John A. Weymark

Felix Bierbrauer and Claude d'Aspremont

This interview of John Weymark (**JW**) was conducted on October 13, 2015, in Cologne, Germany, by Felix Bierbrauer (**FB**) and Claude d'Aspremont (**CdA**). The text of the interview has been edited to improve its readability, to clarify some of what was originally said, and to provide bibliographic details for the works cited.

CdA: Let me start with a biographical question. In your CV, you mentioned that you were born in Moose Jaw, Saskatchewan. Did you spend all your childhood and schooling there, and when did you move to Vancouver?

JW: I moved to Vancouver when I was eleven, in the middle of the school year. I did all of my schooling from what's called sixth grade—except for one year in Winnipeg—in a suburb of Vancouver, and then went off to the University of British Colombia (UBC).

CdA: So you were in Vancouver for most of your school education. Then you did your B.A. at UBC. Did you meet some future colleagues like David Donaldson there, and how did you decide to go to Penn for your Ph.D. afterwards?

JW: In response to the first part of your question, I met David Donaldson my first day as an undergraduate. At UBC at the time, most courses were a year long. It was a semester-based university, but they didn't split courses up a semester at a time. I took a special program called Arts 1. It was essentially a great books program that

F. Bierbrauer (✉)
Center for Macroeconomic Research, University of Cologne, Albert-Magnus Platz, 50923 Cologne, Germany
e-mail: bierbrauer@wiso.uni-koeln.de

C. d'Aspremont
Center for Operations Research and Econometrics, Voie du Roman Pays, 34, 1348 Louvain-la-Neuve, Belgium
e-mail: claude.daspremont@uclouvain.be

M. Fleurbaey and M. Salles (eds.), *Conversations on Social Choice and Welfare Theory - Vol. 1*, Studies in Choice and Welfare,
https://doi.org/10.1007/978-3-030-62769-0_16

was thematically oriented, interdisciplinary, and it counted for 60% of my credits. We read Plato, Dostoyevsky, Darwin, and Freud, among others. My section's theme was freedom and authority. The instructors came from a variety of disciplines. There was a group of 120 split into small groups of 20. One semester I had one group leader, and the second semester, I had a second group leader. We met once a week as a whole group.

To get us going, we went on a retreat where we read Conrad's *Heart of Darkness*, and that's when I met David. He was my group leader for the second term. David is a remarkable person and very intellectually engaging. It's because I found him so interesting that I decided in my second year to actually take an economics course. That's how I ended up in economics.

David Donaldson subsequently became a co-author, and then later when I was on the faculty at UBC, of course, a colleague. He's the person from whom I first learned about Arrow's Theorem (Arrow 1951) as an undergraduate. There were many faculty who were very influential, like Chris Archibald and Erwin Diewert. The course I took from Erwin is where I learned duality theory as an undergraduate.

There is another connection that, who would have known, played a big role in my career. As an undergraduate, you can't really understand a research seminar. But somebody—it might have been Erwin—told me that Peter Diamond was coming and what he would be talking about is really important, so I might want to go hear him. The Diamond and Mirrlees optimal commodity tax papers (Diamond and Mirrlees 1971) had just come out and Peter talked about them. I went to the seminar. I didn't understand a whole lot, but I got some of the basics because I'd learned some basic welfare economics from Chris Archibald.

So I had learned about Arrow's Theorem and social choice from David and went to my first research seminar, and it turned out to be on optimal commodity taxation. Also, David Donaldson had me read John Rawls' first paper (Rawls 1958) where he started working out his views on original positions and a theory of justice. All that was as an undergraduate.

CdA: Wow, that's impressive! And did the decision to go to Penn for your Ph.D. come later, or did you decide immediately to do a Ph.D.?

JW: Well, I was very academically inclined. I don't know at what point I decided to actually do a Ph.D., but it was pretty early on as an undergraduate. The choice was partly dictated by who would give me money to go.

FB: Was there a well-defined alternative to being an academic?

JW: Not really. I hadn't really thought about it. When it came to deciding to go to grad school, my two best choices were going to Northwestern or to Penn. I was admitted with some support at other places. I wrote an honors paper as an undergraduate with Terry Wales. Terry had been on the faculty at Penn and was able to give me lots of good advice. He was influential in getting me support that didn't have any service requirements. My initial offer was to be a research assistant. I think that was the final deciding factor. It was a good choice.

FB: In terms of your family background, was this an expected path for somebody having your background—becoming an academic—or was it a surprise to your parents that you chose this path?

JW: I don't think it was a surprise to my parents because I was very bookish and liked math and sciences. None of them went to university. I'm the first generation to do that. My father, who was the youngest of five, was the only one of his siblings that didn't have post-secondary education—he would have, if it hadn't been for the war. His father was a stonemason; I don't even think he went to high school. My parents went to high school, but that was it.

CdA: You completed your thesis in 1977 under Karl Shell's supervision, *Essays in Public Economics*. Were parts of your thesis published?

JW: Yes. I left Penn in '76. There was just a tiny bit of my thesis left to do. I finished it in the first couple of weeks after I left, but I wasn't able to schedule a time when all my examiners could meet until the following spring. That's why the degree is '77.

My thesis had two parts. I initially started working on congestible goods, goods that are intermediate between private and public. I had a way of modeling them as a technological relationship where you have some inputs, and the outputs are how much of this good each person gets. In the case of pure private goods, one unit more for one person is one unit less for another, so it's a kind of sharing technology. If it's a pure public good, there is a fixed maximum consumption for each person possible with given inputs, with free disposal permitted. It's like having a production surface that is a reverse Leontief. There is a ray, and the frontier is kinked at that ray pointing downwards. You could also have convex technologies in between the polar cases.

There were two essays on that. One of them was looking at optimality conditions, welfare optimality. In one polar case, these conditions reduced to the public goods Samuelson rule; at the other extreme, they are the standard conditions for private goods. The second chapter provided a general equilibrium analysis of this problem. I developed a concept of equilibrium which at one extreme is Walrasian and at the other extreme is Lindahl.

At Penn, there was a theory topics course every semester and Karl Shell taught it one term. In that course, there was a mix of topics that were kind of hot at the time that he was interested in. We studied optimal commodity taxation and modeling transaction costs in general equilibrium because Karl was working with Walt Heller on trying to pull these two issues together (Heller and Shell 1974). I started thinking about commodity tax problems as a result of that. The other half of my thesis dealt with tax issues.

There is the famous Diamond–Mirrlees Production Efficiency Theorem (Diamond and Mirrlees 1971). Peter Diamond and Jim Mirrlees had sufficient conditions on properties of the excess demand functions that would guarantee that it was optimal in their environment to have aggregate production efficiency. One of my chapters developed a necessary and sufficient condition for that. It was also related to some work that Frank Hahn (Hahn 1973) had done. The final chapter was basically a note. There was an obscure paper by Diamond and Mirrlees (1976) dealing with shadow pricing, and I extended it to get some partial results when you have a non-convex technology.

The material in the first half of the thesis had been a really active area of research, but it kind of died at the time I finished my thesis. I extracted a short note out of it (Weymark 1979a) and just put the rest of this part aside. The chapter with the necessary and sufficient conditions for the efficiency theorem I published in the *Journal of Economic Theory* (JET) (Weymark 1978a). I didn't do anything with three of chapters initially, but over the years I've ended up publishing two of them. People had heard about my thesis—local public goods and congestible goods came back in vogue. I got invited to a conference and eventually published the chapter on optimality conditions in its proceedings (Weymark 2004). I have still never published the general equilibrium one, although it gets cited sometimes. I published the shadow pricing chapter in a festschrift (Weymark 2005b).

Let me say one more thing about my graduate education. My research career has been very interdisciplinary. This was a period—I think primarily because of the influence of Rawls' *A Theory of Justice* (Rawls 1971), which came out just before I went to grad school—when economists got excited about Rawls' work. There were lots of interdisciplinary courses, and I took advantage of them. I took a course that was actually run out of the law school jointly by Bob Pollak and Bruce Ackerman, who is a legal scholar. We read parts of *A Theory of Justice* and considered other issues related to that, but once a week Bob had an extra session for the economists. We worked through Sen's collective choice book (Sen 1970), which was only a couple of years old at that time. I also went to the philosophy department and took a course in decision theory from Dick Jeffrey. I actually did that before I did the course I just mentioned. That was my first introduction to John Harsanyi and his work on decision theory.

CdA: That's an impressive variety of things that you did. Did you do that by yourself? Did your supervisor suggest that you do these things, or was that your own choice?

JW: We didn't really have a supervisor until we got to the thesis stage. It was my choice. When I went to the philosophy department to take this course on decision theory, Dick Jeffrey was kind of taken aback. He had taught it for years and said, "You're the first economist who's come over and wanted to take this course." He didn't get many students.

There is something we may come back to later. I wrote a term paper in either that course or the other one I mentioned where I tried to make sense of Harsanyi (1955). Harsanyi is very imprecise in formulating his decision theoretic arguments for utilitarianism. There are gaps in his proof. He has an implicit assumption that he uses in the proof which is not in the theorem statement. The first person to actually prove Harsanyi's theorems precisely is a philosopher of science named Zoltan Domotor. I never met him, but he was at Penn too. When we were talking about Harsanyi's work in Dick Jeffrey's course, Dick told me that his colleague had a manuscript (Domotor 1979). It was a handwritten manuscript, and Dick gave it to us. It uses functional analysis, and at the time I could hardly read it. It turned out to be something that was influential later on when I finally understood what was in it. My first attempts at trying to make sense of Harsanyi's work as a graduate student weren't completely successful, honestly.

FB: Did your supervisor, Karl Shell, in the end have only a minor role, in the sense that he just came in at the very end? How was your relation with him, and how were your relations with other colleagues you met at the time?

JW: The normal way that the graduate programs in the US worked, and still work now, is that you spend a couple of years doing course work first. Karl was on leave my first year, so I didn't meet him until the second year, but there was a personal connection. Karl and David Donaldson were housemates in graduate school and very good friends, and it's actually because of David that Karl met his wife. So there were these close personal connections.

I got to know Karl when he came back. That was the semester that he did this course on optimal commodity taxation, transaction costs in general equilibrium, and non-convexities in general equilibrium. It was a mix of topics. But faculty weren't giving advice on what courses to take. I'd taken a course on public economics with Bob Inman. Bob had devoted part of his thesis (Inman 1971) to issues related to impure public goods, and that's what got me started thinking about that. As a result of Karl's course, I'd started thinking about commodity taxes, so he was a natural person to want to work with. Bob Inman was on my committee as well. I had a two-person committee and they were both very accessible.

FB: Let's move on. The next step was that you went as an Assistant Professor to Duke University. What is your memory of that time? There are various aspects that are interesting. What did you work on, but also, how did you handle the pressure of being an Assistant Professor and having to qualify for tenure?

JW: I actually didn't feel pressure. I was probably being very naïve because Duke at the time actually had a terrible reputation. They had gone years without giving tenure to anybody. I just did what I could. When I went out on the market, my thesis—although it was theoretical—was all public economics, and that's how I presented myself. And that's essentially what Duke hired me for.

But I was also attracted by the fact that there was an active group of people working in micro theory, including Roy Weintraub and Dan Graham. Wes Magat in the business school was working on mechanism design issues. Dan was away the first year, and he normally taught the first graduate micro course. They needed someone to teach that, so I got put into it. Then the next year he came back and took his course back, but Roy was on leave, so I taught his course. That was the whole graduate micro sequence. In retrospect, this was one of the best things that could have happened to me because you just learn material so much better by teaching it. I think a lot of the precision that's in my papers came from having to be careful teaching. I never ended up actually teaching public economics when I was at Duke.

We had a nice reading group that first year. This was a period where people were trying to integrate money into general equilibrium models, people like Jean-Michel Grandmont, Jean-Pascal Benassy, Yves Younès, and so on. Roy Weintraub ended up writing a book on micro-foundations of macro based on this literature (Weintraub

1979), and then he drifted off into the history of modern economic thought. Dan worked on other topics, so there was nobody working on theory anymore.

I was on the faculty at Duke for five years, but I only actually spent three of them there. At the end of my second year, I had an opportunity to go to UBC as a visitor for a year, which I did. One of the things I worked on in that period was tax reform problems. I was looking at a particular kind of dynamic process, and it was very close to some work that Henry Tulkens at CORE was doing in a different context (Tulkens and Zamir 1979). I sent him the paper (Weymark 1981b) and he liked it, and he asked me whether I would consider coming to CORE for a postdoc. The people at Duke were very gracious. They let me have a second year's leave to do that, and then I went back to Duke. But I got spoiled because UBC was so active in theory, and I developed good working relationships with David Donaldson and Chuck Blackorby during that year. CORE was also active. I had already started working on social choice and interpersonal comparisons before going to CORE. Of course, there is Claude d'Aspremont's paper with Louis Gevers (d'Aspremont and Gevers 1977)—it was a great joy to go to CORE and have them as colleagues.

To answer the other part of your question on what I was working on—well, when I went to Duke, I quickly realized that the research on congestible public goods wasn't going to go very far at that stage, given how the direction of the field had changed. So, at the time I only drew a short note out of it (Weymark 1979a) and sent the paper related to the Diamond–Mirrlees efficiency theorem to JET (Weymark 1978a). The condition introduced in it ended up being used a lot in international trade theory for studying tariff policy. It's actually called the Weymark Condition, although it is picked up from a later paper of mine (Weymark 1979b), not that one.

It was at that point that I started working on inequality measurement. Sen's lectures, *On Economic Inequality* (Sen 1973), came out the year I was taking the course with Bob Pollak and Bruce Ackerman. It was available in England before it was published in North America. One of my classmates was from England, so when he went home for Christmas, he had an order from half a dozen of us to bring the book back. We read it, and it was very influential. This was before I went back to UBC as a visitor, but I was reading the research that Blackorby and Donaldson were doing. There is some nice geometry underlying Gini indices, and they generalized it to look at the normative foundations for different inequality indices (Blackorby and Donaldson 1978). I was very influenced by that. I also started learning a bit about formal measurement theory, particularly the book by David Krantz, Duncan Luce, Patrick Suppes, and Amos Tversky, *Foundations of Measurement*—the first volume (Krantz et al. 1971), which was the only one out at that time. I started seeing that the kind of structures they were looking at were what you needed for an axiomatic analysis of inequality measurement.

CdA: Let me interrupt you. When you came to CORE in '79, Jean Gabszewicz was Research Director, and we worked with Alexis Jacquemin on this cartel stability paper (d'Aspremont et al. 1983). This was an IO paper, although the concept is larger than just for IO applications. You had another paper that was said to be in IO, on concentration indices, with Chuck and David. How do you view this work now in retrospect?

