

## An interview with Thomas Schelling

**Abstract.** Jean-Paul Carvalho<sup>1</sup> interviewed Thomas Schelling, winner of the 2005 Nobel Memorial Prize in Economics, for *Oxonomics*. The interview took place at Professor Schelling's house in Maryland on 14 May 2007.

### Intellectual Roots

**Jean-Paul Carvalho (JPC):** Let me begin by asking about the inspiration for your work. Your ideas have been very influential, they have been highly original, and the results have often been counter-intuitive. How did you arrive at your ideas? Did you generalize from everyday experience? Was there anyone in particular who had a profound impact upon your work?

**Thomas Schelling (TS):** When I finished graduate coursework at Harvard, I was about to take on a wonderful fellowship. But a friend of mine had just joined the Marshall Plan in Washington D.C. in the spring of 1948. He called me up and said he had a chance to go to Paris, but he could not go until he had a replacement, and he asked me if I would like to come to Washington for the summer. And I said, 'Sure.' So I went to Washington and got involved in thinking about dividing up Marshall Plan money among the countries, working mainly on Greece and Turkey. My boss was on loan from the State Department. He was appointed Program Officer in the Marshall Plan mission to Denmark and asked me to go with him. Then after a year, I was invited to go to the Paris headquarters of the Marshall Plan. I went there for a year, and spent all of my time negotiating, mainly negotiating something called the European Payments Union, which was to be something like a clearing house among the fifteen or so Marshall Plan countries. The whole purpose was to liberalize their trade and payments arrangements. Then the head of that office, Averell Harriman, was invited to become Harry Truman's foreign policy advisor. So he went to Washington and took my boss with him. My boss arranged for me to follow, so I went and joined the White House.

We were mainly involved in negotiating foreign aid, both the Marshall Plan money and the military aid, because NATO was now a big part of the negotiation. Then in 1951, the Congress revised the foreign aid business, changed what had been the European Recovery Program into the Mutual

Security Program which was explicitly designed to help induce the Marshall Plan countries to build up their NATO defence forces. I was essentially in charge of negotiation with all of the European countries, negotiating what the US would do by way of economic and military aid, in relation to what they would do by way of conscripting troops and training troops, and if necessary stationing them in Germany. All of this involved bilateral negotiations with all of those countries. And I got fascinated by what I thought were interesting principles of negotiation.

When I left the government in 1953, I decided I would make negotiation my main interest. So I started working on it, and I discovered that there are a lot of other areas of interest in negotiation – negotiation with criminals, negotiating with one's own children, divorce negotiations, labour negotiations. And I began to see what I thought were principles that applied in all kinds of negotiation. Somewhere along the way it occurred to me that the key to having your way in a negotiation was some ability to become committed to a position. I developed the idea of commitment and then saw that it applied not just to a bargaining position but to threats, because most threats involved the prospect of carrying out an act one would prefer not to carry out, and therefore one would need some motivation to carry it out to make the threat credible. That meant finding a way to get committed behind the threat. And the same with promises.

So I eventually wrote what I thought was going to be a book on the subject, but when I got to about 250 manuscript pages, I decided this is probably too short for a book and too long for an article. So I decided to see whether I could trim it down to article length. I spent several months going over and over it to try to get it short enough to be an article without losing anything. And I finally sent it to the *American Economic Review* which, after it was refereed by somebody, immediately accepted it with virtually no change (Schelling, 1956). The person who refereed it was Kenneth Boulding, one of my favourite economists. He was so enamoured of it, that he wrote me a long letter about it. He

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asked me to join an editorial group he was establishing for the *Journal of Conflict Resolution*. And somehow I happened to meet him on the streets of New Haven, Connecticut. I met him, we chatted and he told me how much he liked my article. I said, 'You know, there is one thing I couldn't figure out how to get in that article, something that really belonged there, but I just couldn't make sense of it.' I started explaining to him what it was, and he said something, I don't know what he said, but he said something that all of a sudden crystallized the idea of focal points. And I thought, 'Aha, I've got it now!'

