Chapter 3

Herbert Gintis

The interview was conducted on March 15, 2002 at his home in Northampton, MA.

You've been at the center of change in economics for quite a while now.

I must say I don't feel like that. I feel as though everything is happening around me, and I struggle to keep up. Sam Bowles and I were movers and shakers when we were doing contested market and labor discipline models. But that was ten years ago, and it really has slacked up. We still love contested exchange theory, but nobody else does it, so I think we failed there. I believe the main reason we failed is our reason for doing the research, its policy implications, never panned out. We did contested exchange because we thought we could make a really strong case for having labor managed firms, but after working on it for a long time we realized we didn't have a strong case. Of course, the models and theory are intellectually and scientifically fascinating, and it does apply to land reform in some less developed countries, as well as to the support for small-scale entrepreneurship in the industrialized countries. What we are doing now—evolutionary biology, game theory, and experimental economics—is something that is happening all over the place.

Can you tell us how you got into economics?

In college I knew nothing about economics. I was in a special program at the University of Pennsylvania for students who had scored high on their SAT tests. In this program we didn't take normal courses. I took only one history course as an undergraduate, and no English courses. I studied mostly math, French, and Spanish language and literature. I was in the program from 1958 to 1961, and one of those years I was in France. I also taught Calculus I, a regular undergraduate course, my last year there, which paid my tuition. The bottom line is that I never had a social science course, and so I knew nothing of economics. I didn't know what "income" was when I went into graduate school in economics; I did know what an "income tax" was, however, because my Dad always complained about paying it.

After graduating from the University of Pennsylvania I went to Harvard to get a PhD in math. I was very political, and as I was writing my dissertation in mathematics; it seemed like I was schizophrenic. I spent hours and hours writing a dissertation, but what I really was doing was spending time organizing for social change in poor communities around Boston, and participating in the anti-war movement. So I decided to switch out of math with just a Master's degree. A friend of mine owned a sandal shop that he ran in Harvard Square. I took over his shop and became a sandal maker and hired some girls to make handbags. It was okay, but I wasn't really into feet, so after doing it for a while I decided to go back to school. I asked a friend of mine, Chuck Levenstein, who's an economist, what should I do. He asked me if I were a Marxist. I said yes, although I didn't really know what that meant. I had read C. Wright Mills, and some writings by

Marxists, so we were radical Marxists. So he said, well then you should do economics because the economy determines everything.

So I took off from making sandals one day (I told my assistant to mind the shop), and in June of 1963 I went to Littauer Center (the economics department) in my leather work clothes—a bearded, long-haired hippy if there ever was one. I wandered about but nobody was around since it was summer time. One door was open, there was a guy named James Duesenberry, so I walked in and said that I wanted to study economics. He asked me why, and I told him the story about Marxism and the economy. He said that he could get me into the department with no problem, because it was a transfer from one department to another and the economics department loves mathematicians. But he told me to read an introductory textbook before I started graduate school, to see what I was getting into. I asked which one, and he said Samuelson. So I read Samuelson and came back. He asked if I still wanted to do economics and I said sure. He said that he thought I was going to find out that studying economics is different than what I was thinking it was going to be, but okay. And that's how it happened.

At first I was very hostile to becoming an economist. I felt like I was not one of these people. How could I tell my artist and hippie friends that I was studying to be an economist? No way. My attitude was that I'm not really an economist, because I care about consciousness and culture and all of that. Then I got totally socialized—to the point where I prided myself in being able to read the national income accounts and knowing what errors in variables means. It took me some time, however. Now I am at the point of bristling when people bad-mouth economists. Who would have believed it?

What graduate courses do you remember?

I don't remember a lot about my classes. Harvard teachers were not well known for caring very much about teaching. They were better than my teachers in the Harvard Mathematics Department, however. Howard Raiffa taught me game theory; he was very good. I was so naïve; I got straight A's my first year when I was a graduate student and then I got B's my second year. I realized later that in my second year I really understood what was going on, and I wouldn't give the profs the answers they wanted. I was obstreperous, even then, and haven't changed much since.

To show you how naïve I was, in my first year, I took International Trade with Gottfried Haberler. I took it because I wanted to study imperialism, so we were halfway through the course and we had read all these abstruse general equilibrium models, and I could throw around all these input-output tables. But we hadn't gotten to imperialism yet. So I raised my hand in class and said, "Professor Haberler, when are we going to study imperialism?" He looked at me and said, "This is not a course in 19th century British history." Everybody in the class laughed. I hadn't a clue why.

Who else were in classes with you at Harvard?

Deirdre (then Donald) McCloskey and Tom Sargent were in classes with me. I was on the Left, though, so I didn't have many friends in the department. My fellow graduate students were more traditional, but they didn't dislike me, for the most part. For instance, they elected me president of the graduate economics club, where I could stir up a lot of trouble. During the Vietnam War, because of the popularity of the anti-war movement, almost everybody was on my side. But I didn't have anyone to bounce ideas off of. Sam Bowles was in Nigeria, so he wasn't around when I was a graduate student. He came back my last year. We got to be good friends right away, and then we formed this group to teach a radical economics course, Soc. Sci. 125. The group of teachers included Tom Weisskopf, Arthur MacEwan, Stephan Michelson, and Richard Edwards. But they were all older than me; they were assistant professors.

You entered the department already radicalized, so the department didn't radicalize you at all. Did you have friends in other departments?

My friends were SDS people, political people, and counter-culture people, but some were students, graduate and undergraduate, at one of the many colleges and universities around Boston and Cambridge.

What was your dissertation topic on and how did you pick it?

My dissertation was called "Power and Alienation." It was a critique of the principle of exogenous preferences in neoclassical economics. I said that preferences are created by the system, and then the system satisfies the preferences; so you couldn't justify the institutions of capitalism solely on the grounds that they satisfy preferences. It explored first how the system works, and second, how one can possibly justify, in a normative sense, the satisfaction of needs if the needs are created endogenously to the system. A lot of it was about preference change and preference analysis. It was very abstract general equilibrium analysis. When Sam Bowles came back we started working together. He was a big shot—a Professor who had a grant with labor economist John Dunlop. Sam put me on his grant, and said that I should focus on how I could prove my hypothesis.

Talk about the way people change over time! At the time I wrote my dissertation I just wanted to do theory. Now I am totally the opposite. I still do theory, but mostly I want to see the evidence, and I spend 99% of my time gathering evidence, paying people to gather evidence, and supporting people who want to gather evidence. How can you have decent theories without good evidence? Clearly, it seems to me now, the Achilles heel of the social sciences is their inability to perform controlled experiments, making it virtually impossible to formulate and test sophisticated theories. But at that time my interest was totally on theory. Sam wanted me to at least *think of* how I would prove my radical theory of endogenous preferences. He also argued that it would be good for me when I went on the job market this. He said, "Why don't you do something about the educational system? At the time everybody was into education, and he had been writing extensively on the subject of educational resources. Sam and he gave me some topics to think about. But I came up with one on my own, which was to show that you could not account for the contribution of education to earnings by looking at the cognitive performance of students, so personality variables must really be what the schools are producing for the capitalist workplace.

