

An Interview with Salvador Barberà

Carmen Beviá*

Universidad de Alicante

Jordi Massó*

Universitat Autònoma de Barcelona and Barcelona GSE

July 25, 2017

It was a nice spring afternoon in Barcelona on April 6th, 2017. We met at 5:00pm at Salvador's place to interview him. We chose this day because Carmen was in Barcelona; the day before she attended the thesis defence of her last Ph.D. student at the UAB. When Salvador received us, Cesca (Salvador's wife) was not in yet, but she joined us later for dinner. We sat in a beautiful living room. In one of its walls hangs Perico Pastor's portrait of Salvador, the present given to him by his friends on the occasion of his 65th birthday celebration, back in June 2011. We tried to follow our prepared questionnaire, but often he moved quickly to his (and quite abstract) work with Dolors Berga and Bernardo Moreno. Salvador wanted to know our reactions about his present and future work. But we finally were able to bring him back to the past. What follows are his answers to our questions.

*We thank Eduard Talamàs for many helpful suggestions.

Q. Incentives and Northwestern in the early 70s: how important was this environment in shaping your interests?

A. The intellectual atmosphere at Northwestern, in the early seventies, was very exciting, due to the presence of different, attractive persons.

Stanley Reiter was a crucial person in the Economics Department and the Business School. He had worked in Purdue before arriving at Northwestern, along with other very renowned people like Vernon Smith and Charlie Plott, and had produced there a generation of Ph.D.'s who eventually ended up at Northwestern along the years: Hugo Sonnenschein, Morton Kemion, Nancy Schwartz, John Ledyard, had been his students. He created the Math Center, and had a magnetic influence on many of us, through a legendary course on Debreu's *Theory of Value* ([1]) that he taught by asking –from the back seat of the classroom– the most penetrating and educational questions to the students who presented this book. In addition, Stan was one of the promoters –along with Leo Hurwicz, Tom Marshak and Roy Radner– of annual meetings on Decentralization, where a small but intense group of people presented work that gradually built the basis for what was to become the large industry of present day mechanism design. These pioneers looked at specific allocation methods as points in a multi-dimensional space of mechanisms, within which the different methods could be compared and classified according to their performance on several dimensions. Decentralization was a major concern of the times, along with efficiency and coverage, as measured by the amplitude of the set of economies for which each mechanism was well defined. And it was one of these people, Leonid Hurwicz, an inspiring personality who visited Reiter very often, who explicitly added a new criterion of performance to that list of dimensions: that of incentives, on which I would end up working.

Along with Reiter and Hurwicz, I had the privilege of meeting many other people whose work is at the origin of incentive theory and mechanism design. John Ledyard

was my first micro teacher and I could see him developing different approaches to study incentives, and using a variety of game theoretical concepts. And mind you, at this time Game Theory was not taught in the Economics Department, nor at the Business School, although I was lucky enough to take a course on the subject from a recent arrival at the School of Industrial Engineering: a very young Bob Rosenthal. Ted Groves arrived with his new ideas regarding incentives in teams, which later on he developed into his much richer class of mechanisms. I remember very well how he rehearsed for the Minnesota meeting where he presented his ideas through joint work with Martin Loeb ([2]), who was one of my classmates. He certainly understood that he had gold in his hands, and he managed to convey his message so well that I have never seen such an enthusiastic reaction to a single paper as the one Ted obtained in that meeting, which I attended as a student. I remember Hurwicz jumping to the blackboard and trying to generalize the paper on the spot, even before Ted had concluded. This was a thrilling moment that gave me an overview of what is best in our research profession. I had seen a paper grow, an author working hard to present his discoveries, and a great idea to succeed in front of a demanding audience. And more people were working in the area: faculty members like Elisha Pazner, Jean Marie Blin and John Roberts (who just arrived to Northwestern from Minnesota), and students, who also made early contributions to the theory of mechanism design, like Andy Postlewaite (who produced a beautiful paper with John Roberts ([3])), Jim Jordan (my classmate), and my friend Xavier Calsamiglia (at Minnesota).

An important arrival to Northwestern was that of Mark Satterthwaite, just after he had proven, independently, the fundamental result that we all know by now as the Gibbard-Satterthwaite theorem ([4]).

Antonio Camacho taught me a course on social choice, based on Amartya Sen's recently published book ([5]). Antonio was a Spaniard who made all his career in the US, a former student of Hurwicz, also interested in mechanism design, a very original

and independent thinker, always a bit critical of the fads that are so influential in Economics research, where some topics are deemed essential today and then forgotten or even declared dead the day after.

