Interview with Kenneth Arrow

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Abstract. Arrow argues that the biggest failures of economic theory are: our failure to explain the business cycle; the missing explanations for the size of fluctuations of prices; our failure to explain the causes of growth and of the spread of innovation. He then discusses several of the existing alternatives to the rational expectations paradigm.

He tells the story of his dissertation, and how Koopmans wanted to decline his Nobel Prize.

Finally, he discusses health care reform, and malaria in Africa.

Juan Dubra: In the press release of the Nobel committee both your work on general equilibrium and your impossibility theorem were mentioned. I find the two works very different in focus, in its basic questions and methodology, not to mention that the impossibility theorem started a whole new area of research. Did you think it was natural to pool those two strands of your research together?

Kenneth Arrow: It was not entirely natural. The reason why it happened that way was that they were giving the prize jointly to me and to John Hicks for our work on General Equilibrium and Welfare Economics. Hicks had made important contributions to General Equilibrium. He had made a very important contribution to Welfare Economics. He enunciated a general criterion for welfare improvements: the so-called Potential Pareto Improvement idea. Of course, my work on Social Choice also relates to Welfare Economics but in a rather different way. His approach to Welfare Economics was very in line with this work in General Equilibrium. But my work was also about Welfare Economics and General Equilibrium. So they awarded the prize jointly, and made the two contributions comparable.

JD: But seen in perspective, Hicks is no longer studied, and both your work on General Equilibrium and on the Impossibility Theorem are all over the place. Moreover, they are two completely different fields nowadays.

KA: They considered my work and Hicks' as contributions to the same purpose. But you are right; I think it was a stretch on the part of the Nobel Committee.

JD: Excluding your work leading to the Nobel Prize, and your 1963 paper on the AER about the health services market that initiated the asymmetric information literature, which area do you think was the one with the biggest impact? The work about growth and learning by doing; the insights about risk and risk sharing; or the work on information, its demand and its implications for returns to scale; or the economics of racial discrimination, and discrimination in general?

KA: It's a good question. The first two areas have had a much bigger impact than the third and fourth.

I think that my work on growth and learning by doing was one of the first, really the first, paper on endogenous growth theory. In that sense this started the field and also brought forward a topic which is very relevant. Anybody who is discussing growth has agreed that learning by doing is one of the factors in growth, though there are certainly other endogenous factors.

I think that maybe the work on risk and risk sharing had the biggest impact, maybe even bigger than the one on learning by doing, though it is hard to say. The part that has had the most prominence is the theoretical work arguing that a security is like a sequence of bets, which is the way I incorporated securities into General Equilibrium Theory. That is true even though we understand that in reality securities do not span the market. Even then, it stimulated a theory of incomplete asset markets. And either way, even the practical people, developing specific works in finance, use this kind of contingent security idea for putting forth their models and ideas. So this has had effect on both practical research and theoretical research, stimulating the theory of incomplete markets. And it has provided the language for research in this area, although the people who use the language may come to different conclusions. This work has had a big impact also in other areas of finance, like insurance.

Within the work on risk, there is also the other part, the work on measures of risk aversion: absolute and relative risk aversion. This work was also independently done by John Pratt. The Pratt-Arrow measures are referred to over and over again.

So I think the work on risk bearing has had the biggest impact of the four, but the work on learning by doing is close. The other ones haven't had not nearly the same influence.

JD: In your book on Essays on the Theory of Risk Bearing you cite the work of Vegh Villegas. ¿Do you know any other Uruguayan economists, or their work?

KA: Yes. I do cite the work of that mathematician. I also know Pablo Spiller and some of his work. But he's been here so long that he must be American by now!

JD: What is a list of relevant topics that you think economic theory has failed to address properly, or explain properly?

KA: That is a big question. First, I want to talk about the role of fluctuations: why are there so many fluctuations in markets and asset markets? Because, after all, if you are discounting a very long future stream of payments, that should be fairly smooth. If you think about a security, or a house, or something else, its value is the discounted expected sum of the future revenue or returns it generates. Then, the volatility of those asset prices should be smoother than the volatility of the returns, because it is kind of an average. But in fact the prices are more volatile than the flow returns. And more important is the fact that we don't understand why the real part of the economy fluctuates so much. That is, I am not really satisfied with the answers to the question 'what drives the business cycles?' by the

existing theories. There is no fully accepted theory. Despite the success of "Real Business Cycles", nobody quite really believes that. There is a very big opposition to that. If you think that money is important, you don't really agree with RBC Theory.

But there are other issues that we don't have good explanations for. The most obvious one, which is a subject that everybody is discussing, is the causes of growth. Why do countries that seem to be not that different from each other grow at quite different paces? My favorite comparison is that in 1949 the GDP per capita in Korea was the same as that in Ghana. Also, what are the differences between North and South America? This is a subject historians work on; but general economic theorists have very little to say. Even the growth theorists do not have really a great deal to say about it that is useful. Why these big differences? Now suddenly everybody's beginning to invoke non economic reasons for growth. It used to be culture and now it is institutions. Not the mainstream economists, although the discussions about institutions are coming through even there but of course they use very crude measures of institutions and culture.

JD: these papers are using deaths of soldiers and priests as measures of institutions, that's the kind of thing you're talking about?

KA: Yes well that's one. "The Colonial Origins of Comparative Development: An Empirical Investigation" published in the American Economic Review, December 2001 by Daron Acemoglu, Simon Johnson and James Robinson. But more generally, there are institutes and organizations that put out indexes measuring the quality of institutions.

There are lots of people who have different measures of institutional performance and these cross country comparisons. Also, before that you have the whole literature and "evidence" going back to Max Weber and his analysis of "culture" -which is not the same as institutions, but they're closely related. The discussion we are having now about the influence of institutions on growth is the same kind of argument that Weber had at his time. The trouble with this discussion is that the problem underlying it is perfectly real: why did Japan react the way it did when it had the chance to become developed? The answer is not obvious if you think that China did not adapt; I mean, China did eventually but in a different timing, it took a lot longer. We don't have a good approach to the question of why similar countries-countries that don't look all that different-have quite different performances.

Then of course there is the problem of explaining innovation. But then maybe it is unreasonable to suppose that an economist could explain innovation.

JD: You mean, that people can't explain why it happens in certain places and not in others.