This was also was the focus of our book *Schooling in Capitalist America*, which we published in 1976. It's the docility and the punctuality and all this stuff that really affects wages, we argued. That's why people pay more for educated workers, and not the cognitive stuff. We argued against people who said skills are unimportant (e.g., Harry Braverman and Ivan Illich), but we said they are produced in such abundance that their shadow price is virtually zero (no longer true, probably). I analyzed a number of empirical studies of grades and cognitive test scores and wages for my dissertation, which I finished in 1969. Arthur Smithies was the head of my committee; I chose him because I wanted a guy who wasn't going to interfere with me. Sam Bowles wasn't around when I finished. I don't think Smithies ever read my dissertation, but he gave me a few pep talks.

My dissertation was very fruitful work for me; it came out nicely. And it's still on the mark. Sam and I have a paper in the *Journal of Economic Literature* (*JEL)* that was recently published with Melissa Osborne, (this is thirty three years later) substantially vindicating our earlier analysis, and in many cases more strongly then even we believed at the time. The data is much better now. We even have something like a smoking gun on what the personality traits are that affect wages, but we still don't have a lot on how education affects wages. We need a lot more data.

Were you heavily involved with the Review of Radical Political Economy (*RRPE)*?

Definitely. I don't remember a lot about how *RRPE* started, but I was part of it. I was there all the time. There were a huge number of people involved. I used to go to the conferences all the time and I was on the editorial board. But professors weren't the only ones--it had a broader base. The *RRPE* journal was not "peer reviewed," any *URPE* member could run for the board of the *RRPE*, and many did.

Then you moved to the School of Education at Harvard? Did you think of other places?

Since I was a radical, I was precluded from getting jobs in many economics departments. In fact, the head of my department, Richard Caves, sent out an unsolicited letter to the schools I applied to saying that I was a dangerous radical. One of those letters went to a friend of mine who was on the faculty at SUNY-Stonybrook, and he gave me a copy. Once in a very vituperative confrontation with the faculty, when the Harvard Visiting Committee, which included Andrew Brimmer and other big shots, was there, we graduate students complained about the rigidity of the economics department. At the appropriate point, Zvi Griliches (someone I really liked, who unfortunately died last year) was saying how the department is unbiased. At that moment, Rick Edwards, who was a graduate student then, gets up and says, "How can you say that? I am going to read to you this document that Richard Caves, head of the department, sent unsolicited," and then read the letter about my being a dangerous radical to the Visiting Committee.

So I didn't receive any job offers from economics departments. I did not consider departments outside the top ten. I received offers from education schools like Berkeley. At one point James Duesenberry, who was on my dissertation committee, told me that he thought he could get me a job at one of the SUNY schools. I told him to forget it, and indeed acted offended. I was a total elitist. There was no way I was going to teach and an un-Harvard-like place. Harvard makes a list of its so-called good students who are on the job market that year, and passes them out to the top ten departments, and I was not on that list. So I didn't get any good job interviews, much less offers, in economics. My good job offers were in education departments. I'm very pissed about that. They didn't treat me fairly. I was a bit unruly, but a really mature department would have acted differently.

I was very pissed at the Harvard department. I did not like those people. In fact, I did not know that academics could be pleasant until I went to Princeton. I was at the Institute for Advanced Study in 1977, and the Princeton faculty members would invite me to dinner, or lunch and say, "let's talk," and they were very friendly. It hit me like a ton of bricks that not every university was as bigoted as Harvard. I am sure it was because of the Vietnam War. We were occupying buildings, and a lot of them were working for the CIA. That included Arthur Smithies, my advisor, who was doing a crop analysis for the CIA in Vietnam (this was discovered when anti-war protesters "liberated" University Hall).

Did you have any supporters at Harvard? How about Ken Arrow?

Ken Arrow wasn't around a lot, but he's always been a big supporter, then and now. He is one of the most completely wonderful people I have known. When I was on the faculty later, he was at Harvard. I was eventually an Assistant Professor and an Associate Professor at Harvard in the Economics Department. But he was not there then. Albert Hirshman and Kenneth Galbraith were there. It has certainly been a privilege to know these great men. They were supportive but they are nobodies—zeroes—as far as the Harvard Economics Faculty was concerned. Galbraith has no power whatsoever. Albert Hirshman and I became very friendly and my wife became friendly with his wife, but those would be the only two who were supportive. Even Duesenberry never supported me, though he was always friendly and engaging. It was extremely difficult for Harvard faculty members in that period. They lived through the 'fifties and they never heard of anybody doing stuff like we were doing, It was totally unheard of. In the 1950's nobody even had a beard, so it was very hard to accept people like me.

How was your experience in the School of Education?

It was great. Sandy Jencks was there, and I worked with him. I'm still friendly with him. He's now at the Kennedy School. David Cohen was also there. They were both very nice, and they gave me a lot of room. They were tolerant of my idiosyncrasies—probably thinking I'd grow out of them. Sandy did a great book on inequality, and made me a coauthor. I was the one who tipped him off about path analysis, but I certainly didn't deserve to be a coauthor of Inequality.

Did you keep contact with the Economics Department during this time?

I tried; I still wanted to be an economist. I didn't think of myself as an education person at all.

How did you move back into the Harvard Economics Department in 1973-74?

The year Sam Bowles came up for tenure, I was appointed Assistant Professor in the Economics Department, and that was because there were, by that time, strong pushes by four people--Ken Galbraith, Albert Hirshman, Ken Arrow, and Wassily Leontief. Richard Caves told me the year before that I would be a member of his department "over my dead body" (yes, his very words), But he lost. This Gang of Four argued that Sam was a good economist, and while one may disagree with him, it was ridiculous not to reappoint him for political reasons. I think the Department threw Sam's supporters a bone by giving me an appointment. When Sam didn't get tenure they threw the four of them a bone again and they promoted me to Associate Professor. The view was that Sam's gone, so we'll promote Herb.

At that point I had a very agonizing choice to make, which is that I could have stayed there for another six or seven or eight years as Associate Professor of Economics until they threw me out, or leave. Meanwhile Sam was working out an alternative, which was to come up to the University of Massachusetts and make a radical department. I was always conflicted by the choice, because I really did not want to leave Harvard. I did not like most of the professors, (besides the Gang of Four, I thought Duesenberry was wonderful, and I liked Robert Dorfman a lot). Basically, I saw the department as a pusillanimous bunch. I didn't get along with them, but I really liked being there; it was intellectually great, the students were totally marvelous. I just last week gave a lecture at Harvard in the Biology Department. The audience was mostly undergraduates, but after about five minutes I forgot they were undergraduates—they were just fellow intellectuals. So I was very conflicted, but I just couldn't pass up setting up a department at the University of Massachusetts.

How did the opportunity to set up a department come about?

Actually if you want to find out some of the details, there was an article in *Science* that went through all this in 1977. *Science* did a four-page piece on the U Mass Economics Department. I was, at that time, at the Institute for Advanced Study.

