Oral History Interview with Roland (Rol) Madden
July 30, 2019
Interviewed by Gerald "Jerry" Meehl, with Laura Hoff
Transcribed by Cyns Nelson

Note: Speakers are identified by their initials, placed at the start of their remarks. Conversational sounds and verbal outputs that are not words are placed in parentheses; peripheral and editorial notes and amendments are placed in brackets; words spoken with emphasis appear in italics.

[00:00:00]

LH: This is Laura Hoff, archivist at NCAR. We are at the Mesa Lab. It's July 30th, 2019, and I'm here with Jerry Meehl and Rol Madden.

JM: Okay. So, I thought what we would do—I mean, the whole focal point is the MJO, right? But, I thought we'd start out a little bit earlier, kind of—you graduated from high school in Chicago, somewhere, presumably.

RM: Yes.

JM: And what year did you graduate from high school?

RM: 1956.

JM: And so, what year were you born? When were you born?

RM: 1938, in May. May 8th.

JM: May 8th, oh. Okay, I'm May 21st.

RM: Oh, good! (Laughs.)

JM: So then—where'd you go to undergrad, then?

RM: Um, I went to Loyola University, in Chicago.

JM: Okay.

RM: Yeah.

JM: And then—but the Air Force intervened in there, somewhere, right?

[00:00:50]

RM: Well, when I went to college, you actually could work your way through college and not have any debt. In fact, when I was thinking about this, last night, I think back: none of the students or friends I had, I never remember anybody speaking about a loan; I don't think anybody had a loan, in those days. So this would be in the late '50s. Anyway, the reason I bring that up is because I worked, like, 20 to 30 hours a week at a grocery store. And I had no debt. But what resulted was: I didn't always get the best grades.

So, in the fall of 1960, I was starting my fifth year of college. And, probably in September, I got a letter from my draft board, to come in and have a physical. Which I did. This was in Chicago. And, I don't know, there probably were a hundred or more of us who passed the physical and were told: "We will be in touch with you in about six weeks."

JM: Oh.

RM: Yeah. I wouldn't graduate ‘til June of '61—this was fall of '60. So on the way home, I remember stopping at my draft board and filling out the papers to get a deferment ‘til I graduated.

JM: A student deferment.

RM: Yeah. So I was safe ‘til June. And then in the spring, with the encouragement of my girlfriend—who later became my wife, and was your friend, Aggie—I applied to the Air Force. She said she'd rather be the wife of a lieutenant than the wife of an enlisted man. (Chuckles.) Okay, so anyway—

JM: But this was pre-Vietnam era, right?

[00:02:57]

RM: Oh yeah, no. I was very lucky. It was after Korea, before Vietnam. Although Vietnam might have been starting up, but it was secret and very minor. Anyway, it turned out that: after taking several tests in the spring, with the Air Force, I was accepted in Weather. And I remember, I was at work, and my mother called me and said, "There's a letter here from the Air Force." And I said, "Yeah, please read it to me." She read it: "You're accepted in Weather." And I'm like: Weather? I mean, that was a chapter in my physics book, which the professor—at Christmas time—said, "You can read over the Christmas break. We're gonna skip that chapter. We're not gonna (chuckling) talk about that."

JM: What was your undergrad degree in?

RM: Oh, my undergraduate degree was in physics.

JM: Oh, okay.

RM: So anyway, I figured: well, they're gonna have to teach me (chuckling), because I don't know anything about weather. So, I went on active duty—I graduated, then, in June, maybe end of May—and I went on active duty in late September of 1961. And my first year—well, I got married on the 30th of December, of '61. And Aggie and I—our first year was at Texas A&M, learning all about meteorology.

JM: Oh, so the Air Force sent you to school.

RM: Yeah. And then I got assigned—oh! Partly at her encouragement and prodding, I turned from a mediocre student to an all A's student. And so I got, kind of, a choice assignment. I was assigned to Patrick Air Force base, which is at Cape Canaveral.

JM: Oh.

[00:04:46]

RM: My first year there, which would be, I think, calendar year '63, I briefed the Gemini astronauts, and the Mercury astronauts, because they used to fly from Cape Canaveral to Houston. And so I briefed the pilots on their flights.

And then the second year, I became a staff—I think it was called Assistant Staff Meteorologist, and I forecast for missile launches. And in fact—

JM: At the Cape?

RM: Yeah. And in fact, in spring of '65, I did the midnight briefing for von Braun and his colleagues. Yeah. It was pretty interesting; it was a great job. And like I said, it was between wars; it couldn't have been better for me.

JM: Let's see, did you meet the astronauts?

RM: Well, they would come in—and they had these papers—and then I would give them a briefing, and then I would sign it, that I had given them a briefing.

JM: But this is when they were just flying to Houston, back and forth.

RM: Yeah, yeah.

JM: It wasn't for their space flights.

RM: No, no. I never was involved in forecasting for a space flight. Except, I did an early Saturn—Saturn was the missile that eventually was used to go to the moon. And one of the early ones, spring of '65, I gave the midnight briefing. There were no humans on board; it was just a test flight.

[00:06:20]

JM: And that was one von Braun was at?

RM: Yeah, von Braun was at it. Yeah.

JM: (Light chuckle.) Did you talk to him at all?

RM: No. I think they may have asked some questions, but I don't recall.

JM: So they just needed, like: "calm winds, clear skies." I mean, you were basically—

RM: Stuff like that, yeah. I do remember kind of—I think I'm correct, in thinking back, that somebody, one of his team, was asking about the wind shear. Because as the missile moves up through wind shear, if the wind shear is big, that can be bad for a launch. But it was a good weather day, so there was no problem.

JM: Were you there for the launch?

RM: I wasn't there for the launch. But I witnessed many, many launches. Yeah.

JM: Did you see any Saturn V launches?

RM: You know, I don't think I ever saw, from the Range Control Center. But we used to go to the beach. I mean, you see in the movies, all these people out there (chuckling.) So we saw lots of—and in those days, I would say, maybe a third of the launches—maybe that's a little too many—maybe a fifth of the launches blew up.

JM: Wow. (Chuckles.) That was part of the attraction, right? You'd see if it was gonna blow up or not.

RM: Well ... yeah. (Chuckling.)

JM: So, did you see a couple of them blow up?

[00:07:45]

RM: I do remember, at the Range Control Center, I had a little—not "I" but the meteorologists. When I was on duty, it was my little console, behind a couple rows of the regular guys' consoles. And, when the countdown got to about minus 15, or so, I'd go up on the roof and watch. I do remember going up on the roof, and a satellite blew up right on the pad—and running back down for fear that they were gonna ask, "Was it the winds?" or something like that. And here I am, up on the roof! (Light chuckling.)

JM: So were you there for, like, some of the Mercury or Gemini launches?