KA: First we need to understand why it happens at all. What determines its rate? Even apart from country differences. We can't explain differences in time. Why is it that certain periods are much more progressive technologically than others? Why is it that since, say, 1700 or 1750 we seem to have had a fantastic acceleration of technological progress? While there are thousands of years of history that nothing much happens, at other times innovation takes place at an enormous rate. I remember once reading in a history of the classical period

that if you look at the technology of, say, the 5th century BC Greece and you look at the technology of the 3rd century AD Rome, there was very little difference; there was almost nothing that the Romans used 800 years later, that the Greeks didn't use earlier. You could see very little change; you could find details, the Romans were better at cement but by and large, the ships, for sailing the Mediterranean, were pretty much the same kind of ships. The way you grew crops, the way you ground the wheat; whatever you can think of, were very much the same sort of thing. And yet even medieval Europe, which you think of as a backward period, was much more technologically progressive than the Romans. They invented the clock, they invented eyeglasses, they invented the yoke for oxen, stirrups and, amazingly, the windmills. That's incredible: they used to employ human labor, or animals to grind wheat, and they began to use wind and water to do it; that's a big difference. The Middle Ages were actually fairly progressive, compared to earlier times. But since the 1750's we have had a fantastic acceleration, and if anything, we still accelerate. Why? What happened?

But even apart from the rate of technological progress, other issues remain to be explained. Of course, the country differences that you mention are important, but the other most important thing is diffusion. What are the causes for an innovation to spread? You think that once technology is created it should spread easily. Well, it seems that sometimes it does and sometimes it doesn't; some places are more receptive than other places. I think all these things which very much bear on the subject of economics, like well being, consumption patterns, production patterns, prices, things we think of, just what economics is about, really depend heavily on innovation, maybe more than ever, and we're at a loss to explain that. An exponential trend, as is usually assumed in economics, is not an explanation. So I've given you a list of relevant topics.

JD: I guess you've read "Guns, Germs and Steel," by Jared Diamond, as an explanation for diffusion. I don't know whether nowadays people consider that there is diffusion of innovations, or there isn't, but this book aims at explaining diffusion in our prehistory through a geographic explanation. Do you think that's a reasonable thing?

KA: Well, I think that up to a point it is reasonable, but it depends how far you are willing to push the explanation. In a very gross way, yes, I think it is reasonable. You are really saying things spread latitudinally rather than longitudinally. At a given latitude, conditions are similar; you could bring wheat from the Middle East to Europe and it will grow in Europe. But you can't go south, because the wheat wouldn't grow. So you have this argument as to where diffusion takes place. But that is, you know, a very, very, very first approximation. A lot of what he says sounds very reasonable, but I'm a little less clear about other things.

JD: I don't remember if he has an explanation as to why Europe developed a lot more than China, for example. In principle, China and Europe have similar latitudes, but Europe developed and China didn't.

KA: Right, at a very gross level, he explains why China and Europe were both relatively advanced; he really doesn't explain the differences between China and Europe at all, and he really doesn't explain why the regions in between, like Central Asia, were never very

advanced. But at a gross level, it does explain why all these areas, while some were rich and some were poor, were all a lot better off than say, sub-Saharan Africa, or New Guinea.

JD: also related to the topics you already mentioned, as a failure of economics let me ask another question. Like many economists, you are suspicious of the Real Business Cycle view that fluctuations in the economy are equilibrium phenomena. There is some recent work ("New Deal Policies and the Persistence of the Great Depression: A General Equilibrium Analysis," forthcoming, Journal of Political Economy, by Harold Cole and Lee Ohanian) that suggests that large fluctuations, like the Great Depression, may be the result of poor policy in a general equilibrium framework. Do you think that is plausible?

KA: I don't find the thesis plausible. The paper does not even attempt to explain depression. The authors' central argument is not related to why the depression occurred. They just argue that recovery was delayed by bad government policy. They do not explain why the depression took place; they're trying to explain why the recovery was so slow.

Do you know their explanation for depression? That the total factor productivity in 1932 was 24% lower, so they say, than it was in 1929. That's what explains the depression, as they see it. In their model, that drop in productivity is exogenous. In my book that's not an explanation. I think the total factor productivity was low *because* there was a depression, not because people *forgot* how to produce goods. I mean a 24% drop in productivity in three years is not credible as an exogenous fact. So that's no explanation of depression, which to my mind is the big thing to explain. Then they may make a claim that bad government policies interfered with the recovery. Now, I think that is one of those things that are too gross. In the first place I think most of those policies were very ineffective. The worst policy measure was the National Recovery Act. It encouraged cartels and trusts. But it did not play a role in the recovery from the depression: in the first place was never much obeyed or enforced and in the second place, it was knocked out by a Supreme Court decision within a year. I don't think it was such a big matter. But then there is the other argument, that the government encouraged unions, or at least permitted them to operate. So I find it hard to believe that this cartelization played a role. I think their big claim is the unionization drove things. But, maybe; who knows? In any case, it doesn't explain the depression, which is my big point.

JD: In Stanford in 1998 I asked you the question about the topics that economics had failed to explain. You said that it was probably the size of fluctuations both of the economy, and of stock prices. I found out recently that you said in a 1995 interview with the Minneapolis Fed that after the 1987 stock market crash it was

"Perfectly obvious that prices were to get back up again. I held on. It seemed to be quite obvious that they were going to come back because there was no reason why they shouldn't. But the fact is that people didn't behave that way. The fact is you have these periods of alternating volatility and lack of volatility. So it seems to me that at least as far as the financial markets are concerned, there is increasing evidence against rational expectations, even at the macro level."

Do you think that the Rational Expectations paradigm should be taken less seriously, and more attention should be devoted to learning, bounded rationality and/or disequilibrium theories? Which one do you think has the biggest potential to address the shortcomings of the RE paradigm?

KA: This is related to my previous answer about fluctuations. Take fluctuations on a much higher frequency, the day-by-day fluctuations on the Stock Market. You know, the change by 1 or 2% in one day is very common, there's nothing extraordinary about it. I think that what that means, from a theoretical point of view, is that the estimate of the future wealth of a country in one day, the estimate of the future wealth of a country!, has gone down by 2%. Ridiculous! The other problem is why the fluctuations in the stock market happen at all. If it drops, then I say to myself, "look, I know, statistically the market goes up and down, I'll just hold on, it's going to go up. Look, I know it's a random walk. So the drop reflects that. Then it will go up in the future." So, why should I sell? But if I don't sell, everybody says that the price would never go down in the first place.

JD: Suppose we take Rational Expectations less seriously. What should step in as more of a working horse type model? Learning, bounded rationality, desequilibrium theory? Which do you think has the biggest potential to address these shortcomings of Rational Expectations.

KA: Well, it depends on your aim. There is a paper that didn't attract much attention by several authors at the Santa Fe Institute, Brian Arthur was one of them. It supposes that people look at the prices and follow a learning model of the kind that John Holland proposed, a genetic algorithm. Everybody does them and they have several possible strategies. The way they change their behavior over time is that they put more weight on one strategy rather than another depending on its recent success and they keep on updating the weights, depending on whether they are successful. If you generate this you get a series which looks very much like a real stock market series. In contrast, the models most widely used to describe stock markets, like Brownian Motion, do not look like real stock markets: they're much smoother (even though it has a random element).

