Interview of G. David “Dave” Forney, Jr.

Interviewed by:
James Pelkey

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James Pelkey: You went to MIT?

Dave Forney: Let me just pick up on what I was saying before you turned this thing on. What comes immediately to mind is that I'm going to send you a short article on the early days of Codex that I did for a customer newsletter called Multipoint. If you're interested in the technology, I have a review article on the history of modems that was published in '84. Pahlavan and Holsinger published one last year in Communications Magazine. These are things you can get yourself, but I think I'll try to dig them up for you, and if other things occur to me, I will send them along.

Pelkey: I appreciate that.

Forney: OK. I graduated in '61 from Princeton. I was at MIT from '61 through 65. I worked in the summer of '62 at Lincoln Laboratory. I was a Lincoln Laboratory Staff Associate. In '65, I went to work for Codex as a Member of the Technical Staff. As you know, we nearly went out of business in '70, and at that time I became an officer and director of the company. I was VP of Research; then for a while I was VP R&D from approximately '75 to '79, when I went back to being VP of Research. I held a Motorola ISG position from '82 to '86, and I’m now back in Codex as a Motorola Vice President, so you have all that.

MIT -- what do you want to know?

Pelkey: Shannon and the information theorists: that was a very intense period of time at MIT. Did you come in contact with those ideas? Were you there specifically for those kinds of things?

Forney: I wasn't there specifically for that. I knew that I wanted to get a doctorate in electrical engineering. I took the basic information theory course from Bob Gallager in my first year, and I thought it was very interesting.

I did my master’s thesis in '63 on a combination of quantum mechanics and information theory. The idea of entropy shows up in both places. I was really trying to connect the “entropy” of physics with the “information” of information theory, whereas what everybody had tried to do before was to show that the two concepts of entropy were the same. That isn't quite right. I still feel that's a hidden jewel that will some day see the light of day.

The question of personal history, I think one thing you should understand is that the word in the halls of MIT then was that information theory had pretty much been done. It was all -- all the action was going to be in applications now, but basically the theorems had been proved, the codes had been developed. There was enough known about how to get close enough to channel capacity that there really wasn't anything terribly exciting to do, and people were looking for other things to do. So there was an exodus of people from information theory.

Shannon was on the faculty then. I think MIT considered it a coup when they hired him away from Bell Labs, but in fact he spent very little time at MIT. He was important to me. To take things out of sequence, which you don't have any problem doing in your HyperCard mentality, after I finished my master’s thesis, I spent the better part of an academic year looking around for something to do my doctoral work on, and I specifically did not look at information theory because of this general atmosphere, even though I was personally interested in it.

In the spring of that following year -- the spring of '64 -- I took an Advanced Topics in Information Theory course from Shannon. His method was just to put some problems up on the board, some of which he'd solved and some of which he hadn't, and I was able to make some progress on some of the ones that he hadn't, and that led me to look at something else, and I was able to get some results there, and before -- basically before I knew it I was back doing information theory again. I found that there were one or two things left to do in information theory.

Pelkey: In spite of what they thought?

Forney: In spite of what they thought. So Shannon was the cause of my staying in information theory.

Actually, probably the reason for my first interest in it was Professor John Wheeler at Princeton, who taught a course in thermodynamics that I took in my senior year at Princeton, and I did a report on Brillouin’s book about information theory and physics that clearly affected what I did later.

So anyway, Shannon wasn't very much in evidence by and large, and people like Fano and Elias, the heavyweights, were saying: “Let's go into computational complexity, Project MAC, computers and so forth,” so everybody, with the exception of Gallager, who was hardline information theory, and Jack Wozencraft, who was actually my thesis advisor and was strongly involved with coding at Lincoln Labs, but he was sort of looking around for new worlds to conquer -- and maybe a couple of other people, but the trend was to get out of it. As a result, MIT, which was absolutely the world's center in communications, lost that position, and it's been a -- I don't necessarily want you to publish this, but -- they've been concerned now for a long time at the Dean and Department Head level that MIT is not nearly well represented enough in communications, because what in fact has turned out is that information theory has, of course, continued to evolve. Communication theory -- new things keep coming up, even in modems, and it's remained a very lively subject.

Pelkey: Absolutely. Now, were you aware, at this point in time, about the Arpanet and what they were trying to do in that realm of communications?

Forney: I'm trying to think -- this was the time of Project MAC, but I think we're a little prior to Arpanet. Arpanet, I really think of as late '60s, early '70s -- if you went to a conference in the field then, half the papers were on some aspect of Arpanet.

Pelkey: That's true. In fact, Larry Roberts -- his thesis advisor was Shannon. He was on his committee, and Ivan Sutherland and Leonard Kleinrock, they all preceded you at MIT, and Shannon was advising their theses.

Forney: That didn't have to do with what I was interested in, and my personal feeling was that it was a whole lot more noise than was warranted by the actual success, either of Arpanet or subsequently Telnet and X.25, but Roberts and others did a fantastic promotional job, and in fact it has affected the way the world has gone, no doubt about it.

Pelkey: So what prompted you to join Codex?

Forney: Well, have we talked all you want about MIT?

Pelkey: We're going to go back to it.

Forney: Well, the reason I joined Codex was that I was coming up on getting my doctoral degree, I was thinking about employment. I went to talk to Bob Gallager, who had consulted for Codex since its founding in '62, and he said: "Well, of course, you ought to consider Bell Labs and IBM Labs is coming up and there's this little company, Codex, that you might want to take a look at." It was 12 people at that time. I did take a look at it. I liked the idea.

Codex had 12 people in a second-story office above a tailor shop on Mass. Ave., Cambridge. The whole group of people were in an area no bigger than this suite of offices here, and I sort of thought that was neat, but to follow up on what I said before, I never really considered an academic job, even though I was interested in the theory of these things, because I was so convinced that, yeah, the thing to do now was to go out and try to apply some of this stuff, and Codex was one of the very early companies trying to do that.

Pelkey: Now they were doing error correcting coding sorts of things for the government when you joined them.

Forney: That's right.

Pelkey: And Arthur Kohlenberg was --

Forney: Arthur Kohlenberg was the senior technical vice president, right, one of the founders

Pelkey: There were two or three founders.

Forney: At the time I came, I was told there were three founders. Since then, Joe Van Horn has been written out of the history books.

Pelkey: Yeah, he has been.

Forney: In fact, I believe the story is that Cryer and Kohlenberg were at the Melpar Research Laboratories. When they moved to Virginia that precipitated them to found Codex. Van Horn came soon thereafter, but he was basically hired by them to be the head of engineering. He stayed at Codex for four years, which I was told was the longest time he had ever stayed anywhere, and when he left, I and a group of my friends -- a syndicate that I put together -- bought all his stock, which was the only way I got a stock position in Codex, but none of the founders had that much stock. I wouldn't say he had that much, so he was in a 'quasi' position. I think that's why you get the two stories.

Pelkey: Revised history.

Forney: Again, all these people are dead now, and 'de mortuis nil nisi bonum.'

Pelkey: Now, Arthur was, if I understand correctly, was a very talented individual.

Forney: Very talented and a sweetheart of a guy, and my mentor, and I loved him.

Pelkey: Then, along the way, he found this little group of three people in Santa Barbara: he located Jerry Holsinger.

Forney: It was really two people that got to Codex, I think, Jerry Holsinger and Eli Eielson, and they came -- they had offices near mine. They were laying out the circuit boards, but yeah, they had developed at least a paper design of a 9600 bit per second modem. I don't know exactly what was done when it was Teldata. At Codex, they reduced it to RTL logic, and then 66 tiny little ‘daughter boards’ that were stacked in a military-like rack, and that made the AE-96, or I should say the first version of the AE-96, because it was constantly being tweaked during its lifetime.

Pelkey: So I understand.

Forney: I'm sure you heard from Jerry.

Pelkey: When you first got exposed to that, having come from this background of information theory, what was your reaction to hearing about 9600? A lot of people didn't think that could be done at that point.

Forney: Well, I really wasn't that involved with the modems for the first two years, probably. This was a separate effort being led by Jerry Holsinger. I don't know why I was so incurious, but I was totally involved in work on coding for satellite communications and deep-space communications. I was aware that these guys were doing something different. I didn't have, I guess, enough background in the industry to realize what a leap it was. I do remember Arthur Kohlenberg telling me about telling what this was to some general who told him in return that somebody at Bell Labs had proved to him that you couldn't possibly send more than 2400 bits per second -- you know, Nyquist or Shannon or somebody had proved -- that that was the absolute limit, so clearly what they were talking about was impossible, goodbye. It is true that the highest rate in the industry then commonly available was 2400, available only on private lines, 2000 bits per second on switched networks with a Bell 201, and to go straight to 9600 probably wasn't that smart from a market point of view, as well as a technology point of view. In fact, Milgo came in with an eight-phase modem at 4800 and that was what they started to build their business on. By the way, I've got literally tens of thousands of pages of litigation material that tells you everything that went on during this period at Codex and Milgo, if you have the patience to look at it.

Pelkey: Oh, I'd love to see that. I do.

Forney: I guess I've only kept, at home, the actual transcripts of the trials, but there's, still in the warehouse somewhere, all the discovery material. I don't think you really do.

Pelkey: No, I don't need all that, but that lawsuit was a very important lawsuit, in terms of intellectual property, because there were, obviously, only a few of these, but at that point in time, characterizing Codex, it was technology elegance for technology elegance's sake. If it wasn't on the leading edge of technology, Codex looked down upon it. They really aspired just to do something that other people hadn't done; that was their culture. Is that a fair characterization?

Forney: I would say that probably goes overboard a little bit. I think there was, probably, a feeling that as a small company, what was our edge going to be? It had to be technology -- that if we weren't doing something that was more sophisticated than what other people were doing, we weren't going to get any business, so that was a matter of allocation of resources, unless we could see that we had a definite technical edge, we weren't terribly interested in something. That would not have precluded us from going to 4800, though. Why Jerry started out at 9600, I'm not really sure.