And then Hugo Sonnenschein joined Northwestern. Hugo was already a star professor at 34, when he arrived. And I knew him indirectly because he had co-authored the first published paper of my Catalan friend Andreu Mas-Colell, then a second-year student at Minnesota ([6]). The paper was an important contribution to social choice, a subject on which Hugo has written great papers, and that he has always defended in many ways. Hugo became my advisor during my fourth year and his advice was absolutely decisive in my career. I was his third Ph.D., which places me among the first of a long list of people who enjoyed his guidance, many of whom populate the best universities in the world. John Roberts –who was his second Ph.D.– and myself proudly call these other students of Hugo our younger brothers, because we came well before Hugo’s spectacular success at Princeton.

Q. Why strategy-proofness?

A. Because I had grown in an atmosphere marked by the idea of mechanism design, and found myself immersed in the excitement regarding the new developments about incentives. I always wanted to write on that subject and from that perspective. I am talking about the first half of the seventies, when the Gibbard-Satterthwaite theorem has just been proven, the Groves mechanisms were being gradually understood, and Hurwicz was adding the incentives dimension to his grand vision of mechanism design. All of this happened around my university, if you add the seminars I attended there by Gibbard, and by a very young Jean-Jacques Laffont.

The ambition to write about incentives was well into my mind, but my abilities failed me on several attempts. One of these involved trying to show that manipulating the competitive mechanism would require a costly effort to refrain from consuming the

preferred affordable bundle of goods, and that this cost might greatly reduce the danger of manipulation, as exhibited in Hurwicz's work ([7]). That led me to try models of adjustment where people could influence the dynamics of prices: the project eventually ended after taking a course on differential games in the math department, that proved to be well above my capacity!

Finally, one day, Hugo met me in the corridor and asked me a simple question. What happens to the Gibbard-Satterthwaite theorem if you have multi-valued social choice functions? And that changed my fate as a graduate student. Here was a question that still related to incentives, but changed my focus from exchange economies to social choice functions. I had learned with Camacho about this framework but had not tried to work on it. I soon realized that I felt very comfortable when working from this new perspective. Indeed, this framework turned out to be very friendly to me. From then on, a thesis emerged in a few months, whose articles were very well published.

Q. Indeed very well: *Journal of Economic Theory* (1977, [8]) and *Econometrica* (1977, [9]). What would you say is the leitmotiv of your research?

A. The importance of simple questions is something that has always fascinated me. A simple question does not always have a simple answer, and when this is the case, you have a beautiful example of what Reiter used as metaphor of research. He described the researcher as someone who walks a path full of pebbles, and chooses which ones to look under. Some of them may hide the entrance to fascinating caves, while others may just have a bit of dust under them. The manipulability of multi-valued social decision functions did not only ask for a wider definition of manipulability, but also raised questions about ranking sets, on which I wrote later as an independent topic, and about the introduction of lotteries in social choice. That topic was the object of my first paper co-authored with Hugo ([10]), and came from a second simple question, that he asked me while waiting at a traffic light: what happens when we enlarge the

framework of Arrow's theorem to allow for lotteries over preferences to be the result of aggregation? And again, the consequences of writing that paper went beyond the starting quest, and led to my interest for stochastic preferences, on which I worked with Prasanta Pattanaik ([11], [12] and [13]) and for probabilistic judgments, a subject I developed with Federico Valenciano ([14]). Let me just add another simple question. Again, while walking, Arunava Sen asked me: what if agents, instead of always choosing their best available alternative, would choose either that or their second best one? It took me many efforts and those of Alejandro Neme, until we found a way to start from that simple question and to find a characterization of agents' behavior that follows this or similar patterns, where full optimization is not attained but people are still guided by a referential preference order ([15]).

Q. Your large network of scientific collaborations has been important for you, hasn't?

A. Yes, it has. Scientific collaborations are important in general, and they have been particularly in my case. Since my first World Congress of the Econometric Society (Toronto, 1975) I was lucky to meet people who were to become co-authors, friends and sources of inspiration for my work. This was the first congress where I presented a paper ([9]), and I was lucky to meet there respected seniors like Amartya Sen and Peter Fishburn, to connect with Prasanta Pattanaik –whose papers had been crucial for my thesis– and to start long lasting connections with Claude d'Asprémont, Louis Gevers and Louis-André Gérard-Varet. Soon after, I made additional friends in the profession through early meetings, like Jean-Jacques Laffont and Hervé Moulin, and I got to know Eric Maskin and Roger Myerson during heated discussions about the revelation principle. A second wave of very important contacts came thanks to Maurice Salles' initiative to organize a Social Choice and Welfare congress in Caen, in the summer of 1980, from which a book emerged. Along with other people I already knew, I first

