BURRILL BERNARD CROHN, M.D.:

AN ORAL HISTORY

Recorded by

JAMES D. BOYLE, M.D.
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BURRILL B. CROHN

FORWARD

This is the first of a series of oral histories undertaken by the American Gastroenterological Association to document the history of gastroenterology in this country. The interview was conducted in 3 late-afternoon sessions, February 27-29, 1968, in the consultation room of Dr. Crohn's office at 1000 Park Avenue, New York, New York.

Dr. Crohn was born in New York City on June 13, 1884. He graduated from College of Physicians and Surgeons, Columbia University, in 1907 and began his internship at Mount Sinai Hospital, New York. He developed a special interest in digestive diseases which led to a long career in research, teaching and the clinical practice of gastroenterology. An international authority on intestinal diseases, Dr. Crohn is perhaps best known for his discovery and later delineation of regional ileitis (Crohn's disease.) Dr. Crohn was President of the American Gastroenterological Association in 1932 and was awarded the organization's Julius Friedenwald Medal in 1953.

At 84, Dr. Crohn remains active as a consultant and practicing gastroenterologist, as an author, and as a man of cultural pursuits. He and his wife, Rose, reside in New York City, and have their country home in South Kent, Connecticut.

JAMES DAVID BOYLE, M.D.

CONTENTS

BURRILL B. CROHN (photograph) . . . . . Frontispiece
FORWARD 1
EARLY YEARS 3
INVESTIGATIVE CAREER 10
GASTROENTEROLOGY IN NEW YORK CITY 24
THE AMERICAN GASTROENTEROLOGICAL
ASSOCIATION: REMINISCENCES 32
THE AMERICAN GASTROENTEROLOGICAL
ASSOCIATION: HISTORICAL TRENDS 48
BURRILL B. CROHN: BIBLIOGRAPHY 61
INDEX OF PERSONAL NAMES 70

EARLY YEARS

EARLY YEARS

BOYLE: Dr. Crohn, I am interested in your early background. Where were you born?

CROHN: Here in New York, on 112th Street and Madison Avenue.

BOYLE: Tell me about your parents.

CROHN: My parents were interesting. Both my grandparents were 49'ers in the Gold Craze of California -- came from the old country; one from Poland the other from Germany, religious, fully-bearded, Orthodox Jews. Forced out of the old country by the repression of the Poles and the Russians during 1848, which you will remember as a year of revolution throughout Europe -- a year of uprising against the so-called Holy Alliance of three emperors after the downfall of Napoleon -- '48 was the year of great revolution in Poland and elsewhere. They crossed the continent, presumably in a wagon train or else down by boat to the isthmus of Panama and across the peninsula to Sacramento, California: They returned to New York after the brief gold craze was over.

My mother saw the funeral cortege of Lincoln pass through New York City when she was a little girl. During the race riots she was told by her father, "Girls, today you stay indoors. Nobody goes out. They're hanging niggers from the lamp posts." These were the draft and race riots in the New York City streets.

My father came over independently, during the Civil War and, as a little boy, sat in Central Park and watched the training of the volunteers for the Northern Army. With the discovery of oil in Oil City, Pennsylvania, he picked himself up from New York, where he was earning all of three dollars a week as a runner in a mercantile business of some kind, and went to Oil City, Pennsylvania. When the big play with oil diminished, and the great excitement over the discovery of oil was allayed, he migrated to Central City, Colorado attracted by the discovery of lead and silver. Here he established with his brother-in-law a very successful mercantile business. A fire swept Central City, wiped out most of the buildings and his entire establishment. In those days insurance against catastrophe had not been foreseen and his business was ruined.

Rather than build the business up again he followed

the next craze, which was the cotton boom down South. They sat him on top of a Wells Fargo Express with a rifle across his knees and so he journeyed from Colorado to Rockdale, Texas, a small town in the far interior of eastern Texas. Again he established a large mercantile business, selling merchandise on credit to the planters during the year. When the crop in the form of cotton ripened and was sold, the planters would pay their debts, clear their record, and then set out for another year of credit. It was customary for a merchant to come North to buy merchandice; on one such visit he met my very charming and motherly mother, who had a voice like a nightingale, and came from a deeply-rooted family. She sang in Saint Bartholomew's Church on Sundays and in the temple Emanuel on Saturdays. She sang in the chorus for Leopold Damrosch with the first Oratorial Society that came to this country. My father met her, married her, and brought her South. But she as a New Yorker, just couldn't take the Deep South having lived in New York all her life and accustomed to the New York cultural life. She told my father he'd have to give up his business and come to New York; and he was amenable.

He sold his business in Texas and became a member of the New York Stock and Petroleum Exchange: for the rest of his life, we eleven children were brought up on "eighths and quarters." The New York Stock and Petroleum Exchange was a smaller exchange, associated with the large New York Stock Exchange: the smaller exchange dealt in fractions of shares, because the Big Exchange dealt only with multiples of a hundred. The fractional shares were sold at an increase commission of an eighth or a quarter of a dollar. By selling fractions of stocks at the spread of an eighth and a quarter, my father was able to raise 11 children -- well brought up and educated in the public school system of that large metropolis.

As one might expect of a man who had roamed all over the country as he did, he followed speculative hunches. I asked him once how he enjoyed the train trip coming from Texas to New York. "All right," he said. "What did you do?" "We played poker on the train." I said, "With whom, friends?" "No." he said, "Strangers. You just walked into a poker party, of complete strangers, and sat down" "What did you play for?" "Stakes, big stakes." "How did you come out?" "Well," he said, "between New York and St. Louis, I lost three thousand dollars." "What? That was terrible! Three thousand dollars." "Yes," he said. "but between St. Louis and New York I not only made back the three thousand dollars, but a couple of thousand dollars more."

Can you imagine times such as these! A little German boy becomes an American citizen, a Southerner, who'd lost all signs of religion, marries my mother and becomes obsessed with her cultural religious background. Like all late converts he became the most zealous Orthodox that I ever met in my life.

Saturdays we weren't allowed to move. Speak about the Puritans or the Pilgrims, or the Methodist observance of a Sunday! On Saturday morning you weren't allowed to walk outside the limits of the buildings; one spent his entire morning, afternoon, and evening in the synagogue and could eat only the restricted Kosher food.

With maturing years I hoped to escape from what I considered bigotry. As an intern at Mount Sinai Hospital, I ate for the first time outside my home and was allowed to ride on a Saturday trolly when the occasion demanded. This was the first time I could break away from this ultra-pharisaical tradition of which I had been a part.

BOYLE: What lead you to go into the profession of medicine, Dr. Crohn?

CROHN: I don't exactly know. It was not a known tradition of the family. I just made up my mind that I was going to study medicine. Perhaps my father's constant complaints about "indigestion" concentrated my attention on intestinal diseases.

There were no high schools in my day, as strange as that may seem, and it's very difficult for me to convince people that when I graduated from public grammer school in 1897, there was only one high school, the Townsend Harris High School on the west side of New York; there were no other high schools. One went directly from public school to college. The public school system in New York City conducted competitive examinations for entrance into college. Out of all the graduating classes of all the public schools, 200 boys with the highest standings entered City College -- fortunate that I could pass the examination among the upper 200. My father told the only lie of his life when he deliberately misstated my age. The minimum age for entrance into the City College was 14; I was only 13. At the age of 18, I had already a degree of Bachelor of Arts (B.A.).

In 1907, I graduated from the College of Physicians and Surgeons, one of the best medical schools in the city or the country; a marvelous medical education. Even as a medical student, I was already interested in the laboratory.

I found the study of medicine very strenuous and very difficult, particularly the memorizing of anatomy and the pathology. My love was with the laboratory, and I was a volunteer in the Biological Laboratory of P. & S. under Professor Geis. Incidently during the first year I taught night school, English to foreigners, to help pay my expenses through college.

In spite of the intensity of medical school program, some of us as students were still able to learn the technique of laboratory research. A classmate of mine, Fred S. Weingarten, and I undertook a research project on "The Effect of Massive Intraperitoneal Hemorrhage Upon the Chemistry of the Body." Dogs were bleed from the femoral artery into their own peritoneal cavity until they were practically exsanguinated; we then spent weeks and months analyzing urine and stool for nitrogen, phosphorus, potassium, calcium -- fantastically useless research, but marvelous for technique. We learnt the technique of experimentation and of operation; and we learnt the technique of fine biochemical estimations. For an M.A. Degree, Fred and I published this thesis which was accepted for publication. Columbia University offered us both an M.A. and a Ph.D. and a Sigma Xi. But I didn't have and I didn't want to ask my father for the $35 for the parchment of the Ph.D., so the degrees were declined without too much concern.

BOYLE: Tell me about your internship, Dr. Crohn, at Mount Sinai Hospital.

CROHN: I interned first in pathology, for one year, and later internal medicine for two and a half years. But pathology, of course, was a very special privilage. There was only one such position open. Of all the interns and externs accepted, only one was chosen by competitive oral examination. Only one out of the entire group, and Dr. Emanuel Libman was giving the oral examination. He found it very difficult to pick one out of so many "smart" men. Suddenly he bent his body over into sort of a cork-screw posture and walked up and down the room. "Can anybody define the disease this posture represents?" Silence, and then I said, "Spondylitis rhizomelique." He was evidently surprised. "How did you know that?" I said, "Alongside of my Osler, I read Strümpell." He said "In German?" "Yes." "You get the job." So my German education at City College stood me in good place.

It was the pathological internship that sent me on the path of combined scientific, laboratory, and clinical medicine. That gave me the beautiful balance which has served

me through my professional career. It eventually lead me to "ileitis," because that observation required the combination of clinical observations and laboratory experience to both of which I had had access.

BOYLE: When you left your internship, did you visit Europe?

CROHN: No.

BOYLE: Why did many men go to Europe to study? Was it a great educational opportunity?

CROHN: Oh, yes. They had well-established laboratories in Europe; they had teachers and were set up for teaching and for courses in advanced studies. Men who would look forward to specializing in a subject went abroad to Europe. This was prestige without a question, and valuable education.

BOYLE: When did you begin to consider yourself a "gastroenterologist," or was that word used back then?

CROHN: No. We invented the term "gastroenterology." My generation invented the term -- when, I don't know -- early.

BOYLE: Did you limit your cases at first to internal medicine?

CROHN: I had a very large general practice all my earliest years and only became a specialist when at Mount Sinai we were able to develope for the first time a Department of Gastroenterology which I helped found with Dr. A. A. Berg.

BOYLE: Could you date that approximately?

CROHN: Somewhere about 1920. The publication by Crohn and Wilensky (12) threw doubt on late results of the operation of gastroenterostomy. Very shortly, all the criticism that I'd made of gastroenterostomy as a "cure" was justified by the enthusiasm with which a new operation of subtotal gastrectomy was accepted. Professor von Haberer of Innsbruck, Bavaria introduced the more radical procedure. Dr. Berg, who would criticize severly all the doubt and skepticism which I had cast upon gastroenterostomy became the greatest convert to the newer radical operation of sub-total gastrectomy. He and I and Dr. Richard Lewisohn, his associate, toured the country selling this new operation. Berg who was the master technician, was able to perform a sub-total gastrectomy with minimum mortality. We became soon at Mount Sinai one of the

leading schools of gastrointestinal surgery, with Berg, the master surgeon, and I and my radical associates with an interest in medicine, physiology and gastroenterology. The two interests were combined in the Department of Gastroenterology.

BOYLE: This is something that grew gradually?

CROHN: It grew gradually. Of course, after the spreading knowledge of ileitis and the vast new clinical material in colitis, we were a rapidly developing department, and since we had such an enormous supply of material we became a rather outstanding institution. My own personal files from my private practice include 1100-1200 cases of regional ileitis, at least 3000-4000 cases of ulcerative colitis and about 270 cases of "granulomatous colitis." This is, I think, next to the Mayo Clinic one of the largest collection of such clinical material. In the course of years cases were referred from all over the country and all over the world.

BOYLE: Now in 1917, when you became a member of the AGA, did you consider yourself at that time a specialist, one who limited his practice to gastroenterology?

CROHN: No, gastroenterology was my "special" interest. Primarily I regarded myself as a doctor of internal medicine. But it is the public that makes a specialist, and the public made me a specialist in gastrointestinal disease, particularly the intestinal diseases -- to such an extent that if they had their way, they would crowd me into the terminal ileum and up against the ileocecal valve. I resent these specializations, and always preferred to have much broader interests.