In fact, the C Series, which was the modem line that we came out with in 1970, which I was very much involved with, started out because Art Carr said: “Hey, it's very hard to sell a product line which consists of one product. We need an entry at 4800,” and it turns out technically that 4800 -- it's hard to do that with the single sideband technology that we had in the 9600. It's just awkward. You need to -- you'd really like to use 1600 Hz of nominal bandwidth, and you'd like to send three bits/Hz, which in single sideband would be one and a half bits per dimension, and it's hard to send one and a half bits. I remember looking at three-level schemes for doing that. The natural way of to do it was in two dimensions with an eight-point signal structure, signal constellation, as Milgo did with eight-phase, and really the whole C Series started from thinking: “Well, I can think of a good signal structure, and eight-point signal structure and use QAM built on the Modem-X work that you referred to, which I was familiar with by then, that “I can see how to get to 4800.” We worked out all the QAM technology. In the course of that, also worked out how to get to 9600, and those products were the ones that really worked. That's jumping around again.

Pelkey: That's all right. Now, when Art came aboard, and Jim Storey came and some management people came in, was that an important event in the company?

Forney: Well, Jim came in '66 as a financial VP. He had previously been with Hydrametals, which was a very small-sized conglomerate, but basically he was doing acquisitions, as I understood it. I didn't have much contact with him because he was purely on the financial side. Art came in in ‘68; Soderstrom came in in '67, I think, to be head of manufacturing. Art came in specifically to start developing some commercial business for the company. Up to then it had been 100% government and remained 100% government up until 1970. Probably the one who had the most personal impact on me was Art. He's a dynamic fellow, as you know, and he was very much involved in product planning. He brought in John Pugh. John also -- they began to form a vision of what kind of commercial data communications business there might be, and of course that led to what I personally was involved in.

I didn't get involved in modems until probably late '68, early '69, when there were some technical problems. What we found at 9600, as you know, what killed us was phase jitter, at least Jerry and the people in that group were finding that out in the field, that when they were finally able to trace down what it was that caused this thing to crash, it was phase jitter on the line, which even AT&T didn't know about at that time. Because the company had a lot of chips on this, Arthur asked me to take a look at what was going on, just try to understand the technology there. There may have also been some hedging of bets here too, because, of course, Jerry left within a year afterwards.

Pelkey: Before that, there was a problem of the modem not equalizing. There's an equalizing algorithm or something, where it would go out of equalization, and that was what led to the Forney Decoder, right?

Forney: No. I believe you’re right, but I don't think -- I think Jerry solved the equalization problem somehow. I don't know what the issue was or how it was solved.

Pelkey: Do you recall a meeting in which Arthur, at the request of Cryer, had filled the blackboards with his formulas, and Art Carr was presumably at the meeting. I'm told you were there, and it got towards the end, and he said here's where the problem is, in terms of equalizing, and Holsinger presumably made the comment: “You're the only one who could have found it.” Do you remember such a meeting?

Forney: I was there and I said that?

Pelkey: No, you didn't say it, but Holsinger said this back to Kohlenberg.

Forney: No, I don't remember it.

Pelkey: Because Jerry's recollection is that he kind of solved that equalizing problem on his own by kind of finally giving up on it, and then the solution came to him. Art Carr's recollection is something quite different.

Forney: I would go with Jerry, because Art wasn't that deep into the technology. Again, this was really before I was seriously interested in modems.

Pelkey: I understand you didn't get really seriously interested until after Jerry left.

Forney: That's when I really got seriously interested.

Pelkey: I'm told Arthur came to you and said: "You're going to become a modem expert. You're going to become a 60-day wonder," or something.

Forney: This is what I'm saying. When did Jerry leave?

Pelkey: '69.

Forney: In '69, OK. Well, let's say late '68, early '69, Arthur asked me -- Kohlenberg asked me to…

Pelkey: In November of '69, it was.

Forney: All right, he asked me to become familiar with the technology just because I was a bright guy and maybe I could give some useful input, so as a sideline I began to -- I remember Jerry explaining to me the equalization equations, for instance, and how a mean-squared equalizer worked, so I was kind of absorbing that.

I don't really recall the genesis of the Forney Decoder. Basically, once I understood partial-response signaling, I thought there ought to be a way that you could use the information over more than one symbol to make better decisions, and I worked out what looked to me would be the optimum way to make a decision for partial response, and later generalized that for Al Berner, and I showed this should buy you about 3 dB, and Al Berner took this, made it into a circuit, the “Threshold Decision Computer” -- I think that was a marketing term -- and hung it on the underside of the AE-96, and it did yield a 2 or 3 dB performance improvement, and I was told that it extended the commercial life of that product by a year or two. It gave it just that much more margin against noise, but it would have had nothing to do with loss of equalization, I don't think.

Pelkey: OK.

Forney: Again, this was just sort of something I contributed from the sidelines to the program. It was integrated into the thing and seemed to work. I was gratified, but I was still off working on deep-space communications.

Pelkey: And Jerry was still there at that point?

Forney: Jerry was still there.

Pelkey: But then Jerry left in November of '69, and then you were asked to get more heavily involved in the modem work.

Forney: Yeah, I was. I had also been tracking what Bob Gallager was doing with Modem X. I don't remember exactly the time sequence, but I was -- we had had some technical discussions on how you do double-sideband modulation, QAM, so I don't remember actively doing anything in the modem area, besides the Forney Decoder, until February of 1970 when there was this product planning meeting where Art Carr said: “What we really need is a 4800 bit modem.” I said: “I think I know how you could do that based on the Modem X kinds of ideas,” and fortunately, by then, I had learned something about how to design logic through designing coding equipment for space communications, so I was able to just sit down and start designing it. In fact, there was a whole sequence of events that year which -- this became the main project in the company. By May, or so, there was a breadboard operating. The initial results looked good. Of course, there became a great deal of urgency in getting it to market ASAP.

Pelkey: '70 was not a good year.

Forney: '70 was not a good year. I don't know any year in any other company where so many bad things have happened.

Pelkey: After the unfortunate death of Cryer and then Kohlenberg --

Forney: Yeah, I remember being called at home on a Sunday morning by our patent attorney, Jack Williams, who was also Cryer's landlord, about Jim dropping dead on the tennis court --

Pelkey: And on Monday you were supposed to go to New York and get some money.

Forney: -- just prior to completing a financing, right. The stock going from 50 to 4, and Arthur Kohlenberg wasn't as much of a surprise because he had had Hodgkin’s’ for a couple of years, but it had seemed to be in remission. By April, I think, it was already clear that it was gaining on him again, so yeah, a lot of things.

Plus, the AE-96 still wasn't taking off in the marketplace. The government business -- that was a terrible year for government business. Part of what happened with me was that I invested a lot of time in a sequential decoder, which was a high-performance coding system for communications satellite applications for the Army. The contracting guy called me up in the early part of that year and said: “Gee whiz, they wouldn't have the money after all.” We had hoped for some follow-on production business. We produced one prototype, they made some tests on it, called up and said: “Gee, we're not going to be able to fund you,” so happily that left me free to do modems.

Pelkey: One piece of bad news after another.

Forney: Yeah, everything happened.

Pelkey: And Art has told me this story, and I've heard it from a couple of people, that you kept yourselves alive that year until you could secure more capital, and I guess you trotted down to the bank as well.

Forney: No, I was not one of those who trotted down to the bank Thursday morning to pay the paychecks. Art and Jim were really the ones who raised the money and talked to the suitors and all that. I was Mr. Technology.

Pelkey: What led to the Series C, the 4800 and then the 9600, was a profound event, not only in the history of Codex, but also in the industry, in terms of coming up with a reliable 9600 baud modem at that point in time.

Forney: Yes, I think it was a landmark development, but what part of it would you like to know about?

Pelkey: Clearly, you had the experience of the AE-96, so there was confidence that you could pass data, not necessarily reliably for a long period of time, but you could pass data at this data rate. There was a real motivation to establish that position.

Forney: What did the AE-96 finally tell us? It would be interesting to get Jerry's comments on this too. I guess it told us, first of all, that there was 2400 Hz of useful bandwidth that you could count on that on a telephone channel. If the phase jitter wasn't too bad, then the signal-to-noise margin was adequate to pass 9600 using the single-sideband technique. It told us that phase jitter was a problem, which hadn't been realized three years before -- was the killer problem, at that time.

From a commercial point of view, there certainly was a lot of interest in it. The people who bought the machine realized that they were taking a risk. Most of them were people who needed to run a line from London to New York or something. If they could get it to work, it would pay back in two months. Initially the thing was priced at $23,000. Even so, a transatlantic cable circuit was $10,000 a month, so it revealed there was a tremendous amount of interest if you could only get something that worked reliably, and that's, of course, what held the AE-96 down. It was said that we sold 300 of them; but actually, it was only 100 boxes, each sold three times.

Pelkey: (unintelligible) I was told each was sold about six times.

Forney: Well, you've heard all these stories already, so just stop me when I --

Pelkey: No, no, it's useful making sure I hear correctly. These are important stories.

Forney: In many cases, I'm just repeating the company myths. So anyway, it also taught us that having only a 9600 was not a viable commercial proposition, that you needed a broader product range than that. It taught us that you needed a time-division multiplexer to go along with it, because many customers wanted to carry more than one channel of data, so in fact, there was a great deal of learning experience from the AE-96, even though it did not prove to be a commercial success. Certainly, we couldn't have done the C Series without that base. We had learned a lot just about working with the telephone company and getting on lines, and all of that.

At the same time, in the background, Bob Gallager had been working on double-sideband ideas, the so-called Modem X. He had realized in general terms that by proper placement of the signal points, further apart in the phase direction than in the amplitude direction, that you could get increased immunity against phase jitter, and that was something that you could get inherently in the signal constellation. I came along and came up with the specific signal structures that we actually used, the eight-point structure for 4800 and also for 7200, and 16 points for 9600, which exploited that. The key patent that all of this, on our side, that all of this litigation was about was the patent on the 16-point signal structure, which was Forney and Gallager. That also became the international standard, as you know.

Pelkey: At that point in time, was there any moment of "Aha!" or was it more cumulatively lots of things happening that led to it?