JW: OK, let me answer this question first. That paper actually grew out of a project that I started in those first two years at Duke, when I started thinking about axiomatic approaches to inequality and their welfare foundations. There are many formulas for the Gini index. In the JET article that was published in the late seventies by Blackorby and Donaldson (1978), when they worked out the welfare function underlying it, it turned out that a very convenient representation was the following. We rank order incomes from highest to lowest in a discrete distribution, where the weights are one on the highest income, three on the next, five on the next, and so on. Then to normalize, we divide by the number of people squared because then the weights add up to one—the sum of the first n odd numbers is n^2. And I thought that this nice linear structure would give you some nice properties. I generalized this structure by instead of giving a weight of one to the highest income, three to the next highest income, and so on as you go down the income distribution, by just having the weights decrease monotonically, what I call the Generalized Ginis indices. I then asked, "What would be an axiomatic characterization of the Generalized Ginis?" And that's where I was using the foundations of measurement. I had that pretty much worked out before I went to UBC.

CdA: This was your 1981 paper?

JW: Yes, the one that got published in *Mathematical Social Sciences* (Weymark 1981a).

CdA: So, you started working on that before?

JW: Before going to CORE—it wasn't published until afterwards. The paper that went to JET (Weymark 1978a) out of my thesis got me to start working on a paper on tax reform while I was at Duke, which I finished off at UBC. In this paper (Weymark 1979b), I have a reconciliation of recent results on optimal commodity taxation. There were some results that Diewert (1979), Dixit (1979), and Guesnerie (1977) had, among others, and it wasn't clear how they were connected. They seemed to be inconsistent. I worked out what was involved and why they were getting different results. I was doing that at the same time as my work on inequality, but I put priority on getting the tax stuff written up.

When I went to UBC, of course, I had both Blackorby and Donaldson as colleagues. David was actually on leave that year. He was renting a house on an island between Vancouver and Vancouver Island and coming back once a week. I rented his house. The arrangement was that he kept a room in the house and he could stay there. During his trips back I told him about this work that I hadn't written up yet, the axiomatic characterization of Generalized Ginis. I told him that I didn't have the principle of population as one of my axioms, but that it would be nice to see what is the class of indices that satisfy the principle of population in addition to the axioms I had used to characterize the Generalized Ginis. We didn't have a clue what the answer was. I went to visit him on this island for a weekend. We had no resources, just the problem and how to proceed.

Some time before that, I think before I went to Duke, I started learning some basics of functional equations. Blackorby together with Dan Primont and Bob Russell had written a book on duality, separability, and functional structure (Blackorby et al. 1978), and they used these tools a lot. They looked pretty interesting to me. Dan Primont had directed to me to the work of János Aczél on functional equations (Aczél 1966). It was pretty simple to pick up the basics. It turned out this was the tool we needed.

After doing a bunch of manipulations, we ended up with a functional equation that is an example of what's called a Cauchy Equation. It's the equation on the real line: $f(xy) = f(x)f(y)$. But the twist with our problem was to solve it when the domain is the non-negative integers. We knew that $f(x) = x^r$ is the solution on the non-negative line provided that you have some continuity or monotonicity. Initially, we didn't have a clue what to do with this integer domain.

We realized—David was really the one that had the key insight—that you need to go back to Aczél's classic book (Aczél 1966) and look at the solutions for the various Cauchy equations like $f(x + y) = f(x) + f(y)$ that don't impose a regularity condition like monotonicity or continuity. You can build up the real line from what's called a Hamel basis. It's like spanning the space. You get to specify what the value of the function is on each element of this basis, and everything follows from that. There's a unique way to express the numbers in terms of this Hamel basis, and David realized that something was going to be analogous on the integers; it's the fact that you can factor any integer into a product of primes. And so without any regularity conditions, we could specify what the value of the function is on each prime number.

But our problem actually had some regularity and continuity. Once you put that in, it just becomes the same solution as the case with the non-negative integers. When we undid all the transformations, we obtained the class of what we called the Single-Parameter Ginis. For one parameter value, which in this case is two, we have the regular Gini. Our class provides a way of moving between having no concern for inequality in the utilitarian case up to maxi-min. We had no clue that that was going to be the answer when we started working on this problem.

We obtained all the results for that paper in that weekend. I've never been so productive in my life. We wrote the paper up within the next two or three weeks. It helped that David wasn't teaching. It was one of the best experiences in journal submission I've ever had—the paper (Donaldson and Weymark 1980) was accepted with no revisions within a few weeks. We had another result though, which we added in. That's never happened since then. So, it was a very gratifying experience.

To connect back to Claude's question about IO—there were a lot of indices floating around measuring concentration, and a few people had tried to look at welfare foundations for them. There's something called the "Numbers Equivalent" way of measuring concentration that sounds a lot like the equally distributed income in inequality measurement. That's what led to this paper on concentration indices (Blackorby et al. 1982). At the time, the *European Economic Review*—this was when it was still associated with the European Economic Association—had a special issue on topics related to IO once a year, of which Claude was one of the guest editors. This material was finished up while I was at CORE. Because of having Claude as a colleague and co-author, we thought that was the place to send this paper—that's how it ended up there.

The other paper that Claude mentioned was on cartel stability (d'Aspremont et al. 1983). CORE had this very interesting system for discussion papers that I quite liked. Before a paper could go into the discussion paper series, it had to be refereed internally. One of the ways that CORE tried to integrate newcomers, visitors, and postdocs was to give them one of these papers to referee and comment on soon after they arrived. I'd never worked in industrial organization—never taken a course in it—before being asked to review a cartel stability paper that Claude had written together with Jean Gabszewicz and Alexis Jacquemin. They used what's called the price leadership model: there's a dominant cartel that sets the price and the other firms behave competitively relative to it. They were working with a continuum of firms and formalized some intuition of George Stigler's that cartels wouldn't be stable (Stigler 1964), that there were always incentives for the cartel to break up. It was a fairly short note, so it didn't take me long to review it.

I was intrigued and I thought that a lot is special because of the fact that they were working in a continuum—what would happen if you worked with a finite number of firms? I don't remember how explicit the concept of stability was in the working paper that I had reviewed, but when you went to the discrete case you had to be really precise. And the relevant concept of stability, I thought, was that the cartel was stable if someone leaving the cartel is worse off as a result when they take into account the effect that the departure is going to have on the price and, vice versa, nobody wants to join the cartel if after they do the prices change in a way that is adverse. It didn't take long to figure out that with that concept of stability in the finite case, things don't unravel like they do in the continuum—there always is a stable cartel. In the end, we combined our research, and that became the article. Remarkably, in retrospect, nobody wanted to publish it—we had it turned down two or three times.

After that, I went back to Duke for a year. Then I got hired by UBC on a permanent basis. The article was still floating around then. One of my colleagues was editing the *Canadian Journal of Economics* (CJE), so we sent it there and they accepted it. But they didn't want the appendix. We had a nice example in the appendix. Eventually, it got incorporated in another paper by Claude and Jean (d'Aspremont and Gabszewicz 1986). I'm really proud of the work that Claude, Alexis, Jean, and I did.

That joint paper and the Generalized Gini paper of mine are my two most highly cited papers. Both of them ended up being picked up by other literatures. The cartel stability paper gets cited so much because environmental economists think about it. They study how to make environmental agreements between countries, and they need some concept of stability. They use the concept of stability that was introduced in that article. In retrospect, it would have been better to exposit our paper in terms of a multi-stage game, which we didn't do.

At some point, Peter Wakker came across my Generalized Gini paper and realized that it was formally equivalent to what John Quiggin had done on decision-making under uncertainty (Quiggin 1982). At that time, Quiggin called it "anticipated utility." It later became known as "rank-dependent expected utility." I didn't use these terms, but I had an independence axiom that is analogous to the co-monotonic independence axiom in decision theory, which played a key role in my axiomatization. There are a lot of formal similarities between inequality measurement and decision-making under uncertainty. If you think about the work by Tony Atkinson on measuring inequality and stochastic dominance (Atkinson 1970), it is related to Michael Rothschild and Joseph Stiglitz's paper on the comparative analysis of risk (Rothschild and Stiglitz 1970). They both appeal to the same stochastic dominance results. So largely because of Peter, who prepares annotated lists of papers relevant for decision theory and circulates them, people learned about my Generalized Gini paper. Then it started getting cited and read a lot.

CdA: Later on, you worked on multidimensional inequality, then you came back to the Gini indices. What is fascinating with the Gini index is that the social evaluation function can lead either to relative or to absolute indices.

JW: That's right. Because the iso-welfare curves are linear within a rank-ordered set, you can easily move between the relative and absolute perspectives. One thing that has characterized my research career is that I cycle back to topics. I'll work on something for a while and think, "OK, I don't have any more good ideas about that so I'll put it aside and work on something else." Then a few years later, I might come across something that sparks my interest. For example, when research on multidimensional inequality started taking off, I came back to inequality measurement (Gajdos and Weymark 2005).

CdA: Now let's switch to something else. You insist on the fact that inequality indices should be normatively founded. In some sense, the idea of introducing interpersonal utility comparisons seems to be a natural follow-up of this assumption. So you went in this direction. First, there was your diagrammatic introduction (Blackorby et al. 1984), which was quite successful, I would say. There was also a publication in *Scientific American* that used it (Blair and Pollak 1983b).

JW: Yes.

CdA: Then you went on with your paper on the proof of Arrow's Theorem, (Blackorby et al. 1990) and your survey chapter with Walter Bossert (Bossert and Weymark 2004). Recently, you also had a paper with Michael Morreau on welfarism, introducing different individual scales to measure well-being (Morreau and Weymark 2016). So, this has been a long line of research.

JW: It actually ties in with my work on Harsanyi as well. Trying to sort out what was going on with interpersonal utility comparisons in his work really goes back to my grad school days. But it was when I worked with Blackorby and Donaldson on diagrammatic social choice that I started publishing on this issue. In this work, we were interested in employing a social welfare functional approach to characterize different kinds of social objective functions in order to move away from the nihilism of Arrow's ordinal non-comparable framework. There was a period in the latter part of the seventies when Blackorby and Donaldson were amazingly prolific in generating a lot of great papers. They have a paper in the *Journal of Public Economics* (Blackorby and Donaldson 1977) where they are looking at quasi-orderings in a social context, and that led them to start thinking about this literature on social choice and interpersonal comparisons that was using Sen's framework (Sen 1974). This was the period when this work was done. What year did your paper with Louis Gevers come out?

CdA: It came out in '77. I started working on it in '75, I would say.

JW: In '77 the paper (d'Aspremont and Gevers 1977) came out, so Chuck and David didn't know it before then. But that and work by Peter Hammond (Hammond 1976) and Amartya Sen (Sen 1974) were really some of the most significant work being done in social choice theory, and very congenial to the kinds of interests we all had. David and Chuck had written one paper (Blackorby and Donaldson 1977) which is closely related to the Suppes grading principle (Suppes 1966); it involved ordinal comparisons, but they could be incomplete. David gave a seminar based on some of the work he and Chuck were doing. When he talked about social welfare functionals with ordinal level comparability, he provided a little diagrammatic illustration in the two-person case to provide some intuition. After the seminar, we started talking about it, and I suggested to David that this idea must be applicable to different assumptions about measurability and comparability of utility. So together with Chuck, we sat down and worked it out. This was towards the end of my time as a visitor at UBC, before I went to CORE. We actually finished and wrote our paper (Blackorby et al. 1984) while I was at CORE. Chuck came for a brief visit to CORE. He was at Nuffield College for part of that year.

I can't overstate how important the year at CORE was for my career. At CORE, I got to explain my research and get feedback from Claude and Louis Gevers. It was the first time I'd ever been to Europe. Both with this kind of work and the work I was doing on taxation theory, Europe was where all the action was. As a graduate student, I read a lot of the work that Roger Guesnerie was doing. It was very influential, but his work is very tough, very technical for what people were doing in that field. I have very geometric insights, and there is a lot of geometry underneath Roger's work on taxation theory. So, I invested a lot of time in learning it.

CdA: You met Roger Guesnerie during that period?

JW: Yes. I had been out of grad school for three years before going to CORE and I was just starting to get publications. Through the connections of people who were very kind to me in arranging introductions, I received a number of seminar invitations that year that proved to be very important. One was a short trip to England, where I gave a talk at Warwick. At the time Avinash Dixit was one of the faculty there who was working on optimal tax, as were Nick Stern and Jesús Seade. The next stop was Nuffield College and Jim Mirrlees' famous seminar—he invited me. That was when Amartya Sen was at Nuffield. It was the first time I met him. Amartya, then as now, is a very, very busy person, but he is one of those scholars who is very encouraging and supportive of younger people, as is Jim Mirrlees. I had a fifteen or twenty minute appointment to speak with Amartya, and I went to explain to him the diagrammatic social choice paper. He ended up giving me forty-five minutes to an hour and has been supportive ever since. Mirrlees was the same way. He was tremendously supportive.

Then later in that year, I got the opportunity to go to Paris at Roger Guesnerie's invitation to give a talk. That's the first time I got to know him. I presented some work I was doing on taxation. When we went to dinner, it turned out that Nick Stern was in town, so it was the three of us. There's a famous symposium in the *Journal of Public Economics* on optimal income tax with discrete types. Nick has a paper (Stern 1982), and Roger has a paper with Jesús Seade (Guesnerie and Seade 1982) which has been very influential. I was hearing about this work in progress during the dinner. The following year I was back at Duke and Roger was visiting at Penn that year, so I arranged for him to come and give a talk, and he gave that paper. I studied it very carefully, and I've used it a lot.

There's one other seminar I gave that year which turned out to be very influential, and that was Martin Hellwig's seminar in Bonn. Martin was a professor at Bonn at the time. That was the first time I met Martin, whose work in taxation has been very influential to me. Much later, during Martin's time at the Max Planck Institute in Bonn, I visited a number of times.

FB: Had he been working on taxation already at that time?

JW: No.

FB: The earliest piece I know is from '86 (Hellwig 1986), I think.

JW: Initially, it was just the case that he was kind enough to invite me, which is how I got to know him. He's been supportive over the years. I've been fortunate that there were many senior people in the profession who were very kind and supportive when I was starting up, and that has had a big influence on how I deal with people. I try and be helpful and supportive with junior scholars in the profession.

FB: I have maybe a more personal question. When I was preparing this interview, I asked a couple of colleagues what they would ask you. One question I got from François Maniquet was whether you are a welfarist. Maybe, we'll just start by defining what it is—how would you define a welfarist? Then it comes time to confess whether you are one or not.

JW: Well, welfarism is a form of consequentialism. A consequentialist's view is that if you're deciding how to rank different actions or different options, you only pay attention to the consequences. You don't care about the procedure, you don't think about rights, and so on. Welfarism is a particular form of consequentialism in which all you care about are welfare consequences, that is, the utilities that result. If you're ranking different alternatives, the only thing we care about are the utilities that are generated by those alternatives, not whether they respect people's rights or anything else like that.

