Dr. John Bader Interview

This is an interview of Dr. John Bader, who made important contributions in the Viruses Cancer Area, taken on March 8, 1996. The interviewer is Dr. Carl Baker, former Director of the National Cancer Institute.

Baker: John, we certainly thank you for your willingness to give us a little time and discuss the areas of viruses and cancer, and, before we get to the questions that I sent you, I wonder if you could just give us a little bit of your background, of where you went to school and some of the jobs you've had and that sort of thing?

Bader: Sure, Carl. First, I'm flattered that you've included me in these interviews and I'm pleased to be able to do this. I hope that I can contribute reasonably well to it. I went to college in Rochester at the University of Rochester and continued in the Medical School as a graduate student in the Department of Microbiology. I received a Ph.D.in 1960, and spent the next two years at the California Institute of Technology in Pasadena, California, working with Renato Dulbecco. At that time quantitative measurements in use of Rous sarcoma virus were just becoming career in virology from that time was on Rous sarcoma virus and similar viruses which eventually got to be called retroviruses. After those two years, I came to the National Cancer Institute. I was the first Staff Fellow in the National Cancer Institute, working in the Clinical Center as a Staff Fellow and then I went out to the Chemical Carcinogenesis Area.

Baker: About what year was this?

Bader: This was 1964 when I went into the Chemical Carcinogenesis Area. There was a good job opportunity with people that complemented the work that I was doing in virology that they were doing in biochemistry. And I continued on there for many years, through evolution of experimental work in the laboratory, until 1988 and, at that time, I took over management of the AIDS Antiviral Screening Program for the National Cancer Institute and I've continued there until the present day.

Baker: And your present position?

Bader: My present position is Chief of the Antiviral Evaluations Branch in the Developmental Therapeutics Program of the Division of Cancer Treatment.

Baker: How are we doing in this area these days?

Bader: Well, it's a very exciting area. From the initial demonstration that AZT had clinical usefulness against AIDS, there has been encouragement and hope that you could find drugs that would work against AIDS. In fact, that's exactly the case.

There are a number of compounds--there are hundreds of compounds--which are active *in vitro,* that is in cell culture systems, or against enzymatic systems, against HIV components, and now they've been developed into approved drugs for AIDS. There has just recently been approval by the FDA for a protease inhibitor of HIV which seems to have clinical efficacy and I'm sure, within the next few years, we're going to see many more compounds of this type. And eventually, whether or not we can ever find a cure for AIDS is debatable, but certainly we'll be able to extend peoples' lives that have been infected with HIV and can be expected to develop AIDS.

Baker: Certainly the whole area of therapy is quite different with the introduction of growth factors and cytokines and differentiation factors plus the dissection, I guess, of the various steps in the AIDS virus production where you can attack at different points along that spectrum.

Bader: Well, to me it's amazing that with the developments in molecular biology which really came from the early Cancer Virus Program days that we were able to find out so much about HIV in a very small amount of time.

Baker: Yes. I think the pace of findings too is something different than when I was at NCI, quantitatively a very striking difference. Well, good. Let's turn to the questions. And the first question is who you think were some of the key scientific developers in the area and what those most significant findings were in the period 1950-1980.

Bader: Well, I've made a short list here but, you know, I'm sure that I've missed some very important developments and contributions. But I think the first demonstration that viruses actually could cause cancer that is that viruses cause leukemia and tumors, by Ludwig Gross and Charlotte Friend, Dick Rauscher, John Moloney, I mean that was very important. It was essential to the whole basis of recognizing that viruses could cause cancer. We knew about this in animals, in chickens, years ago that they could cause leukemias from the early 1900s, but to be able to demonstrate this in mammals gave it an impact that it never had before. The association of EB virus with Burkitt's lymphoma I think was very important in suggesting that viruses could cause cancer in humans because I think just the fact that you could demonstrate it in animals wasn't sufficient to attract peoples' attention that this could be important in man. I thought Dulbecco's work in establishing quantitative techniques to work with viruses was a tremendous development because now you didn't have to rely just on animals to be able to work with the virus and do definitive work, and the later demonstration by Wally Rowe and Harry Rubin and their associates that there were such things as

endogenous viruses, that is normal, healthy- looking animals could produce, or were even producing, viruses or virus-like particles, was a very important finding. And, of course, later on this led to what was known about proto-oncogenes and oncogenes in normal human cells. Of course, Bishop and Varmus were probably the most prominent in that, but there were very many people involved in the demonstration that there were such things as genes in the body which, if they were malfunctioning or somehow hurt, in some way could make cells develop into cancer. And the fact that there were genes that were normal genes functioning in those cells could affect tumorigenesis was very important.

