ECONOMISTS IN PARTICULAR DESPERATELY NEED TO LEARN FROM PHILOSOPHY

Interview with Professor Ken BINMORE

RFA: Professor Binmore, we would first like to discuss your general, multidisciplinary background. Your background is that of a specialist in mathematics and economics. Later you have embarked on a philosophical mission.

Can you please give us some insights about how this multidisciplinary background shaped your work?

Professor Binmore: Well, I went to college to study chemical engineering. And after a week or so, my tutor asked me "how do you enjoy the courses?" and I said "I found it dull, but presumably it will be more interesting" and he said: "oh, you're quiet mistaken!" And he encouraged me to study mathematics. So I went to study mathematics. I came from a very humble, poor background, so I was mainly interested in getting a decent job and making money. So, when I finished my degree in Mathematics at Imperial College – University of London, I looked for a job in operational research which was quite new in those days. I got an interview with Exxon Petroleum and I was interviewed by one guy and another guy and another guy and I was talking to the big London boss and he was patronizing me and the telephone rang – there were many telephones on the desk – and he went quiet in the face and said YES SIR, NO SIR, IMMEDIATELY SIR and I thought I don't want to work for this organization because I don't have any capacity for subordination. So I thought OK, I'll try to be an academic, but what shall I write my PhD about? I decided to attempt a PhD in pure mathematics and real analysis. I was successful. I remember being unhappy that in my undergraduate time I didn't study foundations of mathematics. So, I guess I was really a philosopher even in those days. After the PhD I got a job teaching mathematics and I wrote various books in mathematics; one is about the foundations of mathematics, and I wrote it not as much to instruct other people, but to instruct myself while writing it. And then by accident (my entire career was a succession of accidents), the chairman of my department was replaced by a very unpleasant man and everybody went to get a job somewhere else. And I got a job at London School of Economics, chiefly because at that time I had two papers in statistics, even though I wasn't a statistician and I had no interest in statistics as such. And I worked for LSE and I was anxious – the department was new, it was harder for us to teach the economists and social scientists a sort of mathematics which it will be useful to them – so I decided to study game theory. So I began to study this and I remember I read von Neumann and Morgenstern's book (I am

probably the only person in the world that read it entirely) and when I came to von Neumann's poker models I just couldn't believe it. I used to play poker at some time, and I thought "well, this cannot be true!" I couldn't fully understand von Neumann's argument, but I found another one that I could understand, and that fully "sold" me to game theory. An LSE economist told me: "You lose your time. It's such a failed research program."

RFA: He was skeptical.

Professor Binmore: Yes, he was. But, I should add, with good reason, because von Neumann only treated two-person zero-sum games.

RFA: What was the other argument, the one that 'sold' you to game theory?

Professor Binmore: You can find it in my textbook, *Fun and Games*, right at the end.

RFA: In those days, what research program was fashionable?

Professor Binmore: In those days people were still exploring the so-called mathematics of free markets. Authors like Kenneth Arrow, who introduced mathematics to the subject and he thought that this will be the solutions to all economic problems. They thought that they were changing the world, and as a result of their efforts economics will become a science and was nothing more to do except what they were doing. Other people disagreed with this (like Robbins at Cambridge), but they were rather marginal. What has this to do with game theory, do we need game theory to study markets? No, we don't need game theory to study markets: we'll have too many biases because what an individual does is irrelevant, only the aggregation is important. Otherwise, what we'll study is monopoly. We don't need game theory for monopoly because we treat buyers as a demand curve and there's only one player. So, if you have infinite players or only single players you don't need game theory. I chose to follow my philosophical inclinations and I didn't care that an economist said it is a failed research program because I could see that it wasn't . Who cares if they thought it would fail? So, I was still seeing myself as a pure mathematician, producing so many papers each year, but after a while something happened to me, which happens to scholars: they made themselves an expert on some particular area, quite often a very small area. And when you are an expert on a particular small area you can write 2-3 papers by year forever. Then it became dull and I started to publish for my amusement papers on game theory, particularly on bargaining theory, so I took notices of John Nash's work long before it was fashionable – for example I mentioned Nash to economists at LSE and they never heard of him…

RFA: When did this happen?

