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REINHARD SELTEN: A PANORAMIC VIEW ON HUMAN BEHAVIOR*

Eric van Damme[#]

31 July 2017

Sehr Geehrte Damen und Herren, Familie Selten,

Ich bin sehr erfreut und geehrt dass ich gefragt wurde bei diesem Akademische Gedenkfeier zu Ehren meines Lehrers und Vorbildes Reinhard Selten zu sprechen. Nachdem wir diesen Morgen beeindruckende persönliche Gedanken gehört haben, bin ich sogar der Erste Sprecher in diese Folge mit wissenschaftliche Festreden. Ich möchte mich bei den Organizatoren dieses Treffens für diese Ehre bedanken.

Liebe Damen und Herren. Obwohl Ich einige Jahren in Deutschland gelebt habe, reicht mein Deutsch leider nicht aus um diese Rede in ihre Sprache fort zu setzen. Wenngleich ich die Spracher verstehe, ist das selbst reden davon eine ganz andere Sache. Ich hoffe daher das Sie mir erlauben auf English weiter zu gehen; ich glaube das wäre effizienter.

Ladies and gentlemen. My presentation today will be in two parts. In the first part, I will talk about my own personal development and I will demonstrate how much it owes to my interactions with Reinhard. In the second part, I will talk about some aspects of Reinhard's work more generally.

1. Reinhard Selten's influence on my own personal development

In the biographical sketch that Reinhard wrote for the Nobel committee,¹ he briefly refers to me. The context is the twelve years, 1972-1984, that Reinhard spent at the Institute for Mathematical Economics in Bielefeld. Reinhard states that during that time he began a close cooperation with Werner Güth, who is also here today, and who is described as being in some sense one of his students, even though they never held positions at the same university. Reinhard continues: "Also other people who later became university professors sometimes came to Bielefeld to seek my advice, namely Ulrike Leopold from Graz, Joel Moulen from

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¹ http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1994/selten-bio.html

Lyon, and Eric van Damme from Eindhoven. (...) Eric van Damme needed very little advice and is now a well-known game theorist.” I think that Reinhard here is very generous to me.

I personally think that I needed a lot of advice, and that it was given generously and wholeheartedly. Maybe what Reinhard meant was that I did not need much supervision, in the sense that I was motivated to do what I was doing and was careful in not making too many mistakes, however, doing things well is not the same as doing the right things. I certainly needed somebody who showed me the right way. Let me explain why that was the case, as that will make clear how important Reinhard has been for me. Let me also note that there will be many other people for whom Reinhard played a similar role in pointing out promising and rewarding areas of research. To mention just one example, the same applies to Benny Moldovanu.

1.1 Before meeting Reinhard

Like Reinhard, I have studied mathematics. I studied in Nijmegen, in the Netherlands, from 1974-1979. During my undergraduate studies, I was fascinated by the beauty and the simplicity of some topics (such as logic and analysis), however, other topics seemed far too complicated to me and, accordingly, I did not like these at all. I was even toying with the idea of switching to Management after my Bachelor’s degree. However, before taking such a drastic step, I decided that I would first investigate what the Master’s in Mathematics had to offer. Hence, already before finishing the Bachelor’s, I attended some Master’s courses, among which an Introduction to Game Theory, taught by Stef Tijs, who was an assistant professor at the time. This course was a new addition to the curriculum and Stef told us that there would be follow-on courses as well. For me, this was sufficient reason to stay in Nijmegen and finish the Master’s Degree as well. I should note that I did not have in mind that my career would be in research. Although, overall I did quite well in my coursework, it was clear to me that I could only be a mediocre mathematician, hence, I was looking forward to a career as a high school teacher of mathematics.

The inspiring teaching style of Stef Tijs enticed me to follow all the courses that he taught. I learned that Game Theory was still a relatively young topic, offering many possibilities for research, but also having interesting connections to various other fields of mathematics. It was, hence, natural to also write my Master Thesis with Stef. We were supposed to come up with a topic ourselves, which I found to be extremely difficult. For me, Game Theory was just a part of mathematics and I did not think of an applied project for my thesis. Having seen games with finitely many players in the courses, having studied Aumann and Shapley’s book on games

with a continuum of players for a seminar, and discovering that there was hardly any literature on the intermediate case, it was quite natural to choose games with countably many players as my thesis topic. A short summary of that thesis was published later.²

Stef Tijs stimulated me to apply for a 4-year research training position at the mathematics department of Eindhoven University of Technology when that became available in 1978. It was a kind of PhD-position, but without having a formal education component. I applied and was accepted. I recall that on the day of receiving my Master Diploma, I went to Stef Tijs' office to thank him for all his help and to ask him a question about Game Theory. I had followed courses in non-cooperative theory as well as cooperative theory and I had written my thesis in the latter area, but I did not really understand why there were two theories that, furthermore, seemed to have hardly any relation to each other. Each theory had its own concepts and was referring to a different type of mathematics. I wanted to know about the relations between them and which of these was more fundamental. I don't recall the answer, but I do recall that I did not find it a satisfactory one; maybe I simply did not understand it. It is also quite possible that I had not been able to formulate my question in a clear way.

