous, remaining so from three to five days, and is not twisted or distorted by the contractions of the body. Therefore we must look to some other cause for our failure to get good results from an immediate operation upon a torn cervix. I can give the cause of all my failures in three short words, viz., lack of skill. Without going further at present into the reasons advanced against the immediate operation, I will mention some rea-

sons for it which I think are sufficient for its maintenance on good surgical grounds.

My first proposition is that it is good surgery to repair an injury to any part of the human body just as soon as possible after it is received, and I can see no good reason why a laceration of the cervix uteri should be an exception to the general rule. I will now point out some of the dangers to which the woman is subjected by the negligence of the obstetrician, which may be divided into immediate and remote. I shall be as brief as possible in my remarks on this, to me, most vital point of all; first in the line of immediate dangers is hemorrhage, with its constant companion shock, if the lacerations involve the circular artery of the cervix on either side, which is usually the case, owing to the increased size of all the blood vessels of the pelvic organs during gestation. Any one who has made a careful study of the physiological enlargement of the pelvic vessels and plexuses during the period of pregnancy or has been called upon to do an immediate Cæsarean section to deliver a child at full term, or to remove some abdominal growth during gestation will not need to be reminded that hemorrhage is severe and dangerous from vessels at other times quite harmless, and will exercise due caution in all parturient cases where there is much bleeding. The amount of shock is not always indicative of the extent of the laceration or amount of hemorrhage; nor the dangers attendant thereon, as quite an extensive laceration may show little bleeding, as the uterus may contract vigorously driving out blood and clots from the cavity to form in the cervix and vagina, only to decompose and send its poison into the system through the ever-active lymphatics.

The accoucheur is called to his patient about the 4th or 5th day after delivery to find that she has had a chill, or several chills with a temperature of 104 degrees or more, abdomen tender and tympanitic, lochia foul smelling, scanty and putrid, and in cleansing out the vagina and uterus by douche and curette he finds that the cervix has been severely lacer-

ated, but it is now too late to repair it and his patient must run the gauntlet of that dreadful disease, puerperal fever.

It is useless for me to speak further of this disease, (as it does not come within the scope of this paper) only to refer you to Dr. Thomas' paper on its prevention, read before the Academy of Medicine in 1883, in which he said to reduce the area of infection to the least possible compass by the immediate repair of all lacerations of perineum, vagina or external genitals, but not one word did he say about the immediate repair of the cervix, although his writings are replete with references to the remote pathological changes dependent upon a lacerated cervix for their origin. All our leading gynecologists unite in telling us that subinvolution and all its sequellæ, hyperplasia of muscular structure in body of uterus, degeneration of arteries and veins, enlarged and distended plexuses, nerve filaments caught in scar tissue of cervix and pressed and twisted by the adhesions and dragging down of the pelvic organs, relaxed uterine supports which were never allowed to regain their former vigor and elasticity because the proper involution of the uterus was interfered with on account of the lacerations. Many of these living monuments to our disgrace are operated upon each year by the gynecologist and restored to health and home, after having as Emmet says, for years suffered a living death. But not one of them save Pallen, of Missouri, has raised his voice or pen to say that all this sad picture could be avoided by a few sutures taken in the cervix at the time the injury occurred. One of the reasons most responsible for this delay is that advanced by most textbooks and teachers, that you don't know how much nature will do to repair the damage unless you wait and see. I fully agree that I do not know what nature will do to repair the damage, and this is one of my strongest points for immediate action. Judging from my limited experience I can safely say I know what nature will not do, because I see living evidences of it every week of my life,

and it is because I know nature is not to be trusted, that I began to do these repairs myself.

It has been my misfortune to be located in a part of the country that had been remote from a physician and when one was called for in an obstetrical case, it was a bad one indeed, therefore such a thing as stitching a perineum or cervix was an unknown quantity.

When I located where I now practice I found that all such cases had been left to nature, and while I do not pretend to know what nature will do in repairing injuries to the cervix, I know altogether too much about what she will not do to trust anything to her care that I feel confident I can do better than she will do it for me.

Other potent reasons why immediate repair of the cervix has been so long neglected by the profession are,

- 1st. Lack of skill by the general practitioner to diagnose and perform such work.
- 2nd. The prevalent idea of trusting to nature rather than to surgery and going to the lying-in chamber totally unprepared for such emergencies.
- 3rd. The well grounded fallacy that septicemia seldom occurs in the lying-in woman unless introduced from without.
- 4th. Fear of shock and post-partum hemorrhage from the use of an anæsthetic.

To my mind there is not a well founded excuse among them. There is not a man who is capable of obtaining a diploma from any of our leading medical colleges, but can, by exercising his knowledge of aseptic surgery, prevent the introduction of septic matter into the vagina or uterus. I am satisfied that all the graduates from good schools, if they had only a few lectures on this subject and felt that the moral support of the profession was behind them, would soon successfully operate on all laceration of the cervix at once.

I shall not attempt to give a history of cases as it would

be too long and tiresome, but will give a short description of the operation as I have done it for the past five years.

After removing the placenta, I wash and thoroughly cleanse my hands, then insert two or three fingers of my right hand into the vagina and with my left one make a firm pressure over the fundus. I begin at the left side of the patulous cervix and thoroughly palpate each sulcus from the body to the extremity of the lip and if there is a laceration I find it, and by palpating the rim of the cervix I can even detect a rent too small to be worth stitching, and if I find a separation of tissue not including the vaginal portion of the cervical membrane and reaching well up to the internal os, I suture it from above downward well to edge of lips, as one of these blind rents are as liable to cause trouble as a complete one, and are far more liable to escape detection; for this kind of tear I use a small curved needle and continuous suture, taking great care to approximate the two edges of the mucous membrane inside of the cervical canal.

On finding a bilateral laceration reaching up to the fornix vaginæ on one or both sides, I chloroform the patient, draw her to edge of bed, place her in Sim's position, I douche and mop out the vagina and cervix with a 1 to 4,000 or 5,000 bichloride solution, wash this off with hot, sterilized water, a Martin's or Sim's perineal retractor, catch the anterior and posterior lips with bullet forceps, have an assistant make firm pressure over the fundus and another to draw down on the forceps bringing the cervix to the vulva, cut off all shreds making a clean edge. I use a short, flat needle with a round edge, one on each end of suture. I take a deep stitch taking in a considerable distance from edge of rent, usually tying one set of sutures so the knots are inside of the cervical canal and the others in the vagina. Two things I wish to emphasize particularly; one is to take in a sufficient amount of tissue on each side of tear and the other is to draw the stitches firmly enough that the retracting of the cervix will not loosen them too soon, thereby perhaps losing some of your work, and

don't be too sparing with your suture. Douche the vagina and cervix, then thoroughly cleanse the external genitals, apply a pad of aseptic absorbent cotton and have the woman placed in bed and rest assured that you will get union by first intention and that the cervix will be as smooth and round as it was before the laceration occurred, notwithstanding the old notions that the uterus will twist the cervix out of shape.

SUMMARY.

- 1st. The primary operation lessens the chances of septicemia.
- 2nd. That the procedure is as justifiable as the immediate repair of the perineum and is as far reaching for good to the patient.
- 3rd. That it hastens involution, prevents subinvolution and displacements, allows the uterine supports to gain tone sufficient to perform their functions which they cannot do when handicapped by a subinvolted and overweighted organ.

DISCUSSION.

By request of Dr. Robinson, the discussion was opened by

DR. MOORE: I have listened with a great deal of pleasure to the reading of this paper. It is a subject which I am especially interested in and one that is receiving considerable attention from the medical profession throughout this country. Every one of us have seen the results of the laceration of the cervix. I admire the courage of the doctor in taking the course he has in this work, and the advancement he is making and the impression that is being created upon the minds of the people of this state in this continual fight upon this subject. I have quite an obstetrical practice, but I do not repair the lacerated cervix at the time. I do it afterwards. Some of the objections to the immediate repairing of the cervix are these: if we have a lacerated cervix, we aim to draw

our stitches tightly and keep the cervix as small as we can. By that means we lessen the size of the cervix and the canal, and we have very poor drainage for the wound. It is almost impossible at confinement to see that everything is entirely removed from the womb. If anything is left there, and we have drawn the cervix so closely as is advised in this operation, it seems to me that however thoroughly we may have cleansed the vagina, however thoroughly we may have done our work in the laceration, there is as much liability to septicæmia following the closing up of the cervix as there would be if the wound were left as large as it was without being repaired. I am frequently called upon to wash out the womb after I have thought everything had been removed at the time of parturition, and find pieces that are the nidus for bacteria to set up fever. I believe that an external tear should always be repaired. I want to see better results from the operations of others before I attempt it, because it is not pleasant work to do. I dislike very much to go all over the ground of giving chloroform which always requires an assistant if it is done thoroughly, because it is quite a task to repair a laceration and particularly after parturition.

DR. THOMAS PARKER: I think the Doctor's paper is an excellent one and I presume there would be very few who would differ from him in regard to immediate diagnosis and immediate repair of the laceration. I would like to ask the Doctor, however, why he favors the Sim's speculum and the Sim's position. It seems to me that in that particular case he necessarily must have two assistants. In the dorsal position I think it is not necessary. It is not always that the physician can have two assistants and the fewer assistants the better, I think. In the dorsal position the uterus naturally gravitates and it is not necessary for one assistant to stand with the bullet-forceps holding the uterus in position.

DR. TUCKER: I have been very much interested in Dr. Robinson's paper and had no idea that anybody had for five

years been doing this work in Oregon. I concur very fully with almost everything Dr. Robinson has said. I have often thought of this procedure, but did not know that it had been done at all until quite recently when I saw an article in the American Journal of Obstetrics by Dr. A. Palmer Dudley of New York, who reports a number of cases before the New York Obstetrical Society in which he had done this operation for the last two years. I thought he was the pioneer in this work. I am glad to find, however, that it started in Oregon. I think there is every reason in the world why the lacerated cervix should be repaired at the time of its rupture just as well as any other wound. The reason for leaving it alone to see what nature will do could be applied to any other wound. When a man gets a lacerated wound on the body he is brought to the hospital and a physician; he is not deprived of treatment in order to see what nature will do toward a recovery. The wound is immediately attended to. I think a large per cent of cases of septicæmia, I might almost say all of them, are caused by absorption of septic matter through an open laceration. I think many cases that do not result in puerperal fever result in a low grade of sepsis that leave marks there for years to come. All these might be entirely avoided if the laceration was repaired at the time under perfectly aseptic conditions. In the great majority of cases the inside of the uterus even after labor is perfectly aseptic; there is no cause for it to be otherwise unless through meddlesome interference on the part of the obstetrician. If no instrument or soiled fingers have been introduced into the uterus why isn't it just as aseptic as it was before, so that there would be no chance of septic absorption into the womb through the laceration itself except it should come through the vagina. If the wound is closed at the time and properly closed it would do away with this opportunity for absorption. I think the great objection to the operation is right here: after we have attended an obstetrical case for several hours, the child is born, the placenta comes away, the patient perhaps has been under

the influence of chloroform and is now feeling easy, has that sense of ease and comfort that comes to every woman after having given birth to a child, and all the pain and agony are over. Then the physician hesitates and does not like to tell the patient that she has to take chloroform again and have another operation. The physician hesitates to do that, the patient objects, and I think that is one of the reasons why we do not do it—one of the reasons that has stood for a long time in the way of doing perinæum operations. I think many are slurred over and allowed to go for the same reason, the physician wants to get through with his case and the patient wants to be let alone. So far as the position of the patient is concerned I also sympathize with Dr. Robinson in a liking for Sims' position. It may be a mere matter of taste or fancy, but we all like to work in the positions we are accustomed to. I think in Sims' position there is less stretching and displacement of the vagina than where the perinæum is drawn down, and for my part it is easier to get at. I should like to ask Dr. Robinson, since he has made a study of this matter and examined these cases, if he often finds a tear from the cervix extending into the vagina. I find often in examining lacerations afterwards, a scar as if the vagina had been torn at the same time the cervix was. I never examined a fresh case and I do not know whether that is so or not.

DR. WALLS: I would like to say just a word in regard to the instrument used. The Doctor spoke about using Sims' speculum. I wish to ask him if he ever used Martin's, and if so what he thinks of it; I mean Martin's as modified by Dr. Rockey. I merely call attention to it in operations where one is alone or scant of proper help. I have found it a most excellent instrument; I have only used it in curetting; I think I can make repairs of that kind with that instrument with less assistance than any instrument I know of.

DR. STRONG: I was not fortunate enough to hear the paper, so am not in a position to talk on this subject directly,

perhaps, but I wish to give my experience as to the condition of the cervix after labor and its subsequent condition. I have had opportunity to examine, perhaps as many women within 24 hours after labor as most any one; and in my hospital work in guarding against puerperal septicæmia, etc., I find very frequently that the cervix appears to be very extensively lacerated, so much so that at first I expected a subsequent operation to be necessary. My observation has extended over a number of years, and I find now that in a very large per cent of cases the cervix after involution has taken place is in as good condition as it can be after confinement. I have never yet felt that it would be necessary to operate upon these cases immediately. I suppose some lacerations might be of such an extent that it would be necessary, but if we put in stitches in every case that appeared immediately after labor as though requiring them, and then assume that the result was the necessary consequence of our operation, I think we would very likely be deceived, because a very large per cent of these cases recover with a perfect condition of the cervix.

DR. MACKENZIE: I want to argue against the position Dr. Robinson has taken in regard to this question of primary operation in lacerations of the cervix. It occurs to me there must be good ground why there should be such practical universality of opinion against it, and the opinion that continues even in the light of the modifications of bacteriological research. There must be some good reason for it, and looking for reasons it occurs to me we can find without going very far, ample reasons for the objections to the primary operation. In the first place the character of a wound of this kind is such that frequently it is unfavorable for primary union. A laceration of the cervix is usually a lacerated wound; not only is it a lacerated wound but very frequently the tissues of the cervix are bruised in such a way that the possibility of obtaining primary union is out of the question. There is another reason, going a little further, which I think is a sub-

stantial reason too, as against the primary operation, and that is the continued hemorrhage that follows parturition and the continued oozing that takes place from the lacerated cervix. The hemorrhage I refer to now would be an obstacle, and a greater obstacle still would be the altered secretions that would naturally follow on parturition. They are not always septic secretion, but secretions that we constantly find undergoing putrefactive changes. On the other hand looking to the immediate effects of the operation, I don't see what could be gained by doing the primary operation over the advantages that present themselves in doing the operation say three or four months later. The chances of infection are infinitely multiplied when the primary operation is done. We have a large exposed surface of the uterine sinuses, the placental sinuses; we have a large exposed surface at the seat of the operation, and the risks of infection are, it seems to me, infinitely greater when the operation is done under these conditions than when, after involution has taken place, the operator proceeds to repair the damaged parts. If Dr. Robinson will show me how he can overcome such objections he will go far towards convincing me of the utility of the operation. I remember in the discussion that took place last year on this paper many of the arguments that were raised, and I can recollect that I related my own experience when I was in Vienna as a student. I remember Dr. Carl Braun advocated the operation then, some fourteen or fifteen years ago, and I remember that the advocacy of Carl Braun was in itself sufficient to give a great deal of prestige to the operation at the time. I can say with certainty too, that the advocacy he gave it at that time has not increased in influence with the lapse of time, but rather that it has decreased.

