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AND

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Founded by
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The motive of this Journal is to replace the cell doctrine by the Electron theory. Vital phenomena are dynamic and the actions of organisms should be regarded as processes and not structures. Exclusivism is excluded insomuch as all sciences are embraced in practical medicine and diagnosis must invoke physical, biological, and chemical methods. All problems in medicine not in accord with the progress made in physical science are doomed to perish.

We invite and encourage articles on ERA, but do not necessarily hold ourselves responsible for the ideas contained therein.

Citation from other sources will be duly accredited.

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SEPTEMBER, 1925

HONORARIUM

Dr. G. E. Thompson, Peoria, Ill., writes "In regard to the statement received of the Honorarium Fund, I wish to state that I am for it."

Dr. Ina Light Taylor, Chicago, Ill., says: "I think this a very fine adjustment."

Every Lessee should subscribe to this fund. It is cheap insurance and will produce results. We feel sure that after the Chicago Convention there will be a revival in ERA work. Several surprises are on the program—scientifically, Dr. Abrams theory has been proved—clinically, the results have proved the method.

It is now time for every ERA Doctor to get his house in order. You have patiently waited for the change; it is coming.

THE PASSING OF DR. J. W. KING

By DR. FRED. E. MOORE

I feel I must add a word of respect and admiration for Dr. J. W. King to the splendid lines from Dr. Ivar Janson, and Dr. J. F. Spaunhurst, which appeared in our last Journal. It was those of us who labored with him as Trustees of the College that realized the worth and sincerity of the man. He was staunch in his loyalty to E. R. A. and to the standards established by Dr. Abrams.

He told me the light went out for him when Dr. Abrams died. He said he felt lost and had not been able to work with the same intense interest. Dr. King was a frequent visitor at Dr. Abrams' Clinic. He was with Dr. Abrams when the building at 2151 Sacramento Street was started, also when the foundation was laid for Dr. Abrams' bungalow by the sea, and again at the laying of the cornerstone for the new College building.

Dr. King was very proud of the fact that his only grandchild was named Albert Abrams Fitzgerald. He had two children, Mrs. Marion King Fitzgerald and Ralph King.

From Dr. J. C. Burnett came these lines: "Deeply grieved. One of the staunch pillars has at last succumbed to the march of time. My dear friend has passed on to a better world. A life well spent and a work well done. Staunch, true and loyal to his last breath."

EXCERPTS FROM "PATHOLOGY AND IMMUNOLOGY OF SYPHILIS IN RELATION TO TREATMENT AND THE COMPLEMENT FIXATION REACTION", JOHN A. KOLMER, M. D.

The article from which these excerpts are taken appeared in the Bulletin of the Battle Creek Sanitarium and Hospital Clinic. This subject is of such importance, and especially to the ERA Practitioner, who is often confronted by the question as to the value of the Wasserman reaction, and how soon after treatment it will become negative, that we take this opportunity of presenting the salient points of Dr. Kolmer's paper. All bold type is ours. Note these carefully and compare with ERA.

* * *

"It has been variously estimated that there are probably as many as **ten million syphilitics in the United States.** This statement is based largely upon the surveys made by means of the Wassermann test. If, then, there may be, roughly, 10 per cent of the population infected with this disease, if this represents a fair average, even if the estimate is too high for certain classes of society and too low for others—we must realize that syphilis is a disease to be reckoned with in every specialized branch of medicine regardless of the particular line of work, and it is a disease well worthy of our best efforts in diagnosis and treatment. And this is emphatically true since the **disease is transmissible to the second and possibly the third generation.**

* * *

"However, before we discuss the pathology and the immunity of syphilis itself, I would like to remind you of work that is today in progress in different parts of the world which tends to show that the parasite of syphilis as we know it, as a spirochaete, is probably in but one phase of its life cycle. At the Research Institute in Philadelphia we now believe that evidence is accumulating rather fast to show that this spirochaete may have a granular stage in the course of its development; and, if it is finally proven that the spirochaete of syphilis does have a **truly granular stage, I think we can then explain more easily the paternal transmission of the disease** and other phases of the mechanism of the infection. In vestigations at the present time would seem to indicate that this germ may have a life cycle **one stage of which is a granular one exceedingly difficult to recognize.**

"Another point worthy of consideration, particularly by the clinicians, is the fact that we are not sure that there must be a break in the skin or the mucous membrane before the germ of syphilis can produce infection. You will probably recollect that many years ago Reasoner, working in the Army medical laboratories, found that the germ of syphilis could pass through a practically intact mucous membrane. Dr. Wade Brown and Dr. Louis Pearce, at the Rockefeller Institute, have shown that the spirochaete of syphilis can go through apparently healthy conjunctiva of the rabbit and produce syphilis, and we have been able to confirm those observations at the Research Institute in Philadelphia. It is not at all improbable that the human being may contract syphilis by the parasite penetrating through the intact mucous membrane; we need not in the future insist that there must be a break in the epithelium before the parasite is capable of gaining access to the tissues.

"I believe that the times has arrived when the clinician should place very little emphasis upon a negative history if there is conclusive evidence of syphilis in the form of a positive Wassermann reaction, because the parasite may infect the human being in this atypical manner.

"Probably within forty-eight hours after the germ of syphilis infects the human being it is already being disseminated through the body. If we take a rabbit and inoculate the scrotum or the testicle with the parasite of this disease, then castrate the animal forty-eight hours later, we are generally unable to prevent subsequent generalized syphilis. I believe it should be realized that in the human being, **by the time the primary lesion of this disease develops, the germ has already become rather widely disseminated throughout the body, through the lymphatics, and through the blood stream.**

* * *

"We consider the chancre an inflammatory reaction toward the parasite which is highly beneficial to the infected individual. For example, if we take a rabbit, produce the chancre, **and then administer treatment enough simply to suppress the chancre without bringing about complete sterilization, we subsequently find that this rabbit will develop a severer type of syphilis than a rabbit left untreated.**

"No one, of course, should accept experiments of this sort as an indication that we ought to treat the disease in the human being

during chancre stage. Not at all. But the lesson that we, as clinicians, must learn from experiments of this sort is that when we do treat syphilis in the primary stage we must treat the disease with sufficient vigor to bring about a rapid sterilization as possible. **If we can apply to the human being what we have observed with rabbits, the lesson can be very definitely drawn that to treat syphilis in the primary stage mildly and ineffectually with arsphenamin or neo-arsphenamin may actually spread the infection more rapidly than would otherwise be the case.** We believe that this is due to the fact that insufficient treatment suppresses the immunological reaction that takes place in the chancre, and that it is very important indeed, when we undertake the treatment of the disease during the acute stages, to push the treatment with as much vigor as we safely can in order to bring about as rapid sterilization of the patient as possible.

