

**Conversation about Reuel Stallones
Between Darwin Labarthe and Dwayne Reed
Recorded by H. Blackburn
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Abstract:

Quotes

Psychosocial variables

I looked up to him as a pioneer in this field [Len Syme] and I thought it was interesting stuff. Again, always looking for something different other than the standard risk factors for what's going on. And that was great opportunities when you find them. I don't know where the idea came from, but he proposed that we not only can look at individual measures, individual differences but at the ecological things. That whole concept. This is the true learning – that just blew me over in terms of understanding these kinds of complexities of epidemiology. Again, this was also very early in the psycho-social epidemiology stage and I don't think there were any other prospective studies going on and so, again, we fit it into the cross-sectional.

But even our cross-sectional were the first negative studies. They would have said, "You can explain the major differences that you found based on the standard risk factors for cardiovascular disease." And I think Len never quite forgave me for that. We certainly went on to the Nihonsan Studies in Hawaii that have continued to find negative results related to these psychosocial things. (24)

While this was going on Stoney had gotten the funding for the ‘san’ part of Nihonsan. He had had much to do with the whole Nihonsan concept. [the origins, the idea of such comparisons.]

His connections with Japan may have begun during his Korea time when he had periods of being either based in Japan or going back and forth. But before that he had been in Japan because of stroke. He had gone to Hisayama where the Hisayama Study was going on and he had been involved with Jim Tool and others in this questions about whether the strokes in Japan were all hemorrhagic because they have so much hypertension, and trying to improve the comparability of data on cost-specific mortality from stroke. Then he did a review of stroke epidemiology in the *Journal of Chronic Diseases* maybe ‘65ish. As far as I know it was one of the first if not the first epidemiologic effort to put together what was known....about stroke epidemiology.

[And I suppose that was part, at least, of how Nihonsan got underway.

The intertwining of all of these influences, those early origins of Nihonsan, migrants in Hawaii later on, getting involved with that, Len Syme coming over when Stoney left. It’s incredible to sit down and try to figure all these things out. I know Abe talked about meetings taking place in Zukel’s office. Kagen was at Framingham. I think that Yano was somehow involved. Yano had come to visit Framingham and was totally amazed at seeing so much heart disease. There was some kind of influence from him and Stoney there. He talked about Stoney and I don’t know where the idea, “Let’s do a comparison of migrants” crossed.

I know Stoney was very active in the origins of NIHONSAN and, of course we know he was very hurt when he went to Texas he was basically cut out of the ‘SAN’ part of that. And he carried that resentment with him, I think. So, again, some of his influence on

some of these early studies, which have gone on for another 30 years after his initial involvement.(30)

We corresponded. I would send tapes from Guam about what we were working on. Actually Stoney even came out. He knew nothing about ALS, but he was chosen for one of the site visits to come out and see what we were doing. And, again, his presence just crystallized things. We had different studies going and he was able to look at the raw data, graphs we had drawn up, and immediately just come up with, “Ok, have you thought about this, have you done this.” Key questions immediately supporting his ability. He’s clearly the smartest epidemiologist I’ve ever met.

DL: I was thinking about the most substantial epidemiologic studies that he was directly involved in. He certainly influenced many. He was directly involved in several. Among the things that stand out certainly Nihonsan was one. The work in the Seal Beach community was another. There was something called the King City Study which he had done early after arriving at Berkeley which was in King City, California. It involved a population sample, family studies of coronary risk factors.

D: It started off with high school students and then recruited their parents or something like that.

DL: I don’t know the details and I don’t know what the publications were from that study.

D: I don’t know if they ever published.

DL: There was a younger physician who was part of that team who was one of the people we had as a lecturer in Epidemiology 101 and his name escapes me at the moment, but it’s someone that I know Stoney had intersections with off and on in his career.

D: Something was published because I remember the correlation between blood pressures, rank order of blood pressures in the

students with the rank order blood pressure of their parents. Something like that.

DL: And then there was a guy at Berkeley he did some work with....early in the days of analysis of large datasets. This was Buechley and the one that stands out in my recollection is the correlation of coronary mortality by state with various state characteristics.

D: One of which was number of lynchings.

DL: Lynchings was one. The greater the number of lynchings reported in whatever time period, the greater the rate of coronary mortality. And there was also a direct association between numbers of psychiatrists per state and the coronary mortality. And a number of other like observations.

D: I remember that it had to do with physicians per capita directly related to the number of coronary deaths, cardiovascular deaths, and I think of hospital deaths, too. All these kinds of things. Yes. That was fun stuff. I don't know if it ever really changed the world, but just wonderful fun ideas that they would come up with. Was it the hardness of the water? Maybe that didn't come out of that one. I'm not sure. (35)

The New UT Houston

When the Texas opportunity came up I don't know what all the details were at Berkeley at that time, but Chuck Smith had died suddenly. There was a vacancy in the Berkeley deanship. My impression is that Stoney had anticipated that he would become the dean.

D: And he wanted it.

DL: Wanted to be the dean and Bill Reeves was selected instead. Maybe by that time it was already apparent that the 'san' part of Nihonsan was not going to be renewed.

D: I don't think so.

DL: Or that came about shortly after because I had always thought that was part of the equation.