I'm not a welfarist, but I find it very useful to understand its foundations. I've given you a general definition. In the context of social choice and welfare economics, you want to know what in the nature of social rankings and social welfare functions makes them welfarist. Certainly in the literature we talked about earlier on social choice and interpersonal comparisons, there were characterizations of welfarism in a multi-profile context. When Amartya Sen got an honorary degree from the Université catholique de Louvain, they had a special issue of the *Recherches Économiques de Louvain* in his honor, and Blackorby, Donaldson and I published a paper in it (Blackorby et al. 1990). The main part of the paper built on the diagrammatic paper (Blackorby et al. 1984) that we talked about earlier, which just used two people to show the basic geometry. We used that geometry to build an n-person, geometrically based proof of Arrow's Theorem. But there was actually a bit more to it than that. We showed exactly what was needed if you have just a single profile of preferences, what this kind of welfarism requires. It turns out that it is equivalent to Pareto indifference.

I've realized that I didn't answer something Claude asked about the *Scientific American* piece (Blair and Pollak 1983b). As I mentioned earlier, Bob Pollak taught me social choice as a graduate student. He also worked on social choice theory. Bob was editor of the *International Economic Review* (IER). We sent the diagrammatic paper someplace else and they weren't interested; we didn't know what to do next. Pollak said, "Send it to the IER." He was very sympathetic to our paper. He really liked it, so he studied it carefully.

Doug Blair became a faculty member at Penn just after I left, so I didn't know him until a little while later. Bob and Doug started doing a lot of research on social choice theory in which you relax the collective rationality requirement and don't demand social orderings. They were particularly interested in looking at acyclic choice (Blair and Pollak 1982). When you look at acyclic social choice, the kind of rules that satisfy a set of axioms depends very much on the number of people versus the number of alternatives. It becomes very difficult technically to get complete solutions. They worked on this problem and had to develop some new theorems on colorings and graphs to solve it (Blair and Pollak 1983a). In the process, they realized that our diagrammatic approach could give some insights to more than just the case where you have social orderings. As a result, they decided to write up a piece for a broad audience, which was published in *Scientific American* (Blair and Pollak 1983b). So, our diagrammatic approach went public.

In fact it was very nice that they did this because their article came out when I was a permanent faculty member at UBC. One of the nice features of UBC at that time—it's since disappeared—was a faculty club where people go to lunch. They had a cafeteria and you would see people from other departments. I had taken a course in differential equations as an undergraduate from Z. A. Melzak. He was a wonderful person and a great teacher. And he'd seen it. He came in one day and said, "I saw your work being discussed." He was proud that one of his students had his work discussed in *Scientific American*. That piece was probably the one thing that disseminated my research more than anything else.

CdA: You said that your interest in interpersonal comparisons of utility came out of your reading of Harsanyi. You said also that you have pursued subjects, year after year, abandoning and coming back to them. One area where you have been very active is the famous question of John Harsanyi's defense of utilitarianism (Harsanyi 1955) and Sen's criticism (Sen 1976). What is your opinion on this now? You mentioned that measurement theory was already important also in your seminal paper from

1981 (Weymark 1981a). On Harsanyi, you also have a recent piece which introduces measurement theory (Weymark 2005a). So what is your opinion now on this debate?

JW: Well . . .

CdA: Now, or you can say what was your opinion in the past also. [All laugh.]

JW: It hasn't changed a whole lot. As I said, I tried to make sense of Harsanyi's statements. His aggregation theorem, roughly speaking, says that given a set of lotteries to be socially ranked, if individuals have preferences over them that satisfy the expected utility axioms, the social preferences also satisfy these axioms, and the social preferences are connected to the individual preferences by a Pareto principle like Pareto indifference, then if these preferences are represented by von Neumann–Morgenstern utility functions (von Neumann and Morgenstern 1944), the social utility function is an affine combination of the individual von Neumann–Morgenstern functions. So, basically, social welfare has a weighted utilitarian form.

With Harsanyi's impartial observer theorem—using Rawlsian terminology—you imagine yourself behind a veil of ignorance in which there is an equal chance of you being anybody. This is a problem of choice under uncertainty. Harsanyi (1955) argues that if you think that you have this equal chance, you're going to end up with utilitarianism. But the arguments are all pretty loose.

In Harsanyi's early work, like the'55 paper, he also considers a different argument in support of utilitarianism due to Marcus Fleming (Fleming 1952) that is based on the separability of a social ordering over vectors of utilities. So, Harsanyi has what he thought were three different decision-theoretic arguments for supporting utilitarianism, or at least weighted utilitarianism.

The year I was at UBC as a visitor, I got Blackorby and Donaldson interested in looking at this problem and we wrote a working paper. It first came out at UBC. Then Claude was our referee when I wanted to put it out as a CORE discussion paper (Blackorby et al. 1980) when I was I postdoc there. The point of that paper was to argue that the social rankings sort of look utilitarianan, but they really aren't. Moreover, in each of these three models, you might get a different ordering of the alternatives, so how can they all support utilitarianism?

It wasn't a very well written paper. We used a lot of functional equations—Pexider equations. We asserted something without actually proving it. It turned out that the relevant mathematical theorem from the functional equations literature for the domain we were using wasn't yet known. I think that in terms of the way we presented our results, we made a mistake. Instead of using the lottery model that Harsanyi had used for uncertainty, we used a state contingent alternatives model of uncertainty, but with objective probabilities. It was a model that Arrow used in the early 1950s (Arrow 1964). We did that because Blackorby and Donaldson (Blackorby et al. 1977) were working with that model.

We sent our paper to *Econometrica*. Harsanyi was obviously one of the referees, and he clearly didn't understand what we did. We had it rejected, and we talked about how to proceed. Bob Pollak was the editor of the IER, and we talked to him. He said: "Send it to me, but revise it, take the reviews into account." But we actually never did that. It's something I regret.

CdA: It was never published?

JW: It was never published. There were parts of it that were published (Blackorby et al. 1999). What we were focusing on at that time was that these three different arguments didn't actually produce the same social ordering. How in the world can it be an argument for utilitarianism when you're getting different orders? We had pretty much, but not precisely, identified what the real issue was. The argument that was used to show this was related to the expected utility representation theorem. It didn't really matter if we used state contingent alternatives or lotteries, as von Neumann and Morgenstern had done. If you use lotteries, the axioms are about binary relations over a set of lotteries: there are ordering, continuity and independence axioms. The representation theorem says that we can represent these preferences with a von Neumann–Morgenstern utility function, so it's linear in the probabilities. It doesn't say that you have to—any increasing transform is a perfectly acceptable representation. I think this is something that Harsanyi never recognized—the difference between "can represent" and "must represent."

When you get outside of normative contexts like welfare economics and social choice, it doesn't matter which representation you use. You just pick one for convenience; the von Neumann–Morgentern one is a nice representation to work with. But when, in the normative context, it is to be used somehow as the basis for interpersonal comparisons, the choice of representation matters a lot.

The basic idea is that you can take nonlinear transforms of the utilities and still have a representation of the preferences. Essentially, in the '80 paper (Blackorby et al. 1980), we had specified exogenously that there were welfare relevant measures of utility, and that what each of these three different approaches was doing was to take different nonlinear transforms of them before summing across individuals. That is why you were getting different outcomes: with the impartial observer model, with Harsanyi's aggregation theorem, and with Fleming's separability approach.

In retrospect, we realized that we had the first proof of a version of Harsanyi's aggregation theorem for a different model of uncertainty. Eventually, we cleaned that result up. As I said, there was a little gap. We had asserted what the solution to a functional equation is, but on a non-standard domain. At some point, Walter Bossert read our paper, and he knew of a paper by Radó and Baker (Rádo and Baker 1987) that had come out in the mid-eighties, a few years after we wrote this draft, that actually solved this Pexider equation on the relevant domain. So, we could appeal to it. Eventually in the '90s, we pulled that part out and it became a *Journal of Mathematical Economics* paper (Blackorby et al. 1999) proving Harsanyi's aggregation theorem in that framework.

But, in the '80 paper, we didn't discuss at all the debate between Sen (1976) and Harsanyi (1975) on whether Harsanyi had actually proven anything about utilitarianism. Amartya clearly understood that there were issues related to the choice of representation, and he has a very important paper in *Theory and Decision* (Sen 1976) where he makes this point. Amartya is usually one of the most clear writers that you encounter, but that paper is not clear. He expressed his argument in the form of a Socratic dialogue. Unless you have actually figured it out on your own, you don't understand it. There was a back and forth between him and Harsanyi (Harsanyi 1977; Sen 1977), and it was clear that Harsanyi never saw the point of what Amartya did. I realized that the paper I had done with Chuck and David was what you needed to really address this.

There was a conference in Berlin in the mid- to latter part of the '80s where I made a first stab at sorting this issue out, but I hadn't actually written a paper yet. That eventually became the "Reconsideration of the Harsanyi–Sen Debate" paper which was published in the Elster–Roemer volume (Weymark 1991). John Roemer and Jon Elster held a conference on interpersonal comparisons of utility. At the time, John Roemer was at Davis, where they held their conference. Chuck and David were invited to present a paper, and I was invited to be a discussant. What John and Jon neglected to tell me was that this was the first of a two-part conference. There was going to be a conference at the University of Chicago where Elster was at the time; I think it was one year later. The people who were discussants the first time would give papers the second time. So, I was supposed to produce a paper. I had been making an attempt at working on the Harsanyi–Sen debate after the Berlin conference, so I thought I would write this work up because it was now starting to come together. I asked Chuck and David to work on it, the three of us, because it was really building on what we had been doing. They said they already had a paper for this volume, so I should just do it.

I regret that they are not co-authors on this paper. The one thing thing that I don't think is clear enough in my paper in the Elster–Roemer volume is how much is based on our joint research. I do say that it is heavily influenced by my work with Chuck and David, but that's an understatement. The key idea that you can take different nonlinear transforms of the welfare relevant utilities is in our unpublished paper (Blackorby et al. 1980). And I'm applying that idea, and specifically looking at the Harsanyi–Sen debate in the first part of my paper. But I had a tight deadline to finish the conference paper. It's a long paper, and I really should have split it into two. The first part was to trying to formally explain what Harsanyi actually has shown, and to show that you really shouldn't be interpreting this as theorems about utilitarianism, which is what Sen's point was (Sen 1976). Sen said that Harsanyi just had representation theorems which are not really about utilitarianism because the utilities you should use should be the welfare relevant ones, and there's no reason to believe that they are in Harsanyi's theorems.

Let me go back to Harsanyi's '55 paper (Harsanyi 1955), which is where he has both the aggregation theorem and an impartial observer theorem. In his '53 paper (Harsanyi 1953a), Harsanyi has the basic idea of the impartial observer theorem, but it is for a very special case. He also talks about Fleming's paper (Fleming 1952). But in the latter part of the'55 paper, he talks about the basis for making interpersonal comparisons. Essentially, what he is saying is that there is some kind of master function for making them. In his terminology, the satisfaction you get from an alternative is a function of all the variables—he calls them causal variables—that affect how much satisfaction you get from the alternatives, with the same function used for everybody. If you know the value of these causal variables, and you apply it for one person, then for another person, you can use this master function to make interpersonal comparisons. It builds on an earlier paper that is not well known, "Welfare Economics of Variable Tastes" (Harsanyi 1953b), in which Harsanyi is trying to do welfare economics when someone's tastes change. He had the idea that because it's the same person, you can put yourself in the place of your other self and make a comparison. Underlying some of this research is also the whole idea of empathetic preferences; it's all mixed in.

In the latter part of Harsanyi's '55 paper, he goes beyond the kind of ordinality you have with the von Neumann–Morgenstern axioms. The latter part of my Elster–Roemer paper was trying to build a case for a utilitarian theorem using the kind of analysis that Harsanyi employed, but without restricting attention just to the preferences. Rather, I used the welfare relevant utility functions, which might not be perfectly measurable. But I was rushed because of the deadline, so that part of my paper is not developed well enough. Most people don't even read that part, I think. It didn't get picked up on for a long time. I think I should have put it aside and done it later.

John Broome has been very critical of Harsanyi's work that uses this kind of basis for making interpersonal comparisons (Broome 1993). He is also critical of the later related work of Serge Kolm (Kolm 1998). I think that Broome's criticism of Harsanyi is off the mark. As far as Kolm is concerned, I think that he's probably right. As I said, Harsanyi was talking about some cardinal measure of well-being—he called it satisfaction. Broome says if we use this causal variable kind of approach to talk about interpersonal comparisons of preferences, it is problematic. I think that his arguments are right. But once you talk about interpersonal comparisons of satisfactions in a cardinal framework, they don't apply. Kolm used an ordinal framework. I've never really followed that up very much, but I think there's something there.

As I said, when I wrote that paper, I was very close to the deadline. The people who were at the second conference didn't have a whole lot of time to prepare their papers, plus mine was long. I sent it to Harsanyi. I had not met him at that time. He wrote back a very nice letter. One of the things that I found very interesting was that he has an argument in his letter justifying interpersonal comparisons that he never put in any of his published work. He builds up the interpersonal comparisons by comparing the gain going from a to b versus c to d—difference comparisons. You can build up a cardinal theory from that, but it's not in his work. I think that the most successful material that he actually wrote on interpersonal comparisons was the last part of his'55 *Journal of Political Economy* (JPE) article (Harsanyi 1955), where he has his causal variable approach.

CdA: That's going in the direction of using measurement theory.

JW: That's right. You asked about my other article on measurement theory. Once I wrote my paper on Harsanyi for the Elster–Roemer volume (Weymark 1991), it was hard to not keep writing about his ideas. I think my next most important paper about Harsanyi is "Measurement Theory and the Foundations of Utilitarianism" (Weymark 2005a), which is where I started looking at the history of expected utility theory and its use in welfare economics. There was a conference in honor of Arrow in Caen that Maurice Salles organized on issues related to the history of social choice and welfare economics. By that time, Harsanyi's early papers were 50 years old. In part because of work that had been critical of my paper in the Elster and Roemer volume by John Broome (Broome 2008) and by Mattias Risse (Risse 2002), I had gone back to looking at von Neumann and Morgenstern's book (1944), seeing how it connected better with normative applications.

One of the arguments of Broome and Risse was that you can go beyond what Harsanyi did with expected utility theory. Broome appeals to "naturalness" when choosing a utility representation. Risse says that there is more to expected utility theory than what I had talked about. If you go back to von Neumann and Morgenstern, they do have more, and it really ties in with the work on measurement theory that I'd done elsewhere. The way we normally present expected utility theory for lotteries, as I've indicated, is that you have binary relations over the set of lotteries, and you also have three axioms. (Things are somewhat more complicated if you allow for compound lotteries.) When von Neumann and Morgenstern develop their expected utility representation, they say that they are also actually considering an operator, and they are making a novel contribution to measurement theory in doing so. The operator is taking convex combinations of lotteries. This is what Risse has in mind.