The University of Massachusetts used to be a cow town, an agricultural extension college. In the late 1960s and early 1970s it received a lot of money, and they hired like crazy. The economics department was just this backwater. One day Hugo Sonnenschein, a recent Ph.D., wandered into the office of the dean and said: "I want to set up my own department, and if you allow me to do this I'll wipe out the dead wood." The dean said okay since that's what they were doing with a lot of departments. So Sonnenschein hired a bunch of hot shots, really good economists, including Vernon Smith, Ron Ehrenberg, Ron Oaxaca, and Jim Cox.

There was one young guy there among the old guard, named Mike Best, who was an SDS person. The students loved Mike, and couldn't bear the mathematical types Hugo brought in. Hugo and his crowd tried to get Mike Best fired since they were cleaning out all the old guys. That was a mistake, and the students were up in arms about it. This caused so much trouble for the deans that they got pissed off at Hugo and they took over the department. They then hired Sam Bowles, me, and some others without consulting the other members of the department. So we came into a bad situation. We didn't really know the story until we got here. We tried to keep the good people, but without much luck. Hugo was gone before we got here. We really wanted to keep Vernon Smith and Ron Ehrenberg, and their colleagues. They stayed around for a few years after we got there, but then they moved on. The deans were happy. We were the ferrets that had come to clean out the mice in the barn.

They brought in three of you right?

They brought in me and Sam Bowles, Rick Wolff, Steve Resnick, Len Rapping, Jim Crotty, and Rick Edwards, so that's seven. We soon hired a few more, until we were about half the department.

Did you get along with everyone in that group?

Yes, we had two major problems at the beginning. One was that we had a ton of work to do to run the department, and the second was to develop a working relationship with the non-radicals. They didn't appoint us, and they were scared of us because they thought we were a bunch of commies. Eventually, we worked it all out, and had a nicely running department for a long time.

How did your research focus change during this period?

Well, Sam and I spent several years trying to shore up Marx's labor theory of value. We called it "Sites and Practices," and we tried to make it work in the early 1970s. Alfredo Medio, who made the labor theory of value an analytical device, inspired much of our work on this topic. But later we realized we faced two problems. One of them was pointed out to us by Geoff Hodgson, who said "after you make the LTV look pretty and work it out so that it is intellectually credible, you no longer have the labor theory of value, but something quite different. So why call it the LTV?" The other thing that happened was, we seriously, at some point, started studying what non-Marxist economists were doing. We were doing analytical model building, so we started reading the journals. I had done an article called "Capitalism and the Labor Exchange" for *RRPE* in 1976, which basically laid out the view that Marx says that labor is not a commodity and that labor and labor power are different. This was before Stiglitz did his paper on labor and the principal agent model, which was 1984. It said the reason labor is different is that although you buy and sell it on a market, you can't enforce the relationship through a contract—there is no contractual enforcement. The worker comes to work, and you pay him a wage but there is no *quid pro quo*, all he gives you is a promise. So this was really a basis for an alternative theory of the firm-- the idea that there's a market failure, a non-enforceable contract. We took this ball and ran with it, giving up Marxism.

So we started doing that, exactly at the same time that Joe Stiglitz et al. were doing it, but we didn't know that they were doing it also. Sam and I were separated at that time too. I was at Princeton's Instituted for Advanced Study for a year, and then at Harvard for a year. I think Sam was at Berkeley for a year, also. Sam did a paper on this, and I had worked with Tsuneo Ishikawa, who was an economist at Harvard. We wrote some papers on the labor exchange in which we used the basic principal agent model. We got scooped on that because Stiglitz published it first. I still say that if you look back at my article in 1976, on the labor exchange, it said what the problem was quite clearly and laid it out properly. So once we figured out that we could model the nature of conflict between capital and labor in the work process using a principal agent model and incomplete contracts, we just gave up on the labor theory of value. At one point I remember Sam saying to me that in a year we're not going to be doing the labor theory of value at all, and he was quite right. We totally moved away from that. Then we got into doing capital theory.

Didn't this cause fights between you and other Marxists, where they no longer saw you as a Marxist?

The change in our theoretical tools didn't provoke much hostility, but at the same time Sam and I changed our politics. Our new approach appeared in *Democracy and Capitalism*, in 1986. By 1990 I was irate that the traditional Marxist Left in the USA hadn't seen that their politics had failed, both for the advanced and developing countries. I went on a quixotic mission to convince my old Marxist friends that it was time to rethink their political positions and analysis. This caused lots of fighting, and it was quite fruitless. It took me about two years to realize it was time to stop arguing with Marxists and just move on.

Duncan Foley said that his interest in Marxism wasn't necessary political. What he was interested in was developing a Marxian model of the economy, just like a Walrasian model of general equilibrium theory. Did you take that approach?

I love Duncan. We've always been good friends, but I am very different from him. I never liked general equilibrium theory. I always thought it was really the wrong way to go. The modeling approach to Marxism that Duncan loved is what I call mechanistic Marxism--you take people out of it and you put in a bunch of equations. He had a book he loved that I hated, by I. I. Rubin. It was Arrow/Debreu for the Marxist. I never bought that view, and today he doesn't either. His statistical mechanics stuff is much more to my liking.

I wouldn't say politics are central to my economics, but the only reason that I do what I do is because I think it's important in the real world. I'm not a shining moral light. I'm just a normal everyday person; I don't make any claims about my moral or political insights anything special. But I can assure you that there is not a moment when I do economics, or whatever I do, that I'm not thinking about how this is going to make a better world. That's the only thing that I care about. Even when I read math and physics, all I care about is how it helps us better understand, and hence change the world.

One of the reasons I got out of math was that I was surrounded by people who never ever thought about whether things were useful or not. I love to study physics and I study general relativity for fun, but I can't stand math when it's irrelevant to the sciences. I never had the view that intellectual life is somehow a tool of politics. That's why I could never get along with the Italians like Garegnani. For them, intellect is just a tool for communist revolution or something. It's a Talmudic exercise and I've never treated it that way. For me, ideas must be true, and truth must be useful.

Things were evolving in Russia and the socialist systems over this time. How did that change or affect the way you approached economics?

I never had any sympathy for Russian-style communism, or for centrally planned economies. I had a deep horror of the apparatchik way of life. When I read George Orwell I totally bought it, so I was never a friend of the Soviet Union. I refused to have my stuff altered for publication there--they said they wanted to publish my articles in Russian, but would have to make certain cuts. I said: No way. Maybe they did it anyway but I did not give my approval. One of the happiest days of my life was when the Berlin Wall came down, and when communism was overthrown. These were distinctly happy days for me, and I thought the Soviet Union was the biggest albatross for making social change in the West. The reason was that whenever you tried to do anything, they would say, oh you're just a communist. So yes, we liked Euro-communism; that was great because it wasn't communism. We were always very strong on democracy and civil liberties. When Amnesty International came around we were right there saying that civil rights are the tools of workers and peasants, which people can use to make a better life for themselves, and that's what really counts. Our book in 1986, *Democracy and Capitalism*, said that the Marxists were right in that history is the history of struggle, but not just class struggle. It's not about the means of production; it's about freedom; it's about having the dignity and freedom to pursue the things you want to pursue in life--collectively and individually. I can't say that we foresaw the overthrow of the Soviet system--nobody foresaw that. What we wrote in 1986, which was really written in 1984, said that we were totally into the democratic revolutions in Latin America and in the Philippines. We saw democratic revolutions as the wave of the future, and the overthrow of the Soviet system would be part of that. Thank God it was.

