Interview of Donald Davies

Interviewed by:
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James Pelkey: How did you come to be involved with packet switching?

Donald Davies: I have a very long history in the computer business. So I really hardly have to explain how I came to be talking with Larry Roberts, but it began in 1965, when the International Federation for Information Processing, the IFIP, conference was held in New York. At that time everyone was talking about Project MAC at MIT, and timesharing was all in the news. I'm not sure I met Larry Roberts actually on that visit. I certainly met some of his colleagues at MIT, and we talked, just a little bit, about the communications business, associated with timesharing, but not in great seriousness. On the way back, I happened to meet a friend called Brian Shackel, who suggested that we arrange a meeting at my laboratory, the National Physical Laboratory, so he could get some Project MAC people over to talk to us about this project. We did this. We held a two-day seminar, November 2nd & 3rd of 1965. Larry Roberts was one of the people present, and also a number of other people from MIT. We were talking not so much about communications, but about the concept of timesharing and how it affects the way you do computing and that sort of thing. The communications aspects had interested me, because I had a history of interest in telephone switching and things like that. Nothing very much in detail was said at that meeting about solutions to the problem, but several people said how difficult it was; how you could get hour-long telephone calls in which not very much data was transmitted; what you could do about making data communication more efficient and so on. During the meeting, I went home and thought about this, and it seemed to me that since the actual data rate was very variable, which is to say a few bits would pass over and then there'd be a long pause and so on. And I thought rather than opening a bandwidth for the whole time, it was sensible to treat the information in terms of short messages and simply transmit these by store and forward methods from one place to another. I simply sat down and did a few samples on the back of an envelope, literally, to see how this would work out. My first thought was that if you had to store messages and transmit them, there must be a delay, and this might defeat the whole purpose of the thing, which was to have rapid communications. It soon became obvious that this wasn't so, and that if you chose fairly high data rates, by the standards of the day, and fairly short messages, the storage time was so short that it didn't really affect your message delay. Simple idea. I began to work this out. You can see that in the papers dating from this first one in the 10th of November, which was just about a week after the meeting I'm talking about. You'll see how these ideas developed.

Pelkey: Was there something that preconditioned you to come up with what turned out to be a very elegant solution?

Davies: No, I don't think so, no.

Pelkey: It was just an obvious way to approach it.

Davies: I was interested in how telephones work. I had also been directed into data communications some time back in a classified project where information was being transmitted over a telex line -- and there were problems of error control all mixed up with this. So I had a certain early introduction of the problems of data communication, and incidentally to the problems of security in communication, which was quite accidental. These two, perhaps, just preconditioned me to being interested in the communications aspects of this new type of computing. So I can't think that anything specially guided me to advance the store and forward idea. Again, I had heard of store and forward message machines. Now, how did that happen? I can't quite remember how, but I was aware of the fact that telegraph messages were stored in bulk, usually on a magnetic store, then transmitted as a whole, and I knew that this involved long delays, because message switching tended to operate in minutes or hours. Everybody felt this was not appropriate to the rapid communication required for on-line purposes. I found that these store and forward delays were really quite small and irrelevant, could be quite irrelevant, that I decided it was the right way to go.

Pelkey: Was there any kind of: "Ah, hah!" at that moment?

Davies: Yes, certainly. I sort of thought that this was really quite something, quite a basic idea, and it's going to lead to something quite big. I realized that at that time.

Pelkey: Where were you at that time?

Davies: Sitting at home.

Pelkey: Was it at night?

Davies: Yes, that's right, I was at the dining table, actually, because I hadn't got an office at home. I suddenly thought: "This is a really very important thing, and I must spend a lot of time trying to convince the Post Office," who are our telecommunications authority, our AT&T. I knew a lot of people in the Post Office, and I had a lot of contacts with them, and my first thought was that: "I've got to be very careful here because, if we start research work at the National Physical Laboratory in the communications area, and then I go to the Post Office and say 'this is what I'm doing,' I shall be told to stop." Because the government has a feeling that it's appropriate to do this kind of research here, and this kind of research here. So I thought that what I must do is get my ideas really straight, and have a thoroughly convincing argument before I go public at all. I mustn't immediately spread my ideas around because I shall get told: "It's not your job, it's our job to do that," because I'm working for the government. There's this feeling of empires over particular subjects. In fact, I didn't need to worry too much because only a year later I was talking to the Post Office and found them very receptive of this whole idea, but at the time I was quite worried. Also, I couldn't start a major research project in my laboratory without a certain amount of paperwork -- budgeting and so on -- so I had to move a little bit carefully. But I was aware, right from that very first time, which was in November 1965, that there was something quite important here. A major new method of communications could develop. I must say, looking back on it, I think it didn't develop very well, and I think you'll see it could have been handled better, but at the time, I was extremely enthusiastic.

Pelkey: Did you talk to others?

Davies: Not very much, because I felt that I had to get these ideas really straight. I guess it must have been maybe later that year that I finally talked to people in my laboratory, but not outside. I think the first person that I talked about it in much detail was to probably my colleague Derek Barber, who I knew to be very interested in communications and was a member of my division. At that time, by the way, I was the deputy superintendent of the division dealing with computer science, so I had a certain amount of ability to start new work, but I didn't have a very free hand. Next year, in the middle of '66, I became the superintendent after the retirement of the previous superintendent. At that point, I had a lot more freedom to do what I wanted. So work really began, as a concerted research project with several people working on it. Up to that point, I had gotten people interested, but I couldn't set up a project and start on the hard work. So certainly there was an "Ah hah!" sort of feature about that. I was convinced that it was something very important. In fact, I think I told somebody who didn't really know much about the subject that I thought it was very important and I pointed out to him some communications equipment and said: "Maybe in ten years time, this will all be quite different." So I had some concept of what it was. Oh, Dick Mills, I mentioned someone at MIT. I met Dick Mills at MIT and talked to him about it.

Pelkey: Was this at MIT?

Davies: At MIT itself. I had previously been at MIT as a fellow and was a bit familiar with people there. I talked to Richard Mills about this, rather than Larry Roberts. I did go out to Lincoln Labs, but that was to look at something else.

Pelkey: At their host-to-host connection?

Davies: No, that hadn't been worked out. I don't think -- you see, Larry hadn't moved out to Lincoln Labs at that time.

Pelkey: I'll have to check.

Davies: I'm not sure, but certainly I didn't talk to the people who worked on the host-to-host connection, and I have a feeling that came a little later, but I'm not sure.

Pelkey: Did you know Wesley Clark?

Davies: No. Later on, when I went back to talk about this in the USA, I met some people in Raytheon who were extremely keen, and there was another man who went on to become an important civil servant in communications in Canada. So I made a number of contacts in industry, but not at that time. By the way, right from the beginning, I was thinking in terms of one and a half megabit communication speeds, because I knew that this would become the basic channel for digital transmission, and I was convinced that modems on the telephone lines and so on was completely out of date, and would very soon be overtaken by digital transmission. Now, in that I had the support of the Post Office people that I knew, except, what they said was that you won't see digital transmission outside the local network, what we call the short trunks of a few miles, like the ones that connect the exchanges in America. They said we won't go much beyond that because of the expense. They thought that around the year 2000, you would be seeing long distance digital transmission. I said: "Well, I think you'll see it a little earlier than that." But anyway, as long as they had one and a half megabit lines, which ran over ordinary telephone pairs, it seems to me that these would be ideal for the purpose. All my calculations were on that basis, and then I had to work out what rate of packet handling you could get from a small computer. Now I think we were very optimistic, and we did think in terms of special hardware. We never believed that you should just buy ordinary minicomputers and program them and that would be your switching center. It's always seemed to me that since telephone switches were highly specialized mechanisms, so data switches should be. So right from the beginning, as you'll see from these papers, we envisioned special hardware which would do all the input and output and so on, and therefore the packet switching would only be concerned with moving pointers around. A pointer to a packet would be assembled in store and be ready for transmission, then it would be moved. So we had envisaged a rather specialized type of hardware and that, to some extent, justified our optimism, though I still think we were over optimistic. In fact, I did some sums on the cost of the packet, of switching it; it came to something extremely low, as you'll see in here. All this I think has now been borne out, but in a different world, in which the specialized hardware is, to some extent, replaced by microprocessors which can do this job so much better. We didn't foresee that, of course, but we were certainly thinking of special hardware.

