1. Early Life

ALLEN BUTLER: Hi. I'm Allen Butler, president and CEO of Daniel H. Wagner Associates, and I'm delighted to have this opportunity to interview my esteemed colleague and good friend Larry Stone.

LAWRENCE D. STONE: Well, I'm pleased to be here and pleased to be interviewed.

ALLEN BUTLER: Well, let's start at the beginning. Why don't you tell me a little bit about where you were born, and maybe a little bit about your family background?

LAWRENCE D. STONE: OK. I was born in St. Louis, Missouri, in 1942. My mother and father both had advanced degrees in social work. My father worked for the American Foundation for the Blind for many years, and was executive director of the Episcopal Youth Counseling Services in New Jersey.

My mother got her PhD in social work in her 50s, and finally got a job as a full professor at the Kean State University in New Jersey. In those days, they didn't hand out professorships to women very easily. She was running a big clinic for retarded children on the campus. And, basically, she said, if you don't promote me to full professor, I'm taking my clinic and leaving. That's how she got promoted.

I have one sibling, a sister. She is very socially oriented too. She worked for VISTA, the Volunteers in Service to America, a volunteer organization in the 60s. And she was a social worker too.

ALLEN BUTLER: So you were sort of the odd man out as a mathematician.

LAWRENCE D. STONE: That's correct.

ALLEN BUTLER: So tell me where you went to high school and what kind of subjects you were interested in or good at.

LAWRENCE D. STONE: I went to high school at Columbia High School in Maplewood-South Orange. That was a very good high school, and also a very nice town in the New Jersey suburbs of New York City.

I liked history and chemistry and did some mathematics, but I was not particularly-- I didn't like mathematics. I found it kind of tedious. So that was really not my strong subject in high school.

ALLEN BUTLER: Was there anything in high school that sort of was indicative of your future career?

LAWRENCE D. STONE: As a mathematician? No. I actually had no introduction to OR or anything of that sort in high school. Of course, this is the early years OR. I graduated from high school in 1960.

ALLEN BUTLER: So what kind of things did you do, if you weren't that interested in math and so forth?

LAWRENCE D. STONE: I was interested in lots of things. But one thing I did was to work on hot rods in those days. I had a 55 Chevrolet, and I spent a lot of time rebuilding the engine, souping it up so that I got a lot more horsepower than it came out of the factory with. I always enjoyed doing practical things, as well as being scientific.

ALLEN BUTLER: And then, after high school, how'd you find a college and where did you go?

LAWRENCE D. STONE: Well, a friend of mine and I took a trip to visit colleges in Pennsylvania and Ohio in 1959 in our senior year. We happened to visit Antioch College, which is in southwestern Ohio, not far from Dayton. And we both loved it.

It was quite different from most colleges in those days. For one thing it was very progressive. But one thing that made it a lot different was it treated men and women equally. Now, this is 1959. So in those days most colleges-- I think almost all colleges and universities-- would lock women up at 11 o'clock in their dorm. With Antioch that didn't happen.

Or another example is that when you went to gym class, all gym classes were co-ed. It was no men's gym and women's gym. They were all co-ed. Another thing that really appealed to me about Antioch was they had a work study program. In Antioch it was operated on a quarter system. You were on campus one quarter and then off campus the next quarter on a job.

There was a mirror image group that was on campus while you were off campus working. And that really-- well, the purpose of that is not vocational education, it was to educate you in the world, get you out there, and find out what it was like out there, and get you used to being on your own so you'd go out to some strange city-- maybe you'd rent a room in a YMCA for a couple of days until you can find an apartment. Then you would rent an apartment and you'd go to your job. So you got to be pretty adventuresome. And that was great.

Also, in those days Antioch College had a very wide range of political activity going on there. And all the way from the Young Americans for Freedom on the right-- I think we had the chairman, the kid that founded the Young Americans for Freedom at Antioch College-- all the way to the left where we had the socialists and communists. It was really quite an active political discussion going on there.

Many students participated in the civil rights activities of the 60s, some of them going south to register blacks to vote in Mississippi. In fact, one student, Andrew Goodman, got killed in 1964 doing just that down in Mississippi.

ALLEN BUTLER: Wow. Well, so at Antioch, did you start out studying mathematics, or?

LAWRENCE D. STONE: Actually, I was most interested in being a chem major. That's how I started out. But fortunately, we took a series of tests before the school started. I managed to test out of Calculus 1. But the math department said, well, that's fine, but you've got to audit the course anyway, which turned out to be quite a good thing, because the professor Julian Blough exposed me to a different kind of mathematics than I'd seen in high school.

He discussed the philosophy of mathematics and the rigor of it, and the beauty of it, and I got hooked. I thought, OK, this sounds like fun. I decided to try some other math courses. And the way the math courses worked at Antioch-- the way the tests worked-- is that you had these open book exams typically.

The professor would hand the exam out to you in class on one day, and you had to turn it in at the beginning of class the next day. You could consult any books that you wanted to. You couldn't talk to other students about it. So naturally the problems you got to solve were never the homework problems you did. And they weren't theorems proved in the book.

What you had to do was take some technique that you learned and apply it to a new problem. I said, you know, I'm pretty good at this. So I decided to become a math major. Another inducement there was that I really didn't like going to the chem labs, which were at 8 o'clock in the morning on Saturday.

The way I learned to become a mathematician, though, was during a National Science Foundation program in summer 1963. It was for math students from small colleges in that middle Atlantic area, no, the Midwestern area. I forget exactly which colleges were there, but there was Kenyon, Oberlin, Antioch, Knox, Carleton, and finally Sewanee, The University of the South at Sewanee, Tennessee.