I went back and spent a year developing the focal point idea, mainly thinking it applied to the outcomes of explicit bargaining, but finally perceived that it also showed up in tacit bargaining. I thought I would try to prove the psychological possibility that people can arrive at common solutions without communicating, and to test that I produced the little questionnaire that so many people are now aware of. I sent it out to fifty people, and got answers that strongly suggested the possibility of coordination using clues that usually had nothing much to do with logic. Sometimes it had to do with symmetry, sometimes it had to do with the power of suggestion. And that ended up as an article (Schelling, 1957) in Kenneth Boulding's *Journal of Conflict Resolution*, in the first issue of the journal actually.

The next thing that happened was that I read the new book by Duncan Luce and Howard Raiffa called *Games and Decisions* (Luce and Raiffa, 1957), and realized that what I had been doing could be sort of construed as game theory. So with that in mind, I spent six months in London in 1958 writing something that also appeared in the *Journal of Conflict Resolution* (Schelling, 1958), in fact it appeared as a whole issue because it was that long. Kenneth Boulding was still the editor of the journal, and I somewhat presumptuously subtitled the article 'Prospectus for a Reorientation of Game Theory,' thinking that game theory might be enlarged to include the kind of bargaining tactics that I had been interested in. Of course, no game theorists took it seriously. But I think it very substantially popularized my kind of analysis, and the *Journal of Conflict Resolution* became a major journal for publishing things on the strategy and tactics of negotiation. So that is sort of the background. I don't think there were any key people involved, except Kenneth Boulding.

## Conflict and Cooperation

**JPC:** The 2005 Nobel Memorial Prize in Economics was awarded to you and Robert Aumann 'for having enhanced our understanding of conflict and cooperation through game-theory analysis.' You mentioned your thinking about the strategic aspects of negotiations, and the several concepts that you developed, including credible threats and promises and the importance of precommitment. One of

your most important, and again counter-intuitive, insights is that a nation may be better off when its enemies are stronger rather than weaker, and defensive measures can provoke conflict. Applying this insight, how was the use of nuclear weapons prevented during the Cold War?

**TS:** In the Cold War – if you think about it as a nuclear confrontation between the USA and the USSR, which is mainly what it was (the British nuclear capability had something to do with it, but not much) – there were two problems with respect to successful deterrence. One was to make it appear highly likely that if attacked with nuclear weapons, there could be and would be retaliation on a scale that would make the original attack not worthwhile. That required demonstrating that after an attack one would indeed have motivation to retaliate, and the capability to retaliate. The other half was to demonstrate that in the absence of attack, one would have no motive to initiate the war. That was essentially the promise to go with the threat.

In this country, it took the government twelve or thirteen years to learn some of the rudiments of being committed to a retaliatory attack. During all that time, we had a threat of retaliation based entirely on a modest number of aircraft based on a small number of airfields. It was not until 1957 that a investigation reported that these airbases were vulnerable to a surprise attack, that all the aircraft on a single airbase could be destroyed by one bomb, and the number of bases was such that enough Soviet aircraft could evade the radar that we had in Canada and probably attack all of our airbases before the planes could be fuelled and arranged for takeoff. That meant that the Soviets might not be deterred by the threat of retaliation, because they could destroy the means of retaliation. That report almost coincided with the agreement between Khrushchev and Eisenhower to hold a conference on measures to safeguard against surprise attack. There was a high-level committee of the government, Assistant Secretary of State, Defense, Treasury, CIA and some other agencies, to prepare for these surprise attack negotiations and to try to decide what the issues were. They came to the conclusion, partly under the influence of staff that had been assembled, that the problem of surprise attack was that it might destroy the means of retaliation and therefore be undeterred.

