

**SORSI**  
SACRO OCCIPITAL RESEARCH SOCIETY INTERNATIONAL

# History of Sacro Occipital Technic

**Dr. Major B. De Jarnette**

History of  
Sacro Occipital Technic



# **HISTORY OF SOT®**

**By**  
**Major Bertrand De Jarnette, D.C.**

Published by:  
Sacro Occipital Research Society International, Inc. (SORSI)

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Major Bertrand De Jarnette, D.C.

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Dr. Major Bertrand De Jarnette

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## FORWARD

Many great minds in the history of science were unknown, if not ridiculed, by their contemporaries. Galileo was executed for his scientific discoveries. Joseph Lister was publicly embarrassed by the medical profession for his belief in antiseptic surgery. Anton Messmer never received the credit due him during his lifetime, and Albert Einstein's brilliant theories are still trying to be proven wrong. Possibly the greatest scientific investigator of the functioning of the human body of our time, Dr. Major Bertrand De Jarnette, is today largely unheard of except for a small circle of students and followers.

The History of SOT by Dr. M. B. De Jarnette is a brief glimpse into the years of dedicated research performed by this amazing investigator. A great part of the detail of his endeavors has been omitted by De Jarnette. Many countless hours are not described. At times, Dr. De Jarnette would spend days on end at his office with little food or sleep investigating an idea. The results of his efforts are scientific descriptions of HOW the phenomenon of chiropractic actually works. This is but one of the 138 books published by Dr. De Jarnette. For a thorough insight into the total investigative process, the ardent student would need to absorb each one of these documents individually.

Some of Dr. De Jarnette's theories include the Category system, occipital fiber and trapezius fiber theories and treatment, as well as the cranial sutural treatment process and the Category I specific cranial procedures in the field of craniopathy. In order to investigate all of his concepts, he utilized the most sophisticated medical instrumentation of the day as well as resources in universities and medical schools. As a pioneer in the early days of color photography, his impressive financial resources were turned to the study of the scientific basis of chiropractic, to which he attributed the saving of his life after a critical injury. As the story goes, one of his investigators was Arthur Guyton, who pioneered the Golgi tendon organ investigations for De Jarnette while developing his theory of the occipital fibers.

Unlike Arthur Guyton, Dr. De Jarnette's theories are constantly criticized in the chiropractic field for their lack of "scientific proof." No one asks Dr. Guyton for the research data requiring him to prove the existence of the Golgi tendon organ. Everyone asks for the "proof" of the existence of De Jarnette's occipital fibers. As were Dr. Guyton's original notes and manuscripts long forgotten by now, so are De Jarnette's. His numerous books and even more numerous pamphlets and articles are the remaining evidence of all of his findings. The ultimate proof is in that of the individual investigations of each and every practitioner.

De Jarnette's lifetime of dedicated research is an impressive example of true commitment. Commitment earmarks the moment in time that changes the whole world for an individual. In a stream of occurrences emanating from that deep inner choice unfolds a life path of greatness. Goethe once said in one of his couplets: "Whatever you can do, or dream you can, begin it. Boldness has genius, power and magic in it."

To quote from the Major's rules of life:

1. Do more than touch---feel.
2. Do more than exist---live.
3. Do more than look---observe.
4. Do more than read---absorb.
5. Do more than hear---listen.
6. Do more than think---ponder.
7. Do more than talk---say something!

De Jarnette took these rules of life and applied them to all of his empirical research. Methodical and meticulous in his scientific investigations, he unraveled mysteries of functional human neurology and the science of chiropractic. Many of his concepts, theories and discoveries are mostly unheard of, even to the majority of the largest body of natural healers in the Western world. His passion for demonstrable truth in chiropractic gave us a powerful system of analysis and treatment of the human condition. It also gave De Jarnette insights into the relationship between the doctor and the patient. Hence, he tells us "do more than simply touch.....feel."

His burning passion for truth led De Jarnette to be consumed by his research. Likewise, the successful physician must be consumed with the treatment process when engaged in interaction with the patient. Therefore, the master physician must also be a master at life. It takes time and dedicated efforts that will entail a lifetime of commitment. The accounts of one man's journey into the truth and understanding of the human body may well serve as a source of inspiration to the future physicians of our world. The History of SOT serves as an example of scie4 | P a g e ntific dedication and commitment to all open and thinking minds. Let these accounts serve as an inspiration for your enhancement of life as well as your studies of health and disease.

Joseph F. Unger, Jr., DC, FICS  
St. Louis, Missouri  
August 27, 1992

### **Addition to the 2010 Edition:**

We are witness to many changes in healthcare. The world at large is getting sicker, and the answers to good health and healing are becoming more and more complicated. In the midst of confusion and chaos any steady light begins to shine more brightly. The research and teaching of Dr. Major Bertrand De Jarnette provide one such deep.

Dr. De Jarnette founded SORSI, and in turn, her legitimate sister organizations to continue his legacy. However, there are those within and without of the profession who seek to take SOT® and SOT® Chiropractic Craniopathy as their own. SOT is truly a chiropractic discipline. The sanctity of this exclusivity is paramount for many reasons.

SORSI represents the direct lineage of De Jarnette, all of SOT, his research and teaching. He designed this relationship for reasons that all may not understand. Other professions want their practitioners to practice SOT. The art science and philosophy of SOT is a chiropractic discipline and requires the appropriate training and education of the licensed chiropractic physician. Without it, the profession as well as the patients suffer.

Practitioners of other than medical doctors may wish to perform surgeries or dispense pharmaceuticals. It is not fair or wise for untrained and uncertified persons to do so. Medicine has its well deserved domain in the health care system.

M. B. De Jarnette DC was a genius, entrepreneur, author of 135+ books, holder of numerous patents and a man totally dedicated to the service of humanity. He was also an engineer, precise and accurate with every deduction conclusion and word that he used. He founded research and developed the Sacro Occipital Technic of Chiropractic and Chiropractic Craniopathy, as well as many hundreds of chiropractic treatment techniques. In this matter De Jarnette made a very specific and important distinction.

### **Technic Versus Technique:**

Throughout Dr. De Jarnette's materials as available through SORSI, you will note the terms technique and technic utilized. "The Major", as we came to know him, never did or said anything without a specific and valid reason. In reference to this subject, Ned Heese DC, FICS provides the following quote:

March 1983 SOTO Bulletin

NOTICE TO ALL WHOM IT MAY CONCERN

SACRO OCCIPITAL TECHNIC IS JUST THAT. It is not Sacro Occipital Technique. SACRO OCCIPITAL TECHNIC is a coined and copyrighted series of words referring to this special chiropractic procedure. There has never been a manual published covering S.O.T. that has been spelled other than Sacro Occipital Technic. There has never been a bulletin published with any other spelling. It is an alternative spelling - it is not misspelled. It is on all our stationery. All copyrights are spelled TECHNIC when applied to SACRO OCCIPITAL and everything put forth from this office is spelled this way. Remember, T E C H N I C. In reference to cranial, extremity and manipulative reflex, etc., we do use Technique.

Further insights and clarifications are provided by Dave Beltakis DC.

“As I had an engineering background when I started learning SOT, the word 'technic' was a more accurate term than 'technique'. Even though they are often defined the same, technic has a professional connotation. It is a term that a PE (Professional Engineer) would use. For example: Billy Bob (down at the hardware store) would share his technique, an engineer would give you a technic. The difference? A technique may work for an individual a technic works for everyone.

As simplistic as this may sound De Jarnette's use of that term signifies a very logical, methodical and technical person.”

tech·nic n. 1. technics (used with a sing. or pl. verb) The theory, principles, or study of an art or a process.

2. technics (used with a pl. verb) Technical details, rules, or methods.

3. Variant of technique. adj. Technical. [From Greek tekhnikos, of art, from tekhn, art; see technical.]

Another definition : Of or pertaining to an art. Now rare.

1. Technical details or methods collectively; the technical side of a subject, esp. the formal or mechanical part of an art.

2. The science or study of an art or arts, esp the mechanical arts or applied sciences; technology.



As one can conclude from the above discussions, The Major had very good reasons for distinguishing his SOT™ Chiropractic and SOT™ Chiropractic Craniopathy as Technics. All of his procedures such as CMRT, category procedures, specific cranial adjustments, extremity procedures etc. are, more accurately, techniques. M.B. De Jarnette, DC founded SORSI to carry on his studies and teachings. SORSI and its authorized international organizations: SOTO Japan, SOTO-Europe, SOTO Australasia and the developing SOTO South America and SOTO-Brazil continue to utilize these distinctions as per The Major's directives.

Joseph F Unger Jr DC, FICS  
St. Louis, Missouri  
22 June 2010

# THE HISTORY OF SACRO OCCIPITAL TECHNIC

## INTRODUCTION

My experience of feeling incompetent when I first opened my office was not totally the reason why I decided to go into research early in my chiropractic career. I have always been inquisitive. I made model aeroplanes even before I ever saw an aeroplane. I made a pedal operated automobile before I ever rode in a real automobile. It does not matter whether I had gone into medicine or chiropractic or some branch of mechanics. I still would have been inquisitive and would have tried to improve whatever I was working with.