DR. WILLIAM JONES: I have no immediate experience in this operation but I have been very much interested in Dr. Robinson's paper and it appears to me a very sensible thing to do to repair a laceration of the cervix immediately; that is, if it is a considerable laceration, one that you feel reason-

bly certain will require an operation afterwards. I do not see any objection to doing the primary operation that does not apply to doing the primary operation on a lacerated perineum, except the difficulties of doing it which Dr. Tucker has mentioned. The advantage of doing it at the time is very great. In the first place it saves the patient a large bill of expense. An after operation on the cervix involves a considerable fee and confinement in bed for a couple of weeks with the expense of a nurse. Now, if this laceration can be repaired at the time the same as a laceration of the perineum is repaired, it saves all expense, except perhaps, a small extra fee and the subsequent confinement in bed, which it seems to me is a great deal. I do not believe it is necessary to repair all lacerations of the cervix. I have been very much surprised often in the amount of repair and perfect repair that takes place in uterine lacerations. I have in a few cases examined after delivery of the child and found what seemed to me extensive laceration and I expected it would be a case for operation afterwards; but on examining the case three or four weeks later, when involution has taken place, I have been surprised at the perfect repair and found that it was not necessary to do an operation. As far as the condition of the tissues are concerned it seems to me they are not different from the condition of the tissues in operations of the perineum and they are bathed with the same discharges as the lacerated perineum is bathed in afterwards; yet the lacerations of the perineum heal kindly and almost certainly after a primary operation. So I would be disposed to do this operation, I think, if I had the courage after a night's work to operate in an obscure light with the fatigue one feels and in the face of the objections of the patient and the patient's friends, and I think I may screw up my courage to that point in a favorable case.

DR. H. W. CARDWELL: I have had a little experience of the immediate repair of the cervix and I have thought a good deal on that subject, and there are a few cases where I would

repair the cervix immediately. I believe there are very few cases, however, because the objections that have been offered are all true. There is also another objection which seems to me to be a very good one; and that is that the uterus is contracting, the muscular fibres are drawing in every direction as involution goes on. This is another objection why I would not repair immediately. The stitches would tear out, I think. Another thing, unless you use silver sutures the sutures would be carriers of infection. But in a case where the patient was already under chloroform, where there had been instrumental delivery, perhaps, and particularly if there was very much continuing hemorrhage, I believe it would be proper to repair the cervix at the time. The patient is already under chloroform, and it does not take so very long to do the operation, it not being a very hard one to do. To do it after the patient has recovered from the chloroform and is very tired and exhausted and when you have a little bit of fear that you will have secondary hemorrhage following the administration of chloroform, one does not very well like to do it, because the friends usually object. There are, however, the advantages of avoiding the additional doctor's bill and the lying in bed, which women usually object to. It is only in those cases where the patient is already under chloroform that I would do the repairing immediately.

DR. FULTON: I would like to ask one question. I suppose the large majority of cases of confinement are followed by more or less laceration. Now, inasmuch as it is impossible for us to tell whether or not that laceration is going to cause after trouble, it would become necessary according to this paper to repair every laceration in order to be on the safe side. The extent of the laceration, unless of course it is extreme; probably has no particular effect on the final result; a small laceration, I presume, can cause sub-involution or serious trouble as well as an extensive one. So that in a repair, it seems to me, the physician would have to repair every

one or only repair those so very extensive that any one could see the necessity of it.

DR. STRONG: I would like to put myself on record with reference to my opinion as to whether repairs are necessary or not. I wish distinctly to be understood as opposed to the question of repair of the cervix immediately after labor, for the reason that a very large per cent of them do not require repair, will get well perfectly afterwards. The stitches you put in at the time in a large laceration of the cervix will be of no account in twenty-four hours on account of the contraction of the cervix, and other conditions that have been ably stated on the floor, and therefore be of no use. I am distinctly opposed to immediate operations for laceration of the cervix.

DR. GIESY: I have delivered a good many patients since I have been in practice and undoubtedly have caused, or at least seen, a good many cases of lacerated cervix. There is only one point I wish to speak of on this question of septicæmia. One of the reasons given why this primary operation should be performed was to prevent septicæmia or puerperal fever. It seems to me that puerperal fever is so rare comparatively speaking, that without some other cause appearing aside from a lacerated cervix or even a lacerated perinæum, the operation ought not to be performed. I do not believe that a lacerated cervix or a lacerated perinæum is necessarily a cause of puerperal septicæmia, unless so far as it may be considered one of the factors when once the septic condition is in operation. When you come to consider that all of us that have seen labors and who have stood by the bedside of a patient for ten or twelve hours and seen how tired the patient is, as Dr. Tucker very aptly suggests, it takes a whole lot of courage to subject the patient to a second anæsthesia or to hold her under the anæsthetic until you do this operation when you do not know but what the repair will be as perfect without the stitches as with them. I believe the

majority of cases of lacerated cervix will heal primarily without any stitching.

I do not believe that it has anything to do with the primary cause of septicæmia, while it may be a factor and assist in absorption if sepsis is once set up. But I believe that the obstetrician's fingers or his instrument are much more responsible for septicæmia than the laceration so far as my experience goes. I have never had a case of septicæmia since I am practising obstetrics that I can recall, and I am quite sure that I have had a great many cases of laceration. It seems to me that where one man has had as many cases as I have, one would have occasionally a case of septicæmia following labor if septicæmia was due to laceration. The objections of inconvenience both to the physician and the patient are factors against the immediate operation of a lacerated cervix. These conditions do not exist in lacerated perinæums. The physician can do up a lacerated perinæum very quickly and without much disturbance and in the majority of cases without an anæsthetic. I never like to give an anæsthetic after the patient has once recovered from it, for two reasons. In the first place it is very dangerous to give it so far as the anæsthetic itself is concerned, because you have not the reacting influences of a contracting uterus, and in that way forcing the blood back to the brain and acting upon the spine and heart. Consequently you are more liable to have secondary hemorrhage. I have seen two cases of hemorrhage following the administration of an anæsthetic an hour or two after labor; and I believe both were due to giving the anæsthetic. I am always a little leery in giving an anæsthetic after the placenta has expelled and the patient has got through with the labor, and in the great majority of cases of lacerated perinæum you can very easily without any difficulty pass in one or two stitches and repair the perinæum perfectly, which you cannot do in the case of immediate repair of lacerated cervix.

DR. STRONG: I would like to ask Dr. Giesy a question

or two. In what proportion of cases in private practice do you use the speculum for examination of the cervix?

DR. GIESY: I never use it.

DR. STRONG: What would be your judgment as to the danger of exposing the whole vaginal canal and stretching it by the use of the speculum?

DR. GIESY: I think it would be infinitely more dangerous than to allow nature to repair the laceration.

DR. BINSWANGER: I wish to put myself on record as being decidedly opposed to the immediate repair of the cervix. There have been some very good reasons advanced why this should not be done, and seemingly good reasons why it should be done. The reasons which Dr. Tucker advanced, that the physician is generally too tired and that the patient is too weak, and that the physician does not want to cause any unnecessary trouble, are seemingly good reasons, but not in reality good. If we think it is best for the future welfare of the patient we should insist upon having the operation done immediately. The fact that the physician is tired should not play any part in his decision. We are very often tired and yet have to do work. Whether the patient objects or not should make very little difference, because I do not think she would enjoy the operation afterwards any better. The argument of Dr. Giesy I can substantiate in every particular. I think that the dangers from sepsis are not any larger with the operation or without the operation. Puerperal fever is a very rare disease and when it does occur we usually do not know why. I had lately a case of undoubted septicæmia; Dr. Mackenzie who was called in consultation is familiar with it. The confinement was perfectly normal and as easy as any I have ever attended; the woman was not in pain more than half an hour. When I was called I had hardly time to clean my hands; and still three or four days afterward the patient shows decided symptoms of septicæmia. Whether stitches or an operation

will prevent septicæmia or not is an open question in my mind. I think the operation, if it is to be done, can be done with much better success and much safer after a couple of months than immediately, for the reasons which have been advanced, and I wish to put myself decidedly on record as being opposed to an immediate operation.

DR. TUCKER: I would like to correct an impression I have given Dr. Giesy and Dr. Binswanger. I did not mean to say that a lacerated cervix ever causes septic or puerperal fever, but that it might be a factor in permitting septic absorption. There is a condition of septic metritis which we find afterwards, which gives rise to little induration in the broad ligaments on each side of the uterus. A simple metritis which causes subinvolution without giving rise to any general symptomatic conditions and which is a very local affair, which I believe is only present when a laceration has occurred. I would like to say in regard to Dr. Binswanger's criticism of my objections, I think they are right, the objections ought not to be valid, but they are just the same.

DR. DOANE: I can not say that I have had any experience whatever in the immediate repair of lacerations of the cervix; and certainly the objections and arguments for and against the operation have been pretty thoroughly covered. There have been suggested one or two points that I want to add to a little. Dr. Giesy has objected to the operation where it involved a secondary administration of anæsthetic, because of liability to a secondary hemorrhage, or rather postpartum hemorrhage; I want to accent that and to add, that in the only cases of postpartum hemorrhage I ever saw, (except one case of placenta prævia, which of course would be out of the category in this instance) were instances where, for some reason or other, an anæsthetic was used after the woman had recovered from the confinement, and the immediate effect of the expulsion of the placenta on the labor was to all intents and purposes terminated. And while neither of the

cases proved fatal, yet they gave me a great deal of anxiety and trouble at the time and I have a very vivid recollection of them, and I do not care to go through the same experience again. So far as this question of immediate operation is concerned I apprehend that, in these, as in most other cases an extreme ground, perhaps, on either side of the question would not convey the certain truth. Truth probably lies in the golden mean. I am one of those who believe that extremes are rarely if ever right. There are certainly some cases in which the laceration gives rise to a great deal of hemorrhage, where the sub-uterine arteries have been involved and torn or where the laceration itself is so very extensive that it would become patent to any one that an operation for repair was necessary to guard against either hemorrhage or some other dangers. In the vast majority of cases these lacerations, severe as they may seem to be at the time will, as has been said by Dr. Strong and others, in the lapse of a few weeks when involution has been completed, be found to have entirely disappeared, or at least to have left so little result that nothing need be apprehended from them. We can see that one of the greatest safeguards against septic complications following labor is in the perfect contraction of the uterus. If the uterus has been thoroughly rid of all its contents; if all secundæ have been passed and no septic germs have been introduced by the hands of the physician or his instruments from without, and perfect contraction of the uterus can be maintained so that the mouths of the blood and absorbent vessels are all closed, the chances of infection are very much lessened, and in fact it is hardly possible or probable that infection under these conditions can take place. It seems to me one of the strongest arguments for the immediate repair of the cervix in these extreme cases lies in this fact, that what nature does through the contraction of the muscular tissues of the uterus for the body of the uterus this operation would do for the lacerated cervix; and where a laceration is so extensive that it must, without this operation, remain spread apart and exposed to

the constant bathing in the fluids that are found in the vagina, that under those circumstances there is something to be gained in warding off sepsis by an immediate repair. However, I think in the majority of cases an immediate repair is not called for.

THE CHAIR: If there are no further remarks we will call on Dr. Robinson to close.

DR. ROBINSON: I will endeavor to answer a few points in detail and others generally. As to the point raised by Dr. Moore in regard to the narrowing of the cervical canal, I would answer that there is no narrowing at all. You simply take a few deep stitches in the cervix where there is plenty of room. You can pass your finger into it two days after you have taken the stitches, and there would be no trouble in that respect. Of course if the condition exists of something being retained in the interior of the uterus there will be trouble anyway whether the laceration is repaired or not, and in my opinion you will have more trouble and more danger to the patient if it is not repaired than you will if it is. It is not intended to narrow the cervix nor to interfere with the drainage from the interior of the uterus at all.

As to Dr. Parker's objection to the Sims' position, that may be a matter of fancy. I may not be any nearer right when I use Sims' position, than if I used the dorsal position. Both Dr. Strong and Dr. Giesy raise the objection as to the distention of the vagina and exposure of the torn cervix to the air. I contend we can do the operation in a few minutes if the physician carries with him the instruments necessary and is always prepared for it. Never go to an obstetrical case without being prepared for it. A physician might as well go out without his pocket-case or any of his emergency instruments as to go to an obstetrical case without instruments of that kind. I was taught when I attended medical lectures never to go to an obstetrical case without some instruments with which to repair a lacerated perineum. What

would we think of a physician now who would go out, under the teachings at the present time, and carry nothing with him to repair a lacerated perineum. We would think he was not fit to practice obstetrics and we would think just right, because he would not be. The very same arguments that have been brought forward in opposition to the immediate repair of the cervix have been urged against immediate repair of the perineum. You can find it in the literature of the past, I have read it and know it exists; the same arguments and specious reasons have been presented. I am one of those who never gets tired. I remember a remark which very aptly illustrates my position because it was said in the way of a joke by the professor in the medical college. One of the students said something, and he was asked what his name was. He answered in such a way that the professor understood him to say his name was Rest, and he thereupon told him he should *never rest*. That is the way I look at this thing, we should never rest so long as there is anything to do for the betterment of our patients. Now, I use the Sims' position but I never said I used the Sims' speculum. Sims has a perinael retractor and when necessary I use it or have it used by an assistant. It can be used with or without an assistant. A Martin's retractor will do just as well. They are not a speculum; they are not over an inch and a half long, and are merely used to retract the perineum. Now, you bring down the cervix and stitch it which is only a short piece of work. Of course you have your needles ready and put them in hot water, scald them thoroughly and have them in an aseptic condition. If your instrument is thoroughly clean you are not going to carry any septic matter into the cervix or vagina. In regard to lacerations, as Dr. Tucker mentioned, I have found the vault of the vagina invariably torn; never found it otherwise than torn when there is a laceration that reaches through the internal os of the uterus to the body. When I say to the body I do not mean it reaches into the internal organs but I mean that it has sundered it com-

pletely, torn the internal os completely through the fibers as it approaches the body of the uterus. I do not know whether Carl Braun made mention of it or not in any of his writings, but it has been mentioned by Prof. Lusk within the year, that in those cases where there was a laceration through the interior os and up to the body of the uterus, tearing some of the vault of the vagina, that you would never get contraction of the lower segment of the uterus without stitching it. Dr. Lusk says he has done the operation for that very purpose, and that it promotes contraction of the uterus. However, I have had no experience in that respect. As to stitching every little laceration, I do not do that, nor can I see how one can be mistaken in a laceration after he once learns how they are ascertained. When a physician tells me he has not had lacerations of the cervix in his practice and that he has examined the uterus, I always wish that I had had his practice. I do not say he is mistaken, but it has been my misfortune to fall into many of these cases. While I am not in for innovations, I do think it is right to do the best I can for my patient. Like Dr. Palmer Dudley, I do not see why a woman should go dragging about for months with a subinvolted uterus from laceration simply because the obstetrician would not stitch it. If he can prevent that it is his duty to do so. I think it is our duty to observe all these things we have been taught by the profession. As to septicæmia, there is one rule: Keep everything clean. When we have done that, we have done well enough. As Dr. Binswanger says, septicæmia is something that we do not very well understand, it is almost an unknown quantity; it slips in when we neither understand it nor want it.