There's one big thing that's missing from most of these models, both the learning models and the RE models: Why in the world should we assume that prices are the only data that affects the market? News spread in the markets. There's information. If you talk to any investment advisor, most of them think all this RE is a lot of nonsense because what you actually do if you work in Wall Street is to analyze a company, look at its prospects, and process a lot of information; there's quite a bit of information publicly available, which is far more than the price. The idea that the price follows some stochastic process or something else is really missing the point. The price is only part of a much larger system. You have these theories that claim that prices fully reflect the knowledge at any one moment, but they are mostly based on very special cases. In my view, the sufficiency of prices for knowledge isn't even true except under special circumstances. I refer to the models that say the price summarizes the information in the market, that it is completely informative. This is true only if you make a lot of very strong assumptions, much more than just rationality; they have to have very specific functional forms, and rather implausible functional forms, in fact. The idea is that there may be news around which isn't captured in

the price, and therefore you do your analysis on the basis of all sorts of information. This is the sort of thing that somebody *actually* on the market will say.

We do know that some investors have done much better than average on the market. Some of the hedge funds and some universities, incidentally, have reached returns well in excess of the average investor, and one of the reasons there is that they dispose of much more information than is available to others. There is so much information that the ordinary investor simply isn't going to be able to handle it, and, rationally, shouldn't be spending his time trying to. You know, if I've got 50,000 dollars to invest, it's not rational for me to spend my time absorbing all the information that is publicly available. Therefore, those who have much more to invest, could, in principle, invest more in information. And the information is not just looking at past prices. There are all sorts of statistics available, and increasingly so because of the Internet, so we actually have more of it. My point is that may be RE is right if you take account of the information processing costs. Secondly, we know that information spreads in ways that are very hard to quantify; "I know somebody who knows somebody... I'll get a tip, I'll get some information." It's very hard to reduce this to a model. The process by which information is disseminated and the courses involved are not easily modeled. In a way, the learning theories could be interpreted as Rational Adaptation, given the costs of information. I think the idea that information spreads in a way that isn't reflected in the market but affects the market, is a key part of the story. You can call that Learning, but it has to do also with what is available to be learned. It isn't just that people have limited capacity to use the information they have, but also there's, so to speak, a social structure that determines how the information is disseminated.

JD: Why did Tatonnement fail as a theory-field?

KA: It was a very attractive approximate model. The only signal that is available on the market is supply and demand and therefore the difference should determine how the market moves. This is of course very basic. Intuitively this probably goes back hundreds of years, but it was formalized by Walras, and he was drawing rather explicitly on the stock market as his example. About the theoretical models, and the one of Tatonnement in particular, it turns out there are no universal theorems. We have examples where Tatonnement would fail and yet, one feels that supply and demand would come to equilibrium. In experiments, supply and demand will come to equilibrium in all sorts of situations. Of course the supply and demand in experiments sometimes come from somewhat artificial situations But experiments of this kind go back to Edward Chamberlin, in his classroom in the 1940's. Most of the original work was with just one commodity, but even if you have inter-related commodities, people find themselves coming to equilibrium. We know theoretically it doesn't have to happen, but the suggestion is that the tatonnement description is not a very good description. The idea that no transactions take place until equilibrium, that all trade takes place when an equilibrium is reached is not right. It's not even right for stock market; when you find a compatible pair of people, they transact, even though other people are bidding higher or lower. Put it another way, at any given moment, there's two sides of the market, but people are bidding at different levels, there's a whole variety, range, of bids. I think it fails as a description of bargaining, and it certainly fails as a universal theory, although a lot of people have invested a lot of time in it.

JD: What do you think was the impact of the Sonnenschein-Mantel-Debreu observation that "anything" can be the result of an equilibrium, on the general equilibrium paradigm?

KA: I don't think it had much effect. Ok, it was an interesting theorem. In fact, people like Donald Brown and Rosa Matzkin have spent time on finding out that there are empirical implications of equilibrium. It depends on what kind of data you have. If all you have is aggregate income, or for example, aggregate prices, then that says in effect, that anything can happen: you can't expect any comparative statics theorems of a general nature. In the first place, in my view General Equilibrium hasn't really been, in a way, generating comparative statics theorems out of pure theory. My view has always been that it's a framework for econometric implementation. Then, of course, if you have particular values, you can calculate the outcome of, say, a change in policy, or shift in some parameter. In the first place, in some sense, from the point of view of someone who thinks that theory is something to be filled in by econometrics, the fact that given an aggregate function you can always rationalize it is not particularly interesting; you're trying to estimate the rationalization. In the second place, if you assume you know the distribution of income, not just aggregate income, then you do certainly get refutable implications. In a sense, what if I tell you there's a distribution of income, and we know that then not anything is possible.

It's a very interesting theory, but I don't think it is devastating for General Equilibrium, or that it has put off General Equilibrium.

JD: There are fans of both proofs of the First Welfare Theorem (the first order conditions approach, and the "revealed preference" approach). Which one do you like best?

KA: Your reference to first-order conditions fits the papers of Oskar Lange (Econometrica, about 1942) and Bergson (Burk, 1938). In those papers the key was comparing the first order conditions of the Pareto problem and the equilibrium problem, and working from those.

The revealed preference argument goes as follows: "if an allocation (x,y) Pareto dominates the equilibrium allocation (x',y') then (x,y) costs too much, and therefore can't be feasible". There are two theorems in my paper, a necessity and a sufficiency theorem. For the statement that a competitive equilibrium is Pareto optimal, I used precisely what you call the "revealed preference" argument; indeed, it was used there for the first time by anyone, as far as I know. The necessity theorem used the separating hyperplane theorem for disjoint convex sets, which might be interpreted as a sophisticated kind of use of first-order conditions. However, it was precisely the possibility of corner solutions (where marginal rates of substitution are not necessarily equalized) that led me to study this problem in the first place, so convex set theory is somewhat broader than the use of first-order conditions.

I think one of my big contributions is to point out that sufficiency and necessity are not equivalent. The proof that the equilibrium is sufficient for Pareto optimality uses the most simple and clear argument, the revealed preference argument. The revealed preference argument is so much simpler than the first order conditions, it is more general, and more economically meaningful.

JD: What do you think is the most promising area of research nowadays?