met Bhaskar Dutta there, and strong connections were established, eventually leading to the creation of our journal, first, and of the Society for Social Choice and Welfare later, after a new interesting meeting that I jointly organized in València with Hervé and Antonio Villar. Another extraordinary opportunity to meet people and renew ideas came during a visit to Stanford in 1984-85. Hugo was also visiting, and along with him a large group of his Princeton students. They were many, now important names in the profession, but let me just mention two of them, Arunava Sen and Faruk Gul, who became my co-authors later on ([16] and [17]). Also among the recent arrivals from Princeton was a first-year graduate student, Matt Jackson. I was about to become his micro teacher but luckily he got exempted from the course: instead, we wrote our first joint paper ([18]). After so many years we still write together, and I keep learning from him, as I did even at that early stage! I could go on and tell you about more people with whom I shared the joy of discovery: Michael Maschler, Federico Valenciano, Anke Gerber, Jordi Massó, Alejandro Neme, Lin Zhou, Shige Serizawa, Anna Bogomolnaia, Ennio Stacchetti, Lars Ehlers, Danilo Coelho, José Alcalde, Dolors Berga, Bernardo Moreno, Clara Ponsatí, Carmen Beviá, Antonio Nicolò, Walter Bossert. At least one special story hides behind each of these collaborations, maybe more. And if I keep an active research agenda after becoming emeritus, I owe this privilege to a collection of co-authors who are also friends, and with whom I share, among other experiences and interests, the thrill of looking for a good and challenging question.

Q. Why restricted domains?

A. Much of my work is associated with the idea of strategy-proofness. Indeed, this was already the topic of my dissertation, and I have kept working on the subject. I already told you that initially I got there through a lucky combination between my interest in incentives and my comparative advantage when using the techniques of Social Choice theory. But my approach to the topic evolved with the passage of

time. At first, I proved impossibility theorems. Then, for a while, I moved slightly away from dominant strategies without plunging too much into the wave of works that used a Bayesian perspective. Still fascinated by the simplicity of certain solution concepts, I explored the concept of protective equilibrium, with Bhaskar Dutta, and its consequences ([19]). I also continued to work with the idea of dominant strategies in contexts where social outcomes are lotteries. Assuming that voters have von Neumann-Morgenstern preferences in that context was already a first way to introduce restrictions on their preferences. And I was happy to produce a number of direct proofs of the Gibbard-Satterthwaite theorem and of Arrow's as well, using the notion of pivotal voters and of option sets ([20] and [21]), as a result of my repeated visits to these seminal sources. It was only later, into the nineties, that I started looking more systematically at the possibility that, although not universally, there might be many instances where non-trivial dominant-strategy mechanisms could be defined. The fundamental and beautiful work of Hervé Moulin on strategy-proof rules under single-peaked preferences was a tremendous stimulus ([22]). Thanks to a series of papers with Hugo and Lin ([23]), Faruk and Ennio ([17]), Jordi, Alejandro and Shige ([24], [25], [26] and [27]) I gradually understood the possibility and the difficulties associated with an extension of domains to cover the case of multi-dimensional alternatives. And, more recently, I have worked hard with Bernardo and Dolors in order to provide additional results regarding domains where it might be possible not only to promote individual incentives, but also to avoid group manipulations, and to attain efficiency ([28], [29], [30] and [31]).

Of course, looking at domain restrictions and sticking to dominant strategies is only one of the many avenues of research suggested by the Gibbard-Satterthwaite theorem. A major line that was also opened by that result is the invitation to accept that voters play games, and go to a full-fledged game-theoretical analysis of their interactions through different mechanisms. The literature on implementation flourished for a while. More persistent is all the literature that models economic agents as the players of

Bayesian games, fit to model a large variety of voting and economic situations. That literature was and still is mainstream, but I see an increased interest in going back to examine the simple idea that, although hard to attain, strategy-proofness and its close relative ex post incentive compatibility are very interesting properties to demand, and ones that may sometimes be satisfied. Part of my fascination with strategy-proofness and dominant strategies, or the equilibrium properties of truth-telling, undoubtedly come from my limitations as a game theorist. But I also like to think that my life-long commitment with these simple topics comes from the strength that I have also appreciated in them. The idea that more and more people explore them from different angles is comforting.