Pelkey: Are these lectures that you –

Davies: That's right. I produced this lecture, of which you have the text here. The text actually was published in June of '66. I do have somewhere -- I couldn't find it today -- a list of all the people who signed into that lecture. They were all people from Britain, of course, and I think most of them were from industry, companies like STL and Plessey and GEC, the main telephone companies, but I think about half the people were from the Post Office, and it was really quite an enthusiastic lecture. Normally, we get 20 to 50 people in a small room, which just about takes 60 people sitting. When I got there, 120 people had turned up, and this was without any special publicity. It was the 18th of March, 1966.

Pelkey: Was it held in London?

Davies: Yeah, at the National Physical Laboratory. I have an attendance list somewhere, and among those people were all the people who promoted packet switching in the Post Office, which led to their early experiment called EPSS, Experimental Packet Switching Service. EPSS was the British Post Office's first packet switching experiment, and unfortunately, it rather held them back, because they chose some very esoteric protocols, which were completely overtaken by development of X-25 and the use of standard data link protocols like HDLC and so on. So everything that came out of EPSS was very quickly outdated. Also, they set the tariffs rather high, and the users were mainly experimenters, like the universities, so industry and real users like manufacturing companies and banks and so on never took part in EPSS. On the whole, therefore, the early involvement by the Post Office with this experimental switching system held them back, whereas the French didn't go ahead until later, and by that time had been able to take advantage of some of the protocol development that there had been. Also, they put it forward as a firmly commercial deal, with tariffs set rather low on a very long payoff timing to attract customers also.

Pelkey: Cyclades?

Davies: No, Cyclades was a sort of computer network, the French packet switching system, which is called now Transpac. I thought you might have been talking to them.

Pelkey: *Not yet.*

Davies: Cyclades, of course, I had close contact with, but this was the French PTT's, Transpac. It was an early packet switching service, but it was run firmly as a commercial service, guaranteed to continue forever. It wasn't experimental in any sense. They set the tariffs extremely low to attract some customers first. Then the French government, who is very autocratic in these matters, laid down that if France wanted banking networks, they had to use this service, and this way they forced people in, and it took off very well. It was, in fact, extremely successful until, unfortunately, it ran out of steam and they had to throw some customers off . At the time the Minitel -- a telephone display for one's desk -- was introduced, the loading on this packet switching service was so high that it broke down under the load, and they had to throw off some of their customers. That was the only real problem with this French network. Because they came in a little later than the British, and also had built something which was much closer to the original X-25 standard, they were able to take off from there, whereas the British Post Office EPSS -- the reasons that - - it's difficult to analyze it now -- in effect held them back to running an experimental service when they should have launched into a full scale thing. Now they have PSS, which is running quite well too.

Pelkey: Was your lecture a success?

Davies: Oh, yes. I expected a lot of criticism from the Post Office people. Far from it, they were quite enthusiastic, and they all went away and so far as I can make out, this raised their enthusiasm for the subject, but it was quite a number of years before EPSS was launched. At that time, I had quite close contact with the Chief Engineer of the Post Office, Merriman. He was the last chief engineer because the Post Office, first of all, was broken up into a separate telephone service, and then the chief engineer was replaced by the chief executive in the engineering department. Up to that point, telephone service was very much engineer driven, and the chief engineer was a very important man. Well, Merriman was the last chief engineer, and I had quite close contact with him, and he was fairly interested. But, I found that whenever they went away to think about their next moves, he always came back less enthusiastic. I think that within the Post Office there was a very strong body of people who were running the telephone service, which was making more money, so they could easily show that packet switching, or any other kind of data communication, was not going to make much money for a long time. Consequently the commercial arm of the Post Office said: "No, really, we shouldn't put too much effort into this. It's an operation that might not happen." Somehow they were, at the technical level, terribly interested, terribly enthusiastic. They went along. They were among the leaders in driving along CCITT, but they didn't actually launch a working service until after the French, which was a great disappointment to us, because they could have done so. The French got into this later on, perhaps through Cyclades, although the man who ran Cyclades, Louis Pouzin, was never popular with the PTT. Very amusing there, because he and the PTT fought each other furiously, the way the French do, at all political levels, for many years. Then finally, the PTT had a master stroke. They dissolved his team in the French research outfit, IRIA, and brought him into the PTT as a member of the PTT. It didn't stop him being highly critical, but it meant that he was, to some extent, defused by having been absorbed into the PTT. Did you meet Louis Pouzin?

Pelkey: I plan to.

Davies: Oh, you must. He's a great guy.

Pelkey: When did you hook up with North America and Larry Roberts, after –

Davies: After this. This is very difficult to remember, actually, because this paper, which I regard as the first major announcement, I sent to a lot of people, including Larry Roberts, and I went to see Larry Roberts later on when he was in the Pentagon, which must have been well after he was really working on

his project, and on his desk there was this paper, which had been -- as you see it's got a rather poor binding -- it had all fallen apart, it was so roughly glued together, and it was sort of thumb-marked all over. So I'm quite sure that it had a lot to do with the early thinking about ARPA. Larry Roberts said so at the time. I went along with a guy from the National Bureau of Standards -- a guy who was always a rather lively sort of individual, who was always in the middle of controversy -- and we had a sort of interesting three-way conversation in which the guy from NBS felt that the whole business of networks was a waste of time. Herb Grosch. You know about Grosch?

