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MD Interview

INTERVIEW WITH JEAN-FRANÇOIS MERTENS (1946–2012)

Interviewed by

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Every game theorist knows of Mertens and Zamir (1985)'s universal beliefs space, which gives deep foundations to Harsanyi's model of Bayesian games, and Kohlberg and Mertens (1986)'s strategic stability, which is the first stone of a complete, axiomatic theory of selection among Nash equilibria. Some French mathematicians refer to the "Mertens–Zamir operator" when using techniques that Mertens and Zamir (1971) introduced to solve a class of repeated games with incomplete information. Readers of *Macroeconomic Dynamics* may instead have seen Mertens and Rubinchik's 2012 article "Intergenerational Equity and the Discount Rate for Policy Analysis."

The previous examples give just a slight idea of the scope of Jean-François Mertens's contributions, which also deal with general equilibrium, stochastic games, nonatomic cooperative games, and the strategic foundations of microeconomic theory. In his 2005 MD interview, Robert Aumann says, "A [. . .] person at CORE who has had a tremendous influence on game theory [. . .] is Jean-François Mertens. Mertens has done some of the deepest work in the discipline, some of it in collaboration with Israelis like my students Kohlberg, Neyman, and Zamir; he established a Belgian school of mathematical game theory that is marked by its beauty, depth, and sophistication." The short interview that follows will definitely not account for the variety and the relevance of Jean-François's research achievements, but is typical of the way in which he talked about his work.

Jean-François asked me to interview him for MD during the spring of 2010. We discussed by e-mail the topics that would be covered and on July 6, 2010, I came to Louvain-la-Neuve with a tape recorder. After lunch, Jean-François suggested that we have coffee on a terrace near the golf course and there, he patiently answered my questions, sometimes in French, sometimes in English, for about

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FIGURE 1. Jean-François Mertens.

two hours. We planned to go on for at least another round but kept postponing the project . . . . When I saw Jean-François for the last time, in February 2012, I gave him the transcript of the July 2010 interview, but he hardly commented on it. He rather told me about an ongoing research article, "A Random Partitions Approach to the Value," to be presented (by Abraham Neyman) as a "von Neumann lecture" at the World Congress of Game Theory in Istanbul in July 2012. At the same time, he was also completing, with Anna Rubinchik, the revision of a companion paper to the MD article referred to previously ("Equilibria in an Overlapping Generations Model with Transfer Policies and Exogenous Growth," forthcoming in *Economic Theory*).

Even if Jean-François did not proofread the transcript that follows[1], I cannot keep this material for myself. I am confident that those who have known Jean-François will take the interview, even incomplete, as an opportunity to remember his enthusiasm and his patience when he was talking about research. He would often start by identifying holes in obvious or well-known solutions to basic problems, and after a few audacious but illuminating shortcuts, would describe the most surprising achievements in everyday words. I hope that the interview will give an idea of Jean-François's approach to those who did not know him.

Quite naturally, because MD was the planned outlet of the interview, we started by talking about the paper on the discount rate for policy analysis, which was already mentioned in the preceding. Jean-François made a number of informal comments, which usefully complement the MD article. He also explained how this paper led him and his coauthor to undertake a thorough analysis of overlapping

generations economies in continuous time. This made a perfect transition to Jean-François's views on general equilibrium theory, his own work in this area, and his early career.

The next step would be Jean-François's meeting with Bob Aumann, who introduced him to game theory. Jean-François pursued Aumann and Maschler's seminal work on infinitely repeated games with incomplete information, mostly with Shmuel Zamir. He went on with the existence of a value in stochastic games, another model of infinitely long games, which was introduced by Shapley in 1953. This research was undertaken with Abraham Neyman at the Institute of Advanced Studies in Jerusalem in 1980. Soon after, Mertens and Zamir started to review and complete all available results on repeated games in order to prepare a reference book on this topic. The material kept growing. Sylvain Sorin joined the team in the nineties and a draft appeared as a 1994 CORE discussion paper. However, in 2010, the book was still unpublished . . . the interview ends up with Jean-François's feelings about the project.[2]

As shown by the list of publications at the end, many important contributions of Jean-François Mertens to game theory and microeconomics are not even mentioned in the interview. During his stay at the Institute of Advanced Studies in Jerusalem in 1980, Jean-François not only worked with Abraham Neyman on stochastic games, but also had his first discussions with Elon Kohlberg on refinements of Nash equilibria. These would be followed by many others, at CORE and Harvard, until the famous *Econometrica* paper appeared in 1986. For the next 15 years or so, Jean-François further developed the theory of strategic stability, by himself and with his students.