RM: Yeah. Gemini—I'm pretty sure all the Mercury was over when we got there in '63. So, Gemini launches.

JM: Okay. So you were in the control room for the Gemini launches.

RM: Yeah. It was the Range Control Center. Like, NASA had their own control, which—in fact, just recently, there's been something on PBS about it, and you see, maybe, five rows of all these things. Well, the Range Control Center was more safety. If the missile went off track, they would blow it up, and stuff like that. It was more safety; and we only had, like, two rows of consoles, and then a little console in the back (chuckling) for the weather. It was kind of funny, because there were no windows in the room. And so, I'd have to call somebody up, to make an observation for me. (Laughing.)

[00:09:26]

JM: That control center was right next to the NASA building? Or were you in kind of a different—

RM: You know, I don't remember where the NASA building was. It was very near the launch pads, of whatever happened to be being launched at the time. Possibly not the Saturn; other missiles. Atlas-Centaur was one of my projects.

JM: I think the Saturn, the Saturns were on their own launch pads, though, right?

RM: Yeah. They were probably away—like, it was Launch Pad 37. I don't remember which ones.

JM: Yeah, on one of these PBS things they've just been showing, on the 50th anniversary, they were—something on the design of the launch pads—and they were computing the blast radius, if the Saturn V blew up on the pad. And they wanted to keep everybody—all the facilities and buildings—away from that blast radius. They'd computed that.

RM: Oh yeah.

JM: So I think they had—for the Saturns, that was a whole different level of facility.

RM: Yeah, it was so huge. Yeah.

JM: Well, it's too bad you didn't see the Saturn V launch that you briefed those guys for.

RM: Sorry?

JM: You didn't see the Saturn V launch that you briefed—

RM: You know, I can't remember—if I saw it, it was from the beach. It wasn't from an official—like I was at the Range Control Center. Because I only—probably the other, more senior people, didn't want to go to the midnight briefing! (Laughing.) I don't know.

JM: How long were you there, at the Cape, then?

[00:10:58]

RM: I got out in 1965. So I was there from all of '63, all of '64, and half of '65. Then I went to the University of Chicago.

JM: That area was pretty wild, right? Because—like, also in these specials, they've been talking about: there were all these young people that were working for NASA; everybody was young, right? Everybody in the space program was really young. So then you left in '65. Then you were out of the Air Force?

RM: Yeah.

JM: Okay. Then what—

RM: I was in some kind of reserve, or something. But I wasn't on active duty.

JM: Then what happened?

[00:11:32]

RM: I went to the University of Chicago, and I started working—this would be the summer of 1965—I started working for Ted Fujita. You've heard of Ted Fujita? Yeah? And I learned spherical geometry. I could grid any satellite picture (chuckling) you could imagine! I just had to know the sub point and the orientation of the satellite, and I could—so I didn't really learn a *whole* lot of meteorology.

JM: But you were taking classes, then?

RM: Oh yeah, yeah. Oh! Well that was—in the summer I worked for Fujita, and then classes started in the fall. He was paying me, so I guess I was like a student assistant, maybe. And doing some research for him. [Amended to acknowledge that RM was supported by a NASA Traineeship after classes started.]

JM: Okay. So then you were doing graduate work there, for a while, right?

RM: Yeah. For two years. It might have been less than two years, but I flunked the German exam, twice. (Light laugh.) And the third time, in the spring of 1967, I just barely passed it.

JM: Wow. That was the language requirement for—

RM: Yeah. There was a language requirement. And I happened to choose German, and I had never taken German. I went to the library—after I flunked a couple of times—and I found this book. And it was, like, "Ten Rules to Translate German." And I did all those rules, and I practiced; I read all these elementary German things, and finally I passed. (Laughs.)

JM: And so then—what degree, then, did you come out with?

[00:13:20]

RM: I got a master's degree. I wrote what I thought was a thesis, which was kind of a clever thing. I figured out—actually, Fujita—I gridded this Nimbus picture. And Fujita told me to come in on Saturday—because he had to go to NASA, to talk about this stuff—and show him the results. And he looked at my gridding, and he spotted right away that my latitude lines were off. They were too high on one side, of the sub-point track, and too low on the other. And that was because the satellite had yawed. He spotted that right away; and he did—by hand—he fixed it all up. He did it in about five minutes.

Anyway, I went into the system that controlled the attitude of the satellite, and I figured out a way to correct, whenever it yawed—it's a little bit complicated, but—whenever there was a cold cloud on the horizon, it would see that cold cloud, and it would roll a little bit. Because—it's too complicated (laughs). Because, it tried to equalize the distance that it saw, the Earth. So if there was a cloud there, it thought the Earth wasn't all the way across here, it started here. And it would roll a little bit. So when it rolled, there was a system that: once it rolled, it thought that it had yawed. So it tried to correct that, and it made a bad yaw. But anyway, by seeing the edge of the horizon, I could adjust for that yaw. I don't know whether it was ever used, but it was very clever.

[00:15:18]

But I wrote this thing up, and I thought it was my master's thesis. And I remember, one day, toward the end of my time at Chicago, going into Fujita's room. And here's this thing I wrote, like, down in the middle of a big stack—he hadn't even seen it. So, I think my master's degree was without thesis. (Light laughing.) Even though I was very proud of this thing that I thought was my thesis.

JM: You figured it out. Yeah. Then where'd you go?

RM: Well by, sort of, a few coincidences and good luck, I came out to NCAR. Got a job in the synoptic meteorology group that Chester Newton headed, and my immediate boss was Ed Zipser. This was, like, September of 1967. And in the spring of 1967, NCAR organized the Line Islands Experiment, which was in the center of the Pacific. In fact, you were involved, I think.

JM: No, no. I was very interested in it, though. Because I was in—

RM: A little later.

JM: Later. Yeah, right.

RM: Okay. So the Line Islands Experiment, it was like the first big field program that NCAR took over, handled. And it was, in part, picked to go in the spring of 1967 because the first geosynchronous satellite was launched in December of '66. And it was over the Pacific. So they were gonna get some ground truths, compared to the satellite. So my job, then—so they were there for about two months, maybe a little more than two months, collecting lots of data. So my job was to help Zipser and people with those data.

[00:17:11]

JM: So that's why you got hired; analyze the Line Islands—

RM: The Line Islands data. Yep.

JM: And you were working here, though.

RM: Sorry?

JM: You were working here—

RM: Oh, yeah. When I came to NCAR in Boulder, I think maybe some—Ed might have still been in Hawaii. But I came to Boulder; I never went to Line Islands.

JM: And this building was brand new, right?

RM: The building had been dedicated in January of that year. Yes, it was. It was a very exciting time.

JM: Yeah. Where was your office? I mean, where were you guys?