KA: What I'm going to answer relates to the foundations, general theory, not specific or applied things. One is the whole question, which we alluded to several times, of what you might call non-rational behavior or adaptive behavior, learning behavior. In particular there is a strand of this literature which seems to be very promising, although we have no idea what it means yet. It is the actual use of brain scans, of checking what's going on in the brain while people are making decisions, what's called neuroeconomics; the use of functional MRI-magnetic resonance imaging-to see how people react to different things. Consider for example the extension of general equilibrium to incorporate the difference between future spot markets and futures contracts today about commodities in the future. In General Equilibrium future commodities are like present commodities, but there seems to be some evidence from the neurologists about how people react to buying. In terms of how the brain reacts, there is a difference between acquiring a commodity right this minute, and the *prospect* of acquiring a commodity in the future. Those two reactions light up different areas in the brain. Nobody knows what that means for economics but it's certainly an interesting fact which must have some implication. I think that's a field whose pay-off is years and years in the future, but it should be pursued.

More important is the study of adaptive behavior; the question, for example, of habits and how sometimes you want to keep them and sometimes you want to break them; the reality that people respond by simple rules, they don't maximize, but they do use rules which probably do well most of the time but break down in unexpected situations.

You know, most of the biases that people like Kahneman and Tversky found are rules that probably work pretty well most of the time. It's just that you put people in an experimental situation and you create situations that are very different from the normal, and the rules fail: people do things that are not "rational" or correct. It's not that in real situations they don't fail: those simple rules sometimes give rise to real problems in the world. Still, our rational-based models also fail on some occasions. This research topic and its methods are an extremely interesting practice, just very hard to reduce to a researchable topic, but that's in the Herbert Simon tradition. This tradition of modeling people as if they follow simple rules is a basic way to get away from the problems of expected utility, which generates a lot of problems, at least in experimental situations and also in some real-life situations, where it also seems to break down.

These are the sort of things that in the long run will make a great deal of difference. It intrigues me that certain important phenomena are not well explained in our theories: the argument that money is really of significance in economics really rests on the fact that there are lags, that there are things that aren't explained by the RE models. Why would money stimulate activity in a somewhat depressed economy? Well, why don't prices immediately rise and wipe out the effect. The standard explanation among monetary economists today is that prices react slowly. There are different stories. One group says there's a cost to changing a price, just the fact that you've got to publish a new catalog or something like that. This story is called "menu costs." Another story is that wages are set periodically, they don't change every minute, and so forth. There are all these explanations. But all make the impact of money on the economic system depend on a series of lagged responses. People

take time to learn the new situation and it's really striking me that this is now a central doctrine, we're not talking about fringe people or unusual, it's a central doctrine. And if you ask monetary economists about their field, they say it is about the fact that adaptation takes time; people have to learn to respond to rules.

Now the question which I know is a little more disputed is the asymmetry between rises and falls in, say, wages, which also is based on some kind of rule. People doing questionnaires actually ask employers about cutting wages; they regard that as something you *can't* do, really, or very rarely. They talk about the morale effect and things like that. They're probably prepared to lay workers off, but cutting wages is much more greatly resisted. It happens, of course anyway, but it's harder. Economics doesn't seem to have any explanation for that.

So these kinds of lagged responses are hard to rationalize. A conspicuous example is the default choice problem. You are told, "Something is going to happen unless you elect otherwise." You can choose. For example, in some states, you can have a mortgage in which if you fail to pay, and your property is not enough to cover the mortgage, you are still liable. Now in some states, you can also have a mortgage where if you default the other party takes the property, that's the end of the story, no matter what the value of the property is. In some states you can elect which kind of mortgage you want. In some states, it says the mortgage will be one kind, unless you elect otherwise. It happens that the default choice affects the percentage of people who choose one option over the other. People in one state choose whatever, and people in the neighboring state, where the default choice is the opposite, take exactly the opposite. In one case, 80% take option A and in another, 20% take option A. Probably these particular examples are economically not very important, but the fact that it happens at all is funny, because the cost of checking a box is not exactly very large.

JD: there's some work by Thaler, where he shows that people choose one n^{th} of each of the investment options that are offered to them in their pension funds. If you have like, say, five investment options for the pension funds in your university for your retirement, people usually choose one fifth of each. And in another university there are four options, and they choose a fourth in each. So I guess that's the same kind of thing.

KA: That's right. And now, employers who offer health benefits to their workers are required to offer several competitive health plans to his workers. This rule has been adopted because it forces competition between the health plans. But now everybody agrees that you should never offer more than three or four plans, otherwise you get great confusion.

These are psychological things that have to do with learning and decision making. Decision making has costs, essentially, which psychologists have recognized for a long time. The view is that if you have too many options, the decision that's made is worse than if you have fewer options.

More empirically, the other area that I regard as promising but is not being pushed very much, it is not very popular, is looking at distributions of things: distributions of price changes, distributions of size of firms, distributions of size of cities and so on and so forth.

There's a small intellectual community that's interested in that and the striking thing is, of course, that most of these distributions tend to have very fat tails. Now this reflects an underlying heterogeneity in the population, and what's wrong with macroeconomics in my opinion is that the populations are modeled as if there were a representative consumer. The representative consumer model masks a great deal of problems, and I think these two are connected: distributions have fatter tails than we expect from models, and models are based on the assumption of a representative consumer. The fat tails mean that a relatively small number of events, or people or something, have a big influence. And I think most of our theories of price changes, of changes in investment in response to different conditions are deficient because they don't take account of the shapes of the distributions.

JD: Could you name the 3 topics that you think are most likely to be recognized with a Nobel Prize in the future? And a topic that you think deserves a Nobel Prize, but is not likely to get it?

KA: I have nothing interesting to say about this question.

JD: You have written about discrimination. How important, economically, do you think discrimination of all sorts is today?

KA: I think the situation regarding discrimination is totally different in different countries- the role of caste in India must be very powerful, but I don't want to say anything about it because I don't know enough about it. I don't really know the situation in Latin America, particularly those Latin American countries where there are enormous inequalities in income; to what extent that's related to discrimination, I don't know.

But speaking for the United States, the matter is getting more subtle; more subtle than in other countries, and more subtle than before. Speaking about the United States, obviously, every index shows that discrimination has decreased over time. Nobody questions that there's been a great reduction in the degree of discrimination. But it's certainly not zero and there's evidence of discrimination in things like lending; probably there is less discrimination in employment now (than there was before, and less than in the lending market).

I think the problem now is that we have a historical element; that is the past of discrimination is still playing a role in creating expectations, in shaping the cultures of the different groups. And I think it's going to be a long time before we see the end of it. There's no question that the degree of discrimination, the economic loss due to discrimination and so forth are sharply less than they were, say, 30 or 40 years ago, both in regard to gender and in regard to race. I'm talking about the US experience only.