During the same period, Jean-François was also making progress on a completely different problem, the extension of the Shapley value to nonatomic cooperative games. Aumann and Shapley (1974) had made the first steps by proposing a value for smooth games. Jean-François proposed a complete answer to the problem in the eighties and, as already pointed out above, kept working on related topics until the very end.

Even without entering into details, a description of Jean-François's more recent contributions would be beyond the scope of this short introduction. As the interview makes clear, Jean-François became more and more interested in the foundations of microeconomic theory. A typical example is his "limit price mechanism," which can be loosely described as a double auction with several goods or as an extension of Shapley and Shubik's strategic market games. Another example is "relative utilitarianism," which, as Jean-François explains in the interview, plays a crucial role in the determination of an appropriate social discount rate for the evaluation of long-term economic policies. Let us listen to him.

Keywords: Intergenerational Equity, Repeated Games, Stochastic Games, Strategic Stability, Relative Utilitarianism

FF: I imagine that many of our colleagues, who mostly know you as a game theorist, do not expect that you be interviewed as an "economist," especially for a journal devoted to macroeconomic dynamics! They likely do not know yet your

most recent work, with Anna Rubinchik, on the appropriate discount rate for cost benefit analysis and the equilibria of overlapping generations economies with exogenous growth, which perfectly fits the standard topics of MD. Could you tell us about this work, in a few words?

JFM: These are not really papers in macroeconomics, they're just papers which try to see concretely the link between what we do in theory and the reference model in macroeconomics, but we use a stylized reference model in which all typical macroeconomic aspects, for instance the ones that can generate cycles, have been eliminated. It's the basic model of growth . . . .

FF: Nonetheless, the starting point of the article "Intergenerational Equity and the Discount Rate for Policy Analysis" is a very concrete question, right?

JFM: Yes, but I was maybe too much influenced by John Harsanyi, whom I met during my very first years at Berkeley and whom I liked very much. For him, there was no difference between social choice, game theory, economics, ... For him, it was the theory of the behavior of rational agents in a society, considered from a bit different points of view, but with the same continuous interest . . . .

FF: This reminds me that, in the above-mentioned article, you use another one, "Relative Utilitarianism," written with Amrita Dhillon, which is exactly in the spirit of Harsanyi's work . . . but let us stick to the discount rate . . . .

JFM: Clearly, since a few years, I am trying to attack problems from their very root . . . here, it all started with an incidental reading of a circular of the U.S. Office of Management and Budget, in which it appeared that there was no basis for the proposed computation of discount rates . . . the circular is a mandate to all executive U.S. agencies: before any proposal of change of regulation, a cost–benefit analysis must be performed and this must be done at a discount rate which is essentially justified as the interest rate. Two rates are proposed but conceptually both amount to the interest rate. Then the circular says: for the projects which have a potentially important long-term impact, a third analysis must be performed at a lower discount rate. There is no precise figure, no justification but a reference to Ramsey's argument,[3] whereby government should treat all generations equally . . . .

FF: But there, there is no intergenerational model, ...

JFM: That's right. Effectively, in Ramsey's model, every agent lives for a single period, it's an extremely simple model . . . the circular refers to a "smaller rate" but apparently, theory could not say anything . . . .

FF: Aren't there many articles dealing with these questions?

JFM: Clearly, by using the traditional model, one immediately derives a formula: the discount rate must be . . . let me use the notation I'm used to . . . $\beta + \rho\gamma$, where β is the rate to discount utilities in the social welfare function, γ is the per capita growth rate of the economy, and ρ is a parameter of risk aversion, or what is called the income elasticity of the marginal utility of income for the considered individuals . . . .

FF: And this is fully standard, isn't it?

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JFM: Completely standard, these formulas are in the literature, probably since Ramsey! In any case, since I have looked at these questions . . . the point is that it is completely impossible to estimate the parameter ρ; the estimations that are proposed in the literature, when ρ is considered as an individual parameter of risk aversion, vary from 1 to 100.

FF: Can one find such differences?

JFM: There is a paper by Jacques Drèze in *Geneva Papers of Risk and Insurance* where he finds the estimates from the behavior of agents faced with insurance problems or portfolios offered by insurance companies. If I remember well, his estimations vary from 5 to 20; there is a much more recent paper by Einav and Cohen in A.E.R. in which they only consider absolute risk aversion, in which there is thus a problem of translation in terms of relative risk aversion. There, one has the impression that the distribution in the population can very well vary from 1 to 100 . . . . In other words, the previous formula does not make any sense: the discount rate cannot depend on a parameter ρ that is typically an individual characteristic . . . . It is because all these standard models are derived for a single agent and in a model with a single agent, there are not many economic problems, it is just an optimization problem.