Forney: Well, let's see. I specifically remember this meeting where Art Carr said: “We really need something at 4800,” and going away from that I had an “Aha! I can see how to build a very specific modulation system based on the general Modem X theory that would be a very nice 4800.” So that was a real kick-start.

Pelkey: Had a prototype been built of Modem X?

Forney: Oh, no, it was just pencil and paper stuff, which was the way I preferred to work and Gallager preferred to work, but we fully understood what it entailed.

Actually, initially, I put in a different 8-point signal structure, which was unbalanced, asymmetrical. Gallager shook his head and said it was ugly. It was there specifically for the purpose of getting an analog carrier that you could track at the receiver with an analog phase-locked loop. I knew, in principle, that you could have a digital phase tracker based on the actual decision output of the modem, but I didn't trust it. About three months into the program, about in May, I remember all of a sudden being converted. I don't remember exactly why, but Gallager had been picking on me all the while, and I said: “OK, we'll do it the digital way, convert it to this nice symmetrical eight-point signal structure, put in decision-directed phase-tracking -- it worked much better, actually -- and this was all -- the whole modulation scheme was ripped out, reengineered, a new one put in in about a week, as I remember it. That was how we did things in those days, and it worked fine from the very beginning.

Further out, I don't remember a real “Aha” about the 16-point structure that all the controversy was about, which really was the key to making a reliable 9600 bit per second modem. It was a matter of evaluating a number of signal structures. I happened to come up with that one. Gallager had come up with some others. I remember there was one with one point in the middle and then 5 points and then 10 points in two succeeding rings. There were various other arrangements, and we did some theoretical analysis. Finally we decided that the one that we used was probably the best from an all-around point of view, so that's the one we used.

Pelkey: When did you come to that conclusion, about the 16 points?

Forney: I think that was in the summer of '70. We were already playing with 16-point extensions of the 4800 b/s modem. Again, we have the documents on all of this in the litigation.

Pelkey: Was there a sense of satisfaction at this point in time on your part?

Forney: Oh, no. It was depressing times. We'd had the deaths of the two founders. We had business drying up, the company clearly in extremis. I did not know that it was being shopped around to other companies, but we laid off half the people. We went from 200 people to under 100 people. There was no manufacturing going on. I mean, I was a stockholder: I knew the financial situation of the company, roughly, and it was time for stoicism and hard work.

We had a group of engineers that we picked for the C Series modem development. I can almost remember the PCA number, but I can't. We got on a schedule where -- I remember Ken Jones saying: “Jesus, I know we're going to lose all of our weekends on this,” and I said: “No, I don't believe in that. We'll work every other Saturday, that's it,” but it was very intense survival-type hard work. It felt like we were in a survival situation. It was not at all a feeling of exhilaration. It was “Let's give it everything we've got, and see if there's some light at the end of the tunnel.”

Pelkey: What kept you going at that point?

Forney: Well, that's a good question. I can't say I ever thought about any alternatives, really. I had the bit in my teeth with regard to this product. I was going to see it through until somebody turned out the lights. Part of it, I think, was that I was really upset about Arthur's dying. This was one of the ways that I dealt with that, just by working hard. I thought it was a good idea, obviously, and the connection between what I was working on -- what we were working on -- and the survival of the company was very clear. We've got -- this company's not going to survive unless we can get out a commercial product that we can sell, that we can make some dollars. Therefore, it has to be reliable, it has to be producible in volume, and it has to be yesterday if possible.

Pelkey: You'd come a long way in five years from joining right out of MIT and doing that government research to being confronted with these realities.

Forney: I suppose I probably confronted it in a different way than most people do because I'm highly analytical and theoretical by nature.

Again, when we got into this litigation, I looked at, for instance, what the Milgo people did to design a modem. They put very little on pencil and paper. It was sort of a rough idea of what it should be like, and then build up something, and try it out down in the lab, and tweak some of the components, and see if you can make the oscilloscope pattern look better. That was the old-time analog way of design.

My method of working was very different, and it turns out it only works in a few areas of engineering. It was to work everything out on pencil and paper, as I said about Modem X, to work out the equations, the algorithms, to make an analysis of how well this would perform, what would be the optimum design, to do a complete design in TTL logic using the TI Data Book in a notebook, and then have Julian Loui and other people build it and debug it. The most amazing thing about modems is that you can do that and it will work. The telephone line is a well enough understood and controlled channel, once you understand about phase jitter -- that that was something you had to take into account in evaluating things -- that if you make a pencil-and-paper design and you build it the way you designed it, and you put it on a telephone line, it will work that way.

The same was true of coding for satellite communications, where it's basically a pure Gaussian noise channel, nothing else. I have since discovered, as my scope has broadened, that there are areas like speech coding or computer networking where this isn't the way it work at all.

Pelkey: It's very different.

Forney: I remember when Bob Gallager finally washed his hands of speech coding, he said: "You know Dave, this is an experimental science," and that's true of most things, but it didn't happen to be true of coding for space communications or for modems, and that's -- the kind of approach that Gallager and I brought was very successful. Holsinger really worked in the same way as well, although he was more experimental in flavor, but he was heavily analytical.

Pelkey: This branch of data communications, modems, if you will, was really a mathematical -- applied mathematics.

Forney: As practiced by us, yes. If you went into Milgo, no. If you went into AT&T Bell Labs, somewhere in between, they had theorists and implementers.

What made us successful was the amenability of the problem to this kind of approach, the readiness of the marketplace and everything else, the very fortuitous advent of digital circuitry that had enough power that you could truly build multipliers out of. Nowadays, you buy them on a chip. Then, in a reasonable amount of board area, you could actually build a 16x16 multiplier that worked fast enough for 2400 baud, 2400 symbols per second. It certainly wouldn't have worked at megabits, or what have you. So the technology, the market, was there, and we came along with a solution which I do believe could only have been discovered by thinking about it, by analytical kinds of means. It was very fortuitous that we had the base of the AE-96 experience, we had an organization that was attuned to try to be successful in modems, it had the salespeople -- a few key ones in place -- that had already talked to customers, so all of the preconditions were there.

So on the one hand, we had more adversity hit us one year than I know of ever happening to any other company. On the other hand, we had this terribly favorable confluence of circumstances such that, by God, we did have the basis of an excellent solution. You ask about exhilaration; I don't think I ever had any doubt it would work as I designed it. The only issue was how quickly could we get it to market.

Pelkey: That refrain, by the way, is very common. It's finding that right combination of knowing the need and having the breakthrough technology and having whatever it is at that point in time -- the drive, the motivation -- to do something about it, to put those things together.

Forney: Well, in line with your exploration of how new industries get started, a couple of comments.

First of all, Art and other people have said that if we weren't out of business in the military area we could never have made the transition from military to commercial. It takes being in survival mode, because the cultures are so different. I think there's probably a lot of truth in that.

On the matching of the technology with the market, I think another key reason for success is that we were small enough so that the trading off of what the technology could do versus what the market needed could be done in one head, or at most in a very few heads, and that's why people like Bell Labs, who were certainly capable of designing the same things, did not come out with anything remotely comparable for four more years.

At the same time, the reason why people like Milgo, who were small, had a difficult time playing catch-up in this very-high-speed technology, was that the analytical approach was foreign to them. The idea of using complex numbers, I found, was a major stumbling block. That's why, when they first went to adaptive equalization, they went back to single sideband. Sang Whang said (in the litigation): “Basically, we wanted to do phase modulation. We'd always done phase modulation -- phase and amplitude modulation -- but we didn't know how to do complex equalization, we didn't know how to do equalization for double sideband,” which is basically exactly the same as for single sideband, except you use complex arithmetic rather than real arithmetic, but nobody in the whole engineering organization there was comfortable enough with complex numbers that they could see that. That's my understanding of it.

Pelkey: Now, you were also made a VP in 1970.

Forney: Yeah.

Pelkey: Do you remember when?

Forney: This was in the fall, in September. Everything -- it was like the closing on a house -- everything happened all at once. At the end of September, we got a million dollars in additional financing, each of our personal arms was twisted to raid the piggy bank and put as much up as we could. I know I never expected to see the money that I put up again, and now I wish I had put in ten times as much. I honestly felt it was down the drain, but ethically I should scrape up what I could scrape up. I felt I should consider it gone. So, at that time, Art was made President, Jim Executive Vice President, I was Vice President of Research, I think there were a couple of other officers . . .

Tape Side Ends

Forney: . . . and Art, Jim and I were made the inside members of the board. Fulton Rockwell, Jerry Katzin and Jim Higgins, who came in as an investor then, were the outside members of the board. Paul Rugo, house counsel, was the seventh board member, and basically the company was restarted. Art did not formally become President until then. We put a new management team in place and the titles and the board and the financing all in the same --

Pelkey: And that was the end of September.

Forney: This was all September 30th, as I remember.

Pelkey: That was the end of the fiscal year.

Forney: Right.

Pelkey: If we might continue about the lawsuit. Milgo had sued Rixon successfully, and you had shared patents with Bell Labs.

Forney: No.

Pelkey: Oh, you hadn't? Excuse me. This issue of intellectual property, suits hadn't been normal in this industry. Milgo was the only one who had been active in that area, with one against Rixon and then they came after you. What year did they start that off?

Forney: Well, let's go back over the sequence. I don't know whom else you've talked to about this. Have you talked to people from Milgo?

Pelkey: No, I haven't completed that processes. I've only talked to Matt Kenny. I haven't talked to Sang Whang.

Forney: OK, it should be interesting. Milgo was paranoid about AT&T from a long time back; I think from the late '60s when they first got into the modem business. Exactly where this sprang from, I don't know, but this had to do with the forming of IDCMA and everything else. They got themselves a patent lawyer in the late '60s, pretty much fresh out of school, I understand, Stanley R. Jones, who was an aggressive fellow, and he was brought in specifically because he was supposed to -- I think he had been an engineer at AT&T, and he was supposed to be an AT&T killer. Milgo was the first successful 4800 bit per second modem vendor, and they had some early patents which they felt covered that as well as other speeds, and so forth.