In what's called the representational theory of measurement—what Krantz, Luce, Suppes, and Tversky's major masterpiece volume (Krantz et al. 1971) is all about—you start with an extensive relational structure. For example, if you're thinking about length, you have an empirical relational structure. Think about having a bunch of rods. How are we going to measure their lengths? You can put two rods side by side, and if one rod goes farther than another, you say that this rod is at least as long as that rod; so we have a binary relation. But we can also have an operator called a concatenation operator, where you put two rods end to end and compare that combined object to the other rod. Then we move into what's called a numerical relational structure, where we want numbers to summarize the empirical relations. It's generally natural, it would seem, that the way you would translate measurements into a numerical structure is that instead of objects you use the numbers that you assign to objects. You translate "at least as long as" into "bigger than or equal" for the numbers, and your concatenation operator is going to be "plus." So, when two concatenated objects are the same length as a third object, in the representation, the length number you assign to the long object is the sum of the other two lengths.

In von Neumann–Morgenstern expected utility theory, you have an infinity of operators, one for each possible weight used to form a convex combination, and you ask that this operation be preserved in the representation. Back in the space of lotteries, the convex combination is, for example: $[\alpha \cdot P] + [(1-\alpha) \cdot Q]$, where P and Q are lotteries. Then in the representation, you want the utility of this combination to be a weighted sum of the utilities assigned to lotteries P and Q, with weights α and $(1-\alpha)$, respectively. This part of von Neumann and Morgenstern's theory got lost. Harsanyi based his work on Jacob Marschak's exposition of von Neumann and Morgenstern theory (Marschak 1950), which is much easier to understand than what was said in von Neumann and Morgenstern's book. Marschak doesn't use convex combination operators; he just works with the binary preference relations. I think that because of this history of thought reason, Harsanyi didn't base his work on von Neumann and Morgenstern, but used Marschak instead.

So, is it possible to resuscitate Harsanyi's conclusion that he really has utilitarianism if account is also taken of these operators? The argument in the measurement theory paper is, no. I have said that we have an empirical relational structure—which here is a set of lotteries, a binary relation on it, and these convexifying operations. We want to translate it into some numerical structure, and we are going to assign utility numbers to lotteries in the order of preference in such a way as to preserve the convexifying operation.

But we do that just because it's simple to work with this kind of number system. You do not have to use this numerical relational structure. Consider the analogous issue when measuring length. Instead of putting two rods end to end, you could put them at right angles and measure the length of the whole thing by the length of the hypotenuse. You have a degree of freedom. So, there is not a unique numerical structure that you can associate with the underlying empirical measurement procedure. This is completely analogous to the fact that you do not have to restrict yourself to the von Neumann–Morgenstern representations when you do not take into account the convexifying operation. There is a degree of freedom. This is recognized in the Krantz, Luce, Suppes, and Tversky book (Krantz et al. 1971). They do not have a big discussion of it, but it is quite early on in the book. I was actually a little bit nervous presenting that paper because Pat Suppes was in the audience, and he is one of the founders of the field and the guru. I thought, "I sure hope I got this right!" And he said, "Yes, it's right!"

CdA: When did you start working on strategy-proof social choice and single-peaked preferences, which is a very important topic in your work with Michel Le Breton?

JW: My work on strategy-proofness started with Michel. Maybe, I should say how our collaboration started. I first met Michel at the World Congress of the Econometric Society at MIT. That was in 1985. Michel's first papers looked at Arrovian social choice and the consistency of the Arrow axioms when you start putting economic structure on the set of alternatives and on the allowable set of preferences. Perhaps, I should just say a few words about that before I go on to strategy-proofness.

CdA: Yes, that's an important topic, too.

JW: There had been some earlier work on that issue, very important work, by Kim Border and Jim Jordan (Border and Jordan 1983) and by Gilbert Laffond. Gilbert Laffond's thesis (Laffond 1980), most of which was never published, related to this. Hervé Moulin's work on single-peaked preferences and strategy-proofness (Moulin 1980) also comes in. There were some important papers looking at structured domains and . . .

CdA: trying to escape the . . .

JW: trying to escape Arrow's impossibility theorem in his framework, not always successfully. Eric Maskin (Maskin 1976) had some early important work on this issue. Norman Schofield was putting together a volume on social choice and political economy. I don't remember if he approached just one of us, or asked the two of us to write a joint paper, or asked us each to prepare a paper. Michel and I had been talking about these issues, and so we decided to write a joint paper (Le Breton and Weymark 1996).

Michel had been primarily working in the Arrow framework where you're ranking alternatives (e.g., Bordes and Le Breton 1989). There is another way to look at Arrow's Theorem from a more choice-theoretic perspective, where you take a profile of preferences and a feasible set, and then choose something from this set. You can reformulate Arrow's Theorem in this framework. David Donaldson and I had been working on this kind of problem (Donaldson and Weymark 1988). I also have an article—without any economic structure—that I wrote with Allan Gibbard and Aanund Hylland (Gibbard et al. 1987). Michel and I had talked a lot about this work.

We wrote an expository piece for the Schofield volume (Le Breton and Weymark 1996) and then we thought we should do something more original together, and we had a couple of ideas. I think the most important one was about strategy-proofness. The proofs of the theorems in the classic papers of Allan Gibbard (Gibbard 1973) and Mark Satterthwaite (Satterthwaite 1975) and the literature that followed on from them were not well suited for starting to include the kind of structure you see in economic models. The reason is that in the proofs you have steps where you take somebody's preference and consider an alternative that is ranked second in the preference. If you are working with economic preferences—for example, if you have two public goods and people have continuous, monotone, convex preferences, or spatial preferences, where there actually is a best alternative—then there's no second best alternative because of the continuity of preferences. So, the proof strategy used for unrestricted domains just doesn't lend itself very well to looking at structured domains.

Then there was a watershed event. It was anticipated by some earlier work of Salvador Barberà (Barberà 1983). The seminal paper using what's called the option set methodology is really the one by Salvador Barberà and Bezalel Peleg in *Social Choice and Welfare* in 1990 (Barberà and Peleg 1990), where they show how to prove the Gibbard–Satterthwaite Theorem on a space of continuous preferences. When you specify the preferences of some subset of the population, an option set is the set of alternatives that are still attainable for some reported preferences of the other people. The way you prove results is by investigating the structure of the option sets. So, if we have a dictatorial rule and I am the dictator, when I report my preference, there's only going to be one alternative in the option set for the rest of the people, my most preferred alternative, because I determine what is chosen. If, on the other hand, I look at any subset of the rest of the population, the option set they generate is the whole set because they have no influence. So, you start to look at what the option sets look like when you consider different structured domains.

After the Barberá and Peleg piece, there was an explosion of activity. At the first meeting of the Society of Social Choice and Welfare in Caen, there were a number of really important papers, some of the early work using their methodology. Michel had arranged for me to come visit him in Marseille, where he was at the time, for a few weeks. I went there to work with him. We had decided that we were going to work on strategy-proofness, looking at a particular restricted domain where you had some separability assumptions. Then in the middle of my visit, we went to Caen for this conference before going back to Marseille to work. We were using the Barberá–Peleg methodology, but it was on a domain where we could not use the kind of constructions that had already been used—we had to come up with some new ideas. That led to my first paper on strategy-proofness (Le Breton and Weymark 1999).

I did quite a bit of reading before I went to work with Michel. It was great because Michel had already started working on strategy-proofness. He had taken some of the thesis research by Gilbert Laffond (Laffond 1980) and worked with him and Georges Bordes to extend it (Bordes et al. 2011). So, Michel knew the literature really well. I was the novice coming into it. We made good progress on what eventually became our main paper on this topic (Le Breton and Weymark 1999).

As a sideline, around that time, Michel was working with Arunava Sen, looking at when, in these separable domains, you can decompose the outcomes into single issue decisions (Le Breton and Sen 1999). I wrote a paper on that as a result of that period of activity (Weymark 1999). It came about as the result of the confluence of two things. First, the fact that we had been working on Arrow-style social choice with restricted domains. Jointly, we had the survey paper in the Schofield volume (Le Breton and Weymark 1996). Separately, Michel had been working on Arrow's Theorem on restricted domains using the social preference approach and I'd been using the social choice theoretic approach. Then, second, all of a sudden, restricted domains became important in the strategy-proofness literature, so we worked on merging the two literatures. Eventually, we were asked to do the *Handbook of Social Choice and Welfare* chapter on Arrovian social choice on restricted domains. We wrote an enormously long chapter (Le Breton and Weymark 2011). It actually came out years after we wrote it. We just missed the first volume and then had to wait seven or eight years for the second volume to come out. It was very fruitful collaboration, and Michel was a great person to work with.

CdA: OK, just a question: recently, you have used tools of social choice to analyze biological problems on measuring group fitness. Do you see this as a promising avenue? Do you see other applications in biology and social choice? I'm curious about that.

JW: On applying social choice ideas in non-traditional areas, more generally, you can think of philosophy of science. With regard to that series of papers, I need to actually step back again to my undergraduate days.

In high school, I'd taken lots of science, but when I went to university, I decided not to pursue science anymore. I did lots of math, but I didn't take science courses. One of the requirements to get my degree as a Bachelor of Arts was to take one or two science courses. I took a computer science programming course. I'd never taken biology as a high school student, and there was a course called something like Genetics and Evolution for Arts students. I thought, "I'll take this, it sounds pretty interesting." I had read some Darwin in the introductory first year course that I'd taken. And the biology course was terrific. Because of my math skills, it wasn't hard for me when we studied Mendelian genetics and needed to figure out how the alleles combine, and their proportions, and all that. Many of my classmates could hardly do anything combinatorial. I thought that the subject was just fascinating. Since then, when I have time, I read popular accounts. The textbook we had was actually a really good book (Lerner 1968). So, I look at some of the biology literature, mostly popular writing.

There's a journal called *Biology and Philosophy*, and there was a social choice article there by Samir Okasha (Okasha 2009), who ended up being a co-author on one of these papers. Samir is a very distinguished philosopher of science who is best known for his work on philosophy and biology.

There's a lot of controversy over the level at which natural selection occurs because not only is it at the gene level, it is at the individual level—there is a biological hierarchy. Organisms are composed of cells. Can there be selection at both levels? So, it's a group selection problem. Samir wrote a book on this subject that won the Lakatos Prize (Okasha 2006), which is a big deal for a philosopher of science. At some point, he started learning individual and social choice theory. He's got a brilliant paper in *Economics and Philosophy* where he looks at cells mixing during meiosis as a "behind the veil of ignorance" argument, tying together Harsanyi with Mendelian genetics (Okasha 2012). In any event, he learned about the social choice theory work of Arrow and Sen.

When you are measuring group fitness, the standard way that it's done by the people who look at this multi-level problem is to think of fitness as having two components. There's what is called viability. Roughly speaking, you might say that this is the probability that whatever organism you are looking at is going to survive to reproductive age. And then if it does, the fecundity part is how many offspring they're going to produce. The standard way of measuring fitness in this formal way is to multiply together a measure of viability and a measure of fecundity.

So, you do this at the cell level. What do you do at the group level? The standard way is just to take an average. And one of the things that people who do this are interested in is what are called evolutionary transitions, understanding what happens when a new level of the biological hierarchy arises—for example, the origins of multi-cellular organisms. If you are going to use this understanding to explain that an evolutionary transition is fitness enhancing, you have to have some more precise notion of fitness than just loose talk.

Samir realized that this was an aggregation problem of the kind studied in social choice theory using Sen's social welfare functionals (Sen 1974). He took the same framework and reinterpreted it in terms of fitness functions. Say we are looking at cells and an organism, where the organism is composed of cells. You're going to have measures of fitness for each of the cells that are actually functions that depend upon the state of the world. You're reinterpreting a utility function as a fitness function. And you are going to aggregate them into the group fitness function. Then, Samir applied his approach to some of the issues that had been considered in the literature. By the way, there is an alternative way other than the average one mentioned earlier of constructing a fitness index.

One of the problems with the averaging approach used to construct a fitness measure is that if you think about the origins of multi-cellular organisms, you get specialization. You get some cells specialized in metabolic activities, which is related to viability, and other cells specialized in reproduction. If you measure the fitness of the cell as a product of some measures of viability and fecundity, the metabolic cells are going to have no fitness because they're not helping in reproduction, and vice versa for the reproductive cells, the gametes. So this is not going to help explain the fitness benefits of specialization that you get in the organisms that are multi-cellular. The point of my first paper in this series was to argue that this is the wrong framework (Bossert et al. 2013a). But Samir was onto the right idea—social choice has the tools to deal with this problem.

Now there's an extension of Sen's framework, largely pioneered by Kevin Roberts, who has done some of the most important work in social choice. He called it the double aggregation problem (Roberts 1995). If you think about social welfare functionals, you are taking a profile of the utility functions for each person and aggregating them into a social order of the alternatives. You capture interpersonal comparisons by saying that if we take certain kinds of transforms of these profiles, things are invariant. In the ordinal non-comparable case, if I take independent monotone transforms of the utility functions, that new profile has to lead to the same social ranking because we haven't changed any of the allowable information. Well, you can think about that as if some planner has constructed this social welfare functional, and it's his or her interpersonal comparisons that are underlying it. But what if we had somebody else making the comparisons? They might make them differently. So, you have a multiple planner problem—there is double aggregation. You have to aggregate for a given planner, and you have to aggregate across them. This is now usually called the extensive social choice problem.

I did this with Chloe Qi—who is a former student—and Walter Bossert in our first paper, which was published in *Biology and Philosophy* (Bossert et al. 2013a). It was really a comment on what Samir did, saying that you have to disaggregate more. You have to think of the viability and the fecundity functions separately so that you can capture the gains from specialization. That's like having two different outside observers. One is looking at it from the viability perspective, and one is looking at it from the fecundity perspective. This is the formal structure you have in extensive social choice.

I had the basic idea after I had read Samir Okasha's paper, but I had a very sharp undergraduate who wanted to write an honors paper with me, Chloe Qi. So, I suggested to her that she work on this problem under my direction. She made some good progress. Part of what she did was related to what I have just described, and part was about another index by Richard Michod and his co-authors (Michod et al. 2006). Our later paper involved characterizing this index. That research became the second article of the series (Bossert et al. 2013b). When it came time to actually clean this work up and publish it, Walter agreed to help out, and we ended up with two articles.

The second index I mentioned is in an article by a large group of authors (Michod et al. 2006). One of them is Yannick Viossat, who is an evolutionary game theorist at Paris-Dauphine. When I was writing this work up—I can't remember if I actually had a draft of it—there was a one-day conference that Philippe Mongin put together at Paris-Dauphine where I presented this research. I had not met Viossat prior to that, but we had a chance to talk. I was going from there to LSE. Their philosophy department is primarily a philosophy of science department, and they have something called the Choice Group that has a regular seminar in which I was to give a talk. I presented this material there. Samir Okasha was at Bristol and I had not met him yet, but I had written to him and corresponded about this research. He said that he would come to the seminar, which he did. We talked for quite awhile afterwards and he suggested that we work together.