That transformed the entire US strategic posture. We immediately began what was called 'Airborne Alert,' which was to keep a fraction of the strategic bomber force in the air at all times. They would take off and fly toward the Soviet Union, turn around just before they reached Soviet air defence radar, and would be replaced by other aircraft which would take off as they returned. This was to guarantee that we would always have the capability to have some aircraft reach nuclear targets in the Soviet Union. At the same time, we cancelled the two main intercontinental missile systems, one was called Atlas and one was called

Titan. These were huge liquid-fuelled missiles. Because they were liquid-fuelled, they could not be kept fuelled, they had to be fuelled just before takeoff. And refuelling was going to take about three hours, which meant that unless they had at least three hours warning they could be destroyed before they took off. Then all the US effort went into completing the Polaris submarine program, it being believed, correctly I think, that the submarines of the time could not be detected and therefore were invulnerable to attack. And the land-based missile became a solid-fuel, small-size missile that could launch almost instantaneously in case of warning, and was small enough to be kept underground, hardened against anything but a close attack and easily dispersed so that no Soviet bomb could destroy more than one outgoing missile. That essentially transformed a potentially non-credible threat of retaliation into a capability that could not be destroyed.

That still left the question, would we under attack use nuclear weapons? Especially, would we respond with nuclear weapons to a non-nuclear attack, an attack in Western Europe? The question was how do you persuade not only the Soviets and the East Germans, the Poles and everybody else, but how do you persuade Britain, France and Germany that we have a nuclear threat against a conventional attack? And there the device was very simple. President Truman asked the Congress to authorize stationing seven divisions of US troops in Germany. When Secretary of State Acheson went up to the Senate to be interrogated on what good seven American divisions would be in Europe, when even with seven divisions the Soviets could overrun Western Europe, his answer was very simple. He said that what those seven divisions can do is not defend Western Europe, but can guarantee that if they are destroyed and captured the American people will not let the war stop there. These are hostages of fortune, they are there to be threatened, to die or be captured, and escalate the war. I think that was a good answer, and I think it impressed the Senate, and I think it surely impressed the Soviet Union. So that was part of making the threat, that was the commitment. Essentially, seven divisions were hostages.

Then how do you promise that you will not launch a surprise attack? That was important because in the event that the Soviets anticipated an attack, they would almost certainly launch their own, because the advantage of making the first launch was that you could destroy a large part of the enemy nuclear force. That raised the question of what was called 'pre-emptive attack' – if you thought an attack was about to come, would you launch a first strike of your own? And on that, Secretary of Defense McNamara, in talking about making the US retaliatory force invulnerable through submarines and this Minuteman missile, said that he hoped the Soviets would do the same. The Congress said, 'Why do you want our enemies to have strategic

weapons that are invulnerable, isn't it better if we could destroy them?' To that, McNamara said, 'No, as long as we can destroy them, they will have to worry that in a crisis we might attack, and that would provide them the only possible motivation for attacking us. As long as they know that we can't destroy their retaliatory force, they will know that we are deterred just as they are, and we will have a stable relationship.' And the Soviets gradually developed comparatively invulnerable forces, not exactly as we did, I think they had some submarines, but they had some mobile forces that we would not know the location of to attack.

So this was a case of having to combine a promise with a threat. The threat of 'if you attack, we will respond,' and the promise 'if you do not attack, you are safe.' What I don't know at all is whether the Nobel Prize committee considered my theoretical writings or my writings about nuclear strategy. Because in 1961 I published, with a co-author, a small book on arms control (Schelling and Halperin, 1961), explaining all of this strategy, and that book became sort of a bible for a lot of people. Then in 1966, I published a book called *Arms and Influence* (Schelling, 1966) which was all about nuclear deterrence and things of that sort. Because already in the 1960s my work was not quite but almost exclusively on nuclear weapons policy. I just don't know to what extent the Nobel Committee took that into account.

**JPC:** You opened your Nobel lecture with the phrase 'The most spectacular event of the last sixty years is one that did not occur.' That is a reference to the fact that nuclear weapons have not been used in war since Hiroshima and Nagasaki. In the last decade, the geopolitical landscape has changed somewhat. A US missile defence shield has been proposed, we have seen China emerging as a political and economic superpower, and we have seen aggressive posturing by countries such as Iran and North Korea. What then are the prospects for the use of nuclear weapons in the next sixty years?