"I am not at all sure from the experiments we have carried out with rabbits that our method of treating the disease in the primary and secondary stages, is not sometimes responsible for the early development of syphilis of the central nervous system—if we can apply the results of experiments with rabbits to the human being. I do know quite definitely that if we take the syphilitic rabbit in the chancre stage and treat it at that time ineffectually we can produce lesions of the central nervous system relatively early. In other words, I very much doubt that there are specific strains of spirochaeta pallids. I do not doubt for a moment that strains of pallids vary in virulence just as we have different races of pneumococci and streptococci and other bacteria which vary in virulence; but I doubt very much that we actually have instances of specific strains of the germ for the central nervous system. I am rather inclined to think that, in those cases in which we see the development of paresis and tabes within two or three years after primary infection, **the individuals were unfortunate in the way they were treated** in the primary stage or they were unfortunate in contracting infection with a particularly virulent strain of the spirochaete, or that they were individuals **who had inherited certain fundamental weaknesses of the central nervous system which favored the localization of spirochaetes in those tissues.**

* * *

"Doubtless most of the physicians who have used the Wassermann reaction in the diagnosis of the diesase have noticed that it is

sometimes positive at one time and weaker or stronger positive at a subsequent time; or it **may be positive at one time and negative at another.** This is a condition by no means unknown to the serologist. It is not a matter of surprise to the serologist because the substance that is responsible for the Wassermann reaction and for the flocuclation test in syphilis is a substance derived from the body cells in direct contact with the germ; and, inasmuch as there are many things that can stimulate the germ into periods of activity followed by periods of rest, it is to be expected and experience shows **that the Wassermann reaction tends to fluctuate** from at least week to week—I do not know that we can say from day to day—and it is **particularly apt to fluctuate during periods of treatment.**

"You may then well ask: What is the nature of the Wassermann anti-body? Is it not to be looked upon as an expression of immunity? **To the best of my knowledge we do not know the exact nature of immunity in syphilis.** We know that it exists, but we do not know just what is its mechanism. The question may then be very properly asked: Isn't the substance present in the blood and spinal fluid of the syphilitic and anti-body? My answer to you, given without any hesitation whatever, is that the substance responsible for the Wassermann reaction is not a true antibody in any sense of the term. If the so-called Wassermann antibody were a true antibody, we would expect to find that during the latent stages of the disease the Wassermann reaction would be very strong; but just the reverse is generally the case. The Wassermann reaction in latent syphilis is very frequently weak, and, indeed, the reaction might be negative, showing that the antibody is not present at all. In other words, the Wassermann antibody is not to be looked upon as a true antibody in any sense of the term. As far as our own opinion is concerned, we belive that the substance present in the blood and in the spinal fluid of the syphilitic which is responsible for the Wassermann reaction is simply a product of the body cells in contact with the germs. In other words, a better term for this substance **is Neisser's term "reagin,"** The Wassermann reagin; or we may call it the Wassermann antibody if we keep in mind that it is not a true antibody.

"The practical lesson to be drawn from a conception of this sort is that the Wassermann reaction is an index of the degree of infection in this disease, not an index of the degree of immunity. When we have a strongly positive Wassermann reaction we have every

reason to believe that the spirochaetes are numerous and active. But the clinicians may very quickly say, "Why is it that we have a strongly positive Wassermann reaction in individuals who show practically no symptoms of the disease?" My answer is that **the symptoms of the disease depend upon the involvement of physiologically important tissues.** We can for instance, have numerous spirochaetes in the periosteum or in some tissue of the body that is physiologically unimportant and those spirochaetes may produce large amounts of Wassermann antibody and yet the individual might be for all intents and purposes quite a normal, healthy man. On the other hand, if we take just a few spirochaetes—sometimes too few to give a positive Wassermann reaction—and pass them down the posterior columns of the spinal cord we produce marked symptoms of syphilis in the form of locomotor ataxia.

"Therefore, it is largely a question of where the spirochaetes are located. A few of the organisms in a very important tissue may produce very profound symptoms and disability and yet the organisms may be so few in number that they may give us a negative reaction even with a most delicate method.

"I repeat, therefore, as my conviction, **that the Wassermann reaction is** to be taken **as an index of the degree of infection. It is not to be accepted as an index of the degree of immunity.** And I believe that the clinician can expect to find in his work that there will be no agreement, no necessary agreement, between the strength of the Wassermann reaction and the clinical symptoms and disability of his patient. As a general rule we find the strongest positive Wassermann reactions in cases of syphilis presenting the most profound symptoms; but, on the other hand, in latent syphilis, we may have germs located in areas of the body that produce no symptoms, while the germs are sufficiently active to produce large amounts of the reagin, giving strongly positive Wassermann reactions.

"The Wassermann reactions, as you doubtless know, when first discovered was thought to be specific for syphilis. **No one, to the best of my knowledge, at the present time regards the Wassermann reaction as specific for this disease.** When the reaction was first discovered, in 1905, and from then until 1907, it was thought that the antigen used in conducting the test would have to be prepared from the germ itself. Very fortunately indeed for medical science, Wassermann and his co-workers, and Detre, a Hungarian who discovered it at the same time Wassermann did,

independently of him, did not have in 1905 pure cultures of spirochaetes to work with. This organism was not cultivated in pure cultures until 1912. Back in those early days they had to make the antigen for this test by extracting syphilitic tissues. It was known, for example, that the liver of a syphilitic infant **contained a large number of germs, and Wassermann** made his antigen from the **liver of the stillborn syphilitic fetus.** The positive reactions that were observed were thought to be due to the fact that when the extract was made of the liver they extracted something from the germs themselves.

"In 1907, however, another German investigator showed that the antigen for the reaction could be made by extracting a perfectly normal tissue, like a beef heart, or a human heart, or a guinea-pig heart, and we now know that wehn we prepare the antigen for the Wassermann test all of value that we get from the tissues in this reaction is the lipoids. The reagin, or so-called Wassermann antibody, is endowed with the peculiarity of being able to take lipoids in colloidal suspension and cause their flocculation or precipitation.

"If the antigen is of a certain concentration and if diluted in a certain way and if the serum is used in a certain dose, we can see these flocculi with the naked eye; but, on the other hand, as we conduct the Wassermann complement fixation test, we cannot see with the naked eye the flocculation of the lipoids by the Wassermann antibody of the serum. Nevertheless that occurs. We can sometimes see it with the microscops, particularly with the dark field illuminator, and if there happens to be in the test tube the serum of a guinea-pig to which the name complement has been given, these lipoids are flocculated by the syphilitic reagin of the patient's serum with the fixation of complement.

* * *

"The Wassermann reaction, therefore, is not specific for syphilis because the antigen is not specific. We can make the antigen from any normal tissue, and particularly well from beef heart. Yet the Wassermann reaction possesses a very high degree of specificity in the diagnosis of this disease. It sounds almost paradoxical to say that a reaction has no biological specificity and still possesses a high degree of practical specificity. Nevertheless, it is only the spirochaetes of syphilis and of frambesia, or yaws, a tropical disease, that are capable of producing this type of antibody.

"If we go through the early literature on the Wassermann reaction, from 1906 to 1912 or 1913, we find many papers which would lead us to believe that the Wassermann reaction may be positive in scarlet fever, in leprosy, in pneumonia, in malaria, in typhoid fever, and even in some cases of diabetes. As far as I am able to determine on the basis of our work, particularly with our new test, this is not at all true. To the best of our knowledge, there are only two diseases that give us a positve Wassermann reaction, **syphilis** and **frambesia.** Frambesia is a tropical disease unknown in this country, and, therefore, I believe that in the United States a technically correct positive Wassermann reaction is to be accepted as an indication of syphilis. The test in the United States and in other localities where frambesia is not present, has a very high degree of specificity for syphilis.

"**Unfortunately, the Wassermann test is very intricate in technic and there are numerous sources of error in conductint it.** These are known as **technical sources of error** and they can creep into the work of even the best technicians and the best workers in this field. If technical errors can be removed; if one is sure that they do not occur, a positive Wassermann reaction, even though it is only weakly positive, is to be looked upon as an indication of the presence of syphilis.