Q. What research environment did you find in Spain on your return?

A. When I finished my Ph.D. I returned to Spain immediately, as did most of the few other people who had pioneered the quest for a doctoral degree in Economics in England or the US. A notable exception was Andreu Mas-Colell, who developed most of his brilliant career in the US. But he always kept well connected with the Spanish reality, and he played a very helpful role for Spanish economics from his American base. Other than him, though, most of us returned to Spain and found that there was a very stimulating task ahead. Not an easy one, but a stimulating task. I returned in the summer of 75, started to work in Madrid in October, and Franco died one month later. He had been in power since years before my birth. We hardly knew what was ahead for our country, but we were full of hope. I was 29 and felt part of a generation that could improve the country at large. And changing the university was part of the task, one about which I had great hopes and was prepared to work on. Academic research in Economics was practically non-existing. Academic journals published translations of articles, and original contributions were rare, often summarizing the readings of those people who read anything at all. I attended the 1975 World Meetings of the Econo-

metric Society in Toronto, along with half a dozen Spaniards, none of us tenured. For many years, any time I met with Andreu in a congress we would count how many more we were, and felt proud of seeing how numbers increased steadily. This increase was partly the result of a social trend toward open relationships with the world that was experienced across the whole of our society. But it was also the result of very deliberate efforts to convince younger generations of Spanish students that investing in graduate studies abroad and then doing research in our country was feasible and exciting. We were in small numbers, but very committed to the task. Joaquim Silvestre, Joan Esteban, Xavier Calsamiglia and Isabel Fradera in the Autònoma de Barcelona, Juan Urrutia, Federico Grafe, Carmen and Inmaculada Gallastegui in Bilbao, Javier Ruiz Castillo and Carlos Escribano in Madrid, myself in Madrid and then in Bilbao, and finally in Barcelona. We preached the good news of economic research, starting from very modest beginnings. In 1977 I published in *Econometrica* and *Journal of Economic Theory* ([9] and [8]), and so did Calsamiglia (*Journal of Economic Theory*, [32]) and Silvestre (*Econometrica*, [33]). These were all-time firsts: no one had published at this level from Spain, and this, along with Andreu's successes in the US, were proof that there was a future ahead for research of competitive quality. We started sending students abroad, and they tended to come back, because the country was getting more attractive, universities were growing, and governments became more supportive. All remained somewhat local for a while, until new universities and research centers started emerging that shared the same values of excellence, and joined forces with the pioneers. But it was no easy task, as we started from very low. I directed the *Revista Española de Economía* for about 15 years, and thanks to a large number of young collaborators it became the first Economics journal ever refereed in Spain, gradually turned into an English language publication, and eventually became the *Spanish Economic Review*. Other journals followed suit, in particular *Investigaciones Económicas*, directed by Rafa Repullo. These journals provided many young Spanish economists

their first chances to learn and practice the principles of anonymity and rigor that are required from a professional researcher and that had been missing before in our culture. We also communicated with other emerging groups that were also rowing against the current in their own countries. Collaboration with Jean-Jacques Laffont's group at Toulouse and Louis-André Gérard-Varet in Marseille led to the creation of a Southern European Association (ASSET) which was later to expand, but provided strength to our somewhat isolated groups, and got voice through a joint series of working papers called SEEDS. The group of researchers at Bellaterra started a yearly congress, the Symposium, that was to become twenty-five years later one of the pillars, along with the *Spanish Economic Review*, on which a Spanish Economic Association emerged. I cannot mention by name all the people who added their efforts to the continued task of building a research profession in an incredibly short time, between 1975 and 1990. By then, when I chaired the local organizing committee of the Econometric Society World Congress, we had attained a size that allowed projecting the image of a research community on the go. That image is now much stronger, but it was very rewarding to be part of the initial push, and Barcelona 1990 was certainly a turning point.

Q. But your first job in Spain after the return from the US was in the Universidad Autónoma de Madrid, right? How was it?

A. My first two years after the doctorate, at the Autónoma de Madrid, were very momentous. It was a time of change in Spain, and democracy needed some years to consolidate, to change regime. I was an interested observer of this change, but concentrated strictly on getting my work published, and was very lucky at these first stages of my career. Less than two years had passed when I got a permanent position in a rather disputed contest. Rules for these promotions to tenure were simply crazy, including six different exercises and the writing of a book on methodology, with a minimum of 250 pages. Luckily, I had two acceptances from *Econometrica* ([9] and

[34]), two from *Journal of Economic Theory* ([8] and [10]), one from *The Review of Economic Studies* ([35]), a contribution to a book ([37]) and a couple of articles in Spanish ([38] and [39], all in twenty months.

Q. Not bad! The first six: *Journal of Economic Theory* (1977, [8]), *Econometrica* (1977, [9]), *Journal of Economic Theory* (with Hugo, 1978, [10]), *The Review of Economic Studies* (1979, [35]), *Econometrica* (1979, [34]) and *Journal of Economic Theory* (1980, [36])! And all from Spain.