In Eindhoven, I came to work within the active and stimulating Operations Research group of Jaap Wessels. Although the group focused mainly on one-person decision problems, some members of the group had a side interest in Game Theory. My research topic was not fully specified in advance, and I had the possibility to influence it. One idea was that I would work on inventory theory, but I lost interest in that area after I had read Herbert Scarf's beautiful paper on the optimality of (s, S) rules. I thought further development would be mainly computational, which was not my strong point and not my interest. Hence, I focused on the second assignment that I had gotten: to get a better understanding of John Harsanyi's recent book *Rational behavior and bargaining equilibrium in games and social situations* (Cambridge University Press, 1977). Jaap Wessels had written a short review of this book for the journal of the Dutch Mathematical Association, and was intrigued by it, but he did not have the time to further delve into the matter. He thought the book would interest me and he encouraged me to read it.

² Eric van Damme, "Cooperative games with countably many players", *Methods of Operations Research*, 40 (1980), 107-110.

Jaap Wessels also encouraged the members of his group to seek contacts with other researchers and to present results to the outside world as soon as possible. As a consequence, I applied to participate in the fourth Symposium on Operations Research to be held in Saarbrücken in September 1979, and I was lucky to have my proposal accepted. It was there that I met Werner Güth and Ulrike Leopold and learned that I was not the only young person to work on problems in Game Theory. This was my first presentation ever, and I was extremely nervous, also since I was aware that I did not yet fully master my own topic. Having Werner Hildenbrand as the chair of our session certainly did not calm me down.

I returned to Eindhoven being more even determined to do my PhD in Game Theory. Fortunately, I had the autumn of 1979 to revise my presentation and to submit my paper to the Proceedings of the Conference, and I was glad to see it accepted.³ I was even more fortunate that Jaap Wessels allowed me to pursue my own interests. In fact, when he saw how I struggled with the material, he encouraged me to study the classics, such as Luce and Raiffa's *Games and Decisions: Introduction and Critical Survey*, Thomas Schelling's *Strategy of Conflict*, and the seminal papers of John Nash on cooperative and non-cooperative games. Always ready to listen to the progress or non-progress that I had made, he allowed me to find a reasonably convincing answer to the question that I had posed to Stef Tijs when leaving Nijmegen. However, Jaap Wessels also imposed one important condition: realizing that he was not an expert in Game Theory himself, he insisted that I find a second supervisor, an expert game theorist, a professor, who was willing to testify that my work was of good quality and a contribution to the field. At this point, Reinhard Selten enters the stage.

1.2 Meeting Reinhard

In the fall of 1979, I saw an announcement of a meeting on Game Theory to be held at the Mathematical Institute in Oberwolfach in March 1980. It listed names that I knew from the literature: Harsanyi, Hart, Kalai, Kohlberg, Maschler, Owen, Shapley, Selten, Schmeidler, Zamir and many others. This was an opportunity that could not be missed. I now had the chance to ask my questions personally to Harsanyi. It was clear that that I had to go to Oberwolfach. I contacted the organizer, Joachim Rosenmüller, who informed me that all rooms were booked. However, he was very kind and also told me that, if I would arrange for a place to sleep, I could attend the sessions and join the meals and other common activities. Joachim also informed me

³ Eric van Damme, "Some comments on Harsanyi's postulates for rational behavior in game situations", *Methods of Operations Research*, 38 (1980) 189-205.

that there was a local hotel nearby, so that it should be easy to make arrangements, which indeed it was. Hence, in March 1980, I went to Oberwolfach.

This meeting changed my life completely. It was one of the first larger meetings in Game Theory since 1968 or so. I had never experienced such intense academic activity and fierce debating. I remember the contributions of the participants from Israel in particular. The atmosphere was one of sharp academic fighting and competition within the seminar room, open discussion, a mix of all kind of questions, from the trivial to the most advanced, a lot of laughter, and an extremely pleasant gentle interaction outside of the seminar room. This clearly was a *community*, a group of people on a joint mission. I had never experienced this before. The seminars that I had attended in the Netherlands had been lectures, expositions, one-way communication, with an occasional clarifying question, in order to be polite to the speaker. Here, there was real debate, a joint effort to come to a better understanding of the issues, and a critical mass of people that understood and appreciated each other's work.