Immigrant groups offer a different question, I don't think discrimination is a slight issue, but there's more of a problem with human capital and the adaptation to new cultures. The adaptation creates tensions. These issues are basically outside of economics proper, although different cultures and their adaptations do have economic implications. One example is trying to explain the high crime rates among certain groups. But I think the

heritage as regards to race and blacks and as regards to women, are historical. The facts perpetuate themselves in certain habits which don't change quickly, although I have no real doubt that they will change just as immigrant groups always show that no matter what the problems of the first generation, the third generation is indistinguishable from anybody else. It just takes time, that's all. I think discrimination is a problem that can be fixed, but one must be vigilant about maintaining a favorable attitude, keeping some doors open. I think it's less of a problem nowadays.

JD: Evolutionary theory tends to explain quite well in retrospect how various species emerged. You have claimed that it's not very good on prediction. Why do you say that? Doesn't evolution take too many years to be tested?

KA: It is true: it explains well in retrospect, but it is not good at prediction. I think no serious biologist is prepared to predict the next species that's going to emerge. What will replace man, for example? There's no reason, if the past is any guide, not to suppose that some mutation of man will emerge. It may be that our close global interconnections are making it much harder for evolution to take place. For evolution to take place, and for the emergence of a new species, you need in general isolated populations. But we're probably not going to get that because there are no isolated humans. But I suppose that maybe other species may emerge. But it's certainly difficult with the past: I don't think that if anybody had lived at the time of the dinosaurs, they could have predicted the rise of mammals-say, cats, or primates.

I don't think there's anything in the theory which explains why some special species will emerge. Some people say it is just random, if you start all over again you get entirely a different configuration: at each stage there are random mutations and one of them happens to be better and then that takes over; but it could have been a different mutation that occurred and took over. Now some people say that therefore what comes out could be anything. One of them is Stephen Gould.

Others say the exact species would be unpredictable but there'd be a tendency towards more intelligence, more efficiency in some ways, some of the characteristics would be predictable. Let me give you an analogy. There's been a lot of discussion among ecologists about how a forest builds up. There's a whole sequence: start with a bare field; then some seeds come on; then the germination of the plant, certain kinds of plants that grow in a lot of sun. As these plants grow up, they start to create shade, then other plants get preferred, so in fact the kind of trees that you have when the forest is mature are quite different from the trees and bushes that have been there along the way. There's an old view which says that the final product will be the optimal one for that region-a community generating an equilibrium system because the trees and plants feed each other. You will have, for example, tall trees and below them, small plants that grow in shade, and things like that. And what kind of tree will emerge, will depend on the conditions: different climatic conditions will produce different kinds of trees. According to this view, if the forest burned down, and you started all over again the trees that would come up would eventually be the same, the equilibrium for those climatic condtions. But according to the random history viewpoint, you might end up with a different set of trees. And finally the views that we seem to get now, say yes, they might be different trees, but they'll be functionally

equivalent trees. Trees will grow that will have the same general characteristics as the ones they replaced. They might be different species; if it's an area where you get evergreen trees-conifers-you might get a fir tree, or a spruce tree, but you'll get a tree of that kind. So you might be able to predict, not exactly the tree that will be there at equilibrium, but some of their characteristics.

Evolutionary theory *may* do that, but I'm not sure any good evolution theorist could say really what he thinks the world is going to look like and what the animal and vegetable landscape of the world is going to be, like say, in ten million years. I don't think they're really prepared to predict that.

JD: There are several "myths" surrounding your dissertation and how it was perceived at the time when you presented it. They were probably fueled by the delay in its completion, and by the novelty of the methods and results. In particular, I have heard it said that since you expected trouble when presenting it, you had a "backup" dissertation that was lost after the "impossibility theorem" was accepted. Did anything like that happen?

KA: The story's really quite simple. It's not that I had a backup dissertation. I was working on another dissertation which was a laborious re-working of Hicks's General Equilibrium theory, to correct what I felt were its mistakes, and the areas in which it made assumptions which were unnecessary or inconsistent, and things like that. It was very laborious, but not, I think, terribly exciting. It was just re-doing it in what I thought was a better way, raising new questions. Well, then in the middle of this I got the idea for Social Choice. In fact, it was in response to a question. The minute I worked through that, I knew I had a dissertation; there was no question in my mind. Pieces of that other one emerged over time, developed. They went into my work on stability theory and into my paper on equilibrium with securities, although for this last one, what I had during the period of my dissertation was only the idea, the question, not the answer. As I said, it wasn't that I had the other as a backup, I just felt definitely that I had a thesis in social choice theory. I had a professor who I don't think understood the thesis in detail, but he had respect for me, and I correctly guessed that he would see the importance of my Social Choice work. I had no trouble getting it accepted. The minute I had it, I knew I had it. So the other dissertation was not a backup, it was something I had been working on *before*.

The question that stimulated my social choice work arose at the Rand Corporation, where I spent the summer of 1948. Game theory was being developed there with the view that it would help analyze the diplomatic and military interactions between the United States and the Soviet Union.

In the intellectually adventurous attitude of Rand at that time, they had on their staff a philosopher, Olaf Helmer. He asked me one day how one could consider either of these abstract entities as players in a game, when each was in fact composed of many individuals with varying values. I glibly replied that welfare economists, particularly Abram Bergson, had discussed that question in the context of justifying economic policies. He asked me to write an expository account; when trying to do so, I was led to formulate the social choice problem and discover the impossibility of a general solution satisfying certain very natural-appearing assumptions.

JD: Was there an occasion in which an economist friend of yours wanted to decline his Nobel Prize because he thought it was unfair that a friend and coauthor of his had not been included? Who were those involved?

KA: That was Tjalling Koopmans. But he was neither friend nor coauthor of George Dantzig, the other person who did not receive the prize. What happened was that for some reason the Nobel committee decided that it wanted to give the Prize to a Russian. It was the Cold War, and they were trying to balance things, and who knows what else. On the other hand, Soviet economists certainly couldn't publish anything worthwhile because they had many restrictions. If they did something worthwhile, different, you know, they would be trouble.

Well, with Kantorovich, the Nobel Committee finally found that linear programming was a great development. Which was correct, fine. And they wanted to have the prize shared by somebody from the Soviet Union and somebody from the West. Kantorovich had laid out the linear programming problem and characterized the solution in the sense of the dual variables, but he didn't have an effective algorithm to solve it. He'd worked this out, published it in Russian, in 1939. Actually it got him into trouble because the Soviets could see that dual variables were very much like capitalist prices and were very upset. So they stopped him from further publication.

Koopmans, during World War II, had been working on shipping problems, how to optimize shipping, and developed a solution to what's usually called the transportation problem. It's a special kind of linear programming problem, you have a number of routes and you have a number of ships and you're starting from here and you want to end up there, but you can send them by different routes, and you optimize it. This is a linear program but it's a very special one, and it's a lot easier than the general linear programming problem. And he very quickly invented a very elegant solution. However, it was not a general solution to what was later understood to be the linear programming problem.