INVESTIGATIVE CAREER

INVESTIGATIVE CAREER

BOYLE: After you finished internship, how did your investigative career unfold, Dr. Crohn?

CROHN: After graduation from the House Staff, I began as a general practitioner; my afternoon job was Associate or Assistant in the Biological Laboratory. My great interest outside of clinical medicine was chemistry.

At that time, all cases of gastric distress were operated upon, if they were intractable to medical treatment. The operation was invariably a gastroenterostomy whether there was an ulcer or not. The operation was highly recommended for ulcer, particularly of course if there was obstruction. If there was pyloric obstruction, gastroenterostomy was a wonderful operation, but if gastroenterostomy was performed for an active ulcer without obstruction, in the large percentage of cases, a recurrent gastrojejunal ulcer might occur. The gastric motility was not improved, frequently the ulcer persisted. The results of ulcer operations were very disappointing. Also, the diagnosis could not be made with any definitiveness, in the primitive state of X-rays which we had at that time, (or in the absence of X-rays when I first began.) The diagnosis was made by history, by deduction, by abdominal palpation, since no definite X-ray evidence of the disease was established. Intractable cases were usually operated upon; whether an ulcer was found or not, gastroenterostomy was performed to relieve atony, to relieve hypomotility, to relieve subjective symptoms of pain. Of course those functional cases in which gastroenterostomy was performed usually ended up very badly with a continuation of all the symptpms without relief.

Dr. A. O. Wilensky and myself were analyzing the results of gastric operations. We were in possession of a kymograph that would show motility of the stomach by tracings. We now had the fractional test meals which Rehfuss had introduced, which I took up so enthusiastically. And we had our clinical studies and to a certain extent X-ray studies. We were discontented with the results and showed: 1) that the operation of the gastroenterostomy did not improve motility unless there was pyloric obstruction; 2)

it did not lower the acidity of the stomach; and 3) it did not relieve the symptoms. I was engaged in the, at that time, very heretical endeavor to discountenance the operation of gastroenterostomy (12).

While working one afternoon, Dr. Libman, who was the Associate Pathologist and also the Associate Physician in Medicine, entered. He doubled as clinician and laboratory head, in that double manner being invaluable. Besides supervising the laboratory work, he performed most of the autopsies. As the pathological intern in Mount Sinai, it was my function to assist at autopsies with him for a year and thereafter, 1907-1908. That afternoon, Dr. Libman walking through the laboratory introduced me to a stranger named Dr. William J. Mayo, who was paying a visit to the laboratory. Dr. Libman said to Dr. Mayo, "You might like to sit down with that young man. He's doing some interesting work." And Will Mayo said, "Well, I'll spend a few minutes with him." Three quarters of an hour later, he was still talking with me, examining the records. "Young man, would you like to do us a favor? Would you come down to the American Gastroenterological Association and deliver a paper on your studies?" I was quite swept off my feet and hesitated but Libman encouraged me to accept.

That May we went to Atlantic City, my associate Dr. Wilensky and myself. The paper was received with such interest that when I finished with the discussion, Dr. Mayo moved a rising vote of thanks to the young men for their interesting paper. Never having heard of a rising vote of thanks before, I stood up with the rest, whereupon somebody put his hand on my shoulder and said "Young man, you're supposed to sit."

The next year, 1917, I was made a member in the American Gastro. Max Einhorn, Jacob Kaufmann, and Morris Manges of this city signed my application. Thereafter I was in attendance, missed none of the meetings and by 1933 was president of the Association, truly a rapid rise!

BOYLE: Dr. Crohn, how did your interest in the small intestine begin? Was there widespread clinical and research interest in the small intestine in those years?

CROHN: There was not much interest. If you look over some of the papers presented at the AGA, for instance

here is one in the 1921 Transactions, by Walter Mills of St. Louis, "X-ray Evidence of Abdominal Small Intestinal States Embodying a Hypothesis of the Transmission of Gastrointestinal Tension." You can see from the title that the small intestine was a closed book at that time. To him the small intestine could be part of a tension mechanism, but nobody could conceive of organic disease. He says, "There seems to be no considerable effort made to investigate the small intestine normal or abnormal conditions by means of the X-ray, with the exception of the duodenum." And now he talks about, "The strikingly apparent dilated and ribboned loops of the obstructed small intestine made visible on the plate by gas and fluid." "It is remarkable that so little has been done toward our investigating the services of the small intestine, the most essential and indispensable part of the alimentary tract." With the exception of gastrojejunal ulcer, there does not appear to have been any conception that direct evidence of involvement of the small intestine might occur nor any effort made to elicit evidence of involvement or impairment or dysfunction other than the instances of "ileocecal valve incompetence" and "ileal stasis."

The fact of the matter is that the small intestine was so badly neglected that when I assisted Dr. Libman in the autopsies, he always insisted that I open the small intestine. His insistence that I open the small intestines was evidence of the fact that most other pathologists did not. In the course of an autopsy people would throw the small intestine aside as though of no importance, having no pathological significance. I did open the small intestine of every autopsy, about 150 in the course of my year as a pathological intern. Thereafter for years we religiously attended every weekly conference on pathology at which all the autopsy material of the week was exhibited. If disease in the small bowel existed we should have picked it up, if it existed at the time in the autopsy material. We never found ileitis or similar diseases in the autopsy material, nor did we find it in the surgical pathology material.

The importance of the whole subject of ileitis and colitis is stressed by the fact that we did not recognize these diseases when I was an intern in 1907 to 1911. It was during that internship that I saw my first case of ulcerative colitis, as I explained in the history of

ileitis which I wrote (159). The wards of our hospital are crowded now with intestinal cases.

Two months ago at the invitation of Dr. Joseph Kirsner I visited the Billings Institute in Chicago. "Burrill, what's happened to the practice of medicine? We have 65 patients. In the olden days if you had 65 patients, you'd probably have 65 cases of ulcer, duodenal or gastric, and liver diseases. Now out of 65 cases, we have 45 to 50 of intestinal cases and very few ulcers. Do you have the same experience?" And I said, "Yes." In our hospital, we have predominantly intestinal cases, and ulcers have become relatively far less common. Something has happened in the course of these 60 years to introduce a whole new series of diseases and to relegate the other ones relatively into the background. Whether there are today numerically fewer ulcer cases than formerly, I don't know. We seem to have <u>fewer</u> ulcers, just as we see very few cases of acute appendicitis today. The intestinal cases have just taken over the field.

Naturally we are all interested in the etiology of this change. What has happened? I like to surmise, to think aloud, just as an exercise in imagination. Is there something in our food, for instance, which is present now which wasn't present 60 years ago? For instance toxic agents or preservatives. When I was young, sodium benzoate was suspect but they seem to have disproved sodium benzoate for there is hardly any toxic effect. Our crops are now sprayed with insecticides whose effects we don't know. The air is polluted with everything. You're eating the same food, but there is something that enters the colon today which either produces colitis or goes into the ileum and colon to produce ileitis or colitis or both. What is there in the last 60 years which was not present before? What is there in the last 60 years that has caused a whole new series of intestinal diseases such that dilutes out the other diseases which previously engaged our attention?

Nor did we know diverticulitis in those years. When a case with an abscess was opened, a foreign body was frequently found, tooth pick, fishbone, or the fragment of the false tooth of a denture. The abscess cavity was opened and the condition was termed "foreign body abscess." Before the X-ray, we knew nothing about diverticulitis or diverticulosis.

Whether it was there or not before us is not certain. With the X-ray we became cognizant that these foreign body abscess near the sigmoid were not actually foreign body abscess, de novo. These were complications of diverticulitis. And Einhorn asked me, "Do you think that diverticulitis is a disease of this century or was it present in the past centuries?" I don't know. I don't think that we could have opened the colon routinely of every case that was autopsied and not have discovered diverticulitis. But something is happening in the colon to cause a disease in it which apparently was not previously present.

Here in my hand is an abstract of a medical report showing the incidence of diverticulitis in Africa (Nigeria); diverticulitis is reported as 40 times greater among Europeans than among non-Europeans living in Nigeria. This would almost look as if diet were the determining factor in producing diverticulitis, because diet of the European is entirely different than the diet of the non-European in Nigeria. Naturally if they live on rice all their lives, they may escape having foreign body effects or something else may be absent. They do not develop diverticulitis.

At any rate, the whole complexion of medicine seems to be changing. Whether it's diet or whether it's something in our food or whether it's something as simple as aspirin or coal tar products, something has so changed the complexion of medicine that the intestinal cases are displacing the other things that were equally interesting.

Here is the case history of the first case of ileitis:

BOYLE: (Reading from record)

Case of Emanuel Solomon, 920 Hudson Street, Hoboken. Age-17. Single, Ward B - Crohn Service. Admission No. 298868. First admitted January 20, 1929, discharged February 3, 1929. Diagnosis on first 3 admissions: "Tuberculosis of Colon, Hyperplastic Ileocecal Type." Diagnosis on 4th admission, 1931: "Terminal Ileitis."

CROHN: The patient was admitted in 1929. A young man, 17 years old, with fever, abdominal mass and diarrhea. And I put down the diagnosis as "Tuberculosis" because it was the only etiologic agent that could cause this. The patient was readmitted in 1930. This time I left the diagnosis out because I couldn't determine it. He was

readmitted the 3rd time in 1930; again the diagnosis: "Tuberculosis of the Colon, Hyperplastic Ileocecal Type", given oxygen intraperitoneal insufflation with improvement. Admitted the 4th time in 1931, he was operated upon. Now the old diagnosis of tuberculosis was out and the diagnosis became: "Terminal Ileitis, with resection of terminal ileum, cecum, ascending colon and of the transverse colon." And that was case number one of regional ileitis. And then, as I said in my book, (112), we began to look for granulomas of the intestinal wall, because Dr. Leon Ginzberg and Dr. Eli Moschcowitz had studied granulomas in the laboratory as pathological specimens but without any clinical collaboration or correlation between the laboratory findings and clinical aspects of the cases; they were doing pure pathology research. It was my opportunity to combine pathology and clinical medicine, having had training in both disciplines. Dr. A. A. Berg and I then re-examined all the obscure cases of diarrhea with abdominal mass and with fistulas; very shortly we had the data on the first 14 cases.

BOYLE: This was the classical article (45) which appeared in the J.A.M.A. in 1932.

CROHN: In 1932, in New Orleans at the Annual Session of the American Medical Association I sat next to Dr. J. A. Bargen, and as I returned to the audience to discuss Jake Bargen said to me, "Burrill, I think you ought to change that title to something else, because this disease may appear elsewhere than the terminal ileum. It may involve other areas of the small and large bowel, so maybe you should anticipate." I, having an open mind, accepted his suggestion and changed it to "regional ileitis." That was a fortunate suggestion because it wasn't long before the entire aspect or thesis of ileitis was advanced to include all of the small bowel and eventually part of the large bowel combined with the small bowel. In the course of the next few years, we followed-up "ileitis" and finally encountered a case of isolated lesions in the large bowel which were not ulcerative colitis because the sigmoidoscopies were always negative.

That lead me to the concept of regional colitis (59), which I also call segmental colitis or right-sided colitis -- right-sided in so far as it did not involve the rectum. I accepted this as ulcerative colitis and not as a granulomatous disease. And to this day the question is still open. If isolated segmental colitis in the right colon is granulomatous in 100% of the cases,

then my pathologists at Mount Sinai let me down. Other writers have made the same comment, that the pathologist let them down in the resection of these right-sided colitis cases. These segmented cases were classed as ulcerative colitis until the English clinicians -- Morson and Lockhardt-Mummery -- brought up that this was granulomatous disease.