Baker: Yes. I relate this to three phases of the field. Before Ludwig Gross' findings most people thought viruses had nothing to do with causing cancer at all, and Gross and Stewart and Eddy, once their work was confirmed, as you say, it changed the whole outlook of the field to look at viruses as causative agents. And then the next change-- phase--in the field was with the oncogenes, which now shifted the attention from the viruses, per se, to the same kind of genetic information in our own chromosomes. And these are real changes in paradigm, to use Thomas Kuhn's terminology here.

Bader: Yes. I think one other feature was the recognition that RNA could be decodified into DNA by the reverse transcription process and, of course, that had great impact not only on molecular biology in general, but it also showed that the information in RNA viruses could become transcribed into DNA and integrate into the cellular genome and that was a phenomenon which had only been seen in bacteria before. And then I guess the one other thing was that RNA viruses could, themselves, encode an oncogenic protein. Ray Erickson initially demonstrated that and some others including Ed Scolnick. There are many other contributions besides these.

Baker: I'll ask you about one other person, Bob Huebner.

Bader: Yes. Well, there are a lot of other people who were involved in this if you want to discuss the people and personalities. Intact, as far as investigators are concerned, we already mentioned a number of these, but to my mind there were specific laboratories that made huge contributions because of the people that they brought through those laboratories, Dulbecco for example. I mean, I was a student of Dulbecco. But also Harry Rubin. And from Harry Rubin's lab, there was Peter Vogt and Hanufusa and Duisburg. Howard Temin came from Dulbecco's lab. Dulbecco's lab was really a very fruitful place in training and developing of scientists in this area. Bob Huebner also. I mean, Bob Huebner was very good in attracting good people, good young people, who then developed into good

scientists-- George Todaro, Ed Skolnick, and many others. Wally Rowe was in his laboratory.

Baker: Fine. That gives a flavor, I think, of some of the main thrusts. I don't know whether you were aware of the key administrative or management decisions that affected the field in this period and who made them?

Bader: Yes, of course. In the Virus Cancer Program, which really supported a great deal of this work, it was Carl Baker, you, Dick Rauscher and John Moloney. This was probably from the Government standpoint, where most of the money came from.

Baker: Two other people we should mention, I think, in conjunction with this is Ken Endicott, who made the key decision to go and ask Congress for a special appropriation, and Bryan, Rauscher and I put together the back-up information that Shannon demanded before he would agree to let Endicott ask for this special appropriation. And Bryan, of course, was sort of the granddaddy of a lot of the work that Moloney and Rauscher did. He didn't take to the administrative side very well, so Rauscher was made the Head of the Special Virus Leukemia Program.

Bader: I would agree. It's hard for me to remember back that far. It was 35 years ago.

Baker: Okay. The third one I wanted you to elaborate a little bit on your activities in the field when you were still in the lab particularly.

Bader: Well, I guess my most important contribution was in demonstrating that DNA was involved and was, in fact, a requirement in the reproduction of RNA tumor viruses and that the reverse transcription process was a feasible one.

Baker: And that was the first demonstration of that, as I recall.

Bader: Yes. I was the first to publish on that.

Baker: It's always nice to have the prior idea, although a lot of these things would have happened sooner or later, but it's always good to be there first.

Bader: Yes. I mean, it's a little hard for me to review. I could show you my bibliography.

Baker: It is worth my seeing it.

Bader: I mean, the things became very clear as more and more people got into the field. What was interesting, at the time that I got into the field, there were very few people working in the area of RNA tumor viruses in the *in vitro* situation. You

could count them on one hand almost. We'd go to the meetings and we would always be the last ones on the program. But when reverse transcriptase was discovered, of course, there was an explosion in the area and there was a lot of support at that time from the Virus Leukemia Program and many other good workers came into the field and there were many aspects of it that were developed at that time.