That's where the program was held, the summer program was held. They got two or three smart math majors from all those places plus me. So, I'm there the first day of the class, first day of the program, and the professor is handing out the books. OK, fine, textbooks. So then he goes down the sections in the book and he says, you're going to teach section one. You're going to teach section two. He assigned each student a section to teach.

So we have to teach the course for crying out loud. And then he handed out this whole list of problems to solve. The problem statements didn't make any sense to me at all. What happened is each day students-- well, before the day, before the lecture began that day, the professors would ask the students if they had solved any of the problems. And if you'd solved one, you had to stand up in front of the class and explain, write out on the blackboard exactly how you solved it and convince everybody that you really did it right.

And these guys were good. I was amazed at how fast they could solve these problems, and before I could even understand them. So finally, I kind of got the hang of it and was able to solve one before anybody else did and present it. And then I was sort of one of the boys, and I was accepted. Before then, I think they had their doubts about how much brain power I had.

Now, another fortunate thing about that summer-- it was a very intense mathematical summer-- was I was rooming with a very smart guy from Antioch. His name was Julian Gevurtz. He basically explained to me how he went about solving problems, how he looked at it, what sort of analogies he made, going from simple to complex to the final solution of the problem. That really helped me to become a mathematician.

So that summer really sealed it for me. I decided mathematics was really, really fun.

2. Graduate School

ALLEN BUTLER: And then, from there you went on to graduate school?

LAWRENCE D. STONE: Right I got a BS in mathematics from Antioch, and then went to Purdue University for my graduate work, and got a rude shock. Purdue was, in a sense, the antithesis of Antioch College. Purdue is a big university, and had a good math department.

The first course that I took there, I was enjoying the subject quite a bit. And the first test that I took I absolutely flunked. Quite an introduction to graduate school. Well, my roommate explained it to me. I complained to him, and I said, I didn't have a chance to think on the test. It was a one hour, in-class test. I didn't have a chance to think on it.

He said, you're not supposed to think. The way you pass the test is you memorize the proof of every theorem, and you memorize the solution to every homework problem assigned. And when you get into that test you just sit down and write. You don't think. So, OK, I took his advice, managed to do well on the rest of tests, and got a B in the course, which is just barely passing in graduate school.

A similar thing happened to me the first time I took my qualifying exams. I flunked those. My roommate said, well, did you study the solutions to the problems on the past exams? Because they had put them out in the library. You can go inside, look at the problems and solutions for qualifying exams for the last five years or so.

So if you do that, that will allow you to solve enough of the problems that you'll pass next time. And I did that, and I passed. At this point, I went to the head of the Stat Department and asked him for a recommendation for a major professor. Well, let me back up a little bit before that. Why the Stat Department?

Because, over the summer, one summer, I took a reading course in Feller's book *An Introduction to Probability Theory and Applications*, Volume 1.

ALLEN BUTLER: Excellent book.

LAWRENCE D. STONE: I would read chapters and work out lots of problems and discuss them with the professor. I was totally enchanted. I just found that very interesting. The solutions to problems that I could recognize in real life. And a lot of the solutions were quite unintuitive, quite counterintuitive. So it appealed to me greatly.

I went to the Stat Department and said, I'd like you to recommend a major professor for me. The chair of the department recommended that I talk to Glen Baxter. I'm very naive. I had no idea that Glen Baxter was one of the best known probabilists in the country at that time. He had quite a reputation.

I waltzed into his office, and this is where naivete helps you out in some sense. Not knowing any better, I started a chat with him, and he was chatting with me. I said, well, you know, I would like to finish up my thesis in a year. I'd like to get out and do applied work.

And instead of throwing me out, which I think most major professors would have, he just looked at me and said, yeah, I think we can do that. So he gave me a copy of one of his papers, recently published. He said, I did this for discrete time problems. See if you can do it for continuous time.

And that's actually where my Antioch experience really came to the fore, because it was like doing an open book homework problem, right? I had the techniques. I had a different problem. And it turned out I had to come up with one new idea to extend what my professor did to continuous time. And I was out of there in a year with a PhD.

ALLEN BUTLER: So what was the title of your thesis?

LAWRENCE D. STONE: It was on *First Passage Problems for Semi-Markov Processes*. All right, pretty daunting title. So here I was looking for a job. I decided I didn't want to teach in academia. And teaching in those days was a pretty good job. I mean, not like the rat race that academia is now, much, much more fun. But still, I wanted to do applied work.

3. First Job at Wagner Associates

I applied to Sandia. I think Autonetics, North American Autonetics in California and Bell Labs. This company I never heard of, Daniel Wagner Associates, sent me a letter saying, come interview with us.

Well, it turns out that Dan Wagner used to read the titles of all the theses of graduating math majors in the Universities around the country. He looked at the title of my thesis, and said, , that's related to work in modeling underwater detection for sonar for the Navy. And Wagner Associates was doing that work.

So he wooed me. He was very persuasive when he wanted to be and insistent. And I decided I would go interview with Wagner Associates. So this is, again, a small company with, I think, four full time professionals at that point, including Dan. I interviewed, talked to the people there. One of the people that I talked to was Tony Richardson-- Henry R Richardson-- Tony was his nickname.

Tony had been on the search for the H-bomb in 1964. So just a little background here. In those days, part of our strategic deterrent was that we'd always fly these B-52 bombers around in the air, constantly, 24 hours a day. In case of a nuclear attack we'd always have those in the air ready to retaliate against the Soviet Union.

