

**Interview with Jacob Mincer, Dept. of Economics, Columbia University
by Claudia Goldin, Dept. of Economics, Harvard University and the NBER
July 26, 2002**

Note: Jacob Mincer asked not to be photographed. The camera was focused on a sofa and a painting. At times, Flora (Jacob's wife) was photographed and at the end, various paintings in the apartment done by the Mincer's son, who died 20 years before, were photographed.

Present:

Claudia Goldin (*CG*)

Jacob Mincer (*JM*)

Flora Mincer, his wife (*FM*)

CG: I am at 448 Riverside Drive, a beautiful place, with Jacob and Flora Mincer and it is July 26, 2002 in the morning. Jacob is going to tell us about his time at the NBER and how he got into economics. Why don't you start with that?

FM: July 26th is a special day for him.

CG: It is!?

JM: It is my second birthday, the day I arrived in the U.S. in 1948.

CG: That is wonderful. It is very meaningful.

JM: The history of my studies—before WWII I studied engineering for about 1 year and then the war broke out.

CG: In Poland?

JM: No, in Czechoslovakia. I was not accepted in Poland. I applied to the Institute of Technology but they didn't accept Jews. They accepted 1 out of 100, or something like that. But I had admission to the School of Engineering in Brno, Czechoslovakia. I went there in the Fall of 1938 and a few months later the Nazis came in, so I did not continue my studies until 1948—an interruption of 10 years. In 1948, I got here on a Hillel Foundation Scholarship. At the time I did not know what I was going to study because I lost interest in engineering. So I decided to sort of shop around at Emory [University] for various courses and I thought maybe the social sciences were meaningful. One thing about the social sciences is that I thought I understood much of it but one subject about which I understood nothing was economics. That was the reason that I decided to go for it and especially because the economics teacher at Emory was a very good teacher. He was the one who pushed me to do graduate work in economics. What he said was: "You either go to Chicago where you will study with Milton Friedman, who is not yet known but who will be known, or if you don't go to Chicago, go to Columbia because

they have a linkup with the NBER. That is the only place that does empirical work in economics.

CG: That person was Swenson and his field in economics was what?

JM: Yes, Swenson. [His field was] price theory. His prediction, his wishes for me both materialized. First I went to Chicago. But Milton was not there. He was away for the year. However, Flora was there. That is where we met.

CG: [takes a video of Flora.] So you met Flora.

JM: She got an offer of residency in radiology at New York in Monte Fiore Hospital. So I had to make a choice—either to study at Chicago and forget about Flora, or go to New York City with her. Of course, the answer is obvious and not just in retrospect. So, when I got to Columbia I immediately started finding out something about the NBER and, of course, I needed a job anyway. I found out who in the department was working at the NBER and who was a staff member. There was Frederick Mills, a statistician, and I asked him if it was possible to get a job as a research assistant at the NBER. And he said “I’ll find out about it.” And the next day he told me “yes, Ruth Mack is looking for a research assistant.” And I made an appt to meet with Ruth Mack. She was not on the staff of Columbia University; she was full-time at the NBER. At that time, a lot of people were full-time at the NBER and did not teach.

I came to the interview at the NBER; it was at Columbus Circle on a very high floor. But despite being on a high floor, it was very dark. Anyway, I came to Ruth Mack’s office and what do I see? The whole floor was covered in graphs, and sitting in the center was a very beautiful woman with a corn-cob pipe and she asked me a number of questions. And I asked her a question: “What are these lines on the floor?” She said: “These are business cycles?” I said: “I don’t see any cycles. I see wiggles that go on the cycles.” Anyway, she hired me.

CG: Even though you were dubious about the way in which they did business cycles?

JM: I didn’t have any idea what was being done so I was very curious and besides I needed money. But she was very nice and I spent several years working with her.

CG: This began in 1951?

JM: No, Spring 1952. I think I worked 3 or 4 years for her.