The English pathologists and clinicians have now gone to the extreme of calling all this group 100% granulomatous and have changed the name to "Crohn's Disease of the Colon" over my protest. When I said, "Why do you do a thing like that?" "Because," they said to us, "If we say to our students 'Crohn's Disease of the Colon' they know this is granulomatous disease. We want to impress the point upon them that this is not ulcerative colitis, this is granulomatous." I question that this is truly scientific, because I am convinced on restudying my cases and reviewing the literature that probably only 50% of these cases can be shown histologically to be granulomatous. The other 50% act clinically like granulomatous disease, but cannot be proven histologically and microscopically to be granulomatous. One cannot demonstrate the pseudomiliary tubercles which are characteristic of granulomatous disease, nor can one demonstrate the foreign body inclusions. At least 50% of these cases of segmental colitis are pathologically ulcerative colitis. Now those enthusiastic "granulomatists", clinicians, and pathologists, have now conceded that there is such a thing as "clinical granulomatous disease without pathological demonstration." In other words, they're straddling. They're saying "Yes, we will concede the point that at least 50% of these cases act clinically like granulomatous disease rather than ulcerative colitis, but the rectum is not involved, the sigmoidoscopy is negative. Even though they act like granulomatous disease clinically, we admit that when it comes to pathologically true, critical, dissection, you cannot prove that they are granulomatous." But for the sake of harmony, I'm willing to concede or call these cases granulomatous disease of the colon even though in half the cases you can't demonstrate the granuloma. Granulomatous disease of the colon differs from colitis. It behaves differently, is less subject to carcinoma, less subject to toxic dilation and is a much milder disease, more amenable to treatment, either surgical or by corticosteroid therapy.

BOYLE: Dr. Crohn, how did other medical centers react to your reporting of the first cases of regional enteritis?

CROHN: The interesting thing about regional ileitis was the rapidity with which the concept was taken up all over the world. As I said in that article on the history of ileitis (159), within less than a year they were discussing regional ileitis in a Surgical Congress in Middle Europe and Germany. Nobody ever questioned the concept. Everybody accepted it as such and confirmed it, so that the paper (42) as it was originally written stands as a classic today without the change of a single iota. Of course, many facts were missing which were added in later years. That was the interesting thing about regional ileitis: that unlike many discoveries which people criticized, this one was taken for granted from the start.

J. Arnold Bargen was one of the first to concede it. He was terribly upset that he hadn't found regional ileitis. As I told you, he had sat next to me at that New Orleans meeting and after he went back to the Mayo Clinic, he wrote me a few weeks later. They had reviewed all the old X-ray films in the light of what I had shown them and wrote me to say they had overlooked this new concept. It was right there as I'd said it was, and now that their eyes were open, they realized that what I said was absolutely true. He was generally first and foremost in almost everything. Bargen was ahead of me in describing granulomatous colitis or ulcerative segmental colitis, which they call today "Crohn's Disease of the Colon." Actually Bargen was ahead of me in that by several years but called it "sclerosing colitis." He described exactly granulomatous colitis involving the left colon but missed the boat because he thought he had found a bacteria, Bacterium necrotans. Nevertheless, he was a peculiar acute observer and a very hard worker. Even to this day, as you know, he's down in Texas in the Scott-White Clinic and he's in his 80's, just as hard working as ever from what I hear.

BOYLE: Was Azulfadine synthesized for use in ulcerative colitis specifically to treat the bacillus which Bargen believed caused ulcerative colitis?

CROHN: No, Azulfadine or Azopyridine came from Sweden from the woman doctor who was head of the Swedish

institute. She was the one that introduced it, but Bargen was very enthusiastic about her use of it. He was the one that introduced it here, though. My big argument with Bargen over all these years was that he wouldn't use corticosteroids for ulcerative colitis when everybody else was accepting it. He was sticking to his Azulfadine. Azulfadine, I find, is a very annoying drug which reduces appetite, produces discomfort and is exceedingly weak as an antibacterial drug. Before we had Azulfadine, we had sulfathalidine and sulfasuxidine for years. You'll note my reprints on the use of sulfasuxidine for ulcerative colitis (84), which, lacking anything else, we thought was a good drug.

But the effectiveness of nonabsorbable sulfa in the intestinal tract is exceedingly mild compared to the antibiotics or compared to corticosteroids. After we had already accepted corticosteroids, for years Bargen wouldn't give and wouldn't give and he stuck to his sulfa drugs. It took years before Bargen finally broke down and used the corticosteroids. I recall a meeting in which I sat with Bargen and somebody from the audience stood up and asked, "How long would you continue corticosteroid therapy once you achieve a therapeutic result with it?" Bargen said, "I'd discontinue it as soon as I had a therapeutic result." "And Dr. Crohn, what do you think?" I said, "I'd continue maintenance doses of it for the rest of the life of the patient." Of course, I really believe that in ulcerative colitis or ileitis or any of these diseases, if you administer corticosteroids you must keep the patient on a maintenance dose for fear of recurrence of disease because these diseases have so great a tendency to recur.

BOYLE: Dr. Crohn, what did you use for ulcerative colitis before introduction of corticosteroid therapy?

CROHN: I introduced the typhoid vaccine treatment of ulcerative colitis before we had corticosteroids. That's an interesting story. I had a patient on the wards at Mount Sinai who had ulcerative colitis and was anemic. We decided to transfuse him. This was many years ago. We took the nearest donor who was another husky male in the ward. His blood was compatible, so we gave this ulcerative colitis case a blood transfusion from the donor. Two or three days later, the recipient developed a terrific chill and a rise in temperature, and shook the bed. I never saw such a chill; it lasted several hours. We pre-

sumed it was incompatibility. It subsided, and we were perfectly contented. Exactly 48 hours later, this man had another terrific chill and a rise of temperature to 104°F and over. At this point some of us became very inquisitive, decided to look at the patient's blood, and discovered the patient's blood was full of malarial plasmodia. We went back to the donor and discovered that he had come from Russia where he had had malaria. We realized that we had transmitted malaria to the ulcerative colitis case and we gave him quinine. He got perfectly well, and not only was the malaria cured, but he became perfectly well of his colitis.

To speak of serendipity, I never saw anything so striking. I decided we'd try to reproduce this, but since I didn't have malarial plasmodia at my fingertips, I decided to use typhoid vaccine intravenously. I expected that would do the same thing, because at that time the favorite treatment for arthritis was intravenous typhoid vaccine deliberately to produce a chill and rise in temperature. It was effective and used universally for chronic arthritis. I discovered that I could take a very sick case of ulcerative colitis and deliberately, at 10 day intervals, give him intravenous typhoid vaccine, producing a chill and a rise in temperature. The patient would go through a very severe paroxysm but would end up very much better. Before we had corticosteroids, this was my routine treatment, and I had very good results. I gave it up when corticosteroids came in because they were so much easier to handle.

This is an instance of serendipity in medicine. Like Fleming's discovery of penicillin by actually seeing a mold. When I think back to my days in the laboratory, the number of times that I looked at plates of bacterial culture, took the cover off, saw a mold from the atmosphere which had accidentally deposited itself, and then without further looking at the dish I threw it in the carbolic acid solution and considered it as spoiled, contaminated. When I think of the number of times that I did that and that if I would have looked at it more carefully, I might have discovered what Fleming discovered: where the mold was there was no bacteria.

BOYLE: Looking over your bibliography, Dr. Crohn, I see that you first wrote an article on acute glanders (1) in 1907.

CROHN: Yes, I was the first one to obtain a positive blood culture in human glanders (2).

BOYLE: In 1910 you also wrote an article on "Duodenal and Stool Ferments in Health and Disease" (3).

CROHN: I then had become interested in the pancreas. I was a young man on the wards; Dr. Max Einhorn paid a courtesy visit to Mount Sinai, to his friend Dr. Nathan Brill the attending physician. He walked through the wards with us; he took out from his pocket a long rubber tube and said, "I am using this tube to pass through the nose or mouth to intubate the duodenum and make diagnoses of liver diseases, by the examination of the bile specimen. Would anybody be interested in it?" Everybody said, "No," but I put up my hand. "Well, young man, I'll make you a present of the tube." I was the first one to use this tube, along with Rehfuss in Philadelphia who was more interested in gastric secretion. I was more interested in pancreatic ferments.

I needed normal controls to find out what the normal pancreatic secretion was. I didn't have the heart to intubate those poor patients in the hospitals and to leave the tube in overnight. It wasn't very difficult to find the normal control I needed. I was the best subject there was and every night before I went to bed, I'd pass that tube into my duodenum, sleep with the tube overnight. In the morning, I'd aspirate the secretion, take it up to the laboratory and estimate the pancreatic ferments, in the afternoon. I also used all of the pathological patient material on the wards. Then I began to be able to make the diagnosis of common duct obstruction and/or partial or complete obstruction of the pancreatic duct. I developed the whole subject of diagnosis by means of pancreatic secretions (3,5,8,9).

BOYLE: One of your first papers on intestinal disease was in 1925, "The Sigmoidoscope Picture of Chronic Ulcerative Colitis, (non-specific.)" (29).

CROHN: For the first time we had electrically lighted sigmoidoscopes, and we began really to scrutinize the mucosa. In that article I explained that previously our primitive sigmoidoscope was nothing but a great big long lead tube, not even plated inside. For lighting, the nurse would hold up a candle or an incandescent bulb; by means of a head mirror you'd try to shoot that little beam of light down the small tube.

Then finally we were given electrically lighted sigmoidoscopes.

BOYLE: In 1929 you wrote a paper on aluminum hydroxide as a gastric antacid (39), Dr. Crohn.

CROHN: That's an interesting story. It nearly cost me my career. A drug company borrowed a new drug from Europe, aluminum hydroxide, and said, "We'd like to have somebody test this out on gastric secretions and determine whether it's a good neutralizing agent or not." Somebody suggested me, and they came to me and said, "We'll pay you $500 if you'll test this out." I was very much interested in the drug anyway and so I tested it out. I simply went to the wards, introduced a tube into the stomach, withdrew fasting secretion, gave them aluminum hydroxide, and saw what happened. I discovered it was an excellent antacid, and it did not have the rebound that bicarbonate of soda and magnesia had. My studies on alkalies showed you that if you give alkalies, the gastric acidity would drop way down and then it would rise high again, in what we called "rebound". I taught them to use very modest doses of the alkalizer and to distribute them over a long course of time, otherwise you would get a vicious rebound which defeated your purpose. The aluminum hydroxide was a good antacid and did not cause rebound. When I came up for presidency of the American Gastro, Dr. Frank Smithies of Chicago, in committee, said "I don't think that Crohn should be allowed to be President because he has commercialized himself by doing a piece of research for a fee." Of course I accepted a fee to do a scientific bit of work on the antacid. Since then, aluminum hydroxide is the basic ingredient in every alkaline powder and in every antacid, and I was responsible for its acceptance throughout the country. What he thought was commercialization in those days is today accepted as common usage.

Today, drugs are tested out by practitioners in offices, in hospitals, in laboratories, and this is a regular procedure. The pharmaceutical firms pay very well for that type of clinical research, and they base their drug advertisements on what the clinician finds. "Other times, other manners."

BOYLE: I see here in your bibliography an article, "Dr. Israel Moses, Surgeon" (37), written in 1944. Was that an obituary, Dr. Crohn?

CROHN: No. I'm a Civil War buff. I've been interested

in the Civil War all my life. Israel Moses was the first surgeon appointed to Mount Sinai Hospital. I was on the committee which set up the exhibit for the 90th and later the 100th anniversary of Mount Sinai Hospital (30). Going over the writings and history of Mount Sinai Hospital, I came across this very interesting character, Israel Moses, who was the first surgeon appointed to Mount Sinai Hospital, in 1959. When the Civil War began, he left Mount Sinai Hospital and became a lieutenant colonel in the militia. He organized his own regiment, trained it on Staten Island, and took it down to General George H. McClellan in the Peninsula campaign. Moses fought as a lieutenant colonel in charge of his regiment. In 1863 he gave up command of his regiment and joined the Army Medical Corps. He's a very interesting character: it was only recently that I learnt that he was the physician who accompanied the crazy contingent which invaded Nicaragua and tried to shoot the President and initiate a revolution. They were caught and nearly all were killed by Nicaraguans. My hero Israel Moses escaped because he was a physician. They let him off, he came back to the United States but I haven't been able to find him again after that date. He simply disappeared.

BOYLE: Dr. Crohn, you are known for your interest in medical history and you are frequently asked to prepare historical articles and obituaries.

CROHN: I would side with Chekhov who said that "medicine was his wife but history was his mistress". The same is true of me -- when I'm not reading medicine, I'm reading history.

Last year I went abroad to attend the 50th anniversary of the Russian Revolution. I just went there out of interest and I had a very interesting time. I was in Moscow on May Day with that great big parade and celebration. We spent 3 weeks in Moscow. When I came back, and I was asked to give a lecture on Russia, I prepared 2 illustrated lectures: one on Czarist Russia and one on post-revolutionary Russia.