Baker: The fourth question sounds like it's similar to the first one, but here I'm after what you might call "science politicians" even, the scientists who influenced the field in ways other than just their lab work, for example Melnick was always very helpful in the primate area in helping us get programs underway. So, it's that kind of person. Of course, at high level was Sidney Farber. Even though he was in therapy he was also supportive of developments in the Viruses Cancer Area. So, does anybody of this ilk come to mind in your experiences? You may not have come across much of that since you were probably in the lab.

Bader: No, I didn't. I was in the lab most of the time. I was a young guy and putting all my energy into the lab work and, in fact, I avoided a lot of meetings. The big meetings I avoided. I got a lot more information out of the smaller meetings that we had. But I obviously heard about all of these guys. No, I really can't contribute very much.

Baker: No. That's fine. That's all right. And you probably weren't too aware of the membership on the NIH advisory committees?

Bader: In fact, not at all. The only thing I would say about that is the Airlie House meetings and the Hershey meetings that were initiated and sponsored by the Virus Leukemia Program and then the Virus Cancer Program, were very valuable at that time, because it brought together new investigators and stimulated a lot of new work in this area. But as far as the advisory committees, outside of that, those sorts of meetings I really am not familiar with.

Baker: That's fine. The next question deals with the resource developments which include special virus preparations, antibody preparations, special animals, tissue culture cell lines and that sort of thing.

Bader: Well, I think with the development of the program and the National Cancer Institute making those things available, at least to program members that were very valuable. The American Type Culture Collection had always tried to do this to a limited extent and it required still a lot of effort of the contributors because at that time the contributors were the ones who submitted samples in the vials that

were ready for distribution. ATCC eventually took over that. They would get samples and grow them up themselves.

Baker: But the quantities, I think, there also make a big difference. The ATCC, while very valuable, was not in the production business on a scale of what was needed to advance the field.

Bader: That's right. If I had one argument with the availability of materials is that they were restricted to program only. They weren't generally available. I worked in the Cancer Institute, outside the program, and had difficulty getting reagents from the program, so that kind of annoyed me at the time. But, nonetheless, the availability to program members was something that was very valuable to the development of the whole area. In fact, David Baltimore got the virus that he used to discover reverse transcriptase from the program.

Baker: I know that, and I gather Varmus got viruses through the program and he wasn't even aware of where they came from. And again, not necessarily should he have been aware of it when he was a young guy in the lab. His department head, Leon Leventhal, who used to be at NCI, got the grant and the contract and that's the way they got the quantities they needed. But he wasn't even aware that that's where they came from I think.

Bader: I'll say that since that time and, you know, recognizing how valuable this was, there have been several repositories which have made reagents available.
Baker: And of course now a lot of these are commercially available, but the developmental work was done, I think, in the program which allowed the commercial development later.

Bader: That's right. There was nothing in the commercial arena at that time for obtaining such materials. There was one company which made large amounts of virus. I'm sorry I can't remember the name of it.

Baker: Pfizer?

Bader: No. It was a local company. Pfizer did. That's right.

Baker: Microbiological Associates got it more on the animal side, I guess, in those days.

Bader: It was a local company which had a centrifuge program, I remember, with John Garon. I can't remember the name of it.

Baker: Yes. It's hard to remember these names. Are you aware of who made the key decisions on developing the Resources Area?

Bader: I assume that it was the administration in the National Cancer Institute, that it was Carl Baker, and John Maloney and Dick Rauscher.

Baker: Well, Bob Stevenson is the one who really ran this program when it was really being developed, although the original idea, I think we should go back to Harvey Scudder who was Executive Secretary of the V&R Study Section and had the idea that the investigators needed these materials and they weren't available, and so he started moving, even in the Grants Area, to develop these resources. But then Stevenson picked up the program with a lot more funding and really developed it beautifully.

Bader: That's correct. I had forgotten about Bob in this regard.

Baker: You probably never knew Harvey Scudder?

Bader: I didn't know Harvey. No. I didn't know him. And, of course, Bob Stevenson eventually became the Director of the American Type Culture Collection.

Baker: Incidentally, he retired there last year and moved to Santa Fe and only stayed about a year and moved to New Hampshire and stayed less than 6 months, I think, and he's back in Alexandria now. So he and I are working on this together.

Bader: Oh, good.

Baker: So I don't know whether we'll get a long paper or book out of this, but we wanted to get the history down. You probably weren't aware of the relative funding of grants and contracts.