FM: Our first vacation as a married couple—one week—was occupied by Jacob going up to her farm in Vermont and leaving me stranded.

JM: She had a big barn and desk for me to work on and she went horseback riding. She was nice.

CG: So this was her project on leather, and boot and shoes?

JM: Yes, that is right. I was at that point already accustomed to think in terms of price theory and I couldn't find any theory in what she was doing. So I kept criticizing her but she was appreciative.

CG: Did you meet others doing similar work on business cycles in the “business cycle shop” at the NBER?

JM: Victor Zarnowitz worked there.

CG: Victor arrived in 1952 from Germany where he got his undergraduate and graduate degrees. [Ed.: Victor Zarnowitz was also a concentration camp survivor.] He gained entrance to the United States fairly late. And others? Who else was at the NBER then?

JM: Don Jacobs—who was not long ago the Dean of the Kellogg school [Ed.: Northwestern]. He worked on financial aspects of business cycles with Steve Solnier. He was the research assistant to Solnier. There was a bunch of other people.

CG: Did you see Simon Kuznets?

JM: I was just thinking of the research assistants. But, yes. As far as the staff was concerned Simon Kuznets was an important figure.

FM: You had something to say about Simon Kuznets the other day to me.

JM: What was it?

FM: That you spoke to the Swedes.

JM: I can't take credit for his Nobel but I spent a semester in 1971 in Sweden teaching labor economics and I talked to the people on the Nobel committee. At that point they had given the prize only a few years and they gave it to theoreticians and econometricians. I told them there is a vast field of empirical economics, national income accounting—a whole new way of thinking about the economy. Of course Simon got the Nobel Prize, but I am not going to take credit for it.

Of course, Arthur Burns was the ruling dictator.

CG: Tell me a bit about Burns, your recollections.

JM: Burns, of course, was teaching at Columbia and he was a protégé of Wesley Mitchell who was the founder of the NBER. When I got to Columbia, Mitchell was either retired or not alive.

CG: He wasn't alive; he died in 1947, I believe.

JM: So Burns was the head of the NBER and his right hand was Fabricant and the second hand was Geoffrey Moore and that was the troika that ran the NBER but there was no question that Burns had the primary power. I will never forget the time when the article by Koopmans came

out “Measurement without Theory.” Well, it didn’t take long for Arthur to write the rebuttal, which Arthur called “Theory without Measurement.” Before that happened, he invited Koopmans to the NBER for a conversation. Everybody knew what was going to happen. And when Koopmans came, people were standing in the corridor, the hallway and trying to listen, to catch the conversation. They didn’t catch the conversation, but they did catch the tremendous yelling by Arthur who completely intimidated Koopmans.

CG: Arthur had a way of intimidating people.

JM: He tried to intimidate me too.

CG: What do you remember about that?

JM: The Bureau promoted me to staff member and Arthur Burns immediately offered me a vice presidency. I rejected it because he needed help. I think that Victor Fuchs accepted it. Anyway I didn’t want any headaches.

CG: There would have been administrative responsibilities.

JM: And I would have shied away from it.

CG: That’s right. Victor headed many things at the Bureau. When you arrived at the NBER I think that Burns was still in D.C. because he was in the Eisenhower administration. He left the NBER and left Fabricant more in charge. Prior to that Burns was the Director of Research but when he returned Burns became President and Fabricant was made Director of Research.

JM: [I want to say] something about Burns’ teaching. He was an excellent speaker. I took his course. That was the year that Hicks’ demand theory came out. So he used that as a basis for reviewing macro-economic demand theory which includes Keynes, Hicks, and a variety of people who published at that time. It was a terrific lecture course, the first semester. The first semester he didn’t criticize anything. He just reported and did an excellent job. In the Spring semester he criticized everything from A to Z. I was tempted to ask him for my money back. One thing cancelled out the other.

CG: You know that Friedman was a student of Arthur’s when Burns was at Rutgers and Milton has enormous respect for Arthur’s mind although he had a slight falling out with him because of wage and price controls.