Professor Binmore: It was in 1965 or earlier – I found lecture notes of mine from that period. Then I used to teach game theory and students liked it a lot, of course, so I think that basically rescued the mathematics department, because once the students wanted

us it was going to be all right. But only 10 years later the economists started to understand Nash and take him as very important – he 'reinvented' Cournot equilibrium in a way that made it usable. So people began to understand that you can study imperfect competition. Before game theory came on scene, imperfect competition was not really studied, as the economist had no way to deal with risk, because von Neumann and Morgenstern constructed the theory of rational decision under risk. And also information - economists had no way to deal with information problems before. I was really lucky because I was the only person in game theory that studied the second part of von Neumann book, on coalitions; of course, mathematicians studied it, but not economists.

And after a while I realized that I had no published papers on mathematics for several years. I was taking this amusement very seriously, so I demanded my transfer to the economics department. I asked my colleagues if they will accept me in their department and the chairman said: "No, we have your services for free, why we should pay for it?" So, I looked for a job in USA. At my complete surprise, I was quite famous in America, because of these working papers which I hadn't published in journals, but they circulated. So I got quite a few job offers and decided to take one. Only then the chairman of the economics department at LSE asked me to stay and join them – "Why should I join the economics department, I answered, when they will pay me three times as much?!" So I went to US and I was very happy at the University of Michigan.

Oh, I forgot to say, but I also started to do experimental work…

RFA: When you went to America?

Professor Binmore: No, before. A huge impact on me had an older guy, Reinhard Selten – back then economists didn't really appreciate him[1]. I invited him to London and we spent a whole afternoon in the LSE bar where he drank every variety of beer that was without showing any sign of getting drunk. There he told me about the Ultimatum Game and the experimental results. So, with his colleagues[2], we ran experiments on the Ultimatum Game. The results were published in *American Economic Review*[3] with great difficulty because the economists refused to accept the approach. I was invited once at the famous Chicago Seminar and in the first intervention I was talking about equilibrium selection – it was in the 1980s – and the response was completely hostile. Basically, they didn't like my idea of multiple equilibria. Milton Friedman was hostile, but I can't remember all his questions. Half way through, I was sweating, because the questions kept coming and they were asking less and less about my paper, but about

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[1] Reinhard Selten was later awarded the Nobel Prize in economics, in 1994, and he is widely considered one of the founders of experimental economics [editor's note].

[2] Shaked and J. Sutton [editor's note].

[3] The results were published in 1985 and this is now a classic paper [editor's note].

what they regarded as good economics. The second intervention was an experimental paper and somebody stood up and with a great discontent said: "Do you not know that economics is not an experimental science?" I didn't know what to answer then, but now I know: "If economics is not an experimental science, it isn't a science at all!" So it was very difficult in both fields (game theory and experimental work), but it was fun and I seemed to thrive on opposition.

RFA: When did you start reinventing as a philosopher?

Professor Binmore: That was by accident too. I was invited in Canada to a social choice conference (someone must have cancelled, because it was on a very short notice). I was always interested in Rawls's original position and I thought to write a model, applying Rubinstein's bargaining theory to original position. To be honest, it wasn't a very good paper. I gave my talk and after it a thin long tall gentleman stood up and said: "It is a pleasure to meet you. I am John Rawls." "Oh my God, I said, I've just explained Rawlsian theory to John Rawls!" He was a wonderful man. He encouraged me to write a book (he was harassed at the time by John Harsanyi, and was happy that a mathematician/economist would join the debate on his side), but I was completely hung up with the problem of interpersonal comparison. The economists said that it is impossible to have an interpersonal comparison of utilities. And I knew this was wrong. So I wrote to Rawls to say I can't manage this problem of interpersonal comparison and I'm giving up the project. He wrote back (actually he wrote me several letters, with beautiful handwriting) to say he neither knows the answer to this problem, but I should persist in the attempt. He knew that I was taking an evolutionary, anti-Kantian, line, but he was a very open-minded man and didn't think that only his position must be heard, like his followers, for example. He was a great man. I took heart from this.