George Dantzig, quite independently from Kantorovich, formulated the linear programming problem as a response to military demands. And then, after several years of work, he developed the simplex algorithm, which for the first time permitted problems of any real size to be solved. So *he* made linear programming possible. For some reason the committee did not include Dantzig. They probably did not include him because Koopmans was a very eminent economist, and deserved a prize, and if they included Dantzig, probably, they would have had to exclude Koopmans. But frankly, Koopmans did not deserve the prize for linear programming.

His contribution to linear programming was fairly minor. In fact some quite obscure person named Hitchcock had solved the transportation problem several years before Koopmans. For some reason the committee decided to give the prize jointly to Kantorovich and Koopmans.

Koopmans was very aware of everything I've said. Indeed, he in fact had learned about Dantzig's work, had greatly encouraged Dantzig. Just as an example, the first paper where

the simplex method was explained was published in the proceedings of a conference which Koopmans organized. So, he was very upset about the question, "should he take this prize or should he refuse it, on the grounds that Dantzig should have gotten it." In fact, he called me up for advice, and I thought, "well Koopmans deserves the prize, maybe not for this, but he deserves the prize." So I talked him out of it. He then gave the prize away, to endow the International Institute for Applied Systems Analysis apparently because he felt that Dantzig had deserved it.

JD: Do you think that your passage during World War II through the Weather Division of the US Air Force; or your taking actuarial examinations in order to go to the private sector; or your passage through RAND shaped the future of your career for the better?

KA: Well, the Weather Division I can't say had any real impact, although it was a very interesting experience in itself. But I can't say it affected my future career in any serious way. Well, there is one good thing about the Weather Division, which is that it kept me from working on my thesis, which was probably a very good thing, because I got a much better thesis!

Now the actuarial examinations are a different matter; they had a profound influence, I really learned. Let me explain something. We are talking about the time when the great depression was finishing. In 1940 things were still not very good and the question of what I was going to do with my life, what kind of job I would get, was a very, very pressing one in my mind. Becoming a professor was something a little outside of my realm, even though I knew professors. But becoming one was outside my field of conceivable careers. My real ambition was to be a high school teacher of mathematics. But the only reason I'm not is because there were so many people who had already passed the examinations who were eligible, that they didn't give another examination. So I thought I'd become an actuary. That was the alternative in my mind. So I took a couple of examinations and then after the war I was resuming that. I was going to get a PhD, but I thought I would get my PhD and become an actuary. I went to graduate school more or less because, well,

a) more education is better. It was my family's view, my view and I was good at learning, obviously, and

b) I didn't have anything else to do;

So, of course, then, after a couple of years out of school, going on to an academic career became real. But after the war it was complicated: academic life was not particularly prosperous. I met people who were actuaries, and they obviously had high incomes-high enough, not high, but higher than a professor. You know, the idea that I could lead a good life was very promising. So I thought about it and took another examination; you had to take nine examinations to qualify. You can get jobs before that, they hired you, but you were not considered an actuary. It's the same thing today; I believe the system still exists, it's one of the few remnants of the medieval guild system still around. I actually seriously was thinking about it after the war.

Whatever the reasons that engaged me in this actuary business, one thing is true: I really learned. One thing I learnt about during the course of this was moral hazard and adverse selection. And when twenty years later I got involved in health economics, it paid off. I suddenly realized insurance people knew what they were talking about: there was a real economic issue which economists had not understood. It turned out that even though I didn't pursue it, it was a very important economic problem. I really understood what risk bearing was about and understood the realities of it. I actually worked one summer as a clerk, an actuarial clerk, just calculating premiums. And during the course of this job, I would always ask questions, and since actuaries are always happy to teach you something, they answered, and I learned about all sorts of things. So between that and my reading for the examinations I learned all about adverse selection, safety loading, administrative loadings: all the things, all the reasons why the premium was something well above the expected value of a loss.

So spending my time in the insurance industry was really extremely valuable. Luckily I didn't pursue the actuarial career. Here goes the story about why I didn't pursue it: I got an offer from the Cowles Commission. Koopmans was working there. I told him frankly I was debating with the idea of going on to a scholarly career; I didn't know whether I wanted to get a PhD and follow an academic career, or be an actuary. I knew that when he came to this country in 1941, he had worked for a while for an insurance company. So I asked him "frankly, what is your experience of an insurance company?" And he said, "Oh no, there is no music in it." And you know, the minute he said that, I knew that I would not become an actuary. I'm sure that anyway, I would not have been an actuary, but I knew at that instant that there was no further question in my mind.

Policy.

JD: The problem of adverse selection has sometimes been mentioned in "state-heavy" countries as ours as a reason for governments to intervene. How does government intervention help in the adverse selection problem? In particular, you have advocated single-payer systems (cheap due to economies of scale, and for the lack of adverse selection problems) for the health care market, but is there any advantage that the single-payer is the government? What would be the key features of a health care market if you could design it?

KA: You think I'm too much of a state-oriented person?

JD: Not you, I'm saying that Uruguay is very state-oriented and that sometimes the problems of markets, in particular adverse selection, are considered a reason for government to intervene. I'm not saying you are a state-oriented person, I'm just saying people use adverse selection and moral hazard and all sorts of excuses for the state to intervene and so my question is: is there any particular reason why the state is better than anything else?

KA: Adverse selection is a serious problem in health and in social security. And moral hazard is also a problem in health and social security. But in both cases there is a role for insurance. Personally I think that insurance-bearing risks, spreading risks - is an extremely

valuable social activity, with some purpose, but only when the risks are large. Some of that can be taken care of by the market, and by market supply of insurance.

Then we have the problems of adverse selection and moral hazard as limits to the proper functioning of the market. And of course, adverse selection can be overcome by some kind of compulsory insurance. That's straightforward: if everybody must be ensured, then there is no self selection. That's why, I think, to a large extent, old age is taken care of, in every country that I know of, in one way or another, by government policies and government sponsored programs, particularly for low income people. Take for example the commercial annuity market, which sells for a fixed price a monthly income for the rest of your life. There is a commercial annuities market in the US, but that is very heavily subject to adverse selection. It's very well known that people who get annuities live a lot longer than people who don't. What happens, and that is the standard assumption in models of adverse selection, is that people who know they're healthy will buy an annuity, and people who are not won't buy. The result is that the annuity market is almost useless, because the insurance have to charge premiums that correspond to that situation. So I'm told that the money you get by buying annuities is practically the same as what you would get if they assumed you would live forever. You get very little monthly money in exchange for your (large) down payment. So you have clear evidence why government sponsored old age policies have some advantages. Interestingly enough, the administrative costs of these policies of extremely low. One problem that ordinary kinds of insurance companies have when they're faced with adverse selection and moral hazard, is that the insurance agents -

JD: they try to pass the buck?