D: It was quite a bit later. When I left Houston two years after going there I went to NINDB, Len Syme wrote me saying that they had been badly treated by the review committee for continuing the 'san' part. So this must have been '71 and could I do anything about it. They had made the mistake I think of going to the Neurology Institute for the grant rather than Heart. This is the renewal we're talking about now.

DL: Well, the original application I think as I recall had been for the baseline survey and there was never funding for a longitudinal component. There was the possibility of doing some mortality follow-up maybe of the survey participants. I think it never had the same kind of financial security that Honolulu did.

D: Honolulu was supported by contract from the Heart Institute, whereas the 'san' part – I don't know when it went to Neurology. I don't know Stoney's participation in the first phase of it. But whatever, it was either a renewal or going to a different institute that Len had contacted me and this is after the first, I think, the baseline exam was done.

DL: I remember Bill Zukel being mentioned in all this and I'm wondering if it was perhaps a renewal application had gone to what was then NHLI and was not favorably reviewed and maybe Len was pursuing the possibility of NINDB.

D: Maybe so. I don't know. That was always sad. Not only that, but all of the baseline data were lost because at one point I wanted to do a comparison of ... I can't remember what it was. It was a follow-up based on some of their stuff and contacted Len and it was stored in some basement or gone or lost or flooded or who knows what. Lost. So that was a sad aspect of that beyond the baseline exam. And only two years of mortality follow-up.

My feeling is that part of Stoney's decision to take the deanship in Houston is based on the fact that he didn't get the deanship in Berkeley.

DL: Yeah, that must have been so. And then his maybe liberation from the Berkeley tradition he had the opportunity to think about how a school of public health should really be organized and that led to the sort of radical idea of a school without walls and a school without departments.

D: Yes, again, a totally new pioneering concept of education in public health and developing a matrix approach to course work or to organization. (43)

Academic Innovation

If you read some of these, again part of searching out the things, some of those papers on what is a school of public health or how to start a school of public health, things along that line, these are totally new ideas, innovative concepts. Where did he get them? He wasn't trained in this in any way either in education or this kind of administration, but he just fell into it naturally and, again, with his creative capacity just began to put together these wonderful ideas.

DL: His approach to faculty recruitment was to recruit good people and let them go. There was very little direction.

D: Dick Remington being at that meeting at Coronado Island and then showing up as the head of statistics. I guess you and I should feel very proud that we were considered in that group to be enlisted.

DL: That's true. And maybe the earliest years of the school and for quite a while what became of the school was most satisfying to Stoney.

D: Every once in a while you get an opportunity. Somebody says, "It's all yours. Do with it what you will." And here was his chance. Every detail – designing a building that's going to do this, designing educational concepts. How are you going to

teach public health? What is public health, community health? Thinking those things through it's just amazing to me this guy's creativity. I have done a lot of looking at epidemiology theory and I don't think he copied these ideas from somebody else. Nobody had talked about these kinds of things that I've seen anyway. (47)

Interview

DL: This is a second opportunity to record some thoughts on the history of CVD epidemiology from Stoney Stallone's perspective, his colleagues taking his contributions into account. We may do more or less justice to these, and Dwayne and Darwin will supplement this with material from some of Stoney's files that are in Dar's basement in Atlanta.

D (DR): That's great. I met Stoney when I signed up for the MPH program as a year's deferment from the Yellow Berets (the US Public Health Service) draft, which delayed my going to Viet Nam, that amount of time. Had no idea what to expect and meeting Stoney was one of those things that happens rarely in your life where you meet somebody who changes your life. Clearly a fork

in the road. I'm not sure how long it took – a few weeks – but within that time, from having a vague idea of maybe becoming a missionary doctor somewhere in Central America where they needed physicians, I discovered the spirit of epidemiology, and knew that – ah ha, this is what I want to do. It was all from him.

The whole course, the whole year of study there..... Basically there was very little information about what epidemiology is or about theory or complex statistical formulas. It was the drama of what we do and the whole concept that epidemiology is different. In fact, after leaving there and going to Washington and meeting people like Manning Feinleib and Jacob Brody I became a disciple of Stoney's ideas.

The question would come up – how does epidemiology differ from other medical sciences? And I realized then that nobody else had ever thought about those kinds of things the way he did. He had really sat down and spent a lot of time working out what were the basic

principles of epidemiology. I've never seen these set out anywhere else. A couple of textbooks since then are sort of moronic platitudes. But Stoney really set down and thought about whether there is a principle to what we do?

Indeed he had framed it very nicely and I think those simple kinds of things are what affected me most all my life.

DL: Your first encounter with Stoney at the School of Public Health Berkeley was in

D: '61-'62. I had just completed my internship and came to Berkeley, as I said, on the advice of Jacob Brody who told me getting a master's in public health was a very valuable thing to do. I wasn't sure why, but, again, thank God for good advice from friends.

That year was so intense. The things I remember as teaching methods, one of them was the requirement that during the first semester of this sort of epidemiology seminar group we had to pick

a topic of epidemiologic importance and write a scholarly paper on it. And the second semester would be the group of us each critiquing the other's papers. I guess it was during that process that he taught us how to be critical sons of bitches, which he himself was. This was another side of Stoney that he'd be a dear and wonderful friend, but a critical son of a bitch at the same time who could just reach into the core of what you just said showing you how stupid it was. I never met anybody like him.