RFA: How did you see Rawls as a person, not as a philosopher?

Professor Binmore: He was the epitome of a nice man. He had intended to be a priest. He didn't have a capacity to be unkind. If everybody would be like John Rawls, they wouldn't need any social contract. He was the epitome of a civilized person. The only time when I saw him disgruntled was responding to Harsanyi, because Harsanyi treated him with contempt. I admire Harsanyi, but some of his demonstrations are incorrect. Rawls had great intuitions and those are more important. You need all this, intuitions, mathematics, analysis etc. I finally saw what to do about interpersonal comparison by borrowing Harsanyi's theory of interpersonal comparison. And just so I was reinterpreting Rawls and also Harsanyi in an evolutionary way. And it seems to me that metaphysical problems they invented for themselves disappeared, by providing a foundation. I wrote this in my books, in *Playing Fair* and *Just Playing* – I thought the second one will be a success, I tried to popularize it, but I was such a naïve. I had had some success with right-wing people, because of my assumptions and individualistic methodology. But it is not enough to share some assumptions, many of them did not even consider my conclusions.

RFA: Why do people from the left-wing side refuse game theory seeing it as a weapon for the expansion of capitalist ideology, as a tool for the 'capitalistic mind'?

Professor Binmore: I'll tell you what I think, it's a speculation, I don't know the answer. They don't want to believe that human beings are or could be unpleasant. They would like everybody to be like Rawls. You can see what frequently happens – some companies go bust and the workers say: we're going to be fired, let's take it over as a workers' cooperative. And see what happens: first, everybody works together; they look not for their individual good (which is a right-wing assumption) but for the collective good (a left-wing assumption). It works for a while, but then people think that "X isn't putting a fair share in, why should I?" And gradually people cheat and this is the problem of the commons. The question is how long it takes – there are workers' cooperatives which survive for a very long time and I would be really interested to study the reasons why some of them collapse quickly and some of them not. But this is not really socialism, it is syndicalism. So people attribute the failure of syndicalism or socialism to the fact that people are evil. And it's not just left-wingers, I've got a quote from Ronald Reagan who famously said "I know in my heart that man is good"; of course, he didn't know it in his head. I've got this appreciation from right-wingers, because for intellectuals in those days, you had to be a Marxist to be an intellectual. You guys don't remember this. Here you were in real trouble if you weren't pretending to be a Marxist, but in England, if you pretended to be an intellectual and you didn't take Marx seriously... I could never make any sense of Marx, he didn't seem to hold together at all... like Immanuel Kant. I'm still a minor icon with the left-wing of the Conservative Party in England. That doesn't do me any good, but there it is. But my conclusions are liberal. Not liberal in the sense of left-wing, but liberal in the sense I call whiggish, because I didn't want to be pretentious and Whig sounds kind of absurd, nobody can take themselves too seriously by calling themselves a Whig, there's really classical liberalism so my conclusions are classical liberal conclusions. This is how I want to see them. I think where the argument leads is not to unregulated free markets or to command. That's not the choice. What we look for is regulated markets, because most markets are imperfect and the subject of mechanism design is hugely important.

RFA: You call that planned decentralization.

Professor Binmore: Planned decentralization, that's right.

RFA: You begun to have a philosophical interest, when you've met with Rawls, but it seems that in a sense that always moved you through fields.

Professor Binmore: When I was 16 years old I've read not all, but some of St. Thomas Aquinas's work about the proofs for the existence of god. If you had told me that I would take an interest in social science, like most people scientifically trained I would say that's all nonsense. It's just people with no idea sounding off. And I held this view

for a very long time. When I came back from America with my wife I was offered a job at the LSE again, but it wasn't a very good deal, and the University College offered me a better deal, so I went to UC London. One of the reasons I left from America was the fact that I didn't want to be in charge of anything. But things had changed so much that you really had to raise money, so I bid for a research center with government money and I won against a very fierce competition. I've got approximately 5 million pounds, approximately 10 millions $, but when you start paying salaries...