I had not many months ago a case of puerperal hemorrhage. It was a pretty bad one, too. I telephoned Dr. Linklater who was not at home and I telephoned for Dr. Bailey and any other physician that could be obtained the quickest. Dr. Bailey could not be found, but a physician came, a very bright young man too, and a willing worker.

By the time he arrived I had exhausted the grip of both hands, and he cleaned himself up and went to work manfully, and we finally delivered the child. Then I said to him, "Doctor, have we a lacerated cervix?" We had to dilate with our hand. He examined thoroughly. I said, "Be sure, now; have we a lacerated cervix?" He said, "No, there is no laceration." To examine the case myself was out of the question, because I had no use of my fingers. I let it go; I had his word for it, and supposed he would know. When I came to douche out the uterus the next day what did I find but a laceration of both sides, a bilateral laceration, clean through and through. Well, neither the case nor I was in a condition to repair it, so I let it go trusting it to nature as so many talk about, but nature did not do anything for it until it was repaired. I sent her to a gynæcologist and had a secondary operation performed. I have had a great many such cases, but I have my first case yet to see where nature did much repairing where the laceration ran above up through the interior os.

PRESIDENT'S ADDRESS.

Courage Sans Peur.

J. A. FULTON, M. D., ASTORIA, ORE.

Since the birth of man, since the days of chivalry, the one thing above all others which has been most admired in man by all mankind, is that quality of mind which gives to its possessor all the attributes, characteristics and virtues that go to make up that which we are pleased to call a courageous man. The poet has said, and truthfully as I believe, that "All the world loves the lover." He might well have added that all the world admires, loves and respects the man of courage.

In speaking of courage I do not mean reckless bravery alone, or what is sometimes called valor, which is often the offspring of fanaticism, and is seen in the barbarian and the warrior; although bravery and valor are admirable always and at all times. The courage I would speak of to-day is the natural result derived from a mind, the quality of which gives a man the determination, the courage to do deliberately that which he knows to be his duty, no matter if by so doing he risk the loss of friends, home, country, or even life. To a man with such a mind, nothing is higher than truth itself, nothing more noble than to do right. It is to men endowed with such a quality of mind, men who dared to advocate and fight for truth in opposition to and against all the powers of wealth and precedent, that the world and mankind to-day owes many of its most useful and valuable possessions.

Without that quality of mind which gives man the determination and will, to act in accordance with the dictates

of his conscience, how many of the brilliant discoveries in science would have remained hidden still from mankind and the world to-day. Without this splendid character which belongs to and goes along with true manhood, how little there would be of note and interest in the history of medicine. Without it who would have dared to desert the old way, go contrary to the teachings of so-called authorities, and advocate and practice that which was new to all other men, and which had no history, no endorsement? I have no patience with a physician who is always talking of authorities. To my mind, in the practice of medicine and surgery, there is no higher authority than the physician in attendance upon a patient, whose disease he understands, the pathology and etiology of which he is informed, and who has a knowledge of the effect produced by the remedies he proposes to use. It is true there are those who may teach what they deem to be true to-day, but who among us will have the temerity to say that such teaching will always be right. It has been these so-called authorities, the veneration of tradition, and the superstition of the ignorant which have ever been the millstone around the neck of the true teacher, the true writer, the discoverer of the new.

That knowledge gives courage, I believe all will readily agree, and that it is to the most highly educated and most thoroughly informed that we usually look to for the signal to advance. The fact that the science of medicine and surgery have been advancing more rapidly during the last twenty years than at any previous time in their history, is evidence that knowledge gives the courage to act. For it will be conceded by all, that the physicians of to-day are more thoroughly educated than ever before and that tradition, superstition and precedent have less to do with the practice of the profession than at any time since the art and science of medicine and surgery have been studied and practiced by man.

Nevertheless how many of us to-day still adhere to some old routine of practice in which we have no particular confi-

dence, and yet fear to change for the reason that our patients expect this particular treatment and would be very liable to criticise us severely for changing, if they failed to receive benefit from the change. Therefore it requires courage, even to-day for a physician to adopt some new or original method of treating disease, some method not recommended or laid down by so-called authorities, for should a consultation be required and a less enterprising, less competent man be called in and he should pronounce the treatment an experiment and one which was unheard of and not endorsed by the authorities, how long would it take for the affair to be published among those upon whom we depend for a living? How well I remember the words of a surgeon who has a national reputation to-day, when after having twice failed in obtaining the result sought for from a certain operation, as he stood before a large body of students and medical men and said honestly, "Gentlemen, I have failed twice. It is not the fault of the operation; the theory and principle of which I know is right, and I must succeed the next time." He did succeed and the operation new then, is recognized as one of the best known to-day. This was true courage. He knew his reputation was at stake, but he knew also that the welfare of the future sufferer was at stake as well, and he risked one for the other. He saved the one and won the other.

Without courage on the part of some men would anæsthesia be what it is to-day? Do we often think what a terrible risk both the discoverer and his first patient took, when they tried the effect of the new agent? If there had not been a surgeon with the courage, if there had not been a patient with the courage to try it in that old hospital in Massachusetts where anæsthesia was first publicly demonstrated, how much pain and suffering might the sick and afflicted have had still to bear, and what an obstacle to the progress of surgery would there have still been in the way?

Did it not require courage of the noblest and grandest type to lead McDowell in that little Kentucky town nearly

ninety years ago to prepare deliberately for and then do successfully the first real ovariectomy? Would a weak man have been the first to do such an operation? It is easy to follow, but requires courage to lead. Without a single authority to guide him, without a precedent and with the traditions of the past and the teachings of the present against him, he, an obscure surgeon, but one with a heart big enough to throb for all suffering mankind, risked everything because he knew he was right; knew he must act, and act at once in order to give his patient the only chance left for her life. He acted without fear and with courage and his name is revered by all who are arrayed against disease and human suffering. Without a McDowell having lived how far advanced would abdominal surgery be to-day? Who can tell?

We may not all have our names enrolled upon the book of fame, yet we all know of instances in the lives of some of our co-workers, which if known to all would win for them the applause of their fellowmen. The country doctor who travels through all kinds of weather, over all kinds of roads, and at all times of the year, day or night is entitled to the homage of his fellow men. Oftentimes, with no friendly brother to aid him, without the necessary instruments, without precedent or previous experience to guide him, and without the time or opportunity to consult even the small library he owns at home, he is compelled to act alone, promptly and courageously, if he saves the life fast passing away before him. To the credit of the country doctor be it said, he rarely fails to be equal to the occasion, and many of the important advances made in our profession we owe to country doctors.

The life of the earnest and true physician everywhere is one of sacrifice, of labor, of worry and of courageous action; and while every year of work we do lessens perhaps the worry, gives us more confidence in, and a better satisfaction with our work. Nevertheless the oldest worker in our midst will tell you of his last year's work, and will likely tell you of some one or more cases that required more than experience would

furnish him, more than books could have given him, and which required the skill of a man true to his profession, true to his patient, and with the courage to act.

How often are we requested to shield some one from disgrace, discomfort, or even the law? And while it is the duty of the physician to protect the fair name and honor of every patient under his care—and I am proud to know the medical profession knows how to be honorable under all circumstances—nevertheless there are times when the physician's sense of duty and his feelings of sympathy may be at variance. We have no right, however, to try to protect one individual from inconvenience and discomfort at the risk of the health of the community. How often are we requested not to report a case of contagious disease, and what a terrible risk he runs who complies with such request. He risks not only his own reputation, but what is far more serious and awful, he risks the lives of every one in the community. Nevertheless until one has had to turn away from the pleadings of a mother or other members of a family and firmly insist on doing that which is best for all, no one can realize that it oftentimes takes courage to do that which is so plainly the right thing.

It is the duty of physicians to try and prevent disease wherever possible. Does it not require courage sometimes to do even this? If we know a certain dairy is liable to spread disease; that a certain groceryman is selling decayed fruit liable to cause disease, or any of the number of things the physician is liable to learn of, does it not sometimes require courage to do that which is plainly our duty? Are we always ready to complain to the authorities, and go if necessary into the court and withstand the badgering questions of the lawyers and stand the abuse of the dairyman and the groceryman, or whoever it may be that is responsible for the nuisance reported? To those who have not tried it and desire experience in this matter, I would say, let them try it and they will find that it may at times require courage of the highest order to do what is so plainly the duty of every good

citizen. To the lover of history of medicine, of men or of nations, away back in the past along the highway which shows the march of progress, may be seen standing like sentinels the men whose names are blazoned upon every epoch of advance. They mark the course all mankind has had to travel onward and upward out of the chaos of the past, the superstition and ignorance of times gone by into the brightly illuminated day of modern progress and civilization. In the far distant future by him who loves to read the history of men, what names known among us to-day will then be remembered and cherished?

Let us hope that out of the list of bright, energetic and intelligent Medical men in the proud State of Oregon there may be some whose names will not be forgotten, but that he who reads the History of Medicine and of Men, will be pleased to know that Oregon did her part toward alleviating human pain and suffering.

CAN FEMALE DISEASES BE CURED?

BY CURTIS C. STRONG, M. D., PORTLAND, ORE.
Professor of Gynecology and Clinical Professor of Obstetrics, Medical Department University of Oregon, Surgeon Good Samaritan Hospital.

Mr. President and Members:

A careful review of the field of gynecological practice, seems to make the question "Can Female Diseases be Cured?" a very practical one. To those who may be suffering from such diseases and to mothers having the care of girls, it is a question of the deepest interest. Important as it is to them, the physician has a greater interest in the matter. For if the question cannot be answered in the affirmative, it militates us to the power of the physician to cure disease and lowers his standing in the community.

Of the patients that come to us we find that a very large majority of them complain that they are suffering from displacements, whites, and that list of complaints which they assume come from some form of female disease. Let us consider these complaints seriatim.

First, displacements of the womb—it is not our purpose to enter into any elaborate dissertation on the subject of uterine displacements: We find them here, what has the profession done for their relief? The works of Hippocrates, Moschion, Ætius and others go to show that this is not a new discovery, but that the ancients were familiar with displacements. The etiology and pathology of the disease was not as well understood then as now, yet there is much reason to believe that our knowledge upon these points is not so thorough

and extensive as the demands of humanity require. Therefore, it cannot be considered impertinent to inquire of the profession what it has done for the cure of this condition.

Three methods may be considered—medical, mechanical and surgical. The first of these we may dismiss briefly, for taken alone it cannot be considered as curative, in well marked and well established cases.

The second or mechanical, will have to be considered a little more elaborately. There are those who believe that pessaries will cure displacements and there are others who condemn pessaries as absolutely useless if not positively harmful.

The middle ground is nearer the truth, because it does not seem possible to believe but what pessaries when properly used in conjunction with other means will do good. They are to be used as a temporary method of retaining the uterus, somewhere near its normal position, till the pathological conditions can be overcome and removed and the parts assume a healthy state, so that the uterus may remain in its normal position.

The surgical treatment for displacements is now receiving considerable attention. A careful review of medical history will satisfy anyone, that there always has been a tendency to run to extremes. A theory, a drug or a surgical procedure when given to the profession is soon picked up and vaunted as a cure-all and applied to all conditions. Then there follows a period when it is dropped as useless; after that it takes its proper place as a remedial agent. You have only to consider the history of scrofula, malaria, tuberculosis, carbolic acid, the germ theory, germicides, clitoridectomy oophorectomies and many others, for these are only a few of the facts the profession have followed during my recollection, to appreciate the truth of the above. This subject is mentioned here, not as condemning any operation but to show the trend of the medical mind to run to extremes.

Let us consider ventro-fixation, hysterorrhaphy for retro-

displacements. This is one of the comparatively new things introduced to the profession by Olshausen, Sanger, Kelly and others. The operation was intended for those cases where the uterus is fixed by adhesions, the adhesions were to be dissected off through the abdominal opening, the uterus brought forward and fixed. It is to be feared that the operation has far oftener been done in those cases where the uterus can be replaced by manipulation, and that the operation is not usually attempted where there are firm adhesions.

Before advising the operation we should carefully consider whether the present suffering and inconvenience of the woman is of such moment as to justify the substitution of one pathological condition for another and whether it justifies the risk of peritonitis, septicæmia, intestinal obstruction, remote as they are; and the evidence upon these points should decide for or against an operation. Not that we are desirous of operating and the patient is willing to be operated on.

The operation is not to be condemned altogether, nor is it the object of this paper to express a disapproval, but to call the attention of the profession to the necessity of great care and judgment in selecting cases. We are not to allow our ambition for surgical fame to over-ride our judgment.

That which is for the best interest of the patient should govern. We will derive more satisfaction and do more for our professional credit, if with judgment we select and operate and thereby get good results in a few cases, than if we operated upon a score or more, leaving behind us a few deaths, several failures, some not benefited, as the result of our desire to be known as an operator.

Leucorrhœa is a general term and covers a broad field. For convenience, it is here considered a condition not a symptom. It is present in about every case that comes to us, and you all know or may know the history of these cases. She has come to you from another physician, and after a trial of your skill will drift on to the next. So the chapter runs till the money gives out, perseverance fails, or both; or

the woman dies of old age. How rare it is that we hear of a cure. The term cure is here used to signify, not the mere stoppage of the flow, but such a restoration of the parts that there will be no discharge and that the disease will not recur without a cause equal to that which produced it.

Can this disease be cured? There can be no doubt how this question will have to be answered, if we bear in mind the causes at work and the difficulty of removing adverse conditions. We must remember that diseases are treated and results obtained on conditions as they actually exist, that while it is often possible to improve the conditions, yet ideal conditions cannot be obtained or maintained. Under proper conditions and with proper treatment the disease can be cured, but as a general rule the proper conditions cannot be obtained. They are not obtainable.

Success depends upon a proper appreciation of all those conditions that influence health—dress, food, hygiene, exercise, non-abuse, etc. If you expect a cure simply because you curette, use electricity, douches, local applications, tampons and the like, you will be disappointed. However, if you have the knowledge and make proper use of the right selection and do not neglect the weightier matters of the law, the results will be more to your liking.

Then we have that large class of cases where the woman does not present any one well marked condition but simply is not well or strong. They have that extensive and varied list of complaints that you are all familiar with. You find some whites, the os of varying conditions, a relaxed uterus that may not be in the best position, a relaxed vagina, a perieum atrophic and relaxed, the result of a former tear, but none of these things very well marked. Within the recollection of the writer the following methods have by strong authority been introduced to, and adopted by, the profession:

A doctor of note in his day, claimed to have discovered a cure for all these conditions and brought an array of cases

to prove that he had, by cutting out the clitoris. This he kept up till he had removed a bucketful or more and only stopped when the husbands arose in just indignation.