I remember well the evening when I recovered the classical formula; it's a computation of two or three lines. I was very astonished and I understood why no precise figure is usually proposed. It was not so late in the evening, so I decided to spend a few more minutes to have a look of what relative utilitarianism would give there . . . .

FF: Relative utilitarianism is this article that we mentioned already, with Amrita Dhillon?

JFM: . . . and looking precisely at it, the computation gave exactly what was required, namely, the parameter ρ had disappeared, it had become equal to 1!

FF: You are going too fast . . . I thought that you had a complex reference model, with growth, overlapping generations, etc.?

JFM: No, no, I am just talking about the ultra-simple case where, as in Ramsey, every agent lives for a single period, and growth just comes from the fact that there are more and more apple trees on the earth!

FF: OK, every agent lives for one period and the population size does not change . . . .

JFM: The population can increase or not, it is a stupid model in which there is no exchange, everything is exogenous, so it is again a two-line computation as in the original case . . . .

FF: Would you detail this computation? Can you tell us more about relative utilitarianism in this simple context?

JFM: In "relative utilitarianism," the basic conclusion of the theorem is that there is only one social welfare function, axiomatized in this way, which is of the form: take individual von Neumann–Morgenstern utility functions and normalize them between zero and one on the feasible set . . . conceptually, the feasible set here has to be interpreted as the set of all alternatives that are both feasible and

just, I mean all the concepts of equity have to be incorporated in that aspect of the description of the problem ... as the set of just alternatives. So, in a growth model, where everything grows exogenously, the most natural assumption is that policy does not influence the growth rate, so at most it influences how many percentage points you are above or below the normal growth path ... and so you normalize all individual utility functions between 0 and 1 of such an interval and that's it! So that leads to trivial computation and it shows that the choice of this interval is completely irrelevant, it does not matter for the final result, and it gives you the neat formula that the discount rate should be the same as the classical one, but you set the parameter ρ to 1 and what's amusing is that in practical applications if you look at Stern's report, for instance, it is exactly what he does: he says we have to use that formula $\beta + \rho\gamma$ but to get sensible results you must set ρ to 1.

FF: And thus, this leads to a satisfactory theoretical justification ....

JFM: That's right: in some sense, it's a theoretical justification of what people do in practice. Arrow had another interpretation: he did not like what Stern did (this is from oral conversations); he said ρ is meaningful, but Arrow still has in mind single-agent economies, with a single ρ. If ρ is meaningful, if there is a real agent behind, let us correct β, namely the discount rate that is applied to utilities in the social welfare function, welfare is always a discounted sum of utilities, β is an arbitrary parameter, so we can as well correct β rather than ρ so as to get the same final result.

FF: But this way of proceeding is arbitrary ....

JFM: Completely arbitrary! And in some sense doing that would be exactly what relative utilitarianism is doing.

FF: Changing β, you mean?

JFM: That's right, because in relative utilitarianism we normalize the successive generations' utilities with an exponential normalization factor, which amounts exactly to this change of β. So, in some sense, what Stern has in mind is a meaningful β, namely a parameter β to mean that generations should be treated equally, so β should be zero; if β is meaningful the only way to get a reasonable approximation to relative utilitarianism result is to set the parameter ρ to 1. Probably what Arrow had in mind is that anyway, a social welfare function is a social welfare function, who cares what are the parameters in it? He does not want to tie β to zero, so let us take an arbitrary β so as to get the results that we want and that way, it is true it is exactly what utilitarianism does. So basically that article was a surprise showing that a methodology or an approach that was designed for purely static problems, in the most abstract and traditional social choice framework,[4] in fact led to very clean results without any impact of the individual preferences over risk on the final result .... On the contrary, the final result is purely technological ... it's that [the social discount rate] $\beta + \gamma$ does not depend on preferences at all ... it was a real surprise ....

FF: I believe that it will be even more a surprise for macroeconomists? It seems to me that you are not using the usual macroeconomic models ....

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JFM: I think methodologically the biggest difference is that they are used to view models as dynamic systems, basically, as a system of differential equations. But for an equilibrium system, namely, in which you have more than one agent, there is no way to do that in continuous time. So you really need to use a system of operator equations between functions of time.

FF: We talked a lot of your first article with Anna Rubinchik, which gave rise to several others. The summary of one of these articles starts with "general equilibrium meets macro." One may be tempted to say that you are "coming back" to G.E., given your contributions to this theory in the seventies. What were the questions that you were trying to solve at the time?