RFA: How large was the team?

Professor Binmore: We had about 12 faculty members in economics, half that many in psychology, and one in anthropology.

RFA: So it was a multidisciplinary research center.

Professor Binmore: Well in those days you had to be multidisciplinary to have a chance to get the money. So I just called some psychologists and I said we need some money, do you want to be in this? I've called the evolutionary psychologists and experimental cognitive psychologists. As a matter of fact, because UCL was a top class University, those guys were very, very good. We cooperated with the psychologists on experiments, and they learned, and maybe it's different now but psychologists have this myth about what economists believe, namely that people care only about money, and this type of nonsense. That was very successful. But then I wanted to get the money renewed, and in those days, it was not long after Margaret Thatcher, you had to generate money, to do things for industry if you were an economist. I thought to get consulting work. I thought I was going to get the contract to redesign the electricity market for England and Wales, to computerize the electricity market and I was astonished when I didn't get this, I knew who my competitors were on the shortlist... just silly consulting companies with no experts and I've got Bob Wilson the big computing-electricity guy in those days but then I have never thought I would get the contract to design the UK 3G Telecoms auction.

RFA: When did this happen?

Professor Binmore: This would have been by 1998 and the actual auction in 2000 and I thought this would be very easy, I would find out what the problem was and I looked in econometrics for many, many papers on auctions for the theorems I would need and when I looked in econometrics I found it all useless - it was all about how to handle incomplete information in auction contexts, but what matters in telecom auctions are information that industrial organizations do posses. But then it was a huge success. I knew it was going to make more than the finance ministry had written (3 billion pounds); that was what they expected to make. And it made 21.47 billion pounds, 35 billion $. I knew it was going to be much, much more than 3 billion pounds but I had no idea... so I pretended all the time that I wasn't surprised and after that I got all kinds of work, so I ran a consulting company not to make money for myself, at least not originally, but I had to generate money for the Center and, more importantly, to find

work for the younger economists because you know, if you are running an economics department in the UK, and you want to be top class you have to compete with American universities. They offer more money and these guys are speaking English already, so it was a way of supplementing their income in a way that was interesting for them...they were around the world designing telecom auctions here and there and some of the stories are good but perhaps not interesting for philosophers. Bottom line, I've got my money renewed from the government, which was important, I think for another 5 years, then we've got renewed later for another 5 years, which was most unusual. Towards the end I began to... well, my selfishness appeared and started to take some money for myself...I was like Thales ... so, I tell that story to all the people who think philosophy is useless because it is an example of applying philosophy to real life; what I always found in doing consulting work is that the industrialists or the directors would turn me up this problem and I already knew the answer straight off (not all the details). So, if I had no idea what to do I would say no, you need to go to another consulting company; that would be better for you.

RFA: So, this could be a clue to the way you see the perks of philosophy. But how did your "sorting out philosophy" take place?

Professor Binmore: In writing the original book... I wrote my working papers probably back to 1982 or 1983, and I've read some analytical philosophy, I've never read anything similar in social, moral or political philosophy and someone said to me "You know, your ideas are very similar to David Hume"…I've heard of David Hume, I knew about scientific induction, I've got the *Treatise on Human Nature* out of the library and I read... oh, how wonderful stuff!

RFA: And this is how Hume became your intellectual hero?

Professor Binmore: Yes, because he had basically the same ideas as me and then, you know, there's no use saying that his ideas are unorthodox and you should pay attention to it because it is an idea of Ken Binmore, they usually listen to that if you say "This is David Hume's idea.....David Hume's!". I did this thing with John Nash, we were working on interpersonal comparison and I read Kant. By this time I was at the University of Michigan; I was in the library and one by one I took out all the books written by Kant and I was looking for the proof of the categorical imperative. And... there isn't one! The nearest proof is in the *Groundwork for Metaphysics of Morals* and when you tell people the argument they laugh and laugh, it's hilarious! And philosophers won't believe that there is no such argument. They say: "Oh, you haven't read the *Critique of Practical Reason*!"

