

Interview with prof. dr. Joshua Lederberg, Rockefeller University, New York, Februari 3, 1992.

Wouters: If I restate the aim of the project, it is writing the history of scientometrics. The aim of the trip is the early history of the science citation index.

Lederberg: So you gonna have a concentrated thing on the SCI. OK. I just wanted to give you one document. It sort of tells the story. And then, you know this book? (The metrics of science).

Wouters: Yes, that is the first book I read on scientometrics. Dr. Lederberg, you wrote in May 59 a memo to then Mr. Garfield. Had you met before?

Lederberg: No. I do not think so.

Wouters: What was your first reaction to the Science article?

Lederberg: That's what I wrote about right there. So, why do not you take a minute and read that?

Wouters: OK

Well, let me rephrase my question. I was curious whether the letter in 59 had a direct relation with the activities you were engaged in at the time, concerning information or concerning science policy, or

Lederberg: In 55 probably not. I have always been very closely connected with the scientific literature and my interest in the Citation Index was almost totally as a way of getting better access to that knowledge, rather than the sociological applications or the kind of statistics that one could collect. And I guess that I still feel that way. Haha. I am rather sceptical about counting citations, I have written some rather quizzical articles about that and I tried to caution Gene from going too far on an interpretation of those statistics. I cannot ignore them and one can also say if you try to measure scientific outputs what other choices do you have? There is very limited possibilities but I think that one should use those statistics with very great caution. I just know that there are extraordinary quantitative distortions about the kind of things that tend to be cited, especially methods versus not. If I go over the citation statistics of my own work, and that offers in some ways a little better control, the important papers are not necessarily the ones that are cited the most frequently, not by my judgment of them.

Wouters: So would you say that the function of the SCI in science policy or in evaluating the output of scientific production would be especially as tool to locate qualitative judgements?

Lederberg: Oh, yes. I use it very extensively to, in just that way. If I have to prepare an assessment about somebodies work, the people who have cited the article give me immediately access, one to what they have already written about it and look at it qualitatively, and two, they can be identified and I can go to call them up and ask them. "You should be an expert on so

and so." And sometimes they say "who?", haha, even if they cited him. Haha. So it cuts both ways. In view of some amusement things, like the propagation of error, those are sociological enquiries that are very amusing to make. And they make it perfectly obvious that people put in citations on things they never read, haha.

Wouters: What was your first impression of mr. Garfield? You met him probably not long after your memo?

Lederberg: I think that is right, I cannot cite the exact date, but he was interested in my response and told me that, I have to recheck what I wrote there, but eh that he would like to try an experiment. I do not know whether he started first or I started first, "say why do not you see if the NIH wont give you a research grant to try it out". But in any case I became a very enthusiastic supporter of the idea and agreed to be on the advisory committee so I was on the Genetic Citation Index project and I foresaw accurately that geneticists would take a special interest in this methodology because they understand lineages, they understand genealogy, the parent-offspring relationship and so forth. Well, it is still not a very complicated idea, still to a geneticist it comes somewhat more naturally. And the idea of kindreds, that there would be a clustering of the scientific literature around bibliographic coupling again, is somewhat selfevident. So that was how I got started with it. We had a very distinguished advisory group with some very famous geneticists, although it was a very primitive product. Have you seen the genetics citation index? You see how limited it is, compared what we have on the CCOD or the CD-ROMS, but it was the beginning. So, a little bit after that, Gene said I am going to try to make a product out of it, and I said, Gene you will loose your shirt! Do not invest your own funds on something like that. It is imaginable if the National Library of Medicine would have subsidized it, as they do the Index Medicus and so on, it would be a very valuable service. But I could not believe that he could recover his investments in it. I was wrong, he made a very good business out of it.

Wouters: After some very difficult years.

Lederberg: It took time, yeah. Well, if he did not have Current Contents as a constant baseline of income, I do not think he would have made it. But that did provide the necessary risk capital to continue with it, and that is very satisfactory.

Wouters: You played a very active role if I am correct. And you already were very busy in that period.