RM: My office was in the second floor, down the hall from the library. It was—actually, it was all the way at the end. It was actually Paul Julian's office. And he happened to be on sabbatical at Chicago, where I had just come from. (Interviewer chuckles.) So I sat in his office.

JM: So this is in Tower B? Or Tower A? Because you ended up in Tower A, for years.

RM: You're right. I've been here for 50 years. Tower B is the south tower?

JM: We're underneath Tower B. So, yeah. It's the one on the south, yeah.

RM: Okay. So it was in Tower B.

JM: Oh, okay.

RM: So it was the—as you come out of the library, going toward Tower B, and you take a little left, and then a right. It was at the end of that corridor.

JM: Oh, okay. Yeah. Just straight down there. That's where Warren Washington was, for a while. And that's where the—I think that's where the CISL director's office is now, in that area. Oh, that's interesting. So then you were here; you got here in Boulder.

RM: Yeah.

JM: And you started analyzing Line Islands Data.

[00:18:55]

RM: Yeah. I think my first thing was, of course, gridding satellite. I gridded all the geosynchronous satellite data. And then—I don't know when, but maybe in October—this big crate appeared in the hall, outside of Chester Newton's office, which was an office at that end of the hall, as well. It was full of punch cards. And what it was, was: between the end of the Line Islands Experiment—which was the end of April—'til, say, September or so, people were taking the recorded information and putting it on punch cards.

JM: Oh, man. By hand.

RM: Yeah. By hand, yeah.

JM: So the idea was: the satellite was just visible, right? There was just visible imagery.

RM: Yes.

JM: And then, the ground truth was figuring out what kind of clouds the satellite was seeing?

RM: Yeah. I don't know how much people, actually, in the end did that. There were—a lot of people brought experiments down. And they, each person, worked on their own experiment.

JM: So that was your, kind of, first encounter with tropical convection.

RM: Yes.

JM: Convective systems.

RM: Yeah. And tropical meteorology.

JM: Tropical meteorology, right. So you were kind of picking that up on the job.

[00:20:17]

RM: Yes. (Light laughter.) That's for sure. Yeah. But anyway, these punch cards had the azimuth, elevation, thermodynamic data from the rawinsondes. And so, Dennis Joseph—who was in the data support section at the time—and I took those punch cards. And our job, then, was to turn that into winds and thermodynamic information. And we used a program written by Ed Danielson, who was a scientist.

JM: Recognize the name.

RM: You recognize his name? He was a scientist here, at the time, and had a student assistant, Bob Gall—

JM: Oh, right. Yeah.

RM: —who later became a director of the mesoscale division?

JM: Yeah, yeah.

RM: But at that time he was a student, I think at the University of Arizona. And they had written this program, which Joseph and I used to construct the data for the rawinsondes.

JM: So NCAR had a computer here, at the time, right?

[00:21:25]

RM: We had a Control Data Corporation 6600.

JM: So you were learning Fortran? Were you—

RM: No, I knew Fortran from—I started, actually, when I was in Florida. There was an opportunity to learn Fortran. And I learned Fortran there, and then I did more at the University of Chicago. So I knew how to write a computer program. But actually, I didn't mess too much with the program that Danielson and Gall had written. And Dennis Joseph really handled most of that; he would run the program.

Though, the initial thing you mentioned, doing all this by hand. Well, I mean, there was so many—you just had to plot up the data. There'd be these big, wild, values that didn't fit. So, the first thing was, probably, months of writing various programs; I wrote various programs to plot up means, and deviations, to find all these errors.

JM: Oh. Oh, yeah. So you were trying to do quality control—

RM: Quality control, yeah.

JM: To get a good data set out of it.

RM: Yeah.

JM: Wow! Yeah, that was a lot of manual labor.

RM: Yeah. (Laughs.)

JM: So, Paul Julian wasn't here, then, at that time.

RM: He wasn't here the first year. Then he came back. Okay, then maybe we'll—

JM: Yeah.

[00:22:58]

RM: So, we got the rawinsonde data in pretty good shape. So now—NCAR was pretty free-and-easy in those days. So I had time; I could look at it myself, these data. So, I recall that it was either a guy called Walt Jones, who was a dynamicist, who, when I later took courses up at CSU, I used to bring my homework up and discuss with him (chuckling). He would help me. Or Bob Dickinson [?]. One of those two told me about a paper by Yanai and Maruyama, which showed kind of wave motions, in 1966, in the tropical stratosphere. So of course, I should look for that in the Line Islands data. So, I started looking—oh, and then—

JM: In radiosonde data.

RM: In the radiosonde data. Yeah. Rawinsonde, actually.

JM: Rawinsonde.

RM: Radiosonde, I think—language changes, but at the time I was working on this, rawinsonde was thermodynamics *and* azimuth and elevations. So it was thermodynamics *and* wind. Radiosonde was just thermodynamics.

JM: Oh.

RM: But now, I think, radiosonde means—just the same. So, yeah, I started looking at the data—and I forgot to mention: when I was at Chicago, I don't remember exactly why, but I got the idea that spectrum analysis was a valuable tool. And I sat in on a course taught by a guy called Christopher Bingham, who was a recent PhD from Yale. And it was on fast Fourier transform. Fast Fourier transform was invented in—or not invented, but first written about, programmed, in 1965, by Cooley and Tukey.

JM: Oh, yeah. Tukey.

[00:25:04]

RM: Well, it turns out: this Bingham guy wrote a paper in 1967—I discovered later, after I got to NCAR—with Tukey and maybe his professor, Godfrey, I'm not sure. It's Bingham, Godfrey, and Tukey, 1967, describing how to use this fast Fourier transform to compute a spectrum. And of course, Julian knew all about that stuff, because he had been a student of Hans Panofsky. And Hans Panofsky was one of the first people to use spectrum analysis with meteorology. Meteorological data.

Well anyway, when I started to look for some of these things that the Japanese—Yanai and his colleagues—and Mike Wallace and his colleagues, at University of Washington, were finding, in the late '60s. Like, mixed Rossby-gravity waves; Kelvin waves; tropospheric synoptic waves. And they were using spectrum analysis. I didn't mention—

JM: But they were just using the rawinsonde data. These are not very—I mean, they're fairly sparse in the tropics, right?

RM: Yeah. But that was the advantage of spectrum analysis.

JM: Yeah. You can fill in the gaps.

RM: You can fill in the gaps; you can—like with one station, you can see the vertical structure of things, how they're related, and at what periods and frequencies.

[00:26:40]

So, I sort of latched onto Julian, because he knew how to do this stuff and how to interpret the spectrum. At that point, I didn't have a fast Fourier transform. And I was using the method—because it took so much computer time to do a Fourier transform, the method for spectrum typically was: you first computed an autocorrelation function, which didn't have that many points. The more points you had, the better frequency resolution you had. But you might have a data set that was a year long; but you would only compute, maybe, 15 or 20 lag correlations. You could do the Fourier transform on the autocorrelation function and not on the total record. Well, it didn't take quite as long.