Of course, I also stayed in the evenings to talk informally to people, or to just listen to overhear the conversations of the other participants. I think I was not really aware of the work of Reinhard at the time, but he kindly asked me what I was interested in and what I had been working on. This made me feel quite excited, because I thought I had found something new. Remember that, at that time, I was working in an *Operations Research* environment in which the focus was on one-person decision theory. In that field, dynamic programming (Bellman's Principle of Optimization) was viewed as an efficient technique to find optimal strategies. This principle could also be used in games, but my "big discovery" was that application of this principle was not justified when there were multiple decision makers. I had discovered that, in dynamic games, there exist Nash equilibria that cannot be found by dynamic programming and that are better for all players (that is, that are Pareto superior) to those Nash equilibria that are found by Bellman's technique. Being strongly convinced that the task of non-cooperative game theory was to find the best possible Nash equilibrium, I was quite excited about this discovery.⁴

I don't recall how Reinhard reacted. I probably was too excited to look him in the face. What will he have thought when listening to my exposition? I don't know; I have never asked him how he experienced our conversation. For those that are not aware of Game Theory, let me tell

⁴ Eric van Damme, "History-dependent equilibrium points in dynamic games", Memorandum COSOR, Eindhoven University of Technology, 1980. The paper was later published in O. Moeschlin and D. Pallaschke (eds.), *Game Theory and Mathematical Economics*, North Holland Publishing Company, Amsterdam, 98 (1981) 27-38.

that, for years already, at least since 1965, Reinhard had argued exactly the opposite from what I was telling him in 1980. For sure, he knew that there can be Nash equilibria that are Pareto superior to those found by dynamic programming; in fact, that probably was (almost) common knowledge in the Game Theory community. But Reinhard had gone further; he had argued that those equilibria, including the Pareto superior ones, did not make sense. He had argued, and had convincingly shown, that the aim of non-cooperative Game Theory is not to find the best Nash equilibrium, but rather to find those Nash equilibria that are really stable when the game is played in a non-cooperative setting. Reinhard had clarified that a game being non-cooperative means that, in addition to it not being possible for the players to make binding contracts, it also means that no player can make binding self-commitments. Already in 1965 he had shown that this implies that only the Nash equilibria that are found by dynamic programming (which he called subgame perfect equilibria, SPE) can be self-enforcing and hence stable. In fact, in his 1975 paper in the *International Journal of Game Theory*, he had gone further and had argued (again convincingly) that not even all SPE could be considered self-enforcing, hence, that it was necessary to restrict the set of equilibria even more.

It would have been very easy for Reinhard to say nothing after our conversation and think “What an idiot” and leave it like that. But this is not what he did. I recall that the morning after our conversation he, gently, tried to convince me that my point of view was based on incomplete understanding. He urged me to adopt a deeper understanding. He set me thinking, and when I left Oberwolfach, he gave me a couple of papers with the suggestion that I would benefit from reading them. In particular, he gave me his 1975 IJGT paper, as well as Roger Myerson’s 1978 IJGT paper on proper equilibria, and his own paper on the chain store paradox from 1978. I have benefited greatly from reading these papers, especially the former two. (The chain store paper is different; I feel that I am not yet done with that one.)⁵ After coming back from Oberwolfach, it was clear to me what I should do for my PhD: trying to understand the papers that Selten had given to me, to investigate the relations between the proposed concepts, and to study the consequences of the ideas embedded in them. Hence, Reinhard showed the way, and once that had been done, it was simply a matter of following it. I completed my PhD in 1983

⁵ See my paper “Game Theory: The Next Stage”, in L.A. Gérard-Varet, A.P. Kirman and M. Ruggiero (eds.), *Economics beyond the Millennium*, Oxford University Press, 1999, pp. 184-214.

and, thanks to Reinhard's contacts it was published by Springer Verlag in that same year.⁶ Following the way that Reinhard indicated has suited me very well.

1.3 Reinhard's Influence

I was also very fortunate that Reinhard agreed to be the second supervisor for my PhD Thesis. During the period 1980-1982, I regularly drove from Eindhoven to the home of the Seltens in Rheda Wiedenbrück to discuss my work. When I called to make an appointment, usually in the evening some weeks before, most often Reinhard was not at home. He always seemed to be in the office, arriving home only after 10 pm. At the time, I only had seen a regular 9-6 schedule for academics, hence, was puzzled, but later I realized the advantages of such a schedule. After the appointment had been made, I sent some material a week or so before, arrived late Friday morning and stayed until Saturday afternoon. We had no lunch, but there was always pie, a lot of delicious pie, with cream. It was then so painful to learn much later that both Elisabeth and Reinhard were diagnosed (and diagnosed late) with having diabetes.