Samir proposed that we work on something called inclusive fitness. Say you have two siblings and there is a choice between saving yourself and saving them, each sibling sharing half your genes. In terms of evolutionary survival, the same number of your genes survive by saving two of your siblings or by saving yourself. This idea had been raised by J. B. S. Haldane (Haldane 1955), but not in any formal way. Bill Hamilton, who was one of the great evolutionary biologists of the last century, formalized this idea into what is called Hamilton's rule (or measure) of inclusive fitness (Hamilton 1964). Samir suggested we work on axiomatizing it. Together with Walter Bossert, we axiomatized it, and that became the third paper in the series (Okasha et al. 2014).

Samir's social choice research was connected to Thomas Kuhn's theory about the structure of scientific revolutions (Kuhn 1962), and his subsequent work looking at selecting between theories (Kuhn 1977). You have these various criteria: simplicity, fit, and so on. And it's an aggregation problem. Samir published a very influential article on this topic in a philosophy journal called *Mind* (Okasha 2011). I refereed it. I had actually read it before learning of his other work. At *Mind*, they use double blind refereeing, so I initially didn't know who had written this piece. But once I found out, I didn't know Samir's work, so I looked it up, and that is how I found the *Biology and Philosophy* piece.

One of the things about Samir's paper on theory choice is that I think that he's dead right that the social choice framework is the right framework to look at that problem. The one weakness of his article is that it is not so clear that unrestricted domain—which he uses—is a natural assumption. Michael Morreau, a philosopher who also became another co-author of mine, wrote a comment on that point that also appeared in *Mind* (Morreau 2015). This has now become a hot topic in philosophy of science. Just a week ago, there was a meeting of the European Philosophy of Science Association in Germany where they had a whole session on it.

CdA: OK, I think we should now go on to optimal taxation. Maybe, Felix could ask some questions on that?

FB: Yes, sure. We'll start from social choice and then move to the core of optimal taxation. I have a recollection of a paper by Samuelson in the'60s about Arrow's Theorem, which is called "Arrow's Mathematical Politics" (Samuelson 1967). What he's suggesting there is that this is maybe about mathematics and maybe about politics, but certainly not about economics. According to Samuelson, the typical public policy problem the economist would study is one where you have enough data available to know what the set of Pareto efficient allocations is. And the set of Pareto efficient allocations is something that only depends on ordinal information. Then you are left with the problem of how to select among the different Pareto optima.

That's the problem the economist faces, and then social welfare functions are just one convenient way of formalizing a preference over the set of Pareto optima. How do you like this defense of the use of social welfare functions?

JW: The exposition in Samuelson's paper leaves a lot to be desired. I don't think we can know for sure exactly what he was claiming, There were a couple of points that I think he's making. One is that he's got a different framework from Arrow. Arrow has a multi-profile framework. Samuelson, I think, is saying that we've got the preferences, and we only need to worry about that one profile. Not everybody agrees with that, but that paper has been very influential in leading to what's called single-profile social choice. When people do single-profile versions of Arrow's Theorem, they have to assume a neutrality principle directly, which is basically saying that if the pattern of preferences for a over b across people is the same as c over d, then the social ranking of a and b is the same as the social ranking of c and d. You get that in the multi-profile version by having a rich domain assumption together with Pareto indifference and independence of irrelevant alternatives. But if you only have one profile, you've got no independence condition to give the neutrality.

An alternative interpretation of what Samuelson was talking about is that preferences may change and you have to allow for that, but you don't require independence. So, you're not forcing neutrality. He's very critical of neutrality. He has an example in terms of chocolates. You've got a hundred chocolates to share between Felix and Claude. They only care about how many chocolates they get. Look at transferring chocolates between people. If we take one distribution and move a chocolate from Claude to Felix, you say, "Well, that is better," maybe because Felix didn't have any. Any time we transfer another unit from Claude, neutrality then implies that it's better. But then Felix will end up with everything. So, neutrality is a terrible principle.

That's Samuelson's fundamental objection. I think he really is in a single-profile framework, which is fine for some applications. I believe that people differ on how they feel about independence in a multi-profile case. This issue played a role in the paper in Sen's honor where we proved the full n-dimensional version of Arrow's Theorem based on geometric arguments (Blackorby et al. 1990). The first part of that paper has a result on single-profile welfarism, where we show that in a single-profile case that welfarism is equivalent to Pareto indifference. That partly grew out of thinking about Samuelson's paper.

FB: Would the single profile interpretation be appropriate for a Mirrleesian model of income taxation (Mirrlees 1971), where we just write down a utility function and based on that utility, we characterize the whole set of Pareto efficient tax schedules?

JW: When you look at the Mirrlees model in detail, you've got some cardinal measure of utility, which you don't have with Samuelson—at least in that paper of Samuelson's. How you interpret whether he allows for cardinality or not depends on which paper you're reading. I think it's an issue that you can't pin down based on textual analysis. Some papers sound like Samuelson only considers ordinal cases, others like there's more. The Mirrlees model is single profile, but it's a profile of utility functions that have cardinal significance, so that you can at least compare utility gains and losses when you talk about utilitarianism.

FB: Right. Given the complications of all these assumptions, do you think it's a strength or a weakness of the Mirrleesian approach to work on the single-profile assumption? Is it a strength, in the sense that it makes us talk about something we want to talk about without having to talk at the same time about too many other complicated factors? Or is it a weakness, in the sense that you simplify the value judgments which are inherent in the analysis so much that it's inappropriate?

JW: No, because we can take a multi-profile approach, the Sen welfare functional approach. I should say that although Sen developed this approach and did a lot of the important work using it, there's actually a precursor to it. There's a brilliant chapter on social choice in Luce and Raiffa's *Games and Decisions* (Luce and Raiffa 1957) book from the mid '50s. They don't use the Sen terminology, but social welfare functionals are there. They have matrices, with rows (or columns—I forget which) being the people, the columns being alternatives, and the entries the utilities. You want to aggregate these matrices into social rankings. They don't do much with it, but you can use that framework to axiomatize social objectives. In the paper I've mentioned before that Claude wrote with Louis Gevers (d'Aspremont and Gevers 1977), there's an axiomatization of utilitarianism using Sen's version of this framework.

So, you've got a foundation for the utilitarian objective function using this framework. You could take the Mirrlees problem and say, here's one profile and we're going to use this criterion. and if the preferences change, we'll redo the analysis. If you go back to my comparative statics of optimal income tax paper (Weymark 1987), when I change a parameter in the preferences, we're moving to a new profile and seeing what happens to the solution. You can justify the objective function in the way that Claude and Louis did. Mirrlees was only looking at a single profile, but when you do the comparative statics, you could be changing the profile and seeing what happens to the solution.

FB: To make sure I understand this correctly, you're saying that there is no need to have the single profile in the Mirrleesian analysis? To allow for multiple ones, you would just have to pick a welfare function for every possible profile. Then you would basically do the same analysis that you did before?

JW: Yes, and in his case it's always utilitarianism.

FB: OK, good. You have the work with Craig Brett about the political economy approach to distributive income taxation (Brett and Weymark 2016, 2017). Is this a way of trying to figure out what the relevant social welfare function is, at least for a problem of income taxation?

JW: Well, that wasn't the way we thought about it. There had been a few early papers on voting over tax schedules showing how you would get cycles. Kevin Roberts had a very important paper (Roberts 1977) where you get around this. The impetus for our work was a very important unpublished paper of Ailsa Röell (Röell 2012). When voting over nonlinear income tax schedules, you will generally have Condorcet cycles. Ailsa's interesting observation was to allow for nonlinear tax systems, but to restrict the set of tax schedules we vote over so as to avoid cycles.

In the Mirrlees income tax problem (Mirrlees 1971), you've got two sorts of constraints. You pick a nonlinear tax schedule that's the same for everybody, so how much tax you pay only depends upon how much income you have. There's a taxation principle which says that having people optimize off the common budget set implied by that common tax schedule is equivalent to picking the allocation directly subject to the standard self-selection constraints. So, one way of writing the incentive constraint in that problem is to pick the allocation subject to self-selection constraints. The allocation also has to satisfy the standard materials balance constraints—you can't have more consumption than you've produced. So those are the constraints in the Mirrlees problem. As an objective, he has welfare maximization, in his case, utilitarian maximization.

Ailsa said, "Let's keep those constraints." (She actually has another constraint which I'll just leave aside because she doesn't use it for her voting result in any serious way.) "We've got different types of people, and we ask each one, if you were a dictator, what tax schedule would you pick?" You have to satisfy the same constraints as the Mirrlees problem, the incentive constraints and the materials balance constraint. Then we're only going to vote over these selfishly optimal schedules. In the case where the utility functions are quasilinear, linear in consumption, she has a remarkable result: because people only differ in the skill level in the Mirrlees model, it's a one-dimensional asymmetric information problem. We can index the selfishly optimal schedules by the type that proposes each one. She shows that the preferences are single peaked over the schedules when you index them in that way. Then you can use Black's Median Voter Theorem (Black 1948). She does that by first identifying some of the qualitative properties of the selfishly optimal schedules. Her proof that there is single-peakedness is quite clever—it's not straightforward.

There's a version of the paper that was circulated in '96. It actually isn't quite complete; the conclusion isn't all there. It's something she worked on as a graduate student in the '80s, but she never did anything with it. I'd gotten a copy from Ailsa when I saw her in the late '90s. I'd read it and thought that this was a really remarkable paper, and that I should work on something relating to it at some point. One time Craig Brett was visiting me to work on something else and it went faster than we thought, so we had a couple of days before he went home. I said, "Let's delve into this," because I thought there were techniques we had used that would simplify the analysis.

In my first *Journal of Public Economics* paper on optimal income taxation (Weymark 1986b), I was working in a quasilinear environment. I investigated it using sub-optimization, eliminating one of the variables in a first stage and then optimizing over the second variable. The quasilinearity played a big role in doing that. When utility is quasilinear in consumption, in the first sub-optimization problem, optimal consumptions are chosen for given incomes. When the functions that show how the consumptions optimally depend on incomes are substituted into the objective function, you are left with something that no longer has the consumption variables. You can't just pick arbitrary incomes because the incentive constraints require that the incomes be non-decreasing, which is the only constraint other than the non-negativity constraint on consumption.

I thought that if we used these insights to reduce the dimension of the problem by sub-optimization, we should be able to do more than Ailsa had done. We first started to do this with a finite type model, which is what she was working with. We got stuck at some point, so we moved to a continuum. I think that we can now move back to the finite type case using the same basic ideas (Brett and Weymark 2020). Using sub-optimization, we obtain a problem that only involves solving for the incomes as a function of type.

Mirrlees (1976) showed that with the incentive constraints, there's a first-order condition—it's essentially an envelope condition—and then there is a second-order condition that the incomes be non-decreasing. Most of the optimal income tax literature just ignores the second-order conditions. In trying to understand the problem, we thought we would ignore that first, and then add it back in.

We quickly discovered that the solution for what a particular type would propose had a very simple form if you ignore the second-order condition. When we're trying to find incomes as a function of type, the maxi-max schedule lies above the maxi-min schedule. If we look at some type in the middle of the distribution, it wants to use maxi-max below its type so as to pull resources up towards itself because it is the biggest type of the people below it. For people above it, it wants to pull resources down towards itself, so it's like it has a maxi-min objective with respect to higher types. But if you do that, because the maxi-max schedule lies above the maxi-min schedule, you're going to have a downward discontinuity, and that violates the second-order condition. It turns out what you have to do is iron. You've got to put in what we call a level bridge. So the full characterization with all the constraints is that as you increase the type, you first follow maxi-max, but at some point you bunch a whole group of people together—they all get the same income and the same consumption, and then for higher types, you follow maxi-min schedule. So you have to prove that it's the right thing to do to link the maxi-max and maxi-min schedules in this way and determine where the end points are, which is what we do.

Once you've got that, it turns out to be relatively straightforward to prove the single-peakedness result. But that is because we've got a complete characterization of what the selfishly optimal schedules look like. So, that paper arose from trying to understand what was really driving Ailsa Röell's results, and thinking that there were techniques we had that might allow us to simplify the problem. In the process, we actually learned a lot more about the structure of the problem.

FB: So in some sense, you are saying that there is a really close relation between optimal taxation and mechanism design?

JW: That's right. But when you're working with these optimal tax problems, you're not allowing right from the start all possible mechanisms. We make the assumption that we're picking the same tax schedules for everybody, so how much tax you pay only depends on your own income, not on everybody else's income. In work that Felix has done, he calls them simple mechanisms (Bierbrauer 2011). You could have a more general mechanism. He's investigated when, in continuum models, these simple mechanisms are optimal. We're following the optimal income tax tradition; you just start with that as a restriction on a class of mechanisms you want to look at.

FB: There was one aspect of my question which you have not yet answered. You can take the political economy perspective on the Mirrleesian problem, and you can take the perspective of welfare maximization. How do these two things talk to each other? One way, and that was what I had expected as an answer is to say that we have the tradition of asking questions in the spirit of the second welfare theorem. So, we ask, "What among these welfare optimal or Pareto efficient outcomes can actually be decentralized in one way or another?" And political competition would be one way of decentralizing outcomes. There could also be a more simple-minded motivation, to say we want to have a prediction about what tax schedule the political process will produce. None of this has been present in your answer.

JW: No, but the second theorem of welfare economics is fundamental for understanding the whole point of optimal tax theory. The second theorem of welfare economics says that under appropriate convexity assumptions, if we pick your favorite Pareto optimal outcome, then we can obtain it if we suitably lump sum redistribute resources initially and let the markets work.

The thing about the second theorem is that in order to determine what those personalized lump sum taxes and transfers are, you need a lot of information. You need to know the preferences of the people, and you need to know the endowments. And the point of departure for optimal tax theory is that we don't know that, and so we're going to look at more limited kinds of information. In the income tax literature, you do that by saying that we can't have personalized tax schedules. Everybody has to face the same schedule, but we allow for nonlinear ones.

It also motivates, in the commodity tax literature, the fact that everybody is going to face the same commodity tax rates. In terms of trades on the market, everybody has the same budget set. If they have different endowments, they're going to have different budget sets in terms of final consumptions, but what you actually observe are market transactions, everybody has the same budget set. So, the motivation behind the whole literature is that we're limited because of the asymmetric information about what kind of instruments we have.

Voting is one mechanism for making decisions. Welfare maximization is another. I think they're both worth exploring and trying to see if there are nice connections between the two. I haven't done much on what the connections between the two are. In a sense, this paper we've just been talking about (Brett and Weymark 2017) has some because the lowest skilled uses a maxi-min objective and the high-skilled proposes the maxi-max one. So, there are connections.

FB: What I found interesting in your answer is that you say both welfare maximization and voting are ways of making a decision.

JW: Yes.