KA: Yes, well, they have a lot of administrative costs; they give medical tests, if you have a medical policy, or, for that matter, if you have an automobile policy. And if you make a claim, they have to investigate the claim, because there's a moral hazard issue. And the result is that these costs are a large fraction of the cost of running an insurance business. A good part of your premium is devoted to these things. So, there are administrative costs, and since there is competition, you have marketing costs. So the result is what we know from international evidence: that the old-age costs in running a compulsory system are very much lower than those in a private system like Chile. The British avoid that by having a system where the individual can opt into it, it's a trust fund which is administered by the State. No choice for your investments. That reduces the administrative costs but it also reduces a lot of the discretion. The British system is not all that different from a state-run system. It is a state-run system; you're buying equity, you are buying a different kind of claim. The problem then really comes down to the fact that the government is better than the private sector at keeping costs down-for insurance purposes. This isn't true in any other industry. If, for example, you are trying to produce electronics, you could hardly do worse than have the government run such an industry. But, in an insurance program, it's a different matter.

Still, even within insurance, I wouldn't have the government run all the programs. Certainly I'm not talking about nationalizing insurance. For example insurance companies want the government to intervene-in the US-with regard to hurricanes, they want reinsurance for

hurricanes. Now that's something I'm opposed to, that's what the private market should stick to.

JD: there's no adverse selection, no moral hazard...

There's not much adverse selection, but there is a relatively small moral hazard problem, because you can design your house in a way that it's more or less resistant, but it's minimal. And insurance companies do inspect how you build your house, and it is probably better than if the government did it. As I said, I am not in favor of the government running all the insurance. I think that in things like health and retirement, there's a big advantage if the government runs the programs because of the deficiencies of risk bearing in adverse selection and moral hazard. The government has an advantage which should be taken account of. The British control their health costs a lot better than Americans do. It's a mixed system, there's a large private enterprise element in it, and it certainly does keep costs down. So that's my view. I'm a pragmatic in these matters, I do not believe any slogan like "markets are everything", or "keep out the money-making greed"... neither of those slogans corresponds to my view of the world.

JD: Health care reform in the US has been delayed for ages. When a reform comes, do you think that your criticisms to the employer based system will be taken into account?

KA: I think the employer-based doesn't make a great deal of sense, but I'm not the only one to say it. I think a lot of people feel that way. Not necessarily just because of my views. I think there's going to be something established, away from the employer-based system; it creates too many problems and for competitive reasons, it encourages employers not to give health benefits and so forth. In particular, for small businesses, employer based health benefits create a kind of an overhead cost that becomes a disproportionate burden for the business.

Theoretically, if this overhead were not present, a company should be willing to offer health benefits because there's a tax advantage. Workers would presumably take lower pay in compensation and since the government does allow deductibility of the health benefits, that means a tax advantage to the business. The interesting thing is that a large employer like Wal-Mart, has refused to give any health benefit. It is not clear why they opted this way. So a lot of workers aren't covered.

JD: General Motors is in big trouble with its own health plan, right?

Well, you know, in a manner of speaking, some people argue "no, that their wages are correspondingly lower and there should be no net cost to it, they pass off benefits." If they didn't, they would have to pay higher wages for the same jobs. This is my position, but other people feel this way too, and I'm no expert on it. Health benefits to retirees -not employees- are a problem for General Motors. Because the health benefits were calculated and offered under the assumption that they were going to dominate the market and the assumptions are not valid any more. Ex-ante it was a good deal...

JD: it turned sour, but when it was proposed it looked good.

But the conditions have changed.

As far as health benefits for car workers, it's hard for me to see why there should be any difference between the theory (that says that companies, due to tax benefits, should give health insurance to its workers) and what actually happens. The reason is that if you give health insurance, you can make wages correspondingly lower. There's no reason why they should differ, in theory. And most people think the empirical evidence supports it.

JD: Health care throughout the world is in trouble. Do you think that that's the case because policymakers do not understand the problems that you pointed out in your 1963 AER paper, or because there are other "political" issues that can't be resolved?

KA: Well, healthcare throughout the world is in trouble, that's right. That is because there's something intrinsic in the nature of health care. Let me explain what I mean. There are three elements which happen to be interactive in a way which makes health care intrinsically a problem.

In the first place, it's a highly risky situation: one person will have a 150,000 dollar operation, the other will not. It's just chance. In a way, it's an ideal situation for insurance. This is exactly the situation insurance is designed to cover.

Then, on the other hand, insurance is difficult to arrange, because of the asymmetric information aspects, the adverse selection and the moral hazard, without going through how you solve it, it creates difficulties.

But then there's a third aspect, our feeling that everybody should have decent health benefits. It is our social judgment that health is different from other commodities. If something is medically available, there is a presumption against denying it because of lack of income. I think that's the thing that distinguishes health from other goods. Education is somewhat comparable in that respect. Education, at least in the US is quite a problem. We do feel that people should have access to education, irrespective of their income. Some people may say "well, why not make education proportionate to income?" Well, we don't feel it is right. But let me stick to health, because that's the one we're talking about; the issues are similar, but not quite the same. Supposing there were no problems about insurance, supposing we could create an insurance policy which didn't suffer from adverse selection and moral hazard. Just imagine. Well, why not let the market prevail. The market will include a market for health insurance so at a given income level, the risks will be spread, in the usual way. Remove the uncertainty of cost. But that means that people who are poor will have to buy a very minimal kind of insurance and if they have an expensive operation it won't be covered. Obviously if a person can pay one tenth of a premium than another then that person is going to also get one tenth of the medical care. Well, we're not prepared to tolerate that. And here, by "we", I mean human beings, in any country, really. Certainly in any developed country. I am saying this just as a matter of fact, not as a moral judgment as to how things should be.

There are attempts to regulate whether, let's say, motor cyclists should wear helmets. The same happens with other safety regulations and safety requirements, like seat belts. Well, what's the argument there? I suppose we can say "the guy doesn't wear a helmet, he gets killed, that's his problem." Or if he gets injured, that's too bad, that doesn't affect us. And the answer usually is not that one. Society is going to do something for him, or her. If a person is injured, we're going to take care of him, so we have a right to prevent him from being damaged. The same thing happens with tobacco. We tax tobacco so it becomes prohibitive, we tax it very heavily. Well, why not, let a person smoke himself to death? What does it matter? We're assuming people are well-informed so, why not let them? Well, part of the reason is, if they get lung cancer, we're going to pay for it. We pay for those operations. Therefore, we have the right to protect ourselves against that individual's bad habits. Also, we're not prepared to let people suffer for lack of medical attention. No matter what their income is. And we therefore take steps. And in this case we take steps by saying we're going to have some kind of redistribution within the system. Some go to the extreme of the single payer system; some like Canada even prohibit private practice. In others like England, you have a system for everybody and then you can buy private practice. I don't know what Uruguay does.