Clearly, things are said about Stoney – such as he's an epidemiologist's epidemiologist. I think that's the whole aspect of his greatness was that he was a person who really thought about what we do, questioned what we do, and made what we do more fun, too.

DL: You remind me of a student I've not thought of for quite a long time. George Lathrup was his name.

D: George was in this group.

DL: It was George who had the task of reviewing my paper on the epidemiology of something or other, which by the time I was there, in '66-'67 for an MPH year, was under much the same circumstances as yours. First year of a preventive medicine residency and academic MPH program with a subsequent assignment to a PHS program of some kind. I was interested in an MPH in administration because I had thoughts about working for the World Health Organization. I had received a form letter in reply to an inquiry I had made years earlier while in medical school that said, "Candidates for positions with WHO must have an MD and a Master's of Public Health degree." Not knowing what that was or why I should have one, I assumed that administration was the area that was needed. And when I applied to the school in Berkeley as part of this deferment program I requested placement in the administration program.

But the letter I got back from Dean Smith was copied to someone named Stallones or whatever that I couldn't pronounce, and was about epidemiology. And I replied, "No, I'm interested in the program in administration." But then three or four months later I got a letter from this person Stallones about the epidemiology program. And about a month before I was to go to Berkeley I sent another letter to Stallones or whoever he was and to the Dean indicating my interest in the program in administration. And got a letter back again from Stallones.

And I'll never forget, he apologized for sending his correspondence in little bunches like grapes. But here was a series of two or three letters about why it was that I needed to be in the epidemiology program, and so, therefore, I was.

When I arrived it was at the end of internship, first day of a PHS traineeship. I just had a 300% increase in salary and dutifully reported at 0800 hours on the first of July only to find that Dr.

Stallones was nowhere around. His secretary said, “Oh, you’re Darwin. Stoney said that if you came in this month while he was away, there were things you could read in his office if you were interested.” And she gave me access to his office and boxes of reprints about stroke epidemiology about which, of course, I knew nothing. So I went immediately to the corner stationery store down on University, bought a box of 3 x 5 cards and sat in the office eight hours a day writing notes on these papers until he showed up.

He came in about a week later had said, “Oh, hi. You’re Darwin.” I said, “Yes, sir.” He said, “What have you been doing?” I said, “Well, I’ve read these papers about stroke epidemiology.” He said, “What? You read them?” I said, “Sure.” And he said, “You must know more about stroke epidemiology than I do.” To which I said nothing, but assumed that he was

probably right. Which was how naïve I was at the time. I'll never forget that.

And a week later we got in his car and drove down to Seal Beach to haul patient records out of the Rosmore Leisure World Seal Beach Community Clinic to see if there was any evidence of residents on their admission exams having had any sign of a stroke. John Cutler was working on stroke there at the same time. And this was my introduction to epidemiology, driving down the coast with Stoney and spending a week in the attic at the Rosmore Leisure World Clinic and one evening going over to one of the beaches to have dinner with Nemat and Patty Borhani who were on vacation there. Nemat was in the health department in Berkeley on holiday at Huntington Beach or some place like that. And I guess I had the sense then that this was going to be a different year than what I had experienced in medical school and internship and I began to see why it wasn't administration.

D: That's wonderful, how clearly one way or another he affects and changes people's lives. It's all very subtle at one level and I'm sure he never sat down and planned to do any of this, but he uses a magnetic aspect of showing you what's really a delightful path.

DL: I don't remember anything very didactic that year either. I remember we used those needles with the Hollerith cards.....

D: Still using those.

DL: The marginal punch cards we used to work with data from a respiratory outbreak among recruits and so on. And we did a bunch of those kinds of things in the first semester and I had the same experience you did. I didn't know really, in my case, what I was supposed to be doing to analyze the data. It was just that general instruction, "Here are the data. Analyze them and provide a report." And I provided a report and I remember Stoney saying, "I don't understand a thing of what you've written." And then it was in the second semester by '66-'67 that we did write a paper

about an epidemiology exercise. And it was me in the library trying to figure out what was and what wasn't epidemiology to be included in this review, which I chose on rhinovirus infection.

D: Freedom to choose.

DL: No restriction on topic and I was already, for whatever reason, interested in maybe cardiovascular disease or perhaps responding to that first inoculation of stroke epidemiology and thinking this was an area of interest. So I decided to do something as different from that as I could think of. I'm trying to figure out what papers were and which papers weren't epidemiologic about rhinovirus infections. That was the thing that stands out most in my mind in that year of learning something about what epidemiology was.

D: So the process of him opening a door and you have to figure it out yourself. I'll always remember my paper..... I think I read Michael Crichton's *Andromeda Strain* sometime the year before, so I came and I said, "I'd like to write a paper about what would

happen if a new virus appeared from outer space on a meteorite or something like that. What would one expect if some new infectious agent arrived here?” And he sort of looked at me like I was crazy and he said, “Well, that’s interesting. There is an aspect of epidemiology that is very interesting close to that which is called ‘virgin soil epidemics’ in which a population is exposed for the first time and there’s a lot of literature on it.”