In about '69 or '70, Rixon came out with a 4800 that was very similar in design and began to take away sales, and I'm sure someone in Milgo asked the question: “Can't we do anything about these people copying our,” by then, “patented technology?” I think the original Sang Whang patent was issued in '70, if I remember now. How quickly they forget. This was all on the tip of my tongue five years ago. So I think what happened was that Sang's patent issued, and shortly thereafter, they hauled Rixon into court. It developed that, the evidence was in that trial, Rixon had in fact blatantly copied it, that the engineers had been ordered to copy the Milgo design, even when their consciences said: “We don't want to just copy it,” or “we can do better than the Milgo design,” and the Vice President for Engineering, or somebody, said: “Just copy it. I don't want any change at all.” Well, they had a judge out in Kansas and he didn't like the sound of all of this, so he threw the book at Rixon. So he found -- by the by, he found Milgo's patents valid and infringed and ordered damages and so forth. In fact, Rixon seemingly didn't take the suit seriously. They developed very little testimony or evidence, and they didn't put on much of a case. By then I think Rixon had been sold at least once, maybe twice. I don't know who was minding the store, but they didn't put up much of a defense, and the judge was outraged and he threw the book at them. So Milgo's patents were held valid in '75.

In about '73, AT&T began to get the idea that it ought to assert its patents against the modem industry and, quote, clean up the industry. We had initiated discussions with them to try to get the necessary patents to build a Bell 208 compatible modem, and we had been sort of stonewalled by them. We approached it in a very slapdash way. I just stopped into their offices one day when I was in New York and said: “What does it take?” They kind have strung us along, but then they started some serious discussions with us, saying: “Here's our large pile of patents. We feel you must be infringing some of these,” and we got into extensive discussions with them. What wound up happening is that basically neither of us felt that we actually did use any of the other's patents. I believe that Bell, with regard to one of our patents, were sufficiently concerned about it to design it out of their 209, and thereby somewhat delay its market entry in 1974, but the upshot was, basically, where either of us had any concern, we designed it out of our next-generation modems, and so, by '75, we didn't have any issues with one another.

Meanwhile, AT&T sued Milgo, who was then the largest modem manufacturer. Basically, having found Milgo completely intransigent -- I mean, we certainly heard AT&T's side of the story, which was that they would schedule meetings and Milgo wouldn't show up. Basically, under Stanley R. Jones’ guidance, they were telling AT&T to buzz off.

Pelkey: What year was this, do you recall?

Forney: '73, I think; AT&T sued Milgo. When Milgo's patents were allowed in '75 -- I think they were allowed, in September of '75 and either December of '75 or January of '76 – they quickly sued AT&T as a countersuit to AT&T's suit against them. I think that was the main event in their eyes.

Meanwhile, our signal structure patent had issued in '75. Milgo had come out with a 9600 bit per second modem using our signal structure in '72, '73, and had supported it in the discussions that were then going on in the CCITT about the V.29 9600 bit standard. When our patent issued -- you're interested in intellectual property -- the most dangerous time in a company's existence is when it gets issued its first patent. Now, this wasn't our first patent, but when it gets issued, a patent, you look at this thing and say: “Geez, this must be worth something,” and you're very tempted to go out and assert the damn thing.

So we got this patent. Milgo clearly had been using the technology. They weren't the only ones, but what we finally wound up doing, from our side, not really knowing what we should do about this, we wrote Milgo a letter and said: “We have this issued patent, and it seems you've been using it, and we ought to have some discussions about it.”

Well, what we were told later by people at Milgo is that they got this letter and what made a profound impression on them was that it came by certified mail, and so they took a paranoid view of this letter, that it was a prelude to a lawsuit. So since they were preparing to sue AT&T anyway, they simultaneously decided to sue us, A) offensively on their patents and, B) in a defensive suit against our patent. They actually sued a customer of ours out in Kansas, Yellow Freight, and wanted the court to find that, in fact, our patent was not valid or not infringed or both, saying that we had been asserting the patent against them.

I think it was very much an afterthought that they swept us into this litigation. Their concern was almost entirely for AT&T, but secondarily, yeah, they had a problem with Codex's patent on the signal structure. So, it is interesting how these things start. In retrospect, it was a great mistake for Milgo to sue us. They had lots of hooks in AT&T and they hassled them to death. We were, it turned out, a much more formidable foe for them. They actually, ultimately, settled with AT&T. Whereas AT&T had, I thought, a great preponderance of stuff against them and their patents.

The key weak link in Milgo’s case was that their patents, in fact, were not very good. The first judge, in the Rixon case, had never really considered their technical merit, both because Rixon hadn't really attacked it very effectively and because he was so outraged at the copying behavior that he was just interested in hanging somebody's hide to the wall. When we got into the patents, we found that if read broadly, they really purported to cover Nyquist, which was only about 50 years old at that time. In fact, Sang Whang and others only had a most nebulous idea about what they really did cover. Good old Stanley R. Jones asserted terribly broad coverage for these things, which really would have covered Nyquist, and with that breadth, which is the way they claimed it was to be read, they were clearly invalid, which is what our judge finally found. He threw one of their patents out on summary judgment, and the other two – well, he found there had been fraud in the Patent Office and fraud -- they were lying -- in his courtroom. He ordered attorney's fees for us as well as everything else, which are extremely unusual in patent litigation, based on bad behavior by Milgo and some of their people.

Pelkey: Was this settled in '84?

Forney: I'm having a harder time with recent events. I don't remember, actually, when the judgment came down. It was well before '84. The final settlement between Codex and Racal was towards '84, but this would be the Massachusetts trial. We had it moved from Kansas to Massachusetts. The trial was in the late '70s. I think the judgment was in '80 or '81 or so, and then, of course, they appealed it. Exactly where it was in that process, I don't know.

The bigger swinger was that we countersued them in Florida on our patent. That suit never got resolved because they made allegations that we defrauded the patent office. I heard that one judge once said: “You know, I used to be bothered about all these cries of fraud in patent trials, but then I found that the first thing patent lawyers say to each other, before they even say ‘hello,’ is they say ‘fraud,’ so I don't worry about it so much anymore.” Anyway, the first thing that came back was a laundry list of charges, including fraud on the patent office, that we hadn't properly disclosed this and that, so we elected to go back into reissue proceedings in the patent office, which are still going on to this day in the US.

Along the way, copying AT&T-- they asserted their patents against the parent company – we decided why don't we do that too? So with Racal, in England, we said: “Gee, that's a good idea. So we filed this suit in England, really our main motivation being we felt that nobody was minding the store, with regard to Stanley R. Jones, who was a loose cannon and not -- I hope you use judgment in editing all of this -- but I didn't have high opinion of Stanley R. Jones. Basically, Sang Whang and Jones were running the litigation. Ed Bleckner seemed sort of content to let them run it. Have you talked to Bleckner, did you say?

Pelkey: No. He's agreed to sit with me, but I haven't done that yet.

Forney: I'd be interested to get his view of this. I tried to talk to him. I had a dinner with him once. I said: “You know, this all was a great mistake. We ought to just walk away from it and wash our hands.” He basically whiffled and waffled and said: “Our legal advice is that we have a very strong position and that we are going to come out ahead in this thing. I'd like to do that. This whole thing is a colossal waste of time, but my bosses in England won't permit me. They think we're going to win this thing.” So I came away from that with a view that we really had to get the attention of the folks in England to have them look into the merits, because we felt that if they really educated themselves on the merits, they'd realize that they were standing on a house of cards. So that was really the motivation to sue them in England, just to bang the donkey over the head with a 2x4 to get its attention.

In fact, that was the suit that was heard first. I was really impressed with British law. It was heard in a very timely way. The judge found for us right down the line. It was appealed right up to the House of Lords. It went up much more expeditiously than in any American courts. The judge, of course, in our view, came to a completely correct judgment, that our patent was valid, infringed, and then we surprised him. In England you have your choice of asking for damages or profits, and nobody ever asks for lost profits because by the time the accountants have figured them up, they've disappeared, so people ask for royalties or damages of something like that. Well, we asked for profits because we knew what a profitable business the 9600 modem business was, so he said: “OK, for what they're worth, you can have their profits.” They of course were substantial. Racal was the principal modem supplier in England and, through England, to Europe, and we had good estimates of their volumes, and the 9600 is the most profitable end of the modem business. So that was the principal element that went into the settlement that, I think, did occur in about '84, which was on the order of $8 million to us.

I think I'm beginning to ramble. What point about intellectual property did you want to get to?

Pelkey: That patent that you submitted to have reissued has not yet been reissued?

Forney: It has not been reissued, no.

Pelkey: Which is ironic, given --

Forney: It was a 1970 invention that we filed for in '71 and it still is tied up in the US patent office.

Pelkey: The patent issue in this country is a really serious issue. Anyway, I think you have helped me. I have more research to do in that area. Let me go back to your getting involved with Rockwell. That was another very important part of the company's history.

Forney: It certainly was.

Pelkey: Rockwell came to your attention because of investigating American Data Systems (ADS). I was advised of another one of these folklore stories about a meeting that you and Jim and Art had at the Disneyland Hotel before meeting Rockwell.

Forney: I can corroborate that, about "tell him 10,000 units."

Pelkey: (unintelligible) they wouldn't do the deal if he wouldn't commit to five, so he decided to go ten just to be safe.

Forney: Never imagining in a million years that we could ever sell that many modems.

Pelkey: Whatever possessed you at that moment in time, that breakfast --

Forney: Oh, that was Art. That was just Art being ballsy.

Pelkey: I also have been told the story about a particular meeting - - I don't know if you were there -- but John Pugh had made a claim that you were going to sell 495, or whatever, 9600 baud modems, and people laughed silly, that you could never --

Forney: This was back around '70, or so. It was the original product plan for the 9600. Well, based on the AE-96 experience, it was a very optimistic estimate, but I'm sure you've read about the initial estimates IBM made about the total number of computers that ever would be sold. Same story.

Pelkey: So you started to deal with Rockwell, and I gather they would have done the deal with you --

Forney: We just had a celebration in the cafeteria a couple of months ago commemorating the shipment of our 100,000th of our current line of modems, which was over about a three-year period.