Lederberg: Well, starting around 1960 I did begin to be more interested in science policy matters and I was on the PSAC panel on scientific information, the Weinberg committee, you know about that, (Wouters: in 1961) and that was my formal entree into that arena. It was my interested background in citation indexing, but I had also become just by that time much more active in studying computers. Well I had some vague contact, ever since 1941 when I was a 16 year old I saw my first computer in an IBM laboratory, and I took some programming courses in 1953. I do not know whether you know the name Fred Grunberger but he was one of the

teachers. So I learned plugboard programming if you can believe it, before we had Fortran. I always had the view that these machines could be very interesting prototypes of systems, of biological systems, not as direct models, but that they would have some of the degrees of intricacy and feedback mechanisms and so on, and I think that it is true. If you do not push the analogy too far it is indispensable to have a notion of how automata function if you want to have a good system view about living organisms. So. But I did not do much about it until sixty, sixty one, because that was when computers began to arrive at the academic scene. At Stanford I did become very actively involved and in fact I eventually became a parttime member of the computer science department out there, as well as in Ch (i-Science?), and I worked with Ed Feigenbaum on the artificial intelligence and the first expert systems, so eh. That was not a trivial involvement, but it only just began then.

Wouters: Was that an attractive aspect of citation indexing, that you do not need an expert to index?

Lederberg: The idea that it could be made algorithmic was very appealing to me. Because otherwise I would be at the mercy of the judgment of an abstractor. And I had done abstracting and knew how arbitrary it might be and that while it sometimes could be very good, it was unpredictable and would be unreliable. Those ideas of axiomization of scientific representation had always interested me, that is what me eventually led me into artificial intelligence and so on. It is a somewhat disparate line and it did not materialize until after 1960. But it is one of the reasons that I would be attracted to this. When I was an undergraduate I did take some courses on logics, and I have another paper I will give you where I talked about Leibniz. Certainly the computers began to make it imaginable that one could have a systematization of knowledge. And it also has to do with the efficiency of communication between scientists and so on. So I had been involved in all those things as well. But they did not really materialize until after the Citation Indexing but they were certainly stewing as part of my very early education.

Wouters: Was that also the reason that you became appointed as a member of the presidential committee?

Lederberg: I do not know exactly why did they pick me out. I must have been making some noises in that direction. I cannot say what product I had generated and certainly none of my publications would have said very much about it. But eh, it is always an interesting question. I did not stop to ask then why did you think of me? I guess I met Al Weinberg at some of the scientific meetings down at Oakridge. Among the biologists I was probably using the terminology of information science and so on as systematically as anyone else. I had read Shannon and Wiener and things like that. But I do not instantly recall what the immediate basis would have been why they asked me. I may already have been talking about citation indexing because my letter to Garfield preceded that, so.

Wouters: You coined the term "Science Citation Index".

Lederberg: Did I?

Wouters: Yes. Did you forget? Garfield used the term "Citation Index for Science".

Lederberg: Well, what is the difference, I mean. I like the initials SCI, because of science.

Wouters: Well, I am asking because it shows your interest from your position to even think about titles and very detailed questions concerning the experiment of the genetics citation index.

Lederberg: Oh, I was quite deeply involved in the design. Although Gene had all the major aspects of it laid out, when it came to questions like what the scope of it might be, which journals would be involved, there were tradeoffs about what you could actually afford to compute and come out with, I was thoroughly involved from beginning to end. Otherwise, I was the chairman of the advisory committee at that?

(Wouters: yes). But I was so intrigued at that because it would answer a very deeply need on my part. I have a great curiosity about many subjects, I run into things I read about and if it is three, four, five, ten years old, I want to know, well I wonder what has happened? And citation indexing was the one way to answer that question. Including what happened with Garfield's idea. That is the reflexive aspect of it. Haha. I really said I got to have it. Somebody else answer would be, gee, it is great you have the telephone, you call up and find out that way. Well I have an aversion to that. But the idea that you have to impose on somebody's time, that you have to rely on their memory, I really like things to be well documented. I am fanatic about that. I believe in citations.

Wouters: Have you changed your own citation behaviour by thinking more thoroughly about the consequences of the citation index?

Lederberg: I do not give a damn about the statistics, I do care very deeply to make sure that you have the retrieval links. I will make very sure that the appropriate citations are there to make it possible to go from an established work to the current one. So, that I do quite selfconsciously.

Wouters: How would you characterize the resistance against the idea of ci?

Lederberg: What, the idea of counting them?

Wouters: No, Garfield had an enormous amount of difficulties before the citation index took off. Maybe I am wrong, but I have the impression that even the scientists were generally not very enthusiastic.