Reinhard had always read the material, but we did not spend much time discussing it. I had sent work that was already done and close to being finished, Reinhard was more forward looking. We discussed the main ideas, with Reinhard giving feedback and suggesting new ideas relating to my material. One suggestion that he gave was to investigate games with control costs. But all of that did not take much time. Most of the time, Reinhard talked about his ideas and his interests, and I acted as a sparring partner. Our discussions were only interrupted for having coffee and cake, dinner and a night of sleep. As you can imagine, when I drove home, I was completely exhausted, but also full of ideas and strongly motivated to further investigate these.

Thanks to the interactions with Reinhard, my PhD went smoothly, and I graduated on schedule in 1983. However, his influence went much beyond the thesis. My thesis was on standard, rationalistic Game Theory, hence, it was based on the assumption of the players being fully rational. However, Reinhard's main interest was in actual human behavior. He has been quoted as saying "Game Theory is for proving theorems, not for playing games".⁷ Many of our discussions were not about rational, but about actual human behavior, as, for example, observed in the laboratory, and how to understand this better. These discussions have sparked my own

⁶ Eric van Damme, "Refinements of the Nash Equilibrium Concept". *Lecture Notes in Economics and Mathematical Systems*, 219, 1983.

⁷ Jacob Goeree and Charles Holt, "Stochastic game theory: For playing games, not just for doing theory", *Proceedings National Academy of Sciences USA*, 96 (1999), 10564–10567.

interest in experiments as well as my interest in applications of Game Theory to real life issues. I recall that many of the discussions were about human motivation, a concept that I found very difficult to grasp at the time. The long-lasting influence of the conversations with Reinhard is demonstrated by the fact that human motivation is currently one of my research topics.⁸

Reinhard has not only influenced the set of topics that I have worked on, he has also influenced the way that I work. He allowed me to see that human behavior is best understood by adopting a variety of perspectives, hence, by combining insights from various disciplines. I always enjoyed the conferences that were organized at the Zentrum für interdisziplinäre Forschung in Bielefeld, and the special research year that Reinhard organized during the academic year 1987-1988 in particular. There I could clearly see the added value of diversity, especially in exploring novel perspectives. As a consequence, I have been able to work together with, and learn from, biologists, philosophers, psychologists, sociologists and a variety of other researchers. In my inaugural lecture at Tilburg University, I thanked Reinhard and wrote “Reinhard describes himself as being addicted to thinking. He is not only addicted, he is also a dealer and his thoughts are irresistible”.⁹ I think that he even transferred to me a bit of his addiction to thinking.

Returning to the quote from Reinhard’s biographical sketch mentioned in the beginning, did I only little advice? Perhaps I needed little advice on how to improve the things I already had done, but I clearly needed a lot of guidance about what things I could best do in the future. The latter type of advice clearly is much more important, even if there can still be discussion about whether by-gones are really by-gones.¹⁰ Reinhard gave such advice freely and generously; without him and his willingness to listen and give advice, I would not be here today. I am sure that, if I had not met him, my career would have been very different and it probably would have been much less successful and less enjoyable. I got crucial advice, and I am extremely grateful for that.

Let me close this first part by mentioning that Reinhard has not only influenced me, but also Dutch economics more generally. Within the international economic landscape, the Netherlands is (or maybe I should say, was) special because of its strong separation between Economics and Econometrics, the latter also including Mathematical Economics. The

⁸ Eric van Damme, “Liefde, geluk en economische wetenschap”, *Economisch Statistische Berichten* 4742S (2016), 57-61.

⁹ Eric van Damme, *Informatie, Incentives en Economische Efficiëntie*, Tilburg University Press, 1990.

¹⁰ Eric van Damme, “Stable Equilibria and Forward Induction”, *Journal of Economic Theory*, 48 (1989) 476-496.

separation led to the economists focusing on national policy issues, while the econometricians focused on international publications, usually of a technical, methodological nature. It has been said that this led to one group knowing the problems, but not mastering the tools to solve these problems, with the toolmakers not knowing which problems to attack. When I finished my PhD, there was hardly any microeconomics in the Netherlands. Things are much different now, and Game Theory has helped to improve the situation. Reinhard Selten did not only influence me (and Dutch game theorists like Stef Tijs), but also economists such as Frans van Winden and the researchers working with him in CREED in Amsterdam. Reinhard was instrumental in setting up the lab there and had a strong influence on that group. Members of that group have spread and there are now strong experimental and behavioral economics groups in Amsterdam, Maastricht, Rotterdam and Tilburg.