Then the profession who had blindly followed his lead, now held up their hands in horror and said "we knew this could not cure these cases."

Then another doctor arose and claimed that the ovaries were the seat of all these troubles and their removal would result in a perfect cure of everything from insanity to laziness. He also proved his position by cases. Then the profession rushed to remove ovaries till women were afraid to complain of a toe ache. To such an extent did this idea run riot with the profession, that one physician in a little village of five thousand, published the fact that he had removed the ovaries of one hundred and fifty women.

Then the relaxed or torn perineum was unfortunate enough to catch the eye of some doctor, and his list of cases proved that its restoration was all that was necessary. So the mucus membrane and skin was sliced off and the parts brought together often without much regard to the muscles or the proper understanding of the perineal structure. Then we went a little higher and followed the lead of one whose list of cases proved conclusively, that if every dent or fissure of the cervix was properly operated upon, all would be well. When we got that high we concluded that the cases of another leader were convincing and that we might with advantage go a little higher up, and curette.

So all sorts of instruments were inserted into the uterine cavity and too often wobbled around without in many cases much conception of what should be done, while the profession sat holding in the other hand a medical journal, eager to adopt the next recommendation with its list of cases. Because these methods and cases have been mentioned in this way do not understand me as expressing any disrespect for the originators or condemning the operation. Far from it, for all of them, except the first, are recognized methods and have stood

the test of the conservative brains of the profession. Their improper use by the profession is to be condemned. They are spoken of in this way for the purpose:

First, there seems to be a necessity of more conservatism in certain quarters. When a theory is advanced it should be received with caution, studied carefully, if deemed useful, we should be sure we understand to what cases it is to be applied and just how to use it.

Second: As all surgical procedures involve a degree of risk, first as to life—although that risk has been reduced to a minimum—next whether we leave our patient as well as she was before the operation, we must carefully consider whether or not the thing complained of is severe enough to justify the risks.

Third: It is not a sufficient justification because the patient or her friends give their consent.

Fourth: We must not allow ourselves to operate simply because the patient wants an operation, and if we do not some one else will. The temptation to operate for renown is very great. If manhood and honor are the peculiar prerogatives of any one profession or set of men it belongs to the profession of medicine and to the medical man.

Fifth: There is no doubt but that the art of diagnosis should be carefully studied, and no one will deny that there is an opportunity for improvement. When the abdomen is opened to prove that there is nothing there, or that the condition present is incurable, when both of these conditions were capable of proof without going into the abdomen, we are prostituting the calling of medicine.

Sixth: We will then make a proper selection of cases and get some good results from our efforts to relieve humanity. It has not been the scope of this paper to throw new light upon any of the subjects mentioned, rather to suggest the possibility of our running to extremes in our methods and of doing more harm than good.

DISCUSSION.

By request of Dr. Strong the discussion was opened by

DR. WM. JONES: I will only address myself to that part which relates to fads in medicine and surgery. I would estimate that when any valuable procedure is brought forth for a certain length of time it does more harm than good, no matter how valuable it may prove to be in the end. Take, as an illustration, urethrotomy, brought forward by Otis. It is a valuable procedure, undoubtedly now that it has found its proper position, but for a time there was too much of it done and strictures that did not need cutting were cut and strictures that did not exist were cut; and violence was done to the male urethra throughout the length and breadth of the land. The strictures that were cut and benefitted did not balance, I think, those that were cut which did not exist. I think we ought, when any new procedure is brought forth, to regard it with a certain amount of reserve and not adopt it with too much enthusiasm or haste, but go cautiously through the experimental stage.

DR. A. C. SMITH: I have listened to a number of criticisms made in somewhat the same strain exhibited in this paper. While I do not consider that the cap fits me, I do, nevertheless, feel that it is my privilege as well as that of any one else who operates, to reply to these criticisms. I have attended the operations of nearly all of the operators in this city at hospitals at various times and with hardly a single exception I have always been benefitted by the experience I have obtained in that manner; and I can not, with possibly a single exception, recollect an instance where an operator was guilty of attacking any part that should not be attacked surgically.

DR. LINKLATER: Speaking as a physician from the country, on reading this title, "Can female diseases be cured," I feel welling up in my bosom, so to speak, a feeling of thank-

fulness that in many cases they can be cured, a feeling of thankfulness that we have in Portland a faculty that we can look up to, so to speak, to help us out in bad cases. Many cases are benefitted by rest. It is a great factor of course in easy cases; but given a case of a large ovarian cyst, how much good will rest do? It would keep increasing. I am thinking at present of several cases of female disease I have cured. Can female diseases be cured? Cases have been cured by men I am now looking at, and the gratitude of patients and physician attest the fact. I was much impressed, Mr. President, with the subject of your paper on courage, and the necessity for courage and there is much necessity for the maintenance of that quality in the medical men of the present day and of the future. Gynæcologists have got to exercise more courage than almost any branch of the profession; look at Simpson, assailed by the church, and look at Marion Sims and all those great men who had to exercise courage to a remarkable degree. We are thankful to see that in the state of Oregon we have men of courage who can take diseases and cure them, and I know cases in my own practice that are now living and well. One lady who went to Salem on an excursion last Sunday whose life was an absolute misery and despair, sent word to me Monday when I scolded about her taking the risk of going on that excursion, that she would be happy to do my washing for three months. Gentlemen, I am happy to say that I believe, notwithstanding all the discouragements of gynæcology, in many cases female diseases can be cured. (Applause).

DR. ROBINSON: In regard to the conversation spoken of by Dr. Strong, I think I pretty fully concur with Dr. Strong's paper. He made mention of the fact that it was necessary in order to cure some difficult cases, to have certain conditions and surroundings. I think that is correct. While I might think differently in some respects, still I have never found in my experience very much of this operating, cutting and slashing as it is called, it has not been my misfortune to

fall in with that kind of surgery; but I have always found that those who were very conservative wanted to make a good diagnosis before they took up the knife. Still it has been said that we cannot all keep the proper equilibrium or balance, we need a good many conservatives to hold some of us back. I think in the main the idea of the author of the paper is that we should better our facilities for diagnosis, should better our facilities in every respect concerning our patients and the conditions necessary to complete a cure.

DR. HOLMES: I think it may be claimed that just as satisfactory results are had from the treatment of gynecological cases as those belonging to any other branch of the profession. I can suggest only one reason why those who have practiced gynecology will criticise the work of others with which they are not very familiar, and that reason is that they have not had very much to do in that line. The man who objects to operate according to any rational principle of surgery, is sure to be the man who has not tried the operation. The man who objects to ovariectomy for cystic degeneration of the ovary, must be one who has very little knowledge of the operation and its beneficial results; yet you are doubtless aware there are those in the profession to-day who object to that operation.

So I repeat, sir, that those who object to operations for appendicitis, operations for the radical cure of hernia, for pyosalpinx, and many others I might mention, are surely those who have not tried the operation and they are really in the profession of critics rather than in the profession of medicine. The man who does try the operation and has the courage of his honest convictions, must see that the operations are appropriate.

DR. FULTON: I had it in my mind that if there was one thing in the profession that we had a right to be proud of, it was the results gained in gynecology. There is one question that has not been brought up in regard to the title that I

want to mention. I refer to the words, "Female Diseases." I know what this refers to here, but the term is not a good one in my judgment. I think Howard Kelly was the man who first drew my attention to the fact in some of his writings that "Female Diseases" might mean diseases of any female. I think perhaps we could use a better term than that, relating to women and her diseases.

If there are no further remarks we will ask Dr. Strong to close the discussion.

DR. STRONG: Mr. President, and fellow members, I was very much surprised in this discussion at the perfect and absolute misapprehension of the gist of this paper. I prepared it with a good deal of care and thought, but the title I will admit, could be improved. It was hastily and in an off-hand way, given to the secretary one day on the street, when he asked me for the title of my paper. Still, I do not understand the construction of the English language, if it can be inferred that when one asks a question it is supposed to be answered in the negative by the one who propounds it; therefore I deny absolutely that there is any imputation conveyed in my title to the effect that female diseases cannot be cured. I recognize the fact that in perhaps no department of medicine or surgery has greater advancement, or more satisfactory advancement been made than in the subject of gynecology. Again, I said nothing against these operations; on the contrary, I said that these operations (with the exception of the first one which I suppose will be admitted was stamped out by the profession) were many times successful. I passed no criticism on any one. I simply say that in the treatment of these diseases very great care ought to be exercised. I did not say, nor imply that the profession was not careful. There is in the profession, possibly always has been, the danger of running to extremes.

DANGERS OF INTRA-UTERINE INSTRUMENTATION.

WILLIAM JONES, M. D., PORTLAND, ORE.

Until the advent of the present gynecological era, the cavity of the uterus was considered as a space not to be entered by the instruments of the physician or surgeon, except under the most urgent necessity, such as the removal of a retained placenta or of an intra-uterine tumor. With the advent of modern gynecology, however, all of this was changed, and physicians rushed to the other extreme. Instruments by the dozen were invented for intra-uterine treatment. Intra-uterine applicators, uterine syringes, port-caustiques, etc., were constantly in the hands of those who treated the diseases of women. This period of intra-uterine applications is rapidly passing away as their uselessness is being recognized. But another procedure has come to take their place; a procedure of the greatest value in the treatment of diseases of the uterine cavity; but a procedure that has its limitations of usefulness and its dangers. I refer to uterine curettage. This operation is at present a fashionable one, and its field of usefulness is very much exaggerated and its dangers very much belittled by the profession at large.

It is not of the uses of these intra-uterine procedures that I intend to speak, but of their dangers.

Whenever the physician introduces an instrument within the womb, he exposes the patient to dangers on account of it. It is certain that the simple introduction of a sound within the womb may set up a violent inflammation of the womb, tubes, ovaries and pelvic tissues, with all their attendant

dangers and evils. The danger of exciting pelvic inflammation is greatly increased where intra-uterine applications are made. These applications being usually strong, add their caustic effects to the injury made by the instrument by which they are applied, and so increase the attending risk. As curetting is a more potent procedure for good than applications to the endometrium, so are its attendant dangers greater. It creates extensive raw surfaces within the womb and correspondingly increases opportunities for absorption of septic material, if there be any present. The danger from intra-uterine instrumentation is much greater if the patient has suffered from one or more attacks of pelvic inflammation. Particularly is this the case if this inflammation has left behind adhesions, indurations of the pelvic tissues, or chronic disease of the endometrium or tubes. To illustrate the risks involved in topical treatment of the endometrium, I will cite some cases I have recently met with in practice.

CASE 1. Was called to see this woman at her home and obtained the following history: Age 30; married at 21; had never been pregnant. Health had been rather delicate for last six or seven years, though she had been able to do her own work. Suffered much while menstruating. Lately had been receiving office treatment at the hands of a physician. Treatment consisted principally of dilation of cervical canal by means of sounds. Six weeks previously, she had received a treatment which had been followed in a few hours by a severe chill, high fever and severe pelvic pain. She had been confined to her bed ever since, and the pain and fever had continued, the temperature ranging from 100 to 103. On examination found uterus fixed by hardening of pelvic tissues; much emaciation and pulse rapid and weak. Advised her to go to the hospital, which she did a week later, fever and pain continuing unabated. At this time could feel a mass to right of uterus, which arose above the pelvic brim. This mass increased in size rapidly, and at the end of another week arose half way to the umbilicus. At this time I did a laparotomy on

her. Made a median incision, and found the mass to consist of the Fallopian tube immensely distended with pus. As it was universally adherent, could not remove it; so incised it, stitched it into abdominal incision, and drained. She made a good recovery, though there is still a small sinus leading into the pelvis.

CASE 2. Entered hospital and gave the following history: Age 31. Had never been pregnant. Health had been good with the exception of an attack of pelvic inflammation several years before, from which she had apparently recovered. She being anxious to have a child, had had the cervical canal dilated forcibly four weeks before. This operation was followed in a few hours by a severe chill, high fever and pelvic pain. She was confined to her bed and symptoms continued. I found her weak and emaciated, temperature elevated and pulse 120 and weak. On examination found a mass on each side of the uterus which seemed to consist of the enlarged and inflamed ovaries and tubes. Did laparotomy. Removed double tubo-ovarian abscess. On one side the adhesions were friable and recent; on the other there were some dense and old, the remains of a former attack of pelvic inflammation. She made a good and rapid recovery.

CASE 3. Entered hospital with following history: Age about 28. Several miscarriages. Two months before, womb had been curetted. Operation was followed by pelvic inflammation. She was extremely weak, had continuous fever, and pulse was 120. Behind and to right of uterus there was a mass which appeared to be the inflamed ovary and tube. This mass gave indistinct fluctuation through vagina. Did laparotomy and removed this mass, which consisted of right tube and ovary converted into an abscess. She made a prompt recovery.

CASE 4. Entered hospital with following history: Age 27. Two children aged 6 and 3. Health good previous to birth of elder. Since that time not good, though she has

been able to do her work. With first child suffered a deep laceration of uterine neck, for which she has received treatment off and on ever since. Lately had been receiving frequent local treatments. These treatments had been confined to the uterine neck. Three weeks before entering the hospital she had received two treatments to the uterine cavity. The second of these was followed in a few hours by chill, fever and local pain, the usual symptoms of pelvic inflammation. On examination found temperature 101; double deep laceration of uterine neck, extreme degree of retroversion with fixation in this malposition, and general hardening of pelvic tissues. She is still under observation and has been for about ten days. During this time she has had evening rise of temperature. Gradually, from the general hardness, there has developed a mass to the left of and separate from the uterus, that can be easily made out by bimanual examination. This mass seems to be made up of a distended Fallopian tube and an inflamed ovary, and I believe will have to be removed by a laparotomy in order to restore the woman to a degree of health.

Besides these extreme cases, I have seen two others where the consequences of intra-uterine treatment were not so disastrous but were extremely serious.

One while undergoing treatment to the endometrium, was attacked by an acute pelvic inflammation, from which she made a symptomatic recovery after several weeks of confinement to bed.

The other had a severe pelvic inflammation following immediately on a curetting of the uterus. She made a symptomatic recovery after several weeks sickness.

From these cases I would draw the following inferences:

1st. That entering the uterine cavity with an instrument is attended by a good deal of danger to the patient, from its liability to excite pelvic inflammation.

2nd. That this danger should not be incurred for triv-

ial reasons, but only when the advantages to be gained are considerable and reasonably certain.

3rd. When it is decided to introduce an instrument into the uterus, it should be done with all the precautions employed in a formal operation, that strict asepsis should be observed, and rest in bed afterwards.

This last would preclude treating these patients as office patients.

DISCUSSION.