**TS:** First, China. I think China has been exceedingly conservative in its nuclear policy. They had a war with North Vietnam, and never considered nuclear weapons. They had a border skirmish with the Soviet Union. They have a very modest nuclear capability. They do not show any sign of trying to catch up to the US in terms of nuclear capability. They do not make nuclear threats. They have nominally adopted a 'no first-use' policy, meaning they will never be the first to use nuclear weapons. That implies no threat against Taiwan, it implies even no threat against US naval warships that might be defending Taiwan. So I think China is not any kind of serious nuclear enemy. A lot of people seem to think that every nation needs at least one enemy, and since Russia is no longer an enemy, it has to be China, and I think that is nonsense. I do not see any reason why China cannot go on becoming a more and more civilized

member of world society, and if they rival us economically, that is all to the good. It means they will more and more devote their attention to being a main player on the global stage, which will require them to become more democratic. So I do not see any reason to boost the idea of US-Chinese competition. They have had nuclear weapons since 1964, and have shown no more inclination to use them than the French or the British. So that part does not worry me.

India and Pakistan have always had people that attend the annual meetings at the Institute for Strategic Studies in England. When I used to attend arms control or nuclear energy conferences in places like Aspen, Colorado, we always had Indians and Pakistanis. They are as sophisticated as the Americans, the Russians, the British or the Germans. And I think they understand that a nuclear exchange would mean the end of the world for them. I think they understand that perfectly. There is always a danger that they will get into some kind of warfare and one or the other will be tempted to experiment with a few nuclear weapons, maybe battlefield weapons, not population weapons. But I think they watched the US-USSR standoff carefully enough to understand that they need to have their own weapons secure against attack, they want each other to have weapons secure against attack, so there cannot be any nuclear pre-emptive strike. And I have a hunch that they will be more careful than ever to avoid any kind of border skirmish that might escalate.

Iran and North Korea are different. In all the conferences on nuclear weapons policy that I have attended over the last forty years, I never saw an Iranian or a North Korean. And I just do not know how they think about nuclear weapons. I consider it important for them to recognize that if nuclear weapons since Hiroshima and Nagasaki have been good for anything, they have been good for deterrence. As I mentioned in that Nobel lecture, the US did not use nuclear weapons in Korea or Vietnam, Margaret Thatcher did not use nuclear weapons against naval vessels in her war with Argentina, Golda Meir the Prime Minister of Israel had perfect targets for nuclear weapons in the 1973 war and she did not use weapons, and the Soviets lost a war in Afghanistan without using nuclear weapons. I hope that there are Iranians and North Koreans who look at this kind of history and realize that while they may believe they have to have nuclear weapons to deter the United States or Russia or Israel or somebody from ever attacking them, that being the first to use them, at least in the case of Iran, would probably lead to the destruction of Iran, because Israel must have secure nuclear weapons it could deliver in response. In any case, if either country initiated the use of nuclear weapons, they would become instantly a pariah, and I think the US would probably withdraw recognition, maybe demand their loss of sovereignty, or some kind of unconditional surrender. So I think if they do not feel the abhorrence of nuclear weapons

that the rest of the world feels, they will at least know that the rest of the world would despise any use of nuclear weapons. So while I do not like to be called an optimist, I would at least say that I am not nearly as pessimistic, but that is partly because I hope that the Iranians and the North Koreans have people that are thinking deeply and seriously about what the responsibilities are that go with possessing nuclear weapons.

### Coordination and Culture

**JPC:** In a set of early experiments in the social sciences, which you have mentioned, you demonstrated that even in pure coordination games, with no variation in payoffs that might enable players to select among multiple equilibria, certain actions are salient. You called these action profiles focal points. Most famously, when you asked your students at Yale to nominate a meeting place and time in New York City, the majority converged on the clock at Grand Central Station at noon. Out of the extremely large space of alternatives, it is astonishing that this was focal. But we are now almost fifty years on, and nobody really understands what is going on with focal points. At the Arne Ryde Conference held in your honour on 23 August 1997 in Lund, Sweden, you mentioned that ‘Focal points have done more for the theory of games, than game theory has done for the theory of focal points.’ What do we need to understand focal points? Where do we start?