My chiropractic college did give me a good education in chiropractic. They lacked many things with which to work--mainly money--but with what they had they did a mighty fine job. Chiropractic as a profession did not fail me. My only excuse for entering chiropractic research was a desire to see if I could not add to what we then had.

I never aspired to teach. I did not go into research as a method of making a living. During the early years of my practice, I had as large a practice as any chiropractor in the state. I used to spend about 15 hours a day in my office.

I did not go into research so that I could become a leader in my profession. I have no desire for the obligations leadership entails. Chiropractic to me was a vast challenge. I knew that a profession so young as was chiropractic in 1925 must have a vast ocean of unexplored territory, and I set about exploring.

During the 33 years that I have been in chiropractic research, I have also been a practitioner of chiropractic. I have never aspired to being the owner of the world's largest clinic, but I have for a great many years been a refuge or many upon whom chiropractic had failed. If my research has done nothing further than to make about 4,000 chiropractors THINK, then it has been worthwhile.

I owe a vast and unpayable debt of gratitude to my wife, Todde. Todde was the one person who kept telling me....TRY AGAIN. IT MAY WORK THIS TIME. I owe a debt to

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chiropractic that I can never repay. It saved my life and gave me the wisdom to save my daughter's life when at six years of age she was stricken with Cerebral Spinal Meningitis. Had I not studied chiropractic, I would not have had the privilege of 33 years' research in the development of chiropractic. I would not have met the many thousands of fine chiropractors that I have met, many of whom I know personally.

## THE FOUNDING OF SACRO OCCIPITAL TECHNIC

Sacro Occipital Technic was founded in 1925. This technic is now 33 years old. SOT is not another technic within the body of chiropractic; rather, it is a method of chiropractic and, as such, is a full science.

This method of chiropractic was born through necessity. In 1924 chiropractic was pretty much a system of popping backs, and the louder you could make them pop, the better chiropractor you were judged to be. Chiropractic was based upon a multiple system of moves for repositioning vertebrae, yet seldom would two chiropractors agree on which vertebrae were subluxated and in what direction, so our moves were by no means specific.

In the early years of my research, Dr. Willard Carver was the one man I studied and admired. Dr. Carver seemed to have a knack for putting all of man into the picture and then explaining what was wrong in terms of classified distortions.

In 1924 one of my classmates suffered from a very severe heart condition. Adjustments offered no hope in that his heart condition grew steadily worse, and it became apparent that he would have to discontinue his chiropractic education. I was called to his home to attend him as a senior student. He had developed a very painful left brachial neuritis. I made every effort to adjust him as we had been taught, but even the slightest movement would greatly increase his pain. Finally in desperation, I secured a pan of very hot water and a heavy Turkish towel. The hot towel was applied to his shoulder and upper dorsal spine. After about three minutes application, my colleague fainted, and in my effort to bring him back to the land of the living, I dashed a pitcher of very cold water on his face and back. Within a matter of seconds he revived and, surprising as it may seem, much of his shoulder pain had disappeared. Two very young chiropractors were now faced with a dilemma. Palpation verified that no vertebral change had occurred during the blackout. Not having a stethoscope, I could not ascertain whether or not the heart had improved in function. I visited this chiropractor daily for five days, and each day I would sit him in the bathtub, take a bucket of very cold water and let him have it over the shoulders. The sixth day this

boy was back in college and to this day, some 36 years later, has no demonstrative heart lesion. I could not have been more mystified had I seen ten ghosts. The college instructors were never told what I did with the water bucket, and for years they would use this student as an example of chiropractic adjusting success.

During the next few years I became obsessed with the idea that what we did to the spine in adjusting did not produce results because we moved vertebrae and removed nerve pressure, but because we either applied stimuli or inhibition. Abrams came upon the scene about this time with his spondylotherapy, so instead of throwing a bucket of cold water onto my patients, I tried pounding the spinouses. I could never recognize one ounce of results by the use of spondylotherapy. About 1925 while browsing around an old bookstore, I found a copy of LANDOIS ON PHYSIOLOGY. This very ancient writer seemed to be talking about the things I had been thinking about. Physiology started to play a greater part in my thinking than did anatomy or pathology, with the result that I started to study the effects of abnormal physiological process as related to skin reactions. I would draw my fingers down a patient's spine, wait a few minutes and observe the different color changes that occurred. I found that some spinal areas became very red with only minute pressure, while other areas would become absolutely white. Within a few months, it occurred to me that such changes ought to be reflected in the blood pressure, so I then started a long series of experiments to determine how pressures along the spine would affect the blood pressure and, inversely, affect the areas innervated. About 1926 I developed the hypothesis that when the muscles of the spinal column showed very red areas due to pressure, those muscles would whiten, I assumed that they contained blood vessels in a state of constriction. In 1928 I announced my system of VASOMOTOR CONTROL. We did no actual adjusting of the vertebrae; rather, by the use of this system of VASOMOTOR CONTROL, we would attempt to locate the area giving the greatest response, by becoming very red or very white, and applying cold to the red area and localized heat to the white area. We did produce some very spectacular results with our VASOMOTOR CONTROL TECHNIC, but it was not the type technic that would be used in the average chiropractic office as too long a time was required for its application. In those days, when a dollar was about all you received for an adjustment, you had to adjust as many patients per day as you could secure.

One of my old notebooks contains a paragraph written in April of 1928 which really opened up a wider field of research. This patient complained of a very painful left ilium and leg. In making the vasomotor test, I located an area over the third left sacral foramina that reacted violently, and in so doing all pain disappeared from the painful left ilium and leg, but instantly changed its locale to his left occipital bone. This was the most astounding thing I had ever seen as a chiropractor, and I must say that the man was not a bit grateful for his pelvis and leg being freed of pain. In an effort to help relieve the pain in his head, which I

had apparently produced by some reflex from the sacrum, I started to feel of his occipital bone and to my amazement I ran my fingers over a fiber that seemed as large as a cigar and as painful as a hip fracture. Not knowing what the fiber was or what to do about it, I did what all of you would do, I PRESSED UPON IT AS HARD AS I COULD. In a few minutes, the man's occipital pain disappeared, and he left my office in high spirits. Fortunately for me, I kept a very accurate record of all my adjustments and still do. In ten days my patient returned and this time complained that for the past three days he had suffered from a splitting headache. Knowing what I had previously used, I proceeded to repeat my application of cold to the left sacral area, and as amazing as it seems, within minutes the headache was gone. I saw this man some 27 times during the next three years, and although he died due to a prostratic carcinoma, up until the last week of his life I could still control his pain with a cold application to his left sacral three area. Even when hospitalized, he begged the medical doctors to let me treat him.

I had come to the conclusion that some manner of reflexes existed between the sacrum and the occiput. What those reflexes were, whether muscular, visceral nervous or intra-neural, I did not know, but a light was shining and knowledge was in the near future. It was very apparent that most distortions centered into the sacrum, and most types of pain produced painful fibers upon the occipital bone. There had to be a connection, and I meant to find it. Had I known the roughness of the road that lay ahead, the cost of this research and its many hundreds of heartbreaks, I believe I would have given up in 1928.

Early in 1929 I came across a small book by a German author named Bing. Bing had done a lot of original research on the tracts within the spinal cord, and from the book I derived inspiration to see what I could do to prove the relationship between sacrum and occiput. During 1929 I worked with stimulating electrical currents, using those currents to develop muscle movements and also to try to see the end results of those movements. It soon became apparent that two primary tracts existed within the spinal cord. One would receive impulses; move them inferior through the hypo gastric plexuses, then superior to the brain. The other would move all stimuli impulses superior without bothering the sacrum. I found that impulses entering the cord inferior of dorsal nine would involve the hypo gastric plexuses and the sacrum. Impulses entering the cord superior of the ninth dorsal went directly superior. I discovered that a pain involving neurons inferior of the ninth dorsal could be controlled by certain changes of sacral position, and impulses entering superior of the ninth dorsal could be altered by altering the position of the occipital condyles in relationship to those of the atlas. I named this new approach to chiropractic SACRO OCCIPITAL TECHNIC, and my system of chiropractic still bears that name. Since 1929 all of my research has been produced through the SACRO OCCIPITAL RESEARCH SOCIETY, INTERNATIONAL.

Now knowing that the sacrum had a direct relationship to pain control, and realizing that the best position to view the sacrum was upon the standing patient, early in 1930 I constructed a foot plate and wall arms by which I could use a permanent plumb string. That was the start of my DISTORTION TECHNIC. When visual observation would show a sacrum out of central alignment, I would reason its mechanical position, then check the muscles and ligaments responsible for that abnormal position, and then try manually to replace the sacrum. This was one of the most hectic periods of my total research. A patient with a bursting headache could not understand why I was looking at the wrong end.

Starting in 1931, I accepted only patients who had failed to secure results through chiropractic adjustments. From 1931 to 1940 I never accepted even one patient for my services who had never been adjusted. I had to prove that what I was trying to do was different from anything any other chiropractor was doing. That shall go down in my notebook as the long years of poverty. Most of my acceptable patients had no money. Most of them had no hope of ever getting well, yet out of that mess I rescued patient after patient; and even to this day, I still care for many of those patients and their families.