* * *

"The Wassermann reaction can err biologically by giving **false negative** reactions much more frequently than by giveing **false positive** reactions. I feel like constantly adivising physicians not to accept a negative Wassermann or a negative flocculation or precipitation test as excluding syphilis. Dr. Warthin, of Ann Arbor, in the brilliant work he has done on the pathology of syphilis has shown us in a clear and conclusive manner that **even at the autopsy table we cannot by looking at heart and aorta and other of the internal organs say that that individual is free from syphilis.** As he has taught us, we must examine the tissues histologically under the microscope before we can draw a conclusion of that sort.

"It is my firm conviction that the **Wassermann reaction is by no means senstitive enough. We have no serological test for syphilis that is too sensitive** and I doubt very much whether we will ever have one sensitive enough to detect all cases of syphilis. Therefore, I believe that in this country the Wassermann reaction is more apt to err by giving false negatives than it is by giving us

false positives. The false positive reactions, particularly in this country, are due rather to errors in technic than to any property of the blood of the individual.

"The only disease in which a false positive reaction might be due to that disease is leprosy. A change takes place in the blood in leprosy which increases the chances of falsely positive Wassermann reactions; but the bloods of lepers which, through the co-operation of the Public Health Service, we have had an opportunity to examine sith the new complement fixation test have shown that falsely positive reactions do not occur in non-syphilitic lepers; likewise Dr. Kahn's precipitation test is in our experience never positive in leprosy unless the leper is likewise syphilitic.

* * *

"Any serologist who employs a test in which he himself has no confidence in the plus one or plus two reactions should not use that test for the diagnosis of syphilis, because we want the Wassermann test to have the specificity and the senstitiveness which will enable us to diagnose the disease in its latent stages, when it has reached a stage in which the diagnosis cannot be made solely by the history or by the clinical examination. **To get a positive Wassermann reaction only in active cases of syphilis, thereby merely confirming a clinical opinion, to me does not mean anything more than taking an X-ray of a fracture of the arm in a case in which you can bend the bones.**

* * *

"We are frequently cautioned not to take the blood for the Wassermann test immediately after a course of treatment because a test made at that time may give a falsely negative reaction. My experience in Philadelphia, however, has been rather the reverse. I very frequently have found that if the blood is taken from the syphilitic immediately after a course of treatment it is positive, whereas if the patient is given a loner rest, say four to six weeks, it becomes negative. The advice that I would give, on the basis of observations of that sort, is this: When we use the Wassermann test as a guide in the treatment of the disease we should not draw the blood until at least four weeks, preferably six weeks, have elapsed since the last treatment, in order to avoid the error of giving too much treatment for the disease. Because if we take the Wassermann reaction right after a course of treatment it is so frequently positive that we would conclude that more treatment is necessary; whereas if we give longer

periods of rest between the course of treatment we will find that our percentage of Wassermann-fast cases is less than we ordinarily believe.

* * *

"The Wassermann reaction is very unsatisfactory in the type of congenital syphilis where the child survives through infancy and childhood and grows to adolescence and adult life. Those are cases of extreme latency in which the Wassermann is very frequently negative.

"I believe it is possible for a mother to give birth to a syphilitic child and later give birth to a non-syphilitic child, and possibly after the birth of that child to give birth to another syphilitic child. To me the question of a mother infecting her child is altogether a question of whether or not during the nine months of her pregnancy spirochetemia develops with the presence of spirochaetes in the blood. I think that if the mother's infection remains strictly localized in the tissues, if the spirochaetes do not get into her blood, the child might come to term and escape infection. On the other hand, if anything happens to that mother to stimulate her latent infection into activity, an accident or an intercurrent disease or the administration of drugs, for example, and if through that accident the spirochaetes get into the maternal circulation, there is a good chance for them being passed through the placenta into the fetal circulation, in which case infection of the fetus would follow. **On the other hand, from the standpoint of practice, I believe that we should regard all children of syphilitic mothers, of Wassermann positive mothers, as being worthy of treatment.** This is my own practice regardless of a negative Wassermann."

BACILLUS COLI COMMUNIS

By DR. JOSEPHINE A. JEWETT
Berkeley, California

While working with the "Colisepsis" of the atlas, it occurred to me that there was a strange similarity between Colisepsis and Gonorrhea in many ways. The polarity of both was — and + or "isopolar"; both had a destructive rate of 4; the ED areas of "Colisepsis" were identical with the SV areas of Gonorrhea. These observations led to experimentation. Colisepsis came through the rheostats at 52/6, the same as any strain comes through at the general VR on one rheostat and the strain VR on the other. Everywhere that I found Colisepsis at 6/6, I could also find 52/24 if not 52/52. The question followed: if Colisepsis is a colon strain of Gonorrhea, what is the Bacillus Coli Communis?

From the University Laboratory, I procured a tube of pure Coli Communis grown on agar-agar and obtained two new areas, SV and ED, from the tube with VR'S of 3, 13 and 31. The polarity was + only, and the DR's were 0 and 11. 0 was the DR for scar tissue and on looking up the VR's for scar tissue, they were 3 and 13, a startling fact. But any piece of scar tissue might be impregnated by Coli Communis, in which case the areas given for scar tissue in the atlas would be the areas for some strain of Coli Communis. But what strain?

Fearing that there might be some mistake in the tube culture, another was procured from the University guaranteed to be Coli from the **human** intestine and to have passed the tests by reactions on culture media to identify it as pure Coli Communis. The second tube gave the same results as the first on several different subjects. The ohmage given by the tube growth was 24.

Then began a search through the patients in the office for Coli Communis in the patient and in each case the identifying of strain VR's and areas wherever the infection was sufficiently modified by its surroundings to become a "strain," i.e., to possess a VR with SV and ED areas of its own in addition to coming through at 31/31 on the general areas. It has been my observation that in all these cases the ohmage on the strain area at the strain VR would be from 1 to several ohms higher than that obtained on the general area at

the general VR. When I found two areas, one SV and one ED, each coming through at the same strain VR and giving the same ohmage, both a little higher than the general ohmage, I felt that the strain was identified, but needed to be confirmed by discovery in several different patients. This work was done as fast as office material permitted, and all the strains given have been proved on several different patients and with several different subjects. They have also been confirmed by other physicians in California, at least.

As to the confirmatory work, I must except the "Skin Strain." I have found it in only two patients; Coli Communis in the skin seems rare.

In all the strains of Coli, I worked out the strain rates from 100 down, and in most of them found two or more VR's for the strain. Occasionally one of the VR's would be common to two strains of the germ, in which case I always use the individual VR, not common to another strain, in working with the rate.

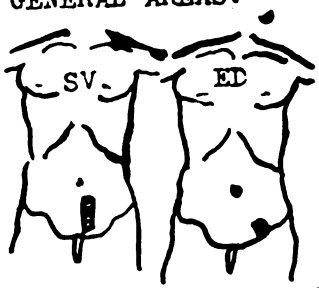
It was not until near the end of the work that I found the strain of Coli Communis giving the same areas as the atlas "Scar Tissue," and identified it as the Duodenal strain inhabiting the duodenal wall and occurring frequently in duodenal ulcers. At the time of operation a piece of "pure" scar tissue could be easily obtained from about a duodenal ulcer and could easily be full of Coli well adapted to that particular tissue and having a modified VR to correspond with its habitat.

The discoveries of Metchnikoff connected Coli Communis with the formation of fibrous tissues in the body, especially the general fibrosis of age, and there resulted the trade in Bacillus Bulgaricus to check this fibrosis and maintain youth in the arterial system and in the body generally. How much Coli Communis may have to do with the process of fibrosis in health and in normal healing of wounds, no one knows. We do know it to be a normal inhabitant of the intestine and concerned in processes of digestion.