By request of Dr. Jones the discussion was opened by,

DR. A. C. SMITH: While I agree with the sentiment generally expressed by Dr. Jones, I do not believe that the introduction of the sound or any other intra-uterine instrumentation would result thus disastrously as he has described, if aseptic precautions were taken. I believe that many general practitioners in making their uterine examinations, especially those who use the sound to determine the position of the uterus, become careless in their aseptics. They often-times introduce the sound after simply wiping it off or dipping it in a little alcohol or something of that sort; whereas they should always precede the introduction with a thorough cleansing of the vagina and as nearly as possible of the cervical canal and an absolute cleansing of the instrument itself. I believe if these precautions are taken where it is necessary (and it is seldom necessary to use the sound) it may be used with safety. I believe that the causes of some of these disturbances, in the cases recited by Dr. Jones, are due to a sort of traumatism in cases previously affected with salpingitis. I believe that if the curette is used cautiously and with only sufficient force to remove the granulation tissue and not with the whole force of the fore arm, thus removing the entire endometrium, but rather if it is introduced gently up to the fundus and withdrawn gently by finger and wrist motion, the endometrial granulation tissue can be gently removed with-

out unduly injuring the membranes. Therefore I believe that Dr. Jones' word of caution in regard to curettage is well-timed and could be applied with benefit by many. I repeat that curettage in nearly all cases where the endometrium is sufficiently coated with granulation tissue, can be resorted to with safety, taking care always to avoid any traction whatever, where there is any tubal trouble or any fixation; whether it be extra-uterine or otherwise, of the uterus.

DR. MOORE: I am very much pleased with the paper presented by Dr. Jones. It has touched upon a point that has been brought out very forcibly to our minds the past year in this city. We see patients in whom pelvic inflammation has been induced and they are to be invalids for the rest of their lives from this cause. It has been my experience in the past few years to see not less than a dozen women who were previously in fairly good health and enjoying life as well as the average of women returning from doctor's offices with the history that they have been there for local treatment for the special purpose of becoming mothers. I have seen this so many times, that I believe that it is dangerous to enter the womb at all except under the most perfect asepsis. I have seen so much injury from it that I think it is a dangerous proceeding for the physician to do.

There being no further remarks the discussion was closed by:

DR. JONES: I do not want to be understood as saying that the womb should never be entered with instruments, but I mean to be understood that it should be done only with proper precautions and not for every little thing; that is, I would not advocate using the sound except very infrequently. I think we can get nearly all information by bimanual examination, that we can get by manual examination and the sound added. I would advocate, too, not making uterine applications of iodine and other intermediate agents. I do not believe they do any good. At any rate, in my hands in the

old days, I did not appear to get any good from them and ceased their use, besides I believe they are dangerous and ought to be abandoned. Dr. Smith believes it is safe to enter the uterus with proper precautions. I believe that also, but every body does not know those precautions. It is safe for a tight-rope walker to walk a tight-rope, but it is not safe for everybody to do it. And so, until a physician has learned the proper way of doing these things, he should avoid doing them at all. I know there is an immense amount of danger done through the country in this way, and I suppose it comes from not using the proper precautions in entering the uterine cavity when it is necessary to do so.

The hour for adjournment having arrived and there being several papers to be read, on motion of Dr. E. N. Wilson the hour of adjournment was postponed until six o'clock, for the further reading of papers. And on motion of Dr. Coe, discussion was postponed until all remaining papers were read.

ANÆSTHESIA.

BY DR. WM. F. AMOS, PORTLAND, ORE.

It was my intention at first, in writing this paper, to make it brief, and consider fairly, carefully four propositions with regard to anæsthesia, but I found that to give proper time to all four propositions would make my paper too long and wearisome. So I will content myself with considering only one at any length, and with barely mentioning the other three. The four propositions are as follows:

1st: That any form of alcohol should never be given as a stimulant in anæsthesia.

2d: That ether when administered and taken properly is not more irritating to the kidneys of a perfectly healthy patient than is chloroform.

3d: That profound anæsthesia in average cases is not only unnecessary but dangerous.

4th: That the routine administration of morphine before anæsthesia is not justifiable.

The fourth proposition, that of giving morphine before anæsthesia, is of special interest to me.

Inasmuch as the First and Second Hyderabad Chloroform Commissions traversed each others conclusions arrived at after a long and what was supposed to be accurate scientific investigation; inasmuch as the Lancet Chloroform Commission, held views at variance with those expressed by the several Hyderabad Chloroform Commissions and Thornton and Hare differed with the other Com-

mission, I believe that I may in all reasonableness make an earnest protest against this morphine habit indulged in by so many physicians, and speak in support of pure anæsthesia.

Thiersch it was, who originated the practice, but Nussbaum first gave the drug for the express purpose of deepening and prolonging chloroform narcosis.

Bernard himself, who investigated the method and placed it on its pseudo-scientific basis, stated that the combined effect of the two agents, morphine and chloroform resulted rather in the addition of the influence of one to the other, than a modification.

McEwen says that the action of morphine becomes intensified in the presence of chloroform.

Kappeler has given his opinion against the administration of narcotics before ether, believing that they increase the danger.

What I wish to do is to explain why I think the above gentlemen are right in their opinion, that morphine increases the danger of anæsthesia.

Upon the respiratory center, the general average effect of both chloroform and ether, as is well known, is that of depression. This same depressing effect upon the respiratory center is eminently characteristic of morphine.

As a result of the combined respiratory depressant action of morphine and either one of the two anæsthetics, the oxygenation of the blood is specially interfered with, somewhat as it would be in mechanical asphyxia. But there is a difference and that difference, so far as results may be concerned, is not one of degree so much as of kind. If you were to put your hands upon a man's throat and throttle him, you would, by preventing the oxygenation of the blood in his lungs, cause to circulate through his medulla and body, an ever increasingly venous blood which would more and more urgently stimulate his respiratory center. If you take your hands off this man's throat soon enough, the exaggerated breathing thus induced would soon restore this blood to its proper de-

gree of aeration, and himself to his original condition of well-being. In pure anæsthesia, metaphorically speaking, you take a vigorous hold of a man's throat, but by increased depression of his respiratory center from a previous injection of morphia, you tighten your grasp, which, so long as the effects of the morphia prevail, you cannot relax, try as you will.

Any interference with the proper aeration of the blood, i. e. asphyxia, has well known effects upon the circulatory system. I do not need to mention that these effects are: (1) Contraction of the arterioles. (2) Slowing of the heart's action from vagus stimulation, and (3) Increase of arterial blood pressure. This increase of blood pressure and the slow beat give rise to distention of the ventricles, which, when a certain point is reached, impedes the working of the heart, and its muscle begins to beat more and more feebly, so that, in the third stage of asphyxia, the pulse can hardly be felt. To briefly make my application, morphine slows the heart by vagus stimulation and increases arterial tension and so favors the foregoing result. The Lancet Chloroform Commission stated that many deaths are due to reflex cardiac inhibition, which usually occurs *early* in anæsthesia. Even if this be true, it can be no argument for the use of morphine. To the contrary, it is an argument against its use, for the very reason that the early vagal stimulation by the morphine, courts reflex disaster.

Morphine, so far as the action on the arteries is concerned, antagonizes chloroform, and is protective, but from its inhibitory influence upon the heart at the very time that the heart muscle and augmentary nerves are depressed by the chloroform, it greatly increases the liability of occurrence of ventricular dilation and sudden reflex cardiac inhibition.

Morphine in the forefront of its action intensifies that of ether on the arterioles but is not able to slow the heart's action because the stimulation of the sympathetic by the ether is stronger than the stimulation of the vagus by the morphine. But morphine in the *latter* part of its action depresses the in-

hibitory nerves of the heart and the heart muscle itself about the time, in ether anæsthesia, that the heart muscle from over action becomes depressed. This will account for the specially fast, weak heart after a prolonged morphine-ether anæsthesia. By way of parenthesis, what are the indications for treatment in this condition? Give digitalis: (1) to stimulate the failing heart muscle, (2) by vagal stimulation, to slow and steady the heart's action, and (3) to contract the arterioles. To accomplish these things the digitalis has first to fight and overcome the depressing effect of morphine upon the heart muscle, the vagi and the arterioles.

It must not be forgotten that, consentaneously with the above effects upon the heart, a sort of asphyxia, accentuated by the morphine is progressing. The Second Hyderabad Chloroform Commission stated that the greatest risk to the heart arises from chloroform, plus asphyxia, and that morphine increases the degree of asphyxia, no one can gainsay. This asphyxia, without warning, may suddenly terminate in complete relaxation of the muscular arterioles and engorgement of both sides of the heart and the great veins. The exhausted heart from want of oxygen suddenly finds that it is unable to empty its cavities, let alone pump the blood out of the veins. The Hyderabad commission found that at this juncture a certain chest manipulation in dogs, was almost invariably successful if commenced within 30 seconds, and almost useless if not begun until after 50 seconds. So in the human subject, moments in such an emergency are precious. The anæsthetic can at once be withdrawn, but the morphine is beyond our immediate control and may be the cause of the otherwise avoidable death of the patient. In this connection, I wish to say that it strikes me that physicians are quite willing to shift responsibility of death in anæsthesia by ascribing it to some divine dispensation of providence, such as "reflex cardiac inhibition." Hare and Thornton in discussing this question make the following pat statement, "We believe that it is improbable that reflex cardiac inhibition can occur in

healthy persons, but we admit its possibility if heart disease or some other condition makes the individual unable to withstand any shock whatever. We believe that true fatty heart, *plus* ventricular engorgment, *plus* possible valvular disease and finally, *plus* extreme vaso-motor relaxation, may result, in previously frightened people, in death.

J. C. Reeve in an admirable article on anæsthetics says, "It is not surprising that death should occur in a state in which all but the most essential vital functions are abolished; and he will be the most cautious in the use of these most powerful agents, who most fully appreciates the physiological fact that a patient in a state of anæsthesia is on the borders of death." The same gentleman, however, deprecates incomplete anæsthesia, in which, he says, high authority and clinical experience both teach that such a reflex influence may proceed from painful impressions made by the surgeon as to extend to the heart and cause sudden cessation of its action. A little further on, this same gentleman whom I love to quote, because he seems to be himself a reflex of all the best authorities, admits that he cannot understand why, in obstetrical practice, in which incomplete chloroform anæsthesia has been induced many hundreds of thousands of times, there has been only one case of death recorded where a competent medical man administered the chloroform. As an explanation of this zero of fatality in this class of cases he advances the theory of pain as being protective. That is, in one breath, he states that pain in partial anæsthesia is very dangerous and by reflex cardiac inhibition has produced many deaths; in the next breath he states that pain in partial anæsthesia is protective. This is a discrepancy of reasoning which may be satisfactorily explained away for us in the dim, scientific future when we shall discover a tuberculin which will prevent tuberculosis and an antitoxine which will be of curative value in the treatment of diphtheria.

Schwechten unqualifiedly condemns the use of morphine anæsthesia for the following simple reason: that it de-

presses the sensory nerves of the skin and so interferes with the reflex stimulation of the vital forces when restorative measures become necessary.

Nothnagel says: "Not only does morphine depress the activity of the respiratory center directly, but it does so indirectly by depressing the sensory nerve endings of the pharynx, larynx, trachea and bronchi." This means, too, that the reflex of coughing is lost and so the throat and air passages are quite liable to be blocked up with mucus. This complication contributes its share favoring a fatal termination of the anaesthesia.

I am not at all unwilling to allow that this morphine-before-anaesthesia practice may be a considerable convenience to the operator and especially the anaesthetist, but the life of a patient should never be put in jeopardy simply because the anaesthetist dislikes a scene or begrudges a few reassuring words to a nervous patient, or because vomiting by the patient is displeasing to his hyper-fastidious senses, or because he desires to watch the operation. When an individual is to be forced to pass through the valley of the shadow of death, I maintain that the unnecessary increasing of the hazards of the journey by deepening and prolonging anaesthesia in this manner, is a rank injustice. While I do not, by any means, stand alone in my position, yet I am aware that there is a very respectable array of authorities against me. So I embrace this opportunity to raise a note of warning against allowing our enthusiasm over any one especial theory on this question, to warp our better judgment. There will surely in the future be a reaction which will relegate to its proper place this morphine theory, which because of being comparatively lately advanced, happens to enjoy an undue amount of credit, and is actually doing harm by keeping in the background other theories, which, each in its own sphere, as a significant part of a complete whole, may be of very great importance in the successful solution of this anaesthesia problem.

RESTORATIVE MEASURES.

This part of my paper, looking from my own standpoint, will be of interest more particularly to those of you who *will* persist in the use of morphine before anaesthesia. Still, no care, no precaution, no method of administration will ensure absolute safety in anaesthetization. Some deaths will occur. Emergencies in anaesthesia calling for prompt action should be encountered in the same cool, rational way in which we endeavor to meet other unforeseen occurrences in operating. Just because the patient stops breathing is no reason why the anaesthetist should become unnerved or excited.

On a stand, to the right of the anaesthetist, there should be, among other things, a bottle of ammonia, and two hypodermic syringes, one loaded with fluid extract of digitalis, and the other with a solution of strychnia sulphate (5 m. equaling 1-20 of gr.) and, perhaps, some atropine tablets if it happens to be a morphine case. The indications for the use of the digitalis, I have given elsewhere in this paper. The strychnine is to be employed where there is failure of respiration, and in cases where the heart itself is weak, but where there is no particular depression of the vagi or vasomotor nerves. Amyl nitrite is never to be used except in the following condition: centric failure of respiration with anaemia of the brain, and suspected dilatation of the heart resulting from stimulation of the vagi and increased arterial tension from contraction of the arterioles. The ammonia can only be used as a rapidly acting but fugacious stimulant.

You will remember that you have 30 seconds after sudden heart failure, in which restorative treatment may be instituted with abundant hopes of success. Nervous haste is inexcusable. Not a move should be made without due consideration as to its rationalism. The skillful anaesthetist, who has had the advantage of closely watching the effects upon the patient of the anaesthetic throughout the anaesthesia, will require nearly no time to decide what is the best thing to do.

In failure of respiration, or heart, or both, artificial respiration according to Sylvester's method, with external intermittent thoracic and abdominal compression, is the most generally useful procedure. Inversion of the body does not enjoy the degree of confidence that it did at one time. The respiratory center is not always anæmic and besides, inversion allows the blood in the great veins to gravitate away from the auricles of the heart, thus losing to the patient, the advantage of the stimulation of the heart from the presence of the blood in it.

Another manœuvre of advantage is the alternate raising and lowering of the patients legs, the object being to stimulate contractions of the heart by alternately increasing and diminishing the blood pressure in the heart.

Another procedure recommended, is to employ pressure over the abdominal aorta, and to make deep, interrupted pressure upon the heart beneath the xiphoid appendix. This last procedure (Iliffe's) judging from McWilliam's experiments upon animals, is one of considerable value. In his experiments, artificial respiration, by compressing the thorax and even by pumping air in, failed to bring about recovery; but opening the thorax and compressing the ventricles between the finger and thumb at about the pulse-rate, re-established cardiac contractions, in one case after thirty-five minutes from the time of collapse.