GASTROENTEROLOGY IN NEW YORK CITY

GASTROENTEROLOGY IN NEW YORK CITY

BOYLE: I would like to ask you about the local history of gastroenterology in New York. You have written 2 articles on the history of gastroenterology at Mount Sinai Hospital (92,118). What other hospitals played an early role in gastroenterology in New York?

CROHN: Lenox Hill Hospital was the only other institution, with Max Einhorn and Jacob Kaufmann. Max Einhorn was really the Father of Gastroenterology in this city. Meltzer was older than Einhorn, but Meltzer was a physiologist and not a practicing gastroenterologist. Einhorn and Kaufmann were among the pioneers in the American Gastroenterological Association.

BOYLE: What kind of a man was Max Einhorn? How did he impress one on first meeting?

CROHN: He was a mousey little man with a slight foreign accent. He came from Europe to the Lenox Hill German Hospital, and he was the first man that I know in this area who was interested in gastroenterology. He was a pure clinician but loved novelties; he devised the Einhorn String Test for hemorrhage. The patient swallowed a string: on withdrawing the string a red coloration determined what level the hemorrhage had occured.

He also devised this tube, the Einhorn Duodenal Tube, for intubation of the duodenum, and he hoped thus to make diagnosis of liver disturbances. Then Rehfuss took the tube, put a metal tube on the end of it, and used it to study gastric secretions. Then I used both the Rehfuss tube for the stomach and the Einhorn tube, which he gave me, for duodenal contents. But I carried it further to the diagnosis of pancreatic diseases and was able by obtaining duodenal contents to determine the amount and the activity of pancreatic and biliary secretions. I could tell whether the ducts were open or not and to what extent the pancreatic secretion was diminished in disease. And then parallel to that, of course, would be the determination of fat of the stool, and blood determinations of the pancreatic ferments (3,5,7-9). The A.M.A. gave me a very brilliant editorial at that time though I haven't been able to find the reference to it. Einhorn was a very ingenous and clever man, and as I say, had a broad interest in gastroenterology. He and Kaufmann began at

what was then called the German Hospital; after the Second World War the name was changed to the Lenox Hill Hospital.

BOYLE: Did their Gastroenterology Department start with Max Einhorn?

CROHN: Yes, but it was never a separate department, nor did they have a specialty ward or clinic. Einhorn was the first one in New York City that limited his practice and specialized to such an extent that he had a reputation of being a gastroenterologist.

BOYLE: I see. He was one of the 8 founders of the A.G.A. Another founder from New York was at Mount Sinai Hospital, Morris Manges. I wonder what your recollections are of him. I could find very little written about him.

CROHN: Yes, he was a member of the American Gastro, but he was really an internist and he contributed nothing to gastroenterology itself. He was a good routine, competent internist, who wrote very little.

BOYLE: Tell me about Arthur Chace of New York.

CROHN: He bacame president of the A.G.A. in 1924 and he was a very good clinician. He was very interesting and a very pleasant man. He had very good routine, sound judgment, and personally was a fine gentleman.

BOYLE: At Lenox Hill Hospital there were 2 other A.G.A. members, William Stewart the radiologist and DeWitt Stetten the surgeon. Can you tell me about them?

CROHN: DeWitt Stetten was a surgeon at Lenox Hill and did most of the surgery for Einhorn's cases. He was an outstanding surgeon and of good progressive mind. DeWitt Stetten was one of the first ones to take up subtotal gastrectomy when it was introduced to replace the gastroenterostomy.

Stewart, the radiologist, was one of the best radiologist of that time and was recognized as an outstanding authority. I recall him particularly in connection with the idea from the Mayo Clinic which was highly publicized at the time that benign gastric ulcers underwent malignant degeneration. This was taught all over and Mayo Clinic was responsible for this to a large extent. The radiologists seemed never able to differentiate

the two, and the pathologists were at times unable to. We, in our section at Mount Sinai, never believed that benign ulcer underwent malignant degeneration. We were very skeptical about it. We said that a gastric ulcer was a gastric ulcer, and a malignancy was a malignancy, and that the one did not become the other. I recall once that Stewart, the radiologist, along with an internist, I think it may have been Arthur Chace, invited us to come down to his home without explaining why he wanted us. We came down that evening and found a battery of microscopes set up and under each microscope was a slide of a gastric ulcer. He asked each one of us, who was supposed to be prominent, to sit down under a microscope and write down his own opinion as to whether this was a benign ulcer or a malignant ulcer. Each one of us went through this test --looked through the microscope, studied the section, and wrote down, "This is benign," or "This is malignant." When we were through he showed us the scores. He showed us how differently we each approached the same subject and how different our opinions were. He convinced us that microscopically you couldn't always tell the 2 apart unless it was a clear-cut malignancy or a clear cut benign ulcer.

BOYLE: Considerably later at Lenox Hill there was Henry Rafsky.

CROHN: Rafsky was the son-in-law of Max Einhorn, a gastroenterologist who died rather early. His daughter still runs a fairly large GI practice in Lenox Hill.

BOYLE: I see. You mentioned some things about Albert Berg (122) in your two articles in the history of gastroenterology at Mount Sinai. I am wondering how he began in gastroenterological surgery, and whether you know anything about his early life?

CROHN: He was graduated from College of the City of New York. He was an outstanding surgical technician. People came from all over to see his technique; he was a fast operator. In those days with the crude anaesthesia, speed was important and he could operate quickly with fine technique. He was very venturesome, very brave, and he was capable of doing things in the surgical field that nobody else attempted. His technique was really outstanding. In those days the usual hospital stay for an abdominal operation was at least 6 weeks. The convalescence was long, and post-operative distention was a problem before we had the Levin

tube and the Cantor tube to decompress the intestine after operations. These operations were followed by days and days of gastrointestinal atony from which the patient suffered tremendous amounts of discomfort. This has all been alleviated now by the use of the Cantor tube and the Levin tube. One can even do, as you know, a gastrostomy to prevent the post-operative distention.

I remember a case of hypoglycemia that Berg operated upon. The woman would wake up in the morning completely dazed, semi-conscious; her husband would slap her face and bring her a glass of milk or fruit juice. She'd wake and say, "Where am I?" I took her blood sugar, and her fasting blood sugar was 46, and I suspected she had a benign adenoma of the pancreas. She had an ulcer like most of these cases; this was before the Zollinger-Ellison syndrome was known. But already we were on the track of these benign tumors. She had a huge benign ulcer with repeated gross hemorrhages. I asked Dr. Berg to operate; he palpated the pancreas and could find no tumor. Finally I said, "Look on the under surface," and he incised the peritoneal covering of the pancreas, and found a tiny benign tumor and resected it. This must be at least 30 years ago. That woman is alive and well today, perfectly cured of her ulcer and one of my most devoted followers.

You could do that with a man like Berg, as in later years you could do it with Garlock and Colp; but Berg was outstanding and capable of his technique. He was adventuresome enough to that you could force him into doing sub-total gastrectomies and overcome his prejudice against them. You could get him to operate ileitis cases and colitis cases when nobody else would tackle them, because he had a successful technique.

BOYLE: I understand that Dr. Berg had rounds on Sunday mornings attended by both the surgeons and internists.

CROHN: Yes. Everybody went to the rounds on Sunday mornings and spent much of the day discussing GI cases and going around the wards. There are still at Mount Sinai no individual GI surgical or medical wards. There are many GI units that visit the wards, and study the cases, but there is still no In-Patient GI Department at Mount Sinai.

BOYLE: Walter Bastedo was president of the A.G.A. in 1918 and he was from New York. What are your recollections of him?

CROHN: Delightful man. I believe he was from Saint Luke's Hospital. He was a very delightful man, good thinking, honest, and very progressive. He made friends with everybody, a very good clinician. In fact, Walter Bastedo also taught me pharmacology down at P. & S. When I was a student. That was my first contact with him.

BOYLE: You mentioned Dr. J. H. Garlock and Dr. R. Colp and their role at Mount Sinai in your two historical articles. I wonder if you can say anything about them as persons? What they were like?

CROHN: They succeeded Dr. Berg. When Dr. Berg stepped out, Dr. Richard Lewisohn took over. He was the one who devised the citrated blood transfusion and made modern transfusion possible. Dr. Richard Lewisohn took over Dr. Berg's services, but he didn't have Berg's technique. He was followed by Dr. Colp and Dr. Garlock. The service was divided between these two. They were brilliant surgeons, brilliant in technique, brilliant in concepts. Both were delightful leaders. Colp carried on the Berg rounds for many years on Wednesday mornings until he quit, and then Garlock took over and continued it. Both had good judgment and both were particularly interested in gastroenterology and in gastrointestinal surgery.

Colp was a magnificent technician. I'll always recall his removing an appendix for me. He said he would began to operate at half past one, and I arrived at 25 minutes to two: he was sewing up the patient. I said, "When are you going to do my case?" He said "I just finished it." I said, "You said half past one." "That's right," he said, "I began at half past one, and now I'm sewing up at 25 minutes to two." Perfect technique, tremendous rapidity, and a very good judgment.

Garlock was really outstanding. He died only 2 years ago of a ruptured aneurysm of the aorta, which he diagnosed on his way to the hospital. He began to bleed on a Sunday afternoon of massive hemorrhage," he had had practically no previous symptoms. He told his wife and the doctors that it was a ruptured aneurysm: he just managed to reach the hospital but died almost immediately. There has just been published his volume on gastrointestinal surgery, which is a masterpiece. He had finished all except the last chapter when he died suddenly. I'm

sorry that I don't have it here to show you, but it is the most marvelous description of technique, judgment, experience and results. It will probably receive the highest encomium and praise. He was my surgeon of choice in all complicated and difficult gastrointestinal cases. He could do anything and it was marvelous to work with him. I can't speak too highly of Dr. Garlock. I have spent a couple of years raising for him a substantial fund in memorium. The new Mount Sinai School of Medicine has accepted it and will establish a special John H. Garlock Memorial Room for consultations among the surgical staff, discussions of surgical material, and surgical conferences along side the operating room.

It's a great loss. He and Colp were both a great loss very difficult to replace. Garlock's surgery of the esophagus was really pioneer work. He and Colp could tackle the massive operation of total gastrectomy or resection of the esophagus. These were outstanding men, and they did most of my gastrointestinal surgeries after Berg's death. Colp left 10 years ago and Garlock took over and continued until 2 years ago. Brilliant surgeons.

BOYLE: I wonder if there are any names that I've omitted that stand out in early New York Gastroenterology? There are 2 other A.G.A. members from New York City: John Killian, a biochemist, and George Daniels, a psychiatrist.

CROHN: Who was the man over Roosevelt Hospital? Cave, Henry Cave. Southern with an accent -- a gentleman. He never appeared in public without white starched cuffs; all of the grand manner of a Southern gentleman.

BOYLE: Speaking of surgeons, I'm wondering if back 40-50 years ago the role of the surgeon in GI diagnosis was any different? Was he his own diagnostician or his own medical consultant? I am speaking of surgeons in general?

CROHN: Surgeons are, in my experience, their own diagnosticians, their own medical men, their own technicians, and their own everything else. Always were and always will be. I have probably referred more medical cases to surgeons than any medical man alive that you know. I keep them busy with every type of gastrointestinal surgery. I don't believe in my entire experience that

on more than two occasions they ever called me in consultation or asked me to help them out with the medical aspects of their cases. Surgeons are very self-confident and self reliant. Medical men are dependent upon the surgeon, while the surgeon is never dependent upon the medical man.

THE AMERICAN GASTROENTEROLOGICAL ASSOCIATION: REMINISCENCES

THE AMERICAN GASTROENTEROLOGICAL ASSOCIATION: REMINISCENCES

BOYLE: I am interested in your recollections of the founders of the American Gastroenterological Association Do you recall S. J. Meltzer?

CROHN: I have a very unique distinction in regard to S. J. Meltzer in that he was our family physician in New York City, and he delivered me into the world. And I recall him distinctly in his general practice in those days with no telephone, and his office was on 122nd Street. We lived at 82nd Street. My mother had 11 children, and when any of the children took sick, I, as the oldest child, was asked to take the trolly car or the horse car and ride up to 122nd Street where his office was and leave a note with the colored boy that somebody was sick in the Crohn household and would Dr. Meltzer please pay a call. Some time later you would see Dr. Meltzer's barouche with a horse -- single horse usually -- and Dr. Meltzer in it, a big, portly set man with a German accent -- typical, countenance of a Bismarck, but underneath it, gentle and kind and very devoted to our family. On one occasion I recall that I saw him to his carriage, and noticed in the bottom of his vehicle that he had a cage with rabbits in it: he was taking the opportunity to observe his experimental rabbits while he was taking these long drives, paying his professional calls.