So there was a B-52 over the coast of Spain, and it was being refueled. The refueling plane and the B-52 collided, crashed. I think three or four H-bombs, not armed, fortunately, fell out of the plane. Three of them fell on land. That was the Air Force's responsibility to recover. And the fourth fell into the Mediterranean off the southern coast of Spain, near Palomares, Spain. So it was the Navy's job to find that.

Tony Richardson was sent over as a mathematician to advise. And he would tell me stories about advising the on-scene Admiral there, the Admiral in charge of the search, about how well the search was going, and where he ought to put more search, and where he had done a good enough job that he didn't need to put more search.

I was fascinated. It was amazing. Here was a guy using mathematics and affecting really important decisions, an important problem. I got offers from Bell Labs in Holmdel, New Jersey, and from Wagner Associates. And I think any sensible person would have taken the offer from Bell Labs.

ALLEN BUTLER: Very famous.

LAWRENCE D. STONE: Right. It wasn't Murray Hill Bell Labs. But Holmdel's a very good place. But I have to say that both the lure of doing this sort of search theory and operations research really pulled at me. And also Dan was very supportive of continuing my academic research too. So I figured I would take a chance on Wagner Associates. And in July of 1967 I joined them.

I studied underwater detection problems in my thesis area. And they gave me a copy of the Operations Evaluation Group report, called *Search and Screening*, written by Bernard Koopman, and said, read this and learn a bit about search theory, which I did.

4. Searching for the US Scorpion

And I worked-- this is July of 1967. In May of 1968 the US nuclear attack submarine Scorpion went missing. It had radioed in from near the Azores that it was on its way back to Norfolk. Now, one has to remember that, in those days, submarines did not communicate very much.

They didn't really expect to hear from the boat until it arrived in Norfolk. What happened was that the families of the crew were standing on the dock waiting for the submarine to come in. And it never came. So the Navy organized a week-long or a couple of weeks search for-- surface search-- for survivors and for debris, which was not successful. And then the underwater search began.

Somewhere at the end of May or beginning of June, Tony Richardson got a call from John Craven. John Craven had an interesting background. John Craven was in charge of the technical advisory group for the H-bomb search, and had worked with Tony on that search. And Craven had been the chief scientist for the Navy's strategic systems programs, which was in charge at that time of building the Polaris submarines, which were our first ballistic missile submarines. And he did a very good job of that.

And next he went on to be the Chief Scientist, the head of the Deep Submergence Study Project, DSSP, Deep Submergence Surveillance Project, I think is what it's called now. It had a cover story of building deep diving rescue submarines to rescue submarines that were lying on the ocean bottom, or building search vessels that could search down to 20,000 feet.

But that turned out-- later, I found out it was actually a cover for them to spy on the Russians with these fancy submarines that they had built for exactly that purpose. The Navy put Craven in charge of the technical analysis group for the Scorpion search too. And he called Tony Richardson down to his office in Silver Spring, Maryland, at the Deep Submergence Project and was joined by Frank Andrews, a retired Navy captain and submariner.

The two of them-- and Tony Richardson brought me along. I was his sidekick-- the two of them proceeded to outline seven scenarios for the location of the Scorpion. And for each of these scenarios they provided uncertainties. And they weighted the scenarios as to likelihood of each of the scenarios.

This is a Thursday afternoon down in Washington. And Craven says to Tony, "OK, Tony, go back to Paoli." Paoli, Pennsylvania, was where Wagner Associates was located. That's on the end of the western main commuter line out of Philadelphia about 30 miles west. And he says, "Tony, you go back to Paoli. And I want you to produce a probability map for the location of the Scorpion," based on the weighted scenario methodology that Craven had developed for the H-bomb.

He turns to me and he says, "OK, do you have a passport?" Well, fortunately, I did, because my wife and I had been traveling in Europe the summer before. He says, "OK, I want you on a flight to the Azores on Friday afternoon. It's leaving-- Friday evening-- leaving out of Boston." And so I managed to get on that flight. This is one day later, you know. I'm on a flight to the Azores, flying there with a number of other people.

We spent a couple of days in the Azores and then caught a submarine rescue ship-- ASR was the designation-- that took us out to the location of the Mizar, which was the Naval Research Laboratory vessel doing the underwater search for the Scorpion.

I boarded the Mizar and joined the search there as a person to apply search theory to the search. OK. So, now, remember, I'm 25 years old at this point. And I just know enough search theory to be dangerous. There's a senior scientist on board, Chester Bucky Buchanan-- very, very capable, very good guy-- a scientist, as well as a senior Navy captain, a submariner.

And I'm telling them, oh, you've got to search here next. You've got to search here. And you didn't put in enough effort in here. And I can just imagine that those guys wanted to throw me overboard. I'm surprised they didn't. What the hell does this kid know? I don't blame them.

But somehow they refrained from doing that. And I did survive seven weeks of on-scene duty there, helping out in that search, which was totally fascinating for me. Then I got replaced by another Wagner representative who came out there to continue on. The search went on until October to find the Scorpion. And that's when they finally found it.

After they found it, and we found out the location of it, Tony compared the location of where the Scorpion was found to his probability map. And it turned out the Scorpion was only about 200 yards away from the highest probability cell in the map. This map covered a lot of area, like 144 square miles, I think. So that's a pretty close miss.

That was the start of my work in search theory. At that point, I was absolutely hooked on operations research. I could never at that point go back to academics.

What really hooked me on doing OR was the fact that you'd make up a mathematical model, which is necessarily a simplification of the real world. You'd apply it to a problem, give advice on the basis of that model, and it would work. And that is such a kick to me every time that succeeds. I keep coming back and doing it again and again and again. That's why I'm still in the business. It's such fun.