So, I wrote a program that would do that; and with Julian's help I computed the spectrum of the Line Islands data. And I reported on it in a paper at a tropical meeting in Honolulu, in 1970. Some of the results I got were slightly different from some of the other people's results. And already, Wallace and Chang had showed that in the low troposphere the results were not always the same. Sometimes you'd get this five-day wave; sometimes not.

So it was clear that what should be done is to look at long-time series, and find out what's the time variation of these five-day waves.

JM: So that's what you got out of the Line Islands data, was the five-day wave.

[00:28:33]

RM: Yeah. That's what we were mostly interested in: mixed Rossby-gravity waves and the transfer, which were there. But in the troposphere, the five-day waves that Yanai had reported on, and that Wallace and Chang had said are there sometimes, weren't there.

JM: So this is—at that time, they were already recognizing these are mixed Rossby-gravity waves?

RM: Yeah. The 1966 paper, Yanai and Maruyama, they did not recognize it. In fact, it's kind of interesting, because in 1966 Matsuno, Taro Matsuno, had written a theoretical paper, which predicted these waves should be there. And when you read those two papers, I'm pretty sure the—I may get it wrong, which one says which—but I think the Yanai and Maruyama one acknowledges Matsuno for reading their manuscript. But they don't say anything about what Matsuno had predicted. (Interviewer is chuckling.) But then in 1967, Maruyama and Yanai identified them as mixed Rossby-gravity waves. So initially—in fact, I asked Yanai about that, and he said he thought: "Oh, Matsuno's waves were just ocean waves." It just didn't click with him, that what he was looking at was what—so in '66 they didn't get it. But in '67, by '67, they did get it.

JM: And so you knew kind of what you were looking for.

RM: Yeah. I knew what I was looking for.

JM: And you found it.

[00:30:15]

RM: Found it in the stratosphere, but some of the results in the troposphere were different. And so, Roy Jenne and the Data Support Section were starting to get long records. It was hard to assemble a long record, in those days. Especially in the tropics. And so, when we came back from Hawaii, we started looking at these longer records. And the one we sort of settled on was Canton Island, because it was ten-years long.

JM: And the U.S. had a base there, right?

RM: Yeah ... I'm not sure.

JM: So you're using the rawin [spoken like "ray-win"] data and the surface data, right?

RM: Well, the surface pressure on the rawinsonde. *With* the rawinsonde.

JM: Yeah, yeah, yeah. Okay.

RM: So when they released the rawinsonde, they made a little measurement of the surface pressure. So that was recorded.

JM: Okay. So you had it all in the one place—

RM: Yeah. All in one place.

JM: So then you you'd start on the Canton data, then.

[00:31:18]

RM: Yeah. And I sort of took the lead of Yanai and company, and Wallace and company, and did the various kinds of things they did—cross-spectrum analysis between levels—to see if there's vertical propagation of things. Because at the phase shifts, you can see what the propagation is, and so on and so forth.

I can't be sure this is exactly what happened, but I remember there was some stuff going on at five days. This would be, like, the coherence squares between low troposphere zonal wind and upper troposphere zonal wind. And there was something going on, maybe at five days, and I kind of remember bringing it in and showing it to Julian: "Look, see, we got it here," blah, blah, blah, blah, blah. And he looked at it, and he said: "Oh, yeah. That's pretty interesting." He says, "But what is this, over here?" And because we had long records, we could look at longer time scales. And at 40 days, there was a *huge* coherence between the low tropospheric U winds, and the upper tropospheric U winds. And they were exactly out of phase.

[00:32:35]

So we—I did cross-spectrum analysis of every variable at that one station. And we speculated that it was large-scale circulation cells. But we didn't have any other data; we only had Canton. And—

JM: And so this is unique, right? Because nobody had analyzed a long record like this, to be able to see that time scale.

RM: Yeah.

JM: They were all looking at the shorter time scales, because they just had shorter data sets.

RM: Yeah. So we were lucky that we had the help of the Data Support Section, to have the long—oh, and then, I mentioned the fast Fourier transform, because with ten years of data, you've got, say, 3,600 [spoken like 36-hundred] daily values. And to do a Fourier transform the old-fashioned way would have just taken so much time on a computer. And both the 7600—which came in 1971—and the 6600 had clock speeds on the order of megahertz, where now a laptop is gigahertz.

JM: Right, right, right.

RM: So although they were big computers for the time, they were not that fast. So the fast Fourier transform was *very* important.

JM: Yeah, yeah, right. So you identified this peak, out there at 40 days. So then what'd you do with that information?

[00:33:59]

RM: I analyzed every kind of variable (laughs), and we got this picture—from the single station—that there was probably some kind of large-scale circulation cells that were varying. But we didn't know whether they were moving, or standing in place varying, but they were varying on a time scale of about 40 days. So, of course, we submitted that paper; we were then well on the way to start looking at other stations.

JM: So how was that paper received? Did you get some critical reviews? Or was there skepticism?

RM: Umm.

JM: I mean, you were kind of this young guy on the block, and here's these legendary tropical meteorologists, these Japanese guys that had been looking at this stuff for 20 years already, probably, right?

RM: Yeah.

JM: And so—but there was no—

RM: I remember meeting Yanai at some meeting in the late '60s and, I mean, he's such a gentleman. And I mean, there was no—there was no kind of bad talk from them [cross talking from interviewer].

JM: They didn't resent this young guy coming in there—

RM: No.

JM: —with this glitzy FFT analysis, and coming up with this weird peak that nobody had seen before.

RM: Not at all. I think, in general, the reaction was, kind of, silence. (Laughing.)

JM: Silence, meaning they were skeptical? Or they didn't know what to think about it.

RM: I suppose they thought: "Well, this is kind of interesting." But they were interested in other things.

JM: Oh, okay. They were still looking at the short time scales, probably.

[00:35:36]

RM: Yeah, yeah. Now one thing about Julian—because we found this peak, but we had no a priori, ahead-of-time reason to expect this peak. If you said, "This is significantly different" from zero, at the 95-percent level, that means there's a five-percent chance, randomly. And since we have no theory or anything to predict this thing, it could well be the five-percent chance. Well anyway: in our first paper, Julian had a long discussion of this, and made the statistical criteria much more stringent than five-percent. And, um, it kind of—later, Livezey and Chen, they wrote a paper which was along the same lines of Julian's arguments in our first paper, about statistical significance.

You know, I don't have a real count, but I think *that* part of our paper got almost as much attention as the wave itself.

JM: (Laughing) Just the statistics.

RM: Yeah. The statistics. And that was all Julian's stuff. So, let's see.

JM: So then what happened next?

[00:36:57]

RM: Okay. So then we started looking at other stations, and I got a lot of pressure data. Actually, I think most of my pressure data was off the rawinsondes.

