CELEBRATIO MATHEMATICA

R H Bing

SUKHJIT SINGH

R H BING: A STUDY OF HIS LIFE

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Introduction

It was during a visit with R H Bing (affectionately called "RH" by his friends and close associates) while he was hospitalized for two weeks in the summer of 1984 that the idea of doing a Bing biography occurred to me. As R H greeted me from his bed, he appeared weak but in good spirits, his eyes shining with anticipation of some good conversation about mathematics. Throughout our talk, R H would tell me of an interesting event in his life accompanied by an anecdote from his childhood or student days. After such an enlightening visit, it entered my mind how historically crucial it was for the mathematical community to have a written record of R H's life.

R H regained his health and strength and returned to his office on the campus of The University of Texas at Austin. It was here that I approached him and solicited his help with this project. My intent was to write his biography under his guidance, not only for authenticity but with the hope of capturing the "Bing persona." R H reluctantly agreed, but became more enthusiastic as the project progressed.

My first goal was to collect biographical data through a series of informal interviews I had with R H several times each week, in which he answered questions concerning the various stages of his life. However, I soon began to realize that my note-taking alone could not serve as a sole resource for accurate biographical data, much less convey the colorful colloquialisms of Bing. It then occurred to me that the use of audiotapes would be an excellent medium for gathering information.

After a considerable amount of persuasion, R H agreed to do a tape, or a sequence of tapes, following an outline of topics that I provided. However, R H proceeded only under the condition that the tape be used only to facilitate the writing of the biography, and that it not be made public. R H was quite concerned that his random comments might be misunderstood, or that he might easily say inaccurate things during the flow of a taping session.

The biographical sketch you are about to read is a culmination of years of separate and joint efforts by R H and myself at rearranging, editing, adding, and deleting material from the original transcription of this tape.

Originally, I began writing the Bing biography in the third person based on this tape, but soon discarded this approach. It occurred to me that the first-person narrative style manifest in the tape was more suitable in exemplifying the "Bing persona" as well as his unique conversational style.

from R. H. Bing: *Collected papers*, vol. 1, edited by S. Singh, S. Armentrout, and R. J. Daverman, American Mathematical Society, Providence, RI, 1988. MR 950859 Zbl 0665.01012.

I started by carefully rearranging, editing, and polishing a transcription of the tape. This draft served as the starting point. Subsequently, R H and I both jointly and separately kept adding to and deleting from this draft. At one point, we agreed that the project was finished, and I left a copy of the final version with him at his home in Austin. Sure enough, R H returned this copy to me with some additional changes only one month before his death. I have incorporated these revisions.

So, welcome to a rare insight into the special character and spirit of R H Bing. The strength, love, and discipline he received and learned formed the underlying core of his commitment to mathematics. Although many contributed to his lifelong commitment to mathematics, the most notable are his mother Lula May, his teacher R. L.Moore, and his wife Mary.

Family background and childhood

My father was reared near Waller, Texas. Grandfather Bing died at a relatively young age, and I never met him. However, I did visit the family farm when I was very young. He and his brother had come from Canterbury, England. His brother, not satisfied with life in this country, returned to England. My father kept in contact with the Canterbury Bings until World War I. When I visited England, I tried to find more about the Canterbury Bings and was told that they had been prominent members of the community and had owned a bottling company. A company there was still called the Bing Bottling Company and manufactured a product called "Bing Soda"; however, it was not owned by the heirs of the original Bings.

My grandfather Bing was a relatively well-educated farmer for his time. Record keeping by farmers was a bit unusual at the turn of the century, but I remember seeing books that he kept showing the price that he paid for farm animals and implements and the price he got in return for crops that he sold.

My maternal grandfather was named Thompson. He had been a bandleader and conducted music at various revivals. This caused him to move about quite a bit. By mother was born as the family moved through Emporia, Kansas. When I visited Emporia several years ago, I looked at newspapers of that time to see if her birth had been recorded, but it had not. Both of my mother's parents died while she was still in high school. This left my mother with a younger sister and brother to be reared by kinfolks in Leonard, Texas.

My mother was quite eager to get out and make a living for herself. She did temporary teaching while still in high school (at age 15). As soon as she finished high school, she took examinations to qualify for a temporary teacher's certificate. My mother told me about how she went about looking for a job. Taking a pen in hand, she closed her eyes, moved her hand over a map of the state of Texas and put the pen down on the map. She did this three times and wrote to the three schools picked in this fashion. My mother received three job offers and accepted the first, located in Oakwood, Texas. This turned out to be the one paying the least, but since she had accepted it earlier, she felt a strong commitment to take the job. Soon after moving to Oakwood she sent for her brother and sister to come and live with her.

