The Princeton Mathematics Community in the 1930s
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MERRILL FLOOD

(with ALBERT TUCKER)

This is an interview of Merrill Flood in San Francisco on 14 May 1984. The interviewer is Albert Tucker.

Tucker: Merrill, how did you happen to go to Princeton?

Flood: I did a master's thesis in number theory at the University of Nebraska with Professor Tracy A. Pierce. The only place he thought I should go was Cal Tech to work with E.T. Bell. I tried to get to Cal Tech, but they weren't offering any money. So Tracy said my second choice was L.E. Dickson at Chicago. I got in touch with Chicago, but they weren't offering me any money. Then Tracy said that my best chance to get money was at Ohio State University. I wrote there, and they were indeed willing to give me some money. But by then I had decided that I'd like to go to the best university I could get to. He and I discussed this, and we agreed that it was Harvard. He said, "I don't know anyone at Harvard in number theory, but it's a good mathematics department." So I had some correspondence with Harvard. They said they'd be glad to have me. They had no money, but after I was there a year I might get a good fellowship.

I forget why I wrote to Princeton, but I did. They required some letters of recommendation. So I got the best people I could at the University of Nebraska to write letters of recommendation. One of them was Orrin Stepanek, my professor of English. I'd taken English as my major and math courses just for fun. Sure enough, Princeton came back not only with an offer of admission, but also with money. Then I discovered there wasn't anybody there in number theory. Tracy Pierce said, "Don't worry about it. It's a good place, and something will work out." So I went to Princeton.

Tucker: Did the fact that [Solomon] Lefschetz was once at Nebraska have anything to do with it?

Flood: I can't remember, Al, but I believe it did.

Tucker: The first job that Lefschetz had after he got his Ph.D. in mathematics at Clark University was at the University of Nebraska. I think he told me that he had to teach 18 hours a week.

Flood: That sounds right. I was teaching full-time, and that was a heavy load with little pay. That may have had something to do with Lefschetz's leaving.

Tucker: He was there I think a couple of years before he moved to Lawrence, Kansas, where he got a little better treatment—also a somewhat better salary so that he was able to get married.

Flood: I have no recollection of that, but I do know that the people at the University of Nebraska were aware of Lefschetz. I think it was Meyer G. Gaba who was aware of Lefschetz. Gaba was the one who got me into mathematics, because when I took calculus in my junior year Gaba became ill, and to my great surprise and pleasure he asked me to take over the course. I had never taught and had never taken calculus. That got me into mathematics. I had been taking it just for fun, because I found English literature not very exciting. I think maybe it was Gaba who knew about Lefschetz. Whether he had an effect on the other end, I don't know. Lefschetz may have liked having someone from Nebraska.

The reason why I told you about this Stepanek matter. I don't know what the other matters were, but when I went there I became an assistant to Dean Eisenhart. Somewhere along the line, I forget just when, probably a few years later, Dean Eisenhart let me know what Stepanek had said. Stepanek had said, "This is the finest young man to come out of Nebraska." That was of course false, but it might have helped.

Tucker: What were your first impressions of Princeton? What year was it that you arrived?

Flood: 1931.

Tucker: I'd been there two years.

Flood: My first recollection of Princeton was after I got out of the railroad station. I carried my luggage up to Nassau Street and went right into a barber shop to get my hair cut. My first introduction to how Princeton operates was by this talkative, helpful barber. He told me that I should go to the Nassau Inn to eat. He told me how to get to the Graduate College, where I was going to live.

Tucker: '31-'32 was the first year of Fine Hall.

Flood: It was in use. I arrived in September.

Tucker: I remember Veblen called me into his office at the beginning of that year and said, "You're the chairman of the tea club."

Flood: The tea club was a great institution.

Tucker: He said, "You've got the Procter Fellowship, and it's the duty of the Procter Fellow to be chairman of the tea club. All the rest of the fellows are supposed to help you." I objected. I wasn't very keen on tea. He said, "Well, have coffee or cocoa or whatever you want."

Flood: The tea club started then?

Tucker: It started when you arrived.

Flood: There hadn't been one in the previous building, which I'm told was Palmer?

Tucker: Before that the mathematics department was in two or three rooms in Palmer, the rooms immediately to your right as you came in the front entrance. There were two fair-sized rooms there with the mathematics library on the shelves on the walls and with tables in the middle of the room where people sat and talked.

Flood: Pretty cozy compared to Fine Hall.

Tucker: Yes, and Veblen had a room on the second floor. The rooms on the main floor had numbers in the 200s, so those on the second floor had numbers in the 300s. Veblen's office was intended for a physicist. It had laboratory equipment in it. Veblen used it, along with a few of his students: they used to make tea on a bunsen burner. I was never one of that group; they were mainly the British students who were around—people like Henry Whitehead and Banesh Hoffman.

Flood: I like that custom.

Tucker: With the building of Fine Hall a common room was designed to hold tea in. Tea became something for everybody. I remember the man who helped most in getting the tea club organized, John Landes Barnes. He was the treasurer of the club. We bought our own cookies from the National Biscuit Company. They delivered wholesale right to Fine Hall. John worked out an index on each kind of cookie; this was essentially what cost least. He wanted to get the most for the money.

Flood: John and I were close friends, and we tried the first year I was there—when I was still single—unsuccessfully to find good double dates. They were always Trenton girls. I'll never forget, always very frustrating and unsatisfying. But later, only a couple of years later, John met his future wife (the mathematician Mable Schmeiser) through us in our home.

Tucker: That was when you were on Nassau Street, 301 or something like that?

Flood: No, it was a year or two after when we were on some other street. I forget the name now.

Tucker: I remember that when you had that place on Nassau Street you had several graduate students renting rooms.

Flood: Ed Titt, George Garrison, George Ball, Frank Cubello, and Alice and me in a 2-bedroom apartment. Alice and I slept in the dining room. Two of the bedrooms were very small, occupied by Cubello and Ball. Garrison and Titt shared the only decent-sized bedroom. We had a great year. One bathroom.

Tucker: That was the year, '32-'33, when I was off on a post-doctoral fellowship.

Flood: The year '31-'32 I lived in the Graduate College. I shared a room with Ed Beckenbach.

Tucker: That's when we played a lot of tennis.

Flood: Ed had coached tennis at Rice, and I had a very bad Nebraska style, chop and lob game. I had a good serve, and Ed sort of taught me how to play tennis. Tommy Tompkins, Joe Hirschfelder, and you and Ed were among my regular opponents.