FB: That's interesting. You wouldn't say that welfare maximization is just a nice thought experiment for researchers, and voting is what happens in the real world? You would say both are ways to come to a decision, even if it's just interesting to compare them?

JW: Yes.

FB: It's not that one is empirically more plausible than the other?

JW: Well, actually, probably not. I think if you wanted to get something more empirically plausible, you'd have to start getting into bargaining solutions.

FB: Right.

CdA: In representative democracy, this would be quite reasonable, no?

JW: The paper I did with Felix and Craig Brett (Bierbrauer et al. 2013) was not looking at voting. You have two countries and people could move between them, and that's going to limit the amount of redistribution. In each country, you have average utilitarian governments. They are welfare maximizing within each country, and they want to redistribute from rich to poor based upon whoever finally ends up in its country. But people can move, so if you try and tax the rich too much, they're going to move to another country. If you offer really great redistribution at the bottom end, the poor from another country are going to move in, and it really limits the amount of redistribution.

There's really nice work on these kind of tax competition models, like Massimo Morelli, Huanxing Yang and Lixin Ye's piece in *AEJ: Micro* with countervailing incentives (Morelli et al. 2012). The middle group wants to draw resources from the top and the bottom, the top want to move resources up towards them, and the bottom want to move resources down towards them. How that gets traded off in the voting process could depend upon the relative size of this middle group.

There are nice things going on in this literature, but there's not a lot of it yet. There's some combining of welfare maximization and political economy. I think they're compatible. I know some people don't think that is possible. Morelli told me they initially had some concerns raised about their work because people didn't like the fact that there was a mix of political economy and welfare maximization in the same problem.

FB: I would like to get back to the second welfare theorem and also to the relation between the theory of taxation and the theory of mechanism design. When you were discussing the second welfare theorem, you made the observation that the moment we have private information on productive abilities, we can no longer apply it. We need something else. But there are two alternatives. One is, as you said, we can go for optimal mechanisms, or we could go for tax systems. And there are some situations where the two are the same thing, but there are other environments where they aren't. For instance, if you have a finite population with a known cross-sectional distribution of productive abilities, that's a particular environment that we know from work of Thomas Piketty (Piketty 1993) that gives rise to correlations among types. If you have just two people and you know one is rich and one is poor, types are perfectly negatively correlated and, in principle, you could come up with a mechanism that exploits this correlation to have a first best alternative.

JW: Yes. Before you go on, the big difference between these finite type models and the continuum models is that if one person deviates by not telling the truth, you can't detect it in the continuum model, but you can in the finite model. That's what

Piketty is exploiting—you know the distribution, it's common knowledge. And so if what you see isn't the right distribution, you know somebody has deviated. But the mechanisms you need to deal with this, what he calls generalized tax schedules, are complex. How much tax I have to pay in one of his mechanisms depends not only on my own income, but on everybody else's income, in principle.

FB: Yes, yes.

JW: So, I think the justification that somebody like Jim Mirrlees would offer is: because we've got this asymmetric information, we need to use anonymous tax schedules. It's a little too quick. As you were saying, in these finite type models, while we could use something more complex and still respect the asymmetric information, we need to justify what in your own work you call simple mechanisms—where the tax schedule only depends upon your own income. At some point, you say that generalized tax schedules are unrealistic. People aren't going to put up with having their tax depend on what somebody else's income is. So, it's a natural way to start restricting the class of mechanisms you look at; you're not looking at optimal ones.

FB: OK, you answered the question that I was after. I wanted to understand the modeling choice that you made. It's an appeal to realism. I have a related question. In public finance, we had this important paper by Atkinson and Stiglitz in '76 (Atkinson and Stiglitz 1976), where they're saying that in many situations, commodity taxation is superfluous if you have the income tax at hand. In what sense is studying Ramsey models of taxation after the Atkinson and Stiglitz critique different from studying Mirrleesian taxation after Piketty's critique?

JW: That's a tough one! [All laugh.]

FB: Because the basic argument is the same. You say there is something better, it's even respecting all the constraints that we have. Why don't you go for the thing that's better?

JW: OK, I've given you an answer in terms of the Piketty approach.

FB: So, your answer to that critique was to say that the Mirrleesian income tax problem looks so much more reasonable. But still you would never study a Ramsey problem.

JW: No, I've never written a paper on Ramsey problems—I teach them.

FB: So, what is the difference? Put differently, what is the difference in the balance between simplicity and theoretical appeal? Why does it make you go for Mirrlees rather than Ramsey in one kind of situation, and make you go for Mirrlees over Piketty in another one?

JW: I really don't have a good answer to that off the top of my head. The Atkinson–Stiglitz kind of problem is something I've never worked on. There's huge literature on it, and I've taught it from time to time. I haven't taught tax theory all that often, and it's not something I've really thought through. But it's a good question; it's something to think about.

FB: Let's move on to your *Econometrica* paper on the comparative statics properties (Weymark 1987), which you mentioned already. You did this based on the assumption that preferences are quasilinear in leisure. How do you think of this modeling choice as of today? Today, many people assume preferences that are quasilinear in consumption. Jonathan Gruber and Emmanuel Saez would defend that on empirical grounds. What are your thoughts on this? Did you have a deep reason to do it with preferences that are quasilinear in leisure, or could it just have been the other way around, and you just picked one?

JW: It could have been the other way around. There is a reason why it was done the way I did it. But let me address your question about people that claim that if you're going to use quasilinearity, you should use quasilinear in consumption, rather than quasilinear in income. Notice that the voting work we've talked about has quasilinearty in consumption, but the earlier work I did on comparative statics is in terms of quasilinearity in income. Laurent Simula (Simula 2010) worked out what it looked like in the other case.

I don't think the empirical evidence is all that relevant. We're looking at optimal income tax systems. What they look like could be very far from what we're observing in the non-optimal tax systems that are out there in practice. So, the fact that empirically it looks like we're closer to quasilinearity in consumption, that's only when you're looking at it in terms of these non-optimal tax systems. What things look like if we're really looking at an optimal tax system could be very, very different.

Much like how the work I did with Craig Brett on voting grew out of trying to deeply understand what, in that case, Ailsa Röell did, the choice of quasilinearity in income in the original paper was motivated by a nice short paper by Jean-Charles Rochet and Stefan Lollivier in JET in the early '80s (Lollivier and Rochet 1983) where they were investigating a Mirrlees model with quasilinear preferences. They assumed quasilinearity in income. They had this very interesting result that in this quasilinear framework with a continuum of types, any bunching is all down at the bottom of the skill distribution if the distribution is uniform.

I haven't written very many papers using a continuum approach because I find that I have much better intuition working with discrete types and spaces. You can understand the geometry underlying the problems and the incentives much easier. In trying to understand the Rochet and Lollivier paper, I asked, "What would happen with a finite number of types?" The reduction of the dimension of the problem that I mentioned before that happens in their paper is kind of implicit. You can do this reduction in a finite type problem as long as you have some mild assumptions. In a Mirrlees-style problem, I used a weighted utilitarian objective. You want the adjacent downward self-selection constraints to all bind and you also want the economy-wide production constraint, or the government budget constraint, to bind.

The way you do this dimensionality reduction—when you're assuming quasilinearity in income—is to first fix the distribution of consumption across types, and find what the optimal income vector is conditional on these incomes. As a first step, just fix the lowest income. We know that you can find the income of the second-lowest person by going up to the consumption of type two that we pre-specified along the type two indifference curve through the type one bundle. When you've got up to that income, it tells you what the type two income is. And then just iterate up. But when all is said and done, you may not have budget balance. Then you just move the whole schedule parallel so you don't upset these binding constraints, and that pins down where you should have set the lowest income (see Weymark 1986b).

Again, the choice of quasilinearity in income was only made only because that's what Rochet and Lollivier did. I could have done it the other way around. Once I saw this in this finite type case, I realized that you could start doing comparative statics, which didn't exist for nonlinear taxes. In fact, there was this paper by Guesnerie and Seade (1982) that I've mentioned where they're looking at nonlinear pricing and nonlinear taxation with finite types. They actually say something misleading about doing comparative statics. If you think about the pattern of binding constraints I described, they say that if I move the bundle for some type, I'm going to get discontinuous jumps in what happens. But that's not the right experiment to look at because what you wanted to do is re-optimize. That's what I did.

If you look at the case where you have an interior solution and there's no bunching, you can solve for each variable separately. The solutions are a function of parameters, and you can start moving the parameters around to get the comparative statics. Doing this also enabled me to understand why you have bunching at the bottom with Lollivier and Rochet—it's not true in general in the finite type model (Weymark 1986a). But if the distance between adjacent types is a constant—so starting with type one, you go up to type two and, say, add two to its skill level, go up to type three and add another two, then the types evenly spaced and there is only bunching at the bottom. Implicitly that's what's going on with Loliiver and Rochet because they worked with the uniform distribution. If you have a uniform distribution in a continuum, it's like having the types evenly spaced.

CdA: So, you seem to say that this specific type of quasilinearity comes out of the problem in some sense.

JW: The choice I used in that paper was simply because my starting point was Rochet and Lollivier and that's what they used, but I could have done it the other way around. And, as I've said, it has been done. For the comparative statics, I only looked at changes in certain parameters. I changed the marginal disutility of income term that appears because things are quasilinear, the slope of the budget constraint, and some of the welfare weights in a weighted utilitarian objective. There are other parameters that could have been changed related to the skill distribution. What happens if we change the value of some skill? What happens if we change the distribution of skills for a given type? At the time, I didn't know how to solve these questions, and so I teamed up with Craig Brett. We tackled them and made progress (Brett and Weymark 2008, 2011).

Craig had been a graduate student at UBC. At the time he was coming through, I never taught any public economics. Chuck Blackorby had decided to learn tax theory and he used Guesnerie's monograph (1995) as the textbook. Craig was in the class and he could understand it. So, Chuck was the supervisor, but I was on the committee. Craig wrote a great thesis, and then afterwards we started collaborating.

FB: So, maybe one last, very broad question on the theory of optimal taxation. I had a disappointing experience when I was a young Ph.D. student. I met Mirrlees, and I had worked on theories of taxation, and it was a great opportunity to talk to him for me. And I asked him what his basic attitude to this whole research program was—whether he thought of this as being a tool for deriving policy recommendations, or an abstract theory of equity-efficiency trade-offs. And he refused an answer. He said I should decide that for myself. How would you answer, and are you sympathetic to this agnostic attitude of Mirrlees?

JW: Well, I think there's aspects of both phenomena. At the level of abstraction that optimal tax theory operates at, it can't pin down specific programs, specific policies. But I think it highlights the kind of considerations that should be taken into account when you design policies. From the other perspective, I think it shows a lot about how equity and efficiency considerations interact, and you need to take them into account when you're designing policies.

Early on when I was working on commodity taxation, I was initially looking at optimal commodity taxes. There was a period around the time that I was a young Assistant Professor that people started looking at tax reforms, I think largely because of work by Feldstein (1976). We're not involved in designing a whole tax system from scratch, we're making marginal changes from where we are. A number of people, like Dixit (1979), Diewert (1979), and Guesnerie (Guesnerie 1977)—in fact, one of Jean Tirole's first publications was one with Guesnerie on tax reform (Tirole and Guesnerie 1981)—started looking at what directions we could move outcomes in a welfare- or Pareto-improving way. In the context of a commodity tax problem, what we need is some knowledge about excess demands for goods on the part of individuals, but we also need to know the elasticities at the aggregate level. We don't need that data at the individual level. There's been some very nice empirical work where you put people into groups and work with aggregate goods, Nick Stern and some of his co-authors (Ahmad and Stern 1984), also Serge Wibaut (Wibaut 1989), Was he a CORE person? He did some nice work where you go out and take the theory to the data, to try and see if there's welfare-improving or Pareto-improving changes in policies.

Actually, I can remember at the time I was at CORE, another person who worked on taxation was Knud Munk. He spent part time at CORE that year while he was working at the European Commission. They were looking at reforming different kinds of policies, like tariffs and agricultural policies. I think what this perspective does, which we talked about at the time, was that rather than looking at one kind of change in isolation, like changing a single tariff, you look for a simultaneous change in the values of a number of policies to see if you can make them, for example, in a Pareto-improving way—trying to make things politically acceptable that way. You can't do this at the level of disaggregation that our models have, but you can do it in terms of more aggregated models.

FB: Thank you. I think you once told me that the only non-academic piece that you have written was on the problem of economic growth in Brazil, if I remember that correctly. And that brings me to the question of how the fields in which you would be ready to communicate with the broader public are related to your fields of expertise as a researcher? Is it that the more you know as a researcher, the less competent you feel about giving advice?

JW: I like academic, theoretical work. I like making precise statements from precise models. And I think when you're giving policy advice, you're going to have to make implicit, kind of fuzzy statements. You're not going to be able to back them up completely by a model. I find that hard to do.

The piece was in a Brazilian journal, *Exame* (Weymark 2010). My Brazilian friends told me that it's the Brazilian version of *The Economist* and they have, on occasion, special issues that matter. They wanted me to write a short piece of a couple of thousand words that they would translate into Brazilian Portuguese on pro-poor growth, if possible related to Brazil. I'd never done anything like this. I knew a little bit about the relevant subject and I knew a little about the Brazilian experience because I'd been reading about experiments with conditional cash payments for doing something like keeping your kids in school or for young mothers going to training sessions where they learn about good nutrition to help raise their kids. I had also read about pension reforms. I knew a bit about these issues and so I thought, well I knew enough about the theory and it wouldn't take me long to put in some illustrations. It ended up taking longer than I thought. I didn't have a lot of time to do it.

This is the first and only work for hire that I did. But one thing I should have known—I knew, but I wasn't really thinking about it—was that the editorial staff would edit the piece quite a bit. They didn't really distort my message, but that's what happens with journalism—you don't get the final say of what things look like. I had somebody who's Brazilian read the piece back to me.

The title in English would be "Dawn over the horizon, how beautiful." I did not pick that title. It deals with the punch line. Brazil in the decade before that piece was written had made unbelievable progress in reducing poverty at the bottom end of the distribution, in part from trade liberalization policies, but also from pension reforms, particularly for the rural poor, and through these conditional cash payment programs. The bottom line of the paper was that the pension reforms had an immediate impact, but these other policies are not going to have an impact for a decade or so because you won't see gains from improving human capital among children until they're on the job market. And so that was where the funny title came from.

Along the same lines, I had another opportunity to do something that had policy relevance, but in the end I didn't do it. In the early '90s, I had a visiting chair at Johns Hopkins. The chair rotated among a few departments. You had to be from the British Commonwealth to hold the chair, and there had been a requirement, which was not being enforced anymore, to give a public lecture on some constitutional issue. This was not long after there had been a lot of debate in Canada about Quebec separating, and in particular among economists about some of the economic issues and consequences of separation. Robin Boadway, who is one of the great public economists and who is also from the same hometown as me, is very good at writing good theory and writing these kind of policy pieces, and he'd written a nice economic analysis of the relevant constitutional issues (Boadway 1992). So, initially I was tempted to do the talk. And then I thought, it's just not me. And Baltimore is so close to Washington DC that I'm sure people from the Canadian embassy would show up and I'll just embarrass myself, so I'll stick with things that I know. So, I just gave talks on my research.