My mother contracted a disease during her high school years which caused the loss of one eye, but she was still quite popular and successful as a primary teacher in Oakwood. During her first year, however, she thought herself as nearly being fired when she introduced phonics. But, her enthusiastic persuasion caused the students to read beyond their parents' expectations and she became very respected. After several years of teaching in Oakwood, she married my father who was then superintendent of the Oakwood School District.

Since my father died when I was five years old, I do not have a good memory of him. However, my mother had great respect and affection for my father and much of what I know about him is probably something I learned form her. My mother stopped teaching after marriage. She and my father had two children of which I was the older. I also have a sister. Having a sister has been a valuable experience for me, although at the time my sister did not appreciate it — especially when I encouraged her to have a make-believe doll funeral.

I recall that I loved my father very much and have pictures of him putting me on a horse and giving me a ride. I do remember him telling me stories. During his final illness, I was taken to a neighbor's home and told of his death. I did not comprehend this, and on returning home, looed for him in the barn, under the house, and other places where I thought he might be hiding. Two stories about my Dad come to mind. Some of this I recall and some I remember only because I was told about it later.

During those days it was the responsibility of the superintendent of the schools to see that order was kept in the classes. The women teachers would send students that had been giving them unusual trouble to the superintendent for punishment. Corporal punishment was much more prevalent in those days than it is today and on one occasion my father resorted to whipping a boy for some bad offense. Several days later this boy, along with his father and two of his larger brothers, waylaid my father on the way home from school, intent on beating him up. My mother saw this and came running with sticks and stones. The men relented and left with a warning that they would get my father later.

My mother was very apprehensive for my father's safety and suggested he order a gun to protect himself. My father pretended not to be concerned and told my mother not to worry. However, my mother went ahead and ordered a pistol from the hardware store; not letting my mother know, my father also ordered a shotgun. Hence, we soon had both a pistol and a shotgun. As far as I know, these were never used during my father's lifetime. But I have heard that my mother carried the pistol in her purse and said she would shoot any man that attacked my father.

Several years after my father's death, my mother had occasion to use her pistol which led to her recognition as a marksman of some merit by the local gun club. Hogs belonging to someone down the street frequently broke into our garden. We repaired our fence and ran the hogs off, but a day or so later they would get out and come and root up the garden again. My mother decided that she would try to frighten the hogs by firing shots near them to keep them away. She got out the pistol and asked me to chase the hogs out; her accuracy, however, was not too good. As I chased out a hog and she shot the pistol, the hog fell over on his back and started kicking. Soon it was dead. My mother called the meat market and asked if they would get the hog because they did not know its owner. The daughter of one of the neighbors brought over a butcher knife and offered to help butcher the hog but the plan was not pursued.

Soon the story was all over town of how my mother had killed a hog — what a crackshot she was with a gun. The owner of the hog never came forward because he was ashamed that his hog had destroyed my mother's garden. As the hog began to stink, my mother had someone put a rope around the carcass and drag it off to where dead cattle were left. The men of the town had great fun telling this story. They had a gun club that was going away to engage in a contest, and were making my mother captain because she was so accurate at shooting. I am not sure of the significance of this story, but thought it quite interesting because it made such a deep impression on my mother. I do not remember my parents purchasing these guns, but I do remember the kicking hog.

Another story about my father, perhaps more pertinent, involved the local schools for the whites and the schools for the blacks. Although these schools were separate in those days, they were in the same district and hence my father was superintendent of both. He knew how to order books and do other things that were useful to the black schools. I recall that one day, when I was about 10, I met several black youths. They asked me if I was the son of Superintendent Bing. I said that I was. They said, "He was a very good man, he was always fair to us. You can never be as good a man as your father was." This made an impression on me.

My father was successful as a teacher and the Oakwood schools turned out many outstanding people during his tenure; one became president of Chrysler Motor Company. Several years before his death, however, my father decided to leave teaching and take up farming. He acquired several farms and a store and was quite successful at managing them. My mother said that my father was a workaholic. As a matter of fact, work may have led to his early death. One day he had some plowing to do and it began to rain. My mother insisted that he come in out of the rain but he said that he had just a bit more to do and continued plowing. As a result of this, he took influenza or pneumonia or some other disease of that time and died in 1920 leaving my mother to take care of two children — the older being just five.