BOYLE: Do you recall Charles Dettie Aaron, the uncle of Abe Aaron?

CROHN: I remember that he was one of the founders, but I don't think that I ever met him personally. I think he had already died or retired before I joined.

BOYLE: Do you recall A. L. Benedict from Buffalo?

CROHN: No.

BOYLE: Do you recall Henry Elsner from Syracuse?

CROHN: I recall him only by name. I think that he was deceased at the time I joined.

BOYLE: How about Allen A. Jones from Buffalo?

CROHN: Oh a very tall man, lanky, 6 ft. 5, with a mid-western drawl, typical of an old-time clinician. Very tall.

BOYLE: How about Charles Simon of Baltimore. You recall?

CROHN: An interesting man, but there is nothing I can recall that was of a personal nature.

BOYLE: Now D. D. Stewart, who I believe was the second president, from Philadelphia?

CROHN: No I don't remember.

BOYLE: How about Charles G. Stockton? He was the first president?

CROHN: He was from Buffalo, but I recall little about him.

BOYLE: Julius Friedenwald. What do you remember of him?

CROHN: Julius Friedenwald was a gentleman. He was of the famous Friedenwald family of Baltimore. He was a gastroenterologist, his brother was an ophthamologist, and there was a third. Julius Friedenwald was a delightful gentleman and an excellent clinician. A man of great probity and broad interest not only in gastroenterology, but in his view of medicine as a whole. The Friedenwald medal, which I have over there on the wall, is the award which was founded by him. It's that gold one to the left over there (pointing). Good clinician -- excellent clinician -- cultured -- historical. He had a fine family background and was a very fine gentleman. I don't know that I can point to any particular type of interest within gastroenterology that he had. His interests were broad, not specific.

BOYLE: Let me ask you, Dr. Crohn, for your recollections of your contemporaries in The American Gastroenterological Association, the people you know best and who impressed you most?

CROHN: Well in my time the inside clique consisted of Sara Jordan and Abe Aaron of Buffalo, Russell Boles from Philadelphia, Harry Bockus of Philadelphia, Jake Bargen out west. This small number of us visited from one city to another, informally between Annual Meetings, and we would demonstrate our newest findings to within this group. I recall, for instance, that Sara Jordan and Russell Boles came up to Mount Sinai to see my work. I went to Boston to see Sara Jordan's material. We were an intimate group hiding nothing to receive

the inside reports and have the privelege of free discussion. It was intimate and good. As an example, we questioned Jake Bargen's discovery of the bacillus which "caused" ulcerative colitis; we put that aside by acclamation and convinced him and the Mayo Clinic that he was wrong about it.

BOYLE: Sara Jordan accomplished so much in gastroenterology, yet didn't she also manage to have a family?

CROHN: She married, fairly late in life (Penfield Mower,) and she had a daughter, I think. Pen was a very quiet, nice fellow who gave her full swing for her scientific activities with all the time she needed for her scientific work. The only hobby she liked was to play bridge; crossing the continent with her on a train to a San Francisco meeting, Sara Jordan and her husband and I and some fourth person played bridge all the way across. She was very pleasant and most intelligent. She entertained us in Boston when we visited that city. I attended her funeral, as did all her friends and associates. A devout Catholic, all the hierarchy paid tribute to her for her life of devotion. Not only for her devotion to medicine, but for her activities on various civic boards and as one of the active participants in reform movements in Boston. She had broad interests besides those of medicine.

BOYLE: You mentioned Henry Bockus as a member of your group.

CROHN: Yes, Henry Bockus, the stormy petrel. He was known for his sharp tongue, his keen sense of humor and his very critical judgment. Most people were afraid of his tongue. I gave a paper once on causes of upper gastrointestinal hemorrhage, and my figures showed that predominantly duodenal and gastric ulcers were responsible for the hemorrhage. This was many years ago, before I realized the importance of esophageal varices. I gave a very satisfactory paper with much discussion. Finally Bockus was asked to join the discussion. "It's a very nice paper. I agree more or less with my friend Burrill Crohn, but don't they have any real alcoholics in New York on the Bowery? Maybe at Mount Sinai they don't see those bums in the Bowery with their esophageal varices. I don't think Burrill realizes the extent to which esophageal varices are responsible for hemorrhage in the upper gastrointestinal tract." And that was the type of discussion he would use with his sharp tone but he never hurt, always exceedingly critical but never personal or unkindly.

He trained a whole generation of gastroenterologists. If you note, there's the Bockus International Society of Gastroenterology and his Fellows are distributed all over the world. Wherever he goes, you'll find his students who adore him because he was an excellent if a very critical teacher.

He asked me once to help out in the examination for the American Board of Internal Medicine Sub-specialty Examination in Gastroenterology. He was short of an examiner; he called me from Philadelphia and asked me if I'd come down and help out. I did. When I got down there he handed me a list of questions. For instance, "Ask the students to give the differential manometric pressure between the pancreatic duct and the common bile duct," and questions of that sort. I looked at him and said, "Harry, I can't answer these myself." He had been studying these matters particularly, had been teaching the students basic science. But it seemed to me unneccessary to ask such examination questions. I thought it was improper but that's the way his mind ran and he was teaching his men accordingly.

He was one of the great enthusiasts of the Meltzer-Lyon biliary drainage. I was down once to visit him, and found cubicles all over his offices and in each cubicle there was a patient lying there, with a tube hanging out of his mouth, undergoing differential biliary drainages. He was very enthusiastic about that type of examination. That was one of my big bases of difference with him but Henry Bockus cannot be commended highly enough for his School of Postgraduate Medicine in Philadelphia, and a whole generation that he taught all over the world. I recall one incident that was typical of Henry Bockus. I called him up and said, "Henry, I'd like you to come to New York for a consultation on the wife of one of the directors at our hospital. She has a carcinoma of the pancreas invading the duodenum with gross hemorrhage -- an obscure case, and I've found that I'd like another opinion, and I have chosen you to come over from Philadelphia." "I can't come," he said, "I'm too busy." "Nonsense, Henry," I said, there's a thousand dollar fee in it for you, and you can spare the time." "I'm sorry, Burrill, but I'm too busy. I'm teaching, and my teaching comes first and I'm not interested in the fee." This is characteristic of the sort of man he was. True to his instincts -- true as he understood them.

Another incident: Do you recall the superior mesenteric artery syndrome? I never understood the syndrome,

but he was asked to speak at the Bronx Medical Society on that subject, and I was chosen to open the discussion. We had dinner together, a very pleasant cordial time and we went to the Bronx Medical Society meeting. He talked at a great length about this brilliant syndrome: the compression of the 3rd portion of the duodenum by the superior mesenteric artery crossing it. I rose and gave one of those white-washing discussions which said neither yes or no and really just got myself out of it by saying it's very interesting and so forth and not being critical, because I was the host and I just couldn't tell him, as my guest, what were my real doubts. There was much applause, and everybody left for the coat room. I was in line waiting for my coat when one of the Bronx doctors walked up behind me and whispered in my ear, "Dr. Crohn, we know all about this "syndrome". We said 'Crohn will put him in his place.' We all expected you would but you didn't. You let us down." I felt I wasn't in a position as his host to really tell him what I thought about it, but shortly after that the superior mesenteric artery syndrome disappeared from the literature.

These concepts such as biliary drainages, mesenteric artery syndrome, chronic appendicitis, adhesions, and vagaries of that type -- I have witnessed through my life time. These concepts came up with a tremendous wave of enthusiasm, the rank and file accept them. The truth comes out sometime later. In arriving at the truth, the fallacies of the past must be disproved.

BOYLE: Dr. Crohn, you must have known Frank Lahey, the surgeon from Boston.

CROHN: Frank Lahey was a wonderful man. He organized the whole Lahey clinic, carried it through Depression. "Frank," I said, "why are you working so hard. You look tired and you've a duodenal ulcer. You ought to know better; take care of your ulcer! Why don't you have it operated upon and get rid of it?" "I haven't got time." "Why haven't you got time?" "Because of the Depression." "What's that got to do with your having your ulcer operated?" "My mortgages," he said, "I've got to work for my mortgages. I can't stop to be operated." Hard working, very hard working. A charming, frank delightful man and a delightful companion. After he had a drink or two his tongue would loosen up, and he'd become very much more interesting. I recall a conversation about Howard University. He said "Burrill, you know Howard University?" and I said "I certainly do. That's the Negro university in

Washington D.C." A funny thing arose; this was the time when the Jewish medical students thought they were subject to prejudice in the form of numerous discriminatory clauses that prevented them from free admission to the medical schools -- which was never so, but thus went the argument. "Yes," I said, "I know it's a good school. All colored people?" He said, "Not all." I said, "I thought it was." "One," he said, "little Jewish boy came down from Brooklyn and applied for entrance to the Medical School at Howard University. Howard University was complimented that they had a white boy coming down so they accepted him. Then he received such a good education that he went back to Brooklyn and told all his friends there about this colored medical school and what a fine medical education he was receiving. Very shortly Howard University was overwhelmed with white applicants from Brooklyn, and Howard University had to put in some restrictive clauses."

Frank Lahey attented a consultation on President Franklin D. Roosevelt. When the President was about to run for his fourth term at the end of the Second World War, several consultants including Frank Lahey were called to Washington to see Roosevelt. Franklin D.'s legs were swollen; he was edematous, he was short of breath, and he looked terrible. He had a very poor heart action, he had rales at the bottom of his chest posteriorly. Frank Lahey said, "I was one of the consultants and after our examination, we gathered outside and decided the President should not run again for office. He was in no physical condition to run for office and he had to be told so. We all agreed. They appointed me as the spokesman for the group; Franklin D. sat there in his chair with a cigarette in a great big cigarette holder jutting out of his mouth. I said, 'Mr. President, we don't think you ought to run for office. You're in no physical condition to do so,' and I went on to explain to him the reasons why not. Franklin D. listened to me and when I finished, we waited for his response. There was a pause, and he looked at me and he said, "Dr. Lahey, you come from Boston?" and I said "Yes." "So I assume you're a Republican!"

Frank Lahey was full of interesting anecdotes and was such a hard worker. He was devoted to Sara Jordan whom he trained, took her as a young student, put her through college, through medical school, carried her through all her internship, took her in to his clinic and made her the head of the Department of Gastroenter-

ology. she was the protégé and close associate of Frank Lahey. They ran a magnificant institution and built it up beautifully, and as long as she was there Frank Lahey was there -- Sara Jordan, Mandred Comfort and the rest of the staff. After Frank Lahey died and Sara Jordan died many of the old staff passed away or retired. I don't think it was the same institution as it was in the olden days.

There are many anecdotes about these interesting fine men. Good fellows, generous, open-hearted, good friends. There was only one incident of anti-Semitism. I've often been asked about anti-Semitism, but I always answer without any hesitation: it doesn't exist among men of culture and education and broad scientific interests. But as I say, there was one exception. I was on the Executive Board and was asked to go to Pittsburgh to sit in on a meeting of which B.B. Vincent Lyon was the chairman. I was assigned to a hotel, in Pittsburgh to be on call for the whole day Sunday. I landed there Saturday night by train, registered, and looked around for the rest of the men. Nobody else had registered, which I thought was strange. I went to bed and in the morning the phone rang, and they said, "Come on over, Burrill, we're waiting for you at the club over here. At the Fort Duquesne Club." I went over and discovered that all the other members had spent the night at that club, and suddenly I realized that as a Jew I wasn't eligible so I'd been assigned to the hotel in the city of Pittsburgh. The social aspects were relatively unimportant, because we were all very close friends. We spent a very interesting lunch and finally dinner time arrived. We were sitting around, having a very pleasant dinner. It was Prohibition time. At the dinner everybody remarked that the wine was so exceptionably delightful. Everybody wanted to taste that wine; it was good. I was curious myself and as the waiter came alone, I said, "Waiter, allow me to have that bottle for a moment. I'd like to see what it is." I took the napkin away from it and read "Kosher Wine, Shel Pesach." Kosher wine for use at Passover! So that was my one experience with anti-Semitism.

BOYLE: We haven't mentioned Russell Boles, Sr. except in passing. Can you tell me about him as a person?