Pelkey: As I understand, Rockwell might have done this deal even if you hadn't committed to any because they just needed the technology -- they needed a systems house.

Forney: Well, I don't know. That was their interest. They had gotten involved in modems both through ADS and they had their own research efforts and some capable people there. They looked around and I think came to the conclusion that Codex was the only one who had a reliable 9600, and so they were interested in the chip market, which I think they saw and, in fact, practiced primarily as -- it turned out to be a facsimile market, mostly in Japan. So yeah, they did want access to the best technology, and felt that we were very complementary. We were small, presumably non-threatening, had the good technology, solely interested in commercial boxes -- their interest was in OEM chips.

Pelkey: Did you know, at that point in time, that they were interested in the fax business?

Forney: I think they already had lower-speed fax modems then. I'm not sure whether I personally was aware of it.

Pelkey: But coming out with the L Series then became a very significant event. If I understand correctly, there was a fear within Codex that Milgo was going to beat you to the punch with an LSI modem.

Forney: Yeah, well, we still felt like the small guy. Part of what underlay this breakfast meeting was we thought it was going to be very difficult to get a semiconductor manufacturer's attention, because the volumes were so small. In fact, our annual shipments rates were at first hundreds and then maybe thousands, but that's nothing for a semiconductor manufacturer. Very few of them were taking the value-added approach that Rockwell ultimately took. I don't know if they had fully developed it then, but if they had a piece of technology that was worth $600, they sold it for $600, even if they could build the chips for $25. So that's the kind of business they got into, which was very profitable for them. Our understanding was that the modem chips were the profitable part of their semiconductor business, period -- or maybe the most profitable. From our point of view, we felt that, given our size, given our volumes, we would have a hard time getting anybody to do a custom LSI chip for us.

I've lost the thread of what your question was.

Pelkey: When you came out with the L Series, you were fearful that Milgo was going to beat you.

Forney: We felt that AT&T, Milgo and the other large players would get the stuff onto LSI. When we came to that trade show, we blew them away. Again, in the litigation -- we got internal marketing memos at Milgo, from marketers, that basically said: “Holy smokes, look at what Codex has done here. This is pretty damned impressive. It's only half-rack width and you look inside it and actually it's mostly empty space. They got it all onto three cards. It doesn't make any noise. It doesn't generate any heat. It performs well.” They went through holy shit when that thing came out, but we still thought of ourselves as the little guy struggling to survive in the marketplace.

Pelkey: After that, you were no longer the little guy.

Forney: It's not exactly that uncomplicated, but that certainly turned out to be the product that fueled our -- we went from $1 million in sales in 1970 to the $24 million range in '75. Do you have our early annual reports?

Pelkey: No, I've asked a couple of people, and no one has them.

Forney: I have some somewhere.

Pelkey: That would be very helpful. I've been encouraged to go to your law firm.

Forney: Yeah, Paul Rugo probably has them. That's probably a better source, because you can tell I've gotten very disorganized.

Pelkey: So you got to $25 million in '75.

Forney: And it was the L Series that really fueled our next stage of growth. When Motorola bought us, we were 40 going on 80, and soon well over 100, 200, and that was primarily on the back of the L Series.

Pelkey: From your perspective, why did you sell?

Forney: Why did we sell to Motorola?

I wanted to say something about the L Series, by the way, which is that the business people negotiated a contract that basically was to develop a 4800 and a 9600. We technical people conspired -- it was our push, but with the Rockwell technical people's acquiescence, to put in all kinds of different modes in there. There was a fast start-up mode for multipoint, there were different rate modes, there was a new equalization technique, and so forth. In fact, what the business people conceived of was simply taking our 48 and 9600s that were already on the market and reducing it to silicon. It became a major new technology advance. There was a hell of a lot of new technology incorporated in there, which was -- we sort of kept this hidden from the business people, probably on both sides, but I believe it was one of the reasons why we stayed out well ahead of the market. It wasn't just that the L Series was small and low power and so forth. It was that it was much improved technology: better equalization, better tracking, new modes, everything else. It would have been a grave mistake simply to take the C Series and put it on silicon. Then the product family would never have lasted as long as it did.

Pelkey: Thank you for sharing that.

Forney: OK, why did Motorola acquire us? I'm giving you an absolutely straight answer on all of these things. I guess I really do want to make use of the privilege of reviewing what I've said because --

Pelkey: I intend to stay in this business and the last thing I want to do is alienate a bunch of you people, so --

Forney: -- because, what I was going to say is that some of us have certainly asked ourselves that question. Not that -- I think it's been a very successful partnership, both for Motorola and for Codex. I'm sure you've heard this from everybody that you talk to. It's one of the best acquisitions that anybody ever pulled off, and it certainly did not adversely affect Codex. Motorola very well managed it.

I'd say there were two principal reasons why we thought we were doing it. One was this small-company paranoia, that increasingly we were going to be competing against the big guys, not our traditional competitors like Milgo, Paradyne was on the screen by then, but the IBMs, the AT&Ts and so forth.

We needed two essential things: one was access to semiconductor technology, which Motorola had, and the other was financial backing, because we felt that we would inevitably be in a negative cash-flow situation. In fact, we ran positive cash flow, and even so, had we been publicly held in the market in the late '70s and the early '80s, there would have been no difficulty in financing, even if we had needed it. So that turned out not to be a necessary reason. For our most recent modem family, the chips are largely manufactured by Motorola, but by that time the semiconductor industry had developed to where we could have done them on our own I'm quite sure, and simply shopped around for the best fabrication facility, so both those turned out to be inessential reasons.

On the other hand, if you ask what harm was done: the investors were cashed in involuntarily before their investment had reached the full flower. That includes you and me and -- half the board became unemployed with regard to Codex. Very little else happened. Motorola, very wisely, left us completely autonomous, on our own, and Codex continued to grow and prosper.

I think the fact that we were now part of a larger company didn't have any immediate effect, but one instant effect it had was that we were no longer able to offer stock options. Over time I think that did reduce the entrepreneurial flavor in the company, through no one's fault. Of course, we were growing, and we had moved to Mansfield, a much larger facility, and we were branching out into stat muxes and other stuff. So, by now, the company is not a small start-up style company anymore. It's a medium-sized company. It's a large company in the traditional data communications world, and that would have probably happened anyway. It's just affected who got the benefit of that investment. Otherwise, not a whole lot of effect, I think.

Pelkey: Jim Storey --

Forney: Did you talk to Jim?

Pelkey: No, I haven't.

Forney: Have you asked to talk to Jim?

Pelkey: Not yet. Building the facility, as I understand, was a personal crusade of Jim's, and many people think that it led to his downfall, in terms of running Codex.

Forney: I don't think that was a primary factor at all.

Pelkey: It's an absolutely incredible facility that any organization should be very proud of, to have created a piece of architecture like that.

Forney: As far as I know, Jim has never been in it.

Pelkey: VanderMey -- were you involved much in the VanderMey process?

Forney: Yeah, I hired Jim.

Pelkey: Did he approach you at a trade show?

Forney: He approached Jim Rothrock at a trade show.

Pelkey: To do a front-end processor.

Forney: To do a front-end processor based on work that he and some of his master’s students at the University of Illinois had done. I went out to look at it. I really liked the guy. I hired him, brought him in -- really, we started to look at what it would take to go into the front-end processor business and realized that it was an order of magnitude beyond the capabilities of Codex -- I'm not just talking about raw technology, probably we could have built one, but the marketing, the support, the necessity of intimately tracking IBM -- all were way beyond our capabilities.

Pelkey: He came in in May of '73?

Forney: Sounds about right.

Pelkey: And for a while he did work on a front-end processor.

Forney: Yes.

Pelkey: Then, I gather, the Intel chip set came out.

Forney: Yeah, he was working on a technology of a Lockheed microprogrammable computer. I think part of what happened was that it became clear that was yesterday's technology as well. He had done some neat things with it at Illinois, but just the passage of two years made it obsolete. Intel had come out with a four-bit microprocessor, and Jim started to get interested in maybe you could program this thing up to be a cheap and dirty mux. That's what we should have done.

Pelkey: If history rewrites itself, yes, although I gather from Jim's interest that --

Forney: Have you talked to Jim?

Pelkey: No.

Forney: He'd be a good guy to talk to.

Pelkey: Do you know where he can be reached?

Forney: No, I have a number that turned out to be his brother's number. Jim is now in Florida. I can give you Dave VanderMey's number. 603-673-3920.

Pelkey: Now, the process of deciding to do a stat mux, is it your recollection that Jim said that when he saw the 4004 that a mux could be done with it? You were doing TDMs at this point in time.

Forney: I'm not clear on whether, from the beginning, it was conceived of as a stat mux. It probably was, because that would be the natural thing. It was a byte-oriented, character-oriented processor. The traffic, as we understood it, was mainly async, and Jim was basically from a computer science type of background, so it would be natural to not treat it as a bit-commutator, which is what a TDM is, but as at least a character-oriented mux, and that very quickly leads you to say: "Why do we send a character when there isn't one there to send? We have the characters framed; we know when they start and when they stop," so I expect that it probably was conceived as a stat mux from the beginning.

Pelkey: Did you guys know Wesley Chu?

Forney: No. Somebody showed me his work fairly early on, after we had started development. We basically worked up stat muxing independently, and then found that Chu had done some papers on it in the early '70s.

Pelkey: And you met with him.

Forney: Did we? I don't really remember that.

Pelkey: He recalls that there was a meeting.

Forney: Maybe there was. Did he -- what was the essence of those meetings?

Pelkey: Just that there were casual meetings and that he was being approached about doing some work with Codex, but it never came to pass. It wasn't a big thing.

Forney: The best I can remember is that I, subsequent to finding out that he had written some papers on statmuxing, I may have run into him at a communications conference and said: "Gee, I've become aware of papers you've written here. We're actually doing some commercial development here. Maybe we should get together," but maybe the reason I don't remember is that I never really seriously pursued it. Maybe he had higher hopes.