DISCUSSION.

DR. CAUTHORN: I wish to compliment Dr. Amos for the very decided interest he has taken in this question and for the very considerable amount of study he has evidently put upon the subject of the physiological action of these various remedies. It seems that his study of the effect of these drugs has been very particularly confined to its effect upon the vascular rather than upon the respiratory and the nerve centers. Although, as the doctor says I have some very decided views on this question, they are in part founded rather

upon my own personal experience than upon the actual consideration of it from the standpoint from which he has viewed it. I am sure of one thing, that all deaths from anæsthesia, either chloroform or ether, are from failure of the respiratory center. I think that fails first and then the effects upon the heart comes afterwards, and therefore in the consideration of the action of these drugs, I think we ought to take into consideration their effect upon the respiratory centers. A man cannot die as long as you can keep him breathing. Believing as I do, that small doses of morphia have a tendency to quiet both heart and respiration—stimulate, if you might so put it, both the heart and respiratory and cardiac centers—and thus enabling you by its quieting effect to get along with a smaller quantity of anæsthetic, you thus add to the chances of your patient during an anæsthetic. For my part I am a firm believer, and have been for a number of years, and the more I use it the stronger my belief is, in the fact that a preliminary injection of a quarter grain of morphia with a small quantity of atropia, is a very valuable expedient before beginning an operation of any magnitude. The doctor speaks of the effect of morphia and ether combined, accounting for the repeated weakening of the heart's action after prolonged anæsthesia. I claim as a practical fact that is just what you do not get. You avoid the repeated weakening of the heart's action that you have afterwards. Theoretically, perhaps, it ought to be present, but actually it is not. It is a question of experience. Like a great many other cases, our theoretical hypothesis breaks down when we take the cold facts of empiricism. My own experience is that when morphine is given as I indicated, before an anæsthetic, that the anæsthesia is more quickly induced, that the anæsthesia is more quiet, that the anæsthesia may be maintained for the same length of time with perhaps not more than 50 per cent, of the anæsthetic, that the reaction afterwards is more rapid, that the return of the mental faculties is proportionately quicker and that the general after-effects of the anæsthetic are in large part done away

with. I think it will be some time until the doctor's theory has overcome the general experience with these things; and it is my own individual experience I am appealing to in this matter, because I think morphia was universally used in Germany in chloroform anæsthesia, and this idea is concurred in by our English brethren largely and I know to a considerable extent by very many operators in the United States. So that I believe the operative authority at present is in the administration of morphia, at least before chloroform anæsthesia; but I believe the effect is practically the same thing in ether anæsthesia.

DR. WELLS: I was not present to hear all the paper, but I heard enough to elicit my admiration and compliment for the author. I only wish to emphasize one of the suggestions, or rather the omission to emphasize one suggestion, that the paper contained at the close, respecting the power or benefit of suspension in cases of narcosis from the continued use of the anæsthetic. While I have not witnessed as many administrations of chloroform or ether as many on the floor connected with hospital work, still I must say that I have witnessed a good many. I have had one patient die under anæsthesia several years ago in this city. That subject was a large, fleshy woman. It was not recognized that she had had any previous cardiac trouble but she died on the table. Suspension was not used. Subsequently I had a patient die again, a patient of Dr. Mackenzie and myself. I say she died, because all the reflexes were gone and she was in articulo mortis and when I called the doctor from the adjoining room he immediately suspended her by the heels and I resorted to artificial respiration and we brought her to. I think but for the suspension she would have been a dead patient.

DR. MACKENZIE: She was suspended twice, was she not?

DR. WELLS: Yes, probably there were two failures. The operation went on for three or four hours and she was

restored. Dr. Koehler was the administrator; he remembers the case. The suspension in that case evidently was the salvation of the patient. There was, I believe, no injection or any other measure used. The doctor did not emphasize the value of the measure in his paper as superior to the methods of injection. I think the first thing to do would be suspension and then to keep up injection at the same time.

THE CHAIR: If there are no further remarks we will call on Dr. Amos to close the discussion.

DR. AMOS: I have nothing further to add.

AN ADDRESS ON DISEASES OF CHILDREN.

BY MAE H. WHITNEY, M. D., PORTLAND, ORE.

When I received an invitation from this Society to write a paper on Diseases of Children, I was glad for two reasons. The first, because I feel most deeply that if there is any place in medicine for the woman physician it is in the department of pediatrics—by the side of a sick child, and feeling it my special prerogative, I enjoy writing upon the subject. The second reason is because there is no branch of clinical medicine so immature to-day as the department of pediatrics, and the field for study and for results, is rich and of absorbing interest. In the development and progress of medicine the adult has been the standard toward which treatment has been directed, and the child has been left to the grandmother for diagnosis and treatment of its ills.

In 1860 there was no instruction given in college regarding children and I shall never forget my own illness in 1863 of which the treatment, in my opinion now, was worse than none. In 1860, A. Jacobi instituted a children's clinic in connection with the New York Medical College. In a few years Bellevue and the College of Physicians and Surgeons followed suit, and one hour each week, during the winter course, was given to special instruction on infants and children. Later the chair of gynecology or obstetrics in the different colleges was made to include lectures on pediatrics; at present a few schools have clinical professorships in this branch and the Harvard Medical School has this year established a chair with the title of full professor.

Our schools in Portland for a few years have had Chairs on Diseases of Children but no attempt has been made to organize a clinic. Post-graduate study is thorough in New

York, and in Germany specialists are very common. The formation of the American Pediatric Society and the late additions of children's sections to the American Medical Society and the New York Academy of Medicine are giving an impetus toward developing a better knowledge that will soon be felt.

These are the most important advances that have been made, yet there are men who realize the necessity for continued labor and one of the last acts of Keating's life was the publication of the *Cyclopedia of Children's Diseases*, and the *New York Journal of Pediatrics* should be on every physician's subscription list. Still there is so much more to learn, these steps are only the beginning. "The child is father to the man." As is the growth and development of the child so is the health and intellect of the adult. Oliver Wendell Holmes says, "The training of a child should begin a hundred years before it is born." And so it is true too that its health as well as its habits depend upon the treatment it has received during the century preceding its birth. These things must be learned from the study of the child and not the adult alone. Anatomy, physiology and chemistry form the basis of all knowledge of man, and the anatomy, physiology and chemistry of childhood is quite at variance with that of the adult. It is very convenient to know that the child's heart and diaphragm lie higher than the man's, and that the protuberance of the abdomen is caused by the obliquity of the pelvis and the large size of the liver, and that the capacity of the stomach of the new born is only about 4 ounces, and often vomiting depends on giving more than that amount of food at once, otherwise most peculiar and erroneous diagnoses may be made. And how many young graduates can tell the amount of feces proportionate to the ingestion of milk, or the difference in the bile of the adult and the infant, or the origin of fermentation and gases peculiar to the bowels in health and disease, yet these and many more things

must be learned before clinical medicine can be materially assisted by physiology.

In diagnosing clinically I must have a thermometer, a nude child and a soiled diaper. The pulse is valuable in watching the strength and regularity of the heart, but aside from that it has been supplanted by the thermometer which tells so quickly whether the case is one of simple febricula or is of serious import, and it is a particular point to be sure that the temperature is correctly taken, which is not always easy to do with a child as it always cries at the first application of the cold hard bulb. The rectal temperature is the most reliable though it is most conveniently taken from the groin. The primary object in studying a child is to get points without letting it cry, but if it be perfectly nude a small child will cry, yet by getting at different parts of the body one at a time or by having it dressed in a night-gown the object is accomplished by tact and skill, the necessity for which you are all familiar with at such times. The soiled diaper reveals the condition and defects of nutrition. It is a book of many pages in which we read much that we do not understand. How little can we tell without it the causes that give rise to the constant crying. "An infant crying in the night, crying, crying for the light, with no language but a cry."

I often think as I note the evolution of the world's progress that we are only, in this latter half of the 19th century, emerging from heathenism and superstition into civilization, of which fact there is no greater proof than the prevalent idea of treating the child in ignorance.

Last November an article was read before the Chicago Society by a Chicago Specialist, entitled, "A Plea for the Study of Pediatrics." It hardly seems possible that such a paper is necessary in a country where the rights and privileges of children are so thoroughly recognized as in America, yet when one looks around in Oregon and sees the dearth of interest in children's diseases and notes the high rate of mortal-

ity in infancy all over the world, one feels that two or three papers each year with that title would not be amiss. I remember so well at the New York Polyclinic an instance of a physician after as close an examination as he knew how to make of the child, pronouncing the case one of simple adenitis, and the quiet sarcasm of Prof. Ripley when he told the class that the adenitis was caused by a developing scarlet fever. Here in Portland where malaria plays such unheard of pranks, I know a story of a poor little child that fell into the hands of the Philistines—the Homœopaths—and because malaria caused its stomach to reject food and the sharp pains run about like little streaks of lightening through its intercostals, it was treated for two weeks for some indefinite form of spinal trouble, and was later cured by anti-malaria treatment. I know of many such cases, as do you all, and diagnosis is no more obscure or less understood in any department of medicine than in this. But while the Regular Profession in Portland is treating children apathetically, the Homœopaths are making capital for their profession and their pockets by cultivating the practice among children until a great many more people than should do so, consider a regular practitioner best for the adult members of the family but the children must be treated homœopathically. This impression has been fostered and developed by the swing the regular profession has carelessly permitted the homœopath to get among the charitable institutions for children in this city. They are all run now, and have been for years, by the homœopaths, who have assiduously cultivated the ideas of the benevolent ladies who mostly manage these institutions, to the belief that the medicine of the allopath is too strong and too vile for a child to take. This is not right, as you all are aware. It is an injustice to the principles on which we found our actions and belief, and in defense of these principles and in order that the laymen may have a better understanding of the regular school, better work should be done among the rising generation. Such a move has of late been made at the Baby's Home, where a regular has lately superceded a homœopath.

PRINCIPLES INVOLVED IN THE STUDY AND TREATMENT OF
DISEASE.

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The philanthropic phase of the healing art has always been its pride and its honor, and it was never so deservedly its pride and honor as in the present time. Witness the untiring labors of the professional men individually, and collectively, through the press, at the lecture desk, and in organized bodies local, national, and international, to teach the laws of health to the multitude, and to establish such legislation as shall protect the people from the ravages of preventable diseases. Withall, ungrateful tongues have said medical men are not sympathetic, and they have even been spoken of as deteriorating in moral nature as a result of experimental research, and this brings us to a brief consideration of "Vivisection." There was a time when experiments carried out without anæsthetics entailed a considerable amount of pain, but experiments on dumb animals causing pain have of necessity become less, and less numerous. The physiologist can readily study the complicated phenomena of the circulation of the blood—of respiration—of various kinds of movement, quite apart from and independent of the presence of consciousness. As his knowledge in ideas and his means of research multiply, the power of analysis will grow more and more, and by and by if physiology be allowed free scope for its development, there will come a day when the physiologist in his experimental inquiries, will cause pain, *then*, and then only, when pain is the actual object of his study, and that he will probably study best upon himself.

Do persons who entertain such unkind thoughts ever

stop to consider what good to mankind is thereby wrought which could not otherwise be gained? That the science of physiology is thereby advanced, that our knowledge of the laws of life has in the main been won by experiments of living animals! No one can tell what "might have been" without such experiments, physics and chemistry aided by mathematics, might have synthetically resolved the problems of life, but as a matter of history, experiments on living animals have been the stepping stones of physiological progress. The history of medicine in past centuries is largely occupied with the conflicts of contending schools of pathology schools, which arose from this or that mater, putting forward a fancy or a fragment of truth as the basis of all medical judgment. These have given place in the present century to a rational pathology which knows *no* school, and adheres to the words of no master, but is slowly and surely unravelling particle by particle, the many separate tangled knots of disease. They have given place, because men have come to see that maladies can only be mastered through a scientific comprehension of the nature of disease, many if not most of these experiments must be made on living beings. Hence it is, that animals are killed and suffer pain in order that physiological knowledge may be increased and disease made less. Take away from the art of medicine all that with which physiology has enriched it and the physician and the surgeon of to-day would be little better than a mystery man or a quack. Take out of the present system of physiology all that has been gained by experiments on living animals and the whole structure would collapse. As far as we can see what has been, will be the physiology of the future, if not hampered by any ignorant restraint, will, out of the death of animals, continue to press on putting a new aspect on physiology, and shedding a gleam of light into the very darkest regions of that science, conferring upon the physician, and not the physician only, but the masses, power to prolong and purify the life of man. Therefore I assert deliberately that the physiologist is justified in

the death he causes, and the pain he gives. The recognition of abnormal life actions, derangement of functions, and of the rationale of cause and cure constitute the science of medicine, the science, however, which teaches to do is indispensable; science, is but the comprehension of the facts and reasons. Art is the skill acquired by study, observation and practice. The facts of anatomy, physiology, pathology and therapeutics, are acquired by most students more readily than the ability to execute with the hands the dictates of the will. The one can be learned from lectures the other only by thoughtful, careful, persevering labor. Discoveries in medicine, looking to prevention of disease, have been more successful than means of cure, and have opened up a field of research, closely connected with the dearest interests of humanity. In this connection, I would like to say something about the microscope, and its wonderful revelations, but my time is limited, and as there are other divisions of this subject which may briefly be considered as worthy of interest, I will press on with a moments reflection in the direction of the study of the prevention and treatment of microbic maladies. Our knowledge of protective inoculations for contagious diseases, dates from Jenner's discovery in 1768. Look at the historical record of the terrific epidemics which at various times have raged so violently as to sweep away in a brief period, whole communities of people. Compare them with conditions at the present time, and you will come to see the gigantic strides with which the science of medicine has traveled, and you will be obliged to acknowledge the life-long labors of men in the profession for the good of medicine and humanity. In that one disease, small-pox, vaccination combined with isolation, has undoubtedly reduced an almost ever present pestilence, to a condition about as manageable as measles.

More recently it has been demonstrated that animals may be rendered immune by inoculating them with filtered cultures containing microbic products but not living organisms; by this method animals can be rendered immune to tet-

anus and diphtheria. The chief features in acquired immunity is the presence in the blood of elements which can neutralize toxic products of bacteria. These elements are called "*Antitoxines.*" These facts are of immense importance, for on these lines will be solved the prevention and treatment of microbic diseases—with the great progress of sanitary science, with the diffusion of the knowledge of personal hygiene among the masses, the day will come when epidemics of contagious diseases will be an almost unknown factor in medicine, and the layman will find himself exempt from the contagion of pestilential diseases and with the prospect of added years to his average life.