Bader: Well, I was, in some regard and, as important as I thought it was to support this area, I just felt that the contracts, at least for a period, were not reviewed in the way that I thought was appropriate. The competition process wasn't one that I thought would encourage the best people to get into the area. I thought it was important to support viruses and cancer. I guess if I'd have had my choice I would have done it through the solicited grant process, the RFA process, rather than direct contracts. And I also felt that the review of those contracts could have been improved. The Hershey meeting, as valuable as it was, was a somewhat closed meeting. It was for program people. The presentations were program people. In other words, I felt that the whole process of support, the program thing, could have been opened up more, could have been made to encourage people from the outside to contribute more to it.

Baker: Well, this is an age old argument on the philosophy of the two different approaches. Certainly for exploratory research I don't think we've done better

than the regular grant system, but for developing specific areas then I think I have to agree with Endicott on why he set up the contracting route for funding in the first place; namely, if you're going to have a Drug Development Program, you've got to not let each participant go his own way, as you did in grants, because you had to have an integrated step-by- step development of drugs. And I think the same philosophy then was being applied in the Viruses Cancer Area. And we had grants that did fund a lot of people in the field of Viruses Cancer under the grants philosophy. So, to me, it wasn't either/or; we ought to do both.

Bader: I thought it was a great initial effort, but I thought it went on too long, you know, as a contract operation. I would have preferred to see it, you know, as more and more people got interested-- And I think a bigger problem was that at that time there were restrictions in funds so that the money was, in fact, being diverted into the contract program from grants. That's an opinion.

Baker: No. A lot of people agree with you and the Zinder Committee, of course, drew those conclusions. Now, I think they were more concerned that Huebner had too much control and too much money. Now, I looked upon Huebner as General Patton. He was a Field General of a lot of contractors on the West Coast, some of whom were very good investigators--well, most of them were--and he saw this as an attack over a wide area, rather than an individual kind of project, and I think he got a lot done through that generalship.

Bader: Well, I have to agree, but that was--

Baker: But people didn't like that, too much concentration in one person.

Bader: Well, I mean, that was one of the compliments of the man is that he was able to recognize talent. He could recognize bright people and people who were going to do good work, and under his aegis there was a lot of good work done and a lot of good investigators developed under him. So I would agree. I mean, Huebner, even though it was a little uncomfortable watching, or seeing, a guy with this much--let's say--power in terms of the money that he was able to bring to the scene, he was terrific in recognizing talent and getting work out of the people that he supported.

Baker: On this review of contracts, I assume you're talking about in a study section review you're looking at quality of the principal investigator primarily in doing sound basic research?

Bader: Yes.

Baker: And in a contract you may not be looking for that; you may be looking for can we develop an assay system, which gets down to the applied end of the spectrum instead of the exploratory research end of the spectrum. And those in study section reviews, I think, are not at home with the reviews that aren't typical study section. So the purpose of the review is quite different. I certainly would agree; we could always make reviews better than they are. I think a lot of the problem was the criteria that were developed in the thinking from study section type reviews was carried over into contract reviews, and that didn't always apply.

Bader: Well, maybe that's true, but I just felt that there were some investigators being supported at high levels who really had never demonstrated their competence and that with maybe a more expanded review that better people would have been- -

Baker: You're not alone in this thought. I've heard this before. So, that's fine. I want your views here. So I gather this takes us into the eighth question about if you could have changed anything you would have made it-

Bader: I would have opened up the program more, made it more generally available to scientists. Eventually-- Well, let me just-- It did develop from what I thought was a program of some good investigators and some mediocre ones into a program of pretty good established scientists. It eventually did attract people, but I just thought that that could have happened earlier than it did.

Baker: Well, in the resource production area, of course, there I think you'd probably find some investigators who you might say were not the best but, in order to get the resources produced, some of these best basic scientists didn't want to fool with that, and so some of the principal investigators on contracts that were really aimed at producing resources, yes, I imagine by the criteria of principal investigators you were used to looking at, they wouldn't rate high on the list. But you couldn't get the ones that were high on the list to develop the resources. So, some of it was for that reason, I think.

Bader: I think the other thing is there was such a desperate attempt to show that viruses caused cancer in man and hoping for the discovery of human cancer viruses that people were willing to accept what I would call inadequate data supporting the premise.