Tucker: Do you remember some of the courses you took? Were there any that appealed to you?

Flood: Yes. I'd hoped to go into number theory. It became plain to me that only Einar Hille had real interest in number theory. I talked to Hille and found that he had no interest in classical number theory. So I realized that my only hope was to do something besides number theory.

I quickly learned that the thing you should do was analysis situs. My friend John Vanderslice worked with Veblen, and Alice and I got socially acquainted with the Veblens. They liked us and we liked them. So I decided it would be nice to work with Veblen. But when I learned a tiny bit about analysis situs from Vanderslice it didn't seem like my kind of thing. The only alternative I had was algebra. So I took the course with Wedderburn and liked it. I didn't like Wedderburn because he was almost completely unavailable for any kind of interaction. But I took his course, and at the end of the year I chose a problem he had proposed in class. I worked on it all summer in Nebraska and came back with some preliminary results. From then on I was dedicated to algebra. Of course Wedderburn was nice but extremely difficult to work with.

Tucker: Do you remember the start of the Institute for Advanced Study?

Flood: I don't remember too much. I remember I was excited that all of these great people were going to be there. I remember an awful lot about all these people, in all sorts of ways, but I don't remember anything about the administrative side.

Tucker: Bitterness is probably too strong a word, but there was a negative feeling on the part of Eisenhart and Lefschetz and Robertson that the core of the Department had been stolen by the IAS—Veblen and Alexander and von Neumann.

Flood: I was unaware of it as a graduate student.

Tucker: I was away that first year, so I was not aware of it either. But years afterwards I understood that Veblen had made some disparaging remarks about the fact that the Princeton Mathematics Department wasn't going to amount to much. This got Lefschetz fighting mad, and Lefschetz was determined to make the Department as strong and active as possible.

Flood: I don't think my friends and I as graduate students were aware of this at all. You may have been.

Tucker: I wasn't at the time. There are things I learned afterwards. Eisenhart, of course, was so involved in the University administration that he did little of the actual administration of the mathematics department. But Lefschetz, as the Fine Professor, was the one who, as I look back on it now, really held things together and made things go. Eisenhart supported him, but it was, I think, a supporting role that Eisenhart played.

Flood: I was unaware of those kinds of issues. I was Eisenhart's assistant my first year. My only function was to fill in formulas for the papers he published and do other miscellaneous chores. It was easy work. It didn't take a lot of time. I enjoyed it, and he was nice to me. And I took his courses, and I enjoyed them. I enjoyed both differential geometry and Riemannian geometry, but I never felt they were the kind of thing I wanted to go into. First of all, my geometrical abilities have always been minimal. I've never, even with the outside help of my good friend Jimmy McShane, been able to develop any kind of intuition that you need to be a good person in geometry. That didn't seem to be my thing.

Tucker: Why do you mention Jimmy?

Flood: He and I spent a great deal of time together. With his wife, Virginia, the four of us were close friends. More than anybody else it was Jimmy in my second, third, and fourth years who helped me sort out my interests in mathematics. To think about it the right way. He was extremely helpful as a friend, and I think he had more influence on me than anyone else. Barkley Rosser also had a great deal of influence. We were co-graduate-students and worked together. Except for reading his book and seeing him maybe all together five times, Wedderburn had little to do with the work I did. Unfortunately I was very unhappy about that. There was nothing else I wanted to work on except that problem in the theory of matrices. I stuck with it and finally got through it.

Tucker: I guess Wedderburn had only about four Ph.D.s.

Flood: My understanding at the time was that I was the first one who formally started to work with him. I think Nathan Jacobson finished with him before I did, but I started with Wedderburn first. When I heard this problem that he proposed every year, I said, "I'll work on that." Jacobson said, "That's a waste of time. Nobody else has ever worked on it, and it's sort of a hopeless problem." I didn't know better, so I worked on it and got some nice results.

Tucker: Was MacDuffee around Princeton then?

Flood: Yes, he was there as a visitor. I knew MacDuffee pretty well. I spent some time with him talking about things, but that was after I'd been into the dissertation. It might have been the last year of my graduate work or shortly after.

Tucker: MacDuffee had, I felt, a certain rapport with Wedderburn that other people didn't have.

Flood: J.L. Dorroh was the one who was most valuable in that respect. Both my last year as a graduate student and the year after, when I was still doing research in algebra, Dorroh and Wedderburn had very good rapport.

Tucker: And George Garrison?

Flood: Yes. He did his thesis later on.

Tucker: And then Ernst Snapper.

Flood: I think that's all.

Tucker: Yes.

Flood: I'm not sure of this, Al, but I think the only person who ever read my doctoral dissertation, besides myself, was Ernst Snapper. I don't think Wedderburn ever read it. He knew what was in it because we talked about it. Ernst told me he read mine. It was a short dissertation. It was published in the *Annals*, eleven pages. I'm ashamed of it now because I tried to make everything as unreadable as possible. It should have been a 20 or 30 page dissertation. So I don't blame anybody for not reading it. I don't know why Ernst read it.

Tucker: I saw a letter that Ernst wrote to a doctoral student [Karen Parshall. A.T.] of Herstein's at Chicago. She had written a somewhat historical thesis on the early days of modern algebra in the United States and got very interested in Wedderburn. She had read Wedderburn's papers and so on, but she wanted to know who this person Wedderburn was.

Flood: Recently she wrote to me. I answered her and gave her an honest statement.

Tucker: Ernst did too. He sent me a copy of the letter, about three pages. Ernst speaks warmly and enthusiastically about Wedderburn. I don't mean that you don't, but you say you felt you had no rapport with him. Yet the way Ernst writes, I think he did.

Flood: I had no intellectual rapport with him. In a typical session—and as I say there were probably no more than five all together—I would present my results to date. He'd make some very good comments abut what I might do next, but I couldn't get any conversation with him started. Then I'd go off and take a week or two to understand what he had said. It always turned out helpful. After I finished my degree, I used to have dinner with Wedderburn from time to time at the Nassau Club. We became pretty good friends, but we'd never talk mathematics. I never had any opportunity to have an intellectual interaction with Wedderburn either during my graduate study or after.