JM: Well, the story about Burns and Milton was that Burns was teaching elementary statistics and at some point Milton pointed out that what he was saying was wrong and Burns dropped out of teaching statistics.

[all laugh]

CG: You said something about the famous piece “Measurement without Theory” and the rebuttal, “Theory without Measurement.” What did you think of this, as someone who combined at the NBER theory with measurement? Was there measurement without theory or not?

JM: I felt there was an imbalance. But I couldn’t put my finger on it because I wasn’t familiar with the theory of business cycles. There was no confrontation of theory with empirical measurement. There was no support for the empirical ... theory wasn’t being mobilized to attack the data.

CG: People weren’t distancing themselves from the data and then looking at these very chaotic series. Burns’ books and Mitchell’s books do have nascent theory, but it wasn’t formalized.

JM: It was ad hoc.

CG: So, you begin in 1952 working for Ruth. In 1954 were you still working for Ruth?

JM: I was working for her only.

CG: Did you get to speak to the others or was there a gulf between the senior people and the research assistants.

JM: They were not inaccessible but I was shy.

CG: But you weren’t very shy when you told Ruth that the lines were just a bunch of squiggles.

[all laugh]

JM: And I wasn’t very shy when she didn’t theorize enough, she didn’t put things in a theoretical framework.

CG: At that time did you come across the work of Friedman and Kuznets and Friedman’s work by himself? You must have had a nascent interest in distribution. Was it sparked by their work?

JM: Their work had a very big impact on me.

CG: Tell me about it.

JM: At Columbia my interest was in wage determination. Why do people have different wages? Why were there wage differentials? I was approaching it like a Bureau person by looking at various comparisons—by industry, occupation, age, sex, race. I had chapters along those lines. There was a fellow student of my name Al Patrinas [?]. He didn’t work at the Bureau but he was a very smart guy and we used to discuss our dissertations in progress (I forget what he did). But when I told him what I was doing he said “that isn’t the way you should do it; you should look for a unifying theory.” He also suggested looking up the book by Kuznets and Friedman which was Friedman’s dissertation [Ed.: *Income from Independent Professional Practice*]. So when I read this book it took me back to Adam Smith and then I realized that all the comparisons I was

making had as a basic principle education and age. If you pushed the comparisons, each one of them had underlying differences in human capital.

CG: Would you have used that term, “human capital”?

JM: Yes, I used it immediately.

This is quite interesting. This was like a revelation. That same afternoon we met again after I read the book and I had this principle established written out and shown with empirical data. My sponsor, Barger, immediately said this is “great” and sent a copy to Ted Schultz. I didn’t know there was a whole industry brewing in Chicago involving economic growth and human capital. He [Schultz] was looking at wage differentials over time and I was looking across people. It is basically the same subject. I had tremendous satisfaction writing it. It took me a few days to turn around all the material I had into an organized analysis of wage differentials. I then got the offer from Ted Schultz to come to Chicago for a post doctoral fellowship

CG: What year was that?

JM: 1957. Then I immediately went to Chicago.

CG: So, you were working on this dissertation for a fairly long time and then you saw the light and suddenly all these chapters moved around and had an organizing structure. That is a wonderful thing to remember this moment of seeing the light.

JM: Absolutely. But when I got to Chicago Gregg Lewis took me under his wing proclaiming that I was a labor economist. I was very insulted because labor economics had a very bad reputation as a subject that never touched economics.

CG: Because it was institutional, labor unions.

JM: Right. I think that Samuelson made some crack about labor economics “the last refuge of the scoundrels.” I was taken aback when Gregg Lewis told me I was a labor economist. I never took a course in labor economics.

CG: So, prior to that you thought of yourself as an economist who knew theory. How would you have described yourself?