Baker: It was a real lesson for us all, because we got up to over 200 viruses, animal viruses that caused cancer in animals and still didn't have anything very decent in man at that point. And sure, we all thought, I think, at that time, that surely we must have something in man here with all this in animals. But this demonstrates, I think, how you can manipulate the situation in the laboratory to make it progress

in certain ways but it's so artificial compared to out in the real life society, and that's partly why Huebner was trapping the animals in the wild, to find out what was really happening, and it was a real instruction to us. But I was very careful never to over- promise anything in my testimony. I always pointed out, here were opportunities for expansion. I think, unfortunately, some people held out to the Congress the promise of results, and I think Vince DeVita made a mistake in talking about we would have cures of some magnitude by the year 2000, because this gets the expectation up too high, and that gives you problems later, and I think we're in a little bit of a problem on that now. People are disappointed because they were expecting too much.

Bader: Yes.

Baker: Well, you've already stated that this program was helpful in developing molecular biology.

Bader: Well, I can go further on that, because the area that I was interested in, of course, was the reverse transcription, making complementary DNA from RNA. In terms of biology this was a tremendous advance because I would say that, you know, a great deal of the molecular genetics that is going on today depended on that enzyme. We were able to detect gene expression by virtue of making DNA copies and looking for cellular RNA copies in the cells, we were able to make complementary DNA libraries where you could find specific sequences for specific proteins. That made it available for the inexpensive production of useful proteins. And, of course, there are many biological houses today which are attempting to, or have already, made many of these proteins which are on the market. They can synthesize genes and at some time in the near future we're going to be seeing gene therapy. There has been a lot of excitement about this but it'll be some time before gene therapy will actually be useful in the clinic. But, nonetheless, none of this could have been done without that enzyme, the reverse transcriptase. I thought the enlarged National Cancer Program, it's taken quite a direction in the last 15 years or so, but the National Cancer Program was really at the basis for molecular biology today, along with several other general areas, I mean, such as, you know, microbial genetics, which made huge contributions in that area. But without it, we probably still wouldn't know the cause of AIDS. We wouldn't have been able to work on HIV, for example, using the old techniques. The newer techniques allowed us, first of all, to discover the virus and to assay it. Whether or not Gallo was the first to discover HIV, he made very important contributions in this whole area, and that was through initially the Virus Cancer Program.

Baker: Interleukin as well.

Bader: Yes. They discovered interleukin-2. Without that, they wouldn't have discovered HIV because they couldn't grow T cells. So it was just the evolution of the whole area. And, of course, with IL-2, there has been a huge interest in biological modifiers of all sorts. There has to be at least 30 of them recognized.

Baker: Just as the oncogenes. What are we, up to about 50-60 of them by now?

Bader: I can't count them anymore. I used to know them all. Ten years I knew them all. I wrote a review on it.

Baker: We thought that we were really there, you know, and as usual it's much more complicated than you first think.

Bader: I wonder, you don't see any reviews of it anymore because I think there are so many it's so difficult to do.

Baker: The other area which is really moving now which is related to cancer is developmental biology, because here you not only have to switch on and off genes, you have to do it in a very orderly programmed sequence, and we're beginning to get a hold of that. The pace of findings in that field is coming rapidly. We're really l ft with the complex set of interacting reactions with multiple feedback loops, which is what gives organisms their tremendous capacity to adapt to the environment and maintain homeostasis and so on, and somehow we're going to have to learn how to deal with that complexity, I think. There are some interesting new books out on that subject, complexity, because I think that's the main reason we're not further along after all this money and investment of time by very good people. Why aren't we further along in the cancer field? Well, the organisms are extremely complex, I think, is the basic answer.

Bader: Well, it's interesting though that you're getting contributions from many other areas now to the cancer field, just in cell biology, whereas things were more focused in the past and, of course, the molecular genetic contributions have always been there in one way or another, but there are people in other areas of development, but also basic diseases, in the Heart Institute, the Neurological Diseases and they're all making contributions to the cancer field.

Baker: Of course cancer now is a little more respectable than it was 40 years ago. There was a famous saying, you know, that the scientific graveyard was to go to work in the cancer field. Well, I think once the viruses area got going that changed the philosophy on that a bit. And then, of course, we benefitted from bringing in a lot

of good virologists who had been active in the polio area, and that happened to be a good timely event, I think, as far as cancer research is concerned, and so we got a lot of help from those fellows too.

Bader: Well the early prominent guys in the cancer field were from the polio area, Salk, Dulbecco, Melnick.

Baker: Sabin.