CROHN: Oh, very lovely, as is Mary, is wife. Russell is a most sociable fellow. I can recall one anecdote, about when we invited Arthur Hurst over from London. I had visited Arthur Hurst in London, and was a tremendous

admirer of him. He later became Sir Arthur Hurst. I had visited with him and made rounds at Guy's Hospital with him. We went on the very wards that Bright had been attending when he described Bright's Disease, and this was quite a thrill. Arthur Hurst was a lovely host and a very interesting man. When I was President of the American Gastroenterological Association, I invited Arthur Hurst over. We entertained him in New York, and we entertained him elsewhere. We were all on the way to Washington for the annual meeting which I think was there that year, and we stopped overnight in Philadelphia to be entertained at Russell Boles' house. Arthur Hurst was sleeping as a guest in the home of Russell and Mary Boles. Mary would do anything because she was really one of us. When it came to sociability or arranging our programs or dinners or anything else, Mary Boles was always ready. Arthur Hurst was the guest overnight and the next morning I saw Mary. She said "You know I don't mind being hostess here. I don't mind doing anything, but when Arthur Hurst takes his pants off at night and throws them under the bed and says, 'I expect to have them pressed by tomorrow morning!' I'll do anything, but I'll be damned if I'm going to press his pants." I said "You know it's an English custom." But that was typical of Russell and Mary Boles. In later years Russell was interested in carcinoma of the stomach; he performed statistical studies on carcinoma of the stomach. Afterwards he was in charge of the Abstract section in Gastroenterology for years. Lovely, pleasant, he and his wife. They were the center for all social and scientific activities.

BOYLE: George Eusterman at the Mayo Clinic who died recently was a notable member of the American Gastroenterological Association. I wonder if you knew him?

CROHN: Did I know him? I most sincerely did. He was, next to Walter Alvarez, the closest one that I knew at the Mayo Clinic. George was absolutely stalwart and scientific, honest and good and much more critical than many of the other men. As an example, he didn't fall for the mistake of the previous years in regarding gastric ulcers as prone to malignant degeneration.

Again, when at the Mayo Clinic they made the statement that as many as 10 or 20% of the cases of gastroenterostomy developed gastrojejunal ulcers, everybody else was astounded at it. In the East here, we knew that there was at least a 2% and perhaps as high as a 5% incidence recurrent gastrojejunal ulcer following

gastroenterostomy, but these high figures from the Mayo Clinic were absolutely startling and we didn't accept their figures. I must say that Geroge Eusterman stood out, as I recall, against his own people and agreed with us on the much lower incidence of gastrojejunal ulcer following gastroenterostomy.

The interesting point is that if you perform a gastroenterostomy for a functional gastric disturbance or for anything except peptic ulcer, a gastrojejunal ulcer never forms; but if you do a gastroenterostomy for a duodenal ulcer or gastric ulcer, then you invite a recurrence of peptic ulcer. We were in combat with the Mayo clinic with their reports of a very high incidence of gastrojejunal ulcer. Nevertheless, it was their insistence upon it that finally lead to the introduction of sub-total gastrectomy as a substitute for a gastroenterostomy and eventually to Dragsted's operation of vagotomy, because we realized "no acid, no ulcer," which was the common saying. No acid, no ulcer, and anything that eliminated the acid in the stomach eliminated the ulcer. I recall Owen Wangensteen, who introduced me to vagotomy. For years, many of the A.G.A.'s papers concerned vagotomy: its influence, results, effects on acid, and The Hollander test. I went to London some years ago, I spent the whole morning with Mr. Burge, the surgeon.

BOYLE: He had devised an electrical check test for completeness of vagotomy.

CROHN: I spent a whole morning with him and this complicated apparatus, seeing him cutting the vagus nerve and then using this electrical check to see if there was a response, until finally he eliminated every part he thought was a fiber of the vagus nerve. Now we know that it was practically impossible to do.

At Mount Sinai it was Ralph Colp who was particularly interested in that subject. Colp felt that no matter how difficult the technique was, he wanted to master it, and he went all the way out West to Wangensteen and spent some time in his surgical clinic watching his technique of vagotomy. Later, when he came back, he would do vagotomies on our ulcer ulcer patients. We would check the Hollander insulin test later and would prove to him that he had not severed all the fibers, because acid could still be stimulated. Yet the vagotomy was one of the big additions to gastrointestinal surgery of this generation.

BOYLE: Dr. Crohn, you mentioned how Will Mayo invited you to deliver a paper on gastroenterostomy to the A.G.A. meeting. Had you been down to the meetings at Atlantic City before, or was that the first meeting that you had attended in the A.G.A.?

CROHN: Oh yes. That was the first time that I had attended, in 1916. I gave my talk in 1916. Apparently it was published in '17 and I was made a member in '17.

In my day the association was restricted to a hundred members in the whole United States, so it was quite a distinction to be one of the hundred. But also gastroenterology was a new specialty. When I first attended the meetings of the American Gastro, a Congress of all the medical specialty associations was held in Washington: our gastroenterological group was not yet recognized. Of course there was no such thing as a specialty in stomach and intestinal disease. The men who controlled internal medicine wouldn't allow any such specialty at that time. This Congress was the parent group and the parent would have nothing to do with the insurgent youngsters. I remember attending the Congress. We were not recognized and we were not allowed to participate as gastroenterologists in the Congress. Then later, the A.M.A. Section on Gastroenterology and Proctology was formed. Of course it was many years before we became a sub-specialty group of the American Board of Internal Medicine.

BOYLE: I see. Could you recall some of the color and flavor of A.G.A. meetings other than the scientific aspects. I understand that there was an annual banquet. Did you attend any of the early banquets?

CROHN: I attended them all. I think the banquet was very interesting and we invited distinguished guests. When I was President in 1933, this was the time of the great communist spread in Europe and the beginning of Hitler. The meeting was in Washington, that year, not in Atlantic City. The guest speaker, by my invitation, was one of the Paulist Fathers from the Catholic community, whom I invited to come over and explain to us the dangers of communism, because the Catholic church was beginning to be very conscious of the threat of communism to the church. He accepted the invitation and spoke to us about the dangers of communism. Of course 1933 was already the period of Stalin and this was before the war, before '39 the period of rapid spread of communism which we thought was a great threat to our democracy.

Many of these banquets were well attended and were

very sociable; I regret very much that they don't have the banquets now as they did in the past.

BOYLE: Yes. Did the meeting last a single day in your recollection or was it 2 days long?

CROHN: No, 2 days, as far as I can recall.

BOYLE: And were there any offshoot meetings held in association with it or did the members gather strictly for the A.G.A. meeting and then disperse?

CROHN: The meeting was nearly always arranged at the same time or just preceeding the meetings of the A.M.A., so it was an offshoot of the A.M.A., as it were. The A.G.A. had no related meetings associated with it. We would go down a few days before the A.M.A., hold the meetings of the American Gastro, and from there stay on and attend the A.M.A. meetings. Many of the A.M.A. meetings were in Atlantic City and we always would precede them there but we would usually follow the A.M.A. to other places, too, as to make it possible to attend both.

BOYLE: Do you recall some of the committee assignments that you held in the A.G.A., Dr. Crohn?

CROHN: I was chairman of the A.G.A.'s Committee for the study of Gastro-Intestinal Hemorrhage. We were very much interested in internal hemorrhage. We were interested in whether patients with gastrointestinal hemorrhage should or should not undergo emergency surgery. The big problem in emergency gastric hemorrhage in ulcer cases was when to operate upon them and when to treat them conservatively. At that time, surgery was less safe than medicine. In the early days of crude anesthesias and the crude technique of operating on a hemorrhage case, surgery was known to be associated with 10% mortality, but gross hemorrhage itself had a mortality, unoperated, of 10% and up. I was just in the generation in which surgery became possible in those cases.

I became an advocate of surgery for hemorrhage, emergency surgery particularly, having such a brilliant surgeon as Berg as my right hand who could carry out the emergency operation of hemorrhage and get away with it. By this time we had transfusions and infusions. When I started my early career in medicine, we didn't even have a transfusion and we did not have an infusion

nor know how to thread a vein. That's how primitive we were. But my generation took over this hemorrhage problem, and little by little we advocated the radical resort to surgery in massive hemorrhage, particularly in the older age group.

BOYLE: What were the meetings like around the time that you became President of the A.G.A. in 1932?

CROHN: My memory of the meetings previous to the early 1930's was that they were the stuffiest lot of sessions. There was nothing reported at those meetings except clinical experiences and clinical experiences which usually were not really substantiated by scientific studies. Don't forget that laboratory work at that time was still in its incipiency except for pathology, that biochemical researches were slow in developing. The members, particularly the older men, would occupy the front seats. Clinical problems would come up, and all they would do was to discuss their experiences and tell about the interesting case they had and their interesting conclusions, which would usually be surmises on clinical bases.

My associates, when I became president, were the more radical group, headed mostly by Walter Alvarez and myself, and Henry Bockus, and Sara Jordan who was beloved by us all and with her open mind -- and of course Frank Lahey. Walter Alvarez came from the laboratory of the Mayo Clinic. He had both clinical experience and laboratory experience, and the rest of our radical group all had not only laboratory experience but were clearly progressive in our minds, and we really feel that we took the American Gastro out of that old rut of staid, repetitious, clinical observations and put a new light into it. We established it as a scientific body.

BOYLE: Did you know Walter B. Cannon?

CROHN: Yes, I certainly did. I remember him particularly because I have a guilty conscience about one thing. I sat next to him at a banquet and he said, "Dr. Crohn, you must contribute to the Spanish cause. This Spanish Civil War is very important. The Russians and particularly the Germans are flexing their muscles and using their newer armanentarium. There'll be war certainly, international war, and you should contribute liberally to the loyalist cause because if Franco takes over, a depotism will be established and he'll be followed by Hitler and Mussolini, so you must help us." I said,

"Dr. Cannon, I'd like to help them, but I think they are nihilists and anarchists and socialists. They're a disorganized group and I'm really too conservative to contribute to a cause which is so discoordinated and so chaotic." As I look back, I probably was wrong. Enthusiastically, he was raising funds for the Loyalists in the Spanish Civil War.

He described to me his first use of X-rays and the goose with the long neck down which he pushed the bismuth -- made the goose swallow the bismuth and took his X-ray films and demonstrated progress of the bolus. -- the earliest experiments in physiology with X-ray. When I was an intern at Mount Sinai in 1907, they showed us a single X-ray film of the hand of a mummy showing all the bones. I remember how startled I was at this new discovery of X-ray that could photograph opaque objects.

BOYLE: Do you recall A. J. Carlson? I understand that he had a very interesting personality.

CROHN: Ajax Carlson! A Norwegian with a Norwegian accent. Very critical. Not a clinician but laboratory -- very interesting work -- similar to that of Pavlov and the Babkin school. He was the American correspondent of Pavlov and Babkin in Russia, and he followed all their work. I have the first editions of his work at home in my library. A. J. Carlson was a very fine scientific and a very critical man; it was known that you just couldn't get away with a statement in front of A. J. Carlson if it wasn't absolutely accurate. I spoke with him on a platform before a Vermont medical audience. I wasn't being very critical in my remarks; I spoke about "hyperacidity", hyperacid secretion of the stomach. Ajax followed my discussion and said "I'm surprised Dr. Crohn doesn't know better. The secretion of gastric juice, its acidity is always at the same level. There's no such thing as hyperacidity; there's only such a thing as hypersecretion."

That was Ajax. He was the type that, "if it was not, it was not." If you stepped out and made the slighest error in a statement which was then open to scientific criticism, Ajax was there. He'd pick you up on it right away, which was good. It made all of us careful about making statements, and this was what we needed in those days more than anything else, because the amount of clinical drool which the clinicians would get off on the basis of surmises or guess work was so

legion that it took out generation of critical individuals to take the whole of gastroenterology out of the mythology of the past, out of the loose statements based on so-called clinical judgment and to pin the thing down to scientific statements. It was our generation that pinned things, so that if a man made a statement, he'd have to stand up and prove it.

BOYLE: Ajax Carlson was from Chicago, as was Walter Palmer. What can you tell me about him?

CROHN: Now you're talking about an outstanding individual. Walter Palmer, at the University of Chicago, Professor of Medicine. He organized their whole Gastrointestinal Department. It was Joe Kirsner who was assistant to Walter Palmer, and who succeeded him and is now the Chairman. Walter Palmer is now practicing privately as a consultant in Chicago, and I understand he has a very large practice. Walter Palmer was the leader of the whole Chicago coterie and could not be excelled for good clinical medicine, good clinical judgment and a very progressive mind. Delightful personality.