Lederberg: Well, I do not know if I am a fair one to judge. They may vary from great enthusiasm to some indifference, but I do not know of anyone who is hostile to it. I mean, it is a wonderful tool and some people use it some more. But some people do not use the library

and I think it is very highly correlated. For people itched in the literature then the citation index is a wonderful addition. Some scientists do not care about the literature, they write their papers to sort of make sure that their conclusions are on the record, but they believe they have already communicated their work in meetings and by word of mouth about as far as they really mean to do. It is basically a question of literacy, it is against the oral tradition.

Wouters: But you are the only one to write a memo. I have seen in the archive that there were a lot of requests of reprints of the second article in Science, vol. 144, but the first one ...

Lederberg: Which year is the second one?

Wouters: Several years later

Lederberg: So it was the first one I responded to? (Wouters: yes). And you say I was the only one who wrote to him?

Wouters: Well, yes. I think your memo triggered a new encouragement to mr. Garfield to start up the whole thing again. He was a bit loosing, not his interest in it, I would not say that, but the feeling

Lederberg: Well, I did not know that. Have we checked what the early citations to that article are? This is the volume from 55-64. You should have (Wouters: I should have done that!). Haha. You know this will be another interesting case in point, there are not a lot of citations to it, I am sure it is undercited relative to, in view of its importance. You have to use the right glasses for this. No, there are eight or ten of them, but four are Garfield, haha, one is Tukey, and I know about Tukey, because he did his own you know, I have seen John over many years, at least until a few years ago, he must have stopped now, and he would prepare his own citation indexes by hand.

Wouters: Was he also connected with the presidential committee?

Lederberg: Was he on that? He might have been. On the Weinberg committee? I think he was on PSAC, the plenary. I do not know if he was on the information science group or not. But it is easy to find out. Anyhow, and then there is one, anyhow in ten years there are only a couple of citations. That would be selfexemplifying on the scientometric aspects. Haha. I tell you the reason it is, you would cite the paper if you were writing a history of the subject but it is a household word. So, there are many and there continue to be many attributions by indirection, without giving any citation at all. If this were a method for the assay of tryptofan then you would probably give a citation to it. The interesting question to poll the people who cite Lowry to ask whether they actually read it. (Wouters: probably a minority). I would think so. OK.

Lederberg: What was your first impression of Garfield as a person?

Lederberg: Just a very energetic imaginative fellow, and I thought probably going to take great risks and going to bankrupt himself. I did not realize that he probably is a very shrewd businessman but he is such a generous person and so enthusiastic. It was hard to reconcile it. But he has done fine and I am so pleased for him. He will be able to retire in comfort and that was not so clear ten years ago.

Wouters: You were the first shareholder I think.

Lederberg: Well, a little later on he told me he was going to go into it and he asked me if I would be interested to invest in it. I thought I was putting so much time in it anyhow and he did want me to go into the board so I said yeah. I never thought that would be profitable but eh by being very very patient, haha, it has taken thirty years, but it has given me a very good return. Haha. I am not even sure how much he was looking for money at that time, I think he just wanted me to keep me engaged and involved, I think it was a fair price, I do not think it was a give away. It was a quite minor, I do not know, one or two percent of the investment in the company and later on he did need some more capital and went to bankresources. It was not part of floating the stock.

Wouters: Would you say that your membership in the science policy committees has played an important role in introducing the citation index as an idea on a broader scale?

Lederberg: No, not at all. Not at all. It has just never come up in that connection. I have never had to be the one to bring up. Since I am not an enthusiast about using it for scientometric purposes, the only place it does come up is in bibliographic context, you know the National Library of Medicine, things of that sort, and eh, those folks already know all about it, and eh then when any decisions are made I have to excuse myself, I cannot get myself into a conflict of interest. It is all completely public and known and understood and I have not sent myself in many decisions involved or any action on behalf of. But I cannot remember when it ever has been an issue. (Wouters: in the early years maybe?). Well, you see it did not depend on science policy but on the scientific community. There was nothing the government had to do with it. Basically, it was a question of a private entrepreneur dealing directly with scientists, is hardly the government business. So there were no science policy issues that I know of that had been connected with it. May have been but nothing that I am aware of.

Wouters: Your contacts within NSF and NIH have been more important, am I correct?