Tucker: I learned more about Wedderburn after he died. He died at home, and his body wasn't discovered until two or three days after. A neighbor said she hadn't seen or heard anything, so the house was searched. He had dropped dead of a heart attack. He had no family in the United States. The only representative he had was the Princeton Bank and Trust, which was his executor. He had specified in his will that his papers and books should go to the Mathematics Department, for whatever the Department wanted to do with them. So I was given the job by Lefschetz, who was chairman, to go to Wedderburn's house and search it for papers, possibly papers that had not been published and were worthy of publication. So I went through all his belongings. I spent hours doing that. He had scrapbooks, and photograph albums, and all sorts of things that gave me a much closer feeling for him.

He apparently belonged to this bachelors' club. The bachelor members of the faculty had a club in a building quite near the Nassau Club that was called the Bachelors' Club. They had meals together, and apparently at one time Wedderburn had gone on camping expeditions with Robert Root, an English-department lifelong bachelor, and Radcliffe Heermance. There were pictures of them sitting around a campfire.

Flood: I remember. I didn't know they were bachelors.

Tucker: Another thing: in his last years Wedderburn was a bit out of his head.

Flood: Is that partly because of his paranormal psychology interest at one time or another? He talked to me about that. I tried to avoid it, but I talked to him about it.

Tucker: He also attempted to do a dictionary of the ancient Scottish language. What was it called? [Erse. A.T.] Anyway, he started to work on a dictionary of the ancient Scottish language into English.

Flood: After I got my degree I tried, unsuccessfully, to get him to talk about his belief that everything is finite. I never could get him to tell me his ideas about this. Were they ever published?

Tucker: No.

Flood: But he was a neat person as far as I'm concerned. The bad thing that happened was that when I took my prelims for the first time he unfortunately couldn't attend. Alonzo Church, who was a close friend and neighbor of mine, substituted. Alonzo didn't ask any questions that I had learned the answers to. The questions were right out of Bocher, and Wedderburn never taught us any of those things. I didn't know any of the answers. I did very badly, and a year later I was allowed to take prelims again. Wedderburn was there then, and I could answer the questions and managed to pass.

But after I was failed the first time, Lefschetz called me in and told me I had failed. He said, "The faculty have considered it, and we want you to stay another year and take prelims again." He added, "We don't do this very often; it's only happened twice before." I said, "Well, I'm glad to hear it. I'd like to stay, and I'll try to do better next time." He said, "You might like to know that those two men were Karl Compton, now President of M.I.T., and R.D. Carmichael, now head of the mathematics department at Ohio State University. I said, "I hope I turn out as well as they did." This was encouraging, and I did work harder the next year. I think I did a pretty good job at the prelims the second time around.

Tucker: Didn't you have H.P. Robertson also on your committee?

Flood: I don't remember. I don't remember the second time at all. I know Wedderburn was there.

Tucker: Robertson had the reputation of being particularly rough.

Flood: What I recall of the first time was Alonzo Church. He asked me questions I'd never heard before. Incidentally, one of the questions he asked me concerned "resultants" that I later wrote a couple of good papers about. So maybe it had a good effect after all. Church was a very good friend of mine, and there was nothing bad about these questions. They were just ones I didn't happen to know anything about. Actually, after the second time Church told me he was impressed with how well I did on the questions on logic.

You asked earlier what courses I liked best. It was Church's logic course. In that course there were some very good people: Barkley Rosser, Steve Kleene, Bill Randels, Dick Pieters, and Kurt Goedel. I learned a lot, and I really enjoyed it. But I didn't feel that I would do well doing research on that kind of logic. I came close to going into that. I liked to work with Church, and I liked him very much, and he was a wonderful lecturer, but it didn't seem to be quite my kind of thing. At times since I've regretted that I didn't go into it, because I would have had others to work with—Rosser, even Goedel, even

Church. Whereas with Wedderburn I was entirely on my own, except for the collaboration with Rosser. Rosser was very good in algebra, very good in physics, and a very good friend of mine.

Tucker: Now let's turn to such things as poker.

Flood: Well, when you get right down to it I think I might have done much better on my first prelims if I had played less poker. I was married, you know, after the first year, and part of the reason I played poker was that we knew we were going to have a child at the end of that year and money was a little short. I always made lots more playing poker than I got from my salary. We used to play all night. The janitor would come and sort of chew us out at six in the morning. I forget who the main participants were, but I know S.B. Myers was one. Jacob Yerushalmy was a perennial loser. I enjoyed these perennial losers, but mostly I made money.

Tucker: Didn't Robertson play occasionally?

Flood: Yes, but not in our group. We tried to get Robertson and von Neumann to play. Von Neumann did a couple of times. He was terrible. I don't think he liked to lose.

Tucker: He was very competitive.

Flood: There were two who won the money most of the time—Myers and me. The others were suckers. In fact, I still have IOUs from famous people. That's part of the game.

Tucker: And then Eisenhart put a stop to it.

Flood: No, he never put a stop to it. Quite the contrary. Remember, I was his assistant the first year. I was told, I think reliably, that there was quite a bit of complaint among the faculty. Eisenhart defended the poker entirely and said, "They're grown young men. If they're going to use their own minds, it's up to them to do whatever they think is best." That's the story I got. Certainly nobody said anything to me. I must admit that after I heard that story I played more poker at home and less at Fine Hall. But I spent huge amounts of time playing poker and tennis, when I should have been up in the library. However, I didn't do that well in game theory that year—no connection there—but I think the general environment was helpful in getting us interested in game theory. In other ways too. We invented lots of games.

Do you remember I invented the game "Goofspiel", which was popular for a while? And Vanderslice invented nonholonomic chess. We played that a while. Hassler Whitney spent his time stacking chess pieces on top of one rook, and P.A.M. Dirac spent a great deal of time trying, unsuccessfully, to find how a king could get by eight pawns. I remember Stan Ulam was a pretty good bridge player. He spent a lot of time trying to find a hand with which nobody could avoid going down two tricks. Going down one wasn't too hard; I don't think he ever got to three. As young graduate students we were part of a crowd like that, with Dirac who was already famous. Whitney was very good. Adrian Albert, who also was very well known, played bridge. That was not during my first year; it was later. All this helped me to feel free to deal with games in all sorts of ways, but I don't think I was interested in game theory.