RFA: He also says in the "Critique…" that the arguments for the imperative are mistaken...

Professor Binmore: Yes, you've got to give Kant credit to be honest. I think that whatever you want to criticize Kant for, there are two things you shouldn't criticize him

for. The first is the fact that he was a very creative author, with many challenging insights. Number two: no problems of integrity at all, unlike Hegel, who I think was utterly phony.

RFA: How do you see Kant as a philosopher of mathematics?

Professor Binmore: Hopeless, hopeless...well how could you be a philosopher of mathematics if you don't know any mathematics. I mean you can't be a philosopher of biology without knowing any biology. Right now I write biology papers and jointly running a project with some biologists, so I think that the philosophy of biology is at a really good state chiefly because of the philosophers who do it …it doesn't occur to them not to understand biology. But that is not true for many philosophers of physics or mathematics. I'll tell you a story, you could delete it if you like. I went to listen to Saul Kripke, he was the keynote speaker at a Congress of the History of the Philosophy of Science at Uppsala. Oh, what a bizarre gentleman! Basically his talk was about "what are the natural numbers". Frege had a view on this and Bertrand Russell had a view on this and Wittgenstein had a view on this. And I know that Wittgenstein had a view on this, but I thought to myself: "Wittgenstein had a view on this? Who cares what Wittgenstein thinks about the foundation of mathematics, he did not know any mathematics at all!" and I don't know how much mathematics Bertrand Russell knew because I've read somewhere about *Principia Mathematica* and what he thought about his book later, self deprecatingly: "The parts I understand are quite good!", because, you know, most of it was written by Whitehead. And now here's Kripke's view on what natural numbers are. The first natural number is 1. And there are people who say that the first natural number is 0 but I say it's 1. And the next is 2...he eventually reaches to 9. And here we have a problem. What's the next natural number? I say it's 10, 1 and 0 in the decimal notation, and this was his lecture on the foundations of mathematics.

RFA: We are back to our questions about multidisciplinary research. From what you say it appears that this is the most important aspect, the multidisciplinary background: you need to know mathematics in order to be a philosopher of mathematics and so on and so forth.

Professor Binmore: There are not just philosophers who need to be multidisciplinary; the economists in particular desperately need to learn from philosophy.

RFA: And the sociologists?

Professor Binmore: I don't know much about sociology. You should say to me "Why don't you learn some sociology?"

RFA: What is there to read for economists in philosophy? What do they need to learn?

Professor Binmore: Well, it's not useful to generalize, so let's take a particular case. The orthodoxy in economics is Bayesian decision theory, the expected utility theory. I think that part of the reason for the credit crunch is that the mathematical models in use take for granted Bayesian decision theory, so uncertainties can be quantified using subjective

probability distributions and then you maximize expected utility. A philosopher would ask the kind of question like this: "Is Bayesian theory right? Isn't Decision Theory based on a collection of axioms which are only consistency axioms (so if you want to be consistent you take decisions based upon that distribution of probabilities)?" A philosopher would ask, or I hope he would ask: "Well, why should you be consistent?" Savage, who wrote *The Foundation of Statistics*, which isn't really about statistics, but about Bayesian Decision Theory, explains and understands this point. He says on page 16 of his book: "It only makes sense to be consistent in small worlds, where you could look ahead and anticipate what your reaction would be to all possible future contingencies. And then, if you would be inconsistent, if you look ahead and in this possible world I would be getting consistent with my past behavior, then you correct your behavior to be consistent". And then he sets to discuss applying the theory to the large world, and he says this would be preposterous, "utterly ridiculous". I've recently read a book called *Rational Decisions*, largely about this, and the problem is that really few economists have read *The Foundation of Statistics*, because they have no interest in this subject. All they want is to use and apply. If they were trained to be a little more philosophical, it would be very good for them.

RFA: How do you see the future of Game Theory?

Professor Binmore: Well, now it's like arithmetic. Game Theory is a branch of pure mathematics, just like arithmetic, so when people accuse game theorists of modeling people selfish they completely fail to understand that game theorists don't have any opinion of what people want. That's a given part of the game.