**TS:** Well, let us go back to under the clock at the Biltmore. That was 1955 or 1956. At that time, most of the private colleges in New England were either all-male or all-female. And if a college man and a college woman wanted to have a date, they had to meet some place, and New York was the obvious place, because that was where there were plays and cabarets and baseball games, and everything else. The only way to get to New York was by railroad, and all of the New England railroads went to Grand Central Station. So if you thought of some New England college boy meeting some New England college girl, they were all going to go to Grand Central Station and the information booth at Grand Central Station was under the clock which was under the Biltmore Hotel which was built above Grand Central Station. So that was sort of obvious.

But then if you ask people, ‘You are to meet somebody in Rome, where are you to meet?’ ‘You are to meet somebody in Paris, where are you to meet?’ They don’t have any trouble. They go to Saint Peter’s and maybe they go to the front entrance, or they go to the Pietà. In France, they all go to the top of the Eiffel tower. Then you play the game ‘Name a place anywhere on earth.’ You don’t know whether the other person is Muslim, Catholic, Protestant, atheist, Jewish, Buddhist, rich or poor, you don’t know what language the person speaks – you must identify some place

on the face of the earth, you don't have to go there, but just name the place. That turns out to be very easy. It is always the North Pole. Once in a while, a smart person says the intersection of the Greenwich Meridian with the equator.

Then you will have to see whether that has application, and I find an interesting application. During the 1950s, the US and the USSR negotiated in Geneva something that was to be called the 'Open Skies' arrangement. 'Open Skies' was to mean that US reconnaissance aircraft and Soviet reconnaissance aircraft would have free access to this area, the purpose being that any preparation for surprise attack would be visible from the air, and the best defence against surprise attack would be for the US to be able to fly over East Germany and Poland and see whether huge amounts of military force were being brought by railroad or road to the border between East and West Germany and the same for the Soviets over West Germany. There were a number of proposals for what should be the area of this Open Skies arrangement. Should it be just East and West Germany? Should it be just Poland? Should it include France and the Lowlands? Should it go north, south into Italy? And there was an interesting feature of every proposal discussed. Every proposal discussed was an area that began at the North Pole and went down two Meridian lines to a particular latitude. So it was a pie-shaped triangle anchored on the North Pole. The question arises: why did they all take that shape? I think the idea was that there was something focal, something magnetic, something sort of natural, that it had to be a triangle based on the North Pole, consisting of two Meridian lines and a latitude line. And I thought, 'What could be a more descriptive, unconscious use of focal points than this notion that whatever this area was going to be, it had to have that shape?' I found so many instances like that, that I thought that a great many focal points have that characteristic without the people who follow them being aware that they are indulging in something that can be analytically identified as focal points.

So this idea I think has wide application, and as I mentioned earlier, my main disappointment in the popularity of the focal point idea is that it is so rarely, if ever, used for the purpose for which I originally devised it, namely to anticipate bargaining outcomes.

### Emergent Social Behaviour

**JPC:** You mentioned that you had a paper published in the first issue of the *Journal of Conflict Resolution*. You also had a paper published in the first issue of the *Journal of Mathematical Sociology*, in which you presented a stunning result: complete residential segregation by race was the most stable outcome even if agents prefer to live in integrated neighbourhoods (Schelling, 1971). That was perhaps

the first agent-based model in the social sciences, and it is a powerful example of an 'invisible-hand' explanation. How useful do you think such agent-based models will be in the social sciences, especially in fields like macroeconomics which at present neglect coordination issues?