The following story about my mother reveals something of her character. She was a strong disciplinarian and made it a practice of confronting students who had done something wrong and having them confess what they had done before revealing what she knew. This worked well most of the time, but I remember one case in which it did not.

I was playing in the backyard and a neighbor told my mother of something that the neighbor thought that I had done. My mother called me into the house and asked me what I had done wrong. I did not recall having done anything wrong, so my mother gave me a few paddles and told me to go off and recall what it was. I still was unable to decide what she wanted, so I received another paddle. This happened several times and finally my mother thought she would give a hint. She said, "What were you saying in the backyard?" Then I remembered that I had been pounding on a stick, driving it into the ground with a hammer and saying, "Go down, go down, go down." The neighbor had heard me and thought I was cursing and saying, "goddamn, goddamn, goddamn," and this is what was told to my mother. I remember seeing the remorse on my mother's face when she found that she had punished me needlessly. She went off and cried and tried to make up to me for the harm that she had done. This impressed me in two ways. She was interested that I did not use profanity, and she was very concerned when she found that she had wronged me. I long forgot the punishment but not these two things.

My mother felt that it was an excellent idea for mothers to train their children at an early age. Long before starting school, I was able to read and do arithmetic and regarded these things as great fun. I think I owe a great deal to my mother's early training for my interest and success in school.

When I started school, I had already memorized all of the primary books and found school very, very easy. During several months of the first year, I was ill, but by the second year I was put in the third grade. Although I had to work during my first few years to stay up with the class, by the time I had gotten to junior high school, I was able to lead my class. One thing that bothered me was my being younger than my classmates and hence at a disadvantage when competing in athletics with boys who were older.

I credit my mother with much of my success in mathematics. Before starting school, I learned that mathematics was fun. In the seventh grade, I entered the county number sense contest. My mother was coach of the team and she taught shortcuts enabling one to do computations quickly. We learned to approximate answers to harder problems. I later learned that my partner and I made the highest grade in the state in the number sense contest that year — more than likely due to my mother's coaching.

When I took geometry, my mother was quite interested in how I went about proving theorems. She taught me that the purpose of geometry was to discover proofs rather than to memorize them. I was never pushed in my pursuit of mathematics and wondered whether I would have liked it nearly so well if I had been offered a speeded-up course such as we give some of our better students today. Today, as well as then, I am grateful that I had the time to think, to contemplate and to work out mathematics on my own rather than to be pushed ahead to learn proofs that were provided by others.

College days

Several years after my father's death, our funds had been depleted to the extent that my mother returned to teaching to earn a living. (The pay was only $6.50 per month.) Yet, there was never a question as to whether or not my sister and I would go to college. The question was only how it could be afforded. I recall that one of our friends wanted my mother to borrow enough money to send me to Princeton. He offered to help in fundraising; however, my mother hesitated about this obligation and thought it wiser for me to attend a school that I could afford.

Two of my favorite high school teachers had finished their college education at Southwest Texas State Teacher's college, or Southwest Texas State University as it is now called, and arranged for me to get a job at San Marcos in the college cafeteria. This is the way that I supported myself during college. I was quite frugal; my mother and I figured it had to cost me less than $300 to get my bachelor's degree, most of which was for tuition. I did, however, complete college in 2 1/2 years — eager to finish in a hurry so funds could be used to send my sister to school. She also went to Southwest Texas State Teacher's College.

Several years after my sister and I had gotten our degrees from Southwest Texas State Teacher's College, and I had even received a masters and Ph.D. from The University of Texas, my mother, by going to school in the summers, also received a bachelor's degree from Southwest Texas State Teacher's college in San Marcos.

Student of R. L. Moore: Graduate school

After I finished college, I had teaching positions in three Texas high schools. Although these 4 1/2 years of teaching did not ad to my mathematical progress, it taught me how to get along with people. While teaching in high school, I went back in the summers and took courses at the University of Texas. It was there that I met Professor R. L. Moore. R. L. Moore appeared to have a very low opinion of courses in pedagogy or education, and I think he suspected I would not be very good since I had spent so long teaching in high school. I was soon able to relieve him of this notion, being able to prove theorems that his students, who had not taken time off to teach in high school, could not prove.