You may be interested to know how that happened. After I finished my degree, which was late 1935, von Neumann gave a lecture in Palmer to undergraduates. I don't know what the title of the lecture was, but I went because of von Neumann, whom I'd come to know well. He lectured on the minimax theorem, although he didn't call it that. In fact, he didn't tell us that there was such a theorem. He gave us examples of how mixed strategies could be used in games. It made a great impression on me, and I remember going to Kleene and Einstein and half a dozen other people to find out if they had ever heard of that. None of them had. I then asked them if, after thinking a while, they could propose a way to deal with this topic. Nobody came up with the idea of a mixed strategy among all these bright people. That convinced me that that's a subtle thing.

About a year later Tommy Tompkins asked me to lecture to the undergraduate students in mathematics. I announced a lecture on the theory of games, something like that. Tommy, without telling me, advertised it as "How to win at games of chance". I still have a copy of that paper, by the way. The large auditorium would not hold all the students that showed up. I then showed them how to play 2-handed poker, using what I'd learned from von Neumann. I presented some calculations on the game of dice and on some bridge things. Clifford Mendel had developed the mathematical for contract bridge. He and I had been playing a great deal of bridge, and I told them about that. Well, the students wandered out gradually. They went because the announcement had said that the theory would help them play these games, and of course it doesn't do that. I was told later on that parents complained officially to Dean Eisenhart about such a lecture being given. Again, Dean Eisenhart handled it terrifically, and I never heard about it after that. That's what got me interested in game theory.

Tucker: I remember that series of talks organized by Tommy Tompkins. They were done at the urging of Eisenhart, because Eisenhart was always concerned that there were not enough students majoring in mathematics. He was always looking for ways to lure students into a mathematics major.

Flood: How many did that room hold?

Tucker: The large lecture room held something like 90.

Flood: I had 100 people at the start, and there were 50 when I finished talking. It was half full when I finished talking.

Tucker: Another one of the talks in that series which was unusual was one given by John Wheeler. It was a talk on nuclear fission, which became a classified subject within a few weeks of when he gave the talk. He had been working with Niels Bohr in Copenhagen. That was some of the early thinking about the possibility of nuclear fission.

Flood: I should say, Al, that for some reason I didn't know at that time of the existence of von Neumann's 1928 paper on the minimax theorem. In order to have it in the appropriate form, for the continuous case, I ended up doing it myself without realizing that it was already a general theorem. So I went to von Neumann, told him I was interested in this topic, and asked him if he could tell me more about it. "Oh yes," he said, and he handed me a 20-page manuscript in his handwriting in Hungarian, which was all he then knew about game theory. I think I thought of the name 'game theory', and I had that paper for a couple of years. It was Oskar Morgenstern who told me that it was in some dialect, not standard Hungarian, and that only people who were part of that group in Hungary could read it. I could not get Oskar to help me read it, and Morris Knebelman wouldn't help me read it. I was never able to read the darn thing. I was too reticent to go and persuade von Neumann to give me the time, which he would have done. In about 1940, or maybe even 1941—I had that paper for several years—he stopped me one day and asked if I still had it and would I give it back to him. I guess it was then that he began to collaborate with Oskar, because they went way beyond that original paper of his.

Tucker: The 1928 paper appeared in the *Mathematische Annalen*.

Flood: Yes, but I did not know of it because he did not mention it in the references. I did not know the minimax theorem existed. I had learned on my own that there was such a theorem without ever proving the general theorem. The first I knew about that work was when I wrote that paper in 1944 and you told me there was a book coming out by von Neumann and Morgenstern. I had not seen the book. Incidentally, in that paper of mine, which was republished later as a secret report called "Aerial bombing tactics", I used the continuous poker game with respect to the bomber and the defender still not knowing anything, except the one item, of the work of von Neumann. I should have pursued the matter with von Neumann. I regret to this day that I didn't, because I'm sure he would have helped me. In fact, he told me that. I might have gotten into game theory much sooner.

Tucker: What other things can you think of?

Flood: Let's see. The best things are things that I know something about that you are not apt to get otherwise. For example, Marston Morse was a great friend of mine. I don't remember when I first got to know Marston.

Tucker: He came to Princeton in 1934.

Flood: He was a very active tennis player. That is how I first got to know him personally. Because of that I came, out of courtesy, when he talked about mathematics that I could not understand. Mainly I knew him socially. I recall two things about Marston that are interesting. One happened in the late '30s. We played a lot of softball. A group of us were trying to see if we could balance one bat on top of another with the ball on top of that, or something like that. Marston, remember, was fiftyish at the time. I was doing that, and he spent the rest of the day seeing if he could out-do us. He was that kind of guy. A very interesting man.

The other thing I remember was when he and I worked together during the war on the problem of finding out where airplanes were in the sky. Marston came to me. He had been chairman of the Math Society committee on how mathematicians could help the war effort. Sam Wilks and I visited Aberdeen Proving Ground, at Marston's urging, as representatives of the Math Society to suggest areas where mathematicians and statisticians might be of real help. I guess it was before the war started for us that Marston had us do this. It was probably early 1940 or late 1939, when Poland was invaded and nothing much else had happened. Norbert Wiener made some remarks. Marston was unhappy that mathematicians could not think of a thing that they ought to do. Well, after you and I were in the game I got him interested in the mathematical problem of determining the path of an airplane in flight from aerial photographs. He wrote an extremely long paper on the subject, which somebody told me year laters he had published somewhere. I didn't know that. Did you know that?

Tucker: Yes. I don't know who it was that published it. I remember there was a lot of projective geometry.

Flood: Later on he got into a lot of other war things, but during the period he was working on the airplane path problem he felt perfectly free to call me at three or four in the morning to talk about some apparent result—what did I think, or was it on the right track. This went on for about six months. We finally got to the point where when the telephone rang I had Alice answer it to tell Marston that I was at the library or something. When he got into something he became all enthused about it. A fantastic person.

Tucker: Did you have any contacts with Hermann Weyl?

Flood: Yes, that is the sad story of my life. After I finished my degree I didn't have a job or anything. Hermann Weyl needed an assistant, and he invited me to be his assistant. I accepted, but he said there was one condition. He had previously asked Richard Brauer if he would come. He didn't think he would, but if he did I would have to be removed and the job given to Brauer. I am sorry to say that Brauer came, so I never had the privilege of being Weyl's assistant. The only other contact I had with him was through Al Clifford, again after my degree. I had a paper to offer for publication in the *Transactions*, called "Equivalence of pairs of matrices", good paper by the way.