JM: The sea level pressure.

RM: Yeah, yeah.

JM: From other tropical stations, from around the world.

RM: Yep, yeah. And what turned up—what showed up right away was: I used Canton as kind of a reference station, and did cross spectrum between all the other stations. And when you looked at the coherence squares, pressures were coherent at 40-day periods all along, say, the 10-north to 10-south. And the phase angles showed a regular eastward propagation. And also kind of a spreading away from the equator. And we had six rawinsonde stations, only. I mean, it's just—this is a story of spectrum analysis. It's the *power* of spectrum analysis. Because we had six rawinsonde stations, and I did cross-spectrum between, like, the reference series was that surface pressure or sea-level pressure—I think it was surface pressure—at the Canton Island, and the winds at all these other stations.

[00:38:16]

And after I figured out a way to plot it, it was so much information, I couldn't—nowadays one would do a complex EOF analysis, or something like that, and a pattern probably would pop out. But the way we were doing it in those days, I had all these phase angles, and coherence squares (chuckling), and to make sense out of them—finally, at one point, I came up with this idea—which is a figure in our second paper (paper rustling)—to plot this stuff. And it showed that these were big circulation cells. (Sound of pages flipping.) It's Figure Six of our—

The length is, let's see, the length tells—

JM: Okay, so this is Figure six of— [looking at something]

RM: This 1972 paper.

JM: Madden-Julian 1972—

RM: Yeah.

JM: —*Journal of Atmospheric Sciences*.

RM: Yeah. The length of the bar tells the phase angle. When the pressure is low at—Canton, I think it was—and I came up with this picture. And you could see in the upper levels, convergence. And the lower levels, divergence.

JM: Big circulation.

RM: Yeah. And also, we had that eastward propagation, which came out real nicely with the pressure.

JM: But I thought you said, at one point, you weren't seeing the eastward propagation; that you were seeing it in the winds but not the surface pressures. And then you were saying you'd have to plot it out by hand.

[00:39:56]

RM: Yeah, yeah. That's an interesting story. That's kind of a eureka moment.

JM: Yeah. So, what was—

RM: Well here's the spectrum analysis [looking at something]; these lines are phase angles.

JM: This is Figure Four, now. [*https://doi.org/10.1175/1520-0469(1972)029<1109:DOGSCC>2.0.CO;2*]

RM: And negative phases means the pressure perturbation occurred first here, and then at Canton. And positive phases later. So you can see, we kind of guessed that it started in the Indian Ocean. And these phase angles suggest eastward propagation, and also a spreading, a little bit.

So, this was all with spectrum analysis. So, I mean, I thought it was a really powerful analysis technique. But I still—I didn't trust it quite—I wanted to *see* it, in the pressure. So I plotted up a time-longitude diagram—or it's sometimes called a Hovmöller diagram—of the pressures along the equator. And I couldn't see—there was kind of a little jiggly—but no regular eastward movement.

[00:40:58]

And my memory says that it may have been the summer of '71 or so; and, whenever it was, Krishnamurti was visiting. And I showed this to Krishnamurti, and I said: "The spectrum analysis says this should be eastward movement, but I don't see it with this pressure." Well, of course, the spectrum analysis, I'm only looking at 40-day periods. But with the pressure, I'm looking at *all* periods. And, especially the low-frequency stuff, like seasonal variations.

Now I know all about filtering and stuff, but at that time I don't think I knew so much. And Krishnamurti said to me: "You ought to take out the time mean." And, of course, that's a high-pass filter. So that filters out the low-frequency stuff, and allows the relatively high—which, 40 days *is*, relative to, say, seasonal or inter-annual variations. Well, anyway, the way we would do this, these Hovmöller diagrams, was to print them all out on the big—remember the big—

JM: Computer printout sheets.

RM: —computer printout sheets. And print the numbers out like that, and then draw—you would analyze by hand. So, that's the way I did it. And I couldn't see the eastward movement. So it was kind of late in the afternoon; I had time to change my program to add computing the time mean, and I printed that out at the bottom, but I had to go home for dinner. So, I took it with me. And when I got home, I remember kneeling in front of our couch, with a board or something, and putting this computer paper on, and subtracting by hand the mean that I had computed. Figuring, tomorrow I was gonna do it by the computer and get it all done.

[00:43:03]

But after about six, or maybe ten lines, I could see this eastward movement. And my kids—our kids were, like, from three to eight, and they were playing around in the family room. And when I saw that, I said: "We're gonna be famous!" (Laughing.) And I remember, my kids got the enthusiasm. And they're dancing around, "We're gonna be famous!" (Laughter.)

JM: They had no idea what for, right? (Laughs.)

RM: Yeah, right. So anyway, I saw the eastward movement *without* a spectrum analysis.

JM: Right. Which, then, was—that was the MJO.

RM: Yeah.

JM: Yeah. *Later* called the MJO.

RM: Yeah.

JM: But at that time, you were calling it something else, right?

[00:43:43]

RM: We called it the 40 to 50-day oscillation, because it was kind of the half power points on our big coherence squares. It didn't get much attention until the MONEX experiment, because a couple of these oscillations occurred, and a guy called Lorenc—maybe? You might know him; he's from the European Centre for Medium Range Forecasting. L-O-R-E-N-C. He did a paper showing the velocity potential, moving eastward, during this MONEX period. I don't think he knew about our paper; I'm not sure about that. But then, after that, Klaus Weickmann did his PhD thesis. He called it the 30 to 60-day oscillation, which kind of took in more of the period. Probably it was a better name.

JM: And that would have been late '70s.

RM: That would have been—his thesis was, like, '85 or so. 1985. During the '70s, there were a couple papers; one by Arnold Gruber, and another by Zangvil, where they did space-time spectra of satellite data. In our—

JM: This is a great— [Not a response to narrator. Referring to something in front of him.]

RM: Yeah. This, kind of, cartoon thing.

JM: This is Figure 16.

RM: We had no satellite—yeah, Figure 16. It's, sort of, a lot of people use it to describe the MJO. We had no satellite data. We just knew there was this low-level convergence and upper-level divergence. And also some evidence in mixing ratios increased.

[00:45:32]

JM: Did you look at precip data from the station?

RM: Not at this time.

JM: Oh. Because you could have—in theory, you could have seen it in the precip data, right? When these big, convective masses go by.

RM: Yeah. Sometime in, maybe, '73 or '74, I invited Jim Sadler, from Hawaii, who had been subjectively gridding satellite data for quite a while. And he brought his satellite data. And so I started looking at that. And I filtered in space to wave one and wave two, and you could see the eastward movement. And then, also, I got some rainfall data from the Indian Ocean, and composited it according to the winds at Truk/Chuuk Island, I think it was. And you could also see precipitation variations. And I reported on that in, probably, the mid '70s or early '70s, at a meeting in Miami, I think. But I was afraid of trying to publish it, because I thought maybe I was filtering it too much. I didn't like this wave one, wave two stuff. So, I just set it aside.