A. I often wondered how all this could happen in such a short time. I get quite mad at the idea that our profession is becoming more and more cruel with young people, due to extreme and unnecessary delays in publication times. My new position was in Bilbao, and even if I would have preferred to join the Autònoma de Barcelona, I moved to that city, where I had gotten my undergraduate degree. I was lucky to meet there with young colleagues who had got degrees in England, and with a young professor, Juan Urrutia, who became a lifetime friend with whom I have developed a great deal of projects. Juan has been a government official, a founder of Universidad Carlos III de Madrid, a banker, a philanthropist. We created an Institute of Public Economics, founded ASSET, started a Master's program that sent many Basque students to study abroad, and is even now giving us support at Bellaterra through a small research foundation, MOVE, that was created to circumvent some of the endemic problems that our large, rather sclerotic universities, impose on any innovative project for change. Bilbao provided an exciting opportunity for individual and collective work, and we ended up spending nine years of our life there. My children grew up there, my wife wrote her Ph.D. dissertation and started her successful career as an economic historian there. It is a second home, full of former students and colleagues. And it has become a beautiful, vibrant city.

In 1986 we moved to Barcelona, where we were born, after a twenty-years detour. I

was forty, full of projects, had started to have some recognition and had acquired some organizational experience in Bilbao. I arrived at a place that already had developed some of the more ambitious projects, and was ready for more. Many students abroad were getting ready to return, and facilitating their arrival was one of the first and greater pleasures I've had in academic life. And as more and more well prepared people joined our ranks, we were ready for more ambitious endeavors: the doctoral program, the extension of our basic principles to other universities and research centers, the creation of a conscience that we were part of a revival of economic research at the European level.

Q. Yes, and then you promoted the creation of IDEA, the International Doctorate in Economic Analysis, which was the first program in Spain to be entirely taught in English and open to students from all countries.

A. I had been very reluctant till the early nineties to retain Spanish students in Spain for their doctoral studies. I thought that the best we could do for our students is to send them abroad. But we had an increasing potential to teach a great graduate program, based on the arrival to Bellaterra of excellent people with degrees from abroad. People like Jordi Massó, Jordi Caballé, Inés Macho-Stadler, David Pérez-Castrillo, Xavier Martínez-Giralt, Pau Olivella, Clara Ponsatí in my department, and Xavier Vives, Joan Maria Esteban, Carmen Matutes or Jozsef Sakovicz at the Institute of Economic Analysis, along with multiple visitors, were the basis for a new project, not oriented toward our own undergraduates, but open to people from other parts of Spain, Europe and the world. It was hard to break with many of the conventions that did plague our universities, too much used to think of graduate students as cheap labor force in the lab or servants of their master for years, and to create an open program where people could identify their interests, choose their advisors, select topics and measure their own strengths. A program that would lead people to mature as integral,

independent researchers, and help them integrate into the international research community, open to all subjects and interests. That was our ambition, which we shared with other leading universities, and allowed us to form ENTER, a consortium that involved from the start Toulouse, Tilburg, Mannheim, and University College London. It always was, and still is hard to sustain such program because of many factors. Our universities have not evolved much, the philosophy underlying Ph.D. programs in Economics is not the same as in that of natural Science departments, and many fellowship programs were severely cut during the recent economic crisis. Yet, the program gave my department a unity of purpose and I am proud of having promoted it.

Q. But let's start from the beginning. You studied Economics at the Universitat de Barcelona, but your Bachelor degree is from the Universidad del País Vasco at Bilbao. What happened?

A. I had been a very good student prior to the University years, at the French Lycée of Barcelona, and had always enjoyed learning. I was fascinated when introduced to the axiomatization of integer numbers, and also enjoyed the rudiments of formal logic. Just before starting my Economics courses I was a finalist of the first Spanish Mathematical Olympics, but in fact I now understand that it was only because I could solve a combinatorial problem that you could easily reduce to the characterization of majority voting, even if it was not worded like that. All other problems involving differential calculus escaped me, but this was a premonition. Later on, at Northwestern, I also took several courses in math, but eventually I made my living with a few combinatorial calculations and a little logic. I think it is, ex post, another of the nice facts of research: with a little luck and some work, you may discover where, in the large menu of technical options, lays your relative advantage.