One word in regard to antiseptics and disinfectants. Within late years these two classes of substances have sprung into great prominence in medical and surgical work—not that they had not been known, and utilized in various ways at a comparatively early date, but that with the greater advances in surgery and medicine, the deeper study of pathology and the causes of disease, the deeper insight into the mysteries of micro-organismal growth, a demand has arisen for some material that would lessen the failures of surgical operations skillfully performed, prevent the spread of malignant and infectious diseases, and destroy the germ believed to be the cause of those diseases. This demand seems to be fulfilled in antiseptics and disinfectants; you are doubtless familiar with these various agents and it is unnecessary to enumerate them. As with all movements in which something has proved itself worthy of consideration, the matter of antiseptics and disinfectants has been carried to an extreme—sometimes to an almost ludicrous one. To-day, many surgeons whose hands and minds have been turned in the direction of the great cavities of the body, the cerebral and abdominal cavities, have in a great measure discarded the much praised antiseptics, and are insisting that cleanliness is as true antiseptics as is needful. The effects of cleanliness are self evident. The fact that therein lies perfect asepsis and disinfect-

tion cannot be denied. For thousands and thousands of years disease has raged among the children of men, slaying alike the aged man and the innocent babe at its mother's breast, and it seemed men and women were ever doomed to continually combat dirt, disease and the devil; but in more recent times intelligent men have come to understand that if dirt is banished from their homes, no place is left for disease and the devil, but where dirt is in abundance there also generally will be found its two boon companions. The principles involved in the treatment of diseased conditions rest on the four greater pillars on which the science of medicine is reared; without a knowledge of these we are powerless to treat disease intelligently. What is required then is, first, an intimate acquaintance with the organization of man, his anatomy—which teaches us how he is made and put together, the uses of the various parts, as well as their names and their relations to other parts. Next, a knowledge of physiology, from which we learn the proper functions of the anatomical parts in life, their action and influence on each other part and on the whole. Then comes histology and pathology, and lastly we have materia medica and therapeutics. Having an acquaintance with these principles we can act understandingly, without them all efforts must be experimental. We cannot treat a disease successfully until we recognize it, and having made a correct diagnosis we must know its character, what its usual course is, what it will result in if left to itself, and what remedies will reach and correct it. In order to cure a disease we must strike to the bottom of it and thoroughly eradicate from the system all trace of it. But while we are doing this we must not forget that one of the best principles of treatment is based on the power of nature to restore itself, or that over-treatment often results in as much or more harm, than under-treatment.

Let us get more into line with physiological action, it is useless to fight against the unceasing activities of nature, we must place ourselves parallel with her path, and not get di-

rectly athwart her course, and endeavor to force her into our command. As irritation enters largely into the etiology of disease, our first step should be to remove this—that is the first effort of nature. In order to do this she immediately establishes a condition of inflammation. Having removed the cause, nature will take care of the result. Throughout the system there is constantly going on a process of waste and supply, and where there is a natural action, nature will bring to the parts requiring it, a sufficient amount of building material to restore them to health, if it is not beyond her power. Take now (to apply this illustration) a necrosis of a portion of the superior maxillary, I select this bone because from its exposed position it is more subject to injury which may cause necrosis. The maxillaries have a greater degree of vitality than most of the bones, and consequently, greater power to resist disease, and greater capability for reproduction—where a portion is lost from disease or otherwise. A part becomes *necrosed, dead*—that part is an irritant, it is in an unnatural condition, the system immediately sends extra blood to the part to set up an inflammation to remove it. If the necrosed part becomes *exfoliated, nature* will succeed in carting it off as it would a splinter from the hand. If it is not detached from the vital portion, more blood is sent, *more* inflammation is set up, *regiment* after *regiment* of corpuscles are sent to aid in expelling this extraneous object, which from its change from a *vital* to a *devitalized* condition has become a *foreign intruder, finally* nature converts her corps of blood corpuscles into *pus* corpuscles. They undermine, they sap, they dissolve the surrounding tissues, they use all their efforts to hurl the enemy out. They fail, capillaries cease to perform their functions, a condition of sluggish action goes on, and comparative stasis is the consequence. Then comes surgery, it comes with its instruments, and removes the dead bone from the living, clears away all the debris of battle, depletes the engorged capillaries, stimulates the absorbents to carry off what is not useful, encourages the capillaries to renewed ac-

tion. *The enemy is gone!* The work of restoration must commence. Little by little, the building material is brought, it is taken up, and each atom is placed where it belongs, a cell is built here, another there, they reproduce, they grow, they increase, and in a short time *all is completed*, and, if the ravages of the disease have not been *too great*, the parts are restored as far as possible to their normal condition, I say as far as possible, for I doubt if any reconstruction ever equals the original. Nature is always at work restoring the waste, and loss which are continual, we wish merely to aid her, and give her a chance. She always does her work well, she wants some assistance at times, but she must not be killed by mistaken kindness. The successful treatment of diseased conditions then, lies in a knowledge of the organization, a correct diagnosis of the disease, and an intelligent, and judicious administration, and application of therapeutic agents, or the skillful use of surgical instruments, as the case may require. But aside from this knowledge many other things have to be taken into consideration, the general surroundings, the temperament, the question whether the disease is local or general, and if local, whether it may not depend on some systematic cause which must be removed before local treatment will be of any benefit; the condition of the blood, the habits of the patient, etc., *not* overlooking the correctness of prophylactic treatment, which if it will accomplish the purpose is certainly much better than remedial.

Before concluding my paper, I would like to advert for a brief moment to the topic of specialism. The rapid advance in the practice of medicine and surgery, that has been steadily and unceasingly exhibited, even within the short period of our personal recollection, is doubtless in some measure due to its sub-division into the component parts that are now made the subjects of *individual* study, and special application. I mean the recognition of the various sections into which the great system of medicine is rightly enough divided, as "*parts of the whole*" branches owing their vitality to the

main stem from which they derive nutrition, and support. *Specialism* must exist. It has been a spontaneous, and I believe a healthy growth, consequent upon the expansion of science, the increase of population, the advancement of civilization. On closing I would only say we cannot over estimate the importance of careful study, close observation, noting the result of our experience, and comparing it with that of *others*, that *thus* we may acquire the means of combatting disease. But his skill is not to be gained in a day, nor can any degree of perfection be arrived at without *labor*. As the microscopic cell by its multiplication and enlargement, forms the several parts of the human body, and these by their harmony, form the structure, man, so do the atoms of knowledge which we gather here and there by observation and experience, and store away in the recesses of the brain, each a power in itself, yet dependent on every other, and strengthened by every additional atom, unite to form that living body of science which is the source of all our art.

There being nothing further before the association the Chair introduced the president elect, Dr. Doane, who addressed the members as follows:

Gentlemen:—I wish first to express to you my hearty thanks for the honor which you have conferred upon me, and I can honestly assure you it was entirely unexpected and took me very much by surprise. I realize there were many other members whose labors in behalf of this Society have been more extensive than mine and whose claims to official recognition were certainly vastly in excess of mine. I have been a member of this Society for some nineteen years and I have yet seldom had the opportunity, unfortunately for myself, to meet with you. I realize that this is the Society that represents the profession in the state of Oregon; that it is not local in its province, but claims to represent the state. The more nearly we can accomplish that purpose the better will we have done our work as a Medical Society. For the physicians who reside in Portland, the local society will perhaps afford ample opportunity for the reading and discussion of scientific papers and for all the business for which a Medical Society is usually organized; but as representing the profession of the state the State Society fulfills its function and has a claim to recognition by every member in the state who can aid it in any way. We have a large number of members who are not here because of distance and the exigencies of practice and business, but I think we should improve the work of the Society and its influence and extend our power for the benefit of the profession by using all measures and all honorable means to encourage the presence and co-operation of the members of the Society outside of the city of Portland. I think the members who reside in Portland realize this, and it is largely through their efforts in the past few years that

recognition has been accredited to members living outside the city and that their interest in the business of the Society has been elicited. I think for that, great credit is due to those who live in the city. I recognize that the president, or any officer of the Society, is but your servant in carrying out your wishes, and it will be my effort during the time I shall fill this office to carry out the wishes of the Society and to do whatever lies in my power to advance the business for which I think it is established. (Applause.)

The Secretary made a statement to the Society of the financial condition of the association, showing that over and above present indebtedness it had \$150 or \$160 in the treasury, but that the expenses for getting out the report of the present meeting would be considerable and that members might look for an immediate call for dues.

OREGON STATE MEDICAL SOCIETY.

OFFICIAL LIST OF ACTIVE MEMBERS.

| NAME. | P. O. ADDRESS. | ADMITTED. | PLACE AND DATE OF GRADUATION. |
|-----------------------|--------------------|------------|--------------------------------------------------------------|
| Adair, Mrs. B. A. O. | Skipanon, Or. | June, 1882 | Med. Dept. Univ'y of Mich., Ann Arbor, Mich., June, 1880. |
| Amos, W. F. | Portland, Or. | May, 1894 | College of Physicians and Surgeons, N. Y., 1892. |
| Bailey, F. A. | Hillsboro, Or. | Sept. 1875 | Med. Dept. Willamette Univ'y, Portland, Or., March, 1870. |
| Beckman, O. H. | Astoria, Or. | June, 1895 | Jefferson Medical College, Philadelphia, Pa., 1884. |
| Beebe, Charles E. | Woodland, Cal. | June, 1883 | Med. Dept. Willamette Univ'y, Portland, Or., April, 1883. |
| Belknap, L. J. | Portland, Or. | June, 1895 | Ann Arbor Med. College, Ann Arbor, Mich., 1888. |
| Binswanger, Otto S. | Portland, Or. | June, 1883 | Med. Dept. Maryland Univ'y, Baltimore, Md., March, 1882. |
| Bevan, A. D. | Portland, Or. | June, 1885 | Rush Medical College, Chicago, Ill., Feb., 1883. |
| Bell, James F. | Portland, Or. | June, 1888 | Toronto University, Toronto, Canada, June, 1882. |
| Boyd, W. H. | Portland, Or. | May, 1894 | Med. Dept. Willamette Univ'y, Portland, Or., 1881. |
| Boys, William | Portland, Or. | June, 1890 | Med. Dept. University of Pennsylvania, 1864. |
| Byrd, W. H. | Salem, Or. | June, 1890 | Med. Dept. Willamette University, Portland, Or., 1890. |
| Beers, H. E. | Wasco, Or. | June, 1891 | Med. Dept. Willamette University, Portland, Or., Apr., 1890. |
| Beers, O. K. | Wasco, Or. | June, 1891 | Med. Dept. Willamette University, Portland, Or., Apr., 1890. |
| Brooks, E. R. | Island City, Or. | June, 1891 | Med. Dept. University of Iowa, March, 1886. |
| Brosius, F. C. | Hood River, Or. | June, 1893 | Rush Medical College, Chicago, Feb., 1883. |
| Botkin, A. W. | Mt. Tabor, Or. | June, 1892 | Med. Dept. Willamette University, April, 1890. |
| Calbreath, J. F. | McMinnville, Or. | June, 1880 | Med. Dept. Univ'y of Cal., San Francisco, Nov., 1875. |
| Carll, W. E. | Oregon City, Or. | June, 1895 | Harvard Medical School, Boston, Mass., 1885. |
| Cardwell, Mrs. Mae H. | Portland, Or. | June, 1885 | Med. Dept. Willamette University, Portland, Or., Apr., 1885. |
| Cauthorn, F. | Portland, Or. | June, 1889 | Jefferson Medical College, Philadelphia, Pa., March, 1879. |
| Chambers, E. H. | Portland, Or. | June, 1895 | Med. Dept. Willamette University, Portland, Ore., 1895. |
| Channell, D. B. | Portland, Or. | May, 1894 | Med. Dept. Willamette University, Portland, Or., 1894. |
| Colman, N. B. | Montesano, Wash. | June, 1888 | Dartmouth Med. College, Dartmouth, N. H., Nov., 1865. |
| Cliff, H. R. | St. Helens, Or. | June, 1888 | Darlington Med. College, Sidney, N. S. Wales, Dec., 1880. |
| Cornelius, C. W. | Portland, Or. | June, 1889 | Med. Dept. State University, Portland, Or., March, 1889. |
| Chambers, R. P. | East Portland, Or. | June, 1891 | Columbus Med. College, Columbus, Ohio, 1877. |

OFFICIAL LIST OF ACTIVE MEMBERS—Continued.

| NAME. | P. O. ADDRESS. | ADMITTED. | PLACE AND DATE OF GRADUATION. |
|---------------------|--------------------|------------|---------------------------------------------------------------|
| Coe, Henry M. | Portland, Or. | June, 1891 | Long Island College Hospital, New York, June, 1880. |
| Cox, G. W. | Brownsville, Or. | May, 1894 | Med. College of Indiana, Indianapolis, Ind., 1879. |
| Cromwell I. N. | Union, Or. | June, 1892 | Med. Dept. Willamette University, June, 1875. |
| Clymer, H. V. | Huntington, Or. | June, 1892 | Med. Dept. Willamette University, April, 1890. |
| Daly, Benard. | Lakeview, Or. | June, 1889 | Med. Dept. University of Louisville, Ky., March, 1887. |
| DeBar, George. | Jacksonville, Or. | June, 1889 | Missouri Med. College, St. Louis, Mo., March, 1874. |
| Diedrich, Edward. | Portland, Or. | June, 1889 | Omaha Med. College, Omaha, Neb., March, 1882. |
| Doane, O. D. | The Dalles, Or. | June, 1877 | Med. Dept. Willamette University, Portland, Or., May, 1876. |
| Dodson, O. M. | Baker City, Or. | June, 1877 | Med. Dept. Willamette University, Portland, Or., June, 1877. |
| Darling, Thos. | East Portland, Or. | June, 1891 | Detroit Med. College, Detroit, Mich., March, 1889. |
| Dickson, J. F. | Portland, Or. | June, 1892 | Toronto University, June, 1880. |
| Eaton, Frank B. | Portland, Or. | July, 1876 | Pacific Med. College, San Francisco, Cal., Nov. 1875. |
| Ellis, M. H. | Albany, Or. | June, 1895 | Med. Dept. Univ. of Michigan, Ann Arbor, Mich., 1879. |
| Farra, George R. | Corvallis, Or. | June, 1879 | University of Kentucky, Louisville, Ky., —, 1876. |
| Fitzgerald, P. H. | Gervais, Or. | May, 1894 | Med. Dept. of Willamette University, Portland, Or., 1886. |
| Fraser, E. P. | Portland, Or. | June, 1879 | College of Physicians and Surgeons, N. Y. City, March, 1861. |
| Fulton, J. A. | Astoria, Or. | June, 1883 | N. W. Medical College, St. Joseph, Mo., March, 1880. |
| Fenton, J. D. | Portland, Or. | June, 1890 | Med. Dept. University of Oregon, Portland, Or., March, 1889. |
| Fried, M. | Portland, Or. | June, 1892 | College of Physicians and Surgeons, N. Y., Nov., 1889. |
| Geary, E. P. | Medford, Or. | June, 1890 | Jefferson Medical College, Philadelphia, Pa., March, 1882. |
| Giesy, A. J. | Portland, Or. | June, 1879 | Med. Dept. Willamette University, Portland, Or., June, 1876. |
| Givens, John W. | Blackfoot, Idaho. | June, 1882 | Med. Dept. Willamette University, Portland, Or., June, 1875. |
| Gillespie, R. L. | Portland, Or. | June, 1886 | Med. Dept. Willamette University, Portland, Or., April, 1886. |
| Geary, John W. | Halsey, Or. | June, 1888 | Med. Dept. Willamette University, Portland, Or., March, 1883. |
| Glendening, R. W. | Portland, Or. | June, 1891 | Med. Dept. Willamette University, Portland, Or., April, 1891. |
| Geisendorfer, J. A. | Arlington, Or. | June, 1893 | Jefferson Medical College, Philadelphia, Pa., May, 1893. |
| Goddard, H. S. | Fossil, Or. | June, 1893 | Med. Dept. University of Oregon, Portland, Or., April, 1888. |
| Guyon, E. F. | Pendleton, Or. | June, 1892 | Medical College of Ohio, March, 1891. |
| Gaff, J. V. | Portland, Or. | June, 1892 | Ensworth Medical College, St. Joseph, Feb. 1889. |