Pelkey: Then the 8008 came out, and Jim kind of wanted to deal with that. Then the 6800 came out and --

Forney: Jim was in fact -- what you said -- Codex always wanted to do the neatest thing.

Pelkey: Presumably John Pugh came to you at this point. When the AMD 2900 came out and Jim was ready to jump over to that one, John recalls coming to you and saying: "You've got to get this guy to settle down and get a product out."

Forney: I don't remember that, but probably people were saying that. Jim was very interested in each new microprocessor as it came out. In fact, the 6030, 40, which were the initial pair of products, same basic design, did have both a 2900 based processor in it and multiple 6800 processors. It was a fantastic tour de force in computer architecture, which in retrospect, we undoubtedly aimed too high. Is that what Pugh said?

Pelkey: That's what everybody said. In fact, other people outside Codex said that the analogy could be made between that and the AE-96, in the sense that you had to ship an engineer with every one in order to get the thing to work. It was a big system, a complex product.

Forney: It was an extremely painful development. It was very difficult to debug. We did ship it prematurely, so we had a lot of field problems, and it was very difficult to get it to just be a turn-the-crank reliable product.

Pelkey: Of course the salespeople loved to sell it.

Forney: Not initially. It was a hairy thing, it was a harder sell, and then when a good modem customer found that this thing crashed, or whatever, you didn't want to alienate the customer, so we had several reboots on the sales force. I was head of R&D during that time, and that's one of the reasons that I quit that job was the pain associated with that development.

Pelkey: Steve Finn came in and played a critical role towards the end of it, after Jim left.

Forney: Steve was certainly one of the -- we had an incredible collection of bright people on it. Steve was not one of the early ones, as you say. He brought in Jim Hart and Bill Tao from Illinois, graduate students of his. Bill Tao is the T in Z-Tel; Jim Hart has been involved in smaller companies in New Hampshire. I don't know what ultimately happened to him. Larry Krakauer, a couple of other people; it was probably the most talented group of people that I would ever expect to see, and it took talent of that kind to really bring this thing off.

Talk about not trying to do a new product and a new process at the same time -- this was trying to do about five different things: line protocols, data compression, computer architecture, 6800, backplanes, bus designs; it was incredibly ambitious. The fact is we finally did get it to work. It became our number two product line, put us in a position where we're, just in terms of sales volume, we're one of the top three, let's say, in statmuxing -- all of them are quite close, it would be hard to say who is number one. So, through a lot of pain, it did become the basis for our second main line of business. Overall, I'd say it was a success, but it was several years of pain just to get the thing stable and performing.

Pelkey: Is it your recollection that you were the first out in the market with stat muxes?

Forney: I believe so.

Pelkey: Did DCA beat you?

Forney: We felt we were at the time. Now, DCA was a non-factor then. I remember going down and talking to them and they also had a very early stat mux. If they felt they came out first, it could be true.

Pelkey: What did you think when you saw the Micom box for the first time?

Forney: I think, again, a mistake was made there in that we probably didn't pay that much attention to it. I don't personally remember it. It was clearly aimed at a very different end of the marketplace. It was a low-end utility type of product.

Pelkey: Not your customer or your channel.

Forney: Different channels, sort of aimed at broad user base, almost retail catalog type of sales, so the way we tend to hear about things is when the sales force runs up against them in the marketplace. I think we recognized it as -- technically, they had done a neat thing. I remember talking to Bill Norred. He was very open about what he was doing, and we thought that it was very neat to have this single processor design, cheap and dirty box that did the basic function; thought it would probably have a lot of market appeal. We were so tied up in our own shoelaces doing the high-end product that there wasn't a lot of energy to do out and do what Micom had done.

Pelkey: They obviously took the stat mux and became the darling in the late '70s, in terms of a high growth company.

Forney: And Norred always described his company as a marketing company. He said: “The technology is a dime a dozen. Anybody can do this technology. What we've done is an ace job of marketing.” I think he somewhat underrated the technology, but in fact lots of other people subsequently came out with similar boxes.

Pelkey: Now, late '70s, early '80s, Paradyne got into the business, although they almost went out of business, and then got back into the business in the '76, '78 time frame, and got in through price originally. They were either the first out or close to being the first out with the 14.4, and at some level they started to take the high-end innovation lead.

Forney: Well, that griped the hell out of us at the time, but it was our own fault. I'll give you this article on the history of innovation in modems. I think you'll find at least the first section of it readable. We had enough people that remembered the pain of the AE-96 so that there was -- Art Carr had come out with the moral that you never want to take a product to market that isn't going to go into 100% of the lines in play. The AE-96 had gone onto maybe 80 or 90% of the lines and worked, and the remaining 10 to 20% were eating us alive. I, as the technologist, had always said: “Gee, you need 6 dB more to go with a 14.4. That would put us back, probably, in the marginal category.”

What Paradyne did had essentially zero technical innovation in it. It was based on a 9600-bit modem, which uses 16-point signal structure to send four bits per symbol, 2400 times a second. All Paradyne did was to put in a 64-point signal structure, which can represent six bits per symbol 2400 times per second. Everything else in the modem was the same. The possibility of doing that was old hat and not new to anybody in the industry. All they did was do it. They brought out a product, and what do you know, a great many lines, if not all the lines, could support this 14.4.

Pelkey: Line quality had improved immeasurably since the early '70s.

Forney: Not immeasurably, 'gradually' would be a better word, but it had improved. It's a probability game. The probabilities of getting a line good enough -- this would now be called an uncoded 14.4, and I think even today, we and carriers and so forth would consider uncoded 14.4 a little on the marginal side. Certainly everybody offers, now, a coded 14.4, which provides at least 3 db more margin, but 14.4 has become a reliable, accepted speed.

So they simply went out and did it and, of course, what griped us was that they were able to make a great deal of hay about 'now we're the innovators,' when, from our analytical, theoretical point of view, the innovation had been trivial. From the market point of view, of course, they were the only ones out there with a 14.4, so our Canadian subsidiary, which I'm sure you've heard, already had a design. Unfortunately in the Rockwell chips, we had little flexibility, and that was a real negative about the L Series. Our Canadian subsidiary was also a modem company --

Tape Side Ends

Pelkey: . . .the Canadian subsidiary had a 9600 that was programmable and flexible.

Forney: So within a year they were chartered to get out a 14.4 as soon as possible. They had a great deal of technical expertise. I'd say their 14.4 was more innovative than Paradyne's, and when it got out it performed better, but we lost a crucial year and a half, and on the image side, we had lost there too.

Pelkey: And then at 19.2, did you come out there first?

Forney: At 19.2 I believe we came out first, and to this day, I believe that we are still the only one that's reliable. Now, some others have started to show up using essentially the same technology, and we're getting close enough to the current day that I don't want to go into depth about that.

Pelkey: I'm led to believe that subsequent to your innovation, that Trellis Coded Modulation (TCM) was the next real innovation in the modem business.

Forney: I agree with that.

Pelkey: And that came out of IBM.

Forney: That came out of IBM Research Lab Zurich, Gottfried Ungerboeck.

Pelkey: There have been lots of things in terms of business, but that was the last technological breakthrough in modems.

Forney: Well, there's been a rapid embellishment of TCM.

Ungerboeck actually presented a paper at a conference in Sweden that I would have normally gone to but didn't, back in '76, describing the rudiments of his idea. He gave one or two more talks; I remember him buttonholing me about how this was the way to go, and he was so messianic that I was sort of turned off. Anyway, the industry just didn't pay much attention to it. His paper was finally published in '82.

In '83, the subject of standardization of a dial modem at 9600 became hot in the CCITT, and on January 3rd, 1983, right after the Bell divestiture, the then American Bell International came into the US CCITT meeting with a coded modulation scheme, using a one-dimensional coding scheme, basically, similar to the Ungerboeck one-dimensional schemes, and almost took the place by storm. In fact, at the US meeting, everybody said: “Oh, my God, this sounds wonderful. Thank you, Bell, for showing it to us.”

There was then a meeting about a month later in London, and I just happened to be in London because of the litigation, so I went to the meeting. Bell presented their scheme. They had four people there. The only alternatives were presented by our Canadian cousins who had a block-coded alternative with similar performance -- better in some respects, not as good in others -- and then I got up and said, really, in Ungerboeck's absence -- there was nobody there from IBM – “Ungerboeck's got a whole class of codes that we ought to be considering. Some of them have better characteristics than the Bell scheme here. In addition, they're not patented, which the Bell scheme almost surely is.” (Strictly, IBM only patented one of the whole families of codes that he came up with.) So I almost single-handedly stemmed the tide.

After that meeting, the subject was sent back for further technical study, and there was an intense series of meetings during the year of 1983, and IBM started to participate, and then the whole industry became alive to the potential of Trellis Coded Modulation. Everybody was doing his own computer simulations and design studies and so forth, so it was most dramatic. Up to December 1982, hardly anybody, at least at the commercial level, was looking at TCM. By the end of '83, everybody was looking at TCM. We had been doing some internal work on fairly simple kinds of trellis codes prior to this, but I'd say we weren't really adequately fired up either. It became hot in the CCITT.

Happily, we were in the middle of building our next-generation modem. We were very familiar with the technology. We knew what to do. We picked an appropriate scheme that ultimately was the scheme that CCITT adopted, so we were the first to market with a trellis-coded modem, in '84, very similar to what the ultimate 14.4 was, since it was pretty well known what way it was going to go by that time. So, commercially, we were way out ahead of AT&T and IBM and other people that really understood it, as well as our traditional competitors who were quicker but didn't fully understand it yet -- once again.

Pelkey: That's a common refrain, as well, that issue of when people start product development activity and when there's a shift in the technology. You happened to be in the midst of, and not too far along, in the product development cycle when a new technology came in and had the ability to impact that development cycle, and you had momentum in the cycle, and you got there first.

Forney: Very favorable confluence of developments, but again, we had the prepared mind as well. We had a strong influence in steering what way the CCITT discussion went. I wrote Ungerboeck and told him what was going on, otherwise IBM might never have even participated.