Unfortunately, the university I found in 1964, after leaving high school, was not attractive at all. With very few exceptions, the quality of courses went from mediocre

to very low. Students were a crowd: my class had hundreds of people enrolled in a single classroom. And there were more interesting things to do. In 1964-65, a student revolt was cooking against Franco's fascist-like trade union system, and I quickly engaged in activities that were more exciting than attending rambling classes by teachers I didn't find too exciting. Now I understand that this involved a lot of arrogance, and that some of them, if properly approached, could have taught me quite a bit. But arrogance at 18 is quite natural, and I ended up essentially wasting my four years at the University, from an academic point of view. But it was not a waste in other dimensions. I felt part of the trend that was starting toward a democratic society, met many of the people that would end up being partners in my US adventure, and met Cesca, my lifetime companion. Yet, things were not good for learning, I got expelled from the Universitat de Barcelona and sent to Bilbao, where I finished my five-years program in four years, just in time to join the Spanish army as a conscript, for an extra fifteen wasted months. What remained from that period, was an unsatisfied hunger for knowledge, and the certainty that I had to catch up after years without learning Economics, even if I had learned some things about life. I still remember the thrill I got when Alfred Pastor, one of my friends who went to MIT two years before I went to the US, sent me a letter that I read while stationed in Ceuta, where I was a soldier. He had just taken a ride on the same elevator than Samuelson, and that made my day: I already felt close to knowledge, through this connection. Just imagine how little we knew, and how large our hope that we could make up for lost time in the US. I was not even thinking about research: just about learning and catching up!

Q. Although you are best known for your work on strategy-proofness, you have been interested in other topics in Social Choice, and also on other subjects relating to individual and collective behavior.

A. I have been fascinated for years by the idea of stochastic choice, which I discov-

ered through work with Hugo ([10]) and developed with Prasanta Pattanaik ([11], [12] and [13]). Some of the projects on which I have spent more unproductive time and effort lie in this area. Even if I could not solve them, I am glad to see that stochastic choice becomes more and more attractive from a theoretical point of view, and that young colleagues like Miguel Angel Ballester and José Apesteguia are among those who push the frontiers on the topic.

Another area that I worked quite a bit on seems rather abstract: how to extend orders from a set to its power set. The initial needs that I felt when writing my dissertation led me to realize that many researchers attribute to economic agents the ability to rank sets of objects and require these rankings to be “consistently” connected with the way in which the same people rank singletons. However, “consistency” can take many forms, and this is because sets of objects admit many different interpretations. I enjoyed writing a survey of the subject with my good friends Prasanta Pattanaik and Walter Bossert ([40]), which gave us an opportunity to touch upon a variety of models that use apparently similar ideas but reflect of very different issues.

I have also enjoyed working on the subject of coalition formation, from different angles, in works with Michael Mashler ([41]), Anke Gerber ([42] and [43]), and Carmen Beviá and Clara Ponsatí ([44]). I have learned the hard way that even the most apparently simple and easy to describe models may become devilishly hard to deal with. Hedonic games are an example. But I continue to be very interested in understanding coalitional structures, and even if again some other people have taken paths that I could not follow, I am always looking for the next challenge that I can address in this area.

As for Social Choice theory, I have enjoyed the stimulus of different people and been able to produce papers with them, a double reason for pleasure. Working with Matt Jackson is a permanent source of challenges and satisfaction. One of the hardest papers we wrote, on strategy-proof exchange ([45]), was also one of the most rewarding.

But that was still on incentives. Later on we have worked on papers like “Choosing how to Choose” ([46]), “On the Weights of Nations” ([47]) or our recent piece on protests and revolutions ([48]), topics that, thanks to Matt, have led me somewhat away from classical topics in Social Choice, and at the same time express the vitality of this research area, broadly understood. This also applies to my interest in matching, rationing models and location models, which I include into a broad view of Social Choice as a particular way to look at resource allocation, as I was taught by my Northwestern teachers. My present projects with Antonio Nicolò ([49]) and with Dolors Berga and Bernardo Moreno (in preparation), on disclosure of information, are also mind openers for me. And I have also enjoyed the possibility of investigating the properties of specific voting rules in work with Ana Bogomolnaia and Hans van der Stel ([50]), with Bhaskar Dutta and Arunava Sen ([16]), and more recently with Anke Gerber ([51] and [52]) and with Danilo Coelho ([53]).

Q. You have had responsibilities related to research policy, both in Catalonia and Spain. How were they?

A. The chance of a long life has also given me the opportunity to participate in two enriching experiences outside of my activity as a professor. One was to birth of the Catalan Institution for Research and Advanced Studies (ICREA). Andreu Mas-Colell, who was minister of Universities and Research of the Catalan government commissioned me to create and direct a new sort of institution, whose purpose was to attract talent in all fields of research to Catalonia. ICREA was conceived as a means to facilitate the arrival of highly qualified researchers, who can work in any institution that operates in Catalonia. It was created as a foundation and given the flexibility to act by the standards of the international market for scientists. Under the supervision of highly regarded experts in different fields, we were able to give permanent contracts to people who would never otherwise thought of moving to the rigid Spanish system.