Being always present in the intestine, Coli Communis is frequent in the blood and is thrown out by the kidney without harm unless it finds a locus of devitalized tissue where it can settle and become a pus former. Laobratory findings have proved the Coli to be a frequent pus former in abscesses. We know it to be a common infective agent in the kidneys, gall bladder, urinary bladder, and of course in the appendix and peri-rectal abscesses. By the Abrams method, I have found it frequently in the sinuses of the cranial and facial bones, supplementing the streptococcus there.

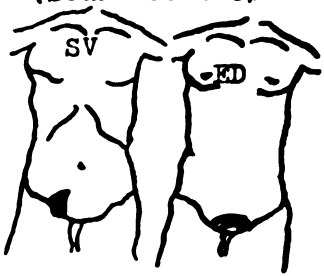
BACILLUS COLI COMMUNIS.

GENERAL AREAS.



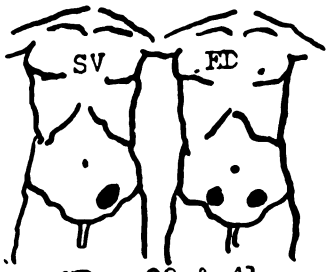
VR = 3, 13 & 31
DR = 0 & 11
P = +

DUODENAL STRAIN
(Scar Tissue)



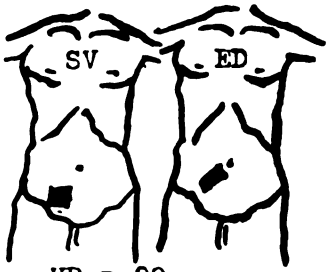
VR = 46

Gall Bladder Strain



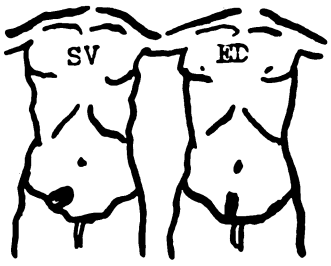
VR = 28 & 41

Kidney Strain.



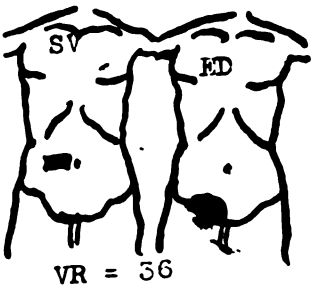
VR = 29

Rectal Strain



VR. = 20 & 51

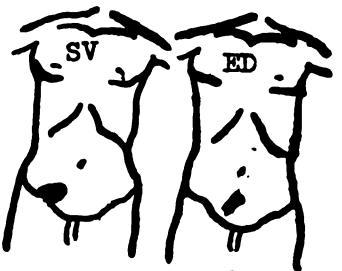
Spinal Cord Strain.



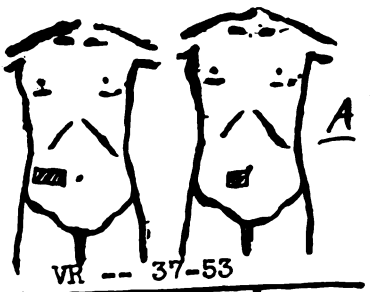
VR = 36

PULMONARY

MASTOID STRAIN.

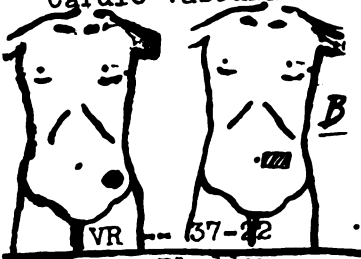


VR = 9 & 18



VR -- 37-53

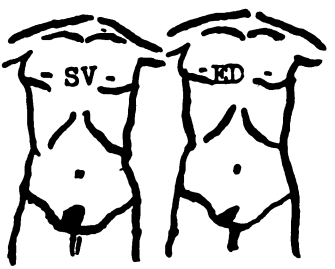
Cardio Vascular



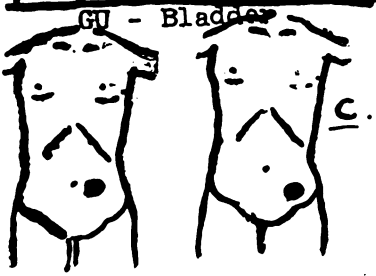
VR -- 37-42

GU - Bladder

Appendix Strain.



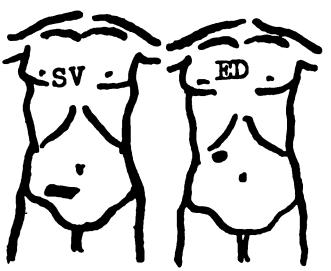
VR = 57



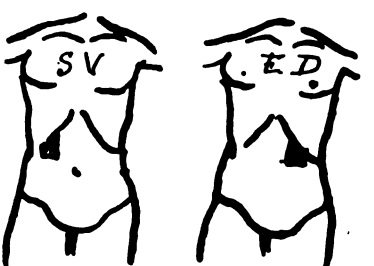
VR - 44-55

Skin Strain.

CARCINOMA-- PRIMARY LIVER.



VR = 6, 33, 53.



Strain

VR = 97, 92, 79.

Also it is a not infrequent complication in malignant growths, and it is my experience that both cancer and sarcoma do **much** better if cleared of Coli Communis as soon as possible. According to the Abrams findings, Coli Communis may be a resident in any tissue where vitality is lowered and act as an irritant, even when actual pus is not present. Clearing it up always seems to give the patient marked relief and help the resolution of the other difficulties; therefore I always look for it in the firxt examination and treat it at once.

Coli does not seem to depend on either 55, 57, or 52 to provide a soil for it. It has occurred in cases where these three basic toxins are cleared up, presisting as the cause of some still unrelieved symptom in the patient, which disappears when the patient is negative to Coli Communis. Furthermore, after the patient has been found negative to all the reactions, including Coli Communis, and has been laid off from treatment for a time, on re-checking a month or two later I have found Coli re-established somewhere again. Apparently after a long course of treatment, tissues which are newly cleared of infection, may still be of lowered vitality and a ready soil for a new focus of Coli entering the blood from the intestine. In such cases there is nothing to do but clear up Coli again and it goes very quickly on a little further treatment. As the patient gains strength, immunity to the infection develops.

Cases coming to Berkeley from the East have shown Coli Communis as the chief cause of symptoms still persistent after a good course of Abrams treatment in an Eastern state. One such is just finishing treatment in my office now. She had 8 units of Coli in in her gall bladder, with a trace of 52/24 and 60; also Coli in the frontal sinus, right antrum and right tonsil. The relief she is experiencing on clearing up this irritation is very marked.

As a rule, Coli Communis yields to the Abrams treatment very readily, and is therefore very satisfactory to treat. But if not cleared up, it maintains symptoms that are troublesome and keeps the patient "feeling badly."

It has sometimes appeared in the middle of a course of treatment, when it had not shown on the first examination at all. Whether this was due to its having been masked by higher reactions at first, or whether it was able to penetrate the tissues during treatment for other diseases, I do not know, but it cleared up readily when found.

Perhaps two other items might be added to this article. Influenza has long been known to leave lingering symptoms behind it, and I have often suspected it where I could find no reaction, but the patient gave a history of feeling badly ever since the "flu" some time previosuly. This led to a hunt for a cryptogenic or "accentuating" rate for influenze which I have found to be 38 55. It has been worth while looking over a patient pretty carefully for this cryptogenic rate, especially where a history of influenza is given.