Because Clifford and I were close friends I showed him the paper in draft form. I was proud of my proof; it is the best paper I've ever done in mathematics. Clifford commented that Weyl would like to see it, which pleased me. A couple weeks later he said Weyl had read the paper and had a suggestion to make. The suggestion was a way to improve on my proof; the proof I had was okay but not very elegant. So I took the suggestion and told Clifford, "Please thank Professor Weyl; I'll mention him in the paper."

A couple of weeks later he said that he'd talked to Professor Weyl and that Professor Weyl did not want me to mention him, that it was my paper. "But," Clifford said, "if you want, you can say it was because of conversation with me." So the paper doesn't mention Weyl, but he gave me a more elegant way of proving the theorem. I think those are my only two contacts with Weyl.

By the way, you might be amused, Al, to hear about one of the nicest things that's happened to me recently. I'm on the advisory board of *Science Citation Index*, because Eugene Garfield was my student at Columbia years ago. So I read the thing, and I always look up my name first, to see if anybody's cited anything. Well, a few years ago somebody in mathematics cited me in the *Proceedings*, or some such place. So I got the paper. It was on eigenvalue theory; it was a long, 40- or 50-page expository paper. It starts off by saying, for the classical theory see the cited paper by M.M. Flood. My *Transactions* paper. I thought one would have mentioned all sorts of other people, especially Frobenius. So apparently that was sort of a fundamental paper on eigenvalue theory. Incidentally, the general problem which Wedderburn posed I never solved. The general problem was to do for three matrices what eigenvalue theory does for a pair of matrices. So my *Transactions* paper does that nicely. I made a lot of progress on the triple of matrices, but so far as I know that's still an open question. So it was a tough problem. But I got a lot of results.

Tucker: Did you know James Alexander?

Flood: I thought he was the greatest lecturer I ever heard. But I never ended up after a semester of listening to him being able to do anything with what he'd said. I'd just sit there in rapture in his lectures on analysis situs. I knew him and his wife socially; she and I used to play a lot of bridge. He didn't play bridge, but she liked it. My wife Alice and I spent a lot of time at their home. So he was a good friend, and I loved to listen to him lecture, but he never had any influence on me. I just couldn't understand him. A brilliant lecturer, as you know.

I have one story that you may remember. The other great lecturer was Siegel, Carl Ludwig Siegel. I used to go and listen to his lectures, and I could follow some of those and get something out of them. Morgan Ward, a visitor from Cal Tech, was assisting with Siegel's lectures. On some holiday when the University wasn't meeting, Siegel was let know that by Morgan Ward, and Siegel said, well, he would be lecturing. So there were thirty of us attending those lectures. Do you know this story?

Tucker: No.

Flood: Well, we were all curious because Siegel was an eccentric, and we didn't want to miss a lecture. So we invited Morgan Ward to check into the situation. We all hid around the corridors and what not. Sure enough, Siegel got up front in the empty room, started in with the beautiful lecture as though he had a full room. Well, he went on lecturing. So Morgan Ward let us all know, and we sheepishly trooped in, and listened to his lecture (laughter). Fantastic lecturer.

Another story I have is second hand. I forget who told me. Maybe you. The story goes that von Neumann's parents had all been lawyers and they sort of hoped that Johnny would be a good lawyer. When he was sixteen or so they sort of tolerated his fiddling around with chemistry and mathematics. Finally they found out he wanted to be a mathematician, or chemist, or some mixture. They were very upset. Well, their attitude was that it wasn't too bad if he was going to be a good one. So they inquired around who the best mathematician in his part of the world was, and it turned out to be Siegel. They had lots of money, and they arranged for Siegel to talk to Johnny. Afterwards they asked him, "Well, do you think he has any potential?" He said, "He knows more mathematics than I do now." Did you know that story?

Tucker: No, but I can believe it.

Flood: It's probably true. The other story I have I just wrote up in some memoirs I'm doing for my own benefit. It is about a memorable tea which the faculty ladies held on the arrival of Einstein for the people of the mathematics department, graduate students, scientists. We all went. Einstein was among a circle of ladies, with von Neumann and me. One lady turned to us and, assuming we were graduate students, said, "Have you had a chance to make friends with Einstein yet?" I spoke up and said, "No I haven't but I'm certainly looking forward to it. I can hardly wait, and I'm glad you're having this tea." And she turned to Johnny, who looked younger than I, but was five years older, "Have you met Professor Einstein?" He said "Oh, yes, I've been tutoring him for some years in mathematics," with a perfectly straight face. She thought that was very funny, and it was true. Remember he used to divide his time between Berlin and Princeton. Johnny was a great practical joker.

Tucker: Do you have any recollections of Robertson?

Flood: Oh, many. He and Alexander used to go and write down dances. Did you know that? They would write down the choreography in New York. They would take their wives, and go to some of the ballrooms.

Tucker: I've heard the story about the Robertsons winning the tango contest at the Rainbow Room.

Flood: Also Alexander was interested in it, at least technically. Well, there are many stories about Robertson.

Tucker: Did you know the first of the Fine Hall versions of the Faculty Song?

Flood: No.

Tucker: "Here's to Robertson, Howard Percy,/ For whose soul there will be no mercy,/ Round of belly, deft of toe,/ His forehead's high, but his mind is low."

Flood: I don't remember that. But I know he did a limerick about me I didn't like. I hope you never find one of those. It was very uncomplimentary [laughter]. I can't remember how it went.

I had a great deal of contact with Robertson. The course that excited me intellectually the most was his course on cosmology. I did a paper on the red shift at the time. I don't mean a published paper. Barkley Rosser and I were taking the course together. I'll never forget at my home I speculated to Barkley that ... remember this in probably my second year there, about 1933, after I'd really worked on the red-shift phenomenon and tried to understand it mathematically and otherwise. I think at that time, as Hubble wrote, we only knew about our galaxy. I remember very excitedly having a long chat with Barkley one night, saying, "Not only are there lots of other things out there, but there's a lot more between us and them than we know about." That's true. I wish I had followed up on that.