Weren't the radicals of the 1960's Marxists? They fought for civil rights.

They were mostly not Marxists at all. Marxists were not in the forefront of the struggle for civil liberties. Marxists hated that stuff. I remember saying liberty and freedom are important, and to Marxists those were fighting words. That's bourgeois crap, they would say. The civil rights struggle, for them, was just a step towards socialist consciousness.

Was that always part of your thinking?

No. I went though a period when I believed that it was bourgeois crap, too. It didn't last very long. I'm never wrote anything that said anything like that. I tried it out. I tried a lot of things. I wrote an article in 1978 in *Socialist Review*, which was a long defense of what we are doing now, which is that the Marxists have this view of culture, which is the same the liberals have, like in sociology, which is that culture is this seamless web of ideas that fit together functionally to make society run a particular way. So the idea behind liberalism is that you have these ideas of due process with civil rights and individualism and all of these fit together seamlessly to make the ideology of capitalism work. So when you say you believe in liberty or freedom then you are buying into that whole seamless structure of liberal ideology.

At that time I was reading a lot of Wittgenstein, and the article was very Wittgensteinian. It talked about tools of discourse and it said culture is not a body of thought at all. It's a set of discourses that people use to communicate and to organize themselves and their tools. When the worker says, "we want our freedom," they are not buying into something. They didn't read John Locke, and they don't know any ideology. All they're saying is that they're using a tool of discourse to get something they want in their struggles. Culture is this web of contradictory intersecting discourses that people use to their own advantage.

The whole Marxist thing that ideology is the ideology of the ruling class is totally wrong, because culture is internally contradictory, self intersecting, etc. Sam and I developed these ideas in *Democracy and Capitalism*, where there are sites like the patriarchal family, the capital economy, and the liberal state. They all have their own discourses but they interpenetrate and people transport discourses from one place to another, where they disrupt the system. So the way you overcome patriarchy is by applying the discourse of the state, which is due process and equality before the law. In short, we were very strongly pushing for using the liberal discourses because they can lead to emancipation when applied to the economy and to the family.

Reading *Democracy and Capitalism* you get the impression that was the time you and Sam where breaking away from the Marxists. Was that your statement of breaking away or did you consider yourself to be a Marxist at that time?

I don't remember. I think at that time we said we weren't Marxists and we weren't liberals. Things just happened very gradually. Where does it get you if you call yourself a Marxist or a non-Marxist? It's just a bunch of words. You don't get anything out of doing it, so I don't think we ever did so. I don't think Sam Bowles has ever said, "I am not a Marxist." Maybe he has, I don't know. With the development of post-modern Marxism, Sam and I basically stopped doing Marxism. We were now doing principal agent models; we were perfectly happy as clams doing neoclassical economics, or what we called non-general equilibrium neoclassical economics with endogenous enforcement and information. We were the left wing of the Joe Stiglitz camp.

After the fall of the Soviet Union the whole Third World Socialist Movement totally fell apart. We found was that the countries that were doing well were capitalist, like Korea, Costa Rica, and the Pacific Basin. We read all of this, and read *Monthly Review,* which was the organ of third world socialist development. It all failed, it totally failed. Cuba is a disaster; it's a joke. So how can you go on and write the same stuff? How can you have the same politics you did before?

Some people just write the same stuff again, they don't care. They're really interested in the intellectual stuff. Other people, like Rick Wolff and Steve Resnick in my department, go into this post-modernism. I am a serious intellectual enemy of post-modernism in any form. I think it is an abdication of our scientific responsibility to find out how the world works and use it to make it a better world. The post-modernists hate science and they can't do math. All they know is words. People who want to understand the world have to be able to do both math and words. I may not be the smartest person in the world, but I do both math and words.

I was very upset at the takeoff of post modernism. All of a sudden the Leninists have become fuzzy-wuzzies. That's when I went off and said: Okay we have to stop being Marxists because it's not getting us anywhere. It lost and if you lose you go home and try something else. What these guys do is that they lose and then they gather their wagons in a circle and they lick their wounds until they die, like the old WWI. They get together every year with banners and hats and become totally irrelevant to real politics. They simply make themselves happy. It upsets me that these smart people, who were so dedicated to social change, just opted out and started doing what was fun for them. That's when I said, besides not being Marxist this stuff you are doing doesn't get you anywhere.

Your approach to problems has to fit the times. Consider unions; the left is in favor of unions as if it's 1920 again. Social change is probably not going to come through unions. The union movement in the US has had its day. Thinking that you are going to bring about social change by supporting the teachers' unions, the AFL, and the Teamsters, and other labor aristocracies is way off base. Luckily, I think the new left in the United States is not like that. Younger people on the left now have a very different attitude towards politics. Politics is a personal moral statement: we don't like inequality and we are going to protest it and we are going to try to be on the side of the poor and the weak. I think that's a fine thing to do.

I can't do that because I still have in me the old Marxist idea that what we are looking for is a systemic change. We're looking for new ways to organize systems, and we want a better world for everybody, and it doesn't come about by just fighting on the side of the weak. You have to have new ideas for reorganizing social institutions. I'm radical in that sense. I still believe that devising alternative institutions is the only thing that is really interesting to do, but I'm conservative in the sense that I realize often that it is hard to do that. Let me put it this way: there are so many things that we know would make a better world for most people that it's easy to be optimistic, even if my goal of institutional alternatives should not pan out.

After you left your Marxist period, and made your peace with the mainstream, you moved to what you once called a post-Walrasian period. What does that mean?

No name has ever caught on. I don't know what to call it. First of all, Sam and I do not consider ourselves heterodox. We don't like the idea of heterodox; we like the idea of homodox, which is if you are a scientist you believe that there is a truth and you should work out what that is and that people who have legitimate disagreements with it should be listened to. You should adjudicate that, and economists shouldn't say more than they know. By the way, most "mainstream" economists consider me a shocking radical. They are wrong, however. I'm just a scientist doing his work.

The hypothesis we have in the introductory chapter of this book is that the profession has become more open since you first entered the profession. When you started, the reality was that if you were outside of a framework that would be called neoclassical, you were heterodox, but now there is much more exploration. Is that consistent with your view?

Yes I think so. Things change. People that were, I wouldn't say enemies, but certainly way far away from us in 1975, are really good friends now. I recently told Joe Stiglitz how much I admire him. I really think he's so exemplary, not just because he's smart but because of what he did in the Clinton administration, and what he did at the World Bank. When he won the Nobel Prize all he talked about to any reporter who asked him questions was that the real problem is world poverty.