Finally as to cancer: A patient applied for treatment for a carcinoma of the liver, far advanced. It had not been suspected as a cause of illness until the abdomen was opened, when the under side of the liver was found to be studded with nodules, although there were none in the rest of the abdomen. He was not coughing or showing any lung complications at that time. Taking this to be a case of primary cancer of the liver, I searched for its strain areas and VR's, finding them as given here. The rates have been very helpful in two other cases where the ohmage of cancer came through much higher on the liver strain areas and VR's than on the general area and the patient died; whereas on the general area showing they would have had a chance for recovery. Prognosis in such cases is important in a treatment where so much effort is being made to call it a fake.

* * *

We were more than pleased to be able to offer Dr. Jewett's findings to the profession. Dr. Jewett was good enough to give us these VR's and strains some time ago. Since then we have made constant use of them, with very gratifying results. We take the privilege of appending strains marked A, B and C, which we found in further investigation of Dr. Jewett's researches.—L. BIGELMAN.

USE OF A CONDENSER IN E. R. A. TREATMENT

By DR. T. P. HALL

In a paper presented to the recent ERA Convention in Chicago I described a means of measuring the relative efficiency of ERA devices. This was done by placing a bottle of water in the place of a patient for a measured time, and measuring the charge obtained by the water as you would measure the intensity of a disease, in the dynamizer. This system was used to compare the relative values of a rheostat and a condenser, in connection with the oscilloclast, for treatment.

Dr. Abrams had made some tests with a variable air condenser for diagnosis. He found the condenser just as efficient for disease detection, but failed to make a satisfactory combination for measurement of the intensity. On repeating these expreiments I found the reactions to be uniformly distinct when using the condenser. This, together with the analogy of radio tuning, let me to compare (1) a rheostat non-inductively wound, with the oscilloclast unit; (2) each of these with an air condenser. The former were set, for example, at the cancer rate, 642 ohms; and the condenser at the corresponding rate as found by Dr. Abrams, 100 micro-micro farads. The result showed that there was practical difference in the three methods. The results of the measurements, were made at VR 50 or at VR 642, were practically identical.

The resistance in each case were carefully measured, and the condenser was a DeForest, capacity .001 micro farad, for which is claimed an accuracy of 1 per cent.

In using the "personal rate," instructions for which have been given by Dr. DuPlessis and Dr. McManis, it is usually necessary to combine a micro with the oscilloclast unit, or to replace these with a good rheostat, or with a condenser. In a few cases I found it impossible to find a personal rate except with the condenser. The latter is also much more convenient to use, and therefore meets with favor.

An air condenser made by the General Radio Co., capacity 500 mmf, mounted, with vernier adjustment, I have found to be satisfactory. The mode of connecting up is as follows:

(1) For experiment with oscilloclast unit—

Oscilloclast—Unit and micro, wire from binding post to condenser. Wire from condenser to patient. The three switches on the unit must be set at zero, and it makes no difference which binding post is used.

(2) Replacing the unit—

Oscilloclast—one wire (of the two formerly leading to the unit) to condenser— wire to patient. I find no practical difference in this case in the two wires from the oscilloclast. If the return wire is also used for the patient, it is safer to connect it first with a small mica plate fixed condenser of capacity .001 or .0005, and from this to the patient.

I am invited to broadcast at W O R D, Botavia, Ill., September 21, 9:30 p.m., Chicago Daylight Saving Time. Wave length, 275 meters. Subject: "Abrams' Electronic Diagnosis and Treatment." It should be announced to Doctors of Western States. W O R D has been heard in California.

Also at W B B R, New York, September 28, 8:20 p.m., New York Daylight Saving Time: "Physical Health—Electronic Treatment." Wave length, 273 meters.—DR. MAE JOHNSON WORK.

DIAGNOSING IN THE LIGHT

By LEO BIGELMAN, M. D.

First of all let us say that making diagnosis in a completely light room is an accomplished fact, tested over a long period of time, and with the Abrams reflexophones. Many men have devised special apparatus with which it was possible to do work in the light, more or less accurately, but now for the first time we are able to duplicate this with our usual diagnostic apparatus, and with no loss in accuracy.

Several weeks ago we were experimenting with a wave meter connected through a crystal detector, in an attempt to gain further information about the character of the wave produced by the Oscilloclast. We noticed that the audibility of the make and break impulse was much more distinct, when the Oscilloclast was connected through the crystal. Then the thought occurred to us that a crystal detector in the diagnostic circuit might have some effect upon the reactions.

We, accordingly, introduced a crystal detector in series with the dynamizer and the reflexophones; that is, between the dynamizers and reflexophones; or, where an amplifier is used, between the dynamizer and the amplifier. We tried our reactions and they came through beautifully—much clearer than previously. Then we assumed that the crystal might have the effect of stabilizing the energy from the specimen. If that were true then the energy would come through more strongly and more constantly. To test this we admitted a small amount of light into the room, which had no effect upon the reactions. We increased the amount of light without any effect upon the reactions which were still distinct, but not quite as strong as in the dark. Finally we introduced all the light into the room possible, but still did not destroy our reactions. This convinced us that we had made a really worthwhile discovery.

Our next step was to find in which position the crystal detector worked best. We found this to be as close to the dynamizer as possible. Putting the crystal at the end of the circuit, between the reflexophones and the cord to the subject did not work. The best place for it was between the dynamizer and the amplifier with the "whisker" end of the crystal set very close to the dynamizer.

Previous to this work we had tried various diagnostic hookups, and found the one in which the subject, ungrounded, makes a com-

plete circuit with the apparatus most effective; that is, reactions were more constant and stronger. Consequently we tried this circuit in connection with the crystal detector and found it even better than either alone. We were thus able to combine the good qualities of the closed circuit with the stabilizing influence of the crystal detector.

Our present diagnostic circuit is, therefore, as follows: The ground wire from the dynamizer, instead of going to the ground, is attached to a plate at the sacral region of the subject. The top plate of the dynamizer connects with the crystal detector, and this in turn with the amplifier, thence through the reflexophones and to the head of the subject. The subject is ungrounded, but grounds himself between reactions. With this circuit we can work in any degree of light, but we must retain the East and West position. Deviation from this destroyed reactions.

The type of crystal we are using has two crystal surfaces, instead of a wire and a crystal. This obviates the need for finding the most sensitive point on the crystal, as it is always in contact with that point.

We hope that this discovery will find confirmation in the hands of other ERA men. So far it seems to promise greater stability and consequently greater accuracy in eliciting the reactions. The greatest advantage is, of course, the ability to work in complete light. Work can be done with greater ease and much less strain. Best of all, it overcomes the criticism, that we work in the dark, because we want to make a mystery of our work. Unjust and false as such a statement is, it nevertheless has great weight with some people, who are also just as easily won over by a demonstration to the contrary. What better demonstration than to do all of our work in the light and more than ever make light synonymous with ERA.

COMMENTS ON THE E. R. A.

By CORA SMIGH KING, M. D.
Pasadena, California

One Type of Lues Superimposed Upon Another—

A case of three separate primary luetic infections was reported in the Journal of the A. M. A. The exposure, the primary lesions and the response to treatment were noted in the history. Occasionally, we see a case where at least one acquired infection has been superimposed upon another, or upon an hereditary infection, of quite diffrent type, the two conditions as separate as two different runs of lava, a gray stratum upon a harder black stratum, such as is seen in volcanic regions, for instance, on "The Barrier," in Mt. Garibaldi Park, British Columbia.