CdA: OK, so maybe moving to some very important tasks that you have done—the many editorial boards on which you have served. You have gone through a huge amount of papers, referee reports, and so on. Can you tell us what for you are the qualities of a good editor? And what challenges you faced, and what is generally your experience in that aspect? It's a very important aspect, and you did a lot of work on that.

JW: Yes. I'm easing out of that because it's very time consuming. Something I said earlier was that when I was starting off, there were some senior people who were very, very good and who were supportive of me. They showed me through example, and so I want to help young scholars. When I do refereeing, one of the things I like about not having double blind refereeing is that if I know it's a young person, I'm willing to spend more time to try and help him or her develop the paper. A young scholar may have a good idea, but it may not be expressed very well—and I'll put a lot of time into providing advice. It's not that I don't give serious comments on other people's papers too, but I try and particularly help young scholars.

With rare exceptions, I want to know that any paper that I recommend as a referee to be resubmitted or accepted is right. Lots of referees don't check for correctness. I became sensitive to this very early in my career. There's a piece by Roger Guesnerie and Jean-Jacques Laffont in JET on taxing price makers (Guesnerie and Laffont 1978). It's an optimal tax paper where they have non-competitive markets. It's a fabulous paper. I was a referee of it and I couldn't understand one result—I thought that it's not stated correctly. I thought I had a counterexample. I puzzled over it, put it aside, came back to it in a few days and thought, "You know, it's close to being right, but I think you need an extra assumption." However, I couldn't find a flaw in their proof. After a couple of days, when I went back to the paper, I found the flaw in the proof, and you did need this extra assumption. I thought, "These guys are brilliant and if they can make a mistake like that, think about mere mortals." A large number of papers I referee have errors that are typically easily fixed. I think it's important when we have papers published that you can count on them. And so as a referee, that's one of my goals.

Initially, my editorial roles were not as a main editor, but often as an associate editor that handles the manuscripts. I would send them out to referees and then I would also prepare a report. In that role, I would also use the same rule. But when I became a Managing Editor of *Social Choice and Welfare*, that was just impossible. And the same is true with *Economics and Philosophy*. I guess a guiding principle is to treat people fairly.

Sometimes there are different ways of approaching a problem, and it can get ideological. I think that on the whole, people working in social choice and welfare economics are pretty open-minded, but it's not true in all areas of economics. On occasion, I'll get something as an editor where I think that the referee is taking too narrow a view and that what the author is doing is reasonable. I think that if reasonable people can disagree about how something is modeled, just because it's not your way, you don't turn it down. Let us get this work out into the public domain, and let people investigate it. So, that's been a major consideration in my editorial policy.

Another thing that I've put a lot of priority on are publication lags. They can get very, very long, which can be a disaster for junior people who are on a tenure clock. I now do research in philosophy too and have edited *Economics and Philosophy*. For young philosophers, it's difficult to even get a permanent job. It's just so much harder in their field. Waiting a year, I just think it's morally wrong to keep people waiting that long. So, when I became a Managing Editor of *Social Choice and Welfare*, I really pushed to do everything we could to make sure that nobody waited more than six months for a decision. And I did the same thing at *Economics and Philosophy*. Many journals ask you to produce a report in three or four weeks. I think that is the wrong margin to push on. You might get a long, complex paper, and if you want a good, serious report, you have to give the referees more time. People don't mind waiting a couple of months, or three months, for a report, to get a serious review. They do mind if they have to wait eleven months, twelve months. And I think in both the journals that I've been a main editor of, there's been good progress made on getting that long tail cut down.

CdA: And did you see an evolution along the years, in terms of the publication process, did it change?

JW: Yes. Now with web-based electronic submissions, for every kind of option, there's a template. A lot of editors—for time saving reasons—tend to have standard kinds of letters and just send them out. They tend to be very impersonal, but you can modify them. *Economics and Philosophy*, when I was of the main editors, wasn't doing web-based submissions yet; they were in the process of transitioning to it. *Social Choice and Welfare* moved to a web-based platform when I was a Managing Editor. You have to put a lot of thought into what your basic letters are going to say because there are variations on the decision letters needed.

So, have a good template, but then don't just use it as a form letter. It's not that hard, once you've got good templates, to make a letter specific to the person it's being directed at. I think it's an obligation as an editor to give a good reason if you're going to turn a paper down. You can't just send the author some standard terminology—regrets, we're turning you down, the referees didn't like it, or whatever. It takes more time, but the actual writing of the letter doesn't necessarily take a whole lot more time if you've got the basic structure in a template that you can build off of.

FB: Do you think that in your fields of expertise, it has become more difficult or easier to publish your work also in general interest journals?

JW: Well, I've never actually published much in general interest journals, so I can't say. I haven't actually sent many papers to general interest journals, so I'm not quite sure what to say about that.

CdA: We know one paper was published fast; it was the paper with Donaldson on Ginis and the principle of population (Donaldson and Weymark 1980).

JW: Actually, my very first publication was extremely quick. It's in a philosophy journal called *Philosophical Studies*, which is actually a pretty good journal. It had a technical result. I was criticizing some work by Nicholas Rescher (Rescher 1975), who had written on altruism. He started with a simple prisoner's dilemma and said, "We have this non-cooperative behavior that leads to something that neither person would like. What if we made them altruistic, so each of them gives a little weight to the other person's utility? By doing that, in some circumstances, we can transform a prisoner's dilemma structure into one where you get outcomes that they all prefer. So, this a good argument for altruism."

The point of my paper (Weymark 1978b) was that you can start with situations that don't have a prisoner's dilemma and by doing what Rescher did, create situations that have the prisoner's dilemma structure. So I wrote this up, and I actually worked out conditions for which the altruism would result in the prisoner's dilemma form and for which it does not, which I included in the paper. *Philosophical Studies* allows a little bit of technical material. This was back when you were corresponding by mail. I had a strong revise and resubmit within ten days of submitting my paper. All the editor wanted was this little result moved into an appendix. That was the fastest acceptance I have had.

CdA: [laughs] And what was the hardest one?

JW: The hardest?

CdA: Maybe it's one which is not published . . .

FB: Always the one you're currently working on!

JW: Well, hard ones to publish were my paper with Claude here on cartel stability (d'Aspremont et al. 1983), which ended up being very influential. My Generalized Gini paper (Weymark 1981a) was hard to publish—I only think I sent it to a couple of places before *Mathematical Social Sciences*. *Mathematical Social Sciences* was in its first year of publication. I was really naïve as a junior scholar in not realizing that most places would count a publication there as almost nothing because nobody's heard of the journal. It was started by two mathematicians, and one of them obviously was the editor in charge. The editors were really good, and one of them suggested a way of simplifying the proof. I had a very good experience with them, but not the actual printing by the publisher—my paper looked like a drunk had typeset it. There were ten million corrections on the page proofs and the publisher didn't correct any of them. The whole issue was like that. Arrow had a paper in that issue, too (Arrow 1981). It was unreadable, so the publisher ended up republishing the issue by doing a second set of proofs. But the editorial process was good.

FB: There's one last topic we'd like to touch on: you have also been an academic teacher over the years. So how would you describe yourself as an academic teacher?

JW: You know, at the graduate level, you always have to make a decision about how you trade off breadth versus depth. I don't think all instructors need to make the same kind of trade-off—it helps to have the depth in some courses, and in other courses, the breadth. I tend to lean towards the depth. I want students to really understand the guts of a model, how to carefully derive results. We may not cover a huge range of topics when I teach graduate micro theory or taxation or social choice. All the arguments are there, the proofs are there, and the students do lots of problem solving.

With undergraduates, I lean more towards breadth, but again I want them to understand the models, and I give them lots of problems so that they can practice. One thing that's been true in the USA over the time since I started is that the courses have just been dumbed down. The levels at which the courses are taught are much less than when I started. I refuse to do this. I want to challenge the students; I want them to learn. Many of them will rise to the challenge, but some of them resist.

FB: Imagine the following situation. You have somebody who has passed the stage of coursework, has been proven to be a smart person, and would like to write a job market paper on optimal taxation, for example. The person is ambitious but just feels unable to come up with a good, specific research question to study in the job market paper. Are you more likely to tell the student that she should work harder, read more papers, invest in her mathematical skills? Or are you more likely to say, sometimes inspiration comes, sometimes it doesn't. Have a nice walk around the university!

JW: You know, I haven't been the main supervisor of many students. At Duke, I was on committees, but never supervised; I was too junior to supervise. The UBC Ph.D. program was quite small, but it had a big masters program where they did a thesis. I had some terrific students, and they all came up with their own topics. At Vanderbilt, you do not get very many students that want to do theory, so I'm not doing much supervising. I really think it's important for the students to come up with their own topics. I think that's a big part of what doing research is about. If they don't have great ideas, maybe their research won't go to the really good journals, but it's something they've done on their own.

I've supervised or co-supervised a few undergraduate honors papers, like the research with Chloe Qi on biology. I gave her the topic. It's rare that undergraduates even do something this technical in the USA; they don't know enough about the subject typically to come up with a topic. Another one that I jointly supervised a year ago was an honors paper in philosophy. We gave the topic to the student and let him see what he did with it—we didn't actually pinpoint the questions, just gave some advice about what to explore.

Another one that I co-supervised two years ago with Alan Wiseman from Political Science was an honors thesis by a young guy named Thomas Choate, who's very good (Choate et al. 2019). Thomas worked on an extension of Baron–Ferejohn legislative bargaining (Baron and Ferejohn 1989). He'd learned in a game theory course the year before about Rubinstein alternating offers bargaining (Rubinstein 1982), but learned about the legislative bargaining literature on his own. He went and read it, and came up with a topic. His model was not tractable—a big part of what Alan and I did was show him how to simplify the model to make it tractable. It was amazing that an undergraduate could come up with that. And he was young; he graduated at age 20. He's now at the Stanford Graduate School of Business in the Political Economy program.

So, I'll help undergraduates with the topic because they just haven't had a lot of exposure. With graduate students, I will not help them with a topic. I haven't had a lot of graduate students, but most of the ones I've had have been pretty good. My first student was Bentley MacLeod, who's at Columbia and a Fellow of the Econometric Society, so I was very fortunate there.

FB: What are teaching formats that prepare graduate students for the challenge to find a research topic that can be fruitfully analyzed?

JW: In the North American system, you do intensive coursework for a couple of years and then let the students try and find a topic. It's changing in Europe, but it used to be more that you'd work with somebody. You'd have a narrower focus. You'd start seeing the kinds of problems that your advisor would work on, and maybe come up with a topic related to them. It's almost like an apprenticeship. Many students in the US system have a horrible time making the transition from coursework to a thesis topic.

It helps them sometimes in the more specialized courses where you don't emphasize breadth. You just read a series of articles that are on the cutting edge; then you start getting some ideas. I don't do much of this in my courses because I want the students to have the foundations. Sometimes, I do a little bit of this. I don't teach graduate social choice much—you know, it's not marketable in the USA. But when I do, I might spend two-thirds of the course covering a set of topics so that the students get the basics of the subject; then we read a few papers. Or I will have them write a term paper or make a presentation where they take a couple of papers that are related and relatively new so that they see what kind of issues are getting attention.

On occasion, when I have time and I'm not overloaded with journal editing or administrative work, I organize reading groups. Right now, I've got a reading group. Paul Klemperer and Elizabeth Baldwin are using tropical geometry to study auctions and other things (Baldwin and Klemperer 2019), and I think this has potential for other mechanism design problems. So, we've got a reading group reading papers on mechanism design and also learning tropical geometry. I've got one Vanderbilt student in this group, but at some other university, I might have had more. I've had reading groups where we've had more students than that. They're not part of a formal course structure, but it's doing math and seeing some current literature, and hoping to come up with new research ideas by doing it. You need both breadth and depth, but it doesn't have to be, as I said, in the same course; it can be one person doing the depth, one person focusing on a few papers.

FB: So, ready for your final question?

CdA: Yes, I think we should really stop here, but the last question is to ask you is if you have something to add or question to answer that we didn't ask? Maybe, we have missed something?

JW: One thing I said is that I tend to cycle through topics. I mentioned the special program I was in as a first year student, a lot of it was very philosophical. I never actually took that many philosophy courses. I took a few. More so at the graduate level, like a course in political philosophy. But I've always had this strong interest in philosophy, and it's partly why I'm drawn to subjects like social choice and welfare economics. I've used the past few years to seriously learn more philosophy and to publish in the area. This is something I'd like to continue to do.

CdA: Thank you very much!

FB: Thank you.

Acknowledgements We are grateful to Erika Berthold, Chris Evans, and Risa Pavia for their assistance in preparing a transcript of this interview.

References

Aczél, J. (1966). *Lectures on functional equations and their applications*. New York: Academic Press.

Ahmad, E., & Stern, N. (1984). The theory of reform and Indian indirect taxes. *Journal of Public Economics*, *25*, 259–298.

Arrow, K. J. (1951). *Social choice and individual values*. New York: Wiley.

Arrow, K. J. (1964). The role of securities in the optimal allocation of risk-bearing. *Review of Economic Studies*, *31*, 91–96. Originally published in French in 1954.

Arrow, K. J. (1981). Jacob Marschak's contributions to the economics of decision and information. *Mathematical Social Sciences*, *1*, 335–338.

Atkinson, A. B. (1970). On the measurement of inequality. *Journal of Economic Theory*, *2*, 244–263.

Atkinson, A. B., & Stiglitz, J. E. (1976). The design of tax structure: Direct versus indirect taxation. *Journal of Public Economics*, *6*, 55–75.

Baldwin, E., & Klemperer, P. (2019). Understanding preferences: "Demand types", and the existence of equilibrium with indivisibilities. *Econometrica*, *87*, 867–932.

Barberà, S. (1983). Strategy-proofness and pivotal voters: A direct proof of the Gibbard-Satterthwaite Theorem. *International Economic Review*, *24*, 413–417.

Barberà, S., & Peleg, B. (1990). Strategy-proof voting schemes with continuous preferences. *Social Choice and Welfare*, *7*, 31–38.

Baron, D. P., & Ferejohn, J. A. (1989). Bargaining in legislatures. *American Political Science Review*, *83*, 1181–1206.

Bierbrauer, F. J. (2011). On the optimality of optimal income taxation. *Journal of Economic Theory*, *146*, 2105–2116.

Bierbrauer, F. J., Brett, C., & Weymark, J. A. (2013). Strategic nonlinear income tax competition with perfect labor mobility. *Games and Economic Behavior*, *82*, 292–311.

Black, D. (1948). On the rationale of group decision making. *Journal of Political Economy*, *56*, 23–34.