JM: Applied price theory—whatever the topic might be. But then the book by Milton on the Consumption Function was a tremendous revelation as well—how to do empirical research. Very different from what the Bureau was doing. Setting up your model and milking the various implications and the principle I got out of that was that empirical work isn’t just estimating a parameter but drawing out implications that are apparently completely unrelated but are caught in the net of that particular model. And that has been my procedure ever since.

CG: So, you go to Chicago and Gregg takes you under his wing and declares that you are a labor economist. Then what happens?

JM: Then I decided to sit in the courses of labor economics. Gregg Lewis was very enlightening, the way he tackled various questions and Al Rees taught at the same time. And Al Rees spent that particular quarter reporting on the book by Clarence Long.

CG: I'm glad we have gotten up to that part because Clarence Long was part of the NBER and he was very different from what was generally done at the Bureau.

JM: He was different but his empirical work didn't have a theoretical background. It was interesting, very interesting. One of the questions that Al Rees kept coming back to was the labor force participation of women because there was this paradox that if the husband's income was high the woman was unlikely to be in the labor force but over time income grew, for both husband and wife, and there was an increase in participation rather than a decrease. Neither Gregg nor Al Rees had answers to this question. They kept on coming up with answers that partially illuminated the story but basically they did not have an answer. Again, there was one particular day when I was sitting in Al Rees' class and I asked myself "what about taking two variables rather than one?" We had income and prices or wages. Shouldn't that tell what was going on? It could if the price effect was stronger than the income effect, but then you have to bring in the family. So after a few weeks I had a rough model and I looked at the data and it looked very promising. So I went to Gregg Lewis and I told him what my approach would be and Gregg didn't take long to say "this is the right approach." He immediately invited me to a conference, NBER, which took place at Princeton in 1960.

CG: That was the 1960 volume.

JM: Yes. Clarence Long was my discussant and he was the most complementary discussant I ever experienced.

CG: He was a very nice person.

JM: Yes. In general everybody was very positive about my findings. Everybody was happy about them. In fact, when I finished I got a standing ovation, I mean applause.

CG: Any reaction would be very good. Now, we are going to come back to the NBER in part because you then come back but I want to point out that you go to Chicago in 1957 and Gary goes to Columbia in 1957. Here you are the two people who will come back together. ... You hadn't met him at Chicago. Had you met him before?

JM: No. But his reputation preceded him. I was told all kinds of stories about Gary, about his approach to demographic economics, something completely due to him, and the fact that he had already written two published articles while an undergrad and the other fact that when Gary applied to Chicago for graduate work Viner, who was his sponsor, wrote a letter to the people at Chicago saying "Gary is the best student I have ever had" which insulted a lot of people.

[laugh]

CG: One has to be very careful.

JM: Anyway, I almost stayed at Chicago because ... I was made an offer during that fellowship year but I ran into competition with Zvi Griliches.

CG: I'm glad that we are bringing up Zvi's name because I wanted to do that. Tell me about that.

JM: Zvi didn't believe my dissertation at first and then he didn't believe that I would get an offer with approximately the same offer of salary that he had and he protested to Gregg Lewis who was in charge of these matters and it boiled down to half a thousand or a thousand a year

CG: That was a lot at that time.

JM: So Gregg Lewis ... I told him how much I wanted which was something like half a thousand, similar to Zvi, higher or lower I forget and Gregg Lewis said he was constrained, he couldn't offer me more. So, I thought if they couldn't offer me more it wasn't worth my while. I took it as a vote of no confidence. Under those conditions, I didn't want to.

CG: So Gregg was chair of the department? Schultz?

JM: Schultz was chair of the department and Gregg did the dirty work.

CG: Gregg did a lot of dirty work, what I mean is that he spent a lot of time with students, even with those whom no one else wanted. He took all the dissertations and made them one notch better. We're all grateful to Gregg for that. He was wonderful to all of us. So you spent this wonderful year at Chicago. Flora, were you there too?

FM: Yes. I wasn't working full time then. I had finished my residency. I worked for one year at an enormous salary for that time 10,000 dollars.