JFM: I indeed started with G.E. theory, a bit by accident, because Gérard Debreu was visiting CORE during the year of my master's thesis in economics at the KUL (Katholieke Universiteit Leuven), and he was my supervisor. My first interests in economics were related to G.E. I gave up some time after, likely because I had the impression that the important problems were solved and that everything would go on automatically. I was quite shocked when I looked at the question we discussed before. We just wanted to use classical results in G.E. theory—regularity, stability, etc.—but the available results seemed useless for the simplest reference model of macroeconomics. Just to obtain the equilibrium equations of the model, we had to write a paper of about 50 pages, a quite tedious paper . . . .

FF: What was the main difficulty? The dynamic aspects?

JFM: The main difficulties come from the fact that the model is in continuous time. People prefer to work in discrete time,[5] it's only in my old undergraduate textbooks of macroeconomics that models were still written freely in continuous time, in a very ad hoc way, of course!

FF: You mean that G.E. models, as they were conceived in the seventies, did not make use of continuous time?

JFM: No, this was work that should have been done right after, because there were no new important conceptual problems . . . in the paper that I was describing before, when we decided to deal with continuous time, we thought that it would not make such a great difference, except for some more technical work . . . we clearly underestimated the amount of additional work that was required. At the same time, our choice to work in continuous time was firm, because we wanted to avoid the pathologies of discretization.

FF: Indeterminacy, for instance?

JFM: Exactly . . . for instance, when we started, we were not familiar with an article of Polemarchakis and De Michelis, which shows that in a quite similar model (but in which there is no production, no technological progress) indeterminacy decreases significantly when passing to the limit of discrete time to continuous time. In the same article, they show that indeterminacy vanishes if, instead of having time starting at zero, one has it starting at minus infinity . . . . In the papers with Anna Rubinchik, we made these two choices from the start in order to avoid possible problems. It is well known that discretization leads to problems, including conceptual ones . . . I believe that there are good fundamental

and conceptual reasons to adopt continuous time in a model that refers to reality! A model cannot be a description of the available data. Once the model is settled, it is the job of the statistician or of the econometrician to estimate it, but that's another problem. Writing discrete-time models in economics looks to me as stupid as would be to describe the planets move in discrete time, just because telescopes take pictures at discrete intervals of time . . . .

FF: Does that mean that G.E. theory must be rebuilt?

JFM: There is indeed much work to do. As far as I am concerned, I just want to finish with the current, rather limited, point, for which we had to construct quite serious tools . . . .

FF: To sum up, what are the most important achievements of your research with Anna?

JFM: There is the paper which says that, in order to compute a discount rate that is equitable from an intergenerational point of view, relative utilitarianism gives the correct result, while traditional methods do not give it, unless they are adjusted in an ad hoc way, as in Stern's report. This paper considers the basic growth model, with overlapping generations, but conceived in a G.E. perspective, namely, with n types of agents, as many consumption goods and types of capital as you like, etc. We show that if the social welfare function is differentiable, the derivative of the social welfare necessarily takes the form of a discounted evaluation of future policies, and the discount rate corresponds to $\rho = 1$. The model is very general in the sense that the primitives are abstract policies, and one differentiates with respect to policies. Of course this theorem makes the implicit assumption that there is such a social welfare function, a result of the policies, and that equilibrium can be written uniquely as a function of policies. Hence there must not be any indeterminacy, and furthermore the resulting function must be differentiable. And thus the next papers really strive to prove that in the simplest case of this model, the assumptions are not empty. Up to now, everybody thought that overlapping generations were synonymous with indeterminacy, that nothing would be possible . . . we needed to write a first paper of I do not remember how many pages, just to obtain the equilibrium equations, starting with a purely conceptual definition of equilibrium, without imposing hidden restrictions to the equilibrium on the grant that this or that function had to belong to such and such space, etc. I must say that a customary way of describing G.E. in a priori given spaces always looked strange to me. We really took things from the primitives: consumption flows, consumption plans, which are just measurable functions of time, with values from zero to infinity, and similarly for prices, etc. Hence the space where the equilibrium belongs is a consequence of the equilibrium conditions.

FF: No restriction a priori thus . . . .

JFM: Right, we do not impose a priori restrictions and we obtain the equilibrium equations just from the conceptual definition. This required a great amount of work, because even in this simple case, nobody had looked at it . . . for somebody who was coming back to G.E. after many years, this was a real shock! Once we had

the equations, we still had to prove that there was no indeterminacy, in the sense that is required for comparative statics . . . .

FF: Namely in a local sense?