**TS:** Well, I have two kinds of thoughts about that. One is that I think these agent-based models are best used to discover possibilities, and to demonstrate possibilities, and to develop very fundamental ideas. I am not sure that they have a great future in becoming more and more detailed, and more and more realistic. I think of them in the way somebody once told me. One time I spoke with somebody who did wind tunnel experiments, and I asked 'What are wind tunnels good for?' And he said, 'They are good for two things. They are good for very basic research on how bodies respond to the passage of air. You can put in the wind tunnel things that are spheres, cylinders, squares, dodecahedra, things shaped like wings, things shaped like propellers, you can have them smooth-surfaced or rough-surfaced, you can have them wet- or dry-surfaced, and you can learn a lot. And if you have a finished model designed for an airplane, you can put it in the wind tunnel and make small manipulations to see if you get better results.' But, he said, 'You never start with a cylinder and a wing and gradually improve it up to an airplane. Pretty soon, you don't know what you are dealing with, it is too complex. You want to learn all the basic things with small models, and then if you want to test the finished thing, you can. But you never get to the airplane by elaborating the basic model.' And I tend to feel that most agent-based models are like that. Up to a point, you can complicate them more and more, and learn more and more. But the dream of making it ever approach reality – you will simply get bogged down, because you cannot get there from here.

Now there may be an exception. Researchers have modelled the entire traffic pattern of Albuquerque, New Mexico.<sup>2</sup> They had identified every vehicle in Albuquerque and studied traffic patterns, and they wanted to know what would happen if they built a couple of bridges over the river. What it would do to where the population settled. What it would do to changes in traffic patterns. And they wanted to look at what would happen if they blocked off some downtown streets and so forth. I had the impression that they were going to be able to get some results, and I wondered whether that contradicted my earlier notion. But maybe if you start small, you can learn a lot of principles, but if you want to go all the way to mimicking down to every car and truck and bicycle, maybe you can do something there too. I doubt whether macroeconomics is going to make much use of it. I think epidemiology is proving to be a useful field, and it may be that some

2. Barrett et al. (2004) summarize some of this work.

migration patterns can be studied. I know one interesting macroeconomic application by Josh Epstein and Rob Axtell at the Brookings Institution, who look at what happens if you change the retirement ages of the social security system (Axtell and Epstein, 1999). It looked to me as if they were getting interesting results. They would postulate a few things about what determines a person's decision when to retire. It had to do with when other people their age were retiring, it had to do with benefits, and so forth. And I thought they got interesting results, so that I think there are a lot of things about behaviour you can learn that way. But, as I said, I think they are good at the basic level and maybe good at the fully implemented level. But you don't build an airplane out of a wind tunnel.

### Behavioural Economics

**JPC:** Some commentators have credited you with being the father of behavioural economics, due to your early work on self-command (Schelling, 1984), and so forth. What do you think about the emergence of behavioural economics as a sub-discipline of economics, as well as the criticisms from Ariel Rubinstein and others who claim that models in behavioural economics are only minimal departures from the standard economic approach, and more radical departures are needed to model actual human behaviour?

**TS:** I had something to do with the beginning of behavioural economics, because the first grants program in what we now call 'behavioural economics' was a joint program of the Alfred P. Sloan Foundation and the Russell Sage Foundation, who had a committee of four of us. They advertised grants of up to \$50,000 for people who wanted to work at the intersection of economics and other disciplines, whether it be psychology or anthropology or sociology. We got proposals, we evaluated them, and gave out money. Eventually, I think the Sloan Foundation discontinued, whereas the Russell Sage Foundation not only continued but then put together what they called 'The Behavioural Economics Roundtable,' consisting of about ten of us. A few of the group got the idea that we should have summer workshops for PhD students in economics. They began to have these two-week workshops in Berkeley, California, which were very successful and drew a lot of people. This Roundtable added a few non-economists like Dan Kahneman, who recently got a Nobel Prize. Eventually, most of the names now associated with behavioural economics became attached to this Russell Sage Roundtable. I am still on it, but I have not attended a meeting for two or three years. I think it has been very effective in making this a respectable line of inquiry. It is sometimes referred to as the juncture of economics and psychology, which I think is pretty good. On the other hand, one could as well say sociology and anthropology.