Herb Grosch, he lives in Switzerland now. He was always controversial. He believed "networks were a very good way of spending the taxpayers' money." Larry and I had a very intense conversation about this thing at that time, but I'm afraid I don't remember the date of that meeting. After that, I visited Larry when he first set up Telenet. (Telenet Communications Co.) I sat in on some of the discussions about tariffs, which I found fascinating, because I had been fighting the British Post Office to try to make realistic tariffs. My view was that the cost involved with packet switching networks was almost entirely in the local area, and not like a postal service, there should be a constant single tariff per packet throughout the country, with maybe a small connection charge for ports. Then maybe a small increase to go across the Atlantic, but I didn't think it should be more than a factor of two or three, and you should go around the world for not much more. The Post Office had a lot of worry about this, which finally, when the announced their tariffs for EPSS, they were not that far from what we actually had said. In fact, their transatlantic tariffs were higher than they should be; higher than was really necessary because of the cost of that line. You used the line so efficiently that the cost of going across the Atlantic for a packet is extremely small, but they couldn't quite reach it, but they did the next best thing. There was one other important step in the history, as far as I'm concerned. I think it was '69. CCITT has this working period of four years, then they have a plenary, and that plenary was coming up. So they had their final meetings in '69. When the Post Office got so keen, they sent me, as an expert, to try to tell CCITT about packet switching and ensure that some standards work got started. The data communications committee was called Special Study Group A - always known as Special A. The details are written in here. The chairman was Jimmy Rhodes, who was a British Post Office man, and they held their final meeting, in which they were going to make their resolutions for the plenary. No new technical work could begin, but they could start to work out their plans for the following four-year period. A chap called Warden, I forget his first name but he's an IBMer -- an Englishman, actually, who was working for IBM at the time -- gave a talk on the future of data communication, in which he said, "Don't worry yourself about computer communications. All we want is bandwidth. Give us megabits at low cost; we'll go straight into mainframes. We'll handle all the protocols. Don't you worry about that sort of thing," which I think was practically the opposite of what I was saying, which was that communication networks ought to be adapted to the traffic. I was a bit annoyed about this, and so, I think, was Jimmy Rhodes, so he came up to me during the luncheon, and said: "Look, this chap Warden has had an hour." I think he had a big lecture with lots of slides. "I can't give you as long as that, but I can give you 10 or 20 minutes after lunch. Could you present your point of view?" So I hastily produced some slides, and I gave a lecture to CCITT on packet switching, very early on, though it was an entirely new concept to them. I think it was the Canadian delegate and the Russian delegate, together (which is a rather peculiar combination, decided that something should be done about it, and so they said that they should have a group to consider new data networks. This was the first idea, the first time that CCITT had realized that something other than just modems and telephone networks might be needed, but they caught on very quickly, and so for the next four year period, a new group, called GM-NRD (Groupe Mixte – Nouveau Réseau de données). It was a group which brought people from to separate subgroups of the CCITT structure, because it involved people from two sides. I forget now how, but a special group was set up, and I attended that on behalf of the Post Office. This was before Larry had got involved at all in CCITT business, because he didn't really do that until he was running Telenet. I can't remember the date. So GM-NRD started off by using two documents that are now enshrined in the two recommendations, X-1 and X-2. X-1 lists the facilities that have to be in any data network. I can't remember what X-2 does, but they still exist, anyway, as standards, those recommendations. That was really the whole of the four years work, a very modest result for four years, from '69 to '73. I guess has to be. Nothing was done in detail, but it laid the groundwork for the next four-year period, when X-25 suddenly came on the scene and the French people, together with Larry Roberts, put that together. It was very much a combined effort of the people from Transpac and Larry Roberts.

Pelkey: Now that standard setting *began in*?

Davies: It would be '69 to -- is it a three year or four year?

Pelkey: Four years.

Davies: '69 to '73. You say that was '73? It must have been '77.

Pelkey: '75 or '76.

Davies: It may have been -- it must be tied in with the CCITT. I beg you pardon. It was the '68 to '72 period. NRT began, but they didn't produce X-25. I think work on -- yeah that would be it; '72 to '76.

I must have given you the wrong date. It was late '67 that I spoke to CCITT. I may have given you the wrong date. It was late '67. They were formulating the questions for the '68 to '72 period, and in the '72 to '76 period, X-25 was invented. This was very much -- a certain amount of battle was going on -- but it was very much a combination of Larry Roberts and the French group. We all took part, but they were the driving force.

Pelkey: I want to go back. You recall going over and meeting with Larry once he had been at ARPA because he came to ARPA with the express intention to -- charged by Bob Taylor - put a network in, and he was searching out for ideas on how to create this network.

Davies: We were in contact right from the beginning because when the first contract was placed with BBN -- now, who are the guys there?

Pelkey: Frank Hart.

Davies: Yes, that's right. Have you spoken to him?

Pelkey: He's agreed to sit with me.

Davies: And Bob Kahn is another chap that worked there. They produced their first study for the IMP, and we got a copy of it immediately. So we were in close contact. They sent it to us at once, and we found it extraordinarily close to what we were doing, first of all because they had chosen the same packet length. Initially, my packet length was 50 characters, I think, that was based on what I considered the average length of a message in a timesharing system to be. Then it seemed more sensible to make it a round number in bits and I thought: "What about 1,000 bits?" In fact, immediately after this March ‘66 lecture, a chap called Lewellyn, who I knew, came up to me and said: "Did you know that a chap called Paul Baran had written papers about his work sounding very much like what you were talking about.” So he sent me the papers, including the one based on 10 papers from the Rand Corporation. Incidentally, the ideas, according to someone I know, were not necessarily original to Paul Baran entirely; the task was set to him by his boss. I met his boss later on. I discovered I had known him for some time, and he told me about the early history of this thing. To some extent, the task of doing a store and forward type network was originally formulated by Paul Baran's boss, but Paul Baran and his small team made, I think, a magnificent set of studies in which they anticipated a tremendous number of the things that came later. For example, low cost microwave systems and the security problems of a distributed network. As I read it, though, their main concern was to make a distributed network that was going to survive under heavy attack, and would survive almost any kind of disaster, because it was intended as a military network. There was very much concern with speech. A lot of the reports are concerned with delta modulation which you could use to turn speech into data, and also protocols for transmitting speech over store and forward networks, something which is actually only coming back now, and the concept of a virtual circuit appears in Paul Baran's papers fairly clearly. Now, we were really rather against the virtual circuit, because we believed that a communication network should only concern itself with packets, and that any protocols involved in assembling these packets should be done end-to-end, between the customers themselves. In other words, we wouldn't have had virtual circuits before Layer 4. We would have had

them above, something that hasn't happened. In fact, virtual circuits are virtually enshrined in communication protocols, but not entirely, because, as you know, ISO now has a class of connectionless protocols, merely introduced to allow for the way that local area network operated. So, in a sense, what we proposed has actually come into operation in local area networks, but the main PTT and similar type networks are based around virtual circuits. There are still, I believe, a lot of applications where connectionless protocols are important, and right from the beginning, I had identified an important one which was payment systems. I work on payment systems now, and security of payments systems for banks, and in virtually in every case, the network is working in a connectionless fashion, because there really isn't any point in having an elaborate protocol to set up a connection just to exchange two packets. Of course, as you know, X-25 does have the provisions for a 'fast select' facility to give you some of the properties of connections, of a vary rapid communication, but it's rather a poor compromise in practice, and I don't think it's very much used by anybody. It's just there in the X-25 definition. It's not widely used. Certainly the Cyclades used a connectionless protocol. I know you're talking to Louis Pouzin. If you ask him about connectionless and connection protocols, he'll take the same view that I do, namely that all of this virtual circuit was a great mistake.

Pelkey: In fact, X-25 is a virtual circuit system?

Davies: It is primarily, but it does have the fast select, which is giving you the chance of setting up a call, exchanging a couple of packets fairly easily, and then closing it immediately.

Pelkey: Now, your lecture was in June of '66 as I recall, then someone came up to you after that and told you about Paul's work?

Davies: That's right. I went out and got Paul's papers. I was very struck by the enormous similarity in what we had done. His motivation was different –

Pelkey: It was very voice oriented –

Davies: Yeah, and also it was intended to work in a hostile environment, but the network that we were doing emphasized economy, because we wanted to match the traffic pattern of computer systems. Paul Baran never considered computer systems -- in fact, data communication appears as just one among many things that he was doing, speech was the main purpose. We were right away starting from the concept that computers require small messages, so let's build something that does that, and such things as resilience to breakdowns came as a bonus to us. So our viewpoint was different, but our mechanism was extraordinarily similar.

Pelkey: And he wasn't -- the concept of having computer-based intelligent switching wasn't part of his process either?

Davies: I think it was. I thought that the mechanism that he came up with was, in the end, very close to what we had.

Pelkey: Connecting computers?