Lederberg: But it goes the other way around. I think I was helpful at the very beginning, when I first got interested in the CI and wrote to Garfield. Yes, I am sure that my contacts with NIH were helpful in getting that started. That was before I had any connection with the company. That initial introduction, I am sure that that was helpful. I cannot recall, that did involve a research grant to try out the idea. And that had to be reviewed by committees of others, it had a very careful external review, but that is about the only time that it has been dependent in any way on the government. Most governments activities in fact have been very competitive with this, and here I differ from Gene. I think what is best for the scientific community and if the

NLM can get more funding and provide more services I have always favoured that, even if it is not to the best advantage of ISI.

Wouters: You once proposed the idea to Garfield to let him apply for a job as director of the National Library of Medicine?

Lederberg: I probably did, I do not remember the occasion, but if there was ever a vacancy, I thought he was such a wonderful, imaginative person and eh, you know he was really a leader in bringing computers into information retrieval. And other people working at it at a theoretical level, and natural language translation, you know people like Salton (?) and so on, but in terms of generating useful products he was really the pioneer, so I thought it would be wonderful if he would use his imagination in that direction. And I suspect, if it would have been offered to him, he would have accepted, even if it would be a substantial financial loss to himself to be a governmental employee instead of part of his own business, but I do not think it was ever offered to him. I do not remember the specific occasion when there was a vacancy.

Wouters: Well, it has been the subject of a telephone call between you and after that Garfield made clear to you that he did not want to leave ISI.

Lederberg: When was that, though?

Wouters: Must have been after the Genetics Citation Index. Somewhere 61, 62.

Lederberg: OK, well I do not recall the conversation but I have no reason to doubt that. I would have liked to see that kind of imagination in that role. Now, it has eventually reached there and Don Lindbergh has been there for several years now and he has a similar modern perspective and I think he is doing probably all that could be done in that particular area. Well we all know each other very well, although it is actually cramping private enterprise to have the government providing high subsidized services, the fact that they provide MedLine and other things at a fraction of the cost of production because they have heavy subsidies, obviously undercuts private competition. But from my point of view the side that gets the best of it is that counts the most.

Wouters: The choice of genetics as the field for the first trial experiment. Was not it the case that you favoured a general citation index immediately, without an intermediary step in a subfield?

Lederberg: I do not remember the details of the argument. I understood that there would be questions of the cost and the feasibility that would have to be traded off, but I am sure that I agreed if I did not propose, that if it was going to be restricted then genetics was a good a field as any. Partly because it was a pretty well defined community that people knew one another pretty well and it would make it a lot easier to assess how adequate the index would be. But especially that it would be familiar with the ideology of it.

Wouters: You did not choose it because you were yourself an expert in it?

Lederberg: I would have been quite happy to consider any discipline but it seemed to me that it would be the community that would be the more effective one since. I am partly answering your question, yes I am a member of that community, thought that community would be the right one. So eh. I could play a more effective role in evaluating the outcome if it was a field where I myself know the literature fairly (very) well.

Wouters: Did you also have a lot to do with dr. Sher? I think he was the principal investigator in ISI.

Lederberg: Oh, Irv Sher. A little bit, not a lot. I mostly dealt with Gene. I still deal with Irv. I eh, he is in charge of quality control and I am constantly reporting when I discover discrepancies in the database I send him a message once a week.

Wouters: So you are very actively using the literature.

Lederberg: Oh, very much so. I have the CD version, I'm very privileged to have that because it is quite expensive but I am kind of a beta test site for their products. But the reason for that is that I am a very active user, it just comes up all the time. I must dial into it ten times a day. It has such an infinity of uses, for one thing it is a good address director. But also in checking out the scientific networks. I am just right now, I have to write reports on five or six fellowship applications. And I have done exactly what I told you. I have seen who have cited their work, did they include people that I can have easy access to, or that I have a lot of confidence in, and then I call them up and say: what do you know about this person? So, and I can look for commentary on work that that fellow has produced, so even if it is a field that I am not immediately familiar with, I can go and locate it. My wife is just finishing a paper on the ethics of dealing with normal subjects in medical research and I suggested to her let us use the citation index to try to locate that and the related records is very helpful from that point of view. There are some substandard questions I am not exaggerating five or ten come up a day. I have been trying to persuade ISI that they should change their marketing strategy, that these are tools that should be on everybody's desk that they ought to gamble on lower prices with greater distribution, or try to figure out some kind of leasing strategy whereby the pricing will be related to the level of use, which is really the only fair way to do it. It would be analogous to what you have on the online base, but this is so much more convenient. They probably have to build a complete standalone machine to have all these ... on, and to be able to constrain it. But you can imagine it, like the gasmeters. I think this will only be successful when it is really brought out to very closely individual availability.