5. Mentors

ALLEN BUTLER: You mentioned Tony Richardson. So would you say he was a mentor?

LAWRENCE D. STONE: Yes. Definitely, particularly in my early years at Wagner Associates I worked most on projects with Tony. I really learned how to do OR work from watching Tony operate. I would go with him to visit a client, and the client would give some fuzzy description of the problem. Tony would take it all in, and we'd go back to Paoli. Somehow, he'd take that fuzzy description, come up with an elegant and sometimes very simple mathematical model, and come up with a way to use that model to produce useful results for the client.

And so that, to me, that's how you operate. That's how I learned to operate in operations research. Tony also was very good at presenting things in a very clear, simple fashion to clients in both his oral presentations and in his write ups. So I sort of adopted his style, that point of view. Try to say it as simply and clearly as I can.

He was definitely my mentor as far as doing operations research was concerned. I think Dan Wagner was more of my mentor in terms of-- he helped bring me along as a researcher, and kept urging me to publish in mathematical journals and keep up my math research.

As a result of the Scorpion search, Tony got a contract from the Office of Naval Research for us to extend search theory to take into account, I think, false targets, and maybe even uncertain sweep width, because one of the problems we found on the Scorpion's search was that we had very bad estimates of the sensor capabilities. So then the question becomes, how do you plan a search when you have a large uncertainty in what your sensor capability is?

I started working on that contract. Thereafter, I think was under contract with ONR to do that kind of research for about 10 years. That allowed me to become a very preeminent researcher in search theory, and led to the publication of the Lanchester prize book *Theory of Optimal Search*.

Now, as I mentioned, Dan Wagner was the guy that was sort of my academic mentor, in the sense of keeping me interested in doing research. And I actually wrote a couple of papers with Dan Wagner. But I finally had to give that up, because our styles were just so different.

He would always want to prove a theorem in its greatest possible generality. And one of the things I remember my major professor saying at the time I was working on my thesis, was that when you get a result there's sort of a natural level of generality for that result for which the result is reasonably easy to prove, and interesting. That's what you ought to head for.

Well, Dan never believed in that. He said I want to take it to the maximum generality. So this probably won't make sense to most people. But we were working on a particular problem where you could have a reasonably good proof, nice result, if you assumed Borel measurability, whatever that is, and if you wanted to extend that a little bit to the class, slightly larger class, of Lebesgue measurable functions, it was a lot of extra work.

And we had a big fight about that, discussion about that, and he won, of course, because he was the boss. I said to him, look, nobody's ever found a Borel measurable function-- no, no one has never found a function that was Lebesgue measurable but not Borel measurable. We know they exist, but nobody's ever found one. So why are we working so hard to extend it to that? And if they did find one, it would be a much more interesting result than this result.

ALLEN BUTLER: Were there any others who played a role in influencing your career?

LAWRENCE D. STONE: Dan Wagner put me in touch with Jay Kadane, who also worked on search theory, and was a very eminent Bayesian. This is the late 60s and the early 70s. Bayesian approaches to solving problems and Bayesian statistics was not held in great repute. But the whole area of search theory lent itself to a Bayesian approach.

So I sort of fell into that naturally. It seemed like a good way to do things. And Jay remains my source. When I need to consult somebody about the right Bayesian way to do something, or get a review of what I've done, I go to Jay. So he's my mentor there definitely.

So another big influence for me was Bernard Koopman. Bernard Koopman was the founder of search theory. He and his colleagues developed it during World War II for the Navy's Operations Evaluation Group, and wrote it up in this report, *Search and Screening*, and later published a series of three papers in the *Journal of Operations Research* that presented an unclassified version of it.

Koopman and I had lots of fights, but I always highly respected him. And we enjoyed one another quite a bit too. But one of Koopman's critiques of my search work was-- and the book *Theory of Optimal Search*-- he said, there's too much measure theory in it and not enough physics and applications. And looking back these many years, I have to agree with him.

6. Searching for Ordinance in the Suez Canal

ALLEN BUTLER: That's good. So tell me a little bit more about your career and some of the work that you did for Wagner Associates.

LAWRENCE D. STONE: Dan was always very good at lining up opportunities for me. He lined up the chance for me to teach the search and detection course at the Naval Postgraduate School in the winter of 1973. So I did that. And I enjoyed it quite a bit. I think I was very hard on my students, because it was very mathematical and not really as applied as they needed. These are Naval officers, you know. They really didn't need a heavy dose of mathematics. They wanted to be able to plan searches.

Shortly after teaching at the post graduate school, I got an appointment as an adjunct professor at the Naval War College. And this was arranged for me by Captain Warren Rodgers, who had become familiar with my search theory work because he was a client of mine.

So I got a whole year up at the Naval War College to basically just write a book on search theory. That was his suggestion. I should write up my work on search theory into a book. That's where I wrote *Theory of Optimal Search*, which was published in 1975, and won a Lanchester price.

When I finished up at the Naval War College, I came back to Wagner Associates. They were involved in helping the Navy help the Egyptians clear unexploded ordinance out of the Suez Canal. All right. So let me explain that to you. This is 1974. In 1973, the Egyptians went to war against the Israelis, the Yom Kippur War. Actually, it wasn't just the Egyptians, it was the Syrians and the Jordanians too.

The Israelis are very complacent. They almost got run over before they could recover. But they did manage to recover in time, and finally, on the Egyptian front, they pushed the Egyptians back across the Suez Canal, and had an Egyptian army surrounded in Suez City, which is on the southernmost part of the canal, and were getting ready to annihilate it.