I read a book in 1930 titled THE HUMAN MOTOR. I have forgotten its author and do not even know where the book is to his day. The author, through collaboration with a man named FISHER, had used many hundreds of cadavers in an effort to locate the human's center of gravity. They used a horizontal board over a pointed support and, by shifting the cadaver back and forth, finally estimated that the center of gravity existed just lateral of the third sacral foramina. That gave me an idea. If a dead human had a horizontal center of gravity, a living human must have a vertical center of gravity. I experimented with every device I could construct, and finally it dawned upon me that I had two index fingers and why not use them. After about 4000 failures, I finally located the spot, and by merely touching that spot with my index finger I could actually balance the patient. I could straighten his spine or bend it. This finally became known as the MASTER TEST AND WAS ILLUSTRATED IN MY SPINAL DISTORTIONS BOOK PUBLISHED IN 1934. About all the master test ever did was to prove that every human did have a balance point and, if properly contacted, you could move his body in any direction irrespective of his weight. About all it did for my students was to scare them when a patient would spin out of control. It really did have some importance, as it finally developed into what we now term the I.B.A. TECHNIC.

During 1931 I became seriously engaged in trying to determine what caused so many people to complain of painful areas on their skulls. It would usually terminate by finding excessively painful fibers on the occipital bone. Fate seems to work with the inquisitive, for had no one ever complained that no one ever did anything about the painful areas on the patients' heads, I probably would never have investigated the occipital bone.



I studied the occipital and other cranial bones from every anatomical standpoint, for that is about the only manner in which they can be studied, but anatomy is anatomy, and every doctor is well-versed in that subject, so I decided that something existed on the occiput that no one had ever discussed. The occipital fiber became an obsession with me. One day during 1930 I happened to run across a set of notes I made while attending Dr. Carl Hawkins' class back in 1924. Dr. Hawkins felt that the occipital condyle subluxation held the key to many idiopathic disorders. I had made a drawing of a cardboard model of the occipital-atlantal condyles which Dr. Hawkins used in his lectures, so I proceeded to make up a model for my own use. Dr. Hawkins taught us to palpate the occipital-atlantal condyles and then to make an adjustment to correct that condyle misalignment. I noticed that in palpating the occipital condyles that I would consistently find painful fibers on other areas of the occipital bone, and that the adjustment Dr. Hawkins taught us would not remove the tenderness from those additional fibers. I tried manipulating the fibers, but no direct results were forthcoming, except in some instances when I would manipulate an occipital fiber, I would produce pain in some part of the spine, torso or extremities.

I found many different types of fibers on the occipital bone. Some were short and fat, others long and lean; some very painful, others moderately painful and still others non-painful. Time passed and I still pondered the occipital bone. Patients who were acutely sick showed acutely painful fibers on the occiput. Patients who had recovered from some self-limiting disease showed only moderately painful occipital fibers. Patients who had been sick all their lives would usually show very hard and non-painful fibers. Patients with acute spinal pains would show an occipital fiber that was different from the patient who complained of a visceral pain. Patients with high blood pressure had occipital fibers different from patients with low blood pressure. Patients with a consistently normal blood pressure, even under varying degrees of pain and emotional upsets, would show few, if any, really painful occipital fibers.

During 1930 I started checking the occipital fibers before every adjustment. Some adjustments seemed to relieve the tension and pain of the occipital fiber; others would increase it. I did no adjusting whatsoever at this time onto the occipital condyles. I found that in very low blood pressure accompanied by a specific area of spinal hyperemia that an application of cold to the spinal area did have a tendency to remove tension from a specific occipital fiber, but other fibers would remain unaffected. I likewise found that a specific area of spinal anemia, when treated with localized moist heat, would also affect a specific occipital fiber. I found that if I applied heat to a hyperemic spinal area that a specific occipital fiber would increase in both tension and pain, and the reverse held true in applying cold to an anemic spinal area.

About the middle of 1930, I noticed that in palpating the occipital bone that an order existed. In other words, there seemed to be an equal number of fibers on each side of the occipital bone providing the palpation was done on the superior part of the occiput, but that fiber order disappeared when the lower part of the occipital bone was palpated. This offered me my greatest challenge. I searched, but could find no mention of occipital fibers in relationship to disease in any written treatise. I wrote letters to three chiropractors explaining my dilemma and asked them to check. Only one responded and offered very little help. About this time Dr. Wigelsworth came out with his PATHONEUROMETER. I bought one of the instruments and used it to check the occiput as well as the spine and pelvis. I used it consistently for about six months, but it offered very little help other than picking up areas of spinal pain which I had already found by palpation. I then started x-raying every patient's occipital-atlantal area. X-raying in those days was pretty much a hit-and-miss proposition, but I did gather some interesting facts. For instance, when the occipital bone was inferior on one side, the leg would always be short on that same side. By tractioning the inferior occipital side, that leg would lengthen. It would not remain lengthened, but at least I had a clue. Late in 1930 I seriously attacked the problem of vertebral reflexes in relationship to occipital fibers. I would select a specific vertebra, do exactly opposite of the indicated adjustment and then repalpate the occiput to see what happened. I found that a certain occipital fiber would always tense up, then when I would do the indicated adjustment to that insulted vertebra, in time the occipital fiber would relax. This was a slow process, because I would often have to wait days before anything would happen. I finally discovered that when I insulted an upper dorsal that a lateral occipital fiber would respond. When I insulted a lower lumbar or the sacrum, a medial occipital fiber would respond. I empirically selected dorsal one as my starting point, because I still was fighting the theory of the VASOMOTORS, and I knew that the vasomotors started at dorsal one. When I would insult dorsal one, a fiber would develop next to the mastoid, but I never knew whether it would be on the right or left side of the skull. By the end of 1930, I knew that there was a very specific reflex field between the vertebrae and the occipital bone. I knew approximately the road traveled by those reflexes, and my first INTERCOMMUNICATING CHART was prepared by the end of 1930. In 1931 I published my first book describing this intercommunicating system of reflexes.

Progress had been made, but had I known then how much farther I would have to go, I honestly believe I would have thrown in the sponge. During 1931 I started my program of very serious research. I accepted only patients who had been chiropractic failures. I would chart each patient's occiput. I would test each patient's dorsal and lumbar vertebrae for blood pressure reactions. I would check each patient's occiput for inferiority and chart the short leg. Then I started what I believed was a course of corrections. On a selected group, I used either heat or cold to the specific spinal area. Then I would traction-adjust the occiput to correct the short leg; then, day by day, I would repalpate the occiput to see what was



happening to that one important fiber. In some instances the fiber disappeared as if by magic, and so did the patient's symptoms. In others the fiber got angrier and angrier, and so did the patient and his symptoms. I had to fight like mad to keep those patients coming back to me. The other group received either a stimulating or an inhibitory adjustment to the specific vertebra, and again I observed and compared. In the summation, I did more with the adjustment than with the heat and cold, but I did not give up on the theory of constriction and dilation and still used heat and cold through 1940.

During 1931 through 1932, I treated some 400 patients. It was not until 1949 that I finally developed the three occipital liens and even later than 1949 before I was able to classify those lines as we know them today. Between 1930 and 1958 I digressed from the occiput many times in the development of other phases of Sacro Occipital Technic.

About 1932 I started a process of pain localization. I would take an area of known pain and try to chart its pathways through the spine to the thalamus. I am getting ahead of my story, but when one thinks back some 28 years, memory becomes hazy. In 1931, I commenced x-raying chests, abdomens, spines and pelvises. I observed that many patients had inequality of the diaphragm. Those same patients always exhibited rib tenderness, especially along the sub-costal lines. Observing also that many of those patients showed heavy concentrations of gas in the stomach, intestines and colon, I used to have these patients drink some type of soda solution; then I would x-ray them to see if the gas had been eliminated. I found that these patients would always show tenderness over the right transverse of the dorsal five, both transverses of dorsal ten and lumbar two. I started using pressure over those three vertebrae and noted that many diaphragms would equalize immediately. I then remembered what my physiology instructor had taught us about CO<sub>2</sub> and oxygen transfer in the lungs and through the blood vessels. I bought an oxygen container and a CO<sub>2</sub> container and started experimenting with oxygen and CO<sub>2</sub>. Those patients with unequal diaphragms always withstood CO<sub>2</sub> badly but used oxygen nicely. I reversed this process and by using collecting bags would have a patient breathe into the bag and measure their CO<sub>2</sub> output. I then discovered that my 5-10-2 technic always produced an increased output of CO<sub>2</sub> and permitted a great intake of oxygen. This CO<sub>2</sub> technic, as I named it, would more quickly level the diaphragm than anything I had previously used and, to this day, some of my old-timers still use the CO<sub>2</sub> technic, and all should use it. I found that every patient with a fever always felt better following the CO<sub>2</sub> technic. Every heart patient responded better following the CO<sub>2</sub> technic. I then made a phenomenal discovery. Some patient's blood pressures would drop severely following the CO<sub>2</sub> technic; others would show a rise in blood pressure. That necessitated the acquisition of instruments for heart study. I acquired about every instrument then available and spent two years learning how to operate them. I ended that phase of my research by proving that a weakened heart muscle always lowered the blood pressure when excessive CO<sub>2</sub>

accumulated in the blood stream. The strong myocardium always raised the blood pressure under the same circumstances.