RFA: In "Natural Justice" you say that Game Theory is neutral to analysis.

Professor Binmore: Well, it's not neutral to analysis; it's neutral to what human beings are like. It says that if people behaved in a consistent way in this interpersonal context, they will not behave in this particular way, but in a range of possible ways. If people are not consistent, then it doesn't apply.

RFA: So the future of Game Theory is…

Professor Binmore: Completely safe!

RFA: …is it exactly about mathematics?

Professor Binmore: No, I mean, I think it has increasingly become that way, it's now a branch of mathematics and it will survive as a bunch of mathematics. But it's for economists, philosophers and sociologists (increasingly), political scientists, and mostly biologists to interpret it. What's going on now is very good, because biologists come along, they have new problems and this needs extending the theory, just as mathematics got extended through applications in physics without being confused with physics, so game theory is a branch of mathematics which I hope will flourish and increase because

of the demands of its users.

RFA: Because we will publish this interview in the Romanian Journal of Analytic Philosophy, you've already pointed out that that you've read some philosophical works in this tradition. Could you please tell us more about this? Also, you are famous for splitting the history of philosophy into the scientific tradition and the metaphysical tradition.

Professor Binmore: I don't see why you should attribute that to me because everybody does that. I mean, everything I do, when I give a talk, because of the metaphysical tradition in moral philosophy is so strong, I just draw attention to the fact that there is this other tradition. You know, Aristotle was partly a scientist; Epicurus, what survives from him, is very Humean. And Hobbes, and I would also claim that about Spinoza, Hume, and Russell in modern times. I would like to claim Rawls but, alas, Rawls was determined to fit his ideas into the metaphysical tradition. And also the emergence of the philosophy of physics… when I started talking to philosophers it was because I knew Karl Popper at LSE – he was a professor and I used to go at his lectures.

RFA: And also Lakatos.

Professor Binmore: Oh yes, I used to go. Occasionally Lakatos and I used to go in pubs. So I'll tell you a Lakatos story.

RFA: And also a Popper story, please.

Professor Binmore: Well, I'll tell you a Popper story first. I don't think that Popper knew who I was. We had conversations twice and both times it was about his bitterness of not getting the original version of *The Open Society and its Enemies* published, and then having to go to New Zealand. He wasn't very pleased about New Zeeland! But he was a very authoritarian gentleman. One of the reasons I was pleased to go to LSE was the fact that Popper was there and I thought his department would be an open society, but it wasn't. It was a command economy. But in private he was more open.

The Lakatos story is the the following: we were in the White Horse pub at LSE and he would drink crème de menthe, which is a sticky green liqueur that tastes like mint. He was telling me that he was a mathematician in communist Hungary, a very successful one, so he decided to rise in the communist hierarchy. He was doing quite well but he overreached himself. After the counterrevolution of 1956 he fled over the border into Austria and asked himself: "well, what could I do for a living?" He said: "I would pretend to be a philosopher of mathematics." By this time the whole pub was silent, so he said: "I fooled all the world, I fooled all the world!". Of course it was pub joking! But I said to him: "You may be a pretend philosopher of mathematics, but you are better than real philosophers of mathematics!". He was very pleased with that.

RFA: Did you also meet Thomas Kuhn?

Professor Binmore: No, I've never met him. I find his book a bit boring actually. I mean he seems to make sense but....

RFA: In your paper about backward induction you state that it is better to have a model of knowledge as commitment and that this is better than knowledge as true belief.

Professor Binmore: Well, careful, I wouldn't argue that it would be better always. I certainly would not argue that it is correct in a metaphysical sense. If you want some epistemology, all we have are models, and I think structural realism is a folly. Because OK, we have models, and when they are not consistent with data we throw them away. For a while we turn up with models that fit certain types of data fairly well and that's what we have.

RFA: But we could end up having data which fit only one model, we could get trapped into our own model.