So he gently guided me from my science fiction concept into something, again, that served me very well as I went into the Public Health Service – a couple years in Alaska....where I actually participated in a couple of so-called virgin soil epidemics. That process of virgin soil epidemics, again, I think that’s where I began to sort of think in those kinds of terms that he was talking about of what happens, who gets it, who doesn’t. Some people get it. Some people don’t get it. How do they differ? What’s going on in those particular situations? Again, very few You know,

our lectures..... I don't remember if it was the same thing when you were there, but the whole first year of Epidemiology 101 was visiting firemen.

DL: Yes.

D: Most of it people coming in describing what project they were working on right then, but without anyone saying, "This is what epidemiology is." People come in and talk about it. And after a while you begin to see the patterns of what they are doing and this concept of how what that is and how it differs from working in a hospital as a physician.

DL: When you met Stoney he must have been fairly new at the school. I'm trying to recall when that program really got underway.

D: Nobody realized at the time. This was, again, one of the largest schools of public health at the time and there were two epidemiologists there, Stoney and Bill Reeves. That was the entire

department. Bill carrying the infectious disease and Stoney the rest.

I'm trying to remember.... Stoney was the son of an evangelist-type preacher, which is an interesting concept in itself because I know at least three other epidemiologists who have that same kind of fathers. He grew up, I think, around Little Rock, Arkansas or somewhere like that.

DL: I know it was Arkansas.

D: I think that was very important because when he applied for the deanship to start the new School of Public Health in Houston some of those people were wary of this Californian coming out until they learned he, indeed, had grown up in Little Rock, Arkansas. That made him all right. I think that might have cinched him getting the job.

I know he had finished medical training and had gone into the Army just about the time that the Korean War started. He

served in Korea as, I think, just a front-line doc. He told me, so I don't know if this is true or not, that his name, which we all learned to pronounce, Stoney – a derivative of Stallones or whatever it was – he was riding in a truck with wounded soldiers going back to the hospital and he was sitting there and one of the soldiers looked up at him and said (you know, I guess bullets and shells were going off overhead), “How can you sit there with such a stone face?” And the other guys started picking up, “Yeah, it's Stone-Face. Here comes Doc. Stone-Face.” Which was then, I guess, shortened to Stoney throughout the rest of his period there.

DL: When he got back from Korea he did residency training. I'm thinking he had training in pediatrics along the way. Does that ring a bell with you?

D: That doesn't ring a bell. Something about his residency training he was exposed to some infectious disease event. I don't remember which. I'm trying to think what it was now. Something that led

him into the literature on this infectious disease. It might have been even rhinovirus. Again, this was his introduction to epidemics in general.

DL: I remember his talking about working on heat injury at a base in the south and I'm not sure where. But I wonder if that Fort Orr respiratory epidemic might have been part of that.

D: Yeah, I don't know how we could find that out, what went on in that period, which as we say was kind of amazing that within that short time he went from a practicing military doc to discovering epidemiology without any formal training. I don't think he had an MPH, for example. But sort of like John Snow he didn't need one and then went ahead and began to pioneer the whole formal concept.

DL: While you were preparing for your dissertation research, I was on active duty in the later Heart Disease and Stroke Control Program. I was supposed to be assigned to the field station in San Francisco

where nominally my position was and I was actually supposed to go to Seal Beach and be in residence there working on studies of cardiovascular disease at Seal Beach.

D: After your MPH year?

DL: After the MPH year. So I went on active duty June of '67 and the project at Seal Beach had been assembled as a joint activity of the school and the state health department and that was Who was at the state health department? It wasn't Lester Breslow. There was someone else in the state health department who was that piece of it and Paul Ehrlich who was at that time the Chief at the Field Station for the Heart Program in San Francisco at the PHS Hospital.

D: Len Syme at that time was just a sociologist.

DL: Len Syme was still at the Field Station then. And there was a problem in the collaboration. Don Loveland was a statistician at the Field Station in San Francisco and somehow a problem

developed in which the collaboration fell apart. In part because I was in effect Stoney's trainee there was suspicion that if I went to Seal Beach I would be Stoney's man on site and not doing my duty to the Field Station in the mind of the statistician.

So it all fell apart. As a result of which I was reassigned back over to Berkeley which was how I got to be available and looking for a project assignment when the Guam Study got under way. And my recollection at that turning point in my career was that there was an office off campus on Shattuck Avenue above the Tupper and Reed Music Store and you must have had office space there.

D: No, actually.....

DL: Well I did and Bea Treman was supervising a small army of telephone book readers who were going through all the Bay Area phone books looking for people with Japanese or Japanese-like names.

D: The 'san' part of the Nihonsan study.

DL: Yeah.

D: Len Syme was not there yet. That was Stoney's project.

DL: Len still wasn't there. Paul Ehrlich left to go to the Office of International Health with PHS. Milt Nichaman who had been Chief of the Laboratory became Chief of the Field Station. But Len had been the Chief in between for about six months before you came over to join the faculty at Berkeley. So that must have been

D: He didn't join until actually Stoney left by which time most of the work was done.

DL: Ok.

D: This is a delightful aspect where we first got together. I had done what little more coursework was needed for my PhD thesis and Stoney said, "Have you thought about it?" I said, "Yeah, I was

thinking about the epidemiology of schizophrenia.” He said,
“Yeah, that’s certainly full of problems.”