In the US, the fact is that before there was any medical insurance, there was always charity. A county would have a hospital, for people who couldn't afford otherwise. And also every doctor was expected to give time free of charge. Medical insurance to some extent relieved the doctors of their obligation for free work. They don't feel that obligation today. And the argument is that if you need open-heart surgery your property should not be a barrier. Once you have that, you can't really have a market system. Now, on top of that, there's the fact that technological progress means there's all sorts of treatments available now. Nowadays there are drugs, surgical procedures, and so forth, which didn't exist before, and that keep on running up the costs. You might say "there's nothing really wrong with a society devoting 15% of its resources to healthcare. Healthcare is a perfectly good thing to spend your money on. Why not? I don't see that healthcare is inferior to more expensive houses or things like that." But, the problem comes that since we no longer provide through the market, we have to have a tax structure, and of course, any tax structure creates inefficiencies. And that's even if the system is at its best. And, when you start with insurance you have moral hazard issues, and now the unnecessary medical expenditures occur. But I think it's intrinsic in the nature of the situation. You can reduce the burden but you can't eliminate the problem.

JD: You have claimed that the economy does not allocate risks efficiently due, mainly, to missing markets. You have often mentioned R&D, mergers and things like that. But for people, most of their wealth is allocated in their homes, and they, traditionally, haven't been insured due to moral hazard problems. Do you think that the work of Shiller on developing an insurance market for home values will work?

KA: I don't have anything informed to say about this question, so I'm skipping it.

JD: You have been involved in several "political" issues, gun control, malaria cures, fighting the Bush plan to open areas for roads and logging, and other environmental issues (advocating emissions taxes to curb global warming) being just a few of them. Which ones

are you most committed to? If you could succeed in having your views prevail on one, which one would that be?

KA: I'm not a great political fighter. About gun control, I've signed statements, but I'm not exactly what I'd call active.

Malaria's a different matter, I was asked to serve on a committee to study it and I've become an advocate of a particular view on malaria. Not on how to cure it, because I'm no expert on that, but on how to distribute the new medicines. It involves some of the same issues that we've discussed in this interview, by the way. The key question is that the new anti-malarial drugs cost so much: they cost something like two dollars a treatment. Which from an American, or a Uruguayan point of view, is nothing, but in Africa, it's very much more than the ten cents they were accustomed to paying before. The two dollar medicine is different from the 10 cent medicine. The problem is that this new drug is needed because resistance developed to the previous drug. It was a drug which was sold for ten cents a treatment-through private enterprise, by the way. The public health systems are useless in sub-Saharan Africa. The drug was of no use any more, because the parasite that carries malaria had mutated and developed resistance to this drug. There are drugs now-I won't go into details-which are quite effective, very effective, but cost more like two dollars a treatment.

JD: so people don't have access...

KA: No, they can't afford it. It needs a subsidy. That's not in dispute, everybody knows that. But the problem is how to proceed and have private distribution. I have proposed mechanisms to try to minimize the waste, the graft, and to generate appropriate incentives. You know a private distribution system is needed because the public distribution systems don't work in Africa. In Thailand, where they have a successful anti-malaria program, it's done through the public sector, but Thailand has a much higher income than Africa, and also is much better organized.

To go on to the other things, I fought the Bush plan for open areas, yes. I'm opposed to that because I figure that (well, this is interesting, because it is related to your view that I am someone who advocates state control) in fact, what the US government does is to provide public services at well below market costs, in the form of roads for logging and renting land for grazing at much below market prices. I consider these policies of opening state owned open areas for logging to be quite bad. In particular, conservation of the open areas is important because those assets are a stock of valuable resources for the market. Everything would be much better if the government got out of the way, its government intervention that's the problem. Of course it amounts to big subsidies.

JD: Uruguay is sitting on one of the biggest reserves of underground water in the world, the "Acuífero Guaraní". Do you think there is a way of insuring our country against the environmental risks of pollution on such a big "asset"?

KA: No, I'm afraid I don't know anything about that. It certainly sounds like an asset that you should preserve at considerable cost. Water is an increasingly scarce commodity. If you've got pure water, you should hold on to it! ... for agriculture...

JD: Could your malaria plan (big subsidies today for the new drugs) pay for itself in terms of increased productivity of the affected population? If so, could a market fix the problem?

KA: For countries with a per capita income of a dollar a day, two dollars a treatment is too much. Also, a big part of the malaria burden is the death of children.

JD: I didn't know that.

KA: Deaths are almost all children because as they grow older, they develop a certain immunity. Children who survive usually do not die of malaria. They get sick all right, but they don't die. Deaths are almost exclusively children. It's something in the order of a million a year, maybe more. It's a quarter of the child mortality in the world that is, of deaths of children under five. So you see it's not just productivity. But the productivity trouble, like a lot of these things, is a capital rationing problem. A child should be willing to pay for his treatment, and that would pay for itself; but today he has no money. And for an adult, even if you paid for his cure, and the cure paid itself in terms of increased productivity, there is no way of enforcing that he will pay back. It's like education. There's no particular reason why children shouldn't pay for their education out of *their* future earnings not the parents' earnings. The trouble is that there's no very good way to enforce the payback. In a sense, ordinary taxes do that, indirectly. That is a way of paying back what you received.

JD: Why did you disagree with the US-Microsoft settlement?

KA: I think it did not really address the problems that were put forward in the suit. I knew the details better a few years ago than I do today, but, the question of using things like position on the operating system certainly makes entry into that market very very difficult. I think it has slowed down enterprise and development of new programs. The experts, my computer friends, all think that the dominance of Windows is a bad thing in terms of development of complementary resources. It's an inefficient operating system but it's acquired a dominant position.

JD: Do you agree with Bush's pension reform plan?

KA: I think, by now, it's trivial. I don't think it makes much difference at this stage.

JD: What doesn't make much difference?

KA: Having a varying part of your contributions to be invested. It's been whittled down to a point where I don't think it makes much difference. I think it's just going to create a complication. But I'm not as violently against it as some of my friends are.

JD: If you knew you wanted to study economics, what undergrad major would you choose? Math, Econ, or some other?

KA: I think I would choose Math.

JD: In 1998 you went to work on your bicycle, do you still do?

KA: I still go to work on my bicycle.