Now back to the pain localization. When I would succeed in localizing a posterior area of pain in one specific area, I would then check the known occipital area that was associated with that particular spinal segment and then try to encourage that occipital area to control the localized pain. I finally developed a method of adjusting the occipital condyles for this pain control, making the adjustment only when the occiput would control that pain. When the occiput would not control the localized posterior pain, I would study the involved segment and try to fit an adjustive technic to it.

I then went from posterior localization to anterior localization. The anterior localization was a mess. Often after an hour's effort and many discouraging remarks from the patients, I would finally find an anterior area that would control the anterior areas of pain. Fortunately, I charted all those anterior areas, and they are now our skeletal reflex points. The anterior control would then be tested against the posterior control, and whichever came out on top was then tested to the occiput. I never developed a conclusive system in this pain localization, but I did develop reflex points that are of great value today.

By 1933 I thought I had the occipital-spinal reflexes pretty well under control. We were seeing patients daily that had failed miserably to respond to other types of chiropractic adjusting, and by use of our CO<sub>2</sub> and occipital-spinal technic, those patients often made spectacular recoveries. I was still measuring legs and adjusting the occiput to correct those legs. I divided patients into three groups--the acute, the subacute and chronic--and developed a technic for each. A doctor came into our Chicago Convention last year who met me for the first time in 1933 and studied SOT that year. He told me that he was still using the technic I taught him in 1933, and it was still better than anything any other person had to offer.

During 1932 I started looking at patients' backs while the patient stood. I looked at patients who had been adjusted from 10 to 100 times by other chiropractors. The more some of the patients had been adjusted, the more crooked became their spines. This did not make sense, and I thought something should be done about it. Some patients with straight spines had more pains than patients with crooked spines. That again did not make sense. The healthiest patient I examined in 1932 had a double rotary scoliosis that had shortened his height about eight inches. The sickest patient I saw in 1932 had the straightest spine. All that I had been taught was disproved in less than three months, so I thought it about time to start a study of SPINAL DISTORTIONS. First, I made a footplate, then took two pieces of metal, fastened them to the wall and suspended a plumb string from the top bar to the bottom bar and centered that string to the central back part of the footplate. I at least could

now stand a patient on the footplate and see what his spine should look like. Some patients would have the central gluteal line to the right; others to the left. Some pelvises were so rotated that the patient's buttocks actually stood side-wise to their foot. I knew if all was anatomically normal, that the gluteal center should center to the plumb line. That is where I started the MASTER TEST previously discussed. I would take my contact, twist my finger, then observe. Sometimes the patient would go to the right and sometimes to the left. Sometimes they would even take a spill. I finally put side bars on the instrument so the patient could grab a bar if he felt himself falling.

My first effort at correcting SPINAL DISTORTIONS again involved the occiput. I would have a look...then have the patient lie supine...measure the legs...traction the occiput on the short leg side, then stand them up and have another look. Sometimes they improved...sometimes they were worse. I must have looked at 3,000 patients before I finally classified distortions into a similarity of what we have today in SOT.

I finally observed that when the plumb string passed over the left buttock, the right buttock would be anterior, and vice versa. I again used the master test to see what it would do to correct the pelvic and spinal twists, and this time I had better fortune, for I finally developed a contact that, when taken on a known distortion side and the finger turned in a known direction, caused the distortion to return to normal. I would then have the patient lie prone, take that exact known contact, hold it, and then with the other hand feel for spinal muscle contractions. I would hold those contractions, then by turning my buttock-contacting finger, feel when the contraction of the muscle let loose. In some instances the buttock contact would perform a miracle; in others, it would excite the pain and increase the distortion. I worked all through 1932 and part of 1933 on this SPINAL DISTORTION idea, and during that year wrote and published REFLEX PAIN. Reflex Pain was placed on the market in 1934. Immediately following the publication of Reflex Pain, I again went back to the occiput for further study, all the time making SPINAL ANALYSIS BY USE OF THE DISTORTION ANALYZER. I had asked for patents on the Distortion Analyzer, and it was placed on the market. The first analyzers were sold for \$7.50 each. Dr. Randolph Stone bought the first two analyzers sold...one for himself and one for Dr. Bluth. By the way, both are still studying SOT and, I hope, using it.

During 1933 and 1934, I worked to develop the occipital-spinal reflexes and also started developing technic for correction of pelvic distortions. I brought out the theory during this time that the adult pelvis was normally a unit of immovable bones, and only under abnormal conditions did the innominates and sacrum become movable, and when they were movable produced pain and distortions. That theory holds pretty much to this day. We know that, abnormally, the pelvic parts become movable. Our adjustment seeks to limit that movement and restore order.

During this time, instead of using the occiput to control pain, I started using it to find pain. Instead of looking to the occiput as something producing diseases, I started to study it as something which might indicate disease and even be a part in disease and pain control.

Instead of trying to bring reflexes to the occiput, I started using the occiput as an indicator of the area in trouble. That was my one great step forward. It took me many years to arrive at that type thinking. Research is often steady progress backwards until you hit a stone wall; then you stop, turn around and try again.

I had now gotten to the point whereby occipital palpation started to tell a story. I had calibrated the occipital positions. I knew that the seven occipital fibers were in order, providing you palpated a certain line on the occiput.

During 1934 I cared for three cases of TB and about 11 cases of pneumonia. I had one case of TB that showed a very acute occipital two and a dorsal three. Of the 11 cases of pneumonia, 8 of them had an occipital two and a dorsal three. Of the other two cases of TB that did not show perfect occipital two fibers, one did show a very hard and long fiber on occipital two, and each had some association with dorsal three, but not conclusive. The three cases of pneumonia that did not show an occipital two, dorsal three did have a fiber at occipital two, but nothing at dorsal three. The one case of TB with a perfect occipital two, dorsal three hookup, recovered and is now healthy. The two cases of TB with inconclusive occipital two, dorsal threes, both succumbed to their disease. Of the three cases of pneumonia with inconclusive occipital two, dorsal three reflexes, one developed a lung abscess and later TB. One developed carcinoma of the colon and succumbed; the other never fully recovered and spent many months in a Veterans Hospital before succumbing. It now became apparent that the occipital fiber was a protector as well as an indicator. Time after time during 1934, '35 and '36, I saw apparently hopeless cases respond simply because they had perfect occipital-spinal hookups. I saw less sick people fail to respond because they had indefinite occipital-spinal hookups. I was now faced with one of my greatest research mysteries. How could a very inconspicuous fiber on the occiput be so important in prognosing the outcome of disease? Why did a patient with a good occipital fiber-spinal fiber hookup respond so well to adjustments? It took me many years to find an answer. I found the answer in nutrition, sanitation, bed rest, emotional correction and a host of non-adjustive, non-chiropractic application.

The soft tissue of the human body started to take on an even greater importance. Instance after instance was followed through surgery. A patient with a good occipital-spinal correlation, even when an organ had to be surgically removed, responded well. A lesser-ill

patient with a non-correlating occipital-spinal hookup would develop an ileus following surgery and succumb.

The motion of organs in their daily tasks now consumed much of my research time. I sought to understand why an organ could function in any position if healthy and surrounded by healthy tissues. I sought to understand why certain positions increased visceral pain and function. I must have done well over 1500 barium examinations in an effort to find my answers. I do not now how many surgical interventions I observed, nor how many autopsies I witnessed. The answers seemed to be in the mesentery. There was something outside the organ that made it sick. Some gastric ulcers would show a perfect occipital-spinal correlation; others with more severe or less severe symptoms, but not failing to develop the desired occipital-spinal correlation, would not respond. The answer had to be in MOTION or functional room. I palpated hundreds of abdomens and made hundreds of vaginal and rectal examinations. I looked into every orifice available. I examined so many abdomens under the fluoroscope that I burned my hands.

One thing always came to the foreground...every abdomen seemed to develop a different percussion sound. I thumped and listened, then finally one fine day I picked up a tuning fork, struck it on the heel of my shoe, placed the fork on a patient's abdomen and listened. Sounds were different even over a two-inch periphery. Finally I developed a probe to be placed on a stethoscope bell, and then I listened to localized areas as the tuning fork penetrated the tissues. What I heard amazed me. I would outline a circumscribed area, then manipulate it, then re-listen. Sometimes the normal returned; sometimes the abnormal remained. When the normal sound returned, the pain disappeared. This was a challenge.