Case: A young man showed the cerebrospinal type with a spastic gait and grandiose ideas. After a course on the Oscilloclast and medicine, he had seemed very well and tested clear, when suddenly the re-infection manifested itself and he read 48 points on 55. The second infection was of the digestive and cardio-vascular type, with a yellow tongue and bluish color to the hands. This patient, a young man of 22, drank and was dissipated and had opportunity for re-infection.

Dizzy Spells—

Reported in a young man who is thin and old looking. Test shows acquired syphilis. The dizzy attacks, which are at times prolonged, might be of the nature of petit mal. Such patients are usually obstinate and unreasonable, and at times irritable and quarrelsome. This mental storm is sometimes premonitory of an attack and sometimes a psychic substitute for the attack. These persons are at other times childishly ingratiating in manner. With such a history, test for cerebrospinal strain under 55 or 57; also test for epilepsy.

Chest Symptoms—

Syphilis frequently affects the lungs, and the lymphatic system, giving a picture suggestive of tuberculosis, but without the bacilli in the sputum and without the characteristic beadlike extensions into the lungs as shown by the X-Ray. These chests will react to 55 and 58 (fibrosis), but not to 42. Syphilitic lungs will clear under

antiluetic treatment. **Case:** A woman patient, fifty years of age, came into the office leaning heavily upon her son's arm, because of her weakness. She had lost weight rapidly since an attack of influenza some weeks before, and had been growing more husky until she could now scarcely speak aloud. The husband, who had been principal in a school, had broken down nervously and given up work. He is now a harmless dement, praying and singing all night, and catching little naps during the day. The brain is continually irritated but not able to function properly. The wife has four children, three of them born after her husband became ill. All but the first one show congenital lines of weakness so that it is to be assumed the infection was acquired after the first child was born. This woman's chest cleared up under constitutional treatment.

Glandular Depletion at the Menopause—

In patients tending to be overweight, with scanty and delayed menses, with symptoms of exhaustion and nervousness, the activity of the thyroid and of the ovaries should be measured. These patients are greatly benefited by the Thyro-ovarian combination. Harrower's No. 4 tablets. These given in conjunction with the Curative Rate, which builds up the metabolism, will carry the patient past the critical age. The tablets and the treatment should be continued for several months. The combination is likely to rejuvenate the woman.

"Never Well Since Vaccination"—

Case: The blood shows acquired syphilis. It is possible that the infection came through the vaccination, as there was a history of "varioloid" following. Patient has pyorrhea; skin redness localized over the nose. The rosacea is nature's flag denoting infection in the system and usually in the genital organs. It has been proved that the nasal membrane and the uterine membrane have a reflex connection, illustrated by the cure of dysmenorrhea following treatment of the nose.

Malaria—

It is surprising how many patients' bloods respond to the malaria test, especially those from the Southeastern States and from the Mississippi and Ohio River valleys, and from Montreal, Canada. The malaria must be treated medicinally, concomitant with the treatment of the patient for constitutional conditions.

"Nerves"—

We must learn to respect those shattered nerves and to rebuke ourselves and not the patient if they do not calm down. There is often a complicated group of causes to be considered—anemia, glandular imbalance, improper diet, and the absorption of toxines from the intestines or gall-bladder.

Acute Neuritis or Rheumatism—

In seeking the probable focus causing this pus metastasis, the reactions of Abrams are of great assistance. Determination by this method is reliable in most instances, and is heartening to the patient and to the doctor. **Case:** Severe rheumatism of the left trapezius muscle, worse at the insertion of the muscle to the ribs, anterior to the axillary line, in a woman of forty, was excruciating for days and kept her from taking a deep breath. Setting the rheostats at 60 for strep., and having the subject localize with one hand to the site of severest pain, we were able to get an intense reaction on the strep. area, when the upper right or lower left jaws were touched by the subject with the other hand. This reaction did not occur when he touched the tonsils or the gall-bladder. X-Rays, and extraction of teeth in these regions, proved that not only were all the suspected teeth badly abscessed, but that there was an infected upper jaw bone, requiring weeks to heal and regenerate. The rheumatism promptly stopped when the teeth were out without further treatment, although it had presisted previously in spite of diligent treatment to the muscles with the hot light and the high frequency.

The Curative Rate as a Diagnostic Test—

Occasionally, to vary the monotony of tests, and to test the tester, it is well to find the Curative Rate before the headlines on the case have been determined. If the keynote of the case is shown by Rate 4 — 10.11, the reading made later should confirm the finding that colon mixed infection is the dominant note. If 6 — 40.32 cancels, we know there is reason to suspect cancer. If it is 1 — 37, we should find an active malaria, etc.

To get the Curative Rate, set the rheostats at 60, or at 57 or 55. 60 is present in practically every case, and has an area easy to use. If a rate cancels 60, it usually cancels all other reactions.

90 North Madison Avenue.

ERA EXPERIMENTATION WITH CLINICAL DEDUCTIONS—MEAT AND CANCER

By LEO BIGELMAN, M. D.

In the June issue of Physico-Clinical Medicine, we published the results of our experiments with sugars and starches, and their effect upon Streptococcic and Streptotoxic conditions. It will be recalled that we found Streptococcic infections were aggravated by the sugars and starches, and pointed out that this was in keeping with Clinical observation. We next took up the question of the relationship, electronically, between malignancy and food. Naturally this is too big a field to cover, and hope to arrive at a conclusion within a reasonable period of time. So we confined our work to the staple food of the city dweller, where cancer is most rampant, namely meat.

This is not an attempt to deal with the cancer question etiologically. It is merely an effort to clarify the manner of care of the cancer patient, so as to create the most favorable conditions for his comfort and possible recovery. Where the question of food is involved—and food is always a problem in any condition—we should at least adhere to the general principle of not feeding that which may have an aggravating effect upon the disease—least of all if there is the remotest possibility of its having a causal relationship.

Our experimentation with meats was along the following lines: We took various specimens of beef, pork, mutton, veal, and fowl, both raw and cooked, fresh and less fresh. We proceeded to test these specimens for their individual reactions, but gave that up as a hopeless task, as one could elicit almost any reaction or infection from raw meat, and very often from cooked, roasted or fried meats. In addition most of the specimens gave reactions of D. R., both congenital and acquired. Whether this was contamination or not, we did not attempt to determine. Some specimens also gave reactions of 50, 58 and 42. All the specimens gave reactions at V. R.'s other than those for disease reactions, which were more or less constant for the specimen, but varied with different specimens of the raw meat. Consequently differentiation on the basis of reactions was a hopeless task. The cooked meats were freer of reactions than the raw meats. Beef and pork gave more reactions than any other type. The others were very much alike. So that we could roughly divide our meats into two classes—beef and pork, and all others.

Our next step, and this is the important part of this work, was to observe the effect of the specimens of meat upon specimens of blood, and upon the reactions elicited from patients directly. Here we obtained some startling information.

Every case of malignancy was aggravated by bringing a specimen of meat in proximity with the blood specimen or the patient from whom reactions were being taken; that is, the reactions were increased tremendously. We found that raw meats of any age affected the reactions most strongly—cooked meats, less so. In some instances we obtained carcinoma reactions that ran up into the hundreds. The effect was more pronounced on blood specimens than on the patient. The important thing was that reactions of carcinoma or sarcoma were increased, and in no case, neutralized by the opposition of specimens of meat cooked or raw. The raw meats were aggravating in direct ratio to their age—the older the specimen the higher it sent the reactions. Of the other reactions some were increased and some decreased by meat, but with not the same constancy as the effects on carcinoma particularly.