It is not the first big effort, because the *Journal of Conflict Resolution* was very open to what you might call 'experiments in behavioural economics.' A lot of these ultimatum games and other experimental games were already being tried out in the mountains of Indonesia, among Peruvian Andeans, and so forth, so that a lot of what you might call 'experimental game theory' had already been going on. But I think behavioural economics began to look into the extent to which decisions were influenced by ethical attitudes, got interested in systematic errors in judgment, and developed this idea I mentioned earlier about whether the default is 'you donate your organs, unless you state otherwise' or 'you only donate your organs if you positively say yes.' I think Dan Kahneman and his deceased colleague Amos Tversky were extraordinarily ingenious in teasing out departures from what you might call 'purely rational choice.' But they did not disparage irrational acts so much as to point out that people use a lot of what they called 'heuristics.' They said that usually these are hugely economical in terms of time and thought, and work fine. On the other hand, sometimes they can lead you badly astray, and it is important to learn when they can lead you badly astray. This is partly so that scientists know what to expect, and maybe partly so that people can learn to avoid errors of judgment. I think some of what they discovered is extraordinarily dramatic. Some of it simply has to do with not understanding sampling theory, and not understanding frequency distributions. Some of it has to do with the power of suggestion, of irrelevant, random suggestions. And I have long believed that, in a lot of areas like the purchase of insurance, not only are people remarkably irrational but even the market appears not to be rationally designed. So I believe behavioural economics is long overdue.

Essentially, it is overcoming an aversion to the testing of the rationality hypothesis. Twenty years ago, I was on a committee relating to the Human Genome Project financed by the US government, which required that five percent of the funds for the project be spent studying the ethical and moral implications of genetic knowledge. I was on a committee to study the implications of genetics for the insurance industry. There are big implications, such as should life insurance take into account what is known genetically about somebody's life expectancy? Should disability insurance take that into account? One of the interesting things I discovered was that the main life insurance companies, such as Prudential, if they gave you a physical exam, the result of the exam meant either that they gave you the policy at the price that goes with your age or they refused you the policy. We had very senior people from Sun Life of Canada, Prudential, and two other major companies, and I asked them, 'If you give somebody a physical exam and it turns out he has a heart problem, why don't you, instead of saying "no life insurance," charge them double the usual rate?' But they did not really have an

answer. First, they said, 'Nobody would buy it.' And I said, 'Oh, come on. If I were a young father of two and discovered that I had a heart condition that doubled my chances of dying before I reached sixty, don't you think I would pay double the rate, because I can't afford to die and leave my wife and two children?' I said, 'Could I go to Lloyd's of London and get insurance?' They said, 'Sure, Lloyd's will insure you. They will study your life expectancy and set a premium accordingly.' I said, 'Why don't you?' I got the impression that the answer was 'Nobody else does it, so we don't.' What an irrational way to run an insurance industry. My colleague Robert Aumann, with whom I shared this prize, is on record as believing that behavioural economics is a flash in the pan, that it is not going anywhere. I think that is partly because he is totally devoted to the universality of rational choice. But I think this has opened up a whole range of the study of behaviour. And I think it has a big future.

### Looking Ahead

**JPC:** What do you think lies in the future for economic theory and game theory? What are the major blind spots today, what are the major deficiencies? What questions do you believe are being neglected?

**TS:** You know, I don't know enough about game theory and what game theorists do to be sure. A book recently came out that interviewed nineteen game theorists including me (Hendricks and Hansen, 2007). One of the questions was, 'What do you think are the gaps or the things that are being omitted?' And my response was, 'I think there is a lot that non-game theorists can do with game theory that they are not doing. But I don't know any place where game theory per se is missing great opportunities.' This is probably because I do not know enough about game theory to know what the opportunities are that are being missed. But I explained that the difference between me and most game theorists, I believe, and it has been all along, is that I am not so much interested in how rational players arrive at a common understanding of a solution, but rather how people influence each other. That has always been my interest. And the whole idea of commitment, of threats and promises, even how to elaborate a focal point, to make something a focal point, this all has to do with influence. In nuclear strategy, it all has to do with influencing another party.