R. L. Moore was quite serious about his teaching, as he was about other things. When anyone talked to him, he looked at the person with penetrating eyes. I was careful to try to not say anything unless it was accurate. He was interested in what a person could do himself. He placed books off limits for his students when these books were dealing with things on which they could work themselves. As a matter of fact, in his course on topology he placed his own Colloquium book off limits.

Moore taught not only in the classroom but in the halls and elsewhere. I remember my landlord telling me how interested R. L. Moore was in my work and how Moore tried to explain to him some of the things I was doing. My landlord did not understand any of this. But this made a profound impression on me that Moore was so interested in my work that he would try to explain some of it to my landlord.

Mrs. Moore was a real asset to Dr. Moore. I remember Mrs. Moore telephoning my wife, Mary, and telling her that she realized what a sacrifice my wife was making in order to make it possible for me to spend so much time on mathematics. I believe the word "sacrifice" came as a surprise to Mary who thought running a household was a 50-50 proposition. But being told by Mrs. Moore that a wife might enhance her husband's career by letting him spend full time on it did have an impression. Mary has always been a big help in entertaining students, managing the family, and providing a pleasant environment.

In Moore's classes, he taught by indirection. I recall Moore having one of his ex-students visit his class. This ex-student had not produced as a research mathematician and after he left, Moore said, "How can I stand having a man like that visit my class? He had a real opportunity and he did not take advantage of it!" The students got the message.

Moore did not like for people to have sloppy appearances. He expected them to sit up straight in their chairs and to behave like ladies and gentlemen. Once he showed a picture of students at the University of Chicago who were being quite sloven and his remarks were, "Do you think the University of Chicago is that bad? Do the students there think so little of themselves as to behave in such an undignified manner?" Again, we got the message.

One of the teachers at The University of Texas had a wife who would come to the building after hours to pick up her husband and blow on the horn to advise him that she was there. Moore thought it was inappropriate to make noise around a building where research was being done. He would ask his students, "What do you think of a man who would permit his wife to come to the building and honk the horn outside?" Again, we got the message.

Moore was very proud of his students who had succeeded in research. He told us stories of how Ray Wilder had been able to prove a theorem in a short period of time. How Gordon Whyburn had had successes. How Burton Jones had been able to do such and such. How one of his students, Miss Miller, had succeeded where others had failed in proving a certain theorem. Again, we got the message.

Dick Anderson was a student at The University of Texas who got his degree several years after I did. He and I frequently spent part of the noon hour playing chess. Moore did not approve of this and told me that I should not waste Anderson's time which could be better spent on mathematics. I also got the message that Moore thought I could make better use of my time.

On another occasion in the halls someone had asked me who had won the football game the preceding Saturday and whether or not I knew the score. I remembered who had won the game, but I was unable to provide the score. Moore's remark was, "At least, thank goodness for that!"

I will always appreciate the good instruction I received under R. L. Moore. It was to my liking because I enjoyed working out mathematics for myself and this is what he encouraged. However, I did not like the close supervision he gave my thesis. When he had me write things, he wanted it in his own way rather than in mine. I felt very glad when my thesis was finished, for I now felt I had the author's prerogative of saying things the way I wanted to say them. Moore felt that if it was said correctly, it didn't really matter whether or not it was easily understood because it was the reader's responsibility to dig it out. My feelings were that we should say things, often repeatedly, in an effort to make it more understandable. Moore's conciseness in written presentation did not always carry over to the oral discussions since he took care to make them clear.

Early research at Texas

After receiving my degree from Moore, I visited him many times. We never discussed mathematics together. In one case, shortly after I received my degree from him, I proved one of the big theorems that had been baffling quite a few people; namely the Kline sphere characterization. After writing this up, I submitted it as an abstract to the American Mathematical Society and also to a journal without ever having shown or discussed it with Moore. When it became known that I had announced the result, we were flooded with phone calls, telegrams, and letters — mostly addressed to Moore — asking if I had gotten the result and if it had been checked.

Moore's reply was to turn these letters over to me to be answered because he had not seen proof. Yet, he did not ask me for the proof. I think that he thought that by asking me, he would show lack of confidence in my ability to check the proof myself. I also did not show him the proof because I felt that this might show that I did not have self-confidence and needed someone else to help me check the result.