By now I had developed an examination routine that took me over two hours to complete. I would palpate the occiput...localize the posterior and anterior pains...go over the painful soft tissue area with the tuning fork and phonendoscope. Then I would stand the patient on the footplate and make a visual analysis. I would mark all deviations, then apply the MASTER TEST. The patient would then lie prone, and I would give the master test to see what happened to the tight muscles. I would then turn the patient over...measure the legs, and traction-adjust the occiput, if indicated. Then, if I could localize to the occiput, I would classify the patient as acute, subacute or chronic and plan an occipital or spinal adjustment. Then came the CO2 test to determine heart function. I knew that all of this was important, yet I could handle but six patients per day. When I found a pelvis or spine that would not respond to the master test, I would x-ray, have a look, correct my line of drive for the master test, re-apply it, then re-x-ray to see what moved that did not move before. All the time I had to keep accurate and detailed reports. Many things were learned. Thousands of mistakes were made. When an adjustment would not work as I planned it for a patient, I



would have that adjustment used on my own person to see what would happen. I nearly ended my career many times.

During 1936, much of my time was spent studying spinal distortions. By this time I had accumulated about 400 patients with spinal distortions that had not responded to anyone's efforts. I built equipment to measure hip rotation...other equipment to measure sacral incline...still other equipment to accurately outline the lateral spinal position. I had three rooms full of experimental equipment. I had also developed the VASOMETER FOR MUSCLE STIMULATION STUDY. I had developed the VASOMETER TENSION METER for the study of blood pressure and its effects upon function. I developed a neon analyzer to study hyperemia and anemia of the spinal muscles. I did over 200 black light photographic analyses of spines during 1936. I made a frame in which I would suspend my patient, then by elevating one side of the occiput, I would make a study of its effect on the spine and pelvis. I made pelvis stabilizers so I could study spinal motion. I x-rayed over 250 patients during 1936, and that was a Herculean task with the equipment I had. I worked from 7:00 a.m. to midnight seven days a week. I did not make any money but accumulated lots of facts.

During 1936 I developed the HAMMER ADJUSTMENT, now termed as the 1 B.A. No. 2, and the gluteal superior-inferior adjustment now known as the 1 B.A. No. 3. The 1 B.A. was developed in 1936, but it is not the 1 B.A. we use today. I developed the 1 B posterior adjustment. During my spare time, I gathered material for my forthcoming book SPINAL DISTORTIONS. That book was published in 1937.

Following the publication of Spinal Distortions, I again turned to the soft tissue problem. I gathered all of my 1932-1933 notes into one truck load and started to sort them. I was looking for reflexes, and those notes contained the world's greatest volume on that subject. I felt that somehow there were vital areas upon the human body that would tell a story if I could but get an opening sentence started.

I knew that a great percentage of my patients had gone through surgery, so I started to explore their incisions for an answer to their remaining aches and pains. I tried to associate a scar on the abdomen with that stiff shoulder or back, or that sciatica. I still used all I had previously developed, but I was trying to eliminate that which was superfluous. I observed that many of my patients had a decided visceroptosis, so I set about finding ways to lift the viscera and keep them in place. I was approaching a technic later named CHIROPRACTIC BLOODLESS SURGERY. During this research I did develop certain manipulations that would favorably affect specific viscus. I did find a way to loosen adhesions. I did find a way to replace malpositioned kidneys and uteri that no other person had discovered. I found technic that would really reposition a malpositioned prostrate. I was writing my

book on THE TECHNIC AND PRACTICE OF BLOODLESS SURGERY, which I published in 1939. Bloodless surgery was too difficult for too many of my students. They simply could not master its skill. They wanted to use force instead of direction. During this time I also produced our system of OPTIC CHROMATIC ANESTHESIA. It was a premature invention, as the chiropractor was not ready for such an adventure, and several barely missed severe mishaps.

My big task was now to correlate my research. I had gotten myself into a pretty deep quagmire, and the thing was pretty sticky. I do not censure some of my students for going into retreat and never showing their faces again.

I had by now firmly decided that we would use the occiput to tell us where and how to adjust the spinal vertebrae. I knew that the occiput was still hiding many of its secrets, but I proposed to dig them out if time permitted. My obsession about now was the parasympathetic system. I had been working with it for some time and even had put some of it into my Bloodless Surgery book. I knew from experience that man does not operate by the exclusive use of just two nervous systems. I knew that there must be a booster system some place along the way.

Just to fill in space and to give my doctors specific things to do for sick people, I wrote HEMORRHOIDAL TECHNIC, PRESSURE TECHNIC, MAJORS AND MINORS AND RIB TECHNIC. They were quite simple procedures, but really effective in many instances.

Through observation I had learned that whenever a patient had a fifth lumbar subluxation, they also had an atlas subluxation. I knew by experience that if the spinous of the fifth lumbar rotated to the right, the posterior tubercle of the atlas would compensate to the left. This was a too consistent occurrence to be merely coincidental.

For the next year, I kept busy preparing material for my masterpiece; namely, SACRO OCCIPITAL TECHNIC OF SPINAL THERAPY, which I published in 1940. For three full months, Todde and I were on constant call, day and night, to read press proofs. I tried to put into that one book much that research had taught me, but I mistakenly assumed that most of my research was behind me; instead, it was ahead of me. I had not as yet walked through the door that would bring to my view the greatness of SACRO OCCIPITAL TECHNIC.

Starting in 1941 I approached a new line of research. I believed that my work on the occipital-spinal reflexes was completed. I felt that I had developed those reflexes to a point whereby we could use them to advise us where and how to adjust and, most of all, when not to adjust. If I could have looked from 1941 to 1946 and could have seen what I successfully did in furthering my knowledge of the influence of the occiput on health and

its help in disease, I would have felt rather embarrassed at believing that phase of my research had been completed.

Having published what I thought would be my best book; I decided to concentrate upon the lumbar spine and to teach my findings in conventions, rather than to place those findings in book form. I felt that through a year-to-year contract with a limited number of chiropractors, all of us could work together, but I found that too few were research-minded. It simply is not written into the records that chiropractors as a profession are research conscious. They would not keep records. All in all, I found only a handful of chiropractors that did make suggestions as to ways of improving the technic. I found a few others that decided teaching was the high road to fortune, so they pulled out and started their own little band of minute men.

During 1941 I concentrated upon the fifth lumbar. I knew that vertebra by vertebra must be studied if we would understand the whole. I knew that we had to see what each would do for all the others before we could understand the spine.

I undertook to do something that, perhaps, had never been done before in chiropractic research. I worked to produce techniques whereby I could sublaxate the fifth as I worked to develop techniques for the correction of fifth lumbar sublaxations. I knew that unless we could actually produce distortions by sublaxating the fifth lumbar, we would never know what a fifth lumbar distortion or distortions looked like. It may sound strange, but it is much more difficult to willfully sublaxate a vertebra than it is to supposedly correct it.

The problems encountered in studying the fifth lumbar were multiple. This vertebra occupies such a prominent place in the human support column, and its symptoms are as widely dispersed as were Osler's symptoms of syphilis, that to actually bring the fifth lumbar into specific focus required nearly two years of research.

I started by acquiring as many patients as possible who were free of lumbosacral stress symptoms. My first objective was to gather at least two dozen patients who could stand upright, flex forward and touch their fingertips to the floor without bending their knees, and could reverse this procedure and bend backwards without producing back or leg pain. I wanted their ages to run from 18 to 50 years. I examined over 200 people before I found 18 that could qualify. These 18 people knew that they were part of an experiment, and most of them cooperated very well.

I now studied each patient by use of the distortion analyzer for three complete examinations on succeeding dates. I would photograph the spine with a camera at a pre-focused distance. This was repeated for three consecutive days. Those films were then overlaid to



see if there was a noticeable difference in the day by day photographs. I had to discard two of those 18 because their spines were too variable. The 16 remaining stayed with me for nearly two years until I completed my experiments.

Each of the 16 persons now received the same type adjustment, and that adjustment was designed to place a strain on the fifth lumbar that would carry the spinous to the patient's right. this adjustment was repeated daily for four days. I started on the fifth day to again photograph each of the 16 patients' backs. One patient developed a severe reaction within ten minutes following his first adjustment, but within 20 minutes his discomfort subsided, so he was able to carry on with the experiment. Within ten days, all but two of the patients showed a decided muscular groove rotation to the right. Two patients developed a pain in their left hips on the seventh day. One complained of a stiff neck, but he had been playing ball, so that was not conclusive. Two patients complained of mid-dorsal pain. One patient complained of cystitis. One patient complained that he had a tight band around his middle. One patient developed a one-half inch shortening of his right leg. One patient developed a severe cold. Only two patients developed the same symptoms; i.e., right hip pain.

I was badly handicapped during this experiment in that I did not have x-ray equipment sufficiently heavy to do good lateral x-rays. I did do A-P x-rays on all 16 patients at least twice during the experiment.

One very important thing developed which was consistent. Each of the 16 patients developed palpatory pain over the left transverse of their atlases. None of the 16 had such pain prior to the first experimental adjustment.

Within four months following the last experimental adjustment to rotate the fifth lumbar spinous to the right, five of the 16 patients suffered acute low back failure. One was hospitalized for three weeks.

X-ray examination showed actual rotation in 11 of these patients...five showed no apparent change. Two of the group that suffered acute low back pain were among those showing no rotation from effects of the experimental adjustments.