JFM: In a local sense, and even in a local sense around "balanced growth equilibria." There are likely other equilibria in that kind of models and if there are others, there is necessarily a form of indeterminacy there, because then any shift in time of a given equilibrium generates another one. This, if we wanted to extend the result to all equilibria, we would have to work more; it would be a completely different problem. Our goal was just to show that the assumptions of the theorem of the first paper were not contradictory, not empty . . . . In some sense, we went much further because we obtained a very fine description of all the derivatives of the equilibrium with respect to all transfer policies. As a consequence, we obtain an explicit formula for the derivative of welfare. To compute the derivatives you would expect to find the inverse of the equilibrium system, which is a system of equations from minus infinity to plus infinity, which does not seem feasible . . . nothing of the kind happens here, and we obtain a fully explicit formula of the derivatives of the equilibrium variables with respect to policies.

FF: Where does the miracle come from?

JFM: From the magician . . . (laughs) from the magician Fourier and the magician Laplace!

FF: It is really refreshing to hear that G.E. can still open such doors, but isn't it hopeless at the same time if we think about the stereotype of, say, the nineties, according to which there were no more exciting questions in G.E.? Did people ask bad questions?

JFM: I always liked to go back and forth between quite exploring questions and looking where they came from and I think that is an aspect that may have lacked in G.E. theory. Maybe part of the explanation why I stopped to have interest in it at some time was a lack of coming back to what are the real problems, how can this be helpful, trying to apply it to real problems . . . . For a theory to stay vivid and useful, it has to relate to this, it cannot continue on its own.

FF: But this seems to apply to pure theory. Didn't macroeconomists, maybe Prescott, I am not sure, look for G.E. foundations with concrete motivations?

JFM: I'm sure those people were doing clean models and were thinking clearly about their models but almost all traditional thinking in macro is really in a single-agent economy, it is maximization problems, you do not face any real G.E. problems and probably the reason for that is the tools were lacking to do it in a more realistic framework . . . .

FF: The mathematical tools, you mean?

JFM: Or even the G.E. tools . . . the profession has become much more a profession of isolated specialties . . . people in macro look at what other people in macro do, they try to improve on that and it is so in every field and it is becoming completely splintered.

FF: You started to talk about your early work, you recalled your meeting with Gérard Debreu . . . what were you working on at that time?

JFM: There was a bunch of theorems to be proved, it was very clear what the theorems were, so it looked like an easy exercise, it was fun to see how the tools worked well, I was 21 or 22, you discover that you can do things . . . .

FF: I understand that you were then behaving as a mathematician, without asking the kind of questions that you investigate today . . . maybe we should start with the beginning, and talk about your education . . . you mentioned a master's thesis with Debreu, in mathematical economics, but I think that, at the same time, you also had contributions in stochastic processes . . . .

JFM: My thesis was in maths, that's right, and was on a rather applied field of maths. There was a vivid literature in applied statistics, it was called "optimal stopping problems," most in discrete time, and the thesis was on how to do this properly in continuous time (we come back to continuous time . . .) for the simplest problem: you follow a process and you want to get into some closed set, you take it closed so as to make optimality and existence easier. Think just of uniform motion to the right on the real line, let the closed set be some interval, clearly your value function is left continuous at the right endpoint of that interval: it jumps down from 1 to 0 . . . so viewed as a supermartingale, it is left continuous . . . it's not a right continuous martingale, so all the existing theory of martingales broke down if you wanted to study such optimal stopping problems and you had to recreate a new one, where basically you took an appropriate measurability condition on martingales, that was the basic condition, plus the fact that the stopping theorem was true for any pair of stopping times instead of just any pair of discrete, constant times . . . so you took the definition of a supermartingale, which is usually stated between two constant times, you said let's assume it applies to any two stopping times. That type of martingale could show then that all the usual theorems still applied, you could still talk about properties of sample path, etc., thanks to this special measurability condition. So the whole thing became the right tool but it is irrelevant to the original problem: I shifted this problem that came from applied statistical problems to some rather basic contribution in probability and potential theory, in a rather abstract part of mathematics, and it was very cleanly rewritten in the book of Dellacherie and Meyer. A large part of the thesis is redigested in the book and there is an appendix for the most specific parts . . . it was fun . . . again, you take something from an applied literature and it leads to something completely different!

FF: Were there followers?

JFM: I think they still use it but I have no idea . . . .

FF: You completed your thesis in the beginning of the seventies, right?

JFM: That was 1968, 69 . . . .

FF: And Dellacherie and Meyer introduced it in their book immediately after?

JFM: Yes, maybe three, four years later . . . .