Most cooperative game theory seems to assume either that a threat is enforceable or is not enforceable. My interest has always been in the ways that people can make believable threats. What are the institutions that allow it, what legal arrangements allow it, how does it depend on the capacity for reputation? How does it depend on the use of agents? How in the world do nations, people, or organizations go about committing themselves to threats and promises in

bargaining positions? This is an empirical and historical study. I have learned much more about it from reading ancient Greek history and by looking at salesmanship than studying game theory. I have a hunch that there are marketing departments in business schools that have a lot of knowledge of the empirical sort that can fit into game theory. So I do not really expect that game theory itself is going to undergo great enlargement for the benefit of economics or anything else. But a lot of elementary game theory could still be used by legal scholars, and anthropologists, all kinds of people.

Somebody once asked me, 'What is the greatest thing that game theory invented?' In fact, at that Lund conference, I said 'I think the biggest thing that game theory invented was the payoff matrix.' If you look at the history of things like that, the most important symbol that was ever developed was the equals sign. That gave rise to algebra, which is central to all of mathematics. And the equals sign is simply an accounting device, because if you have two things that are equal, you know that if you add the same thing to them, they are still equal. If you multiply them by the same thing, they are still equal. If you take the square roots they are still equal. The same idea showed up in double-entry bookkeeping which began in Venice in the 15th Century. Eventually, national income accounting became based on a kind of matrix accounting system. And the idea that a simple situation involving as few as two people and two choices, can be put together in a matrix, with either ordinal or cardinal payoffs, proves extraordinarily useful. No matter what kind of game theory I teach, about the only thing the students take away that they will ever use again is a matrix. If you show students how many  $2 \times 2$  matrices there are with ordinal payoffs, they become fascinated. I have used a set of sixteen matrices to talk about arms control, the different attitudes two nations can have toward a single weapons system.

So I am a great believer that the most elementary game theory is usable everywhere. And whatever is at the boundary, the frontier, of game theory for game theorists, I am not sure what it is, but I am also not sure that I will ever find it useful.

### References

- Axtell, R. and Epstein, J.M. (1999) 'Coordination in transient social networks: an agent-based model of the timing of retirement', in Aaron, H.J. (ed.), *Behavioural Dimensions of Retirement Economics*, Brookings and Russell Sage, Washington DC.
- Barrett, C.L., Eubank, S., Anil Kumar, V.S. and Marathe, M.V. (2004) 'Understanding large-scale social and infrastructure networks: a simulation-based approach', *SIAM News*, 37 (4), May.
- Hendricks, V.F. and Hansen, P.G. (eds) (2007) *Game Theory: 5 Questions*. Automatic Press/VIP.

- Luce, D. and Raiffa, H. (1957) *Games and Decisions*. Wiley, New York.
- Schelling, T.C. (1956) 'An essay on bargaining'. *American Economic Review*, 46, 281–306.
- Schelling, T.C. (1957) 'Bargaining, communication, and limited war'. *Journal of Conflict Resolution*, 1, 19–36.
- Schelling, T.C. (1958) 'The strategy of conflict: prospectus for a reorientation of game theory'. *Journal of Conflict Resolution*, 2, 203–264.
- Schelling, T.C. (1966) *Arms and Influence*. Yale University Press, New Haven, CT.
- Schelling, T.C. (1971) 'Dynamic models of segregation'. *Journal of Mathematical Sociology*, 1 (1), 143–186.
- Schelling, T.C. (1984) 'Self-command in practice, in policy, and in a theory of rational choice'. *American Economic Review, Papers and Proceedings*, 74, 1–11.
- Schelling, T.C. and Halperin, M.H. (1961) *Strategy and Arms Control*. Twentieth Century Fund, New York.

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