At Chicago I announced the result. Others had announced partial solutions to the result, and before I got on the program I was offered additional time so I could better explain my proof. I was only able, however, to give the presentation that I had planned. I do recall Paul Erdös asking me at that meeting whether I had had a first-class mathematician check the proof. I rather resented this because I thought that it was a first-class mathematician who had provided the proof.

My thesis dealt with simple plane webs and I wrote several papers on this subject. I learned, however, that the mathematical community was not very interested in webs. There was much more interest in the Kline sphere characterization; at least in knowing that it was true. I found that in the case of hard theorems, the mathematical community is quite often interested in knowing whether the result is true. But many mathematicians will not go through the work of checking to understand the proof.

I spent two years at The University of Texas after getting my Ph.D. There were other interesting results that I got while I was there; one of the most interesting dealt with the pseudo-arc. Ed Moise had written his thesis about the pseudo-arc and showed that the pseudo-arc had the interesting property that it was homeomorphic to each of its nondegenerate subcontinua. That is the reason he called it the pseudo-arc. It turned out later that other mathematicians had described the pseudo-arc earlier by other methods. I was quite interested in Moise's description, but wondered if a different description might not show that the pseudo-arc was homogeneous and, indeed, found that this was true.

When Moise was still a student and I already had my Ph.D., I was reluctant to announce the result that the pseudo-arc was homogeneous and thought that Moise should at least have a chance to make such an announcement if her were close to discovering it. I asked him if he had considered the matter as to whether or not the pseudo-arc was homogeneous (knowing all the time that it was). He said that he had checked this and found indeed that it was not. So several months later, I announced that a pseudo-arc is homogeneous. I found that while some people were interested in the pseudo-arc, many were not because the pseudo-arc did not have the nice geometric properties of differentiability or linearity of many geometric objects. I decided that mathematicians were more interested in manifolds than they were in exotic objects.

Wisconsin years and mathematics of this time

Two years after taking my Ph.D. at Texas, I went to the University of Wisconsin where I had a very successful career. After two years at Wisconsin, I took a leave of absence and spent a year as a visitor at the University of Virginia. I had as an office mate, Ed Floyd, and learned quite a bit about manifolds from him. I taught a class there in which the members were Gordon Whyburn, Ed Floyd, Vic Klee, Truman Botts, and Bob Williams. This was a very interesting class and we studied current literature. I obtained several very good results during the year I was at the University of Virginia.

Floyd told me that people were interested in knowing whether there was an involution of a 3-sphere onto itself different from ordinary involutions. There was a suspicion that probably the union of two solid Alexander horned spheres sewed together along their boundary gave $\mathbb{S}^3$ and that an involution that interchanged these two solid spheres (or Alexander balls) would be an involution inequivalent to a standard involution. By the use of strings, rubber bands, and other methods, I was able to show that the union of two solid horned spheres sewed together along their boundaries with the identity homeomorphism was indeed a 3-sphere.

I found that many people were quite interested in this result. Since a proof was not very hard, others also learned the proof. It is only this year (circa 1984) that I have discovered a new proof which gives other results along this line. Mike Freedman and Richard Skora have made use of these new results.

The pseudo-arc is an indecomposable continuum. It is not the sum of two proper subcontinua. In fact, it is hereditarily indecomposable since each of its proper subcontinua has this property. A student from the University of Virginia, John Kelley, who is now at Berkeley, had shown that if there is a two-dimensional hereditarily indecomposable continuum, then there is an indecomposable continuum of dimension three, one of dimension four, and in fact, for each positive integer n, a hereditarily indecomposable continuum of dimension n. However, it was not known whether there was a hereditarily indecomposable continuum of dimension two.

I remember spending many hours at the University of Virginia trying to solve the problem of whether or not there is a two-dimensional hereditarily indecomposable continuum. As a matter of fact, late one evening, while I was still at my desk thinking about this problem, Vic Klee came by. I remember telling him, "I think I have this problem about solved. If I could put all of the ideas together that I have in mind, I believe that I would have a solution. I am going to sit here and work on this until my mind is able to put these things together and provide a solution." And, indeed, I succeeded; in fact, I was able to provide a proof quite different from that of Kelley's that there are hereditarily indecomposable continua of all dimensions. The example constructed was an exotic continuum and not so many people were interested in it as they were before the problem had been solved.