Wouters: At the moment it is more aimed at the libraries.

Lederberg: Exactly. That is the distinction. Some of them can network those locally, but that is pretty clumsy to do. But I am sure we will see this in a few years, that it will become personal.

Wouters: You think that will be the trend?

Lederberg: Or like this, in a department where a group of people working very closely together share access to it.

Wouters: Talking about scientometrics, have you met Derek Price?

Lederberg: Yes I did. Used to have a few scraps with him. Not intense ones, we liked each other but I guess I did not like to being regarded as a statistic. Haha. I was actually producing science, grinding me up in a machine and trying to make a sausage out of this.

Wouters: You did not like that.

Lederberg: Well, I thought the interesting thing about science is its qualitative content, so, but I have quoted him often enough about the rates of growth of science and so on, and I had no other place to go. Although I think the simplest method is the number of scientists, if you are talking about the growth of the field, but even there are ambiguities, who is a scientist, on what level do you include the technicians and the engineers, that can get complicated too. And then you have all the automate that work for you, so individual bodies, I have a million horses for me in that little terminal, so hahaha.

Wouters: What was the first book of Price that you read?

Lederberg: I read Science since Babylon, quite early. I do not know when I met him. I can look that up if you are interested. I may have some correspondence with him.

Wouters: Yes, that might be interesting.

Lederberg: Oh yes. When was his first book? (Wouters: 65 I think). Yes, then I already was interested in science policy questions. Started in the early sixties. I do not recall the circumstances when we first met, but we would see each other. I have done no research in that subject. Harriet Zuckerman and I wrote a piece on postmaturing in science. It is based mostly on some retrospections on my own work in 46 and my argument, the argument in the debate, was why was that not discovered earlier. And I did argue that it was in some ways a postmature discovery and then you ask what do you mean by that? Then you get some complications. Well let us look it up. We had a lot of our own critical inspections about that idea, the validity it might have and so on, and it eventually occurred to me, and I think I persuaded Harriet not to worry anymore, that if there can be resistance to a discovery then there can also be a internalized resistance, a deterrence of it. And if the discovery is deterred, then when it does happen it is postmature. I sort of concocted that diagram but the main notion is the idea of the epicycle. I wanted to stress that this kind of continued recirculation with eventually some progress around the wheel, is much more typical of the progress of a project than a monotonic success. And that is subject to either regression or failure on any of a member of points. At one point, if it gets this far, you would call it resistance, and then the

community reacts to it, but if it is deterred at an earlier stage on the same considerations, then you can call it a postmature discovery. A premature one would counter resistance. I could criticize this, why just one cycle, I could think of a large numbers of vortices, and the connections between one another, but still as a fundamental framework it is not too bad.

Wouters: Thank you! Do not you think, one of the ideas of scientometricians is that you could discover patterns by processing huge amounts of data.

Lederberg: Well, the co-citation data are very helpful on that. You can do it semantically, but that does require a very delicate insightness, because you are dealing with a very wide range of sciences. And even then the meaning of terms changes. It is a very elaborate analysis to do that. So, ok, it has its role but eh, the next best is bibliographic coupling, it has hazards, but I think it is an illuminating way of looking for how science is differentiating. I have tried to get Gene to, well he is not so very active anymore, but Cawkell in England has been continuing a lot of this stuff and they were one time publishing an atlas of science which they were using these clustermaps. And I said, one of the serious problems is what do you call these clusters. The names are not assigned algorithmically, and they are often misleading. The second is why do not you do a crosscheck and validation

(new tape)

...