The most noticeable occurrence was the manner in which this experimental fifth lumbar adjustment affected the pelvis. Prior to the first experimental adjustment, only one of the 16 patients showed any distortion of the pelvic girdle. Following the experimental adjustments, all but two of the 16 patients developed pelvic rotation. Strange as it may seem, not all developed a left pelvic rotation. Now this was exactly opposite to what we had anticipated. We assumed that if we moved the spinous of lumbar five to the right, the pelvis would eventually compensate by moving the right innominate anterior and the left

posterior. This did not happen, so now a problem arose. How much does the pelvis accommodate for actual fifth lumbar subluxations? Which is first the pelvic rotation or the fifth lumbar subluxation? We answered that last question with our statistical facts. Fourteen of our patients that developed pelvic rotation twisted exactly opposite to what we had planned. We now had this picture. A purposeful adjustment to rotate the fifth lumbar spinous to the right...16 patients involved in the experiment...two of those patients developed right pelvic rotations...11 developed left pelvic rotation, and two did nothing. One of the patients would show a right pelvic one day and a left the next day.

In this experiment, we planned to make a study of leg balance. We measured the height of the superior iliac crests in the standing position. We measured the femoral heads by x-ray study. We measured the malleoli in the supine and prone positions. We weighted each patient daily on the double scales to determine bilateral weight distribution. Statistics on those findings were too complicated, but can be summed up thusly: those patients with unstable sacrums showed leg deficiencies in the prone position. Those with innominate instability showed leg deficiencies in the supine. It soon became apparent that the spinous would more easily rotate toward a low leg side than toward a high leg side. It became apparent that even in the presence of a short leg, if that short leg was on the side opposite spinous rotation, a lift might equalize the legs, but it would not change the spinous position or the pain associated with that position.

We concluded our experiment with 14 of our original patients. One moved to California. The one that was hospitalized did not return. The 14 were carefully analyzed before attempting corrective adjustments. Many things had changed. One patient developed a severe upper dorsal scoliosis. One patient developed a migraine-type headache. One patient developed hay fever. One patient developed a severe acne. One of the males developed an enuresis. We must recognize the fact that some of the above conditions could have developed had these patients not been under an experimental fifth lumbar adjustment. In chiropractic research, the great difficulty arises in not having perfect control over your patient.

Each of these remaining 14 patients received a corrective fifth lumbar adjustment. The left transverse of the atlas remained painful on each until correction was completed. One patient responded very poorly to the corrective technic. Other than the one patient, the remaining responded nicely.

I now had many problems facing me, and throughout 1942 and 1943 I studied pelvic distortions, particularly trying to separate the sacral from the innominate.

During all of my research years, it often amazed me to hear of some unknown chiropractors suddenly appearing on the scene with an amazing new discovery that was to end once and for all times any problems that all chiropractors might have. I often wondered if they worked out their discovery the hard way, or got it by revelation.

My research on the spine and pelvic had definitely established the fact that the lumbar and pelvic are always involved when either is specifically incapacitated. The lumbar subluxation will produce pelvic distortions, and pelvic distortions will produce lumbar distortions. We had to establish a method of knowing which was the cause and which the effect. My research on the fifth lumbar had convinced me that I could be specifically subluxated, and as such would produce a specific-type distortion, but I could not as yet predict exactly what type symptoms the patient would manifest. I had not as yet tied the occipital-spinal pathways up with the osseous subluxations of the spine or pelvis.

I now proceeded to make a one-year study of sacral and innominate distortions. I had discovered many years ago that, while standing in the DISTORTION ANALYZER, a great percentage of my patients would show one buttock posterior and the other anterior. I recognized the pertinent fact that the plumb line would pass over the posterior buttock. I had coined a name for this type distortion; namely, "SACROILIAC MUSCULAR DISTORTION." If the plumb line passed over the right buttock, we would then designate the distortion as a "LEFT SACROILIAC MUSCULAR DISTORTION". I had previously discovered, and put into my 1940 book, the ILIAC FOSSA DIAGNOSTIC SIGN of an innominate rotation. I had discovered that when an innominate was rotated posterior that the upper iliac fossa would become tense, the medial lower femur painful to palpation, and that the leg would be short when the patient was supine. I also discovered that when the innominate rotated anterior, the lower fossa would become tense; the lateral femoral muscles painful to palpation and, in the supine position, that leg would measure long. We had designated those as the U.M.S. and L.L.L. innominate lesions or subluxations. Many facts had been cleared up prior to 1943, but many still existed. I still had to develop a more specific method of knowing when the pelvis was subluxated, what part was subluxated and a method of knowing when to adjust and how to adjust. I had discovered in 1936 that rotation of an innominate would in some instances produce a reflex to the first rib necks, but this was not a constant reflex. Some had it; others did not.

My experiment involved some two dozen people who had worked at benches. I had one-fourth inch plywood cut so the worker would spend part of his day standing with a one-fourth inch support under the opposite foot. The object was to determine exactly what would happen to the spine when continuous and alternating support was applied that would imbalance the pelvis. Strange as it may seem, we gained little information from this experiment, for most workers do not place equal weight upon their two feet. I then used

the same two dozen workers, but this time we removed one half of the leather from one shoe heel. This brought an almost immediate response. I do not know whether the first part of the response was mechanical or psychological, but we did produce some very noticeable pelvic and spinal imbalances. I did not particularly concern myself about the pelvic and spinal imbalances; rather, I concerned myself about the many so-called reflex pains I expected my patients to develop. Before the experiment started, each person was x-rayed and each was visually analyzed by use of the DISTORTION ANALYZER. Each patient's distortion was photographed and sketched. We then palpated each patient thoroughly and graphed all areas that were painful to palpation. It took us nearly two months to prepare for this experiment.

All patients in this experiment were seen once per week, and also were instructed to report to the office immediately if any skeletal pain developed.

This experiment taught us many things we did not previously know. It developed our INNOMINATE INDICATOR. It developed the inferior fifth lumbar indicator. It gave us a clue as to the sacral indicator. It developed for us the upper four lumbar indicators. This year's experiment was the most productive of any we had made to this date. Probably we were at last learning how to conduct experiments.

Throughout 1945 I gathered notes on my forthcoming book, SACRO OCCIPITAL TECHNIC 1946. This book was published in 1946 and brought forth the R PLUS C Factor Technic. The R PLUS C Factor Technic is perhaps the finest technic ever developed in the art of healing for the control of muscular contractions and their resultant pains. We might explain that this R PLUS C Factor Technic deals with RESISTANCE AND CONTRACTION. The chemical formulas need not be reproduced here, but it does work on the theory that all contractions develop a different chemical reaction than does resistance. Contraction is known to every chiropractor. Resistance is used rather glibly when speaking of a patient's resistance to disease, but resistance in the R PLUS C formula means something entirely different. It is the patient's inherent ability to fight an invader. It is the patient's ability to stop a contracting set of muscles before they do permanent damage. It is the work of one side of the pelvis or spine to control the disease producing tendency of the opposing side. This applies not only to striated but also to non-striated muscles.

We had to develop our ADJUSTING INDICATORS BEFORE WE COULD DEVELOP THE R PLUS C FACTOR. The adjusting indicator is merely the site of REFLEX RESISTANCE.

It does not matter how painful a distortion may be, if that distortion has developed an INDICATOR, it will have set up a network of R PLUS C Factors for protection against that specific distortion. That is why the atlas rotates opposite the fifth lumbar. That is why the

skull tips when a fifth lumbar is inferior. That is why a shoulder drops when an innominate meningeal irritation exists. That is why so many patients develop ear disorders when they have an innominate subluxation.

During 1947-48-49 I spent most of my time re-analyzing the previous work I had one on occipital-spinal inter-communicating areas. I felt that I had missed a lot of data on this subject during my previous research. I saw too many instances where the facts I had gathered did not hold water. I was still trying to make the occiput point out specific adjustments for acute spinal pains. I was still trying to use the occiput as a pain control in the wrong conditions. I found many fibers on the occiput that had me completely stumped. I asked my colleagues for help, and all I got was silence. Many said "THE OCCIPUT IS IMPOSSIBLE. It is a dream. I simply won't fool around with it." The D.C. still wanted an adjustment that would make a lot of noise. He still wanted MOVES, not scientific facts.

By now I knew that the occiput concerned itself with the formation of a fiber only when SPECIFIC NON-STRIATED TISSUES WERE THE TRIGGER POINT FOR SKELETAL PAIN.

I selected a small group of young men as my experimental material. I chose six non-smokers for my first experiment. First off, I palpated the occiput of those six and charted all the fibers found. I repeated this daily for four days until I had a controlled chart. I palpated each person's spine over the spinouses and transverses. Taking one subject each day for six days, we started the experiment with a ten cent cigar. Four of the six reacted violently as a non-smoker is supposed to react. In all four instances, occipital area three and dorsal five reacted immediately. The reaction occurred over the SPINOUS. The two that did not react were discarded as research material for this experiment. In each instance, the four that reacted could be immediately controlled by a recoil thrust into the spinous of dorsal five. When the transverses were adjusted, no control was gained, and three became worse. I now had a real problem on my hands and had to seek outside help to gather the answers I needed. I observed that the four who reacted would always produce a fast, low volume pulse with typical shock symptoms...cold perspiration, fast, shallow breathing. The typical yellowish-green facial mask appeared...vomiting, of course, malaise, then a headache. The blood pressure on three of the subjects dropped alarmingly for a few minutes.