The Russians were putting troops on planes, getting ready to support their client. Kissinger was madly negotiating to try to get a truce. And he finally did. He convinced the Israelis and the Egyptians to come to a truce. And one of the things that the United States does and Kissinger does in situations like this is they throw in sweeteners.

One of the sweeteners he threw in was the US Navy would help the Egyptians clear the unexploded ordinance that the Israelis had dumped into the canal out of the Suez Canal so they could reopen the canal for commerce. That was a big, big source of income for the Egyptians.

So the US Navy went over to do that, and they contracted with Wagner Associates to provide analysis support for that clearance operation. Tony Richardson, that was another one of his projects, he organized that, as he always did, just beautifully. Tom Corwin was over there, who is-- you will meet later as the founder of Metron. That was when he was first a Wagner associate.

Later on, I got sent over to the Suez Canal to do some of that analysis too. We were very well equipped for those days. We had these handheld programmable calculators, HP35s.

And we had these Wang desktop programmable computers with a plotter. We would make recommendations for how they would organize their sonar sweeps so that they could detect the unexploded ordinances. Then take the detections and kind of cluster them and say, this looks like a location where the divers ought to go down and explode that ordinance and get rid of it.

Then we'd chart how carefully that was done and say to the Egyptians, the Suez Canal Authority, OK, this area is well cleared. This area is well cleared, or you need to go back and maybe do that area some more. So that was quite an experience.

7. Monte Carlo Search and Particle Filters

ALLEN BUTLER: Well, one of the things I always thought was interesting was how much you were able to accomplish before that without any computers and so forth.

LAWRENCE D. STONE: Well, yes. So just to compare the Suez Canal clearance operation to the Scorpion search. When I went out on the Scorpion search, I had a book of mathematical tables, a slide rule, and on board the Mizar they had an electronic desktop calculator that had one memory cell in it.

You could put one number in memory and use that for your calculations. And that's what I had available for me on the Scorpion search, compared to these desktop programmable calculators and HP35 handheld calculators, which, although they seem minimal these days, made a lot of computational power available to us.

ALLEN BUTLER: So, were there some non Navy projects you worked on as well?

LAWRENCE D. STONE: Yes. Again Tony Richardson lined this up. He convinced the Coast Guard to-- the Coast Guard does search and rescue planning probably every day off the coast of the United States. In 1969 the way they were doing their planning basically was manually. They adapted the various methods in search and screening to looking for people lost at sea and drifting on the ocean.

The Coast Guard wanted to computerize that. So Tony Richardson convinced them to give a contract to Wagner Associates to do that. He designed a Computer Assisted Search Planning System, CASP. It was a brilliant design. He was looking at--

The problem typically is you get, say, a last known position for the person-- might be in a raft, might be a boat, or a person might even be in the water—that position has some uncertainty to it and a time associated with it. That person or boat is going to be drifting according to winds and currents, and you're going to get out there some time later and you're going to try to search for him.

So problem for Tony was, how would you model that motion? The Scorpion was a stationary target problem, and now he had to deal with modeling this motion. He looked at lots of different approaches, Markov chain approaches, all sorts of cellular approaches, analytic approaches. And finally said, you know, I think the thing to do is to do this by Monte Carlo.

I'm going to Monte Carlo these possible paths for the drift of each possible path of the search object. I'll keep that file, a computer file of those paths, and I will advance it to the time that I want to search, compute a probability distribution, recommend search effort. The Coast Guard would go out and search that day based on that recommendation. If they failed to detect, you'd compute the posterior distribution given failure, and then update for motion with those particle paths to the next time period. And continue the search again.

And this was, to my knowledge, the first operational particle filter. For those in the detection and tracking community, this Monte Carlo tracker, what Tony called it, really was a particle filter, although he didn't name it that and he didn't publish it or publicize it very much.

He then later went on to extend this to work for the Navy. This is 1969 or 1970s, developing this for the Coast Guard. The Coast Guard system went operational in 1974. In the meantime, Tony was off applying these ideas for the Navy. In that case, it was looking for Soviet ballistic missile submarines operating in the Atlantic. The way that would operate is the underwater surveillance system, SOSUS, would get these detections, which usually had large uncertainty regions to them, and you'd send ASW surveillance, anti-submarine warfare surveillance aircraft, out with sonobuoys to drop on these locations, and try to get a localized detection of these ballistic missile subs.

Tony developed a method for helping to plan those searches. The Soviet's patterns were very consistent in those days, so you could make a nice probability model for how the submarines were moving using your particle filter-- Monte Carlo tracker. And you'd apply it. That would be the prior you'd apply these surveillance detections, compute-- update for that positive information, compute another posterior, send the aircraft out to search. If they failed to detect, you'd update for the negative information.

Typically when you updated for the detections, you had to do something called resampling. So we actually had a full particle filter going at that time in 1972. But since it was very classified, he never wrote that up for publication. Later on, in the late 1990s, Neil Gordon, in England, rediscovered this Monte Carlo tracking, renamed it particle filters, and popularized it.

Since computer capabilities were so much better then, it became a very popular and powerful technique for doing detection and tracking. It's one of the mainstays of the detection and tracking community nowadays.

8. Leaving Wagner Associates for Metron

ALLEN BUTLER: So you left Wagner Associates in 1986. Where did you go from there?

LAWRENCE D. STONE: In 1986 I joined Metron, which was formed by Tom Corwin. Metron was located in Washington, the Washington suburbs. Tom was the founder. It was a small company when I joined it. But Tom and I built it up from 1986. And I'm still working at Metron these 30 years later.