JM: The work on participation of women benefited from observing Flora.

CG: Very good. Your salary went up, but her salary went up even more so she was pulled back into the labor force. Very good. Then there was this decision, and your mind is thinking about female labor supply and households and how they interact. Then the year is over.

JM: Yes, and we go back to New York. And I immediately made an appointment to see Gary.

CG: You are at City College as an Associate Professor. You had been an Instructor.

FM: An unknown.

JM & FM: I had the longest title, Assistant Substitute Tutor, renewable monthly which I didn't know.

CG: So you come back to NY and you are at City College and Columbia makes you an offer and you go in 1959.

JM: Again I run into competition with Zvi. Columbia made him an offer but he didn't accept. So I owe him two decisions.

CG: You come back to Columbia and renew your relationship with the NBER.

JM: Yes. The NBER promoted me to staff member. I was very eager to meet Gary because I had heard all these things about him. So I met him for lunch and we made it a custom to meet for lunch once a week, each week. The first thing I was curious about was his reaction to my dissertation. He was already planning a project on investment in education. He had an article in 1960. The first thing he told me was that there were a lot of weak spots in my dissertation that could benefit from a lot more work.

[laugh]

CG: I'm laughing because Gary never holds anything back.

JM: It was an eye-opener to me because I thought I had finished and then our conversation revolved around research in that area. There was another NBER conference on investment in human capital where Gary had his theoretical chapter unifying education and job training and I had an empirical part on job training using his theory at that point, and Stigler had economics of information.

CG: Stigler was at Columbia and also at the NBER. Stigler had done nascent work on human capital at the NBER and something on education. There was also his work on wage differentials for teachers and professors, and his work on domestic servants. I say "nascent" because he was asked to do these projects, but his heart doesn't appear to have actually been in it. It was flat.

JM: Didn't have much of an impact. Incidentally, Stigler was my ally. It should be known that Stigler never complimented a student for his work. He always looked for criticism. He was on my dissertation committee. He read the draft of my dissertation. Ruth Mack told me that George said it was a "very good dissertation." He wouldn't say it to your face.

FM: He did 50 years later.

JM: In 1991, when I got an Honorary Doctorate from Chicago, we had a big party at Bob Fogel's apartment. George came over and said "that was a great dissertation." During the defense of the dissertation one of the people on the Committee was a mathematical statistician named ... I forget the name ... he immediately launched an attack against me saying that he didn't find any tests of significance in any of the statistics. First of all I was using Census data. But even if I were using a sample it would always be significant because of the large sample size. George

said “when you take on a dissertation you should read it first.” The guy got so insulted he walked out.

CG: So you come back and there was a veritable human capital team meeting up at Columbia and also at the NBER. Gary had a position at the NBER, you are just getting a position at the NBER, Stigler is at the NBER. Victor Fuchs is at the NBER. Who am I missing in 1961? Tom Juster?

JM: Finis Welch, Sherwin Rosen. Sherwin was in NYC for a while—a year, then he would come during the summer. I was just about finishing my book *Schooling, Experience, and Earnings* and Sherwin was the main reader for the NBER, also Welch. There were lots of discussions going on.

CG: But that brings us a decade later.

JM: The late 1960s.

CG: This group working on human capital—it constituted the founding of human capital at the NBER. You were working on these various subjects having to do with education, training, the female labor force, family decisions, all coming together very nicely. Gary is working on similar topics.

JM: It is not correct to say that I was working alone. I had discussions with Gary every day. He was bouncing ideas off me and vice versa. There was one joint article, which got lost. It was on rational expectations. In 1963 there was a memorial book for Yehuda Grunfield. In that book there is an article by Milton on the consumption function where he uses the kinds of weights that Cagan uses, backward expectations and he was trying to get the consumer discount rate out of this. Gary and I asked: “Why go backwards, why not go forwards?” Let’s assume that people on average have the right guesses. That was in the 1960s. We divided the job into two parts. One by me and one by him. I finished my part and sent it to Gary and that was the last I heard of it. I asked Gary where it was and he said he didn’t have it. It took several decades for Gary to find it in his files. He found my part and he sent it to me. But it was no longer of interest because Bob Hall had done something similar. That was the only article we tried to write jointly and it didn’t work out. There is some message there. I don’t know what.