When I first met him, Dunlop insisted on the young radicals getting together with the real smart guys who were not the radicals and Stiglitz was one of them. So we got together for lunch at the Harvard Faculty Club. It was a little strained at first, but we had a nice talk, and I finally said to Stiglitz, "listen Joe, do you really believe that the interest rate is the intersection of the supply and demand for capital?" I said exactly that, and he said, "Yes, I believe that." So twenty years later I said to him, listen Joe, you really threw me a curve ball when you told me you believed this. I thought if this is what the smartest people in the profession believe I'm not going to even talk to them; meanwhile he totally revolutionizes capital theory along the lines that supply and demand don't determine interest rates! Either his views changed or he was being "in your face" to me. I would say the same thing about George Akerlof, who is a much calmer person than Joe. George helped Mike Reich get his job at Berkeley. We weren't really friends when we were younger, but now we are really quite close. George is in my research group and I think the world of him. Sam and I have gotten closer to the profession, and the profession has gotten closer to us. They totally accept the work we do. But there's still a lot of tension when we complain that the profession isn't moving fast enough in what it teaches undergraduates and graduate students.

When the MacArthur grant I have with Rob Boyd, an anthropologist, came up for renewal, they sent our proposal to three economists and one non-economist to evaluate whether it should be renewed. All the reviews were unbelievably superlative. This was a dream—reviewers are always s.o.b'. They said that the work that we did, and the work that we were going to do was totally outstanding work. However, they were angry at the tone that I took in writing the proposal. Three of the four were angry because I said economics believes in rational actor models, which they vehemently denied. One called my description of the economics profession "snake oil salesman" tactics. They said that the economics profession is totally open to new ideas. MacArthur didn't ask me to rewrite the proposal. In fact, most non-economists agree with my position, which is not that economists are closed minded—they emphatically are not. But they consistently choose textbooks that teach material that they know is false and/or completely out of date.

That's one of the reasons we are doing this book, because we don't think people have the right view of where economics currently is.

You can't write all economists off as ideologues, because they're not. They're open to new ideas. However, there's still this incredible tension in what we teach. I am so displeased at the way undergraduate and even graduate economics is taught. Undergraduate economics is a joke--macro is okay but micro is a joke, because they teach this stuff that you know is not true. They know the general equilibrium model is not true. The model has no good stability properties, it doesn't predict anything interesting, but they teach it. The production theory that is taught is also a joke. They use this old Marshallian production with LRACs and MRACs to determine firm size. This doesn't determine firm size, it determines plant size. Totally different things determine firm size. So why do we teach undergraduates this? Why do you teach income and substitution effects and Giffen goods, when there are so many interesting things to discuss? So I am so upset by what they teach. I am retiring from UMass this year (Sam is, too), so I won't have to deal with this anomaly any more.

If this were physics or astronomy, when they get new ideas at the forefront, they immediately teach them, but in economics they teach the stuff that even thirty years ago, people didn't believe. My view is that economists should not be so tolerant of teaching out-of-date ideas. Micro is a total disaster. So I think there's still a tension, and that there will be one for a long time. I guess sometimes people treat Sam, me, and all these people who do behavioral work, like they treat the dentist, it hurts but you should go do it.

Is the reconciliation with people you shared tension with twenty years ago because you changed or they changed or some combination? What has allowed you to feel more comfortable with these people?

If you dispassionately and seriously study the world, you should come to similar conclusions about how the world works, whatever your values or political commitments. So when Joe Stiglitz looks at the world over a period of years, he comes to see it more the way it is. And when we look at it more and more, from very different starting points, we come to see the way it is. So it is not surprising that we have come together. What's surprising is in the social sciences it appears to be acceptable for different disciplines and different branches of disciplines to have totally different ideas and not be bothered by it. Sociology has one way of teaching the theory of the firm and economics has another, and psychology has another. They all have different theories about the individual, and they seem perfectly happy with that. Well that's totally scandalous, absolutely unacceptable. Why would a sociology department teach things with a very different model of the human actor from an economics department? That's crazy. Of course, everybody has their own expertise--if you are an anthropologist you know a lot more about the organization of simple societies than if you're an economist--but if we study the same thing we should have the same theory. If we disagree, we should look at the evidence and let that decide. I think a lot more social scientists are doing that today. They're not so much ideological today; people are much more driven by the facts; they are more humble about the strength of their theories. So there's a lot more ways for us to agree about stuff.

You might be seeing that change within the top twenty schools but there's really a major demarcation between that and lower level schools, isn't there?

Absolutely, even graduate education. I think if you are real smart you can get away with being deviant. If you get a job at Princeton or Stanford, you can be a deviant because you have a good job and you're smart. If you're at a second rate school, you can't be a deviant. They'll kick you out. No one listens to you because you are not at Stanford, and you're probably not as smart as the guy at Stanford anyway. But it is also true that the way they teach economics, even graduate education, at all the schools is pretty bad. There was no behavioral economist who had a job in an economics department before David Laibson got his job at Harvard, and now Sendhil Mullainathan at MIT. So now maybe it's okay. And they hired Al Roth to teach game theory, who is a great experimenter, and who is started out being the least behavioral game theorist you can imagine, but now is doing behavioral experiments himself. Outside of a few places, people are afraid to do dissertations on behavioral or evolutionary topics because they are concerned where are they going to get hired. They go and give a talk somewhere and people never heard of this stuff. I think only now experimental economics has cracked through.

You are our representative for experimental economics in our set of interviews. Can you discuss where you see experimental economics fitting in?

My research concern, starting from my dissertation, has been to figure out a decent model of the human actor--how people make decisions and how they strategically interact. What we had up until recently, with the experimental economics of Vernon Smith et al. at Arizona (now at George Mason), was just a bunch of statements without any empirical evidence. The reason we tend to lack evidence is that we can't do controlled experiments on humans. You can't set up a world and run it one way and then set up another one and run it a different way. You might be able to do that with monkeys but you can't do that with humans. Experimental economics is to allow us to see how human beings behave in controlled environments, in ways that are scientifically replicable.

Game theory gives you the intellectual apparatus for describing a controlled environment for strategic interactions. You have who the players are, what the strategic choices open to them are, what the information symmetries and asymmetries are, and what some of the payoffs will be, especially the material payoffs. One of the things we've found in experimental economics is that there is not a lot of disagreement about what the experimental findings are. In other words, if someone does an experiment in one place and someone else does it in another place, they find the same thing, if the places are somehow similar. So these experiments can be replicated. Also, there's a possibility of changing the parameters to get better variance. This allows us to model human behavior. So we can change the payoffs, we can change the information; we can add stages to games. It's a very flexible tool for finding out how humans behave. The implications are that sometimes they appear to behave exactly the way economic theory tells you. For example, in a market type setting, they almost always fit neoclassical theory. Indeed, with demand and supply, humans behave even more efficiently than the theory tells you. But whenever you have real strategic interaction, so people can affect one another's behavior by their strategic choices, we find people are much more complex than standard economic theory predicts. They don't conform to any of the standard paradigms.