This does not mean that the specimens of meat gave malignancy reactions. Some of them did—most of them did not, but if there was a malignancy reaction in the blood, **it increased it regardless of its** own reactions.

The pertinent question arises here—what would happen to the reactions if the patient were fed the meat? This we did not do, but will at a future date to prove or disprove the following theory: We are examining our patients right along and we do not find any startling increase in the reactions of carcinoma from time to time, and yet most of the patients eat meat. Offhand this would seem to disprove our findings, but closer analysis will show that another conclusion is probably much nearer the truth. Is it not possible that the something in meat which increased our reactions is a ptomaine or toxine, which, when ingested is more or less neutralized by the system. But this ptomaine or toxine when not neutralized stimulates the cancer reaction. If that is the case it is self-evident what an effect these substances would have on malignancy in an individual, when resisting or neutralizing forces of that individual are at a low ebb. And if Wm. Koch of Detroit is right in his theory of cancer, namely, that it is a reaction to a general toxemia in an effort to convert toxines into antitoxines, it is again obvious that if meats help to produce these toxic states, they must stimulate and

maintain the presence of cance.r This could take place without an actual increase in ohmage.

Is it not at all significant that certain conditions like carcinoma, tuberculosis, and strep. infections, although their ohmages are always less than those of diminished resistance, still require, as much and often more treatment than diminished resistance? Is it not also significant that these conditions clear up much more quickly under proper dietetic regime than without it? Then is it not probable that these conditions are more or less reactions to, originally, food toxemias through improper dietary control? These questions are offered not as conclusions but as thoughts worth considering in planning the general care of the chronic invalid, and especially the cancer patient.

We do not presume to say that meat has any etiological bearing on cancer, except insofar, as by contributing to a general state of toxemia and assuming that cancer may be a reaction to the toxic condition, it may have a contributory causative effect. But that meat has a favorable influence on the development of cancer once established, we are convinced. Our reactions prove that to a high degree of probability.

SOME DIAGNOSTIC SUGGESTIONS

By DR. HOWARD C. ATWOOD

When sitting in the clinic of Dr. Abrams while he diagnosed morning after morning, and hearing and seeing the smooth-going and the difficult days I used to wonder just what did make the difference. Dr. Abrams, master that he was, could sense and run down trouble in an uncanny way, even the fact that a reaction was smothered by interference rather than absent, he could discern in a way the rest of us could not fathom. I think all of us have been busy along the line of eliminating trouble ever since we took up the work. So that the things I may mention are not given as something new but simply as reminders, possibly of things we have known so long that we may have neglected them in the search for other peculiarities in this delicate job of diagnosing.

Although the reactions seem to be within the sphere of the parasympathetic, the vagus does have a decided influence with them. They are reduced or destroyed by a slight bending back of the

body or the head, even a yawn clears them all out. Differences in the tension of the neck and shoulder muscles of the subject is a preceptible variable.

It may be well to re-emphasize the two points in the diagnostic circuit which are most sensitive to outside influence, the subject and the dynamizer. The slightest pointing toward the subject of either her own or the operators fingers will change the reactions or destroy them, or if the assistant or a spectator points toward her, the result will be the same. If the subject points hand or elbow at the dynamizer it will change the result and the same is true of any other person pointing that way.

All of these last causes of interference seem to be more the result of direct radiation from the person rather than any capacity or inductance, and yet, capacity and inductance do play a part in the work and can be a source of interference.

For instance, if you will disconnect the reflexophone wire from the lid of your dynamizer and attach it to the secondary of a Ford coil and then place the coil alongside the dynamizer, anywhere within two feet of it you can make clearer and better diagnosis than with the regular arrangement and practically as well as when the primary of the coil is connected with the lid of the dynamizer. Without the dynamizer and coil connection the reactions can be stopped by the interposition of a piece of tinfoil, provided only that the tinfoil also shields the wire leading to the rheostat, so that I am inclined to call this a case of induction even though it does resemble a direct radiation effect.

In trying out what did and did not happen to that coil by shielding in various ways with tinfoil, I made some interesting discoveries. As far as shielding was concerned it seemed to matter little what was done so long as the edge of the condenser top of the dynamizer was in the plane of the coil and clear of the tinfoil, but the reactions seemed better if a curved piece of foil was placed on the opposite side of the condenser from the coil. No change if a piece of foil was placed over the top of the lid but not touching it, but the reactions were killed if the foil touched lid or its stem.

Further experimenting with tinfoil and the dynamizer developed the most important findings and the real reason for this article.

The Ford coil used as above gave decidedly better results than without it, but seemed to tire the reflexes too quickly. Putting the dynamizer on glass with a piece of tinfoil between and another piece of foil curved round part or all of the body of the dynamizer,

but not projecting above it or touching the projection from the sides, gave equally good results and made diagnosing a much clearer and easier procedure. In my own work I have found this to be an amazingly easy and extremely valuable way of making the diagnostic circuit much more sensitive and reliable.

One other way by which a doubtful reaction can be clearly shown and also a cryptogenic reaction brought out more quickly is to have the subject touch one hand to the magnet suspended over her head. I cannot detect any difference in the touching of either or both arms of the magnet.

Riverside, California, July 30, 1295.

DO YOU KNOW?

Conducted by LEO BIGELMAN, M. D.

DO YOU KNOW?

That Waves from Brain have been picked up by Radio?

The following article from the San Francisco Examiner for Augut 22, 1925, is but another corroboration of ERA. The Oscilloclast wave is 7.65 meters, and here they speak of a four to ten meter wave.

> Paris, Aug. 21 (Associated Press)—Sound waves from a human brain have been picked up by a radio receiver **on a four to ten meter wave length.** The experiment is described by Prof. Ferdinand Cazzamali, head of the department of neurology and psychiatry at the University of Milan, in an article prepared for the forthcoming issue of "Revue de Metaphysique" and reviewed by "Le Matin." As a result, Professor **Cazzamali foresees the transmission of sound waves from one brain to another.**

* * *

That Certain Bacteria Give Off Light?

The Scientific American for September, 1925, has a photograph of a Living Lamp.

It is a beaker containing a pure culture of Bacteria that give off a cold light which is said to be strong enough to recognize objects in its immediate neighborhood.

There seems to be nothing illogical or incongruous to the mind or minds that constitute the Scientific American, in the fact that they accept the phenomenon of bacteria giving off light, a definite

form of vibratory energy, and reject the phenomena of Electronic Reactions which detects the energy from all types of bacteria. The emanation of this light is simply a definite corroboration of the fact that all things radiate energy manifest and detectable in different ways. Those bacteria which do not give off light, give off their energy in some other wave form. The fundamental fact is the same in both cases. It is a radiant emanation of a vibratory character, and as such is detectable by ERA methods. We take this opportunity of thanking the Scientific American for helping us to prove our case. We know that they will continue to prove it more and more, as often as they publish the findings of modern advanced research in the realms of vibrational phenomena, which means all phenomena.

Henri Fabre, the great naturalist, wrote in one of his books: "The sure sign that one will be right in the future is to be unpopular in the present."

If the Scientific American had noted this statement, they probably would have taken less pains to enhance our unpopularity.

* * *

That the Scientists (?) are Puzzled Again?