The task was exciting in many ways. Creating a new institution is an inspiring task, which requires understanding the many legal, political and financial aspects of actual institutional design. Also fascinating was the opportunity to deal with scientists of all sorts, to discover the rich variety of people involved in archeological research, in photonics, in astronomy or in so many types of medical topics. ICREA provided me with a glimpse at science at large, through the contact with the experts that gave us support and advice and the people that I had the chance to interview, whether or not they ended up accepting offers. When I left, after only three years, ninety people in all fields had already agreed to join, from all parts of the world. It was a pleasure to work with an experienced politician like Andreu, who gave me freedom, resources and advice, and also enjoyed the project a lot. And since it was a novel initiative, it also needed the support of the Catalan parliamentary groups, which led me, however slightly, to get in touch with a variety of political leaders. Luckily, I have come to realize that science is the least partisan of the issues, and that, at least in my times, there was a wide consensus regarding research. Unfortunately, this consensus is not so big when it comes to the organization of universities.

The ICREA experience paved the way for a surprising twist in my professional life. My former student at Bilbao and Princeton Ph.D. Maria Jesús San Segundo became Minister of Science and Education of the Spanish socialist government in 2004, and asked me to join as Secretary General of Scientific and Technological Policies. To make a long story short, I found myself in less than twenty four hours in charge of all financing of science in Spain, including all large scientific facilities, all research projects, all fellowships. Luckily, these were times of abundance, and my task was more that of stimulating the arrival of deserving projects than that of cutting budgets, as people in the same job after 2008 had to do. These were two years where I did not have one second for anything else. Research came to a halt, and other concerns took over. It was a very intense period, on a completely different job. I took a close

look at many different things: negotiations in Brussels on the ITER fusion project, agreements about Spain's entry to the European Southern Observatory organization, the creation of a new Supercomputing Center and of another for the Study of the Origins of Humans, the supervision of works for the building of a synchrotron and a new telescope, the development of an innovative program to create jobs for high quality individual researchers in universities (I3), and of another program to support large research teams (CONSOLIDER). And at the same time, learning how to report to Congress, how to push for a better budget, how to relate with other ministries. Maria Jesús, my minister, was extremely supportive, I had a very good team, and I enjoyed the job. After two intense years, and when I was developing a project to create a national research agency that might circumvent many administrative problems and give more power to scientists, my Minister was sacked on grounds that had nothing to do with her research policies, and so was I. Two years later, and having seen a different world, I returned where I belonged to, and after a short adaptation period, I started research again. Luckily, I had only been away for two years, and I could go back on track.

All and all, I learned a lot about the importance of science, its rich variety, and the need for continued support to it. About how foolish it is to treat investments in science as current expenditure, that oscillates with yearly budgets. I also understood how difficult it is to be a good politician, and learned that I am not, although I am proud of having been, I think, a good administrator, attentive to the needs of scientists.

Q. You directed a Master in Economics of Science and Innovation at the Barcelona GSE, didn't you? How was it?

A. Very large amounts of resources are used to promote scientific and technological advances. The money comes from a rich variety of sources, both public and private, at the international, European, state and regional level. It is essential for institutions to

understand how to operate in this universe, which is in a way very competitive but also very hard to grasp in all its complexity. During my period in the Ministry of Science and Education I realized that there was a shortage of well-prepared professionals with a broad understanding of the overall system and able to advise governments, firms and research institutions.

When the Barcelona GSE started its new Master's programs, I thought that there was room for a line of teaching that would endow students from different intellectual origins with general skills in that direction, and prepare them to become useful professionals, as advisors to public and private institutions, as fund-raisers or promoters of science and technology-based firms. The program ran for several years. Its graduates came from different scientific fields and acquired an economic perspective, while also being presented with an overview of the scientific, technological and institutional challenges they would face if they chose to find jobs of this sort. Most of them did. The program was eventually discontinued, partly because it was not theoretical enough. I am somewhat surprised in this respect. While very theoretical in my research, the approach I have always taken when it comes to institutional issues has been quite pragmatic and based in my on-the-job learning.