Davies: The motivation was different. He was approaching it from a different direction, but we both came to design a similar mechanism and this shows that that concept, the store and forward systems with short messages, is very powerful one. Another reason I thought it was powerful (and this was part of the 'Ah ha' process) is that every time I thought of a new problem, a solution within the system, without having to complicate it in any way, came up in a natural way. For example, you have to deal with questions of routing and questions of congestion and control, and that sort of thing, and every time I came across one of these problems, there was a neat solution without having to complicate the network in any way. Monitoring of the network, dealing with faults, sort of fault monitoring and reporting; the use of a network maintenance center -- all came out extremely neatly, and it seemed to me that, as compared with telephone switching, with which I was very familiar, this was actually working out better. You build a marvelous, highly specialized, telephone network, but around that you have to put the signaling system to

carry the information about where the messages are going. You have to build a whole maintenance network so that the engineers can talk to each other and find out what's happening. You have to build a whole other network, another system for monitoring the amount of traffic, and so on, and all these are separate. Now what had come out nicely in packet switching networks was that the same mechanism, because it handles short messages, that nodes could naturally talk to each other through the system by just making a new virtual circuit. They could very easily set up all these mechanisms to do control, maintenance, within the network concept, and therefore, in my view, and I expressed this very strongly in '68 in the IFIP Congress. In my view, it was a very natural type of switching system that had enormous development possibilities.

Now, as for the way in which it has actually developed, the ARPA network disappointed me very much, because it only used 64-kilobit lines. Within our local area network, we used one-megabit lines. It's a strange thing that -- I think the reason was that -- this was starting in '66 when we began working seriously, but working properly in about '69 or '70, depending on how you define 'working properly.' Our local area network was built around one-megabit lines because we used coaxial cable. We were very lucky, because an earlier project had been to collect experimental data around the whole laboratory for central processing, using a parallel interface, and eight-bit parallel interface with about 10 or 12 wires, and I said: "How can you do this around the whole laboratory? We're going to have messes of coaxial cables, and there will be 12-wire banks of coaxial cables." All these cables were being laid in. They were never used for their original purpose. They were there, so we just turned them into our local area network, so these cables were carrying one megabit. They were actually modulated to one and a half, or 1.2, I can't remember the details, but they were actually carrying one-megabit data streams, using a link protocol that is the so-called one-bit protocol. In a lot of the theoretical work on protocols, this has been used as an example, because it's so simple and neat. It was invented by two people: Scantlebury and Bartlett. Together they invented this protocol, a very, very neat and simple protocol. There have even been people who have written papers to show it didn't work, but in fact we were using it, and everywhere, on every link, in our local area network, this very simple line protocol was being used. Above that, we had a very complex multiplexing scheme, with eight-way multiplexing, and 512 potential terminals. Three levels of 8-way multiplexing. So it had the possibility of 512 terminals, and very soon after it was working properly. About a year after it first worked, we completely rewrote the software, because we realized we were not going very fast. When it was under full steam, it was carrying about a million packets a day, which was rather more than any single ARPA node, which is not surprising, because we had a lot of people around the laboratory, and as you can imagine, local area communications is always more intense than distant, because you've got more of a community of interest.

Pelkey: What kinds of computers?

Davies: Mainly small computers; small machines, minicomputers, PDP- 8s and things were coming into wide use as experimental equipment. Nearly every experiment of any consequence in the laboratory was by then becoming computer controlled, and generating very large masses of data, so we built a local area data network -- we had central computers and so on -- and we even introduced, at one point, special minicomputers for doing data reduction.

Pelkey: I'm confused. You coined the phrase 'packet switching.' Your early work was very important, and that you at NPL had created a one line network –

Davies: It was a local area network, but very complex. It was a very complex network.

Pelkey: My view is that it was kind of a one-node network, and never really was meant to develop into this concept of distributed –

Davies: No, it wasn't, you're quite right, because, you see, this comes back to the very first conversation I had about the project when I became Superintendent in the middle of '66. I called in Derek Barber and I said: "Look, now we can really begin with this project. What are we going to do?" We realized that we could never build an Arpanet. We had no mandate for building a nationwide network, and couldn't imagine who was going to communicate. Not much point in having a network without people wanting to

communicate. We had nothing like ARPA to act as our community. So we said: "If we look at our concept of a packet switching network, all the work we need to do about the wide area network, we can do by simulation, because in fact, we could just throw dummy traffic on the network and see if it worked." What we will do, then, is concentrate on what we called 'the interface computer.' In our concept of a network, we had something that came later to the Arpanet, the terminal IMP, as you probably remember. The terminal IMP was a late introduction. In fact, it was in our original concept as an interface processor. This would be attached to a network of nodes which would likely be the ARPA network. I'm not quite sure, but anyway, the point is that at these nodes, there was an interface computer here that would be the way in which all the various types of terminals would be connected. We realized, for example, that there would be a great variety of different types of terminals. This was a very much more complex device than the network. Whereas we couldn't afford to build one of these networks, we had no mandate to build it, we could do this part by simulation, and there's a lot of simulation work that we published; mainly a colleague of mine, Wyn Price, did that work. We would concentrate our efforts on this interface computer, which became, in modern terms, a local area network. I'm quite certain we had the world's first local area network of the present era, but it was largely unnoticed because the rest of the local area networks hadn't been trumpeted at all at that time. All the emphasis was on Arpanet, which was fine. Arpanet was a tremendous achievement. Really, we didn't make a great deal of publicity.

Pelkey: There was not much press.

Davies: Yeah, we didn't make a lot of publicity out of our work, but we had a file server. The other thing that I said to Derek in this rather important conversation was: "People are not going to use this local area network unless there's something good. Should we put some goodies on it that they can come to us for?" Well, the main thing we found about small computers in those days, was that they had very good processing by the standards of the day, but they were very expensive when they came to adding some storage. It was expensive, because you had to add disk stores, which in those days were great big cabinets that cost you more than the computer itself. So what we said was that what we can do for all our mass of small minicomputers, PDP-8s and things around the laboratory, was provide them with a central storage facility, a file server, using the latest technology. So we built a file server to test this network, the another group in the laboratory quite spontaneously came up with the idea that we could attach to this network a kind of office automation system, consisting of word processing and storage and so on. So they built a thing that they called Scrapbook, which was an office system that was attached to this, so within a few years, it was a very important facility that, really, the whole laboratory became dependent on. It was functioning as a full-blown local area network in a modern sense. We never publicized it partly because it was so important to us we went on using it. It was never published, except for a very few papers like this one, which I think was probably for a conference somewhere. In fact, the only conference of any note on our local area network was given in Novosibirsk, Russia.

Tape Side Ends

Pelkey: You had multiple kinds of devices hooked up to your network, and cables were around the building and –

Davies: Well, it was more than a building. It was the whole site. It was about a kilometer wide with this switching center here. The paper for Novosibirsk, I think, had a map of it. I haven't got it here. No, you see, we already had a special interface that had been developed by Derek Barber and others, which actually became a British standard, 4421, but was never adopted very widely. It was very much like the Centronics interface, and eight-bit parallel interface, and having invented that, we had an easy way of hooking on all kinds of terminals. We got most of the manufacturers in Britain to adopt this British standard interface and attach it to their systems so you just plug them straight into our network. We had a very easy way of doing this. The main problem we had was with computers. There were a whole range of different machines, Modula-1, which you would never have heard of, a British machine, a very good one for its time, was our main machine, but we had a lot of others. All these had to be interfaced to the network, but in order to do that; we had a kind of host-to-host protocol that was very, very crude and simple. Because we regarded the network as highly reliable, we didn't carry out very much in the way of error control from host-to- host. We had an extremely simple host-to-host protocol. The local area network

was itself a packet assembler/dissembler. It did the function for you, so the pad was built in. Plug into this other simple terminals, using the Centronics type interface, actually British 4421, or you could plug in a host using this host-to-host interface, and we could make the whole thing work very neatly. So a commercial company, Scicon, was taken on to develop the file server. In fact, it produced two different models. After the first one, we decided we hadn't gotten the thing quite right, so we did the whole thing all over again. That was a tremendously valuable service to these small computers around the laboratory. We also attached, later on, the main computer services of the laboratory and a number of small machines that were available for data reduction, which you could access through the network. To us, this was a network with attachments. These were not part of the network, they were just attachments; either what you would now call X-25 level, but it was with our peculiar protocol, or pad level, like an X-3 type of a pad.