And he told me all the people who had sort of dropped by the wayside trying to take on that one. And he said, “Why don’t you think about it some more and come back?” And, honest to God, that night I got a call from Jacob Brody

(A quick aside. Between my MPH and coming back for the PhD I had spent a year plus in Guam working on the ALS problem there and in the meanwhile, Jacob had taken over that project. He called and said, “What we want to do is to trace down Guamanians living in California and to see if they have ALS, see how long they’ve been there, and does the high risk for this disease carry when people migrate to California. And I’ve got a pot of money and we could fund this study for you.”

I said, “Let me think about this a bit.” So I went in the next morning and talked to Stoney and said, “Look, Jake called and has

this money and there's money for us to do a survey for ALS in Guamanians.”

And Stoney's eyes lit up and he says, “Oh, and we could do all kinds of cardiovascular risk factors measurement at the same time, couldn't we?”

After kicking that around for a while I went back and said, “Jake, we'll do the California survey if we can also measure standard cardiovascular risk factors and we can go out to Guam and get the same kinds of risk factors on the Guamanians so we can do this comparison.” And that was the days of “while you're up, get me a grant” kind of thing. So Jake immediately agreed because he wanted to do this migrant study for ALS.

So after Jacob agreed I sat down with Stoney who said, “Well, you can't do this by yourself. This is just too much work. You're going to need somebody to help you do that.” I said, “Yeah, I don't know what to do. I guess we can try and hire

somebody, we do have that much money.” And he said, “I’ve got an idea.”

So, I guess at that point he must have contacted Milt or I don’t know what negotiations took place, but the next thing we all got together and we were introduced in terms that we were going to do this project together.

DL: I remember sitting in Stoney’s office and talking about it and I remember vividly that you made clear at the start that there was one requirement.....

D: That’s true, I was very clear.

DL: And that was that I become certified in order to be a diver.

D: You probably didn’t understand why.

DL: And I remember that I took the YMCA scuba course in the outdoor pool at the Oakland Y in November.

D: Freezing cold.

DL: It was cold. It was in a quarter inch neoprene rubber suit, 60-pound weight. My check out dive was in Monterey Bay and Fort Ross. So I was ready for warm water diving.

D: You really did heroic training.

DL: And the first dive in Guam, of course, was a night dive.

D: I thought you had done everything but checkout. Maybe it was everything but your 10 whatever practical dives or something. You needed to dive in Guam.

DL: There was one last step because I had had two checkout dives and I had something that said that I completed the course. And, of course, the other duty I performed and I recall that this also took place in that office sort of trying to see though Bea Treman's smoke accumulation during the day, to draw the layout of a 10 by 55 foot trailer so we could do the exams in California while you and Milt and Stoney went to Guam for a feasibility trip. So when you got back there was a layout of a trailer. There was a beginning

mock-up of a physical exam that you would do to pick up ALS and do whatever else we had to do. Oh, gracious.... Things you forget.

There was a project assistant in the Shattuck Office, Kathy Karusaitus, who was going out to do the code sheets and so on so that we would have pre-coded forms all laid out. And there were other staff whose names don't come to my mind.

D: John Houghton was a statistician with the Nihonsan who was later a great help to us. I think the only statistical aide as far as I remember in programming.

DL: This was when data analysis was a hard-hat job and you had to wear ear protectors to be in the same room as the printer-sorter. Checka, checka, checka, checka at very loud volume.

D: Not as fast as what we have now.

DL: No.

D: Well, that was a delightful time. Again, an example of Stoney's approach. It's amazing to me now, but here we were..... I had

some epidemiologic experience working in the Public Health Service in Alaska. But I knew nothing about cardiovascular disease. Except that here was a way of parlaying an opportunity into a bigger thing. It's really interesting because all the papers that we published from that and also the numerous follow-up studies that came out of that kind of survey were all basic cardiovascular disease epi.

But Stoney basically..... You know, here it is, here is an idea.... When you think about it a monumental kind of undertaking that if you would try and do that now, not only would it cost ten times more, but it would probably take three or four years of planning and getting all the forms cleared through various organizations. He turned us loose.

DL: He didn't intervene or advise that I recall.

D: No. We'll talk about the final advice coming up in a while. But, no. I'm not sure what forms we used to make our forms and deciding how detailed we were going to be.....

DL: Well, you collected a bunch of stuff. You had spent some time in the literature of what became a lost era of cultural comparisons and sort of social-oriented population studies, and questions about how you investigate culture differences. Whether it's diets, or attitudes and so on and you developed some of those scores from stuff that was in the literature or needed to be created.

D: Again, it's timing that's important. During the planning phase or early in it, Stoney invited John Cassel as a visiting speaker who then got me together with John Cassell for an afternoon afterwards. And I guess that was my first exposure to the psycho-social aspects of disease. John was, again, another kind of pioneer person who had done these wonderful studies in North Carolina and so that was the stimulus to include all of these big long questionnaires.....

And then I can't remember how we got Len Syme's laundry list of all of the psycho-social measures. I don't remember actually talking to Len that much at that time, but somehow.....

DL: He was at the Field Station then in San Francisco.

D: Yes.

DL: And there were papers from Larry Hinkle in New York who had done the telephone worker studies.

D: Yes. Cassel's study, Len's study.....

DL: Cassel. Len had done studies on cultural discontinuity or social discontinuity.