FF: It must have been extremely exciting for the young mathematician that you were at the time . . . . This should have led you to go on in the same area . . . .

JFM: No (laughs). I continued a bit with that, but the next step was to apply this to a basic formulation of optimal stopping, where you have a reward function

that describes an arbitrary stochastic process and you get the reward at whatever time you decide to stop: you get the value of the reward then. Now, there is a particular case where the underlying stochastic process is a Markov process and the reward function is a function on the state space of that process, that's a Markovian description, then your value function becomes . . . it's no longer a supermartingale because that's for arbitrary processes, but it is a function of the state space and the analog of the class of supermartingales that I had introduced earlier becomes what is called strongly supermedian functions, a variant of excessive functions or superharmonic functions. It's again the right correction for being able to have closed subsets of the state space . . . .

FF: And then you gave up . . . .

JFM: I stopped doing that, it was a conscious decision when I decided to come back to academics after a short stay in the private sector.

FF: It seems to me that you are jumping over a few years. Your work on stochastic processes was part of your thesis, in Louvain . . . .

JFM: That's right!

FF: I think that you worked in a quite autonomous way. You mentioned G. Debreu, but for a research in mathematical economics . . . .

JFM: Economics was at the Flemish university.

FF: And mathematics at the French-speaking one, right? Did you work by yourself?

JFM: No, I had a very nice advisor, José Paris, but he left me quite free . . . . I was not alone, I met Meyer . . . . I went often to his seminar in Strasbourg when I was in Heidelberg, so that was during the last six months of my thesis.

FF: So, from Louvain, you went to Heidelberg, you completed your doctoral thesis, but you already had a position there, didn't you?

JFM: Yes, I was visiting professor . . . .

FF: How did you get this opportunity?

JFM: Thanks to Werner Hildenbrand, whom I had met at CORE . . . everything happened during the same year 1968–1969, the year I spent at CORE . . . .

FF: As doctorate student?

JFM: Not at all, I was preparing my master's thesis, as I said.

FF: You did everything at the same time, I am confused . . . .

JFM: So, I spent the academic year 1968–1969 at CORE doing my master's thesis in economics at KUL with Debreu as advisor and I remember it very well, because 1968 is also the year of my first CORE DP with Birgit Grodal. During that year I completed my undergraduate studies in economics, but in the meantime, I was also working on my thesis in maths. I had finished the studies in maths the summer before. And so, at CORE, W. Hildenbrand told me about joining Klaus Krickeberg at Heidelberg and I said "why not?" and I went to Heidelberg the next year, I finished my thesis there, and that's where life started, basically, because I had a very nice visit to Jerusalem in the middle of that year.

FF: 1969–1970 thus?

JFM: Yes, I was invited in Jerusalem by Robert Aumann.

FF: Whom you had met at CORE?

JFM: Right ... and in fact when coming back from that, it was probably in May, Krickeberg asked me "What are you doing next year?" I replied that I did not think of it and he said that I should, that it was time and I wrote a letter to Lucien Le Cam. Apparently, Krickeberg wrote a letter too to Le Cam and a couple of weeks later I got a letter inviting me to spend the next year at Berkeley ....

FF: So you spent 1970–1971 at Berkeley, and there, you saw G. Debreu again, right?

JFM: Debreu was there, of course. I arrived at Berkeley with the position of "acting instructor" ... but during the year the position changed, first into "instructor," then into "acting assistant professor," then "assistant professor". .. and one or two years later, they made me "associate professor" with tenure ....

FF: All this in two years! What were you teaching at that time?

JFM: Well, I taught probability, statistics ... and I think that's probably the time where I got disgusted ... I thought it was impossible to be at the end of your career just in a few steps, at that age ... I needed to start a new life ....

FF: But at that time, you had already written your first papers in game theory, and you had met Shmuel Zamir, with whom you were going to write other articles. Wasn't game theory enough to open new horizons?

JFM: Yes, that's right ....

FF: But you decided to leave Berkeley for a private company, Solvay, in Belgium. You were on leave from Berkeley ....

JFM: And on leave from Louvain as well.

FF: After the short period at Solvay, you said you had decided not to work in mathematics any more ... maybe, this is not the right way to put it ....

JFM: It is right. I just continued to use it when I needed it. As I said, this was a fully conscious decision, when I came back to academic life ... I am not sure that I made the decision of not working in G.E .... but in any case, I gave up working in G.E at that time ....