The company size now is about 200 people, quite substantial. Both Tom and I followed the Dan Wagner model of how you ought to organize a company to some extent. I'll tell you where we differed. But we wanted to work on interesting problems, problems with a good mathematical content. And, in fact, if you look at our tagline for Metron, it says scientific solutions, which pretty much describes what we try to do.

And Dan Wagner always made the point of-- to back up a little bit, Dan Wagner was in the Navy's Operations Evaluation Group during the Korean War. The way the Operations Evaluation Group worked was they would hire-- starting back in World War II-- they go out and hire all these very smart academics in technical areas, chemistry, physics, mathematics. And they put them to work on Navy operational problems. And they did really good work. They did terrific stuff.

So Dan took that aboard, if you will, and said, that's his philosophy for hiring too. At Wagner Associates that's how we would hire people. He was good at-- he had very good connections to the mathematics community, and he knew good mathematicians when he saw them, and he was good at recruiting them.

He'd recruit the smartest people he could, the smartest mathematicians he could who were interested in doing applied work. He didn't care what their math background was. We had people in number theory, logic, algebraic topology.

ALLEN BUTLER: Functional analysis.

LAWRENCE D. STONE: Functional analysis, which is a little closer. Mine was probability theory, which is more applied than normal for the folks that Dan hired. And he'd do the same thing. He'd send them out on field assignments, and had them work on Navy problems. And they did some really good things. Wagner Associates, in those days, developed a lot of important results for the Navy, particularly in the area of anti-submarine warfare.

So Tom and I continued that philosophy in hiring. We would hire people that way too. And we very much modeled Metron on Wagner Associates, but what we try to operate in a bit more businesslike fashion than Dan did.

I ended up being sent out on a field assignment too. I was sent to Hawaii in 1978, 79. I was working for 3rd fleet, which was located on Ford Island off of Honolulu in those days. I was basically running the sort of programs that Tony had developed to track submarines. I was running them out in the Pacific to help the anti-submarine warfare forces out in the Pacific to track Soviet submarines, detect and track Soviet submarines too.

So that was a great experience for me, and very typical of Wagner Associates. Corwin did that too. He was assigned to Commander Submarine Force Pacific for a number of years in Hawaii, doing a lot of interesting search analysis for them, and analytic work. We very much believe in that.

9. Searching for the SS Central America

ALLEN BUTLER: On another subject, I know you were involved in the search for the gold ship SS Central America. Why don't you to tell us about that?

Oh, yes. While I was working at Wagner Associates and overlapping to my beginning at Metron, this group called the Columbus America Discovery Group approached me to produce a probability map for the location of the SS Central America.

The SS Central America was a wooden hulled, iron, side wheeled steamship. It was a US mail steamer. If you were traveling from California to New York in those days, you would take a US mail steamship from San Francisco down to the west coast of Panama, take a train across Panama, be picked up on the east coast by another US mail steamship, in this case, the SS Central America, commanded by a Navy commander, Captain Herndon.

It took you to Havana and then from Havana up to New York City. So this is in September 1857. They set sail from Havana, on a gorgeous day. What they didn't know was they were going right into a very strong hurricane. The ship went down in that hurricane off the coast of South Carolina in about 8,000 feet of water, carrying a lot of gold bars, tons of gold bars and gold coins from California.

All the women and children got off the ship. Herndon was an amazing person. He was a professional, and made sure that women and children got off. So there were lots of survivors. A lot of people died in it too. And the survivors had records of where the ship went down.

In 130 years or so, nobody had really seriously tried to locate this wreck, but Tom Thompson put together a group of investors, because there is lots of gold, to try to find the ship. And he hired me as a Wagner Associate to produce a probability map for him. And that overlapped-- I finished up that work at Metron. And they did use my probability map to plan that search. I think I started working for them in 1985 or so.

In 1987, they started their search. In 1989 they came into the harbor at Norfolk with one ton of gold bars and coins, and to a very celebratory reception at Norfolk with bands playing, and the mayor was there giving speeches, and US marshals were there to arrest the gold. It was quite a scene with lots of news coverage.

10. Tracking and Locating Targets

ALLEN BUTLER: Well, search theory is not the only area where you're a published author. Tracking and locating targets is another. How did that work come about?

LAWRENCE D. STONE: I mentioned the Monte Carlo tracker that Tony Richardson developed. This is really what's called a Bayesian non-linear tracker. And I started to-- well, actually, so let me back up a bit. When I first joined Metron, we got a contract from Lockheed out in California to develop a search planning system to be used aboard ASW search aircraft to plan sonobuoy searches for submarines.

And I developed a very nice system that did real time planning of the searches. We'd take feedback from the search, calculate new probability distributions. Allow them to replan their searches all the way down to search detection, localization, and eventually to where you ought to drop your torpedo to attack the submarine.

This was developed for Lockheed for -- what's called a demonstration validation part of their proposal to the Navy to update the electronics, the avionics, on their ASW patrol aircraft, the P3 they were called. This was going to be Update IV. Boeing was also bidding on the contract.

Unfortunately, Boeing won. Lockheed lost. So I never got a chance to implement that system. After that, I was the lead developer of a very nice multi target Bayesian tracker for the underwater surveillance system. My timing was quite unfortunate. The system was scheduled to be introduced into the surveillance system. But the Soviet Union collapsed, and the Cold War ended, and nobody cared about searching for submarines anymore. They just said, forget it.