Pelkey: Did Ungerboeck then participate in this?

Forney: Ungerboeck then participated in the whole thing.

Pelkey: Is he still in Zurich?

Forney: Zurich. I just visited him there last summer. He's a good friend, and a very able guy.

Pelkey: Is that important in your history as well?

Forney: Yeah, this was written in late '83, so it would capture some of that.

Pelkey: Good.

Forney: I want to challenge, though, that nothing has happened since TCM, because the scheme that was adopted was a so-called two-dimensional trellis code. Within a year, we had a guy working here named Lee-Fang Wei, who started to investigate multi-dimensional schemes. Again, Bob Gallager, who has done an amazing amount of things for this company over 25 years, had -- we had been looking at simple schemes. He had come up with a simple multi-dimensional scheme. I had come up with an excessively simple scheme. Other people had come up with little things. Bob, happily, has one of the -- I think has THE earliest patent on multi-dimensional codes. Lee-Fang Wei, who is an incredibly productive, hard-working, bright engineer, came up with very implementable multi-dimensional schemes, so a year later, in 1985, we came out with a 19.2 modem based on a multi-dimensional coding scheme that I regard as the next major step forward. It has a couple of important advantages . . .

Interruption in Interview

Forney: . . . and there's continued to be development.

In the past few years, I've gotten heavily technical again, and this has been what I've been working on, coded modulation. I've written at least half a dozen papers by now. The field is, intellectually, just as alive as information theory was 20 or 30 years ago, as modems were 10 or 20 years ago. You know, modems were declared dead, intellectually, in the early '70s, commercially all through the '70s.

Pelkey: Digital Dataphone.

Forney: DDS, right, almost from the time --

Pelkey: There's an Arthur D. Little report that I've got to get my hands on that proclaimed the end of the modem business. I understand it was important --

Forney: Yeah, from 1971, just when we were getting into the modem business, we were told that it only had a limited lifetime. I will tell you something very interesting, which is that since at least 1974; we have spent more of our development money outside the modem area than in the modem area, because our five-year plans always looked the same. Small growth for next year, less growth for the second year, maturing and, depending on our particular mood, one percent up or one percent down, and every year we just moved it out, changed the axes, both axes.

I consider it truly remarkable that there is this intellectual rebirth in the modem area, that trellis coding, it turns out, involves a whole new kind of mathematics. What I've been involved in is the development of a general theory underlying these schemes that work so well -- why do they work so well? It turns out, on the one hand, to be an extension of traditional coding theory -- it's basically a problem of sphere packing in high-dimensional spaces, except the notion of trellis coding puts a whole new spin on it -- and, intellectually, it's one of -- I have absolutely no doubt that when people write, in the year 2000, what was going on in the 1980s in electrical engineering and communications, this will be right up there. For this to have happened again was unexpected and truly remarkable.

Pelkey: Trellis coding certainly had something to do with it, but the extensions beyond it, was there a paper or an event that you would say was seminal in this?

Forney: Well, as far as the multidimensional codes and the laying of the foundation for a general theory, Lee-Fang Wei's work is certainly the most important here. At the same time, two guys at Bell Laboratories, Calderbank and Sloane, wrote a paper which had codes in it -- well, they actually wrote a couple of papers -- with codes that, in many respects, were as good. They missed one key thing that Lee-Fang didn't, so his are actually better for practice.

Pelkey: Did Lee-Fang Wei publish?

Forney: Yeah, he published his paper in 1987. It's unreadable. Actually, what got me started in this technical work was trying to help Lee-Fang with his paper, just to edit it. I became very interested in the underlying ideas because, certainly the way he expressed them was not a way that I could easily understand them, and I wanted to understand what was really going on here, which began to get me involved in the whole theory. There were some very important theoretical notions introduced in this Bell Labs paper that are clearly the right language to be talking about the field in. So these guys made important contributions. I've written a mammoth tutorial/survey/research paper that will be appearing later this year -- it's actually a two-part paper --

Pelkey: In the ACM?

Forney: No, it'll be in the Transactions on Information Theory. That's where most of this stuff appears. There's been an explosion of research work. I'm only mentioning the stuff that's most main line to what we're actually doing.

Pelkey: That paper that you're doing, that part of it that would be a general description of what's happening, would it be possible to get some of that?

Forney: I don't know how (unintelligible) it would be to you. It's primarily mathematical.

Pelkey: Oh, I see.

Forney: I would call some of the early parts in it tutorial, but unless you are familiar with partitions of n-dimensional lattices, you're not going to be too happy, whereas this paper [showing a paper to JLP] I think you'll find a lot more accessible. It starts off with a little history of modem (unintelligible). Have you seen this paper?

Pelkey: No, I have not.

Forney: This one, I think, you'll be able to get through most of it.

Pelkey: Is there anything that, in layman's terms, that would describe this area that you think is exciting and new now, that I could read?

Forney: Ungerboeck wrote a couple of papers, which are probably not exactly what you're looking for, that appeared in the Communications Magazine, aimed at the practicing communications engineer, last year -- February of '87, a two-part paper. Again, I (unintelligible) papers. I think, as a technical tutorial -- I don't remember exactly what was in them -- oh here. As a technical tutorial, they were very good. As something for you, it would be better than my stuff, I think, but it's -- he basically starts: “Let's recollect what is done classically in constellation design and coding…”

Pelkey: I should try to struggle with that a little.

Forney: “… What was I thinking of when I thought of this? I wanted to maximize Euclidean distance, rather than Hamming distance. The first code I came up with was a simple four-state code for eight-phase modulation, and here's how it worked.” So this is the first part. Here's -- well, it's difficult to read, but -- a nice chart showing where each of the types of coded modulation has its application.

OK, in a subsequent issue was this Pahlavan and Holsinger article on the history of modems, which I think you -- that one you really can read.

Then finally, towards the end of Part Two, [Ungerboeck] gets into multi-dimensional modulation. He was a little slow to realize the benefits of multi-dimensional modulation, but by the time of this paper, he had begun to see that what it was about. There have been other refinements, but we're getting down into stuff that excites specialists now.

Pelkey: The concept that it can regenerate itself, and that it reaches a new stage, and how technology change and innovation takes place is obviously intertwined with all of this stuff.

Forney: Well, yeah. If you stand back, here we were, the world's modem experts, and we genuinely believed -- I'll just speak for myself -- genuinely believed that basically it had all been done. Of course, we knew how to build bigger signal constellations, 64 points, but commercially it wasn't viable. From a theoretical point of view, there was really nothing more to do, and in retrospect this is remarkable, because Shannon proved what the ultimate capacity of various channels is. Basically, a telephone line is fairly well modeled as a band-limited, point-to-point Gaussian noise channel, if you forget about the amplitude and phase characteristics, and it's very easy to calculate that, on that sort of a channel, the traditional signal structures are something like 9 db away from channel capacity. They're 6 db away from a figure called R0, which is often taken as a practical theoretical limit on practical capacity --

Pelkey: Which is around 30,000 bits per second or something?

Forney: No, it turns out, in this scheme, for each 3 dB, you only get 2400 more bits per second, so to go from 9600 to 14.4 is 6 dB, to go from 14.4 to 19.2 is another 6 dB, if you keep your bandwidth the same. So, in terms of data rate, being 6 dB away isn't necessarily that bad, but nonetheless, there was this 6 dB gap between conventional techniques, practical capacity, 9 dB to theoretical capacity, and nobody ever filled the gap. Then Ungerboeck basically came along and presented a family of codes, simple ones that got 3 dB easily, and the more complex ones got up to about 6 dB, and just in 'one swell foop,' this gap was basically eliminated. Why hadn't -- people knew what Shannon said. Why hadn't people been actively working to fill that gap? It's remarkable, in retrospect, how complacent we were, and everybody else.

So this book is well conceived, in a sense, because modems in particular are a very clean line of development. It's a sort of relatively self-contained field. You can identify what the milestones are, what it would take to make the next quantum leap, so I think it does make a good paradigm. Maybe not a good paradigm because most other fields aren't so clean. Things proceed in a more messy way.

Pelkey: But the fact is that we get locked into a mindset about constraints.

Forney: Yeah, well that's what happened here at 9600.

Pelkey: That's right. You get complacent and you say: "That's it," and you move on to something else, and companies fund other projects, much like you did, and all of a sudden, somebody else in that cauldron of human spirit all of a sudden comes up with something where they don't have the same mental block, they haven't constructed the same rules, and they look at it differently and boom!

One of the advantages you had at Codex is you had a lot of computer types here. You had Bob Gallager and yourself and so on, information technology and VanderMey with computers. You brought multiple disciplines together, and I think this issue about bringing multiple disciplines together is clearly an important issue, in terms of paradigm shifts and looking at things in a new, fresh way and being challenged differently.

Forney: I'm not sure how important the fact that we had a lot of people working on computer communications here has actually bent the evolution of our modem business. They've been quite separate. I think, as I said earlier, the fact that people like Gallager and myself and Holsinger came to the modem business from a theoretical, analytical perspective, that MIT type of training and background -- Gallager is really a great guy for standing back and saying: "What is the essence of the problem here," and then finding appropriate tools to say: "How, really, should we be signaling?" That's the way he decided that double-sideband was really a better way to go in the first place. When you just stand back, isolate the essence of the problem; it looks like double-sideband would be a better way to go. Then there are umpteen practical problems to making it happen.

The story in the '80s has been much more our ability to react rapidly and in a leapfrog way, having enough of the modem talent, resources and technology base in place so that when Paradyne came out with an encoded 14.4, we were able to be second to market, at least, with a 14.4 that was better than Paradyne's, at least. Again, when Trellis Coded Modulation came along, we were playing with simple schemes in the lab, not really seriously considering it -- well; we were planning to incorporate some form, sort of as a premium mode in the next-generation modem. When it all of a sudden became hot, we were prepared. We understood the implications, we knew the alternatives that we had, we had five different ways we could go, we had the technology base so that we could be first to market, and then we leapfrogged with Wei's work. We were very fortunate to have a really bright guy working in the area. We leapfrogged to the next better scheme.