I should say that the weakness of experimental economics is that we have to be able to correlate what people do in experiments with what they do in daily life. If someone behaves altruistically in an experiment does that means they behave altruistically in daily life? We don't know; we haven't studied it and that's what we are beginning to study now. If people are very sharing in a public goods game, do they vote for income redistribution at the ballot box? How about survey materials? When people say that they are very tolerant of other ethnic and racial groups, that doesn't translate into their trusting them when they play a trust game, we have evidence of that. So there are all sorts of things that we don't know. But as far as I'm concerned, the way we are going is the way we should go.

A very long time ago, Steve Marglin said something in a talk that I never forgot. He said that first of all we should be humble in the social sciences because we don't know a lot. He said a lot of the reason we have these divergent theories is simply that we don't have the evidence to say any of the things either side is saying. So of course we will disagree because there's no evidence that it's either right or wrong. That really hit me. In my recent game theory book, *Game Theory Evolving,* the first quote is from Wittgenstein, and it says, translated into English, "that which you do not know, you should shut up about." Later on I say, that's not exactly true, because you have to be able to formulate your thoughts if you are not sure. But basically the problem of different views is that people are willing to talk about all sorts of stuff that they know little or nothing about. So I think experimental economics is giving us almost a Hubble telescope into human behavior. It's a way of studying something that's never been studied before.

Now we're also trying to mend our fences with social psychology, because social psychologists get very upset with economists' recent entry into experiments. They've been doing this stuff for years, but we say that there's a big difference. The only way to move forward, as far as we know now, is to have a model where you think of agents optimizing subject to their constraints, which is what game theory does. You then vary the constraints to see what it is they optimize. That's not what they do in social psychology. They have hydraulic models or identity models and they never vary the payoffs, they don't even use payoffs usually, it's all implicit in their models. So we say to them that you've got to get your act together; there are a lot of interesting things that should be redone.

For instance, we have a whole project on so-called insider/outsider behavior, which is the study of how human beings tend to make arbitrary distinctions of who are the good guys and bad guys. Then they are partially altruistic in that they will sacrifice on the part of whom they consider the good guys and hate the bad guys. The social psychologists figured this out in the sixties, they did some wonderful experiments that are quite suggestive, but again we're doing it now where you have real monetary payoffs. We think this will be as important as the reciprocity work that we've been working on.

The point I'm trying to make is that what we need is data. We need data in order to adjudicate among theories. All the talk in the world, and all the theorizing in the world, is not going to substitute for having good data to build good theories on. Social scientists have to recognize that they're scientists. Whatever your politics are, and I have very strong political ideas and very deep commitments, they shouldn't affect your work. Politics don't affect the work I do, except to the degree that I work on some things as opposed to others. I never talk about my politics with the people I do research with. I don't care and they don't care; and we get along just fine. People who are making love don't talk about their politics while they are making love; people who play tennis don't talk about their politics when they are playing tennis. We don't have to talk about our politics when we do economics. The people who do aren't playing by the rules, and they probably don't know how to play by the rules. They're probably cheating because they can't play by the rules. If you can't make your argument on the basis of evidence, and you say, well, politically this and that, you are not playing by the rules. That's why I think experimental economics is so important.

So will game theory catch on this time, though it has tried a few times before to become the organizing framework, because it is associated with experimental economics?

Game theory is so central to everything in the behavioral sciences that without it experimental work would be of little use. The auctioning off of the airwaves by the FCC was more important than experiments for game theory. The fact that game theory is used in biology is also very important. The fact is, game theory is not something that is unique to economics; the way it developed in biology is as interesting as the way it developed in economics, and very different, because it wasn't associated with economist's notion of rationality. You don't think of pond scum as rational or irrational.

Do you believe now that your work has been drawing from game theory and experimental economics even though you weren't aware of it?

No. I studied game theory with Raiffa and loved it; his book with Luce was great. I still remember some intellectual experiences I had. I remember being in class with him, and he would say something that blew my mind. But I gave up on game theory. It didn't appear to be going anywhere. If you look at Luce and Raiffa now, it doesn't move you. The reason why is that it's mostly cooperative, not non-cooperative, game theory. The real people who brought game theory back were the gang of four: Kreps, Wilson, Milgrom et al., and other people of the Stiglitz generation, my generation.

Sam Bowles and I came to game theory from Marxism. We started talking about sites and practices. Well, that's the rules of the game and the strategies people use. In our 1986 book, *Democracy and Capitalism*, we never attached it to game theory. One day we got the idea that somehow this is game theory. I started reading all about game theory and I started teaching game theory; no one had ever taught game theory in this valley. So I developed the first undergraduate game theory course in the Connecticut River valley; it may still be the only one. So we came from Marxism.

We never liked classical game theory and the rational actor model; we never thought the economy had anything to do with being self-interested and rational. One of the first things we read was the biologist Maynard Smith's book, *Evolution and the Theory of Games*, which of course is a totally evolutionary book. So I had no problem thinking evolutionarily about game theory right from the beginning. It allowed me to integrate cultural and genetic evolution into my models. So we came to it from a very different place than most people did.

When I was doing my early work I had never heard of experimental economics. I found out about it in 1992 when I was reading *Scientific American*. I'm a total scientist. I read *Scientific American* and *Nature, Nature Genetics, Science, Trends in Evolution***,** and other science journals and magazines. I get important ideas from them. It was there I read an article by Vernon Smith on experiments. Now we had thought we knew what experiments were—you had pigeons operating on supply and demand, and you prove Adam Smith was right. Vernon did this article in which he said just the opposite. He said that if you do auctions, and you change the rules for the auctions, you get very different results. I was totally blown away. So that's when we decided that it was important to do experiments.

We had a friend, a younger Austrian colleague, Ernst Fehr, who was kind of a disciple of Sam and mine. He started doing stuff on fairness. I remember seeing him and asking what he was doing these days. He said he was working on fairness. I thought it was just brilliant. What he has done with experiments has really affected us very seriously. That's part of the reason that I say, I don't feel that I am in the forefront of anything. Ernst is on the forefront; he's the leader. Similarly, when I got to work with Rob Boyd, the anthropologist, when I read his 1985 book, *Culture and the Evolutionary Process* with Pete Richerson, I was delighted. It's just wonderful. I don't agree with the entire book and now we are going way beyond that, but the evolutionary game theory approach they use is a wonderful way to think about culture scientifically. It fits with a lot of other stuff. What we are doing now is bringing all of these things together into one unified behavioral framework. But thinking about how it comes together it's important to recognize that we're learning a lot from younger people.

We also interviewed Ken Binmore, Matt Rabin, and Bob Frank. How would you differentiate yourself from them or see similarities with each of them?