I have a story that, whether or not it's appropriate here, I'll tell you about. I never got to know Einstein very well. Except once he had me try to teach him to play pingpong, and the ball ended up in his hair. He was not very well coordinated. I never had an intellectual interaction with Einstein at all. He was a neighbor of ours on Westcott Road and liked my seven-year-old daughter Susan. I tried in different ways, but it didn't happen. But I read his books, and many of his papers, and one of his papers—this is long after I left Princeton—one of his papers which you may not know about is on baseball. It gives the equation for the curved path of a baseball when thrown by a pitcher. Are you aware of that paper? Remember now, he was uncoordinated. I don't think he was interested in sports. Why did he write such a paper? It bothered me for years. Why did Einstein write that paper? Well, various people said he wrote a lot of papers about a lot of things. So I guess it's true, but I didn't know this.

Well, I was lying in the hospital in Ann Arbor many years later, about twelve, fifteen years ago, reading miscellaneous things. For some reason or other I had learned that nobody knows why the earth spins in twenty-four hours. So I checked the literature, first the classical literature, then the recent literature, then the current literature, and I checked with astronomers in Ann Arbor. I discovered it's still an open question. Well, I had the rather bizarre idea to hypothesize that that's what Einstein was interested in. So if you think that, in accordance with Kepler's laws, Earth is forced to go in the curve, and if the solar wind is proper, then you can reverse the whole situation. And so I had a theory about why Earth spins. And

I've done a bit of serious work in the last ten years looking into it. The thing that makes it very difficult to spend time at it is the fact that Venus is retrograde. And I can't reconcile that with the conjecture. On the other hand, the great expert on solar wind is at UCSD, H. Alfven, the Nobel Prize winner. I've talked with him about it, and the theory is not easily rejected. Alfven has given me some of his papers on solar wind. He's the great expert, so I work on that a little now and then. But I suspect it's not a good theory. I can't imagine why Einstein ever wrote that paper on baseball. I have a suspicion that he was thinking about rotating bodies, and I hope to learn more about it.

Tucker: Could you get in touch with the man that's editing Einstein's work for Princeton University Press?

Flood: Good point. I'm sorry I can't think of anything about Robertson. There'd be many things; they just don't come to mind.

During the Thirties, I was fortunate, because I was probably the first graduate student to bring in a wife, contrary to everybody's expectations. I didn't know any better. I'm glad I did, but nobody expected anybody to show up with a wife. We had the great good fortune to become very good friends with the Condons and the Robertsons and the Alexanders, even the Veblens, who were much older. They all took a liking to Alice, who was extremely likable, so suddenly we were an accepted couple. So I got to know things about the Condons. Even the Lefschetzes. Lefschetz liked Alice, and I took advantage of that. We could go over there and have tea any old time. So I had the social relationship that very few graduate students those first six years ever have. And that was true of Condon. Now afterwards I had a relationship with Condon when I was in the Pentagon and so forth, but not during the 1930s. I never took his course.

Tucker: Bohnenblust?

Flood: I just took a course with him in analysis. We were friends. I don't know that much about Bohnenblust. He was not one of my closest friends. The close friends I had were Barkley Rosser, John Barnes, Ed Beckenbach, you, Joe Hirschfelder, George Kimball, the Clifford Mendels, George Shortleys, and John Turkevichs—through games and other social activities. Then intellectually, Rosser, Clifford, and Pitcher. Remember Everett Pitcher? I had some interaction with Nathan Jacobson, none with Bob Walker, a lot with J.L. Vanderslice. I wasn't interested in that field as such, but I liked Vanderslice. He got me thinking a little bit about a field I never worked in, differential geometry and Riemannian geometry. Then, of course, a lot later Jimmy McShane, and a lot with Samuel Wilks. Sam was one of my very closest friends. Of course Gena Wilks and Alice were very close friends. Others were the Ed Titts, Leo Zippins, Deane Montgomerys, Alex Moods, Norman Steenrods, Adrian Alberts, and the Petersons. Of course, you and Alice and the Bob Singletons were our specially close friends among the mathematical group.

You might be interested to know how I got away from pure mathematics. After my doctorate I had an instructorship at Princeton to teach mathematics, and among the things I taught was the mechanics course on statics for engineers. And then the second course in dynamics, which I had never really learned. I knew that teaching a new course was a great way to help me continue my education.

Well, by then we had three children, and I wasn't paid a high salary as an instructor. Somebody asked Sam Wilks if there was anybody around who could help over in Trenton on a WPA project studying prisons and mental health and what not. Remember, I had never thought about statistics or even probability at all. Well, Sam knew that I would like some outside income. Sam didn't want to do it for some reason or other, so he told them to talk to me, and I got the job. I checked first with the mathematics department, and they had no objection to my also holding a full time job in Trenton. I quickly learned enough statistics and what not to handle the job, through the statistical literature. So it was no problem, and really very easy. In fact, it was quite entertaining. I directed about 40 people there, including studies of murderers in prisons, inmates in psychiatric institutes, and all sorts of things for the other public institutions of the State of New Jersey. And ever since then I've been primarily in applied mathematics simply because of that, and enjoy it. I still fiddle around in pure mathematics of course.

Tucker: Then from that you went into the Princeton Local Government Surveys.

Flood: Yes, the Local Government Surveys Section of the University. But again it was for financial reasons. I liked it and was good at it, and I did a little switch to applied mathematics, or applications in mathematics. But I didn't see much of Fine Hall after 1936, except to go over there and socialize. The building, of course, was just magnificent. You may remember that the year I was a full time instructor after I got my degree they let me have Gillespie's office. I think there was a coat closet and a toilet in that room. There was a blackboard with doors you could close, and very fancy furniture.

Tucker: I don't think there was a toilet there.

Flood: A lot of nice features.

Tucker: Oh yes.

Flood: As a brand new doctorate I had no right to such a room. I think I had that a year afterwards, too. They let me use that after I had finished my instructorship. At any rate, when I started working with the State of West Virginia, it was through John F. Sly that I became advisor to the Governor and Legislature. We had a meeting in that office in Fine Hall of the Governor, the President of the Senate, and Speaker of the House, John Sly, and Dean Eisenhart, where I presented the things I thought the State should do. Which they did do, by the way. We wrote two hundred bills and five constitutional amendments in four years that were passed without essential change. Fantastic experience. And that all started in that room. It was very interesting because Governor Holt had studied differential gometry from Eisenhart's book *Differential Geometry* and was a great admirer of Eisenhart. Eisenhart thought it was wonderful that we had these people who were doing this kind of thing. He was very friendly toward the statistical approach and that helped.

Tucker: Sam Wilks would have never made a distinction between the mathematical and statistical areas.