OFFICIAL LIST OF ACTIVE MEMBERS—Continued.

| NAME | P. O. ADDRESS | ADMITTED | PLACE AND DATE OF GRADUATION. |
|------------------------|--------------------|-------------|---------------------------------------------------------------|
| Hall, C. H. | Salem, Or. | Sept. 1874 | Med. Dept. Willamette University, Portland, Or., March, 1868. |
| Hall, J. E. | Clatskanie, Or. | May, 1894 | Jefferson Medical College, Philadelphia, Pa., 1869. |
| Holmes, H. R. | Portland, Or. | June, 1887 | Med. Dept. Willamette University, Portland, Or., June, 1877. |
| Honan, M. F. | La Grande, Or. | June, 1889 | Bellevue Hospital Medical College, New York, March, 1879. |
| Humphrey, T. C. | East Portland, Or. | June, 1889 | Med. Dept. Willamette Univ'y, Portland, Or., April, 1886. |
| Hampton, L. Victoria | Portland, Or. | June, 1889 | Med. Dept. Willamette Univ'y, Portland, Or., April, 1889. |
| Huntington, John | Kelso, Wash. | June, 1891 | Med. Dept. University of Oregon, Portland, Or., April, 1891. |
| Hollister, O. C. | The Dalles, Or. | June, 1892 | Chicago Medical College, March, 1885. |
| Jones, Henry E. | Portland, Or. | Sept. 1875 | Bellevue Hospital Med. College, New York City, —, 1869. |
| Joseph, Simeon E. | East Portland, Or. | June, 1878 | Univ'y of California, San Francisco, Cal., November, 1877. |
| Jones, William | Portland, Or. | June, 1879 | Pacific Med. College, San Francisco, Cal., November, 1878. |
| Jefferson, G. O. | East Portland, Or. | June, 1891 | University of Michigan, Ann Arbor, Mich., 1889. |
| King, Mrs. Lydia M. H. | Portland, Or. | May, 1884 | Women's Medical College, Philadelphia, Pa., March, 1880. |
| Kime, James H. | Bandon, Or. | June, 1890 | Medical Dept. University of Oregon, —, 1890. |
| Koehler, Geo. F. | Portland, Or. | June, 1890 | College of Physicians and Surgeons, New York, —, 1889. |
| Kelly, Richmond | Portland, Or. | June, 1891 | Miami Medical College, Miami, Ohio, March, 1884. |
| Lee, Nathaniel L. | Junction City, Or. | June, 1877 | Med. Dept. Willamette Univ'y, Portland, Or., March, 1871. |
| Lane, Harry | Salem, Or. | June, 1877 | Med. Dept. Willamette Univ'y, Portland, Or., May, 1876. |
| Lang, E. R. | North Powder, Or. | June, 1893 | Western Reserve Medical College, February, 1871. |
| Lemieux, L. J. | Portland, Or. | April, 1893 | Laval University, Montreal, Canada. |
| Linklater, S. T. | Hillsboro, Or. | June, 1888 | Med. Dept. of Edinburg Univ'y, Edinburg, Scot'd, Aug., 1882. |
| Littlefield, H. R. | Portland, Or. | Sept. 1875 | Rush Medical College, Chicago, Illinois, February, 1870. |
| LaMoree, DeWitt M. | Portland, Or. | June, 1892 | Albany Medical College, New York, December, 1870. |
| Mackay, A. E. | Portland, Or. | June, 1890 | Toronto, University, Toronto, Canada, June, 1887. |
| McCormac, J. T. | Marshfield, Or. | June, 1883 | Med. Dept. Willamette Univ'y, Portland, Or., April, 1882. |
| MacKenzie, K. A. J. | Portland, Or. | May, 1884 | McGill, University, —, Canada, June, 1880. |
| Moore, A. W. | East Portland, Or. | June, 1886 | Med. Dept. Univ'y of Burlington, Burlington, Vt., June, 1883. |
| McDaniels, W. J. | Albina, Or. | June, 1888 | Jefferson Medical College, Philadelphia, Pa., April, 1883. |
| McElroy, J. G. | Portland, Or. | June, 1889 | Rush Medical College, Chicago, Illinois, February, 1874. |

OFFICIAL LIST OF ACTIVE MEMBERS—Continued.

| NAME | P. O. ADDRESS | ADMITTED | PLACE AND DATE OF GRADUATION. |
|----------------------|--------------------|-------------|---------------------------------------------------------------|
| McDonald, J. J. | La Grande, Or. | June, 1891 | Univ'y of California, Univ'y of Pennsylvania, 1877, 1887. |
| Macdonald, B. J. | Portland, Or. | June, 1895 | Med. Dept. Willamette Univ'y Portland, Or., 1886. |
| Molliter N. | La Grande, Or. | May, 1894 | Rush Med. College, Chicago, Illinois. |
| Maxwell, W. E. | Portland, Or. | June, 1893 | Jefferson Med. College, Philadelphia, April, 1885. |
| McCallon, B. H. | Dallas, Or. | June, 1893 | Vanderbilt University, Tenn., March, 1893. |
| McDaniel, E. B. | Cove, Or. | June, 1893 | Jefferson Med. College, Philadelphia, May, 1893. |
| Marsden, W. L. | Burns, Or. | June, 1892 | Kentucky School of Medicine, June, 1891. |
| Maston, G. W. | Albany, Or. | June, 1892 | Cincinnati College of Medicine and Surgery, Feb. 1876. |
| Nottage, George E. | East Portland, Or. | Sept. 1875 | University of California, San Francisco, Cal., Oct., 1874. |
| Nunn, Richard | Portland, Or. | June, 1889 | Trinity Hall, Dublin, Ireland, Dec., 1883. |
| Panton, A. C. | Portland, Or. | May, 1884 | University of Toronto, Toronto, Canada, June, 1882. |
| Page, J. Frank | Mystic, Iowa. | June, 1887 | Minnesota Med. College, Minn., Feb., 1884. |
| Price, Helena J. | Portland, Or. | June, 1889 | Woman's Med. College, Philadelphia, Pa., Mar., 1886. |
| Pruett, J. M. | Pendleton, Or. | June, 1889 | Ohio, Med. College, Cincinnati, Ohio, Mar., 1875. |
| Parker, Thos. | Portland, Or. | June, 1892 | Rush Medical College, Chicago, February, 1886. |
| Pernot, H. S. | Corvallis, Or. | June, 1892 | Cincinnati College of Med. and Surg., Feb. 1888. |
| Quigley, J. M. | Portland, Or. | June, 1892 | Med. Dept. Willamette University, April, 1892. |
| Rand, D. H. | Portland, Or. | June, 1890 | Med. Dept. Willamette University, Portland, Or., 1885. |
| Rowland, L. L. | Portland, Or. | Sept., 1874 | Med. Dept. Willamette University, Portland, Or., Mar., 1872. |
| Raffety, C. H. | Salem, Or. | Sept., 1875 | Med. Dept. Willamette University, Portland, Or., March, 1869. |
| Richardson, J. A. | East Portland, Or. | Sept., 1875 | Med. Dept. Willamette University, Portland, Or., March, 1869. |
| Raffety, David | Salem, Or. | June, 1890 | Cooper Medical College, San Francisco, Cal. |
| Robinson, F. M. | East Portland, Or. | June, 1883 | Med. Dept. Willamette University, Portland, Or., 1882. |
| Shorey, J. L. | Beaverton, Or. | June, 1888 | Med. Dept. Willamette University, Portland, Or., April, 1885. |
| Strong, Curtis Clark | Woodburn, Or. | May, 1894 | College of Physicians and Surgeons, Baltimore, Md., 1891. |
| Saylor, W. H. | Portland, Or. | Sept. 1874 | Bellevue Hospital Med. College, New York City, March, 1872. |
| Stott, John S. | Portland, Or. | Sept. 1875 | Med. Dept. Willamette University, Portland, Or., Mar., 1869. |
| Stanley, H. B. | Gervais, Or. | June, 1883 | Med. Dept. Willamette University, Portland, Or., Mar., 1883. |
| Steincamp, E. D. | Dallas, Or. | June, 1890 | Med. Dept. State University, Portland, Or., April, 1888. |
| | LaGrande, Or. | June, 1893 | Ohio Med. College, Cincinnati, March, 1883. |

OFFICIAL LIST OF ACTIVE MEMBERS—Continued.

| NAME | P. O. ADDRESS | ADMITTED | PLACE AND DATE OF GRADUATION. |
|-----------------------|--------------------|------------|--------------------------------------------------------------|
| Smith, I. M. | Moro, Or. | June, 1893 | Med. Dept. Willamette University, April, 1892. |
| Smith, A. C. | Portland, Or. | June, 1892 | Cooper Med. College, Nov., 1887. |
| Sutherland. | The Dalles, Or. | June, 1892 | Toronto University, April, 1891. |
| Svenson, E. O. | North Yamhill, Or. | June, 1892 | University of Nebraska, March, 1885. |
| Smith, C. J. | Pendleton, Or. | June, 1892 | Bellevue Med. College, N. Y., March, 1890. |
| Tamiesie, J. P. | Hillsboro, Or. | June, 1890 | Med. Dept. University of Oregon, Portland, Or., 1889. |
| Thornton, E. H. | Portland, Or. | June, 1895 | Med. Dept. Oregon State University, Portland, Or., 1892. |
| Tower, Chas. W. | Marshfield, Or. | June, 1880 | Med. Dept. Willamette University, Portland, Or., June, 1870. |
| Tucker, E. F. | Portland, Or. | June, 1893 | Harvard Med. School, June, 1884. |
| VanDyke, F. W. | Grant's Pass, Or. | June, 1892 | Bellevue Med. College, March, 1875. |
| Wilson, Holt C. | Portland, Or. | June, 1880 | Bellevue Hospital Med. College, N. Y. City, March, 1877. |
| Wheeler, C. H. | Portland, Or. | June, 1881 | Ohio Med. College, Cincinnati, Ohio, March, 1875. |
| Wells, G. M. | Portland, Or. | June, 1882 | Bellevue Hospital Med. College, N. Y. City, 1874. |
| West, O. J. | Wasco, Or. | June, 1890 | Med. Dept. Willamette University, Portland, Or., Mar., 1889. |
| Warren, Mrs. A. L. F. | Portland, Or. | June, 1887 | Med. Dept. Willamette University, Portland, Or., June, 1887. |
| Wright, H. A. | Linkville, Or. | June, 1888 | Med. Dept. University of Oregon, Portland, Or., Apr., 1888. |
| Wilson, Geo. F. | Portland, Or. | June, 1889 | University of Virginia and University of N. Y., Mar. 1880. |
| Wall, Geo. | Cottage Grove, Or. | June, 1891 | Rush Med. College, Chicago, Ill., Feb., 1889. |
| Wood, W. L. | Portland, Or. | June, 1892 | St. Louis Med. College, March, 1886. |
| Wilson, E. N. | Portland, Or. | June, 1892 | Rush Med. College, Feb., 1887. |
| Walls, J. T. | Portland, Or. | June, 1892 | Ohio Med. College, Feb., 1877. |
| Young, Philip R. | Portland, Or. | June, 1890 | McGill's University, Montreal, Canada, 1876. |

The following members have been dropped from the roll for non-payment of dues:

Viola M. Coc, F. Crane, E. O. Bostwick, F. M. Brooks, W. H. Ehlen, M. A. Flinn, J. W. Frazey, A. Frank, A. T. Fulton, J. Holt, G. E. Houck, C. Hines, E. L. Irvine, J. B. Laughary, H. F. McCornack, H. Meesman, J. W. Norris, S. Parker, R. Pryce, M. J. Patton, E. P. Pickel, J. J. Sellwood, E. A. Sommer, A. Sonnenfeld, W. B. Watkins, J. F. Watt, H. A. Wall, J. H. Wells, A. D. Walker, W. D. Wood.

MEDICAL DEPARTMENT

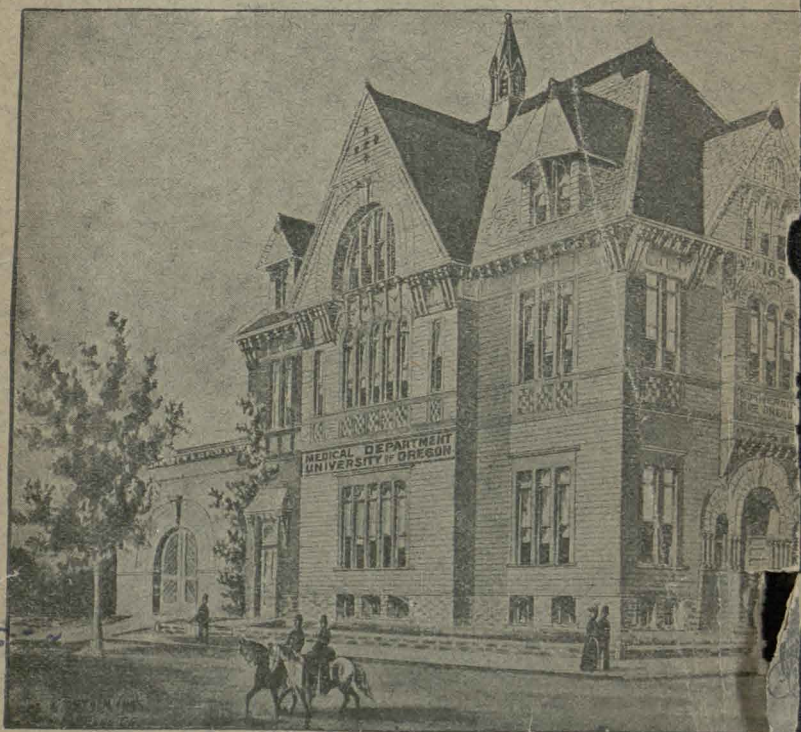
UNIVERSITY OF OREGON

CONFERS THE DIPLOMA OF OF THE
STATE UNIVERSITY.

Thorough Clinical and
Didactic Instruction
High School

EIGHTH REGULAR SESSION
WILL COMMENCE OCTOBER 7, 1896.

Exclusive control of Clinical Work at St. Vincent's
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