D: Yes.

DL: Remember? Cultural incongruity.

D: Yes. He invented all those terms and had done a couple of case-control studies.

DL: Leo Reeder.

D: Yes.

DL: There were other people working in this arena, which for whatever reason seemed to disappear until someone thought up the term ‘population health’ and began recreating what was a very lively intellectual and scientific area in the mid-Sixties. I don’t know what happened to it.

D: Well, Len again, I looked up to him as a pioneer in this field and I thought it was interesting stuff. Again, always looking for something different other than the standard risk factors for what’s going on. And that was great opportunities when you find them. I don’t know where the idea came from, but he proposed that we not only can look at individual measures, individual differences but at the ecological things. That whole concept. This is the true learning – that just blew me over in terms of understanding these kinds of complexities of epidemiology. Again, this was also very early in the psycho-social epidemiology stage and I don’t think

there were any other prospective studies going on and so, again, we fit it into the cross-sectional.

But even our cross-sectional were the first negative studies. They would have said, “You can explain the major differences that you found based on the standard risk factors for cardiovascular disease.” And I think Len never quite forgave me for that. We certainly went on to the Nihonsan Studies in Hawaii that have continued to find negative results related to these psychosocial things.

This was great. As a project we had to do everything. I remember ordering the stuff, shipping hundreds of bottles of glucola (*unsure of word*), all the equipment, centrifuges, the ECG machine, all this stuff. Taking it with us first all around the state in the trailer. I mean the logistics now of outfitting a trailer and moving it – San Jose, Oakland, Long Beach, San Diego. An

incredible project that we undertook. And then shipping all that stuff to Guam, hiring people along the way.

DL: And you'll remember our pressing into service as a public relations figure the very bright, perky, energetic study participant from Guam who we examined in Oakland, as I recall.

D: I thought it was San Diego.

DL: Maybe it was San Diego.

D: She was the one who was going to get married.

DL: In Long Beach and who was frequently going back to Guam and we enlisted her to provide a television appearance in Guam and sort of be the person who would recruit people to participate. Then we found out after the fact that she had divorced a very popular legislator in Guam, which was an absolute no-no in the Catholic community in which they lived. One of the lessons of

D: Again, this study changed all of our lives in many ways. The beginning of many of our studies

DL: Yes. While this was going on Stoney had gotten the funding for the 'san' part of Nihonsan. He had had much to do with the whole Nihonsan concept.

D: Yeah, the origins, the idea of such comparisons.

DL: His connections with Japan may have begun during his Korea time when he had periods of being either based in Japan or going back and forth. But before that he had been in Japan because of stroke. He had gone to Hisayama where the Hisayama Study was going on and he had been involved with Jim Tool and others in this questions about whether the strokes in Japan were all hemorrhagic because they have so much hypertension and trying to improve the comparability of data on cost-specific mortality from stroke.

D: I didn't know that part.

DL: Then he did a review of stroke epidemiology in the *Journal of Chronic Diseases* maybe '65ish.

D: Yes, I remember that article.

DL: As far as I know it was one of the first if not the first epidemiologic effort to put together what was known....

D: About stroke epidemiology.

DL: And I suppose that was part, at least, of how Nihonsan got underway.

D: Kagan has written a book on the Honolulu Heart Program. What I can do is to look at some of the opening chapters. I think he talks about.....

DL: I've never been able to get a copy of that book.

D: When we're through, we'll go and see whether I've got one copy. I loaned it to somebody; I hope I still have it. You can borrow it.

DL: And Abe died now more than a year ago?

D: Yeah. The intertwining of all of these influences, those early origins of Nihonsan, migrants in Hawaii later on, getting involved with that, Len Syme coming over when Stoney left. It's incredible to sit down and try to figure all these things out.

I know Abe talked about meetings taking place in Zukel's office. Kagen was at Framingham. I think that Yano was somehow involved. Yano had come to visit Framingham and was totally amazed at seeing so much heart disease. There was some kind of influence from him and Stoney there. He talked about Stoney and I don't know where the idea, "Let's do a comparison of migrants" crossed. I know Stoney was very active in the origins of NIHONSAN and, of course we know he was very hurt when he went to Texas he was basically cut out of the 'san' part of that. And he carried that resentment with him, I think. So, again, some of his influence on some of these early studies, which have gone on for another 30 years after his initial involvement.

I think one of the interesting stories is we had completed the Guam and Rota phase and still decided to go out to Palau as part of that.....I don't know if Palau was part of the original, because it certainly didn't fit into any hypothesis we had.

DL: The plan was to go to Guam, Rota, and Tinian (*unsure of last 2 names*).

D: Tinian or Saipan, yeah, Tinian.

DL: And there had been a typhoon.....

D: In Tinian, yeah.

DL: And the typhoon blew everything away and the village in Tinian was being supplied by military airdrops with tinned corned beef. And we thought that would not be auspicious for the seven-day dietary recall.

D: I remember walking around that airfield and seeing the plaque where “The Enola Gay had taken off on such and such date and dropped a bomb on Hiroshima.”

DL: The Peace Corps was playing softball and they were of no help. So as I recall a telegram was sent to Jacob to say something like, “Tinian inaccessible. Going to Palau instead. Please advise if any problem.” And then we got on a plane with the supplies.