So we had kind of looked at precip.

JM: But, I mean, this schematic here, in Figure 16—you were just assuming that there was a convective mass that was—

RM: Exactly.

JM: —just based on the winds. Figure it had to, if you have low-level convergence or divergence, there had to be a big convective mass.

RM: Yeah. And there was some—a little signal in mixing ratios.

JM: Mm. Okay.

RM: Which was consistent with more precipitation or more moisture in the atmosphere.

JM: So, I mean, this paper—this is the classic MJO paper, right?

RM: Yeah.

JM: This is the 1972 paper. So when this came out, what was the—I mean, do you remember the review process? Were people critical of this?

[00:47:38]

RM: Yeah. One reviewer—I can't remember—didn't have too much to say; said it was okay. Another reviewer said that Gan Island—let's see, I think maybe by the time the paper finally did come out, we added Gan Island.

JM: In the Indian Ocean.

RM: In the Indian Ocean. I'm pretty sure it was Colin Ramage.

JM: Oh.

RM: Who, unfortunately, I just saw—in the Bulletin—passed away.

JM: Oh!

RM: But anyway, he was a tropical meteorologist. He knew about all of these data. And he said, "Gan Island; you should get Gan Island and see if it fits in with your picture." Now, I'm sorry, I should have reviewed this, but I can't remember—I think we got a hold of Gan Island, and I think, in the revision, we included it.

JM: Oh, okay. Yeah, yeah. But other than that, they were—

RM: There was no big argument. I mean, the statistics, with Julian, (chuckles) was ironclad, you know?

JM: But the thing was: this didn't fit into any known tropical wave phenomenon.

RM: No. It kind of looked, maybe, like a Kelvin wave? But it was a 40-day period, so the theory said the Kelvin wave should be 10,15 days.

JM: So this slow, eastward propagation—I mean, people still struggle with this, right?

RM: I don't think it's fully understood, to this day.

JM: But that—you would have thought a reviewer would have picked up on that and said, "Well, where is this coming from? Why should this exist?" You'd think somebody would have been critical. Did you get any comments like that?

RM: You know, I'm gonna have to go and look. I'm not sure.

JM: Oh, okay.

RM: We didn't have trouble getting it published. But somebody might have said something like that.

JM: Yeah, yeah.

RM: We just had two reviewers. And like I say, one didn't have too much to say, is my recollection. And the other, the only thing I remember was: "You should get this Gan Island."

[00:49:41]

JM: The TWERLE project came up in the mid '70s, and Paul Julian was the chief scientist for that. Did you work on that at all? Because, I thought one of the objectives of that—

RM: I didn't, and I'm—you know, that certainly would have been a data set that would have been, at the time, would have been really helpful.

JM: Because, that's the one I was involved with. And when I look at the literature coming out of that, it seemed like he was looking for intraseasonal variability in the upper-level wind data from the three tropical stations, through launching the constant-level balloons, and they were up at about 150 millibars. And Dennis Shea was doing the wind maps, just based on interpolating the tracks. And there *was* some evidence of intraseasonal variability, in those winds, which were, basically, tropic wide—and then going to the southern hemisphere, but the initial thing was looking at the tropics. But I always thought that was one of the motivations for Paul getting involved with TWERLE—or leading it, I think, was looking—

RM: Yeah. Was he the chief scientist?

JM: Yeah. He was the chief scientist, yeah.

RM: Well, weren't there occasions when you'd send a balloon up, and when it got up in the upper troposphere it would go toward the east? And then another time it would go toward the west.

JM: Exactly. And they'd kind of bunch up in locations where there was upper-level convergence. And he could see that happening.

RM: I don't know—what year would that have been?

JM: That was 1975, '76.

RM: Okay. I think in '75, '76, I had moved on to long-range predictability and stuff like that.

JM: Oh.

RM: Because, that—I mean, I often think back: Why didn't I look at those data? (Interviewer chuckles.) I mean, had the perfect opportunity.

JM: Yeah. But, CSU came into play, here, at some point. Where did you—you were doing stuff at CSU, too.

[00:51:32]

RM: Yeah. Um, I started—well that was another lucky thing. I came in '67, and I think—and like we touched on earlier, I was learning tropical meteorology on the job. Because, the nearest tropical meteorology was Cape Canaveral, Florida, which is, whatever, 28-degrees north.

But, a lucky thing was that CSU started to offer courses—they would videotape these courses, and—a big tape, which was, like, maybe a foot across—and we had a recorder that was, well, it was three-feet across (chuckling) and two feet high. It was a huge thing. And we could play these tapes in the Director's Conference Room.

JM: Oh!

RM: And so I started to take courses. And the first one was tropical meteorology, taught by Herbert Riehl. It couldn't have been better. He was, like, the most famous tropical meteorologist at the time.

JM: And he'd started the department at CSU, didn't—

RM: He started the department at CSU. So this would have been in, I think, the spring of '68. So, I started taking courses there. I remember going up, and wanting to get into a PhD program, and being told: "You've got to move up here; you can't—" By this time we had four kids, and I wasn't about to quit my job, you know? But anyway, I kept taking courses this way, with the tape business. And then occasionally I would drive up, a couple days a week, and take a course. And probably five years later, I went up and I had all the courses. So then they accepted me in the PhD program.

JM: Oh, okay.

[00:54:17]

RM: Oh! And another good thing was that Bernhard Haurwitz had just gone up there. And I was interested in Rossby waves; and, of course, Bernhard Haurwitz was the expert on those. And he became my adviser.

JM: So, did you have to write a thesis, then?

RM: Yeah. I wrote a thesis about the 5 and 16-day wave, I called them. They were normal-mode Rossby waves.

JM: Hmm. So this was based on the Line Island data? Or was this—

RM: No, no. This was based on—well, there was one analysis that was done for World War II. It was the surface pressure on the Northern Hemisphere from, like, 1900 to 1939.

JM: Wow.

RM: Yeah. I think they did it for analog forecasting, for the landings. So they—in a way, it was like a reanalysis project, in 1939.

JM: For the Northern Hemisphere.

RM: For the Northern Hemisphere.

JM: The whole hemisphere.

RM: Yeah. And then—

JM: These were, like, daily data.

RM: Yeah. Daily maps.

JM: Daily maps.

RM: In fact, we have books, in the library, of these maps.

JM: So you have to go through and somehow pull data off those maps?

RM: No, no. They were digitized. I don't know who did that—I can't remember who did that.

JM: So you could analyze it. So it *was* like reanalysis data. You could write a Fortran program to analyze—

RM: Yeah.