2. Reinhard Selten's work: a very selective sample

Let me now turn to the second part, Reinhard Selten's work in general. Reinhard has made path-breaking contributions to many areas of research, which are also very diverse. One can easily imagine that a person who encounters the name Reinhard Selten in the literature will sometimes ask himself whether this is the same Reinhard Selten as the one that he encountered before. At first, it might seem that there are multiple Reinhard's. Indeed, in a comment on Robert Aumann's influential "What is Game Theory trying to accomplish?" Reinhard has called himself a methodological dualist.¹¹ In other words, Reinhard is not wedded to a certain approach; rather he adopts multiple perspectives to try to understand human behavior. Reinhard is driven by the desire to understand, without committing himself to any field or method.

As a consequence of this attitude, Reinhard has been active in many fields. In fact, he has made fundamental contributions to both cooperative and to non-cooperative game theory, to theory as well as to experiments, to perfect rationality and to bounded rationality, to learning and to evolution, and to the disciplines of economics, psychology, biology and linguistics. Few persons have made path-breaking contributions to so many different fields within the social sciences. In many cases Reinhard not only has contributed, but he can even be seen as one of the founders of a new approach. In any case, he has always been an independent researcher and

¹¹ Robert Aumann, "What is Game Theory Trying to Accomplish?" and Reinhard Selten "Comment on R.J. Aumann: What is Game Theory Trying to Accomplish?", in K. Arrow en S. Honkapohja (eds.), *Frontiers of Economics*, Basil Blackwell, Oxford, 1984,

an early contributor. As Reinhard once wrote: “Since I am slow, I have to try to be early”.¹² He did not only try, he also succeeded. He arrived early to many unexplored territories, leaving landmarks that proved to be very useful for researchers that came later. He pioneered several different directions. Given the large variety of the contributions, it is clear that I have to be very selective. I will pick three areas in which my own research or teaching has been strongly influenced by Reinhard’s work.

2.1. Equilibrium refinement and equilibrium selection

The first area that I would like to briefly touch upon is that of equilibrium refinement and equilibrium selection. Above, I already mentioned that Reinhard clarified the concept of a non-cooperative game. Indeed, he took the idea of a game being non-cooperative to the limit: in his view, players cannot commit, not even to a strategy: whenever a decision has to be made, there is an opportunity for reconsideration, and there is no possibility to block such reconsiderations. As a consequence, the strategic form of a game is not an adequate representation of the underlying extensive game. In his 1975 paper on “trembling hand” perfection, Reinhard also came to the conclusion that full rationality could best be viewed as a limit of near-rationality: “a satisfactory interpretation of equilibrium points in extensive games seems to require that the possibility of mistakes is not completely excluded. This can be achieved by a point of view which looks at complete rationality as a limiting case of incomplete rationality”.¹³ This proved to be a seminal idea. For example, it led to the more general idea that, for an equilibrium to be really self-enforcing, it should be stable against all perturbations within a certain class. As far as refinements are concerned, this line of research culminated in the work of Kohlberg and Mertens.¹⁴ The same idea would also contribute to the literature on equilibrium selection, on which I will focus here.

Together with John Harsanyi, Reinhard Selten, for many years, worked on a theory of equilibrium selection. In fact, several different theories were developed before the final version appeared in book-form in 1988.¹⁵ I remember the thick, blue-covered discussion papers from the IMW in Bielefeld in which the earlier versions were presented; they strongly influenced my

¹² Reinhard Selten, “In search of a better understanding of economic behavior”, pp. 115-139 in A. Heertje *The Makers of Modern Economics*, Vol. 1. Harvester Wheatsheaf, 1993.

¹³ Reinhard Selten, “Reexamination of the perfectness concept for equilibrium points in extensive games, *International Journal of Game Theory* 4 (1975), 25-55.

¹⁴ Elon Kohlberg and Jean-Francois Mertens, “On the strategic stability of equilibria”, *Econometrica* 54 (1986), 1003-1037.

¹⁵ John Harsanyi and Reinhard Selten, *A general theory of equilibrium selection in games*, MIT Press.

own thinking on a variety of game theoretic issues. The reason why John and Reinhard devoted so much time and energy to the matter is easy to understand. In his unpublished Ph.D. dissertation from 1950, John Nash provided two interpretations of his equilibrium concept: the standard, rationalistic, interpretation and a “mass-action” (evolutionary or learning) interpretation. The first one, however, is relying fully on the idea that there is a unique solution. Hence, this justification of Nash’s equilibrium concept only holds in case the game has a unique solution. However, since a game typically has multiple Nash equilibria, this justification is seriously incomplete. Accordingly, Harsanyi and Selten set themselves the task to come up with a convincing theory that selects a unique Nash equilibrium in every non-cooperative game.