Pelkey: Coming back, after June '66, do you recall then going to Washington to meet Larry Roberts? He obviously had a copy of your speech.

Davies: Well, he had a copy of this paper –

Pelkey: Do you remember going to MIT subsequent to the June '66 lecture. To the best of your recollection, was there only this one additional meeting with Larry –

Davies: I was certainly in Boston. In fact, I also was at BBN and met Mr. Bolt himself.

Pelkey: This is post '66, but pre '69?

Davies: I'm not sure about that. I think by that time, the Arpanet was pretty much in operation. It must have been probably '71 or '72 that I was with BBN. Of course, the development on protocols was still going on. Incidentally, TCP, you may be interested, it was first conceived, I think, or at least its earlier discussions, took place not far from where I live in Roger Scantlebury's home. We had a party, I forget what the occasion was, but Vint Cerf was there. And Bob Kahn, who were the originators of TCP, and we were having a dinner. My wife was there and I was there and Roger Scantlebury and his wife. I remember the occasion very well, but Bob Kahn, Vint Cerf and Roger Scantlebury disappeared for a long time. They were working out the details of TCP in the hall. Yeah, there is a lot of ferment of where discussion on protocols was going on at this time. Of course, by that time, Arpanet was well –

Pelkey: Was it the dinner conversation that caused them to go –

Davies: I don't know. It just happened to be the right time.

Pelkey: They charged off into the corner.

Davies: That's right. I didn't take part too much myself.

Pelkey: This must have been around - after the ICCC show?

Davies: I don't recall that.

Pelkey: To the best of your memory, there was one meeting with Larry subsequent to your June speech.

Davies: I know there were more, at the time of Telenet, but that was quite a lot later.

Pelkey: I mean in the early –

Davies: Oh, yeah, in the earlier conceptual days, yes, probably just one, but I'm sure we had more than one meeting with BBN, because after the Gatlinburg meeting, which was '67. I think a similar meeting took place every year under the ACM. ACM was organizing it. It gradually changed its name. Gatlinburg was the 'principles of operating systems' or something, because I hadn't really begun to think very much about communications, but gradually it became more communications oriented. There was a series of

these meetings. We gave papers there. ARPA people gave papers there, and we usually took the opportunity to go and visit somebody, but it was not so much Larry himself or to ARPA as the BBN people, who were engaged at the sharp end. We also had contacts with many of the ARPA nodes.

Pelkey: Do you remember talking to Bob Kahn during a period of time when he was putting the document together? They were creating the first IMPs.

Davies: Oh, yes. Always. We had very close contact with Bob Kahn at that time.

Pelkey: BBN was awarded the contract in the end of '68 –

Davies: Awarded it on the basis of a document showing, in principle, the design. It wasn't a detailed design. That document was sent to us, whether by BBN or ARPA I don't know, that we should in fact comment. From then on, during the early development –

Pelkey: '69?

Davies: Yeah, we were certainly in touch with BBN; not only with Bob Kahn, but who was the tall chap with him? McQuillan – he was a very good guy on protocols. He turned out to be one of the leaders in protocols. So we had close contact. I met this guy from Raytheon, and he brought a lot of his friends to his house one evening, and I gave him a long talk about what we were doing, and they were extremely enthusiastic. One of the people there, in particular, kept in touch with me afterwards, and he became the head of the -- a large part of Communications Canada, and promoted the concept of packet switching there. So I think there were other contacts that I had which were influential in the USA, but I'm afraid it's all rather a long time ago and I don't remember all the details.

Pelkey: After you read Paul Baran's papers, when did you first contact Paul Baran?

Davies: That was some time later. I think many years later. We just happened to discover we were both in the same conference. I have a feeling it might have been Atlanta ICCC, and he was there, but it was much later. He'd already left and formed Cabledata, and his interests had moved on, so it was a lot later.

Pelkey: Did you talk about packet switching?

Davies: Not so much talking about this. He had got other interests than I had, and we talked around the communications area, but not very much about this subject. We regarded this, by that time, as old history.

Pelkey: So then the US was actually the one--it's clearer to me now how influential you were, in terms of how these ideas developed--The United States that put it into use.

Davies: Absolutely, the ARPA network was the key thing, because it demonstrated the whole thing working on a large scale. The ICCC meeting in Washington in 1973 was absolutely crucial because, for the first time, you could see a large community of people all working together. That was a watershed, because that was the first time that you had lots of people together, all of who were convinced that it was all going to work and be significant. Before that time, whenever I talked about it, I was always defending it and saying: "Yes, in spite of what you say, it's going to be important." After that time, most people who knew about it had already been convinced that it was a watershed.

Pelkey: Can you share any remembrances of that conference? Was there a lot of energy? Were people excited?

Davies: Oh, it was the first ICCC. There was tremendous enthusiasm. They had a big room full of IMPs mainly, connected to the Arpanet, and people had brought along their equipment that they were using at their own laboratories in the universities, and were talking to their computers back home. A lot of it wasn't working, things were going wrong, but what you did get was the enormous intellectual power that was

there, all these people concentrating on doing things. It wasn't one or two oddballs like Larry Roberts and myself. People were talking about the possibilities. It wasn't just academic anymore. It was real working stuff. So there was a tremendous difference. I think everybody was very much affected by the success of that as a demonstration.

Pelkey: Do you remember any interactions with the people who were involved in it at all?

Davies: Not a lot of detail. I remember having -- who were the people who ran Network –

Pelkey: Jon Postel –

Davies: No, there was a group in Washington, Network Analysis Corporation. Have you met them?

Pelkey: Howard Frank?

Davies: Yeah, they were great guys. They had -- they would make a very nice addition to your story, because they were asked as students at MIT -- I'm not sure whether they were graduate students -- they were asked if they could do something to optimize the layout of the pipelines in the Gulf of Mexico, and they did a bit of mathematics on it, and it saved so many millions of dollars that they said to themselves: "My god, if we can save that many million dollars, why don't we make a few of them?" So they set up Network Analysis Corporation, and they got a more or less standing contract from ARPA. They got quite a large sum of money every year just to do research on computer networking. As I said, we were working on simulation ourselves, so I had some extraordinarily interesting conversations with the Network Analysis people at the ICCC. I remember that very strongly.

Pelkey: Do you remember whom you talked to?

Davies: There were two of them. Howard Frank, that's right.

Pelkey: I've interviewed them and I have heard the whole story.

Davies: He was there at the ICCC, and I was interested very much in the simulation of networks. I think that our work at NPL was a success in one sense; we managed to build a local area network and get a lot of experience. It wasn't a very great influence outside, because people weren't interested in that aspect of it. They were interested in the wide area networks, but we did, in fact, do a lot of simulation, and, in some ways, this was easier. It was easier for us to get results than you could in the ARPA network. Once we built our network, we were stuck, because it had to work all day long, and people wouldn't have changes made, so we couldn't do very much research in local area networking, but we could do very much wider research in wide area networks. We wrote a lot of interesting research papers, in particular on congestion control, which you couldn't actually do once you got a working wide area network like ARPA. You can't just make it congest. Everybody will scream and ask for it to be put back into operation, so in some ways, we were freer to work in the important aspects of flow control and routing and so on than the ARPA people were. Wyn Price led that work, but he had a succession of people working with him, and all of them worked with him for a couple of years, and then were immediately whisked off into industry somewhere. One of the things I can claim to have done is to train most of the people who are taking a lead in data communications in Britain right now. When I got to conferences and heard that so and so is the head of networking for a certain company, he's usually somebody who worked for me on one occasion. In fact, I was talking to one this morning. So, one of the effects of our project was to train a great number of people, who were eventually attracted away by industry who could pay them more. We didn't begrudge this because, in fact, we felt that we were making a very big contribution to the development of practical networking in this country.