So it's really happened twice in the '80s, and I think it's a different moral than the initial scheme, but I can just sit here with you and try to isolate the factors. It's a little hard when you're right in the middle of it.

Pelkey: The leased-line and the dial-up companies evolved differently. You had UDS and Vadic, and then you had --

Forney: Well, UDS wasn't particularly dial. They had much stronger dial business than Codex had, but they're basically “we'll build any kind of modem you want, whether it's on a card or a box or whatever,” but you're right, the dial world is a different world.

Pelkey: And Codex got into the dial world and started playing, but didn't come to dominate that as they did leased-line. Was it because the channels of distribution or the technology or what? It didn't seem to me to be a technology issue.

Forney: Well, it's been a variety of things. First of all, as I say, in the L Series, we didn't have a very flexible technology, so that when dial 2400 became hot -- we never even began to think about dial 1200, because that's, EE's felt, low-grade technology. Dial 2400 had to have adaptive equalization. We came out with an adaptation of the L Series, but it was just much too expensive. We sold it back in the '70s to some government customers. Then we had an internal development project, new technology, and had just a variety of problems. It wasn't a great technical success. The product -- I've forgotten what it's particular idiosyncrasies were, but it wasn't a complete technical success, and the distribution was wrong, the sales force that we had wasn't that interested in it.

The world moved very much more rapidly in the dial business, and we have sort of come back to that a couple of times, but basically now, we are dug in at the 9600 end of the dial business, where it's a high-technology game, where the technology is very closely related to the high end of the leased-line business. There I think we're doing much better, but still, we've found it's an almost completely separate culture. As you say, you look at the people who participate in the dial business and it's almost orthogonal to the people who ware in the leased-line business. The methods of distribution are certainly different. The pace of change, the emphasis on features, the types of the features are different. You don't have multiplexing in a dial market. You hardly have network management, but you have very strong features for interacting with a PC, which we've been slow on. Hayes set the standard for the command set.

Pelkey: Good old Hayes.

Forney: Yeah, well --

Pelkey: $200 million modem company.

Forney: its like Norred said: "Technology, schmology. We're a marketing company." Hayes found the market, filled the niche, got all the shelf space long before anybody else was even awake.

Pelkey: Let me ask you a more general question. You've been generous with your time. As I shared with you in the beginning, I have this view that the world is changing from a cognitive science viewpoint. This issue about information strikes me as a very interesting issue. Whereas matter and energy degrade over time, information grows on itself.

Forney: Getting back to my master's thesis.

Pelkey: When there was Pony Express, you fought a battle after the war was over. Because the information flow was so long, you reacted to things very differently. As we have shortened the time between an event happening and knowing about the event, the rate of change of information grows on itself.

Forney: There are certainly facts having to do with the proliferation of scientific journals and the number of articles published and so forth that would tend to support that, and it has to do with the rapidity of interchange in the scientific world between scientists.

Pelkey: And there's a relationship between the amount of information and ambiguity and uncertainty. When you don't know things are happening, you can be much more certain about things. When everything is just happening around you, it really does introduce doubt and uncertainty because there's such a huge information flow.

Forney: Do you realize what a contrary hypothesis that is?

Pelkey: Absolutely. I think I do.

Forney: That's not the standard received wisdom. I guess you said that much earlier today, that people have the feeling that the more information you have, the more certainty you will have -- that certainly would be the conventional wisdom.

Pelkey: And I don't think that's the case.

Forney: The more information, the more uncertainty.

Pelkey: Yes, the more uncertainty, which is paradoxical.

Forney: Well, let me restate it, because I think it would be hard to argue that increased information increases uncertainty. That goes against certain fundamental theorems of information theory. I think what you're really saying is that increased complexity decreases certainty. You talk about the Pony Express world; that's a simple world. The President of the United States could understand most of the things that went on in the United States.

Pelkey: At least he had the illusion that he could.

Forney: Yeah, and at some gut level, he understood farming, he understood banking, and he could probably have an intelligent conversation with most of the people who were actually making things happen in the United States at that time. That's just not possible right now. That's, I think, what you're really talking about.

It's not that -- the information is racing to catch up with the complexity. That's what all of the military command and control is concerned with. Take the battlefield situation, how incredibly complex that's become. No longer can Napoleon move the Blue Army against the Red Army, and military historians write about a battle and it seems fairly simple. Well, I don't think that kind of history can be written anymore, and that's why the military is interested in artificial intelligence and anything you can do to reduce this incredible -- it’s complexity, not information; it's information about the complexity -- to something that they can deal with and make a real-time tactical decision about, and it’s a losing game. They're losing it.

The same thing happens in business. What happened to me -- my scope expanded, first to be head of R&D, then when I came up into the Information Systems Group responsible for Four-Phase as well -- is that all of a sudden, I was supposed to be technical expert in areas just too broad for anybody to really understand deeply, and it happens to everybody. It happens to the president -- John Lockitt, president of Codex. It happens to anybody fairly quickly now. You rapidly get beyond the point where you can really master what it is that you're dealing with. That's the new fact, and all of the information that's going around is just our efforts to deal with that. I don't think you can say the information hurts; it's the complexity that hurts.

Pelkey: This industry that is shortening the time frame between when events happen and knowing about them, and being able to get information about events -- there's a relationship there between the fact of being able to know about it and the fact --

Forney: I agree, that's a separate factor, and you really see that in the investment business: how decision time scales have been pared down to nothing. Everybody around the world, from Tokyo to London to San Francisco, knows the same thing within milliseconds of one another -- trades within milliseconds -- but no one can possibly get an advantage anymore, so the time availability of information, I think you're right, that is a separate factor from the complexity of the world.

Pelkey: But the amount of information in the world is increasing, and it’s increasing because of the nature of the human species. We're an inquisitive information generator.

Forney: And our ability to access it all in real-time has certainly changed dramatically in the last 25 years.

Pelkey: At the same time, because of the complexity that's related to the accessibility of information to understand, that makes it more uncertain.

Forney: Well, would the world be more certain if information traveled more slowly? Again, I think the intermediate step is complexity. It would be less complex. When a US manufacturer only had to worry about US competition and didn't have to worry about what was going on in Japan or Europe, except on a rather longer time scale, the world was less complex for him. Now, somebody can be competing with him tomorrow who's based in South Korea. It's because the world has gotten so interlinked. Again, I think everything factors through the complexity layer. Information is -- I agree, it's supporting the increase in complexity, just the ability -- you couldn't have a worldwide enterprise unless you had rapid communications.

Pelkey: Right. You have a biological metaphor, the idea that at the very beginning, before you even had a cell, all of a sudden things started to cooperate with each other and create cellular structures and nuclei and so on, in which elements would give up something in order to be able to gain something, creating increasingly more complex structures, hence where we sit.

Forney: Entropy degrading all the time, according to the physicists, but it sure doesn't look like it, does it?

Pelkey: It sure doesn't. Now, in the course of that happening, there's a relationship between that biological system, at some intellectual level, and the process of how the reality of what we live in isn't really real until you engage it. That is, you and I can go through a common experience. When they ask you about it and they ask me about it, you can get very different stories, both of them being very real and truthful, but none the less very different. So there is no one absolute out there, and therefore, this issue of information and complexity, there is something inherent in this, because of this personal perspective that one brings to the interaction --

Forney: You're cranking subjectivity into it. Again, I think it's going to be important, when you write this up, to try to distill out the different factors. It may be that in your thinking all of these relate together and you can put that together very elegantly, but I, again, would think that subjectivity is another dimension.

With regard to practically everything that we care about, there is an absolute reality. If there's a meeting that both you and I are in, and we come out with a different report of that meeting, that doesn't mean that, in fact, there's no absolute truth about what went on in there. You could have tape-recorded the meeting, videotaped it, and that would be a record of what people actually said.

The fact that Art Carr remembers one thing and John Pugh remembers another and I remember a third thing, well that's another element, that what we remember is subjective and what we interpret is subjective, which does go to the management problem that you ultimately want to get to, but you're taking awfully fast jumps here, and I will resist some of them along the way. To say there's no absolute reality, sure there's an absolute reality, except at the quantum-mechanical level.

Pelkey: I agree with that point, and I'm struggling very much with trying to understand how to be logical in this process, because it's not clear to me --

Forney: Well, ultimately you're getting to the problem of the poor manager, who ultimately has to make a decision, right?

Pelkey: And the poor organization that's got to try to be successful.

Forney: So you're talking about a number of factors that enter in. A) His world is inherently much more complex, has many more technologies, competitors; B) information goes around rapidly. There are two effects of that: one is that he is flooded with information that he needs to organize in some way; another is that because this information is widely available, he has to react quickly. He doesn't have the luxury of sitting around and analyzing it because the guy in a trading room in another city is making a trade right now while he is thinking about it.

The third element you've identified is subjectivity. All of this information is filtered to him. Most of it is peoples' reports to him of their understanding of things, so it all has this subjective cast to it, and he has his own subjectivity, and so how the hell is the poor guy going to make a judgment in the middle of all of this, right? And your answer to that is: “go with the flow.” I'll be very interested to read your conclusions.

Pelkey: I haven't reached my conclusions yet, actually. The only conclusion I have reached, and there is a slight bias in it, is that ignoring that issue is not the solution. The solution is that as a country, in terms of industrial structure, in order to be competitive in the future and be able to create economic growth as a society, that these information networks, if you will, need to be put in place, and how we got about doing that -- if you look at the last 20 years in this industry, how it has happened, in terms of being very haphazard and chaotic and so on and so forth, we have two-lane dirt roads now, and we need modern highways. How do we, as a society, confront the investment and the policy required in order to be able to do that.

Forney: Well, AT&T and the other carriers will be very happy to hear that, and they'll say: "Leave it to us: ISDN. We will wire up the country and make sure that everybody can talk to everybody with their computers just as easily as they always have with their telephones," and it's very plausible.

Pelkey: All 64K of it.

Forney: That's right. “Anything that can be handled like voice, boy can we do a good job with it.”

Pelkey: Dave, thank you very, very much for your time. It's been very enjoyable.

END OF INTERVIEW