CG: I take from reading the annual reports that Gary was made the head by Sol Fabricant of the project on education and you had money from Carnegie.

JM: I don’t think I got money.

CG: I think you thank Carnegie in some of your papers, maybe for research assistants. Every year people, such as Gary, write in the annual reports that they thank a foundation and they promise various things. Every year they promise the same thing. There weren’t many people he [Gary] reported on. For example Al Fishlow who was doing an historical study that Al never really finished. Tell me a bit about human capital at the NBER. Who else was working on

human capital? Did you feel that Fabricant, Geoff Moore, and Burns were pleased about the human capital group?

JM: Fabricant and Moore were open-minded. Burns was a little bit dissatisfied because he felt it was a move toward theory. He felt unhappy about it. Exactly why, I don't know.

CG: Maybe it wasn't his agenda. His agenda was macro and business cycles.

JM: Maybe that is right.

CG: Who else was there?

JM: I mentioned before Sherwin Rosen.

CG: Do you remember Tom Juster having any importance in the group?

JM: Tom Juster was doing consumer expectations. It is interesting. In 1960 when I was appointed to the staff I replaced a statistician name Millard Hasting. So my initial task as a staff member was to help people who did statistical procedures. That's how I got involved with Tom Juster. He was doing things on consumer expectations.

CG: You edited a volume on *Economic Forecasts and Consumer Expectations*. That seems a little odd. Right?

[laugh]

JM: Yes. It came out of consulting at the NBER including students of mine like Stanley Diller on the term structure of interest rates and Roseanne Cole on measurement of GNP where she was looking at the revisions.

[CG turns off tape and gets a new one]

CG: I'm back with Jacob and Flora Mincer and some delicious apricots and we are into the 1960's and the beginning of human capital. This was a vibrant time, a change at the NBER. There was still the business cycle shop, people working on productivity, but now there is also a group working on theory with measurement, training, education, human capital, the labor force, bringing people in because of money from the Carnegie Foundation. You had wonderful students at Columbia. Think back and tell me how often you went to the NBER.

JM: We [Gary and I] went everyday. Everyday that was free of teaching, but even days when we taught, we often went to the NBER. This was a wonderful period and we had conversations with the group everyday.

CG: When you say the group, this is you and Gary. This is around 1964. Who else is in the group? Is Victor in the group?

JM: Victor wasn't doing work on this precise topic. He did it in terms of health economics, but it was part of human capital too. I mentioned before Sherwin.

CG: Then there are students. Who do you think about in the 1960's as being the students who were working at the NBER?

JM: I should have that list.

CG: Reuben?

FM: Even Listerly (?) from the conference.

CG: Yes. Bob Michael?

FM: We were living in Mount Vernon at the time and we went on our five-month trip in 1964 with the three children to Israel, Greece, Johannesburg, and South America. You [Jacob] actually went a month earlier to Argentina and then we met at the airport. It was a double purpose. One, to see the Diaspora of Jacob's family and the other, to coordinate it with meeting economists, and working and teaching in different places.

CG: I see. Very meaningful trip.

FM: We took a boat across the South Atlantic from Capetown to Rio.

CG: So you never saw Kuznets at the NBER then?

JM: No. Kuznets was at Hopkins and then he went to Penn. He had contact with Dick Easterlin.

CG: Did you see Dick at all?

JM: Yes.

CG: Did you talk to Dick about human capital, demography?

JM: He was more interested in demography. He was not quite sympathetic to the framework that Gary and I used on the family. But he was interested and interesting.