FF: We are thus in 1974 and CORE has moved to Louvain la Neuve. You are professor of mathematics ... you are completing the series of "Mertens–Zamir" papers .... Recently, in Paris, Sylvain Sorin gave a brief talk over "the Mertens–Zamir operator" and proposed a quite impressive diagram, a tree with many branches, showing the impact of your work with Shmuel, specially in the context of differential games, in continuous time ... did you follow these developments?

JFM: Sylvain is the one who knows better these recent achievements, of course. The problems solved with Shmuel Zamir just start with some results of Aumann and Maschler which remained a bit mysterious. They had proved that, in infinitely repeated games with lack of information on one side, the value existed. In the case of lack of information on both sides, they knew the minmax and the maxmin and that these two quantities could be different (namely, that the value could not exist). And so the question was to know how the v_n's behaved. Shmuel and I showed that the limit of the v_n's or of discounted values still existed and was given by an explicit formula ... this is the formula that gives rise to what Sylvain or

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others call the "Mertens–Zamir operator." Now all the recent progress was done, I think, by people who do optimization in France. They obtain beautiful results, they essentially show that, instead of thinking about an n times repeated game, which is played from instant 1 to n, and then let n go to infinity, one can consider an arbitrary game in continuous time (we come back to it once more), which is not repeated, which is not even stationary; the payoff functions can be a function of time but since, in continuous time, people play at least once over any small interval of time, the previous operator turns out to be the key of the problem over any such small interval of time and becomes thus a basic tool in these nonstationary problems in continuous time . . .

FF: These problems take the form of a zero-sum game, right?

JFM: Yes, they are zero-sum games . . . .

FF: Your articles with Shmuel Zamir were I think all published by *International Journal of Game Theory*, so they were widely accessible, for instance to doctorate students (like myself at the time), while the original work Aumann and Maschler remained confidential for a long time . . . .

JFM: I have the feeling that along all this period we were talking about, the seventies, Shmuel and I were the only ones to work in that area . . . .

FF: Your cooperation went on for quite some time . . . .

JFM: Somehow . . . let's say from 1974 to 1978, 1979 . . . I remember that in 1980, after a long stay in Jerusalem, I already did not work much with Shmuel . . . .

FF: I heard about that great meeting of game theorists in Jerusalem in 1980, it lasted for several months . . . I guess that was Aumann's idea?

JFM: Yes, it, in fact, went on for a full year, 1979–1980, in the Einstein building of the Institute of Advanced Studies . . . it was a charming old building, with a view over the Judean desert and the old city . . . it was wonderful! I had an office on a corner, with a view on both sides . . . wonderful!

FF: At that time, did you still work on repeated games?

JFM: Among other things. At the same time, I had conversations with Shapley over strategic market games, I was also working over stochastic games, with Abraham Neyman, since one year or two, but the good solution was found this very year . . . it is also at that time that I started to look at refinements of Nash equilibria, with Elon Kohlberg . . . .

FF: I remember that, at a conference that I attended much later, in 1985, again in Jerusalem, Aumann said that you had contributed to all important questions that could be asked then in game theory . . . we know how Aumann likes to make such lists of major problems! We just mentioned stochastic games, which are somehow related to repeated games: you proposed yourself a general model of which both classes are particular cases. Could you say more about your work with Abraham Neyman?

JFM: It's a very simple model, which was proposed by Shapley . . . but the time at which he proposed it and the way in which he wrote it are really remarkable! This was before Bellman . . . and all the principles à la Bellman are there!

FF: Shapley's article was published in 1953?

FIGURE 2. Participants in the conference on repeated games, Jerusalem, Israel, June 1985. From left to right (top): Constantine Melolidakis, Françoise Forges, David Blackwell, Thomas Ferguson, and Jean-François Mertens. One can also recognize Roy Radner, Truman Bewley, and Robert Weber (bottom).

JFM: Exactly! And Bellman was just the one-person case! This paper of Shapley is the source of everything that was done later in dynamic economics, etc.[6]

FF: But Bellman worked independently of Shapley . . . .

JFM: Yes, sure . . . .

FF: But Shapley anticipated everything . . . .

JFM: Yes, everything is there, and for n persons! He had proved the existence of the value and with the same argument, I don't remember whether it's in the paper, one can show the existence of equilibria, in the discounted case . . . so the natural next step was to know what happened when payoffs are not discounted, or when the discount rate goes to 0, . . . there must have been a paper, by Gilette, which maybe gave an example where it did not work, but some time later, there was the paper by Blackwell and Ferguson, "The Big Match," which is a marvelous paper, which shows on an example, that the value exists but can be nontrivial . . . David Blackwell, I liked him a lot! There were two colleagues that I liked particularly at Berkeley: Jerzy Neyman and David Blackwell . . . one should read Blackwell and Ferguson's paper again: not a single superfluous word in it!