D: Yes. Again, a good example of things that could not possibly be done nowadays without six months of NIH deliberations.

DL: And went to Palau and had the foresight to say that the first place we should work in Palau was the most traditional area because things change so fast and there was no idea when we might be able to get back. So let's get the most traditional place while it's still traditional and off we went to Naralong (*unsure of name*). It was a small field team, but we had

D: Small, but efficient, yes. We had a cook.

DL: We had able recruits. We stayed at a teacher's house.

D: David.

DL And Nancy ____.

D: Nancy, you and I.

DL: We were the only statesiders. The three of us.

D: Yes, everybody else was Guamanian.

DL: And we recruited the two locals to do the exams.

D: The hospital public health department, loaned us the boat to get all the supplies up there and introduced us to the Chief when we got there.

DL: Yes.

D: A memorable day. It was two in the afternoon and we were just finishing up exams and we hear a motorboat coming up – a big Boston whaler – and could see from afar Stoney sitting in the back. As they pulled up to the dock, the boat immediately began sinking because they had forgotten to put the main plug back in and they had to go around and drain the boat. Stoney with his one suitcase. How he found us out there is, again, a good story. One side of his suitcase contained scotch, gin, and whiskey and the other side was Doritos, potato chips, and dip. We had our first cocktail party in months in Naralong.

DL: And we learned the next morning, as I recall, that it had been a recruitment trip.

D: That's right. Stoney had just been appointed Dean of the newly to-be-built School of Public Health in Houston and he was on a recruiting journey. Probably used his recruiting funds to come out and to offer us both jobs.

DL: Which we accepted (we were told) the next day.

D: Jean Henken was there.

DL: Jean Henken was from the University of Hawaii's School of Public Health and she was doing.....

END OF SIDE A

D: By this time, of course, the line between student and friends and colleagues had blurred. That's one of the nice things. That happened very quickly as I remember. I remember in my MPH year a bond had already formed with Stoney. This is a year later I went to Guam. There was a Japan trip, too. Stoney came through. Colin was probably a six-month-old baby in Guam and he stayed with us a couple nights and it's interesting how clear things stand

out from Stoney being in your house. I think we fixed scrambled eggs and they weren't bone dry and he had to take his back in and turn the fire back on and dry them out. We corresponded. I would send tapes from Guam about what we were working on. Actually Stoney even came out. He knew nothing about ALS, but he was chosen for one of the site visits to come out and see what we were doing. And, again, his presence just crystallized things. We had different studies going and he was able to look at the raw data, graphs we had drawn up, and immediately just come up with, "Ok, have you thought about this, have you done this." Key questions immediately supporting his ability. He's clearly the smartest epidemiologist I've ever met.

DL: I was thinking about the most substantial epidemiologic studies that he was directly involved in. He certainly influenced many. He was directly involved in several. Among the things that stand out certainly Nihonsan was one. The work in the Seal Beach

community was another. There was something called the King City Study which he had done early after arriving at Berkeley which was in King City, California. It involved a population sample, family studies of coronary risk factors.

D: It started off with high school students and then recruited their parents or something like that.

DL: I don't know the details and I don't know what the publications were from that study.

D: I don't know if they ever published.

DL: There was a younger physician who was part of that team who was one of the people we had as a lecturer in Epidemiology 101 and his name escapes me at the moment, but it's someone that I know Stoney had intersections with off and on in his career.

D: Something was published because I remember the correlation between blood pressures, rank order of blood pressures in the

students with the rank order blood pressure of their parents.

Something like that.

DL: And then there was a guy at Berkeley he did some work with....early in the days of analysis of large datasets. This was Buechley and the one that stands out in my recollection is the correlation of coronary mortality by state with various state characteristics.

D: One of which was number of lynchings.

DL: Lynchings was one. The greater the number of lynchings reported in whatever time period, the greater the rate of coronary mortality. And there was also a direct association between numbers of psychiatrists per state and the coronary mortality. And a number of other like observations.

D: I remember that it had to do with physicians per capita directly related to the number of coronary deaths, cardiovascular deaths, and I think of hospital deaths, too. All these kinds of things. Yes.

That was fun stuff. I don't know if it ever really changed the world, but just wonderful fun ideas that they would come up with. Was it the hardness of the water? Maybe that didn't come out of that one. I'm not sure.

DL: There was this stroke thread that had its Japanese connections.

There was a stroke neurologist at Minnesota who wrote a lot about the Japanese stroke mortality experience. And I think Stoney went with him. It must have been part of his study section.

D: I know his name.

DL: The study section role when NIH was doing more, I suppose, to fund research in Japan and elsewhere than is perhaps true now.

D: Foreign studies, yes. In fact he did pathology studies of Japanese and I think was one of the first reports that indeed the pathological nature of strokes was the same. There was three quarters atherosclerotic and one-quarter or so, 20%, whatever hemorrhagic. So it was not changing in that aspect. A.B. Baker.

DL: A.B. Baker. Correct. Good. So there was the stroke interest. And in fact one of the first meetings, perhaps the first scientific meeting I ever went to was a meeting of stroke people at the Del Coronado and it may have been during my MPH year or maybe immediately after, but I remember that's where I met Bill Fields who was the Houston stroke neurologist. It was the first time I met Dick Remington who was a part of that group also. And this just has that same aura you referred to earlier of sort of dissolved distinction between teacher and student where it was just natural to go to a meeting together. He would introduce us to the people that he knew in the field and so on. And I think he continued to consult and in some degree collaborate with the Japanese stroke investigators for a long while. And he had a deep and very, what would you say, very constant interest in what was going on in Japan, which had something to do.....