BOYLE: Do you recall Babkin and Komaroff? Did they have much interaction with the other members of the A.G.A.

CROHN: No. I don't remember even meeting Babkin. I know that Pavlov came to this country on a visit. I remember particularly the very unhappy incident of his falling asleep on a bench in Grand Central Station waiting for a train; somebody picked his pocket and took his money. The Rockefeller Institute reimbursed him immediately, but it was very embarrassing for the old man.

BOYLE: What can you tell me of Frank Smithies? He was the original editor of the American Journal of Digestive Diseases. He died in 1938. Apparently there was some difficulty between him and Beaumont Cornell who owned and published the journal. I have heard Smithies was rather difficult at times.

CROHN: It wouldn't be surprising that there were difficulties between Frank Smithies and anybody else because he was one of the most contentious men that I knew. He would insist on having his own way about everything. He was difficult to get along with, stiff-necked and obstinate, and most men found it difficult to work with him. He was a good clinician, he was an

THE AMERICAN GASTROENTEROLOGICAL ASSOCIATION: HISTORICAL TRENDS

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THE AMERICAN GASTROENTEROLOGICAL ASSOCIATION:
HISTORICAL TRENDS

BOYLE: I have heard that a major reason for the founding of the A.G.A. was the bad atmosphere created in this country of many self-styled "stomach specialists" especially in the period of 1880-1900. Were these people giving the specialty of gastroenterology a bad name?

CROHN: There were several prominent so-called "stomach specialists" in New York City who had very large practices. While they did specialize in gastrointestinal diseases, the only qualification that they had was that they called themselves "stomach specialists." They had no special training, no particular clinical training, few hospital association. They conducted a huge office in a wholesale manner. Their only attribution to distinction was their own decision accepted by the public that they were "stomach specialists."

BOYLE: So I take it that the legitimate internists and the general practitioners did not particularly look highly upon these men?

CROHN: On the contrary, they looked down on these fellows as being ambitious interlopers who called themselves specialists but had no qualifications except that their particular interest in gastrointestinal disease. They had office hours which would begin at 8:00 in the morning and go through 6 or 7:00 at night. They practiced continuously, charged small fees, used no X-ray in those days. Everything was a clinical impression and empirical treatment.

BOYLE: What diseases did they diagnose in those days?

CROHN: They knew very little about psychiatry or psychosomatic medicine, but they knew usually enough to recognize a functional case from organic. They rarely had hospital associations or were at least not with any hospitals of good standards. Everything was office treatment. This was the period of colonic irrigations, where everything in the body that they did not understand was due to "auto-intoxication". The large bowel was the cesspool of the body as Metchnikoff, the Russian, had said, and all disturbances -- flatulence, indigestion,

constipation, and gas -- everything was due to poisoning from the large intestine. Many of them had outfits for continuous colonic irrigations.

I must admit that even I at one time fell for this and established in my large office a special nurse and a special table for colonic irrigations and the patients were supposed to come to the office and have their colonic irrigation once a week or once in 10 days to "wash the poisons out of the intestinal tract." Every cure was attributed to these marvelous colonic irrigations. The nurse would wash bag after bag of soap suds and saline, and she would wash and wash until there was no more feces returning from the intestinal tract, and then she'd feel herself justified in saying she'd done a very successful colonic irrigation. Then when all the "poison was washed out of the patient," he came back once a week or once in 10 days to have this treatment renewed. I had special nurses that would go to the homes of the patients to wash out the colon. Whole establishments were set up for colonic irrigations where you could send patients to have their colons washed out.

I shortly came to the conclusion that this was a philosophy with no sense to it, and that whatever effects achieved were psychological. It made a tremendous impression upon the patient if he was sent to have his intestine washed out, to be cleansed from all his sins -- intestinal sins. Psychically it was a wonderful method of treatment, but I never convinced myself that it ever did anything for the patient, and I discontinued it.

BOYLE: These men called themselves stomach specialists but they also did colon work and by the term stomach they referred generally to the entire gastrointestinal tract.

CROHN: Don't forget that practically none of the intestinal diseases that we now treat, that fill our hospitals, were recognized then: ileitis, ulcerative colitis, diverticulitis. None of those diseases were known or recognized. The only disease of the intestine was constipation, distention, and gas. And there were acid conditions of various types. With a lack of a sigmoidoscope and with a lack of decent X-ray, it was very difficult to accumulate knowledge of intestinal diseases. Practically nothing was known.

BOYLE: Did these men persist for long after the A.G.A. was founded? Were there any left when you entered practice?

CROHN: They had practices several times as large as my practice. They were financially most successful people. They died out very slowly.

BOYLE: What was responsible for doing away with these men?

CROHN: I believe it was the raising of the standards by the introduction of the American Board of Internal Medicine and then the establishment of the Sub-specialty Certification in Gastroenterology. After that, if you wanted a hospital appointment of any kind, you had to prove that you were a member of the American Board. So the whole standard of hospital appointments was suddenly raised by the introduction of the American Board. People who couldn't take the examination couldn't have a hospital appointment, no matter how successful they were in their practice, and no matter how many hundreds of patients they saw in a day in the office at $2.00 for a call, unless they were a Diplomate of The American Board. And of course, if they didn't have a hospital appointment they were not recognized. The establishment of the American Boards was the most remarkable event occuring during my professional life.

BOYLE: Then, you feel that the development of the American Board of Internal Medicine has had a bigger effect on deterring the practice of gastroenterology by unqualified specialists here in New York than the establishment of the A.G.A.?

CROHN: Yes, I think the American Board of Internal Medicine had the bigger effect. For instance, when I sat for years on the qualifying board of the Workman's Compensation in New York City, after I got interested in trauma, names would come up -- men wanting to be recognized as capable of testifying for Workman's Compensation -- and you'd ask them one question, "Are you a member of the American Board?" and if they were then automatically they were accepted. If they were not, we had to look at the qualifications and decide further, but from the time of its origin, the American Board became the standard.

BOYLE: How about the role of the A.G.A. itself in medical practice in New York. Do you feel that the A.G.A. itself has had any direct affect on improving practice in New York?

CROHN: No, again I think the Boards are the important thing. I think when a man comes up for hospital appointment, whether he is a member of the American Gastroenterological

or not is very secondary importance to the Committee on Admissions in a large hospital. I don't think that the fact that he's a member of the American Gastro means an awful lot. I'm sure that if he's a member of the American Board of Internal Medicine, if he's a Diplomate in his Sub-specialty, then they will accept that whether he's a member of the American Gastro-enterological Association or not. Membership depends upon the whim of the man as to whether he wants to belong or not, whether he would be considered a valuable member and whether he would be accepted or not. Belonging to an association is an optional thing but American Board certification is practically obligatory.

BOYLE: What do you feel has been the A.G.A.'s role in shaping the practice of gastroenterology?

CROHN: It has accomplished much by enlarging its sphere of interest. In its meetings it now includes all the research work and endoscopy, when it established preliminary meetings a day or two ahead of the clinical meetings. So it has broadened its field to not only clinical medicine but to laboratory research and in that way it has done wonders. It has established research funds and fellowships and has encouraged research.

BOYLE: Dr. Crohn, do you think the A.G.A. has been an effective force in shaping gastroenterological research today or in saying what is good research and what is bad research?

CROHN: Yes, yes, I think it has been a factor for the good. I think that the Federal Government has been much too liberal in the matter of grants by the National Institutes of Health -- much too liberal with research. The amount of research that is completely wasted and funds which are wasted is enormous. There should be much more discrimination in approving research grants. At times it seems that if you simply stand on your feet and wave your hand and ask for a grant from the N.I.H. and that's all that you have to do to obtain it. It's practically true that almost anybody who asks for money can get it, no matter how abstruce the research is. The amount of repetitive research, the going over of old stuff and rehashing of the things which had been settled by our generation 30 years ago! The repetitive work going on today which you see in the literature every day is almost to the point of nausea, and I think there's a tremendous wastage in you funds.

There is a great excess of importance placed upon what a man does in the way of research as far as appointments

within a hospital are concerned. You know perfectly well that appointments as heads of department are usually made not on the basis of one's stature as a clinician or physician but on, "What did you publish?" If you can't publish an article once a year, you can't hold your position as a full time Professor. This is an absurd emphasis on research. I think that it would make a tremendous saving if the federal government and the pharmaceutical houses would be much more discerning and discriminating in what and how they spend their money, without insisting on expanding laboratories all the time for non-essential research. This results in the denigration or deterioration of emphasis on clinical medicine, and heads of the department are brought in because they've done research not because they are good bed-side clinicians or diagnosticians. We in medicine decry today the fact that almost all of our medicine is computer medicine, to the point where the hospital beds are now so overcrowded that the really sick patient cannot find admission into a hospital because the beds are all occupied by cases which are being sent in by the practitioners for a so-called study, and thorough review of the case. Doctors will send patients to the hospitals and not even visit the patient but just sit at their desk in the office and over the telephone order test, test, ad nauseam. Every test is to be done. Every little thing, every blood chemistry, and all sorts of studies to the point where there are times when the resident recites a case to you, he will forget to tell you that he did a physical examination or took a history. It's nothing but a recitation of laboratory figures. And now with the advent of Medicine, anybody over 65 can be sent to the hospital and have the government pay the whole bill, or with Blue Cross and other insurance schemes, overutilization of beds in the hospital has resulted, and now you can't admit a case in even in an emergency.

BOYLE: You predicted some bad times ahead in your Presidential Address in 1933 concerning the dangers of socialized medicine. Do you think you've seen some of these things come to pass?

CROHN: In 1933, we had been warned by the British who already had a form of socialized medicine. At one of our annual banquets we invited 2 English clinicians to address the annual banquet, and I never will forget how they described socialized medicine in England. They raised their hands in holy horror and said, "Gentlemen, if you know what you are doing, don't let Aneurin Bevan come

anywhere near you because he'll subject you to socialized medicine, and you'll be the sorriest people in the world." It was the English clinicians who came over and warned us to not let socialized medicine happen. And, of course you know the most conservative organization in the world was the American Medical Association, and they felt the same way as we did against socialized medicine. Now it has gotten to the point where we are all stymied. I had three emergency cases two weeks ago over the weekend and couldn't get one of the three cases into Mount Sinai or to the Doctor's Hospital. It is just impossible to have an emergency case admitted, because the beds are crowded with cases being studied under Medicare and Medicaid.

BOYLE: Dr. Crohn, you spoke of the change in A.G.A. meetings which have taken place since you joined the association. What factors do you feel were responsible for this?

CROHN: Such members as Boles, Bargen, Jordan, Lahey among others. The younger people, such as these, replaced the older clinicians completely. We took over and built up this institution.

BOYLE: Was this before or after you were President?

CROHN: No, it was largely after I was President, because I was older and most of these men followed me. The only one of the group ahead of me was Walter Alvarez. Controlling the Executive Board, we saw that we could put into office men of our liking, men that we wanted who would build up the institution rapidly on a progressive and scientific basis, replacing the old clinicians.

BOYLE: That really started with your Presidency in 1932. The only one that was before you was Walter Alvarez.

CROHN: Walter Alvarez was President in 1928. Then came Frank Smithies, Ludwig Kast, and Clement Jones, who were the old-fashioned type of clinicians. I became President in 1932, and I was followed by John Bryant who was a clinician, and B.B. Vincent Lyon , Howard Shattuck, and then Chester Jones in 1937. There's a man that you can't pass over. Chester Jones of Boston was very important. The last I heard of him, he was still actively practicing in Boston, one of the few members left over from the "old crowd". He is a man with critical judgment, strict scientific honesty and a good clinician.

BOYLE: Dr. Crohn, in the early days of the Association,

what types of scientific papers were presented at the Annual Meetings?

CROHN: As you look through these copies here of the transactions, you see there is nothing but clinical material, clinical discussions and observations usually not backed up by any scientific data, all clinical discussions of clinical disorders. It was too bad that scientific medicine hadn't advanced further.

For instance, there was the Meltzer-Lyon technique for non-surgical biliary tract drainage. That was the period when B. B. Vincent Lyon introduced his technique of Biliary drainage for the diagnosis of gall bladder disease. I was a heretic and didn't believe in it. I was castigated and criticized, because I became engaged in all of the discussions. Max Einhorn and I were the skeptics. We didn't go along with Bockus and Vincent Lyon , the Philadelphia crowd, and all of the rest of the country that fell for this.