Well, Binmore shares our stress on evolutionary game theory and on evolution itself—the belief that humans have evolved as a species. If you look at his magnum opus, you see this idea as being pervasive. What the original position was for Rawls or Hobbes, or the state of nature for Rousseau, he takes as a historically understandable fact about the nature of human environment. It is a summary of the social and environmental situation that humans were in for their whole species of evolution up until 10,000 years ago. So we agree totally with that. However, up until this year he's been one of the few people to really hold out for the rational actor model, where everybody else has said no. He almost has to take this position because he uses it so centrally in his books. In my view there is almost no evidence that his position is correct; he shores it up in really weird ways. I think he's easing out of doing economics; he's done his magnum opus and now he is done. He's the most conservative person around so in that sense we differ a lot.

Bob Frank did some of this stuff before anyone else. I used his book, *Passions within Reasons*, in my undergraduate classes before I even knew anything about this stuff. I think he's very important. I find him to be a bit dilettantish, that is, he does something and then he moves on and does something else. He doesn't stick to one thing long enough for it to make an impression on the field. The way you make an impression is to do the same thing over and over for six or seven years until you get it right. That's what Ernst does and that's what Danny Kahneman did with his stuff on loss aversion and non-rational choice theory. You just do it over and over, taking care of every objection, and going back and doing it again because you are never right the first time. Bob Frank doesn't seem to follow that approach; he writes this great book, but then moves on and does stuff about inequality and big ponds and little ponds. So I think he's great, but he doesn't stick to things enough to affect the profession. Matt Rabin is a friend and a member of our research group. It is a privilege to know and work with him.

Could you see the two approaches as working together, one sows the seeds for people to go in and others do the tilling?

Sure, absolutely. Matt Rabin is one of us. He doesn't like evolutionary models. He keeps away from that completely, but he's a real innovator on a lot of this stuff. He thought about justice and fairness early on; he takes psychology as being important, and he's brilliant.

What do you see as really going on over the next twenty years in terms of the profession? If we revisit you in twenty years what do you think you'd say?

I would not venture to answer that, but I can tell you what I'd **like** to see happen. I'd like to see a better integration of the behavioral sciences from biology to psychology to economics, politics and anthropology. I'd like to see all the behavioral sciences integrated in an intellectually satisfying way, instead of being fragmented, hostile, antagonists. It's very hard; there's a lot more to do, but I think that's really where we are going to figure out more about humans and how they work.

What do you see as the most important changes in the economics profession in the last twenty years?

The big thing is the abandoning of the neoclassical general equilibrium model as the basic way you think about the economy. Economists tend to use it as a crutch because it's a big whole grand theory and it gives them peace of mind not to reject it. But nobody does it, so they use it for ideological reasons. People are fond of saying, let the market decide, well, yeah, if you have a good theory; if there's unique solutions or stable solutions, you can do that. But there aren't.

So I think the big thing doing more strategic interaction studies of particular institutional settings with labor markets, capital markets, consumer goods markets. In theory we are replacing the general equilibrium model with some notion that what happens is a game theoretic model. You have Nash equilibria that aren't necessarily market clearing or don't have the optimality properties of Walrasian general equilibrium. What's really hard for people to accept is a theoretical construct from another field. They can accept empirical results from another field, but not theoretical results. Game theory is fine because that's economics, but not other theoretical ideas.

One battle that Sam and I lost that I don't think we should have lost is our interpretation of the weakness of neoclassical general equilibrium theory and our sketch of an alternative, which we actually published a couple of times, once in the *Journal of Economic Perspectives* and once in the *Quarterly Journal of Economics*. Sam is now working on a book (Princeton, 2003) that argues that the major markets in a capitalist economy are not *quid pro quo* markets; they're markets with money on one side and something on the other side that cannot be guaranteed, but can be cautiously enforced by a contact. So we have labor markets, we have the wage and then labor, whatever that is. We also have capital markets. A guy lends you some money and you give him a promise to repay it. But what's the promise worth? It's not enforceable in a court of law, unless you already have the money. In consumer goods markets, I give you money and you give me a product. Why is it a good product? To answer that you can get into reputational models. Our work provides a general approach to answering these questions. There is a quid pro quo on one side, but on the other side requires money. You have to have money be on the short side of the market. Money is power, and the people on the short side of the market have power. In labor markets the boss has the power over the worker. In consumer markets the consumer has the power over the producer in the sense that consumer sovereignty means that the producer is always running around trying to satisfy your needs. In capital markets the lender has power over the borrower, because they are all on the short side of the market. These markets do not clear; price is higher than marginal costs. If they weren't, the firm wouldn't want to sell you more stuff. But we really haven't developed an acceptable model that incorporates all these issues.

In my view a very real problem is that these issues involve *power*, and talking about power is not something economists like to do. Game theory allows us to integrate power into the model. When you say the person on the short side has power, it's something very simple. If you're on the short side of a market you have many options and you can get people to do what you want them to do because you have the possibility of choosing them versus choosing somebody else. So you can hurt and help. In a clearing market you can't hurt or help anybody. So we have what I think is a really nice theory here, but economists don't buy it. I'm sure it's our fault, but I tend to say that it's not our fault; it's really that economists don't like the idea of power. They're so wedded to market clearing that if you say that in equilibrium markets don't clear, you're sounding like John Maynard Keynes or something.

I think basically people don't like to take paradigmatic structures and constructs from other parts of the discipline. My comparative strength is to recognize the core controlling ideas in a discipline and recognize their validity and the importance of including them in any final theory of how society works. So, for instance, for most of my life I hated the sociologists' approach that people are socialized to behave in certain ways, because that goes counter to the idea that people make strategic choices, that they evaluate and can change their beliefs. But there is something right about socialization theory, which is the whole idea that human beings have this capacity to *internalize norms*, meaning that certain types of behavior become not constraints on actions but become goals of action. We internalize them unless we are sociopaths. An example is empathy towards others—the desire not to hurt others who don't warrant it. That's something we really learn and we really internalize it. Many of us would rather die than violate that norm by hurting someone egregiously. But it's not present in economics and it's not present in biology. I just wrote an article for the *Journal of Theoretical Biology,* which uses the concept of the internalization of norms, and it's probably the first time that anything like that has happened. It's still hard because biologists really hate sociological theory. My point is that people tend not to understand the core ideas of another discipline and that's sad because we have to bring them all together.

How did you become involved with the Santa Fe Institute? What's your view of its evolution?

Sam and I became involved with the institute through some friends: Larry Blume and Steve Durlauf, and Ken Arrow who is a mentor-friend. Ken Arrow was involved with the Santa Fe Institute from its beginning. He is one of my heroes, because he's a guy who does something, and then moves on. He's moved on from general equilibrium to complexity. Larry and Steve ran the economics program quite successfully for a number of years and they got us involved in the Santa Fe Institute. We went there a lot.

What would you say is the approach to economics at Santa Fe?

There's no one approach. They started out doing econophysics, but have since branched out. Brian Arthur emphasized path dependency, Steve Durlauf did statistical mechanics. Larry Blume is more of a traditional economist but he has worked creatively to expand the range over which you do your theorizing. When their tenure came to an end, Ken Arrow asked Sam and John Geanankopolos to be the new economics program directors, and they trade off. John is one of the few creative neoclassical general equilibrium people around. His approach is fundamentally different from Sam's and mine.