I had run into typical sympathetic failure, but what was back of that failure? I presume a number of my readers went through the same experience as a boy with their first chew or smoke, but that was behind the bar or at the swimming hold, not under controlled experiments.

The one clue that helped was the low grade occipital ache each of the four experienced. The occipital bone houses the choroid plexuses of the ventricles, and those plexuses secrete



cerebrospinal fluid. Now this experiment must have produced some type of shock to the choroids, and I meant to find out what type shock was produced.

All of you know that the cerebrospinal fluid is housed in the subarachnoid space of the meninges, and all of you know where the meninges are located. I now had to see if I could interfere with the meninges by some type spinal adjustment. Determining actual cerebrospinal fluid pressure is outside the domain of chiropractic, so I had to have help on that score. A long series of experiments proved that if you would press anterior upon the very tip of a dorsal or lumbar spinous, you could affect arachnoid cerebrospinal fluid pressure. If you could adversely affect it, why not benefit cerebrospinal fluid pressure by just the opposite; i.e., move the spinous headward?

My most astonishing finding during this experiment was the manner in which the occipital fiber would respond. I had known for years that the superior part of the occiput contained seven fibers upon each side, and I also knew that the central and lower part of the occiput did not contain fibers in series. I never knew why, or particularly what such a formation meant. I found that when I would produce spinous tip pressure to the anterior, the corresponding occipital area fiber would swell and become painful on the superior occipital line. Until this date were merely palpated the occiput. WE HAD NOT AS YET DIVIDED IT INTO LINES.

I found that the superior part of the occipital bone was definitely related to cerebrospinal fluid pressure, and any alteration of that pressure would affect the fiber or fibers on that superior part of the occiput. First of all, I tried manipulating the occipital fiber to see if such would reduce the fiber's pain. It only increased the pain. I also found that by such manipulation I could produce pain in the spine. I even had instances where an arm or leg would respond by becoming numb.

I now began a close examination of photographs of the spine and also of lateral x-rays. I again returned to black light photography of the spinal muscles. What had obviously been present on thousands of patients for years now came to light. I OBSERVED INDENTATION AT VARIOUS AREAS OF THE SPINE, AND THOSE DIMPLES OR INDENTATIONS WERE ALWAYS OVER THE SPINOUSES. Pressure into those indentations produced pain, and oftentimes reflexes into other areas of the body. I naturally thought, "MY HEAVENS, HOW MANY TIMES HAVE I MADE THOSE INDENTATIONS WORSE BY ADJUSTING?"

I finally discovered an important truth. By using the vertebra next inferior of the indented area, and by applying our thenar as leverage, we would have the patient lie supine over our

hand, then by counter-pressure over the anterior part of the body, the indentation could be elevated. This developed into our ANTERIOR ADJUSTING TECHNIC.

This seemed to answer our problem in several respects, but such technic did not seem to alter the occipital fiber. We could not prove that the anterior adjusting technic did too much to permanently alter the spinous pain.

When you get stuck with a problem, your best bet is to go back as far as you can and explore old records. Two personalities entered the picture to help me solve this problem: Jim Drain of Texas and Leo Spear of Denver. Dr. Drain way back in the twenties developed an adjusting table that placed the patient in position so his spine would form an arch. Dr. Spear advocated the headward thrust. By putting those two ideas together, I started to solve the problems related to movement of the cerebrospinal fluid. I knew that this fluid moved, and I further had proven that respiration produced spinal movement. I demonstrated that, by forced respiration, one could increase cerebrospinal fluid pressure, and by enforcing light breathing, one could lower cerebrospinal fluid pressure. Using the idea of arching the spine as advocated by Dr. Drain, and the headward thrust as brought out by Dr. Spear, I finally came closer and closer to a solution to my problem.

Oftentimes as you near the solution of a problem, unexpected problems pop up. The occiput now indicated the vertebra involved in the delay of cerebrospinal fluid movement. We knew that this encroachment had to be made into the dura by a special position of the involved vertebra. We knew that if you arched the spine, the indentation would smooth out. We knew that a headward thrust more easily moved the vertebra than any other thrust but--and this was a big problem--WE OFTEN MOVED TOO MANY VERTEBRAE and worsened our patient. One day while giving an adjustment, I noted that the patient reacted quite violently; in fact, the patient fainted. Naturally, being a D.c., I started to manipulate the cervicals, and upon touching the cervical spine, I found it to be very wet. What in the world had happened? From then on, whenever I was approaching the problem of trying to relieve this cerebrospinal fluid subluxation, I would first palpate the cervicals, then give my adjustment, and again palpate the cervicals. In some instances, I noticed that the cervical spine would be quite moist, and in such instances the results from the adjustment were excellent. I now started thinking back and wondering what sort of an effort I was producing into the vasomotor system. I knew that the vasomotors are supposed to begin at dorsal one, but there must be some fibers lying in the cervical plexuses, the vagus, recurrent laryngeal or some other cranial nerve. The answer came by reading just any good book on neurology.

I now started to use pressure against the involved spinous instead of thrusting upon it, and while holding this pressure, I would place the fingers of my freehand onto the cervical

spine. In some instances, even minute pressure would produce cervical spine moisture; in others, no amount of spinous pressure brought forth moisture. I finally started to use a finger vibration to the spinous tip, and this worked better than steady pressure, with the results that by 1951 I brought out the SPINAL PUMP. I took a few more months of experimentation to learn that motion was not sufficient to maintain correction, so I added the spinous tip recoil, and that solved the cerebrospinal fluid stenosis problem. Additional information accumulated, and today we specifically analyze and adjust to correct the dura pressure and its resultant meningeal stenosis.

It is impossible to remain with one research project for months at a time. If you do, your brain fogs over and people start talking about you. I still believed that the soft tissues developed most of our reflex pains, and all the while, I kept charting those areas of pain. During 1947-48-49 I kept finding new reflexes. It was during this time that I developed our coronary technic, cardiac technic and a host of other soft tissue reflexes. By 1948 I knew that the occipital bone contained three separate reflex lines. In my book entitled "CHIROPRACTIC BLOODLESS SURGERY, PATH-NEUROGENIC VISCERAL REFLEX TECHNIC," I brought out the three specific occipital lines and designated them as ANALYTICAL, DIAGNOSTIC AND TREATMENT LINES."

1949-50-51-52 were busy research years. During those years, I developed the INNOMINATE MENINGEAL LESION, the specific innominate subluxation and the oblique sacral subluxation, various types of lumbar and dorsal distortions and improvement in the soft tissue reflexes and manipulations. I was finding more and more instances of cranial involvements. Patients with an apparent common disorder simply did not respond. More and more spastics and cerebral palsied patients were brought to me. Those patients did not respond to what I knew. I had always been interested in anatomy, and for years would spend a few minutes each day reviewing some particular anatomical area. Way back in 1933, while excavating for a new home in Nebraska City, the excavators dug up a skeleton with a skull in fair condition. I became the owner of that skull and used to spend a few minutes now and then surmising why a person so young had to leave the world. I observed that one of the temporal bones was badly misshaped. I worked with that skull until I finally loosened the sutures enough to remove the temporal bone and have a look at its inside. The inside told me a story, and from that time on, I felt of every skull that came into my office. I found more intriguing bumps, depressions and abnormalities in the skull than I did in the spine and pelvis and came very near deserting my spinal research so I could devote my time to skulls. One has to make a living somehow, and you could not do that feeling of farmers' skulls.

During my work on the cerebrospinal fluid lesion, I would have my patients alter their breathing to see what effect such procedure would have upon the movement of the



cerebrospinal fluid. I observed that this altered breathing often eradicated depressions of the skull. When I published SACRO OCCIPITAL TECHNIC OF CHIROPRACTIC in 1952, I had enough information on cranial technic to fill two chapters of that book.

One does not look at crooked spines and twisted pelvises all day long without also noticing feet and ankles and legs. I often observed that a patient would stand with one of his internal malleoli very close to the floor. I noticed people as they walked and observed that some seemed to walk on the insides of their feet. Crooked ankles became a project, and it is really amazing what you can do for many crooked spines by straightening a pronated ankle. Ankle pronation was nothing new. The D.O.'s had been doing it for years, but to me it was new, and I believe our approach and the problem is new.

All through these years I had been trying to impart what I had observed to those who would attend our SACRO OCCIPITAL AND CHIROPRACTIC BLOODLESS SURGERY CONVENTIONS. The financial support given me by those hundreds of fine doctors made my work possible. You cannot do research without money, and although I always had a nice practice, parts of it were not too successful financially. I owe a deep debt of gratitude to all of those doctors who attended our conventions and bought the many books I published. Every chiropractor owes a debt to those same doctors, because some day all chiropractors will profit by what they helped me accomplish.