there are very good data sources for that because they put out the index on scientific proceedings, technical meetings and proceedings, and eh those have a title, many of them are old hat and many of them are new, and why do not you look and see whether the insight that was involved in the organization of a conference had the same kind of envelop as to what was pertinent and relevant enough to bring a conference together as it is involved in a cocitation cluster. So there are some cross checks that could be used, there are other ways in which scientists themselves define their own envelopes of activity and I think you would do much better if you made an effort of that kind of cross validation. But the data are too clinical, as you say, so it is not so easy to extract any real meaning out of them. You wonder what kind of questions to get to and I suppose a lot in the extent to which scientific progress is driven by internal versus external factors and what those are. And I have to give some credits to both. It is certainly possible for there to be individual insights that can make a big difference and transform a field and can make possible what was not before. It is also true that there is a lot, you can see it happening almost at the moment, that given a new technology, a new set of cognitive instruments, that OK there will be an army of ants, all ready to devour it all and they will find out whatever nature has to offer. You cannot always predict what that is going be, but you will certainly know the effort will be applied there. And today a lot of molecular biology is in that category. It is almost too simpleminded to make a discovery in this field today, because there are welldefined techniques, a wide variety of objects and materials, and almost anyone would be something usefull thing to study. Just get the sequence of gene acts and what the product is, and what the binding is of these substances. And so on, and you have important and

useful work. I am not in any way demeaning it, it is just that it has become much easier to do it. It does not require the same degree of imagination and extraordinary insight that some kind of science did forty years ago.

Wouters: But not every question is as important as another one. Do you think the SCI can somehow provide insight in crucial problems?

Lederberg: I think you need qualitative information to find missing pieces. I do not think that the, I have never got much out of the organization of a field, that is what is existing from those sources, but indispensable are the individual components. Because I, you can only be descriptive in that kind of aggregational analysis, and by no means convinced that what everybody is doing is the optimum. All kinds of institutional constraints, plus different people have different imaginations and so on. But, certainly, if you want to know what is going on right now, then it is also very helpful. But then, the idea is, you want to find the dog that is not barking.

Wouters: Yes, when, it is a difficult question again, when did you have the feeling that the SCI would take off? In the beginning it was very difficult

Lederberg: Once they got the first volume actually printed. So in sixtythree I think, I was pretty sure. I had no objective evidence, I saw it and I said how could the world do without it? Haha.

Wouters: Was it more than you expected?

Lederberg: Yeah, I would say so. I do not know how to define it, it is not a rational level, but I was really thrilled, began to use it right away and would find things I had not known about before, be able to do a much more proper and thorough job of analysis. If I had to write a paper, a bibliography and so on, I really felt that I really got a powerful tool to do it right.

Wouters: Do you think that the personality of dr. Garfield has played an important role? How important was the fact that in that time, in that part of the US a mr Garfield was trying to

Lederberg: I do not think that anybody else would have tried it, that anybody else would have taken the chances involved in risking his life savings for that kind of an enterprise. There has been a shift in entrepreneurialism, in sources of venture capital and things of that sort, which did not exist in that time, he was very much on his own, I do not know whether he was able to get a bankloan maybe, but if he gone out and try to sell stock he would not have got anywhere. And then he had to use his own resources, he was gambling on his own basically to do it. So his willingness to take those risks was a large part of it. But he also had the insight to know it could work. And I guess his positive experience with Current Contents and so on, now I was much less sophisticated in business matters at that time than I have become since and I do not know that I ever asked him what was his business plan, what marketing test had he ever done, what would make him believe that you could make a profit. My instincts were all against it, as I think I have told you, but it was not based on any kind of analysis. I would have not known how to do it in those days.

Wouters: You just had the feeling that it should be done

Lederberg: No, on the contrary. I thought it was wonderful to do it, but I was sure that nobody would buy it. That it would not be a commercial success. I was very worried that he was going to throw everything into it and would lose it all.

Wouters: Why do you think he pursued?

Lederberg: I have forgotten the detail of it, but I think he was on to what he saw as a very good idea and if Current Contents could become profitable, he thought something like this could too and in the end, he was right. I was not surprised that it took a long time, I was amazed that it ever did succeed, ok? Haha. But eh, I do not know, I would have to reexamine, you know why did I invest in that time, what in the circumstances was quite a lot of money, maybe laughable in retrospect but at the time. I guess I thought there was a chance it would work, I must have, I would not just throw it down. But I also wanted to give Gene, if he was going to invest that much in it, and so on, yeah I am sure it was that. I said, look here is a guy, who is, he has made it, he is no fool, if he thinks it is a good idea businesswise, even if I do not think so, there is at least a chance that he is right. And that was good reasoning. Haha.

Wouters: OK, Dr. Lederberg, thank you very much.