What Sam has brought to the Santa Fe Institute is more of a notion of the behavioral science rather than physics or pure economics, and they have been very supportive. Sam is just an amazing person--an amazing organizer. He's indefatigable. I run a really large research project, but he runs a big research project like mine plus he does the Santa Fe Institute, and more.

At first, many years ago, I was hostile to the hype surrounding the Santa Fe Institute, with their talk of "complex nonlinear dynamics," "genetic algorithms," and the "edge of chaos." I said, give me a break, isn't that just differential equations? But then I realized that's exactly what I do, complex nonlinear dynamics. I have all the problems everyone else has. I do not do chaos and I have not found that useful for what I do, but I use Mathematica and I program agent-based models all the time because you can't solve these complex nonlinear dynamic things just using closed form analytical expressions. It's not just economics, there's good stuff going on in chemistry, in computer science, in biology. We get along very well with people at the Institute. I think it's a great place.

You received a copy of our introduction about cutting edge economics. What was your reaction to it?

I have three comments. The first is that I don't feel cutting edge. Whenever there's new research, that's cutting edge. What else is new? Maybe it sells to call something cutting edge, but I get very offended when people say this is cutting edge, especially when they say it about something that I disagree with. New research that overturns some of the old stuff is true in any field that is in flux. Second, I liked that it stressed that economics has moved from being an ideological closed field to being much more open to links in and out of other fields. I think that's a real step forward. Third, I don't like to be thought of as heterodox. I know other people think of me that way, but that's just my personality. It's not really true at all. I do take strong stands as a way to shake things up, but I'm just a traditional scientist.

One final question. We had to choose a limited number of people to interview. Do you think we have all the fields represented?

The only thing I could add are young economists who collect a lot of data and that would be people like Sendhil Mullainathan, Steve Leavitt, Michael Kremer, and Abhijit Banerjee. These people are young; they are very smart, and they do lots of theory. Leavitt did work explaining declining crime associated with abortion. A lot of people think that he's Fascist, but he's just doing his work. Michael Kremer went into Africa, collected data to answer questions, or he does experiments where they just lay money on people and see whether they build schools or not. Banerjee collected data on small scale farming in parts of India where they passed laws favoring it. You just can't work from the big data sets that come from the federal government. You have to go out there and collect data. I believe that the NSF should pay people to go and collect data. Collecting data is something no one did in my generation, but the new guys are saying, okay we've got to collect the data so let's go and collect the data.

Bibliography

Boyd, R. and P.J. Richerson. 1985. *Culture and the Evolutionary Process*. Chicago: The University of Chicago Press.

Braverman, Harry. 1974. *Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century*, Monthly Press, New York.

Bowles, Samuel. 2003. *Microeconomics*: *Behavior, Institutions and Evolution*, Princeton University Press, forthcoming.

Fehr, Ernst & Klaus M. Schmidt, 1999. "A Theory Of Fairness, Competition, And Cooperation," *The Quarterly Journal of Economics*, Vol. 114 (3) pp. 817-868.

Gintis, Herbert. 1976. "The Nature of the Labor Exchange of the Theory of Capitalist Production," *Review of Radical Political Economics* 8,2:36–54. Reprinted in *Radical*

Economics Samuel Bowles and Richard C. Edwards (eds.) in the series *Schools of Thought in Economics*, Mark Blaug (ed.), Edward Elgar Publishing Company, 1981.

Gintis, Herbert. 2000. *Game Theory Evolving.* Princeton: Princeton University Press.

Gintis, Herbert. 2000. "Strong Reciprocity and Human Sociality," *Journal of Theoretical Biology* 206 :169–179.

Gintis, Herbert and Christopher Jencks, et al. 1972. *Inequality: A Reassessment of the Effect of Family and Schooling in America:* New York: Basic Books.

Gintis, Herbert and Tsuneo Ishikawa. (1987). "Wages, Work Discipline, and Unemployment," *Journal of Japanese and International Economies* 1:195–228.

Gintis, Herbert and Eric Alden Smith and Samuel Bowles. 2001. "Costly Signaling and Cooperation," *Journal of Theoretical Biology* 213:103-119.

Gintis, Herbert and Joseph Henrich, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, and Richard McElreath. 2001. "Cooperation, Reciprocity and Punishment in Fifteen Small-scale Societies," *American Economic Review* 91 pp. 73–78.

Gintis, Herbert and Samuel Bowles. 1976. *Schooling in Capitalist America: Educational Reform and the Contradictions of Economic Life.* New York: Basic Books.

Gintis, Herbert and Samuel Bowles. 1986. *Democracy and Capitalism: Property, Theory, and the Contradictions of Modern Social Theory.* New York: Basic Books.

Gintis, Herbert and Samuel Bowles. 1996. "Time Preference, Labor Discipline and Earnings: Explaining the Economic Return to Education," University of Massachusetts Working Paper.

Gintis, Herbert and Samuel Bowles. 1996. "Productive Skills, Labor Discipline, and the Returns to Schooling," Paper prepared for the conference on Meritocracy and Equality, University of Wisconsin.

Gintis, Herbert, Samuel Bowles and Melissa Osborne. 2002. "The Determinants of Individual Earnings: Skills, Preferences, and Schooling," *Journal of Economic Literature*:1137-1176.

Illich, Ivan. (1977b). *The Right to Useful Unemployment and its Professional Enemies*: Marian Boyars.

Kremer, Michael. "Randomized Evaluations of Educational Programs in Developing Countries: Some Lessons," forthcoming in *American Economic Review Papers and Proceedings*.

Kreps, D., P. Milgrom, J. Roberts, and R. Wilson. 1982. “Rational Cooperation in the Finitely Repeated Prisoners’ Dilemma.” *Journal of Economic Theory* 27: 245–52.

Locke, John. *An Essay Concerning Human Understanding.* (1690; New York: Dover, 1959).

Luce, R. Duncan and Raiffa, Howard. 1957. *Games and Decisions: Introduction and Critical Survey*. New York: John Wiley and Sons.

Medio, Alfredo. 1972. "Profits and Surplus Value: Appearance and Reality in Capitalist Production," in *A Critique of Economic Theory*, ed. E.K. Hunt and J.G. Schwartz. Harmondsworth: Penguin Books, 1972.

Rubin, I.I. 1972. *Essays on Marx's Theory of Value*, Detroit, Black and Red Press.

Stephen A. Resnick & Richard D. Wolff. 2002. *Class Theory and History. Capitalism and Communism in the USSR*. Routledge.

Stiglitz, Joseph and Carl Shapiro (1984). “Equilibrium Unemployment as a Worker Discipline Device,” with Carl Shapiro, *American Economic Review*, 74(3), pp. 433-444. Subsequently reprinted in *New Keynesian Economics*, 2, N.G. Mankiw and D. Romer (eds.), MIT Press, 1991, pp. 123-142. Also in *Macroeconomics and Imperfect Competition,* Jean-Pascal Bénassy (ed.), Edward Elgar Publishing, 1995, pp. 453-464.