FF: I remember that you recommended it to me . . . .

JFM: Everything that must be said is said, and that's it! And the same in his talks: he was so precise! He was going deeply into the matter, but in such a parsimonious way, it's just wonderful! I liked him very much! Anyway, after Shapley's article, the question was clear from the start: what happens to the value

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if payoffs are not discounted? This amounts to basic questions in mathematics, sums of series and the like . . . how do we take the sum or the average of an infinite sequence of numbers if this sequence is generated by strategies in a game that is originally finite? This problem was largely solved, the main progress was done in papers by Kohlberg and by Bewley and Kohlberg.

FF: This was for so-called games "with absorbing states," right?

JFM: Bewley and Kohlberg was a general result on stochastic games, showing a regularity property of the value as a function of the discount rate, when the discount rate goes to 0. Then Kohlberg, in an independent paper, showed the existence of a value in games with absorbing states, and this was essentially a generalization of Blackwell and Ferguson's . . . .

FF: Yes, the "big match" is the prototype of a game with absorbing states . . . .

JFM: And my contribution with Neyman, was to show that there was always a positive answer, that everything worked well . . . we used Bewley and Kohlberg's result . . . .

FF: If I remember well, this was really viewed as the solution of an open problem . . . like in mathematics . . . .

JFM: (laughs)

FF: I just wanted to say that the problem was well identified and that maybe several scientists were trying to solve it at the same time . . . .

JFM: . . . .

FF: I mentioned earlier this survey where you present repeated games with incomplete information and stochastic games as particular cases of a single unifying model. Sylvain Sorin, whom you must have met at the end of the seventies and who was at the Institute of Advanced Studies of Jerusalem in 1980, would, under your influence, work on models which share features both with stochastic games and repeated games with incomplete information . . . .

JFM: Sylvain was working on . . . .

FF: Wasn't it a "big match" with incomplete information?

JFM: No, no . . . at that time we could rely on a complete analysis of games with incomplete information in which information did not depend on the state—state independent signaling—and in his very first work, Sylvain was trying to see what would happen in models in which one would get rid of this restriction . . . it is true that some of these would make think of a "big match" or could be reinterpreted as a "big match" . . . I should go back to Chapter 8 of our book to talk more precisely . . . afterwards, but that was in the eighties, it is true that he looked at a variant of the "big match," a non-zero-sum variant, but that's much after . . . .

FF: So we talked a lot about repeated games with incomplete information and stochastic games and now we are reaching the beginning of the eighties . . . I have the impression that you left that topic for other ones, which we mentioned already: refinements, Shapley value . . . repeated games with incomplete information remain nevertheless alive in a book project, which you started with Shmuel Zamir . . . . A preliminary version of the book appeared much later, as a CORE

FIGURE 3. Conference "Choices, Games and Economic Organizations: A Tribute in Honor of Claude d'Aspremont and Jean-François Mertens," CORE, Belgium, June 2011. From left to right: Elon Kohlberg, Jean-François Mertens, and Yair Tauman.

Discussion Paper, co-authored by Sylvain Sorin . . . that's also a reason to mention him . . . this book seems to have played a crucial role for a selective group of game theorists but it seems to have been quite an adventure!

JFM: It seems to me that some years were wasted there . . . (laugh).

FF: Why wasn't the book published?

JFM: The material grew up without any proportion with the initial project . . . the initial project was to put together, in a bit more systematic way, everything that was available on repeated games with incomplete information . . . but the book was also going back to the basics that were behind and gathered them a bit like in a handbook of everything that was available on that kind of topic at that time . . . then I was fed up, I got the feeling that I was working alone . . . .

FF: Maybe you wanted to push it too far?

JFM: . . . .

FF: So you leave it like it is for the moment?

JFM: Yes.[7]

NOTES

1. I limited the editing work to the strict minimum, e.g., translation of the parts in French. I am grateful to Pierre Dehez, Enrico Minelli, and Anna Rubinchik for having read early versions of the transcript.

2. *Repeated Games* is now to appear from Cambridge University Press.

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3. According to Ramsey (1928). "we do not discount later enjoyments in comparison with earlier ones, a practice which is ethically indefensible and arises merely from the weakness of imagination."

4. I.e., "Relative utilitarianism."

5. Rubinchik and Mertens were not aware of Cass and Yaari's (1967) paper at the time.

6. As opposed to Bellman's model, Shapley's has an infinite horizon.

7. As mentioned in the Introduction, *Repeated Games* is to appear from Cambridge University Press.

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