Professor Binmore: Yes, we mostly are. It's better if we weren't, because of the orthodoxy among the philosophers of science which I know in England is realism of some kind and my attitude is that what we actually know is that models fit data fairly well and predict in certain circumstances. OK! Who cares? Is there an alternate reality in some metaphysical world? I have many ideas, but again, who cares? So I don't think the philosophers of science should do metaphysics.

RFA: Please give us some thoughts on being here.

Professor Binmore: It was a very easy decision, really. I mean, I've never came to Romania before. I like traveling, I tend to forget that I got old and tired. When I was in my 20's I never got tired as I see it now, looking back, and then I wrote my books and I really thought that philosophers would take me to their hearts. Just like David Hume with his *Treatise* – he thought that everybody would see the truth, but his *Treatise* did awful in the press, or at least this is what he said. He did get reviews incidentally. Anyway, whenever someone expresses some interest in my work, philosophers in particular…

RFA: How did you come to shape your own account?

Professor Binmore: When I got back to England I was very depressed, because in England we talked about how to raise money and to be in charge, and I didn't want to be in charge, to be the director of an institute, I didn't want to have to be on hiring committees and try and persuade people, but taking a well paid job meant also taking a commitment to improve. The reason why I was offered a large sum of money was that the president of the University decided that it was a good deal to invest in the economics department. I was very depressed and I found myself sitting in front of my computer, writing the books... A good advice for anyone, philosophical advice, is that when you feel depressed, you should make yourself do things. I wrote *Playing Fair* for myself, but *Just Playing* was very much for myself. I knew that neither one was going to be a triumph. I had this revival when I wrote *Natural Justice*. When I wrote *Just Playing* it was

just for myself, so I structured it for myself – that's why you'll find footnotes on items of history. I have a long footnote on the origins of Tory and Whigs.

RFA: So it was like a therapy.

Professor Binmore: Absolutely, it was a therapy.

RFA: You didn't answer the question about analytic philosophy.

Professor Binmore: I did, partly. I'm not an admirer of all the work on modal logic. For example, I forgot the name of the young man who did the paper on the analysis of the Muddy Children's Puzzle.[4]

RFA: Alexandru Dragomir.

Professor Binmore: Alexandru. A very clever, well explained paper but the formalisms seem clumsy. It really is a measure of Alexandru's talent to solve the problem using that framework. For a mathematician like myself you just don't take a framework, part of the problem is first of all to frame the problem in a tractable way and this is often gives you the solution. It seems to me that modal logic (not necessarily modal logic but logic in general) is something like this: here's this formalism, and you've got to stick with it because this is what gets you published and now we'll extend it a bit. But, you know, you should get back to the beginning. When game theorists like Bob Aumann came to this he wouldn't accept this. He wrote this beautiful paper in 1966, borrowing the idea of a knowledge operator from Michael Bacharach. Michael got this idea from philosophers – his wife was a Kantian philosopher from Oxford, so he learned about the necessity operator. In Game Theory you have really talented people, great analytical philosophers if you would allow calling them that. And then they have a simplified framework where a lot of that stuff that is in the modal logic written in terms of propositions just doesn't appear. I've lost count of the number of analytical philosophers I've met applying modal logic to the surprise test paradox, the so called Newcomb's Paradox. They formalize too soon.

RFA: Ironically, in a way, analytic philosophers might be a good audience for game theorists.

Professor Binmore: The very persons who listen to it. I mean, not people who have not analytical ability or interest by and large, neither people like the American moral philosophers. They are completely uninterested because they are Kantian and anything

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[4] Professor Binmore talks about the NORMEV conference that took place in 2011, where he was a keynote speaker [editor's note].

else they are just not interested about, unlike Rawls. Or else, they want to pick up little bits of game theory and pretend they have a solution, to prove that it is rational to cooperate in Prisoner Dilemma situations and other nonsense like this. It would be very good if more philosophers had analytical training and could see what analysis has been done and relate it to subjects. So it's really back to the interdisciplinary.

RFA: Thank you very much, Professor Binmore!

Saturday, November 12th 2011

Interviewers : Emilian Mihailov, Radu Uszkai and Constantin Vică.

Transcript: Radu Uszkai, Constantin Vică.