I have noted that many other results that are of considerable interest before they are proved meet similar fates. Dick Anderson and I gave an elementary proof that there is a homeomorphism between Hilbert space and the cartesian product of a countable number of lines. The question of whether or not there is such a homeomorphism had been of interest not only to topologists but also to many analysts since Hilbert space was in their arena. However, after the result was proved, interest waned, and many who had previously seemed deeply interested, now were willing to accept the result without caring to know why it is true.

A somewhat similar reaction met the discovery that all 3-manifolds can be triangulated. Many topologists now accept the result without knowing a proof that it is true. A complicating factor is that there are no known elementary proofs. I have great respect for those that understand the basis of their own work.

I have long been interested in metrization problems. When I was a graduate student at The University of Texas, I worked on the problem as to whether or not every normal Moore space is metrizable. This question had been called to Moise's attention when he was visiting a meeting of the American Mathematical Society in New York. I noted that an affirmative answer could be given by using a result of F. B. Jones. Jones, however, did not remember how his original proof went and began to suspect that maybe it was false. Others took up the hunt for the solution. On different days we heard the rumor that various Moore students had either proved or disproved the theorem. In fact, Moore himself let it be known that he had a solution. An examination of his archives in the Humanities Research Center at The University of Texas showed that his write up had a gap.

I found many results about normal Moore spaces and finally decided to publish a paper telling what I knew, even though I had not gotten a complete result. For example, I published a paper which showed that a Moore space was metrizable if it was collection-wise normal. For those wanting to know what this means I remark that a Moore space is a regular Hausdorff space which has a countable family of open coverings $G_1, G_2, \ldots$ such as that if p is a point and N is a neighborhood of p, then there is an integer n, such that each element of G_n that contains p lies in N. A space is collection-wise normal if for each discrete collection of closed sets, there is a collection of mutually exclusive open sets such that each of the elements in the discrete collection lies in one and only one element of the collection of open sets.

My paper contained the result that a Moore space is metrizable if it is collection-wise normal. It also contained a necessary and sufficient condition telling when a Hausdorff space is metrizable. It turned out later that both Smirnoff from Russia and Nagata from Japan had gotten similar results at about the same time. This suggests that when it is time for a new result to be born, and that if one person does not get it, another person is likely to. No research mathematician is indispensible for progress in mathematical research.

A square has a convex metric; namely, if p and q are two points of a square, there is another point of the square halfway between them. People have long been interested in the question as to whether or not each compact connected, locally connected metric space has a convex metric. Karl Menger had been very interested in this problem. Gustav Beer had what some people thought was a solution; however, others found fault with his proof. He submitted his paper to a journal many times and each time it came back with suggestions for corrections. Menger told me that the decision to publish the paper was made so that it would have a wider range of critics. I examined this paper and whether or not it had errors, I did not know. But I did see that Gustav Beer was considering only the problem where the space under consideration was one-dimensional. After studying Beer's paper, I decided that indeed a locally connected metric continuum of any dimension could be partitioned and this partition could be used to show that the space had a convex metric. Moise obtained a related result at about the same time but his proof had a minor error.

Although for many years I was quite interested in the normal Moore space problem, I lost some of my interest when Cohen proved that the continuum hypothesis could not be proved on the basis of the Zermelo-Fraenkel axioms. It was later proved that the normal Moore space problem could not be solved on the basis of these. It seems like there were two points of view. One was that something could be considered as true if it could not be shown to be false even if in a larger space it could be proved false. Another point of view is that a thing can be considered as false unless it can be proved to be true in the space under consideration. The notion of forcing is related to these ideas.

While we might find something that at present we do not recognize as an axiom, the above does show that on the basis of the axioms that we do recognize, we are unable to prove all theorems in mathematics. In other words, mathematics cannot be based on axioms (as we know them) alone.

I will now tell of an idea I got as a boy that helped me in later research. I was assigned the task of drawing a map. One way was to take a map, put it on the window, place a piece of paper over it and then trace. I had more difficulty, however, when I was asked to draw a map of a different size. However, I found that if I divided the page containing the original map into rectangles and then similarly divided the paper on which I was to draw the map, then I could make a reasonable copy of the map without resorting to the use of the window. In fact, even if the paper on which the drawing was made was of a different size from the original map and if one put the corresponding parts into the corresponding rectangles, then one got a reasonable copy of the original map, especially if the rectangles were small. A mosaic copy of a picture looks somewhat like the original.