Flood: That's right. Well, you'd be surprised to learn that on two occasions Eisenhart got hold of me about two years beyond my degree, to get my opinion about the value of Sam Wilks. The second time whether it was a serious mistake for his son [Churchill Eisenhart] to be in statistics. In the first case I assured him that there was nobody I could think of that would be more valuable to the math department than Sam Wilks, and in the second case, it would be a great career for Churchill. But he was still very questioning about that in his own mind. Then, of course, they treated Jimmy McShane better than Sam Wilks, which Sam didn't appreciate. By the way, Jimmy McShane talked to me about that. He was unhappy about it too; he realized that Sam was very good. It was tough for Sam in those days. Thinking back, it's all so ridiculous.

Tucker: Well, I was always very friendly with Sam. We had a certain mathematical bond, because you know I had at one time thought I was going to become an actuary. I had passed the first set of actuarial exams and studied on my own. And then, I stayed on a year at Toronto after my bachelor's degree to get a master's degree, because I couldn't make up my mind where I wanted to go for graduate study. During that year I was a teaching fellow at the University of Toronto, and one of the courses that I got to teach was a course on mathematics of economics. And it was a year course. So I pretty soon ran out of the material that then existed, and I decided the best thing for the students was statistics.

Because of these experiences that I had had, I had a kindred feeling for Sam. I could sense the difficulty that Sam felt in being an outsider in the department. And of course, after Eisenhart retired, Lefschetz was often very hard on Sam. This showed up particularly in that Lefschetz would never let any of the research funds be used for statistics. I think one exception was the time that Harald Cramer was a visiting professor. Also Sam always had to fight to get graduate students. He felt that this was utterly ridiculous. Sam trained some of the very best graduate students in the Forties.

Flood: Well, I was aware of all of this. Incidentally, a story that I don't know if it should be made public or not. Sam and I were working together, I forget exactly when, I suppose about 1937. One of the students that Sam had had was doing very badly, but Sam felt that he was a pretty bright guy. Sam said, "I've got this fellow Mosteller, and he's not getting anywhere. He seems to be a bright guy; I don't really understand it. Would you meet him?" "Sure, sure." So soon after Sam brought him over, introduced him, and left. I talked to him for a while; he was a bright guy. I finally figured out that probably his trouble was that his girl wasn't there. So I hired the girl. I brought her there from Pennsylvania. And everything went fine with Freddie from them on. I don't know whether Freddie understands that. Virginia was a very fine secretary. They didn't get married for another year or so. They lived there, as you know, during the war. Freddie just needed something or other that wasn't there. I think we helped him.

Sam was very good that way, with people. For example, when Sam heard from Tjallings Koopmans that Tjallings wanted to come to the United States, he came over to me and said, "Is there any way you can do something about it?" I worked around a while and finally found some money, though not much. I had him come to work at the Local Government Survey, and brought him over from the League of Nations. We were going to do a book together on time series theory. I had a book announced by the Princeton University Press on time series. I had about a third of the manuscript done, and I knew Tjallings was very good from the things I'd read and heard about him. Then the war put a stop to that. We didn't get him cleared to work with us during the war because his family would be endangered in Holland, which had been his home.

Tucker: I had forgotten that Tjallings worked at the Princeton Surveys.

Flood: He was part of my group there. We didn't pay him very much. It was a shame, but that was all I could get.

Tucker: His Ph.D. was in theoretical physics.

Flood: Not only that, he was the most promising young theoretical physicist in his country. A very interesting thing happened. I got a paper a year or two ago. I guess I got it through *Science Citation Index*. A paper in physics, a recent paper. I looked it up, just to see why they referred to me. It turned out that the pioneer in this particular subject was Tjallings Koopmans, so they referred to both Tjallings and me. They were using the computational methods I had developed for non-linear programming. They referred to Tjallings and me. So I wrote to Tjallings, and said isn't it interesting, and I have never heard from him. That disappointed me. We were cited in the same paper for totally different reasons.

So his work in physics was first-rate, not just incidental. Incidentally, I saw him recently at UCSD lectures and heard a lecture on economics there.

Tucker: He was at Princeton giving a lecture in the engineering school about a year ago. I went to the lecture and to a meal with him and other people. I of course worked with him quite closely right after that linear programming conference in 1949, which is now regarded as the origin of the mathematical programming movement. I helped edit the proceedings of that 1949 conference.

Flood: Well, some of you may not realize that, I forget just when—probably when I was at Rand—I talked with Tjallings about all sorts of things, and sort of marveled at how well his mathematics was going, because when he first joined me he was not the mathematician he was later on. And he said that everything he knew he'd learned from Al Tucker. Is that true? It sounded to me like he thought you'd had a lot of interaction with him on mathematics.

Tucker: Well, that was the year, '49-'50, when I was on leave of absence from Princeton at Stanford, when the Prisoner's Dilemma came out. I had had a leave of absence in '40-'41, and then I was only back in Princeton two days from that when you came to see me. And you drafted me for Fire Control Research, and from that time on until 1949-50, I really never had a breather of any sort. I was teaching a full load all during the war as well as doing things that you know about.

And then the post-war period. There was so much teaching to be done; all the students were coming back. We also had such enormously good graduate students. I got involved in game theory and in linear programming. But that year, '49-'50, I was suddenly free of all the time-consuming things and could really think about things, and it was also that year that Tjallings had me help him edit that volume.

Flood: Oh, I didn't realize you did that.

Tucker: We wrote back and forth about a letter a week. If he learned things, it was mainly in correspondence.

Flood: Well, he told me he learned a lot.

Tucker: One thing, though, that he was adamant about—this was just his background in physics—he insisted that a vector had to be written as a column. And that a row vector had to be column transposed.

Flood: Well, I agree with it [laughter]. Wedderburn would have agreed with it.

Tucker: I don't.

Flood: That isn't just physicists.

Tucker: I think that rows and columns should be treated equally.

Flood: You're logically correct, of course.

Tucker: It simplifies the notation so much to be able to write a vector on either side of a matrix, and its position tells you which it is.

Flood: Logically you're correct—you're great at the ways of elegant matrix theory—but we are all creatures of habit and vectors as columns is one of mine.