By 1953 I had solved much of the riddle of the occiput. I knew that the superior part of the occiput was disturbed the increased or decreased cerebrospinal fluid movement. I knew that this part of the occiput was the only part that you could count fibers on and also knew that certain spinal areas would disturb certain occipital areas.

During 1953 I developed the visceral part of the occipital reflex arc. I discovered that vertebral transverses always react to sympathetic abnormalities, and that occipital line two, or the center of the occiput superior-inferior always responded to abnormal transverse stimulation. The problem resolved itself onto the stubborn fact that the spine did not choose the same pathways for the sympathetics as it did for stimuli arising from abnormal cerebrospinal fluid movement, so I had to spend a lot of time re-charting the pathways from the spine to the designated line two areas on the occipital bone.

One thing that is very important to all who use the occiput as a guide to certain types of specific therapy is this: ALL TRUE INVOLVEMENTS OF OCCIPITAL LINE TWO ARE ACCOMPANIED BY A NODULE. This nodule will appear on the most painful part of line two, when line two harbors the MAJOR OCCIPITAL FIBER. Occipital line two reacts from abnormal visceral reflexes, and all visceral reflexes are sympathetic in origin. It is possible

that all visceral disturbances are produced by viruses, and that occipital line two is an indicator of virus activity.

By 1955 I had brought to light that all structural changes in the collagen makeup of a tissue will react and produce a reflex fiber on what we now designate as occipital line three. This once again gave us a long siege of headaches, as once again we were confronted with different intra-medullary pathways, and it again became necessary to chart new cord pathways.

Nutrition had now become a part of our problem. We knew that many patients were sick, not because they had a subluxation, but because they were actually deficient in some vital food element. While this writer did no actual research in the development of any nutritional, he did try to fit specific nutritionals into SACRO OCCIPITAL TECHNIC.

It does not matter how well you have done a job, you have never done it perfectly. I felt that my research since 1924 had opened a lot of unexplored avenues, yet patients did continue to come to our office, and some did refuse to respond. We were still meeting low back problems that no others had answered, and we likewise did not have the answers. Although throughout the years I have spent considerable time with specific problems, I have always looked upon man as a pretty general sort of a machine, no part of which can function perfectly until all parts fit and perform as a unit. I had always aspired to bring man back into good function, whether I did so by a specific or a general application of principles. I can look at my notes of past years, and even of yesterday, and find a lot of question marks. I have no patience whatsoever with the chiropractor who has all the answers, for such does not exist.

In one series of my experiments, I made a contraption that gave me quite a lot of information on this breathing business. This experimental device consisted of a plastic snake. A plastic snake is a jointed ruler that will conform to any surface. I drilled holes in this snake and through those holes suspended short wires and on the ends of the wires I placed a drop of rubber cement. The snake was suspended eight inches from the prone patient's spine, and each little wire was directed into a vertebral spinous tip. With this device, I could actually see the wires move as the patient would breathe. Some vertebral areas gave greater motion than others. The sacrum moved most of all. I would then suspend a patient with the head as low as possible by reversing them on the Hylo table. In this position, most spinal motion was lost during respiration, but sacral motion increased. Then I would use the snake with the patient in various degrees of incline. One thing developed out of this experiment that is important...the position in which the sacrum produced the least respiratory motion always produced cervical spine tension. This finding led to the development of our RESPIRATORY TECHNIC.

Looking to the sacrum as a respiratory mechanism and also as a well for cerebrospinal fluid also brought to our attention that many people were sick because the sacrum lost its normal working position with the innominates. This was not new, for the D.O.'s for years taught the sacroiliac lesion, but we did not look upon sacral phenomena as a sacroiliac lesion, but as a specific SACRAL SUBLUXATION. We now started to x-ray our patients' pelves in three positions: standing, supine and sitting on a stool. We had previously tried to x-ray the sacrum with the patient prone, but results were not good.

We also observed in our visual analysis that a a great many patients had an incline distortion of the spine that also included an incline of the sacrum. While standing still, those patients experienced trouble balancing themselves, so they would turn one foot quite lateral to give them a sort of brace-type support. It became apparent that many fifth lumbar subluxations were not subluxations of the fifth lumbar, but of the sacrum. The important thing we noticed was that every patient who manifested a sacral subluxation also had a lot of tension of the nuchal ligaments.

During the many years I interested myself in research, I carried on a general practice, and like all chiropractors, was often called upon to adjust elbows, shoulders, feet, knees, ankles and what-have-you. I claim no originality in such extremity techniques, for I believe that if you know that mechanical problem underling the subluxation of one articulation, you understand the mechanics of all articulations. I have found that force is an unnecessary part of chiropractic. I believe the day of terrific force is gone forever. The two objectives in making a correction are direction and position. Position puts the segment in its most disadvantageous position for resistance. Direction is the route it must take to go from OUT TO IN.

To fully cover all of my research years would require a volume so large that no one would care to read it. I have merely placed in this small pamphlet the highlights of my experience in the hope that others may be encouraged to look deeper than I have looked, and to see thing I have not seen.

Research is a game. It is fun in a left-handed manner. All of us thrill at finding the unknown. Research is expensive and time-consuming. If you could use the weeks and years you waste, much more could be accomplished. Research has been a stimulus to me. I honestly believe it has kept me alive. I do not consider that my research has cost me even one penny. My doctors and my patients have supported this research. I consider that I have done as well in the fields I have followed as I would have done in practice, and I sincerely hope that I have contributed much more.

I have never considered myself a technic peddler. I have tried to develop something the chiropractors could use, and to include with that something enough inspiration to keep them using it. I feel that through my efforts, many chiropractors have remained in chiropractic and have done well.

In my early years of teaching, I was a pretty crude article. Oftentimes I was more confused than were my listeners. God bless all of them for being so patient.

I hope to teach as long as my students need and accept my help, and as long as I can be helpful. Much remains to be done. SACRO OCCIPITAL TECHNIC STUDIES ALL OF MAN AND SEEKS TO FIND WHERE MAN IS UNADJUSTED AND TO REMEDY MAN BY A READJUSTMENT.

We study man's complete nervous system, even that part housed between his ears. We study man's structure and seek to find its faults and to remedy those faults. We seek to study man's soft tissues and to eliminate any faults found in those tissues.

Now to sum up. The occiput explains man's three nervous systems, indicates the wrongs of those systems and tells you where and how to make your readjustment.

Our VISUAL ANALYSIS tells us where man's structure is bent, and our INDICATORS tell us how to un-bend man.

Our R PLUS C FACTOR TECHNIC lets us adjust man's muscular tension without force, and to make our anatomical adjustment of POSITION AND DIRECTION.

It takes five minutes to make the average SACRO OCCIPITAL ANALYSIS the first visit, and less than a minute the second visit.

The R PLUS C Technic, when indicated, takes about two minutes for the average case.

The indicated adjustment is timed, not by how quickly you can make something pop, but by how long it take you to POSITION YOUR PATIENT, THEN DIRECT THE ADJUSTMENT.

## WHAT INSTRUMENTS DO WE USE?

We use the DISTORTION ANALYZER for our visual analysis.

We use our fingers to palpate the occiput and spine.

We use the x-ray to confirm or deny our findings.

We use our hands to give the adjustment.

In closing, may I write a few words relative to the position of our chiropractic colleges and their views on SACRO OCCIPITAL TECHNIC. No chiropractic college has ever been criticized by me for not teaching SACRO OCCIPITAL TECHNIC. Most of our colleges know something of my efforts, and my books are found in most of their libraries. It is not the function of our colleges to teach every technic or any technic originating in the minds of investigators. The college's duty is to prepare a student's mind for the practice of chiropractic. They take a young person out of high school and hurl them into a science for which few have advance preparation. It is tough enough to master the chiropractic practice sciences without also having to decide which technic you will use. Our medical colleges merely prepare a student to choose which branch of medicine he will practice. He learns that branch after he becomes an M.D. The graduate chiropractor must choose how his practice will be conducted.

## NOW TO THE FUTURE

Research in chiropractic must go on forever. Someone must do this type work, for it simply will not take care of itself. A profession cannot stand still. Momentum must constantly be generated. Chiropractic research needs many things it does not now have. During our great Depression, it was easy to accumulate research patients. They did not have money to spend with other doctors, so they came to me. Things are vastly different today. Just about every working person has insurance policies. Material for research is getting as scarce as hen's teeth. Without material, you do not do research. The only real source of research material is our penal institutions, and we do not have access to that source of material. It is impossible to go into our public schools as chiropractors and make spinal studies. How much farther we go in our research depends upon two things: AVAILABILITY OF RESEARCH MATERIAL AND MY OWN STRENGTH. I have the ideas, but I do not have sufficient research material that I can control for a long-term study. You discover nothing without controlled material.

Sacro Occipital Technic, like all chiropractic technics, needs further study. We certainly do not have all the answers to all of man's problems, and neither does any other group of people. You cannot conceive of an idea, then impart that idea to others and ask them to approve it. Chiropractors are not built that way. They simply are not original investigators.

The medical doctor is not an investigator either, but he has hundreds of lay people on medicine' payroll who can spend all the time and money necessary to meet a demand.