	R	S
R	$(1, 1)$	$(0, x)$
S	$(x, 0)$	(x, x)

Table 1: A Stag Hunt Game

The Stag Hunt Game from Table 1 is a game with multiple Nash equilibria. If $0 < x < 1$, this 2-person game has one (pure) Nash equilibrium in which each player plays R , and another equilibrium in which each player plays S . Each equilibrium has something that speaks in its favor: S is a safe strategy, which yields $x > 0$ no matter what the other player does; R is more risky (it could result in a payoff of 0), but it also might yield the best possible payoff of 1. One reason why it took John and Reinhard so long to complete their project is that they could not agree on how rational players would play such a Stag Hunt Game. John argued in favor of the Pareto dominant equilibrium (R, R) , while Reinhard argued in favor of (S, S) for those cases in which this latter equilibrium is risk dominant. John and Reinhard proposed and investigated several versions of risk dominance. One version relies on the principle of insufficient reason. If both equilibria are equally acceptable, then *a priori* one expects the opponent to play each of his strategies, R and S , with 50% probability. But in that case, a player’s best response is to choose R if $x < 0.5$ and to choose S if $x > 0.5$. Hence, risk dominance leads to the conclusion that one should play S if $0.5 < x < 1$, and risk dominance and payoff dominance are in conflict in that case.

In joint work with Hans Carlsson, we relied on the stability idea mentioned above to provide a fully non-cooperative justification for the selection of the risk-dominant equilibrium.¹⁶ We

¹⁶ Hans Carlsson and Eric van Damme, “Global games and equilibrium selection”, *Econometrica* 61 (1993), 989-1018.

assume that x can take many values and that each player i makes an independent noisy observation x_i on x . We also assume that both $x < 0$ and $x > 1$ occur with positive probability. In the former case, only (R, R) is a Nash equilibrium; in the latter case, the unique equilibrium is (S, S) . It is now quite intuitive that, when there is noise and x_i is close to 0 (resp. is close to 1), player i will choose R (resp. S). We show that, in the limit, when the noise vanishes, the “global game” has just one Nash equilibrium and that each player i will choose R when $x_i < 0.5$ and S when $x_i > 0.5$. Hence, in the limit, the global game selects the risk dominant equilibrium. To summarize: Selten’s point of view that “complete rationality is best viewed as a limiting case of incomplete rationality” not only helps to refine equilibria, it can also select a unique equilibrium and, thereby, it rescues Nash’s rationalistic justification of this fundamental solution concept for non-cooperative games.

2.2. Industrial Organization

The second area that I would like to briefly touch upon is that of game theoretic applications in the field of Industrial Organization, the area devoted to the study of the functioning of markets. As Jean Tirole explains on p.3 of his classical textbook from 1988, this field was revolutionized in the 1970s after non-cooperative game theory imposed itself as “the standard tool for the analysis of strategic conflict, thus bringing a unified methodology to the field. Furthermore, it made serious progress in two crucial areas: dynamics and asymmetric information. The stage was thus set for a reappraisal of the many informal stories that were floating around.”¹⁷ The work of Reinhard Selten and that of John Harsanyi was very important in making this revolution possible. Selten’s concept of subgame perfect equilibrium (SPE) enabled the analysis of dynamic markets, while Harsanyi’s idea of Bayesian equilibrium allowed work to proceed in the area of asymmetric information.

Next to his methodological contributions, Reinhard also made several substantive contributions to this area. In his “Are cartel laws bad for business?”¹⁸ he argued that cartel laws may be good for business, since without cartel laws there may be excessive entry: more competition may lead to less entry and, hence, larger average joint profits. In the paper, “A simple model of imperfect competition where 4 are few and 6 are many”¹⁹ Reinhard showed that collusion will

¹⁷ Jean Tirole, *The Theory of Industrial Organization*, MIT Press, 1988.

¹⁸ Reinhard Selten, “Are cartel laws bad for business?”, pp. 86-117 in H. Hauptmann, W. Krelle and K. C. Mosler (eds.) *Operations Research and Economic Theory*, Springer-Verlag, 1984.