Pelkey: You remember meeting with Larry Roberts after he left BBN to go to Telenet?

Davies: Oh, yes.

Pelkey: And you were involved in his efforts, although as you indicated the French were much more actively involved, in the efforts to create the standards –

Davies: That's right. I had got myself in CCITT meetings, and Larry came in. You might not like to put this in the book, but just out of interest, CCITT is a very closed club. Once you're in, it works extremely efficiently. It can produce standards at twice or three times the speed of ISO, but it does so on the basis that people know each other, they know who to listen to and who to ignore, and basically, people get listened to if they have a real commercial interest. Just to take an example -- again this is not for the book -- the French and the British and the Italians were talking about their networking projects and how these would affect standards. Everybody will listen, but no one will take any notice of the Italians because they know they won't actually build this. This is rather cruel, but it's a fact. Everybody -- well, it depends, really, on who has a real commercial interest, and that influences what CCITT does, and it's basically -- AT&T, of course, has a so-called recognized operation -- AT&T was there. STC was there, as the telecommunications authority of Chile. You remember the Allende affair. Now, Larry Roberts came in, rather like a bull, and so they at first wouldn't listen to him. Rather, they listened to him and quietly ignored him, and he was very frustrated, but eventually he found out how things work. It takes a while, and people began to trust him and to realize that he was talking about something that would be built, and gradually he managed to get accepted. But at first, he had a great deal of problems. You can't just burst into a place like that. It was easy for me, because I was always there as a representative of the British Post Office. On the other hand, that, to some extent, stopped me from saying what I wanted to. I had these guys listening to what I said, and I had to align myself with Post Office policy. Larry was his own man there, but he had to work very hard before they listened to him. Eventually, he became fully accepted with the X-25 standard.

Pelkey: It got adopted as a standard very quickly?

Davies: Well, CCITT does work quickly; much faster than ISO, because once they decide something, like X-400 for electronic mail, that was a tremendous effort, but once it's done, it's there. People use it. It's a very effective organization.

Pelkey: During this period of time -- now X-25 is created. The next thing that starts happening is OSI. It started off in IFIP.

Davies: Well, it was Hubert Zimmerman who really pushed that along, but there was one British man who I think invented the term 'open systems' actually, MacGregor or something like that. You probably won't hear of him because he's not a very forceful person, but he was involved a lot in the pressure to get OSI started. I think he invented the term 'open systems,' but Hubert Zimmerman was the real driving force.

Pelkey: What was your involvement in the data communications industry post X-25?

Davies: Well, I think it began earlier, because right from the start, before even EPSS was built, the Post Office placed three studies. One was with ICL, one was with STC and the other was with Plessey, one of the two companies. I think one was supposed to explore packet switching, one was supposed to explore circuit switching, and the other one I'm not quite sure. In fact, they almost all of them turned out to do something they weren't asked to, but it gave us a contact, and particularly with STL -- Standard Telephone Laboratories – they're now, of course, all broken up and they've gone into different kinds of conglomerations. I'm not even sure which is which now, but STL, Harlow, had a contact with us for a long time. On the whole, though, British industry didn't get heavily involved. I think -- as you know, when the Post Office finally decided to build a packet switching system with X-25, they went to Larry Roberts' company, or the one that worked for him, I forget now, it was a company that supplied Telenet. There's a manufacturing company that supplies their nodes, and those were the ones that were brought into Britain, imported, I think, by Plessey, who provided the support, but finally, it was the US equipment that came here to run our network, again because, on the whole, the British communications industry wasn't very interested in it.

Pelkey: What was it about markets in the United States that caused the US -- because when you think -- obviously Soderblom and the Token Ring -- did you know him?

Davies: Oh, yeah, it's very funny because -- I better not put this on the tape. He was trying to defend his patents, and I nearly got involved as an expert witness, but I had to withdraw because I found that the infringer who was being pursued was a great friend of mine.

Pelkey: Anyone else?

Davies: There was a Canadian -- what was his name -- who built an early ring network.

Pelkey: The guys who left AT&T, Newhall and Farmer -

Davies: Yes, but before then there was a Canadian –

Pelkey: There was the Farber-Net.

Davies: Yeah, but it was earlier than that. If you go back to the origins of ring networks, these two guys were quite important. Theirs was very much like the Slotted Ring, like the Cambridge Ring. Somewhere -- you might even find a reference in here, but somewhere in our papers, there will certainly be a reference to this guy, maybe a bit later than this. Farmer and Newhall. Ah yeah, Pearce was the Bell Labs man. This was Farmer and Newhall.

Pelkey: You might have thought Canadian because after they created the ring environment at Bell Labs, they left and went to Canada and created a company outside Toronto.

Davies: And Pearce went off in a different direction.

Pelkey: Right.

Davies: Ok, so it is Farmer and Newhall I was trying to think of. I had assumed they were Canadian. They are the earliest researchers to ring networks.

Pelkey: Although AT&T didn't assert itself.

Davies: No, Pearce was director of research, wasn't he?

Pelkey: Yes.

Davies: So he had quite a lot of influence and wrote several papers on packet switching.

Pelkey: Soderblom, I presume I will have to go back to early work that was done at a bank, and what became the IBM 8100 or something, then the Zurich Ring. MIT's Jerry Saltzer consulted to IBM, and at Zurich, and did an analysis, and I have a copy of his paper on the Cambridge Ring, the slotted ring.

Davies: Be careful about putting this into the book, but the origin of the Cambridge Ring, as far as I remember, was that Maurice Wilkes rang me up one day, and he said they were thinking of doing some work in this area. Could I give him some references or tell him what they might do? I don't know the extent to which I suggested the ring, but I certainly gave him Farmer and Newhall's paper as a reference, so I think the Cambridge Ring I might regard as a derivative of Farmer and Newhall.

Pelkey: It's interesting that AT&T was asked after the war to build modems, and never advanced the state of modems. They built a very, very early local area network and did absolutely nothing with local area networks. If Carterfone hadn't happened, this innovation that happened in the United States which, other than Soderblom and your work, it's hard to look to anything other than standard making where

innovation in this industry to date has come from anywhere but the United States. Some day that may change. Presumably it will.

Davies: I think that the different communications environments are very important. As I said to you, when I first began to think about this, my immediate worry was: "Am I ever going to be allowed to do any work on this" because of the monopoly position of the British Post Office? There was no question of it really becoming significant in Britain, except with their knowledge and approval. As it turned out, they did support it pretty well, but in the USA, it was quite different. I went to the MCI hearing at the FCC and the whole emphasis there was on how could we reduce the monopoly of AT&T? At that time, the FCC was known as the Tower of Jell-O, because it was so easy to persuade, but in fact, it became quite a powerful force for breaking up the monopoly.

Pelkey: Do you know Bernie Strassburg?

Davies: Yes, I met him. Well, he took us in. I was there with a Post Office delegation to look at computer network development, and we went along to the FCC, and he said: "Well, there's something very interesting going on today. Why don't we go down and listen to it?" So we sat in the front row, in especially reserved seats. What I did notice is that one of the commissioners was asleep, but I particularly admired the barristers, the lawyers, who were working for MCI. It seemed to me, I never heard anyone who was a lawyer and who knew so much about communications. They were really hot stuff. They really had got the whole story absolutely perfectly, which is quite extraordinary.

Pelkey: It was a critical development at that point in time. The United States had this market environment that allowed for better solutions to find their way to the market, as opposed to dealing with politics of a PTT, or, in our case, the politics of AT&T prior to –

Davies: The PTTs in Europe fought, and still are fighting to some extent, a kind of battle against the private networks. At that time, the nearest thing to a packet switching network was SITA, a sort of message switching network that the airlines had –

Pelkey: That was a very early network.