Bader: Sabin.

Baker: I was very lucky to work with all these people. So I think you'd agree that this not only helped develop molecular biology, but biotechnology, much of it, rests on developments from the NCI viruses-cancer programs.

Bader: We couldn't have done it without them.

Baker: Certainly not at that speed. And in order for commercial development you've got to have a market, and you have to therefore have a certain size, minimum size, of investigators in the field who want to use these products, and I think the program helped develop that cadre of investigators that then became a market for the biotechnology people. Do you agree to that?

Bader: Sure.

Baker: Now, the tenth question changes direction here and is a more general question about the way the public looks at research and the public's knowledge of science is it better or worse than it was in 1950-1960?

Bader: I don't know that the public is any more interested in science now than it ever was. Maybe there are students who are, but I'm still disappointed that science isn't a requirement, sort of the way English is, or history, in the colleges. It is in the high schools, if you're going on to a college curriculum, but once you get to college you don't have to take science courses and, to me, every bit of our culture relies on knowledge of science as well as these other features, these other general topics. But, you know, in terms of how the political climate and public knowledge has changed, I think that in terms of money and support with grants and contracts that there are more constraints now, that people are much more accountable. There are many more issues certainly than there were 30 years ago.

Baker: The ethical questions are different than they were, radically different.

Bader: Yes. And you could focus a lot more, I think, 30 years ago than you can now. The other part of it is that scientists knew more about general biology and what was going on in other fields, in immunology, in development, and those things, because there wasn't so much information. Now there is so much information coming down the pike every day that you have to focus on a very narrow area. And in a way it's a disappointment because most scientists these days don't know very much about areas outside their own particular interest.

Baker: Yes. The competition is so severe a fellow has to keep working to produce results and it's so complex and there is so much information, as you say, he has to narrow it down. So I can understand that, but it's a shame because there is so much going on across these minds.

Bader: Well, it's a curious development because I have never seen so many bright people in one area as I see in biological sciences in general today, but yet they're so focused that they don't have general interests.

Baker: Yes. Well, I was always kind of a generalist, although I got pretty narrow in the Lab of Biochemistry on the enzymatic resolution of amino acids, but I always tried to go into a scientific meeting and also hear papers on subjects I knew little about. And this habit of looking across the various fields helped me, I think, very much at the higher levels of NCI because, in looking at budget priorities, you want to have a pretty good grasp across the whole mix and, of course, cancer research includes most of the disciplines; so I found that if you focused on the main thrusts and didn't get too lost in details, you could keep up fairly well with the main developments. So, I like to think I know a fair amount about molecular biology, even though I never worked on it in the lab. And, as you say, you could learn a lot in talking to people, and I guess I was known as a guy that was always asking questions. That particularly helped me in the job of budget development. It ought to be a balance, you know, and you shouldn't have favorites. You should treat everybody in the whole spectrum. But it's tough to be a generalist these days.

Bader: Yes. I don't know of very many people who are.

Baker: I like to think I am.

Bader: No young ones. No young ones.

Baker: Okay. Any additional comments or anything else you want to bring up?

Bader: Well, I thought it was terrific growing up through this era because there were lots of surprises. These days there really aren't very many surprises. You can anticipate what's coming up and then just wait for the work to be done, because the people are so good that they're going to do it. If there is a good idea, they're going to do it. The techniques are there and the technology is there. It wasn't there during these earlier times. You had to somehow work around things and find other ways to do it.

Baker: When I was in biochemistry at Berkeley, California, right after World War II, I had to count radioactive samples one at a time. I had a little lead chamber, and I'd open the door and put the sample in, and you'd wait for the damn count. It was amazing.

Bader: Well, I spent most of my career looking through a microscope at petri dishes, and now these guys, they have 96-well plates where they can do robotic counting.

Baker: Count them automatically. Well, certainly automation and computer developments are dramatic in the help they give us in these other developments. And what the Internet is going to do, I'm not sure, but I think it's probably to the good, if we don't clutter it too much with the commercial ads. At least if they keep the tree arrangement you can go down branches and don't clutter it up with advertising, I guess.

Bader: We're getting our program on the Internet now. It's up now, but it has to be fine-tuned.

Baker: Well, John, I certainly thank you for your time and thoughts.

Bader: Well, it's nice to see you, Carl.

Baker: Nice to see you again, and keep up the good work.

End of Interview