¹⁹ Reinhard Selten, “A simple model of imperfect competition where 4 are few and 6 are many”, *International Journal of Game Theory* 2 (1973) 141-201.

typically be stable only in markets with few participants as in larger markets it usually is more advantageous to be a “maverick” outside of the cartel. These insights, which have been “replicated” in experimental research,²⁰ are clearly important for competition policy. Here, I will limit myself to discuss Reinhard’s paper on leniency schemes.

Business cartels are problematic since they lower welfare and consumer surplus in the short-run and they limit innovation, which negatively effects welfare in the long-run. Hence, it is important that cartels are detected, or even better, deterred. Clearly, since detection is associated with punishment, these two aspects are related: the larger the risk of detection, the less attractive it is to form a cartel in the first place. Cartel policy is important, but the life of antitrust agencies is made difficult by the fact that cartels are not so easy detect (and, hence, may not be easy to deter). Important tools of the authorities are leniency policies and whistle-blowing programs. Under the former, a cartel participant can get a fine reduction, if it voluntarily provides information on the cartel to the authorities; if a firm is the first to do so and if the authority has not yet started its investigation, the firm even can get complete amnesty. Under whistle-blowing programs, employees that report on cartel activity can get bonuses. Both tools can be quite effective. In the European Union, in almost all of the cartels that are nowadays detected (91 %), leniency is involved in one way or the other.²¹ Hence, it is important that leniency programs are well-structured.

In joint work with Jose Apestiguia and Martin Dufwenberg, Reinhard has enhanced our understanding on the question of how to design leniency schemes.²² Although not the first paper on this issue, it is an early one in this area, and it probably is the first one that contains both a theoretical and an experimental part. Other papers either are purely empirical, or “just” theory, or exclusively experimental. The Selten paper stands out since it, first, fully specifies a set of stylized market games that correspond to different ways in which anti-trust legislation may be structured in practice. One of these games is close to the actual leniency scheme as it exists in the EU, while a “bonus” treatment actually rewards a firm for being in a cartel and then reporting it. By analyzing these games theoretically, predictions are derived that are contrasted to the views held by competition authorities. One prediction is that “bonus” may

²⁰ For example, see Miguel Fonseca and Hans-Theo Normann, “Explicit vs. tacit collusion—The impact of communication in oligopoly experiments”, *European Economic Review* 56 (2012), 1759-1772.

²¹ Wouter Wils, “The use of leniency in EU cartel enforcement: An assessment after 20 years”, *Concurrences Review* 1-2017 (February 2017), Art. N° 83458.

²² Jose Apesteguia, Martin Dufwenberg and Reinhard Selten, “Blowing the whistle”, *Economic Theory*, 31 (2007), 143-166.

outperform the leniency schema that is currently used. In the experiment, however, it turns out that the theory predicts well, except for the “bonus” treatment. In that treatment, the authors observe “bounded rationality”: the players rush into the cartel in the hope of being able to get a bonus. All in all, it is the combination of theory and experiment that enhances our understanding.

In closing this subsection, let me mention that I also had the pleasure to work with Reinhard and some of his assistants in one of the spectrum auctions that were organized in Europe around the turn of the century. The Dutch DCS-1800 auction involved a design which made for complex bidding strategies. What contributed to the complexity was that we were advising a bidder that was itself a consortium of Deutsche Telekom, France Telecom and two Dutch banks. We conducted experiments to prepare the bidder for the actual auction. These experiments gave the bidder a good idea of the intricacies of the auction and allowed the members of the consortium to agree on a bidding strategy. The bidder won on a license and avoided mistakes that some other bidders made; it was quite pleased with the outcome.²³

2.3. Bounded Rationality

The final area that I would like to briefly touch upon is that of bounded rationality, that is, the limited rationality that is exhibited by actual human behavior. Human rationality differs considerably from the full rationality as modeled by Bayesian maximization of subjective utility. In some situations, to understand human behavior, the standard Bayesian model is reasonably helpful as a first appropriation, but in other contexts it does not seem helpful at all. We all know our limited cognitive abilities. We also know that thinking takes energy and can be painful. We can prefer not to think; many decisions are automatic. In making decisions, emotions play an important role. Furthermore, decisions more often emerge than that they are made. We can deviate from well-reasoned decisions, because they don’t feel good. As Reinhard has written “Behavior cannot be invented in the armchair. It has to be observed. Therefore, the development of theories of bounded rationality needs an empirical basis.”²⁴

Anyone who has done experiments will probably have been surprised by the limited rationality that is sometimes displayed by humans, even in tasks that require little cognitive effort. Even

²³ Eric van Damme, “The Dutch DCS-1800 Auction”, pp. 53-73 in Fioravante Patrone, Ignacio García-Jurado, Stef Tijs (eds.), *Game practise: Contributions from applied game theory*, Kluwer Academic Publishers, 1999.