D: At the same time this is when the 'hon' parts of the Nihonsan was taking off, exams were started. It was a couple years later before the 'Ni' part. So I don't know if he was helpful in working with the Japanese.....

DL: Whether he had something to do with Bob Worth's involvement in the 'hon' part, I don't know. Bob had been Stoney's PhD student like us.

D: Yes, but he dropped out very early in the 'hon' part.

DL: He was still on board, or was credited at least, when the main Nihonsan papers were written years later.

D: That's right. He was at the first cross-section of the whole series.

DL: One or more of those papers and was the co-author on all of them.

D: That was like '74 or something in that series.

DL: And I think from what I understand, Bob is not well enough to be interviewed at this point.

D: I hadn't heard that.

DL: When the Texas opportunity came up I don't know what all the details were at Berkeley at that time, but Chuck Smith had died suddenly. There was a vacancy in the Berkeley deanship. My impression is that Stoney had anticipated that he would become the dean.

D: And he wanted it.

DL: Wanted to be the dean and Bill Reeves was selected instead. Maybe by that time it was already apparent that the 'san' part of Nihonsan was not going to be renewed.

D: I don't think so.

DL: Or that came about shortly after because I had always thought that was part of the equation.

D: It was quite a bit later. When I left Houston two years after going there I went to NINDB, Len Syme wrote me saying that they had been badly treated by the review committee for continuing the 'san' part. So this must have been '71 and could I do anything

about it. They had made the mistake I think of going to the Neurology Institute for the grant rather than Heart. This is the renewal we're talking about now.

DL: Well, the original application I think as I recall had been for the baseline survey and there was never funding for a longitudinal component. There was the possibility of doing some mortality follow-up maybe of the survey participants. I think it never had the same kind of financial security that Honolulu did.

D: Honolulu was supported by contract from the Heart Institute, whereas the 'san' part – I don't know when it went to Neurology. I don't know Stoney's participation in the first phase of it. But whatever, it was either a renewal or going to a different institute that Len had contacted me and this is after the first, I think, the baseline exam was done.

DL: I remember Bill Zukel being mentioned in all this and I'm wondering if it was perhaps a renewal application had gone to

what was then NHLI and was not favorably reviewed and maybe Len was pursuing the possibility of NINDB.

D: Maybe so. I don't know. That was always sad. Not only that, but all of the baseline data were lost because at one point I wanted to do a comparison of ... I can't remember what it was. It was a follow-up based on some of their stuff and contacted Len and it was stored in some basement or gone or lost or flooded or who knows what. Lost. So that was a sad aspect of that beyond the baseline exam. And only two years of mortality follow-up.

My feeling is that part of Stoney's decision to take the deanship in Houston is based on the fact that he didn't get the deanship in Berkeley.

DL: Yeah, that must have been so. And then his maybe liberation from the Berkeley tradition he had the opportunity to think about how a school of public health should really be organized and that led to

the sort of radical idea of a school without walls and a school without departments.

D: Yes, again, a totally new pioneering concept of education in public health and developing a matrix approach to course work or to organization.

DL: He was a kid. He was in his mid-forties roughly. I think he was born in '26 and he went to the school in '68 so he would have been about 42 and had the opportunity as Dean of the School of Public Health to report directly to the Board of Regents of the University of Texas system and educate them about public health and epidemiology (which became obstructed by multiple layers of bureaucracy over the later years)..

D: If you read some of these, again part of searching out the things, some of those papers on what is a school of public health or how to start a school of public health, things along that line, these are totally new ideas, innovative concepts. Where did he get them? He

wasn't trained in this in any way either in education or this kind of administration, but he just fell into it naturally and, again, with his creative capacity just began to put together these wonderful ideas.

DL: His approach to faculty recruitment was to recruit good people and let them go. There was very little direction.

D: Dick Remington being at that meeting at Coronado Island and then showing up as the head of statistics. I guess you and I should feel very proud that we were considered in that group to be enlisted.

DL: That's true. And maybe the earliest years of the school and for quite a while what became of the school was most satisfying to Stoney.

D: Every once in a while you get an opportunity. Somebody says, "It's all yours. Do with it what you will." And here was his chance. Every detail – designing a building that's going to do this, designing educational concepts. How are you going to teach public health? What is public health, community health?

Thinking those things through it's just amazing to me this guy's creativity. I have done a lot of looking at epidemiology theory and I don't think he copied these ideas from somebody else. Nobody had talked about these kinds of things that I've seen anyway.

DL: I don't know of any textbook that has ever presented epidemiology as Stoney did. The closest example may be the McMahon and Pugh text of epidemiology, which I recall being presented at the time I was a student.

D: We didn't have a textbook.

DL: But he went beyond that to find out why the web of causation idea was really insufficient to incorporate what we know of various links between things.

END OF CONVERSATION

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TRANSCRIBED BY: Jan Pearson
612.626.8644 (w)
612.729.0093 (h)

pearson@epi.umn.edu

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