CG: In many ways, it was you, Gary, students, and a couple of visitors. What is interesting is just how large a change there was at the NBER on the basis of two large figures.

JM: It was basically an addition to what they [NBER] used to do before because they didn't stop.

CG: Right. Did you get together with others at the NBER at the time and was there any cross-fertilization of ideas? I'm trying to understand how the NBER was functioning then as this business cycle shop and other group. Did you get together? Did you have lunch with various people? Did you meet Arthur?

JM: We always had lunch in a group and discussed all these things. At the moment, I don't remember all the names.

CG: Did you talk a lot to Fabricant about what you were working on? Did he give you ideas or was there somewhat of a divide between the people who did business cycles and productivity and human capital?

JM: Fabricant was interested in my labor supply studies. I was interested in his productivity studies, so we used to discuss these things. What came of it, I don't know. Zvi Griliches spent a year there.

CG: Before we end, let me just ask you one other question. We're in the 1960's and big changes are going to happen at the end of the 1960's at the NBER. You were interested in what you were doing, what Gary was doing, what this group was doing. I don't know if you took a larger interest in what was happening at the NBER about the fact that there was a sense that Arthur was losing his hold on the NBER and its direction. Were you concerned and interested in that?

JM: I felt that the NBER was losing its character. It was going to be doing what all the other economists were doing in various universities, except they're going to try to monopolize the market. To me, it looked like the end of what was characteristic to the NBER.

CG: How would you define what was characteristic of the NBER?

JM: Large-scale empirical work.

CG: Also, many people have told me and in recollections that I have read, that the beauty of the NBER was that you could work on what you wanted to work on and for a long time. It wasn't like working in the government or a private concern. It was serious.

JM: Yes. Projects could go on for a very long time.

CG: Right. Something is happening and you sense it. There's something fading at the NBER. How did you sense that? What seemed to be the problem?

JM: It simply meant that the NBER was going to consist of individuals doing their own work and there was no plugging in into something bigger. The style would be model building and both theoretical and econometric. In other words, there was no unity anymore.

CG: You date this somewhere around 1966?

JM: No. It was much later.

CG: If it's later, it's after Burns leaves. Burns leaves in 1967, 1968. This was the transition to John Myer. Did the switch mean anything to you or was it meaningless?

JM: We felt it was the end of the NBER as we knew it. You were on your own. The NBER is helpful as an overhead infrastructure, but there is no such thing as NBER style, purpose. It just became individualized.

CG: This is with the transition to John that something was lost. Then we move to John and something was perhaps further lost. It's an interesting transition.

JM: John Myer was an imperialist trying to expand the NBER to its maximum, not in terms of content.

CG: He had grand visions. He is an extraordinary person, an entrepreneur in many ways. He has had many different scholarly lives.

You have been extremely helpful Jacob.

JM: Not really because I don't remember a lot of things.

CG: Nobody remembers everything. That's the point. It's a collective. That's what the NBER is. No one's an individual at the NBER.

[laugh]

Let me end by thanking you very much and Flora for having me in this beautiful house.

Flora wanted me to take some of the wonderful pictures in their house. Tell me a little bit about the picture I am focusing on.

FM: It happens to have a cat skull which I got from my father. It went through the family.

CG: That is Felix Domesticus?

FM: Well it's a cat skull that was in the family. Not our own cat, just a skull. It's the painting I like but it also has the connection.

[camera moves to another painting]

I like this switch.

CG: A piece of very interesting realism. Should I follow you around?

FM: Just in here.

[moves to kitchen]

These are his self-portraits. This is my favorite self-portrait.

CG: So tell me his name.

FM: Daniel Mincer. He was probably around twenty here. This was after his heart operation.

[camera moves to another painting]

This is one of our family favorites. He did all of them. Since you were taking the ones in the other room which are less unusual, I wanted you to have a little bit of this.

CG: I like this very much.

[CG turns off tape]