But, fortunately, I got a contract from the Office of Naval Research to look at the foundations of detection and tracking from a Bayesian point of view. The point was to look at detection and tracking as a Bayesian inference problem. I did that, and as a result Tom Corwin and I wrote a book called *Bayesian Multiple Target Tracking*, published in 1999 and updated with a second edition in 2014.

11. Searching for Air France 447

ALLEN BUTLER: And you were involved in another high profile search.

LAWRENCE D. STONE: That's true. Rather late in my career, in 2009. Let's see, the flight Air France 447, crashed into the Atlantic Ocean near the equator. It was on a trip from Buenos Aires to Paris. Right in the middle of the Atlantic Ocean it crashed and they lost contact with it. The French BEA, Bureau of Enquiries and Analyses, is the French government group that's in charge of searching for missing aircraft, they organized a surface search for people to see if they could find survivors and debris. They did find some debris.

They also organized an underwater search for the pingers, which was not successful. When an aircraft goes down in the water, they have two water activated pingers on board. They have a battery. They're battery powered, and they will ping for about 30 days at a certain frequency. So the best way to locate an aircraft that's under water is by detecting the pingers, and following the ping to the location of the aircraft.

They tried to do that, but that was not successful. Metron was involved a little bit in 2009, but by the time we did our analysis, they had already-- we were too late to have any effect on the search in 2009.

In 2010 the political winds changed and Metron was out. They had somebody else plan a search. So that somebody came up with a wacky search plan. When I looked at it, I couldn't believe it. But the French followed it for the whole year-- search year 2010, didn't find the aircraft. And so at that point, Metron's star rose again.

They said, OK, Metron, would you produce a probability map for us based on all the information available, all the prior information, all the debris that was recovered, the unsuccessful search, everything? And we did that. We produced the map in January of 2011, sent it to the French. In March, the end of March 2011, they started their search, and they found the wreck within one week. Also they found the wreck in the high probability area of the probability map that we produced. That was very surprising and very satisfying, because it doesn't always work out that way.

12. Latest Book

ALLEN BUTLER: And you've been doing some more writing very recently here, haven't you?

LAWRENCE D. STONE: Yes. The book *Theory of Optimal Search* really was concentrated on stationary target problems, because that's where the results were. I did discuss moving target problems, and what results there were, but there just weren't very many.

But shortly after the book was published, there was just an upwelling of results, just a large number of results in optimal search for moving targets. And it started this way. It started with actually, Alan Washburn, at the Naval postgraduate school.

He published a paper-- or he wanted to publish-- he submitted for publication a paper that was a one dimensional search problem where the search target could move one cell in one direction. You know, one left or one cell right. So it's a one dimensional diffusion. Alan had some rather complicated method of coming up with necessary conditions for an optimal plan for that moving target.

And I was the referee of it. I was asked to referee it. And I said, OK. My comments back to the editor and then back to Alan, eventually, was that what you really ought to do is set this up as a Markov chain, a much more general motion model and solve it for that. So a comment from Alan through the editor back to me was, that's a good idea. But I can't solve that problem.

So we had this very smart guy, Scott Brown, at Wagner Associates. He had a degree in Logic from Princeton. I said, Scott, you know, this looks like a convex programming problem to me. Why don't you see if you can solve it using a convex program? So he looks at it he says, yeah, you know, you can.

But when you do the iterations in a convex program, in the standard convex program, it changes the effort in one cell at a time. And when he started looking at what it was doing, he realize that you can do it much more efficiently. In effect, what you're trying to do is to maximize the detection probability for a given time period. You've got capital T time periods here.

If for one of those time periods, little t, you look at the posterior distribution on the target's location at that time period, given the effort you put in at the other time periods was unsuccessful, and what you're trying to do is allocate your effort optimally for that stationary target problem. And that's a necessary condition for optimality.

And so one way to iterate through this problem is you start at time 0, and you do a myopic search. You search in a way to maximize your detection probability for time 0. Time 1 you maximize the detection probability at 1 given failure at 0, work your way all the way up to time capital-T. That's the myopic plan.

Then you come back to time 0. You take the search effort off at time 0, re-allocate that. to be optimal for time 0, given failure at all times in the future. And if you just keep doing this, going forward and backward in time, then you generate a sequence of search plans that converge to the optimal search plan. So that was Scott's breakthrough. And once he saw that, we were able to solve all kinds of moving target problems.

Alan Washburn came up with a very nice bound. So that if you applied what's called Brown's algorithm that I just described to you, you could tell each step of the sequence how far you were in probability from the optimal plan.

It turns out often it didn't take very many iterations through that sequence to get very close to optimality. Washburn generalized this to a class of forward and backward problems, more general than just detection problems. Being the mathematician that I am, I generalized his result. His result was in discrete time and for a finite number of cells.

So I said, OK, I can do this for continuous space. I can do it for continuous time. So I did continuous space, continuous time, and produced a very nice set of necessary and sufficient conditions.

The nice thing about these results are that you can use the necessary and sufficient conditions to design numerical algorithms that compute these optimal plans. So a very, very satisfactory result. It was all out there in papers but never pulled together.

So finally, about 2015, or the end of 2014, I said, OK, I've been meaning to write up these results for about 30 years into a book. I think I better do it now. And I convinced Washburn to be a co-author with me. We laid out the book, chapters of the book. At one point we got to this class of problems called constrained searcher path problems.

So the solutions I'm describing to you where we are able to solve the problems nicely, you're allowed to spread your effort out as far as you want to in each time period. There's no constraint

on where you put your effort at-- there's a constraint in total amount of effort, but not how you spread it out. And where you put effort in time 1 didn't affect where you put it in time 2.