One historical thing that I'm not sure whether you know about or not. One of the friendly arguments that Tjallings Koopmans and I had concerned an appropriate name for what is now known as linear programming theory. When I was responsible for organizing the December meeting of the Allied Social Science Associations in Cleveland, probably 1947, I wanted to include a session of what was then commonly referred to as input-output analysis, after the work of Wassily Leontief. Tjallings agreed to organize such a session for the meeting, and we met in California to discuss the arrangements just prior to Neyman's Second Berkeley Symposium. Actually we discussed this while enroute from Stanford to Berkeley in a car whose other passengers were John Tukey, Francis Dresch, and a Stanford mathematician (Spencer?) who was driving. I knew a bit about Leontief's work because of the work under Marshall Wood, by George Dantzig and others, that had been pushed and encouraged by Duane Evans, who was then at the Bureau of Labor Statistics—because of my position as Chief Civilian Scientist on the War Department General Staff, with some minor responsibility for the effort in the Air Force under Marshall Wood.

When Tjallings and I were trying to decide what to call the session in Cleveland I was unhappy with the input-output analysis title and wanted something that was broader and peppier, partly because of the related Air Force work. Tjallings proposed "activity analysis" as a name for the session, with some support from the economist Dresch, but Tukey and I were not satisfied. As you know, John Tukey is very good at creating good names for things, and between us John and I soon settled upon "linear programming" as an excellent name for the session. As I recall vaguely now Tjallings did not call the Cleveland session "linear programming" but his own 1948 Chicago conference went by that name even though he used 'activity analysis' in the title of his published proceedings. I forget just how Tukey and I arrived at the name 'linear programming', but it has certainly stuck. I doubt that Tukey even remembers the California incident now.

Tucker: The first paper by George Dantzig and Marshall Wood was called "Programming in a Linear Structure".

Flood: Well, it is possible that is where we got the idea.

Tucker: And the two words, interchanged, were pulled out of that. I think that's the official story.

Flood: When was that paper? Was that before that, do you think? It may be the other way around.

Tucker: Well, that paper appears in the Activity Analysis volume. But it appeared earlier. The Activity Analysis volume was published in 1951. I think that the paper had appeared about two years before that.

Flood: That would be later. I suspect that it came the other way around. I'm remembering vaguely the conversation Tukey and I had, and I don't remember any awareness of any such terminology by Dantzig and company. Well, when Bellman chose 'dynamic programming', he talked with me about it, and the idea was that he learned that that's the way to get a peppy title. He just called it dynamic programming, meaning that that was the next stage. And it caught on. Prisoner's Dilemma. I always wondered what would happen if you hadn't given the name Prisoner's Dilemma. I don't know whether it would have made any difference or not. It certainly caught on, world-wide. Incredible.

Tucker: Oh, yes, I've had official inquiries from the *Oxford English Dictionary.*

Flood: Yes. It's incredible. I don't know whether the choice of name matters or not. Mel Salveson and I named the Institute of Management Sciences, and the name 'management science' has certainly caught on. Your 'Prisoner's Dilemma', our 'management science' and 'linear programming', Wiener's 'cybernetics'—I don't know whether or not the names of things are that important, but they seem to be.

Tucker: I think so.

Flood: Apparently they are. Developments that started in the 1930s at Princeton have interesting consequences later. For example, Koopmans first became interested in the "48 States Problem" of Hassler Whitney when he was with me in the Princeton Surveys, as I tried to solve the problem in connection with the work by Bob Singleton and me on school bus routing for the State of West Virginia. I don't know who coined the peppier name 'Traveling Salesman Problem' for Whitney's problem, but that name certainly has caught on, and the problem has turned out to be of very fundamental importance.

George Dantzig and Tjallings Koopmans met with me in 1948 in Washington, D.C., at the meeting of the International Statistical Institute, to tell me excitedly of their work on what is now known as the linear programming problem and with Tjallings speculating that there was a significant connection with the Traveling Salesman Problem. As I understand it, Tjallings had published a major report on his pre-Princeton work in Europe on scheduling a tanker fleet, then extended this work during the war while he was with some wartime shipping agency, all of which is an early part of the general research that led to his Nobel Prize in economics. As I said, this is not Fine Hall in the 1930s, but I find it a fascinating story, with Hassler Whitney and Tjallings Koopmans as players. You and I could of course add to this tale at great length, as later players, but that is another story.

Tucker: About others?

Flood: Claude Shannon was another of my close friends in the late '30s, mostly intellectually but also socially with our wives. Claude was very greatly interested in how the brain works, and he stirred up my interest in this too, perhaps in 1937 or so. I'll never forget how he asked me once, "How can you really tell whether or not something is human? What could you do to test this?" I recall that I came back a few days later and proposed a test that is very much like the one attributed to A.M. Turing and known as the "Turing Test". I said, as I recall it vaguely now, "Forget all about the details of language and communication system, and if a hundred people interacting in this mode with the two cannot tell which is the human and which is the machine then both are human." I would be interested in Shannon's recollections, as to whether Turing and I had the same kind of idea independently or whether one or the other of us was primarily responsible.

Tucker: Oh, I think he thought of it himself.

Flood: Well, that's interesting because I have expressed the idea in conversations before I knew of it as the Turing Test. When was Turing there?

Tucker: I think from '36 to '38. He completed his Ph.D. with Church in '38.

Flood: I don't think I ever knew Turing, or had even heard of him at that time.

Tucker: Well, the records show that I was a member of his generals committee, along with Lefschetz and Church. Bohnenblust substituted for me for his final oral.

Flood: Well, I'm sure it was independent. I've often claimed credid for this brilliant idea. Claude might know. Have you interviewed Claude?

Tucker: No.

Flood: You may recall that I served as reporter, soon after I received my degree, for your seminar on combinatorial topology. I can say now that I was tremendously impressed with your talents as a lecturer, and that experience increased by respect for rigor and elegance in mathematics. I had two other experiences as a kind of reporter for seminars; once for Wedderburn and very briefly once for Mayer who was Einstein's long-time colleague and assistant. For Wedderburn, I mimeographed his notes that became his *Lectures on Matrices* when it was published by the Math Society. For Mayer, I was quickly overcome by his subject matter and Deane Montgomery graciously relieved me from the task. Looking back, I would recommend to every aspiring graduate student that they undertake this kind of arduous task, for it forces a kind of careful attention that makes learning faster and surer. My work during my first year as assistant to Dean Eisenhart also served this purpose well, for it showed me how a real professional dealt with his research and his publications. This attribute of honesty, professionalism, and dedication to one's work is perhaps the most valuable experience of all that I gained from my lucky association with all of you at Fine Hall in the '30s.