In the Literary Digest for July 25, 1925, we read the following:

A DRUG WITH X-RAY QUALITIES.

"In an article contributed to Le Matin (Paris) Dr. Pierre Louis Rehm tells of a communication made to the Academy of Science in which two French collaborators, one of them director of the Bureau of Hygiene at Reims, announce the discovery that a familiar antiseptic, hypochlorite of sodium, may exericse its germ-destroying action without being brought into actual contact with the germs. A quartz tube containing the antiseptic diluted with water from the tap is placed in a receptacle containing a contaminated fluid, and left there for twenty-four hours, when, according to the report, about one-fourth of the microbes have been destroyed.

"Under these paradoxical conditions, the germicidal action of the drug is reported to be more active in the dark than in the light—a matter of significance, since sunlight is known to be germicidal. Says Dr. Rehm:

> 'The explanation? There is only the hypothesis of M. Philippe Bunau-Varilla, of the experimenters, to the effect that the molecule of soidum hypocholrite, in attacking organic matter, must emit rays analoguous to ultraviolet rays

in their germicidal action. That is the reason why quartz tubes, which transmit ultraviolet light are used, instead of glass, which is opaque to this light.

'It is possible that this unpredictable discovery may have important applications in the fields of medicine and hygiene. Galvini's expreiment with frogs was a small affair, yet it contained the germ of the modern development of electricity. **Yesterday, for the first time, it became known that an antiseptic may act without contact, as it were, by induction.'**

"All of which may be said to be important if true. Doubtless experiments so startling in their seeming implications will not long await verification or refutation at the hands of other workers."

Note the bold face (ours)—"Yesterday for the first time"? Where have the alert scientists been all this time that ERA has been treating and curing disease—as it were by induction"? Another case of "None so blind as those who will not see."

ERA has been amongst them curing them and yet they are surprised and puzzled when they find that a drug may act without, contact through its emitted energy. I wonder will they dare go a step farther and theorize that some day perhaps drugs and other things will be indentified by the energy which they emit?

* * *

That the American Bath Tub was Declared a Nuisance?

The first bath tub was installed at Cincinnati, Ohio, by William Thompson, in 1842. Doctors discussed the dangers lurking in such bath tubs and their too frequent use. Philadelphia and Boston passed ordinances prohibiting the installing of bath tubs.

When Buchanan was President the visitors to the White House asked permission to "see the bath tub."

Today, in this country, over 1,000,000 bath tubs are sold annually.

This is another proof that the opposition to any new departure is not founded upon scientific principles. No matter what subject is brought to the attention of the Public, there will always be controversies—**difference in opinions is what keeps the world going.**

PERSONAL NEWS FROM ABROAD, DRS. A. J. AND C. B. MOLYNEUX

Dr. Albert J. Molyneux and Dr. Cora Belle Molyneux, of 2859 Boulevard, Jersey City, N. J., and "Camp Osteopathy," Lake Hopatcong, N. J., have returned from a nine-weeks tour of Canada, England, Scotland, Ireland, Germany and France. They attended the American Osteopathic Convention at Toronto and the British Osteopathic Association convention at London.

They visited Dr. W. E. Boyd's laboratory in Glasgow, Scotland, and found Dr. Boyd very courteous. He generously showed them the test rooms and "Boyd Emanometer," outlining how the "Electronic Reactions of Abrams" tests were made for the "Sir Thomas Horder Committee."

The doctors stated that this valuable and interesting research work is to be continued and the findings of these British scientists in conjunction with the research going on here in America and other parts of the world is convincing proof that E. R. A. is fast coming into its own. The reading by the British committee of Dr. Caves' classical analysis of their report will, I am sure, stimulate them in their further work and serve as an invaluable guide for future phases of investigation. To Doctor Boyd we owe a debt of gratitude for the excellent work done in establishing the Electronic principle and also to Dr. Cave and other American researchers. Dr. Cave's masterly Commentary will, I am sure, do much toward bringing about closer co-operation between the European and American E. R. A. researchers.

Dr. Boyd mentioned the following new E. R. A. publications, published by John Bale and Son, England, which are of interest to Electronists: "The Boyd Emanometer Research and the Related Physical Phenomena," "Abrams' Methods of Diagnosis and Treatment," (Foreword by Sir James Barr, C. B. E. D. L. F. R. S. E. Electronic Medicine in the August 5th number of "Truth."

The ancient mineral hot spring baths at Weisbadden, Germany, were especially interesting. They are world famous for their healing powers and are visited yearly by thousands from all over the world. They were known and made use of by the Romans and stress the value of proper elimination for the maintenance and restoration of health.

A DEPOLARIZER USED ON DIRECT CURRENT

Several attempts to construct a depolarizer for use with direct current has been made, but without success. Heretofore a motor converter has been too expensive for the average office and the Doctor had to be contented with the horse shoe magnet or use no magnetic instrument. The depolarizer plays a very important part in successful treating. We now offer a motor converter which will operate satisfactorily. This converter is made for use on 110 or 220 volt D. C., but is not of the universal type. Always give the voltage of your direct current supply, otherwise 110-volt generator will be shipped. Delivery within 10 days after receipt of order.

110-volt Direct Current Converter	$45.00
220-volt Direct Current Converter	55.00

NEW DIAGNOSTIC SET

Some Doctors have expressed a desire for the New Diagnostic Set without the heating device and minus the Dynamizer. This set may be used with the dynamizer you have without the heater or by having a dozen -inch holes bored through bottom of your dynamizer you may use heater.

Diagnostic Set Complete	$150.00
Without Dynamizer	125.00
Without Electrodes deduct from above	5.00

Guaranteed Accuracy less than 1-10 of 1%.

APPARATUS FOR TREATMENT AND DIAGNOSIS

New Style Depolarizer, see March, 1925, Journal,	$22.50
Oscilloclastophone, Detector of Oscilloclast Vibrations	10.00
Reflex Sets, three pieces, $2.00 each, or set	5.00
New Reflexophone, 1111.11 ohms	45.00
Reflexophone ,61 ohms	30.00
Micro-Oscilloclast, 111 ohms	30.00

Chicago A.E.R.A. Convention

REDUCED RATES! IMPORTANT!

The American Electronic Research Association, The American Association for Medico-Physical Reserach, and the American Association of Orificial Surgeons, will all hold Conventions in Chicago about the same time. The Convention of the A. E. R. A. will be held at the Congress Hotel from September 20th to 23rd.

I am glad to convey the news to you that the Passenger Associations have agreed to give reduced rates to the above three Associations, counting them as a unit for this purpose. Tickets may be purchased from September 17th to 23rd, inclusive. They can be validated on dates from September 22nd to 26th, inclusive, and the validated certificate is good for the purchase of a return ticket up to and including September 30th. The certificates must be countersigned by Dr. J. M. Kilborne of Sioux City, Iowa, who is a director of the A. E. R. A., and a member of the other Associations. His transportation desk will be in the Auditorium Hotel which is just across the street from the Congress Hotel.

This arrangement absolutely insures reduced rates, as we are only required to have 250 certificates.

In view of the above, every electronist should be present at our Convention in Chicago. **Let us all go.**

Be sure to ask for a Certificate when buying your ticket to the Convention.

NORMAN T. JOHNSON, Sec'y.

Kearney, Nebr., July 22, 1925.

ERROR—On some pamphlets sent out the date for purchase of tickets was September 22nd to 26th. This was an error. Note that tickets may be purchased from September 17th to 23rd, inclusive.