But, of course, there are many problems in which that's not true, much of the time your search efforts is constrained. Those are much harder problems. Washburn had done some work on those many years ago, more work than I had. So I said, Alan, you write up those chapters. He said, I'm not writing them up. He said, you ought to write them up.

I said, Alan, I don't know this stuff. So we went back and forth. And finally, Alan said, let me talk to one of my younger colleagues here at the Naval Postgraduate School, Johannes Royset, and see if he's willing to contribute chapters on that. And he was. And he contributed two very good chapters to the book.

It was published in March of this year, March 2016, *Optimal Search for Moving Targets* by Springer. The work by Royset has a different character, though, than what Washburn and I were doing. For our stuff, you've got these necessary and sufficient conditions that told you sort of what the optimal plan was trying to do, right?

Well, Royset was very clever. He was able to set up these much more complicated problems with very realistic operational restrictions on them. He was able to set them up in an efficient way, as a mixed integer program, then hit it with one of those big commercial mixed integer program solvers, and was able to get really good results for very hard problems.

But, you know, it's a black box. All you get is the answer out of it. You don't know how it got it. But that's the nature of the way he was solving the problem, much, much differently than the way Alan and I were attacking our problems. But he's able to solve a really good set of hard problems.

13. Legacy in OR

ALLEN BUTLER: Well, so we've talked about a lot of different things. Why don't you summarize for us what you think are your most important contributions to OR?

LAWRENCE D. STONE: Well, I think the search theory work with the optimal search for stationary targets and moving targets, I think that's an important contribution. The participation in these high profile, high interest searches, really, I think helped popularize the OR method, OR, and what I like to call the Bayesian search method. So I think that's a contribution.

Another contribution is that Metron was one of the contributors to setting up the Wagner prize, Metron put in part of the money that's used to fund the prize. And I guess I have to tell you what the Wagner prize is. OK. In memory of Dan Wagner, I guess it was Joe Discenza convinced the CPMS, which stands for-- I'm not sure I remember what that stands for.

ALLEN BUTLER: It's OK because we changed the name of it. It's now the INFORMS Section on Practice.

LAWRENCE D. STONE: To institute this prize in honor of Dan Wagner, shortly after Dan died. Most people are familiar with the Edelman prize, which is a very prestigious prize for OR. But the Edelman prize is kind of about big projects with big payoffs, typically financial payoffs. And if there's some mathematics to it, that's OK. But it's not crucial.

The Wagner prize, by contrast, really wanted to require the people, the participants, to write papers with good mathematical content. They had to have important problems they're solving, but also had to have good mathematics too. And the papers had to be well written. This is very much in Dan's philosophy of how you do OR. You apply good mathematics to hard problems. And you write them up in a-- you document them carefully and you write them up well.

So the Wagner prize I think is really designed to reward that kind of activity. I've been a judge in the Wagner prize. You, of course, have been in charge of running it, the chair of the prize committee for many years. I think Joe Discenza was the original chair. And, you know, I'm very proud of that. I think that's a definite contribution to the OR community.

Going back to the Edelman prize, I actually was-- participated in the Edelman prize. I was an Edelman prize presenter back in-- I remember the dates. I'm sure it was a-- 1992, I think. In those days, Edelman prize presentations were much simpler. So I did it pretty much myself. But my coach for the Edelman prize presentation was John Kettelle, who was Dan Wagner's original partner. Because Kettelle and Wagner started off together.

They both started off in the OR business, and then they split, and Dan formed Daniel H Wagner Associates, and Kettelle formed his own company, John D. Kettelle Associates, and other companies later on.

Unfortunately, I lost out, didn't win the Edelman prize, I lost out to something called yield management developed by American Airlines and Tom Cook. . After about a year, I decided that was a pretty reasonable choice. It took me that long to come to that conclusion. But that was fun, participating in the Edelman.

ALLEN BUTLER: How do you think the field of OR has changed throughout your career?

LAWRENCE D. STONE: Well, in two major ways. One, when I first started, OR was dominated by military OR. And it was also dominated mostly by people from the United States. , Nowadays, when you go to an OR conference, now INFORMS, it's almost all industrial. And it's very international, much, much, much different. Military OR is a very small part of the operations research activities these days.

The other is, of course, the computer equipment. So, again, remember, when I started on the Scorpion search, I had a slide rule and a set of mathematical tables and this little calculator that had one memory unit in it. Nowadays you have these very powerful and inexpensive computers and gorgeous graphical displays.

And data, I mean, you have data available now that we just dreamed of in those days. So you've got data. You've got computers. You've got displays. So when you're working on a problem, you

can do the mathematics the way it should be done, and you can compute the answer, because you've got the power of the computers to do it. And you got the data available to feed that problem. And when you want to present a solution to a user, you can present it to them graphically in a way that they can understand it.

And it's just so terrific. I mean, it's why it continues to be fun to do this, because you can-- your mathematics now lives in that gorgeous software and displays as far as the user is concerned. You can do this incredibly complex thing, and it's easy for the user to understand it and use it.

ALLEN BUTLER: So, finally, where do you see OR going as a field going forward?

LAWRENCE D. STONE: Yes, One opportunity for the OR community is to-- and they're doing a pretty good job of this-- is to take this area of data mining, and to show how valuable it is to apply analytics to it. The fact that it's not just data, but it's the analysis of the data where you get the real value. And I think there remains an incredible opportunity for the OR community to do that.

And analytics, by the way, is a better name than OR, if you're trying to explain to people what you do.

ALLEN BUTLER: Well, thank you very much, Larry. Really enjoyed it.

LAWRENCE D. STONE: OK, Allen, thank you.