Davies: It was used for sending, not operational messages, but messages about delayed flights and things like that. Anyway, it was based in Paris, this network, and the PTTs, in their internal discussions -- I was sometimes let in on these discussions -- were trying to work out how to change the tariff to make that kind of network economic, but making them pay a traffic related tariff, instead of just buying their bandwidth. They hated the idea that someone could buy a leased-line and sell it more efficiently than they could, and they fought furiously to try to prevent it. Now, in my view, that was the wrong attitude. If they built packet switching networks with a good deal more ingenuity and sold them with a good deal more commercial know-how, the way that Larry had to, then I think they might easily have defeated these private networks by simply being able to show how much better they could do it, on the basis that they can bring in so much more traffic, that they could make the thing more economic. They chose the opposite thing, which is to use their monopoly power to prevent these things from happening, while giving people uneconomic tariffs. I think it was the wrong approach. They could have made a real killing in this market by diving into it. The French did the best –

Pelkey: AT&T was offered twice to take over the Arpanet and they said they didn't want it.

Davies: There's another technical point that I would like to make, and that is that right from the start, the concept of packet switching, at least in ARPA network, was still thought of as a private network that you could build using facilities lent to you by some other kind of communications agency. My thinking right from the start was that this would be a public network, run as a service, whether by a private company or by a public owned one is not the important thing, but it would be a service to the whole county, and you wouldn't have lots of separate private networks, because, it seemed to me you could get, in this way, the economies of scale. That meant you had to build packet switches with enormously greater throughputs than anything like ARPA, which is trivial by comparison. If you look at the carrying capacity of the ARPA

network is trivial by standards of public networks. Now that, I think, still could be done. In fact, there are nowadays developments on packet switching with those very high throughputs. Gradually, the concept has been modified, as it has to be with new technology, and is now coming forward under the title of 'asynchronous time division,' but it's still the basic concept, but developed. Now, there's quite a large amount of development going on in AT&T, with very high speed A.T.D. to carry speech –

Pelkey: Fast packet switching.

Davies: You could call it fast packet switching, but I think gradually, the concept will get modified and turned into something else. So I think nowadays the developments are taking place, but they could have taken place a lot earlier. People were far too happy with the ARPA network as a model, using ordinary minicomputers with a very small amount of extra hardware, doing a more or less satisfactory job, as a low throughput packet network. My feeling was that they should have developed, right from the beginning, specialized hardware, and make really big nodes. Certainly they could offer a service that couldn't be bettered by anybody, but then, I think, the PTTs would not have the problem they have now. They wouldn't have had the commercial problem of all the private networks that only pay rental for the lines, rather than for the whole service. If I had been able to influence the PTT in those days, it would have been to go to packet switching with far greater zeal, and grab the traffic away from -- and making it uneconomic for banks to build their own networks and airlines to build their own networks. That seems to me to be a great pity. In the end, of course, because of X-25, these things are nowadays fairly compatible. It's still a problem, though. Banks still tend to have a mixture of OSI and SNA networks, so there is still a big compatibility problem.

Pelkey: I want to go back to Soderblom and Token Ring. You were aware of those kinds of thoughts pretty early on?

Davies: Yes, mainly through Pearce and Turner and Newhall.

Pelkey: Did you follow up on those?

Davies: That's a very interesting point, because it illustrates another, I suppose important, theme for your study. We had heard of Ethernet. We didn't do anything originally in that direction. We thought: "Now, what is the next kind of development from Ethernet we would like to have?" No, sorry, before Ethernet. This was the ALOHAnet in Hawaii. So we thought: "Well, we would like to build a local area network using the ALOHA principals."

Pelkey: ALOHA came before Arpanet?

Davies: Oh, yes, sure. I knew the guy who ran that quite well. Norm Abramson. We heard a lot about ALOHA, and we thought: "That's a brilliant idea. How can we apply it to a local area network?" What we wanted to do was to use a local radio system around the laboratory in order to begin working on the ALOHA principle. In other words, it would have been an Ethernet, but instead of having a wire running around the thing, we would have had a radio system, and to make a radio system, we wanted to use a leaking coaxial cable, which produces a very limited range radio system. When you're close to the coaxial cable, the signal strength falls off 'one over r' or something like that, so you get a very good, moderately uniform field, but when you get to distance which is large, compared with the dimensions of your cable, then it falls off as 'one over r cubed,' and so it falls off quite rapidly. By using low power, you can make a local area network this way, which doesn't interfere with a local area network 10 miles away. So, we decided we'd like to do this. One of my groups at the laboratory was quite keen. Because we were going to use radio, we needed to get permission. The Home Office, I think, was allocating frequencies. We identified a television band from some distant point which wasn't used anywhere in the London area, and we said: "We can use this with however many watts it was, here; they'll never hear us out there. We should get some interference from television channel, but because of our error correction property, we're not worried about that. We might get occasional beeps coming through, but we can correct for that. We won't interfere with them, they won't interfere with us." We were turned down flat. We were told that radio systems, because of the fact that the bandwidth is limited, must only be used either for mobile, where you

couldn't do anything else, or emergency services and since we didn't fall into any of those, we weren't going to have any allocation of bandwidth, and so we gave up. In a sense, we might have homed in on the Ethernet principles –

Pelkey: Very interesting.

Davies: I'm not sure how this appears in time, in relation to Ethernet, so I mustn't claim too much here. Ethernet was certainly, I guessed, inspired in some ways by ALOHA, which had established the basic principle, and we were inspired by ALOHA, but whether we had heard of Ethernet at this time and were trying to do it another way, I'm afraid my memory is too weak on that. It was a disappointment to me, because I thought that this would have been another interesting technology for local area networks. We had to give it up because there was no way of getting the radio band that we needed. We thought a television band was just about right for making a good experiment.

Pelkey: One of the things that struck me is that there is this period of someone being exposed to a new way of getting something done, technolog*ical* innovation, and a need in the marketplace, and there's a recognition in seeing a problem and understanding that there's a new way of solving it, and then putting these things together, and having the personal energy to go on and do something. There are lots of other people who could have done it, but there are very few people who actually do it.

Davies: You have to be fired with some enthusiasm, because you're going to get a lot of opposition. There are a lot of people who tell you: "Oh, yes, that's really old hat. It's been done before and it won't work, and so on." The most frequent reaction I had in the early days was: "That's just message switching. How does it differ from message switching? That's been done in the telegraph networks. It's terribly slow." In fact, I often heard this thing about packet switching: "It's terribly slow, and it's not really going to contribute very much." In other words, people who couldn't see beyond what they already heard about. The other thing was: "It's going to be so complex you won't ever make it work." That was another reaction I had. You've got to have a certain amount of energy and enthusiasm to get over those initial discouragements, which are automatically thrown at you. In our case, I think we didn't really succeed too much in getting the industry in Britain to take this up. The PTT, the Post Office, which it then was, I think did very well, in the sense that a lot of people, about half a dozen really enthusiasts within the PTT, and they were given money and they eventually built this experimental system, and eventually, fairly quickly, they got on to the full scale PSS, but it didn't involve British industry very much. We found this was the hardest point of all. They were involved with us, because they did these studies. We talked to them. At the time I was doing this, I was in charge of a thing called the Advanced Computer Technology Project, in which we had some 20 companies, the whole of the computer industry and the communications industry, including STL. We had lots of contracts with STL, and I was just sort of mini-ARPA project, in which I was dispensing what was in those days a modestly large sum of a few million pounds a year, to all these companies, to advance computer technology. We have several quite important developments to our credit. We came out with associative file store. We were in close contact with industry. It wasn't that we didn't know the guys working in the communications field at all. We talked to them all the time, but somehow it never took off. I think part of the reason was that the communications industry in Britain worked only for the Post Office. Everything was done to a specification written by the Post Office. They knew that within the Post Office, there wasn't enthusiasm at the top. The people who ran the Post Office knew that telephone was their real traffic and that telephone switching was going to stay circuit switching forever, which possibly it will. They never saw packet switching as commercial. I was always being told that the revenue we're going to get from data communication is terribly small. It's never going to approach what we get from our ordinary telephone network, so we concentrate on what matters. People who were making the commercial decisions in the Post Office were very much opposed to it. I think that the guys in industry naturally see that. They wouldn't go out and make a special development of packet switching and hope that the Post Office might come along. There wasn't, also, really, much chance of building private networks. Private networks were closely controlled. It was very restrictive. Modems had to be supplied by the Post Office, and if you built a private network, you weren't allowed to let any of it to provide service to anybody else. That was absolutely forbidden.