I noted that a person could take a map whose boundary lines were curved (or even squiggly) and approximate it with a map whose boundary lines were polygonal; then if one did not examine the changed map too carefully, it resembled the original. This is the idea behind the fact that if one looks at a mosaic from a distance, it looks like an object which is not polygonal. Also, if one looks at a TV set, one does not see the dots, but a picture.

Let me discuss how I used this notion many years later. In the 1950's, I learned that Moise had proved that if h is a homeomorphism of a 3-cell into Euclidean 3-space, then this homeomorphism could be approximated by a piecewise linear homeomorphism. This result has many applications, and I felt that it was very important. Reading Moise's papers proved very difficult; in fact, I have heard that others had difficulty in reading these papers, even the referee whose name was Hu. Hu is said to have met with Moise frequently in order to have Moise explain the proofs to him. There is somewhat of a related joke.

Some people make the remark that Hu (who) refereed Moise's papers. It sounds as though they were asking a question, if "Hu" was mistaken to mean "who". "Hu refereed Moise's paper. Who? Hu! Who? Hu!"

The first step in Moise's proof seems to show that if h is an embedding of the cartesian product of a 2-sphere $\mathbb{S}^2$ and an interval [0, 1] into $\mathbb{R}^3$, then there is a polyhedral 2-sphere in this image which separates the two boundary components of $h(\mathbb{S}^2 \times [0, 1])$. His proof goes something as follows. Let X be the closure of the bounded component of $\mathbb{R}^3 - h(\mathbb{S}^2 \times \{\frac{1}{2}\}$. The first Vietoris homology of $\mathrm{Bd}(X)$ is trivial. In a sequence of steps Moise changed X to a polyhedral 3-manifold M with boundary so that $\mathrm{Bd}(M)$ has trivial first Vietoris homology and separates $h(\mathbb{S}^2 \times \{0\})$ from $h(\mathbb{S}^2 \times \{1\})$ in $\mathbb{R}^3$. Then $\mathrm{Bd}(M)$ is the required 2-sphere. Using this as a start, he showed that any homeomorphism of a 3-cell into $\mathbb{R}^3$ could be approximated by a piecewise linear homeomorphism.

As I wrestled with Moise's paper, I decided that there might be a different way of proving the same result and recalled the techniques I had learned for copying a map when I was a boy. Hence, I decided to triangulate $\mathbb{R}^3$ and use the 3-simplexes as I had used the rectangles. I regarded $h(\mathbb{S}^2 \times \{\frac{1}{2}\})$ as the boundaries of the states and produced a polyhedral 2-sphere approximating $h(\mathbb{S}^2 \times \{\frac{1}{2}\})$ by aping the methods I used as a boy to copy a map. With this step I got the same results that Moise did and was able to make the same applications that he did; namely, that a homeomorphism of a 3-cell into $\mathbb{R}^3$ can be approximated by a piecewise linear homeomorphism and that any 3-manifold can be triangulated. Later I was able to prove the side approximation theorem and got even more results. Many papers resulted from the side approximation theorem. I suspect that my results on the approximation theorem and the side approximation theorem even have a larger impact on 3-dimensional topology than any other of the results I have obtained in this area.

Collaboration with others

Let me make some remarks about collaboration with other mathematicians. First, I would like to talk about collaborating with one of my own students, Jim Kister. I mentioned to him as I have to my other students that it was the responsibility of my students to keep me informed of what was going on as I became older. Kister and I were visitors at the Institute for Advanced Study at the same time and we discussed the embedding of objects in Euclidean 3-space. I was pleased to learn that Kister had learned many things since I had him as a student, and we put some of these notions into a joint paper.

Another person with whom I wrote a joint paper was Ed Floyd. When I was visiting the University of Virginia and had Floyd as an office mate, I discussed with him partitioning of sets and how the notion of partitioning might be used to duplicate some of the things we knew about manifolds. Now, the triangulation of a manifold is a special example of a partitioning of a 3-manifold. Floyd knew much more about manifolds than I did and as a result of some observations of his, we were able to use the notion of partitionings and expand them.

Burton Jones and I also wrote a joint paper. Actually, this paper did not result from joint work, but resulted from the fact that both Jones and I had gotten some results about the pseudo-arc of the same nature. It seemed inappropriate for Jones and me to write separate papers covering essentially the same topic. We then decided to write a joint paper with each of us writing one part of it based on work we had done independently.