JM: And pick out these, and do a spectrum analysis.

RM: And then, I think I must have had—not reanalysis data, but operational data daily maps. That's funny, I can't remember.

JM: So, what year did you finish your PhD, then?

RM: 1978.

JM: Wow! '78.

RM: Yeah. So I went to—took courses there for almost 10 years.

JM: Geez! (Interviewer and narrator both laugh.)

RM: But I never had to move up there! (Laughs.)

JM: Never moved, no. It's interesting, you had those big tapes. It was like: remote, distance learning; early distance learning. Which is now very popular.

RM: Yeah.

JM: Huh. See, and it worked for you.

RM: It *did* work for me.

[00:56:26]

JM: So, you'd kind of—you published this stuff on this 40 to 50-day, and then later 30 to 60-day. And other people were kind of looking at it. But then you kind of left it, you said. Or did you pursue it, then, after?

RM: Well, in the '70s, nobody picked it up. And I mean, I didn't have the kind of confidence to push it, which I should have done. But.

JM: But in MONEX, they came up with some results—

RM: MONEX—there was one or two of these oscillations. And they were so very clear.

JM: Because, that was '78, '79.

RM: Yeah, but Lorenc's paper was, like, 1982 or '83.

JM: Yeah, 'cause that was—so I was in—we were looking for intraseasonal variability. 'Cause I was out on the north coast of Borneo, with a portable radiosonde set. And I was there with the MIT radar. And we were doing two balloon launches a day, at 0Z, 12Z. And then we get an alert to start launching *four* a day.

RM: Ahhh.

JM: And they detected, like, a cold surge coming down off of China. Or, there was something going on. Who knows what was going on. But we had to—they wanted these intensive observations to pick up whatever was going on, which was really difficult for us. Because then, that meant we had to—every six hours, around the clock—we had to launch balloons. But that's interesting, that they were—that somebody *did* analyze some intraseasonal variability, probably from the winter monsoon, right?

RM: But when you were looking at intraseasonal variability, there was no reference to our papers.

JM: I didn't know what was going on; I had no idea what any of this stuff was. We were just told to start launching balloons.

RM: I don't think anybody really—not anybody, but most people—paid much attention. This was an interesting thing, but "we're interested in something else." (Chuckles.)

JM: So, your interest in this—did you, kind of, keep pursuing this? I mean, how did that work out, then?

[00:58:35]

RM: I think—I remember Julian was somewhere, maybe in England. And he—did we have email then? Or maybe he wrote me a letter. I'm not sure. Somehow he got in touch with me, and he said that—what's the guy's name, can't think of his name right now [amended to add that the scientist was Yasunari]. That he was—*had*, or was about to publish a paper about some kind of clouds going northward over India every 40 days or so. And Julian says: "We've got to resurrect that stuff you did about the satellite and the rainfall. So I said, "Okay, yeah." So he more or less resurrected it. And we had a short note, I think—he was first author, and I was second author—in, probably, about '83.

So then, I think I started getting interested because I saw other people were starting to be—

JM: But this is looking in summertime, northern summer, looking at the northward propagating intraseasonal variability over the Indian monsoon.

RM: Yeah, that was summertime.

JM: And before, you'd always looked at winter, right? Because you were looking for the—the 40 to 50-day wave was a wintertime thing that you were tracking, right?

[01:00:02]

RM: Well, in some ways, like when we looked with the Canton Island data, we looked all year—we just took time series. And in fact, that's another interesting aspect: we concluded the meridional, v wind, was not involved. Because there was no coherence between u and v. I learned later, that when you broke it up by season, the v wind was very *much* involved. It was very coherent with the u, but it was coherent and *in* phase in one season, and coherent and *out* of phase in the other season. And when we didn't pay attention to season, in our early papers, we missed that. Completely missed it.

JM: Is that just because it was—the latitude of—the maximum of the MJO was shifting, and so the v wind—

RM: Yeah.

JM: —that you were getting low-level convergence—

RM: It was, it was. Let's see, in northern winter, the convection was mostly south of the equator. So, there were surges from the southeast. So southeast, that's out of phase, right? Yeah. East is a negative, and south is positive. So it's out of phase. But then in northern summer, the surges were from the northeast. So that was *in* phase; they were both negative. But when we strung it out, not paying attention to season, we missed it completely.

JM: Right. But then, that's—this [Japanese] scientist [Yasunari], detecting this intraseasonal variability over the monsoon—that got you interested in, kind of, revisiting it and looking at summer seasons as a different kind of thing, maybe.

RM: Yeah, I think—'86, I think I had a paper about that, showing this kind of interesting thing.

[01:01:55]

JM: So, Klaus Weickmann's thesis was on the 30 to 60-day, building on the stuff you guys did.

RM: Yeah.

JM: And you say, that was in the early '80s.

RM: That was, like, '83.

JM: In '83. So there was *some* interest. And then you guys kind of rekindled it when this intraseasonal variability, over the Indian monsoon, came up. But then—

RM: I can't say that we really—that little note that Julian and I wrote about—I mean, I don't think anybody—I don't remember that even being cited. I can't say that *we* did it.

JM: So, when did the MJO become so popular? Or, how did that happen, do you think?

RM: Yeah. Bill Lau had a paper in the Bulletin, which—I'm not sure, probably was about '85 or '86—and on the cover of the Bulletin, he showed these clouds going across. I mean, it was just amazing; it was on the cover of the—and then he had an article, telling about it, in there. So that was a big, important—

JM: Citing all of your work with Paul.

RM: I'm sure, he probably cited our papers. And Weickmann's thesis was important, too, to make it popular. And then, in 1987, there were two papers that said "Madden-Julian Oscillation" *in* the title.

JM: Oh!

RM: That was 1987.

JM: '87. So that was kind of, then, the start of the modern interest in the MJO, then. The *naming* of the MJO.

[01:02:39]

RM: I would say, yeah. Starting, probably, in about—Lorenc's paper in '83, and then I think Weickmann was about '85.

JM: But you guys never called it the Madden-Julian Oscillation, obviously. You were calling it the 40 to 50-day wave—

RM: (Laughing) No, we didn't. (Laughter.)

JM: You probably didn't object to them calling it—

RM: No, no. We didn't object. (More laughter.)

JM: Because, you told your kids you were gonna be famous. They were waiting for something to happen.

RM: Yeah, right! (Laughing.)

JM: Twenty years later, they're still waiting. "We're gonna be famous, what's happened?"

RM: Yeah, right! Yeah, by that time, they—what the heck? They were almost 30 years old! (Laughing.)

JM: So then—so now, the MJO is a big deal, right? Because, for intraseasonal prediction and everything else. But, like you say, the basic—the theory of it is still a little unclear, right? Why you get this very slow eastward propagation of these large, organized, convective masses.

RM: Yeah.