Pelkey: A question of another kind, since you ran research facilities. I have great concerns in the United States now that ARPA, when it became DARPA, became much more short-term oriented, and with the breakup of AT&T, Bell Labs another sound source of long-term research in the United States is going to become more short-term oriented and won't be funded the same way it had. That American industry doesn't really do research. The amount of research dollars spent is very small. It's development dollars that they spend.

Davies: I fully agree.

Pelkey: The kinds of research projects, in terms of communications - - building gigabit networks -- these are larger and larger dollar commitments to push on the state of the art –

Davies: It's very worrying. I entirely agree. Major developments, it seems to me, involving a complete change of thinking, hardly ever arise in industry. They very often get taken up by small- scale industry, and rapidly developed, but if you think of the computer itself, IBM was one of the last into computers. It was a punched card company, and was dragged in. The first developments of computers took place in the universities and, in our case, government laboratories. We built one of the earliest machines, but it was not in industry. So ideas have to come from somewhere else. Universities are one source, and in this country, the pressure has been continuously to go shorter and shorter term in their thinking. When I first was working in government research, we were allowed to do almost anything we felt like doing. We had virtually full control over our research programs, but gradually, this became more and more controlled by committees. In fact, I was very lucky in that the committee that was set up to monitor our research was extremely helpful, and always had complete faith in what I was doing. So I got a full endorsement of my programs for most of my time. Since then, communications research in my division has been largely closed down, with one exception, which is that they're still working on the verification of protocols, to verify the protocol in accordance with the standards.

Pelkey: But even just from this conversation this afternoon, there were moments -- the example of wanting to do this radio network - - where were you going to get funding for doing research? If you didn't have freedom as a research manager, and it strikes me that there has to be a certain amount of un-allocated funds that can be used at the discretion of top research managers, which is a problem in and of itself, developing research managers versus development managers.

Davies: The committee structure, that controls research in the National Physical Laboratory, which was called Requirements Boards. It was something like: we were contractors who would do our research at the behest of somebody who knew more about the world than we did, and would tell us what to do. This seemed to me quite wrong, because in fact, the very people who hadn't come up with any earth-shattering ideas, were now being set up as masters of what we could do. We could wriggle out of this to some extent, but where major projects were concerned, these all had to be approved by these Requirements Boards. They had the bright idea that they would actually allocate 10% of the funds to be at the command of the Director of the National Physical Laboratory. Unfortunately, it didn't work out very well. For various reasons, that wasn't free for, really, innovation. It is a real problem. The more expensive it gets, the more they feel it has to be directed, the less likely the possibility of some really bright idea changing the way we think.

Pelkey: I agree. I think this is a major issue. At this point in time, Japanese society, given the way they look at problems, and in England, because it's much more centrally controlled than in the United States, one can presume that there may be a longer term orientation to some of these spending patterns

Davies: Japanese in computer industries seem to be throwing large amounts of money at it. They have successively larger and larger budgets, and as I understood it, in those projects like the Fifth Generation thing, at least part of what they say, they recognize that the major objectives are set so high, they probably won't achieve them, but they think in the course of research, some spin-off will happen, that there will be some sort of accidental discoveries along the route. At least, they're throwing the money and large numbers of people, and there's something to be said for that. In Britain we tend not to have that sort

of money for research, and it tends, now, to be very heavily directed, and consequently, in many cases, I think bright ideas won't pop up . . .

Tape Side Ends

Pelkey: . . . that's an issue for me too. Is it possible to recognize the people who, in fact, have this motivation and ability to be able to see this paradigm shift, to be able to see this new way of looking at the world, versus the people who think they do and they really don't?

Davies: No, I think it would be very hard to say this. Of course, I've left being involved very much with research. Certainly, among people who came to join my division, I could recognize those who were individual thinkers, and make sure that they had a good chance, like not being loaded down with committee work and so on, to develop their ideas. It's fatally easy to turn down an idea because it's unfamiliar, because it looks crazy, I'm afraid. I probably have done the same thing to other people what was being done to me, without knowing it. It is extremely difficult. I think what you do have, though, is to be able to devolve to the level of people who really understand the subject, a chance to have some money to spend on oddball schemes. The guy who is at the top of a large laboratory really can't understand any one individual subject. You've got to somehow be able to devolve down to a fairly low level, the possibility of bringing in some unexpected funds for a project that can't be proved, to go to work. I don't know how you tackle that.

Pelkey: You've thought a lot about communication networks. We talked before the tape was on about private corporate networks and where these networks are going and these being the highways, the information infrastructure of the society of tomorrow. Do you see anything, in terms of the implications of these high-speed networks in the way business is conducted or the way we organize our society? Have you thought about this relative to an information economy versus an industrial economy?

Davies: Oh, yeah, I've thought a lot about that. I was involved in trying to promote the idea of more funds for information technology. Many years ago, there was a superb study by a guy who went off to Vienna -- he was a Viennese originally -- of the information technology, in terms of input/output analysis, which is one of the arguments I used to use for the importance of the information society. I don't know that I've got anything very much that hasn't now been said by a lot of people that I can say on that subject. I think that the early analogies with a power network, sort of the idea of having computer power at a plug in the wall I think was completely fallacious. I never went along with that idea. It seems to me that computer technology is quite different, and that it doesn't have any analogy with power stations at all. In fact, it'll probably move entirely away from that sort of way of thinking. All my recent projects have been involved almost entirely with microcomputers, doing fairly simple tasks themselves, but achieving something more significant by the way they inter-work. So, I think that networks are going to be important there as the glue that holds the whole thing together, but I don't think we very much have a concept of how to do this.

Pelkey: Let me propose something to you, this concept of Von Neumann's view of the world -- that reality exists outside, an organization or an individual, and we just capture enough about it that we can reconstruct reality. For example, in computers, you can have this big mainframe, we can collect a lot of data, we can model that data, and then we can make better decisions from it.

Davies: I'm always of very suspicious about that. When I've been involved in large-scale modeling, I've found it extremely difficult to verify that what you've got as a model is in any way related to the real world outside. It's fatally easy to draw wrong conclusions in this way. I always went by taking the simplest model first, testing it against the real world, and then elaborating a little bit and testing it, sort of making sure we never lost touch with -- what actually happened, this was modeling communication networks. It's true. I did actually start in modeling much earlier. I modeled communication networks in coalmines at one point. Those were the first computers that we built, in about 1950. My first task was simulation of road traffic and communication systems. So, I've been involved in modeling right from the start, and I think it's very, very difficult to make sure that your models aren't losing touch with reality. I'm very suspicious of the idea of making decisions on complex systems by any kind of modeling. Things like Star Wars fill me with horror, from that point of view, and many other things, like economic models of the whole world and

so on are likely to be way out, and unfortunately, you can use them to prove what you want. So I'm very suspicious. I must go now.

Pelkey: Thank you very much for your time.

END OF THE INTERVIEW