Two joint papers were written with Karol Borsuk. One of these resulted from a discussion we had in Warsaw, Poland, and the other from discussions we had when Borsuk was a visitor at the University of Wisconsin. I was amazed by the amount of knowledge that Borsu had about 3-space and the dedication with which he worked at mathematics. I learned a great deal from Borsuk — not only about mathematics but what devotion to mathematics really means.

Still another person with whom I wrote a joint paper was Joe Martin. We were both working on the Poincaré conjecture and had frequent discussions about it. Most of the work in our joint papers was done independently.

I have received much satisfaction in writing joint papers with Dick Anderson, Steve Armentrout, Woody Bledsoe, Karol Borsuk, Mort Curtis, Ed Floyd, Burton Jones, Jim Kister, Andrzej Kirkor, Vic Klee, Joe Martin, Dan Mauldin, Mike Starbird, and others. Mathematics has been produced and friendships strengthened. But, much of the joint work was created when partners were working separately. In general, I think better results are obtained by those who stand on their own feet and work alone.

Teaching methods and related philosophy

Let me make a few remarks about training students and how my methods of training are similar as well as different from R. L. Moore's. We both try to train students by having them *do* mathematics. Moore, however, would have his students work on things in which he was interested — whether or not these were of great interest in the mathematical community. I have tried to lead my students to work on things that were of general interest to other mathematicians so that if they were to go to any one of the leading mathematical centers in the country, they would find people with kindred interests.

My method agreed with Moore's in that I tried to get students to discover their own thesis rather than assign these topics to them. As we study papers I frequently ask them, "Why did not the person writing this paper prove more? What would happen if one changed the hypothesis? Are there examples that would show that the theorem is false if something is dropped?" If a person studies a paper in this frame of mind, most good papers will lead to other research.

Also, like Moore, I tried to make all of the students in my classes participate in proving theorems. Those inept at proving theorems can at least have the experience of trying and encountering difficulties before seeing the proofs of others. They can learn (and perhaps appreciate) definitions. However, I give greater praise to those who can make original contributions themselves, who can discover examples on their own and unearth new attacks or new theorems. I recall one unusually successful class of 40 students I taught at the University of Wisconsin. The class was supposed to meet 3 times a week. I lectured to the students for one of the periods and divided the class into the other 2 periods, letting half of them come one day to give their results and the other half come another day for their results. Several members of this class went on to get Ph.D.'s and to produce even more significant results. I can't help but think that this technique I used encouraged them.

Another innovation I have used in teaching seminars, particularly when studying new results, was to have the seminar meet for about 2 weeks and on the last day only those people who could bring a conjecture related to what had come before were permitted to attend. The idea being that those who were not able to come with a conjecture had not been thinking enough about what was going on to merit being permitted to attend the final conjecture session.

I have a basic feeling that mathematics is fun and should be fun for the participant. It does few people any good to force feed them into learning things that they do not enjoy. They are unlikely to make much use of the material they learned — only having to do it in order to make a grade or meet a requirement.

At lower levels students will not be able to prove many theorems, but they should be able to show what they have learned. We should be interested in what they are doing and praise them for their progress. I do not have much sympathy for programs where we try to give a strong dose of mathematics to students who find mental activity distasteful. What I do have great sympathy for are those who do not find mathematics easy. If I were to limit my friends to those who are good in mathematics, I would lose many of those that I have. I believe we should strive to have students enjoy mathematics whether they are gifted in it or not.

Let me make a few remarks about teaching. I think that as we lecture to people, we should try to put ourselves in the position of the audience and try to see how its members view what we are saying. I like to be able to look into the eyes of my audience so I can gauge listeners' interest. There is no need to lecture to ourselves and go off and leave students. It seems that too many teachers are lecturing to themselves.

Not just in lecturing but in talking to people about mathematics and other things, one should try to look at things from their point of view. I've heard that a person does not really understand another's problems until they have stood in that person's shoes. I recall a word of wisdom given to me by a cleaning woman working at my home. She was a member of a minority. I had heard another member of that minority say something that sounded very ridiculous — something that I thought even reasonable members of that minority would not accept, and discussed this with her. She said this, "You should listen to what people feel rather than to what they say." This is good advice.

from R. H. Bing: *Collected papers*, vol. 1, edited by S. Singh, S. Armentrout, and R. J. Daverman, American Mathematical Society, Providence, RI, 1988. MR 950859 Zbl 0665.01012.

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