JM: And then the other variant was the westerly wind burst associated with that, coming across the Maritime Continent can trigger El Niño events. So there's all these things that kind of started coming out of this MJO phenomenon, that people like to look out now, right? Mid-latitude teleconnections; so, for short-term prediction, you can figure: "Okay, if the MJO is coming across, you maybe get some teleconnections, which can affect mid-latitude weather and give you an edge on seasonal predictions." This has come up again, now, with this—what's now called S2S, sub-seasonal to seasonal prediction. Which is now a whole new area of initialized prediction, where you're trying to predict these time scales. And a lot of it *does* depend on the MJO, as something that at least you can identify that's happening on these time scales, that could affect a lot of things in different places.

So it's still—it's really an active area of research, and you started it.

[01:04:51]

RM: Yeah. (Interviewer chuckles.) A thing I got interested in, in the mid '80s, was the rotation of the Earth. And kind of the way I got interested in it was, at least in part: Ray Hide, who was a British—he had, like, rotating tank, kind of stuff. He did that sort of thing. He came to look at FGGE data. And Dennis Shea helped him. He compared the angular momentum that he could compute from the FGGE data of the atmosphere, with the rotation of the Earth. Because, the Earth atmosphere ocean system maintains constant angular momentum. So if the Earth—if the winds pick up, and the angular momentum of the atmosphere gets bigger, the Earth's rotation slows down. (Chuckles.) And so the length of day is longer. But we're talking about milliseconds. So it's not something you notice. (Laughter.)

Anyway, there was, like, a 40-day period, I think, in Ray Hide and Shea's paper.

JM: For length of day.

RM: For length of day, and total atmospheric angular momentum.

JM: Mm-hmm.

RM: And, I think there was a paper by some French people, who also showed that, during FGGE. So I got surface-wind data, to look at surface-wind stress and mountain torques. That's the thing—those are the two things that exchange momentum between atmosphere and solid Earth. And I was able to show that the MJO—when the clouds pick up, say, in the Indian Ocean, the easterlies really get strong. And so, they're slowing down the atmosphere—I mean, they're slowing down the rotation of the Earth, and they're picking up westerly momentum from the rotating Earth. So the momentum is leaving the solid Earth, going into the atmosphere, when these clouds are building up in the Indian Ocean. And then, later on in the cycle it's reversed.

[01:07:13]

JM: It's amazing you can measure length of day to that—

RM: It's amazing.

JM: —level of accuracy.

RM: Yeah. The seasonal variation, which I guess was known for many years, is on the order of milliseconds. Like, one or two—that's a thousandth of a second, the length of day. [Making mental calculations.] There's 86,400, something like that, seconds. Eighty-six thousand, four-hundred seconds (laughing) in a day. And just one or two milliseconds, thousandths. How they measure that, it's amazing. But anyway, the MJO's effect is about a tenth of a millisecond. So it's about a tenth of what the seasonal variation is.

JM: Yeah, yeah. That's amazing. So maybe just one last thing: since you have now been linked, by history, with Paul Julian.

RM: Yeah.

JM: Um, kind of: what was your working relationship like with Paul?

RM: Um.

JM: Because, he was senior to you, obviously.

[01:08:19]

RM: Oh yeah, yeah. He was senior to me. He's just about ten years older than I. It was good. He—and I think Dennis Shea will tell you this story (chuckling) too, because we both worked with Julian. You had to kind of be—have humility (laughing). But if you had humility, he was smart. And you could learn from him.

RM: But, the basic answer to your question is: we had a good relationship.

[01:08:54]

JM: Okay, yeah. You were able to work with him, because he treated you—he, obviously, was interacting with you.

RM: I mean, he would—I'd say something, and he might ridicule it. But I knew he respected me. (Laughs.) Deep down! (Laughing.)

JM: Are you still in touch with him?

RM: Yeah, occasionally. Maybe I heard from him a month ago, or so? Yeah.

JM: So, I mean, this whole thing—just the last piece would be—this whole thing with the MJO, when you first figured out, like that "Eureka!" moment in your house, on the living room floor, at that time you probably didn't ever think it was gonna be that big a deal, right? Or maybe you *did* think it was gonna be that big a deal. Or, what did you think was going to happen with that discovery?

RM: Well, I must have thought, when I saw that eastward propagation, that it was something new that nobody else had seen before. So it was important. But, I mean, I don't think I ever thought that it would—that it apparently has, you mentioned, such a big impact on the general circulation.

[01:10:04]

JM: Yeah. And models are still struggling to simulate it, because nobody quite knows how it's supposed to work. People worry about it—if your model has an MJO in it, or not. It's great if you have an MJO, but maybe the phase isn't quite right. Or maybe something else is not right. So people agonize over trying to get that in the models now. And so, it's a big deal. And it's *the* one thing; it's like El Niño and the interannual time scale. It's the MJO and the subseasonal time scale: that's the one thing you can look at and say, "Okay, this is something that we can try to predict, that has big impacts." There may be other things, but at least that's one thing you can kind of identify. And so, that was your contribution. (Chuckles.) And you're famous!

RM: Yeah! (Laughs.)

JM: Alright. Did you have any other questions? [Directed at Laura Hoff.]

LH: I guess, just, is there anything else you want to add? That you think we didn't cover yet, that would be important to include? Anything, looking back?

RM: No. I think the interview is thorough.

JM: (Laughs.) Well, thanks for doing this.

RM: Oh, you're welcome. Thank you for inviting me.

[01:11:16]

JM: Because, see, he was telling me the story about being on the living room floor. And I asked him if that had ever been recorded. And he goes, "No." And I said, "Well, we gotta record this. It has to be recorded somewhere." (Narrator is laughing.) And then I ran into George Kiladis—another acolyte of Rol's, MJO person—at one of these CMF concerts [?], and he goes, he says, "You know, AMS is doing these interviews, these oral histories." And he says that maybe you should clue in AMS, that we have this. And they may want a copy; I don't know exactly how that works.

LH: We do work together with AMS, on the oral history projects—

JM: He was really glad to hear that we were doing this. He says, "Yeah! That should be recorded somewhere." Because he'd heard the story. You told him that story.

RM: Yeah.

JM: It's a good story.

RM: There's a lot of luck involved.

JM: Do your kids, now, realize—are they aware of this at all? That the MJO's kind of a big thing?

RM: I think so, yeah. In fact, right now my oldest daughter is writing a Wikipedia for me.

JM: Oh! You're gonna have a Wikipedia page. (Laughs.) You better edit it, to make sure it's accurate.

RM: Oh, yeah. (Laughter.)

LH: You can link to this interview! (Laughs.)

JM: Link the interview, yeah, right. Alright, I think that's it.

RM: Okay, thank you very much.

JM: Yeah, well thanks for all—

RM: Appreciate it.

[01:12:40] [End of recording.]