We used to spend hours with biliary drainage just trying to make some sense out of it, and I never succeeded. Finally I went down to New York University, and asked for laboratory facilities. I took dogs, injected india ink into their gall bladder, under anaesthesia of course, sewed them up, and I would wait 2 or 3 days and then open the abdomen and perfuse the duodenum with magnesium sulfate to see what would happen. According to the theories, when you perfuse the duodenum with the magnesium sulfate, the gall bladder should contract and the india ink should come out. It didn't. I let the dogs rest for 2 or 3 days, went back and looked again and observed the fact that the india ink took a long time, several days, before it leaves the gall bladder. I couldn't convince myself that perfusing it with magnesium sulfate had helped it at all.

I recall that old man Einhorn and I used to go all over the country discussing biliary drainage. One terribly wintery day, the old man and I went out to New Jersey and addressed the medical society. We were heretics and were the only two that held out -- to such an extent that apparently I was such a turbulent element that they appointed a committee to investigate the subject of the magnesium sulfate drainage to see who was right. A committee was appointed and each one of us had to go up there and testify, and of course the committee was in no position to make a final decision. If anything, Einhorn and I were sort of relegated to limbo; we hadn't proven

the point that instilling magnesium sulfate was of any use, while the others were so enthusiastic about it that we lost out to the majority. But in the course of time in science, everything reaches its evident ultimate truth, and very shortly after that and little by little, the whole subject was dropped. Of course, it is not used today.

BOYLE: Wasn't that unusual for a committee to be appointed to try to settle a scientific controversy?

CROHN: Yes, that was very unusual. In going back to some of these old transactions, here is a paper which was presented, "Opaque Meal Versus the Stomach Tube in the Diagnosis of Gastric Hypomotility." They were making a big to-do about atony of the stomach. These were some of the old fashioned ideas.

"The Effect of Stasis in the Ileum." This was 1918. Stasis in the Ileum! Here is a group of 200 patients with X-ray examinations "6 to 12 hours after barium. In 42, the small intestine emptied slowly and there was a residue in the ileum. In 3 cases, this persisted for 19, 48, and 54 hours respectively." Even though there was stasis in the ileum, it never dawned upon these people that any disease could occur in the ileum, although they all played around with the ileum.

About this time, we really had a blow-up with one of the biggest errors in medicine, "chronic appendicitis." Here is a paper on "Roentgen Rays in the Diagnosis of Chronic Appendicitis," by George Fowler, the outstanding roentgenologist of Philadelphia. Everything we had known which you couldn't explain when I was young was "chronic appendicitis." An appendicitis operation was an everyday affair. Every day there was an appendectomy, regardless. If you didn't know what was going on with an abdominal pain, every neurotic, every nervous woman, every histerical girl who had a belly ache immediately had "chronic appendicitis," and the appendix had to be removed. This was really very sad medicine, because it became quite obvious to a critical person like myself that we were getting nowhere in removing the appendix in these cases. And if the appendix were removed and showed atrophy, as it naturally would, or fibrosis or something of that kind, or if they opened it up and found a little nugget of hardened feces in it, then they were elated. But it was clinically quite obvious that we were getting nowhere with it at all, and we were just operating upon neurotics. This craze of chronic appendicitis really was the dominant fashion of it's

day. I recall one instance -- we were making rounds with one distinguished physician -- I really shouldn't mention his name -- who was one of the most enthusiastic of the exponents of chronic appendicitis and every abdomen that he would palpate had chronic appendicitis. I was making rounds with him on the wards, and he demonstrated to a crowd surrounding the bedside. He said the man's diagnosis was chronic appendicitis, that he should have his appendix out, and that he would be cured, and he moved on to the next bed. Something about the case attracted my attention. While the others of the crowd moved to the next bed, I went back to look at that abdomen and found a very fine scar where his appendix had previously been removed. This only confirmed all the skepticism I had about chronic appendicitis but it took a long time for everyone to overcome this enthusiasm, this fixation to remove an appendix on the grounds of chronic appendicitis. It wasn't until we got rid of that fixation that you could began to think of other diseases which might cause lower abdominal pain.

BOYLE: What do you think of the structure of current meetings of the A.G.A.?

CROHN: For 1 or 2 days preceeding the clinical meetings, the presentation of the laboratory work is very worthwhile for anyone really interested in the laboratory side.

I haven't attended a meeting for years because I really think that the custom of making men who are past 65 become senior members and denying them a space on the program -- not utilizing their talents at all for anything -- I think does themselves an injustice. These senior members are still around and could contribute a great deal. They would be much more critical than the present generation who can spend a whole session on nothing but the malabsorption tests, talking about nothing else but malabsorption.

I really think that they exhaust themselves and the subjects unnecessarilly. Much too much laboratory work, much too little clinical work, and much too little emphasis on the clinical sides of medicine.

This is what is happening in medicine. My son is a physician up in Buffalo. He goes to the research meetings in Atlantic City every year, the so-called "Young Turks." He was down there and stopped in on the way back, and I said, "Show me your program," and he showed me his program

which looked like a volume of the Encyclopedia Brittanica. I asked, "How many papers?" "About 5000," he said. "Five thousand papers? How long were you down there?" "Three days," he said. I asked, "How many did you hear?" "Not many, but altogether there were 5000 papers. There were meetings in every hotel." I said, "Are these all published?" "Oh, no. Maybe 2000 are." "What happens to the other 3000?" He shook his shoulders and said "I don't know. Maybe in the waste basket." So of the 5000 papers, each represents a research effort and each probably is endowed by the National Institutes of Health, and you realize what a fortune is going into research, you can imagine the amount of waste, -- the waste of money and the waste of time. I am only conversant with medicine but when you think of the amount of money which goes into research in the industrial world, the pharmaceutical world, and other branches, you can see what an enormous amount of money is spent.

As I told you the other day, the research foundations at Mount Sinai alone have made public in the last 6 years the use of a million dollars, and I am sure it doesn't begin to cover the amount of research which is endowed at that hospital. If you multiply this by the several thousand hospitals and teaching institutions throughout the country, you can see what this amounts to.

BOYLE: What do you think is going to happen? Do you think that the pendulum will swing back away from so much research?

CROHN: No, the world is getting more and more socialistic. I don't think it will swing back, because the prestige of medicine is in research -- full time appointments. Of course all departments are now "full time." I served at Mount Sinai Hospital for 61 years and never received a salary for any work that I ever did, nor did any of my confreres. Today each department is headed by a full-time man with salaries of 35,000 dollars a year upward. Most of them receive their appointments on the basis of research and outstanding achievements in some particular small field and this is the way the hospitals are run. I was an intern for 3½ years. At the end of this time, I received the magnificant emolument of $50 and a small black bag containing a blood pressure apparatus, a thermometer, and a stethescope. Today, the interns and residents start with $2500. Within a year or two it's up to $3500 or $4200, depending on whether he is married and

has children. So are we to doubt who's paying for all this huge expense of medicine, this increase of the cost of medicine that goes on year after year until it exceeds in the rapidity of its increase any of the branches of industry throughout the United States. Who's paying for it all? Blue Cross, Blue Shield, Medicare, Medicaid, the State, and Federal government, and I think this trend will go on to the detriment of the clinical side of medicine. These full-time men! I recall one head of the Department of Medicine who received his appointment on the basis of a very creditable piece of research; he was rarely seen on the wards. He rarely came down to the wards to examine a case. He sat up in his cubicle, on the Pavillion on the roof, and directed the research from there. That was his concept of being Head of Medicine -- how much research could be produced and how much could be published. That was his interest, quite different than Osler and Libman. I made rounds twice with Osler.

BOYLE: Oh, really? That must have been an experience.

CROHN: It was quite an experience. I can remember even to this day that tall, austere-lõoking, very pleasant man. He was making rounds on the invitation of Libman, and I remember till this day the bed in the ward in which he examined the abdomen. It was a question of an intra-abdominal carcinoma, and he felt for the spleen. I remember his turning around and saying, "You can feel the spleen. There can not be any intra-abdominal carcinoma. You can rule it out if the spleen edge is palpable." I have often quoted that experience, and everybody questions it and says, "Is it true?" I think with one exception it has been true, and the exception was a case with thrombosis of the splenic vein. This was the type of thing that Osler could say that made him so brilliant, so pleasant, such a delightful man of culture.

BOYLE: To talk of research once more, do you think that the trouble in gastroenterology is too much basic research to the exclusion of clinical research, or just too much research that is not good research?

CROHN: There is so much basic research, but at the same time I recognize that if you don't have masses of basic research you may miss some very important discoveries. Something unexpected may materialize. Many years ago, when I took the trouble to go up to General Electric in Schenectady to see what their research laboratories looked like, I went through these massive research laboratories

and said to the director, "What do you get out of this? Does it pay?" He said, "Doctor, if one man is able to devise a slightly better insulator for our copper wires that we have at the present time, no matter how small the advantage is it will pay for all the research." In medicine, we have the example of Fleming who made the original observation of the effect of penicillin mold but paid very little attention to it, and of Florey who during the war saw the possibility of adopting penicillin to the infection of wounds and discovered that he could mass-produce penicillin. From all the research that went on, just the single instance of finding penicillin mold made it worthwhile. Perhaps ninety percent of the research through the years in this area were not directly productive but just that one idea justified the efforts.

Still, there is such an unnecessary tremendous expenditure of money for research. At Mount Sinai it amounts to almost a million dollars a year from the federal government. At Columbia P. & S. it is said that if the United States Government ever withdrew its research money, most of the laboratories at P. & S. would close down, and this is true all over the United States. The United States Government and pharmaceutical firms are practically supporting all the country's laboratory research. Now this is good if they produce something, but everybody knows what a large percentage of research is wasted and repetitive and gets nowhere.

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INDEX OF PERSONAL NAMES

INDEX OF PERSONAL NAMES

	Page
Aaron, A. H.	32,33
Aaron, Charles D.	32
Alvarez, Walter C.	39,43,53
Babkin, B. P.	45
Bargen, J. A.	15,17,33,34,53
Bastedo, Walter	28
Benedict, A. L.	32
Berg, A. A.	7-8,15,26,27, 28,29,42
Bevan, Aneurin	52
Bockus, Henry	33,34,35,43,54
Boles, Mary	38,39
Boles, Russell	33,38,53
Brill, Nathan	20
Bryant, John	53
Burge, H. W.	40
Cannon, W. B.	43,44
Carlson, A. J.	44,45
Cave, Henry	29
Chace, Arthur	26
Checkhov, Anton	22
Colp, Ralph	27,28,29,40
Comfort, Mandred	38
Cornell, Beaumont	45
Crohn, B.B.	
Wife	1
Parents.	3-5
Son	56-57
Daniels, George	29
Dragstedt, Lester	40
Einhorn, Max.	11,20,24,54
Elsner, Henry	32
Eusterman, George	39,40
Fleming, Sir Alexander.	19
Fowler, George	55
Friedenwald, Julius	33
Garlock, John	27,28,29
Geiss, Professor	6
Ginzberg, Leon	15
von Haberer, Professor	7
Hurst, Sir Arthur	38,39
Jones, Allen A.	32
Jones, Chester	53
Jones, Clement	53
Jordan, Sara	33,37,43,53
Kast, Ludwig	53
Kaufmann, Jacob	11,24

Killian, John 29
Kirsner, Joseph 13,45
Komarof, S. A. 45
Lahey, Frank 36,37,43,53
Lewisohn, Richard 7,28
Libman, Emanuel 6,11
Lockhardt-Mummery, H. E. 16
Lyon, B. B. V. 35,38,53
McClellan, General G. H. 22
Manges, Morris 11,25
Mayo, William J. 11,41
Meltzer, S. J. 24,32,35,54
Metchnikoff, E. 48
Mills, Walter 12
Morson, Basil 16
Moschcowitz, Eli 15
Moses, Israel 21
Mower, Penfield 34
Osler, W. 58
Palmer, Walter 45
Rafsky, Henry 26
Rehfuss, Martin 20,24
Roosevelt, Franklin D. 37
Shattuck, Howard 53
Simon, Charles 33
Smithies, Frank 21,45,53
Solomon, Emanuel 14
Stetten, DeWitt 24
Stewart, William 25
Stockton, Charles G. 33
Wangensteen, Owen. 40
Weingarten, Fred S. 6
Wilensky, A. O. 7,10