²⁴ Reinhard Selten, “Features of experimentally observed bounded rationality”, *European Economic Review* 42 (1998) 413-436.

in relatively simple problems, such as playing children's games like Nim, people may not be able to find an optimal strategy. Furthermore, learning processes can be terribly slow and may not converge to the optimal action even when there are many opportunities for learning. Real learning may require reflection, but people simply may not take the time or make the effort to reflect. All of this has been demonstrated clearly in the work of Reinhard. His work on people not learning to avoid the winners' curse provides a nice illustration.²⁵

Imagine a bidder B who bids on an object of which he does not know the value precisely. In contrast, the seller, S , does know how much he values the object. Assume that B knows that the value of the seller, v_S , is drawn from the uniform distribution on $[0,100]$ and that his value, v_B , is always 50% more than the seller's value, hence, $v_B = 1.5v_S$. The buyer knows that S will accept his bid b only if $v_S \leq b$. What bid b should the bidder make? Somebody who has never seen this problem is likely to find this a complicated question. It is natural that one does not know the answer, however, one might still hope that, if the situation is repeated, B will over time learn to bid correctly. Reinhard has shown that this hope is idle. Even after 100 attempts, and with real money involved, student subjects are far removed from the correct answer, which is not to bid at all, or to bid zero. Our intuition tells us that this is a valuable object, hence, that we should aim to get it. However, that intuition is wrong. If we don't reflect, but adjust our bids myopically, then we will not see that it is wrong and will continue to fall prey to the winner's curse.

In his work, Reinhard has uncovered already many regularities in human behavior, but we clearly are far removed from having a unified picture. Reinhard himself has written: "The problem of bounded rationality has occupied my mind much more than one would think if one looks at the moderate success of my efforts in this direction".²⁶ Perhaps that is true, but there is no reason to be pessimistic. Reinhard has left us with the clear task to make bounded-rationality models intellectually competitive. As the task is well-described, there is every reason to assume that it will be picked up.²⁷

²⁵ Reinhard Selten, Klaus Abbink and Ricarda Cox, "Learning direction theory and the winner's curse", *Experimental Economics* 8 (2005), 5-20.

²⁶ Reinhard Selten. "In search of a better understanding of economic behavior", pp. 115-139 in A. Heertje *The makers of modern economics*, Harvester Wheatsheaf, 1993.

²⁷ Ronald Harstad and Reinhard Selten, "Bounded-rationality models: tasks to become intellectually competitive", *Journal of Economic Literature* 51 (2013) 496-511.

3. Conclusion

Ladies and gentlemen,

I am bringing my speech to a close. I have spoken about Reinhard's influence on me, on how he has determined my research agenda and on how his ideas continue to be relevant for my current teaching and my current research. I have also informed you that Reinhard's way of working has inspired me to try to work in a similar way.

I have also spoken about how Reinhard can be viewed as a founder of several fields in economics. It is remarkable how flourishing some of these fields currently are. In this connection, I am thinking of the current German school of experimental and behavioral economics in particular. In the 1980s or 1990s, when I went to international meetings of economists, Germany was always underrepresented. Nowadays, if I go to meetings of the European Economic Association, for example, I meet many young German economists, of which many are working in Reinhard's footsteps. Some people are even arguing, perhaps jokingly, that a bit more diversity would be good. Who could have imagined this back in the 1950s when Reinhard was doing his first experiments here in Frankfurt?

I have also briefly spoken about the future and the agenda on bounded rationality that still needs to be explored. I already mentioned that Reinhard himself was disappointed by what he had achieved. He has written: "It is now clear to me that it will take many decades of painful experimental research until an empirically defensible general theory of bounded rationality emerges. At the beginning of my career, I was more optimistic in this respect".²⁸ However, at the end of the same article, he concludes in a much more optimistic tone: "The interest in bounded rationality is increasing. We can hope for the emergence of a body of experimentally based descriptive theory. It is possible that the rationalistic approach has already reached its peak and that attention is going to shift to more realistic explanations of behavior. (...) I would not be surprised if, after some time, empirically based theorizing about boundedly rational behavior becomes a substantial part of mainstream economics".

Ladies and gentlemen. We can all be glad that we were fortunate to know Reinhard and to benefit from his knowledge and insights. His ideas will live on. I am sure that even some of his papers which have not been picked up thus far will become influential in the future.

²⁸ Reinhard Selten, "In search of a better understanding of economic behavior", pp. 115-139 in A. Heertje *The makers of modern economics*, Harvester Wheatsheaf, 1993.