Q. How do you see the research in Social Choice now?

A. I see many interesting avenues for research in Social Choice, broadly understood. My main concern is about the negative role of fads in shaping the junior market. Social Choice is not a popular subject at this point, for reasons that I think have little to do with the intrinsic interest of the questions it addresses or the quality of its major results. This has an impact in the job market, and keeps many good students away from presenting themselves with that banner. But this unfortunate initial shock will not erase the main concerns of Social Choice from the intellectual horizon of talented individuals. I am optimistic that, as all fads, this one will also be reversed. Moreover,

academic lives are long, and our topics will find their way into the agendas of first class researchers whatever their starting point may have been, as it has already been the case in the past. If you look back, you will easily identify great economists, political scientists, philosophers and mathematicians who have never been perceived as social choice theorists and yet have made important contributions to this field, because at some point in their careers they have been intellectually challenged by the exciting issues that it deals with.

Q. Social Choice is not a subject offered in our undergraduate degrees except in very few cases. What arguments would you give in favor of introducing it in the curricula? What should be the emphasis in this subject?

A. Let's not be slaves of labels, but look at contents. I think that undergraduate students can appreciate the importance of normative, axiomatic analysis if it is properly presented. But Social Choice issues are usually introduced in the middle of Micro courses, through a hasty presentation of Arrow's theorem that has then very little continuity in the rest of the course. This is a double waste, because students are usually neither ready to understand the depth of that result, nor to appreciate the importance of the axiomatic method as a tool for better thinking. In my experience, it would be better to introduce the axiomatic method through the analysis of other, may be simpler issues. For example, as a way to better understand what is the meaning of fairness, using results from cooperative game theory. Or by showing how the problems of measuring inequality, poverty or human development are better understood through axiomatic analysis.

What I miss, then, is an appropriate exposure of students to the methods of reasoning of Social Choice theory, and a positive valuation of the important issues to whose understanding it can contribute. Whether or not this is part of a special course is not so important.

And, by the way, I believe that Political Economy is a rather different discipline than Social Choice theory, in purpose and methods. Mixing one with the other is, in my opinion, not a good idea.

Q. Will Social Choice and theory in general have any prominent role in ten or twenty years from now?

A. As I said before, my impression is that the topics we study are intellectually challenging and our method of analysis is useful: therefore, they will stick around. But of course other points of view will be added, as they have continually been added in the past: just think of the recent rise of computational Social Choice theory.

As for theory in general, I am convinced it will have an essential role at different points in the future, with the ups and downs that it has always had. Think of the enormous amounts of work that was made to study the data that became available at the turn between the nineteenth and the twentieth century. Much of it involved no theory, until the then young Econometric Society came to rescue and vindicated the right mix of evidence and theory. I believe that the excitement about the new possibilities opened by new techniques and data in the last decades will eventually bring about a renewed need for unifying theoretical ideas.

Q. In what research projects, and with whom, are you working at the moment?

A. For the moment, I am engaged in many research projects. Of course, they are driven by my intellectual curiosity, but they also respond to something I cherish more and more: the opportunity to work with colleagues who are friends, and whose personal contact I enjoy. Because of that, I am overcommitted, because I am lucky to be friends with many excellent researchers of different ages and varied interests. I have projects with Anke Gerber, Dolors Berga, Bernardo Moreno, Matt Jackson, Alejandro Neme,

Antonio Nicolò, Danilo Coelho, Yves Sprumont, Walter Bossert and Kotaro Suzumura, and I am still looking forward to have new opportunities to work with you, Carmen and Jordi, as we did in the past.

Q. What other things are you doing besides research? Enjoying the grandchildren?

A. Cesca and I have been together for 52 years, and we are enjoying the luck of these first years of retirement in good health. She has always had an artistic vein, and is now spending a lot of time painting. I still use much of my time to do research, but I am trying to learn a bit about writing fiction. We both enjoy the opportunities that these activities provide to enlarge our understanding of the richness of life, and its many facets, beyond the professional activities that have already given us so much. And then, of course, our two children, and our three grandchildren are major players in our life projects. Being parents was a fascinating challenge; being grandparents is an opportunity to rediscover purity, beauty, and the marvels of childhood.

Q. Last but not least, a tip for young generations of researchers?

A. Make friends with the colleagues that deserve your friendship: they are a treasury for the whole of your academic life. I was lucky to meet, early in my career, many of the people I mentioned before, and I found inspiration and support from them, as colleagues and friends. I understand research as a collective endeavor, and I think that cooperating with people that you like is the better part of it. Of course, competition is out there and has to be met, but it is better to enjoy our activity as a joint venture. There are many questions to share, many avenues to pursue. Science is a collective effort. Enjoy the individual excitement that discovery provides, but look further, and also enjoy being part of the whole adventure of knowledge.

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