Blackorby, C., & Donaldson, D. (1977). Utility vs equity: Some plausible quasi-orderings. *Journal of Public Economics*, *7*, 365–381.

Blackorby, C., & Donaldson, D. (1978). Measures of relative inequality and their meaning in terms of social welfare. *Journal of Economic Theory*, *18*, 59–80.

Blackorby, C., Davidson, R., & Donaldson, D. (1977). A homiletic exposition of the expected utility hypothesis. *Economica*, *44*, 351–358.

Blackorby, C., Primont, D., & Russell, R. R. (1978). *Duality, separability, and functional structure: Theory and economic applications*. New York: North-Holland.

Blackorby, C., Donaldson, D., & Weymark, J. A. (1980). *On John Harsanyi's defences of utilitarianism*. Discussion Paper No. 8013, Center for Operations Research and Econometrics, Université catholique de Louvain.

Blackorby, C., Donaldson, D., & Weymark, J. A. (1982). A normative approach to industrial-performance evaluation and concentration indices. *European Economic Review*, *19*, 89–121.

Blackorby, C., Donaldson, D., & Weymark, J. A. (1984). Social choice with interpersonal utility comparisons: A diagrammatic introduction. *International Economic Review*, *25*, 327–356.

Blackorby, C., Donaldson, D., & Weymark, J. A. (1990). A welfarist proof of Arrow's Theorem. *Recherches Économiques de Louvain*, *56*, 259–286.

Blackorby, C., Donaldson, D., & Weymark, J. A. (1999). Harsanyi's social aggregation theorem for state-contingent alternatives. *Journal of Mathematical Economics*, *32*, 365–387.

Blair, D. H., & Pollak, R. A. (1982). Acyclic collective choice rules. *Econometrica*, *50*, 931–943.

Blair, D. H., & Pollak, R. A. (1983a). Polychromatic acyclic tours in colored multigraphs. *Mathematics of Operations Research*, *8*, 471–476.

Blair, D. H., & Pollak, R. A. (1983b). Rational collective choice. *Scientific American*, *249*(2), 88–95.

Boadway, R. W. (1992). *The constitutional division of powers: An economic perspective*. Minister of Supply and Services Canada, Ottawa: A study prepared for the Economic Council of Canada.

Border, K. C., & Jordan, J. S. (1983). Straightforward elections, unanimity and phantom voters. *Review of Economic Studies*, *50*, 153–170.

Bordes, G., & Le Breton, M. (1989). Arrovian theorems with private alternatives domains and selfish individuals. *Journal of Economic Theory*, *47*, 257–281.

Bordes, G., Laffond, G., & Le Breton, M. (2011). Euclidean preferences, option sets and strategyproofness. *SERIEs*, *2*, 469–483.

Bossert, W., & Weymark, J. A. (2004). Utility in social choice. In S. Barberà, P. J. Hammond, & C. Seidl (Eds.), *Handbook of utility theory. Volume 2: Extensions* (pp. 1099–1177). Boston: Kluwer Academic Publishers.

Bossert, W., Qi, C. X., & Weymark, J. A. (2013a). Extensive social choice and the measurement of group fitness in biological hierarchies. *Biology and Philosophy*, *28*, 75–98.

Bossert, W., Qi, C. X., & Weymark, J. A. (2013b). Measuring group fitness in a biological hierarchy: An axiomatic social choice approach. *Economics and Philosophy*, *29*, 301–323.

Brett, C., & Weymark, J. A. (2008). The impact of changing skill levels on optimal nonlinear income taxes. *Journal of Public Economics*, *92*, 1765–1771.

Brett, C., & Weymark, J. A. (2011). How optimal nonlinear income taxes change when the distribution of the population changes. *Journal of Public Economics*, *95*, 1239–1247.

Brett, C., & Weymark, J. A. (2016). Voting over selfishly optimal nonlinear income tax schedules with a minimum-utility constraint. *Journal of Mathematical Economics*, *67*, 18–31.

Brett, C., & Weymark, J. A. (2017). Voting over selfishly optimal nonlinear income tax schedules. *Games and Economic Behavior*, *101*, 172–188.

Brett, C., & Weymark, J. A. (2020). Majority rule and selfishly optimal nonlinear income tax schedules with discrete skill levels. *Social Choice and Welfare*, *54*, 337–362.

Broome, J. (1993). A cause of preference is not an object of preference. *Social Choice and Welfare*, *10*, 57–68.

Broome, J. (2008). Can there be a preference-based utilitarianism? In M. Fleurbaey, M. Salles, & J. A. Weymark (Eds.), *Justice, political liberalism, and utilitarianism: Themes From Harsanyi and Rawls* (pp. 221–238). Cambridge: Cambridge University Press.

Choate, T., Weymark, J. A., & Wiseman, A. (2019). Partisan strength and legislative bargaining. *Journal of Theoretical Politics*, *31*, 6–45.

d'Aspremont, C., & Gabszewicz, J. J. (1986). On the stability of collusion. In J. E. Stiglitz & G. F. Mathewson (Eds.), *New developments in analysis of market structure* (pp. 243–261). Basingstoke, UK: Macmillan.

d'Aspremont, C., & Gevers, L. (1977). Equity and the informational basis of collective social choice. *Review of Economic Studies*, *44*, 199–209.

d'Aspremont, C., Jacquemin, A., Gabszewicz, J. J., & Weymark, J. A. (1983). On the stability of collusive price leadership. *Canadian Journal of Economics*, *16*, 17–25.

Diamond, P. A., & Mirrlees, J. A. (1971). Optimal taxation and public production. I and II. *American Economic Review*, *61*, 8–27, 261–278.

Diamond, P. A., & Mirrlees, J. A. (1976). Private constant returns and public shadow prices. *Review of Economic Studies*, *43*, 41–47.

Diewert, W. E. (1979). Optimum tax perturbations. *Journal of Public Economics*, *10*, 139–177.

Dixit, A. K. (1979). Price changes and optimum taxation in a many-person economy. *Journal of Public Economics*, *11*, 143–157.

Domotor, Z. (1979). Ordered sum and tensor product of linear utility structures. *Theory and Decision*, *11*, 375–399.

Donaldson, D., & Weymark, J. A. (1980). A single-parameter generalization of the Gini indices of inequality. *Journal of Economic Theory*, *22*, 67–86.

Donaldson, D., & Weymark, J. A. (1988). Social choice in economic environments. *Journal of Economic Theory*, *46*, 291–308.

Feldstein, M. (1976). On the theory of tax reform. *Journal of Public Economics*, *6*, 77–104.

Fleming, M. (1952). A cardinal concept of welfare. *Quarterly Journal of Economics*, *66*, 366–384.

Gajdos, T., & Weymark, J. A. (2005). Multidimensional Generalized Gini indices. *Economic Theory*, *26*, 471–496.

Gibbard, A. (1973). Manipulation of voting schemes: A general result. *Econometrica*, *41*, 587–601.

Gibbard, A., Hylland, A., & Weymark, J. A. (1987). Arrow's Theorem with a fixed feasible alternative. *Social Choice and Welfare*, *4*, 105–115.

Guesnerie, R. (1977). On the direction of tax reform. *Journal of Public Economics*, *7*, 179–202.

Guesnerie, R. (1995). *A contribution to the pure theory of taxation*. Cambridge: Cambridge University Press.

Guesnerie, R., & Laffont, J.-J. (1978). Taxing price makers. *Journal of Economic Theory*, *19*, 423–455.

Guesnerie, R., & Seade, J. (1982). Nonlinear pricing in a finite economy. *Journal of Public Economics*, *17*, 157–179.

Hahn, F. H. (1973). On optimum taxation. *Journal of Economic Theory*, *6*, 96–106.

Haldane, J. B. S. (1955). Population genetics. In M. L. Johnson, M. Abercrombie, & G. E. Fogg (Eds.), *New biology 18* (pp. 34–51). Harmondsworth, UK: Penguin.

Hamilton, W. D. (1964). The genetical evolution of social behaviour. I and II. *Journal of Theoretical Biology*, *7*, 1–52.

Hammond, P. J. (1976). Equity, Arrow's conditions, and Rawls' difference principle. *Econometrica*, *44*, 793–804.

Harsanyi, J. C. (1953a). Cardinal utility in welfare economics and in the theory of risk-taking. *Journal of Political Economy*, *61*, 434–435.

Harsanyi, J. C. (1953b). Welfare economics of variable tastes. *Review of Economic Studies*, *21*, 204–213.

Harsanyi, J. C. (1955). Cardinal welfare, individualistic ethics, and interpersonal comparisons of utility. *Journal of Political Economy*, *63*, 309–321.

Harsanyi, J. C. (1975). Nonlinear social welfare functions: Do welfare economists have a special exemption from Bayesian rationality? *Theory and Decision*, *6*, 311–332.

Harsanyi, J. C. (1977). Non-linear social welfare functions: A rejoinder to Professor Sen. In R. E. Butts & J. Hintikka (Eds.), *Foundational problems in the special sciences* (pp. 293–296). Dordrecht: D. Reidel.

Heller, W. P., & Shell, K. (1974). On optimal taxation with costly administration. *American Economic Review, Papers and Proceedings*, *64*, 338–345.

Hellwig, M. (1986). The optimal linear income tax revisited. *Journal of Public Economics*, *31*, 163–179.

Inman, R. P. (1971). Towards an econometric model of local budgeting. *Proceedings of the Annual Conference of the National Tax Association*, *64*, 699–719.

Kolm, S.-C. (1998). *Justice and Equity*. Cambridge, MA: MIT Press. Originally published in French in 1972.
Krantz, D., Luce, R. D., Suppes, P., & Tversky, A. (1971). *Foundations of measurement, Volume I: Additive and polynomial representations*. New York: Academic Press.
Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
Kuhn, T. S. (1977). Objectivity, value judgment, and theory choice. In T. S. Kuhn (Ed.), *The essential tension* (pp. 320–339). Chicago: University of Chicago Press.
Laffond, G. (1980). *Révelation des preferences et utilités unimodales*. Thèse pour le doctorat, Laboratoire d'Econométrie, Conservatoire National des Arts et Métiers.
Le Breton, M., & Sen, A. (1999). Separable preferences, strategyproofness, and decomposability. *Econometrica*, *67*, 605–628.
Le Breton, M., & Weymark, J. A. (1996). An introduction to Arrovian social welfare functions on economic and political domains. In N. Schofield (Ed.), *Collective decision-making: Social choice and political economy* (pp. 25–61). Boston: Kluwer Academic Publishers.
Le Breton, M., & Weymark, J. A. (1999). Strategy-proof social choice with continuous separable preferences. *Journal of Mathematical Economics*, *32*, 47–85.
Le Breton, M., & Weymark, J. A. (2011). Arrovian social choice theory on economic domains. In K. J. Arrow, A. K. Sen, & K. Suzumura (Eds.), *Handbook of social choice and welfare Vol.1*, (pp. 191–291). Amsterdam: North-Holland.
Lerner, I. M. (1968). *Heredity, evolution and society*. San Francisco: W. H. Freeman.
Lollivier, S., & Rochet, J.-C. (1983). Bunching and second-order conditions: A note on optimal tax theory. *Journal of Economic Theory*, *31*, 392–400.
Luce, R. D., & Raiffa, H. (1957). *Games and decisions: Introduction and critical survey*. New York: Wiley.
Marschak, J. (1950). Rational behavior, uncertain prospects, and measurable utility. *Econometrica*, *18*, 111–141.
Maskin, E. S. (1976). *Social welfare functions for economics*. Unpublished manuscript, Darwin College, Cambridge University and Department of Economics, Harvard University.
Michod, R. E., Viossat, Y., Solari, C. A., Hurand, M., & Nedelcu, A. M. (2006). Life-history evolution and the origin of multicellularity. *Journal of Theoretical Biology*, *239*, 257–272.
Mirrlees, J. A. (1971). An exploration in the theory of optimum income taxation. *Review of Economic Studies*, *38*, 175–208.
Mirrlees, J. A. (1976). Optimal tax theory: A synthesis. *Journal of Public Economics*, *6*, 327–358.
Morelli, M., Yang, H., & Ye, L. (2012). Competitive nonlinear taxation and constitutional choice. *American Economic Journal: Microeconomics*, *4*, 142–175.
Morreau, M. (2015). Theory choice and social choice: Kuhn vindicated. *Mind*, *493*, 239–262.
Morreau, M., & Weymark, J. A. (2016). Measurement scales and welfarist social choice. *Journal of Mathematical Psychology*, *75*, 127–136.
Moulin, H. (1980). On strategy-proofness and single peakedness. *Public Choice*, *35*, 437–455.
Okasha, S. (2006). *Evolution and the levels of selection*. Oxford: Oxford University Press.
Okasha, S. (2009). Individuals, groups, fitness and utility: Multi-level selection meets social choice theory. *Biology and Philosophy*, *24*, 561–584.
Okasha, S. (2011). Theory choice and social choice: Kuhn versus Arrow. *Mind*, *477*, 83–115.
Okasha, S. (2012). Social justice, genomic justice and the veil of ignorance: Harsanyi meets Mendel. *Economics and Philosophy*, *28*, 43–71.
Okasha, S., Weymark, J. A., & Bossert, W. (2014). Inclusive fitness maximization: An axiomatic approach. *Journal of Theoretical Biology*, *350*, 24–31.
Piketty, T. (1993). Implementation of first-best allocations via generalized tax schedules. *Journal of Economic Theory*, *61*, 23–41.
Quiggin, J. (1982). A theory of anticipated utility. *Journal of Economic Behavior and Organization*, *3*, 323–343.
Rádo, F., & Baker, J. A. (1987). Pexider's equation and aggregation of allocations. *Aequationes Mathematicae*, *32*, 227–239.

Rawls, J. (1958). Justice as fairness. *Philosophical Review*, *67*, 164–194.
Rawls, J. (1971). *A Theory of Justice*. Cambridge, MA: Harvard University Press.
Rescher, N. (1975). *Unselfishness: The role of the vicarious affects in moral philosophy and social theory*. Pittsburgh: University of Pittsburgh Press.
Risse, M. (2002). Harsanyi's 'utilitarian theorem' and utilitarianism. *Noûs*, *36*, 550–577.
Roberts, K. (1995). Valued opinions or opinionated values: The double aggregation problem. In K. Basu, P. Pattanaik, & K. Suzumura (Eds.), *Choice, welfare, and development: A festschrift in Honour of Amartya K. Sen* (pp. 141–165). Oxford: Oxford University Press.
Roberts, K. W. S. (1977). Voting over income tax schedules. *Journal of Public Economics*, *8*, 329–340.
Röell, A. (2012). *Voting over nonlinear income tax schedules*. Unpublished manuscript, School of International and Public Affairs, Columbia University.
Rothschild, M., & Stiglitz, J. E. (1970). Increasing risk: I. A definition. *Journal of Economic Theory*, *2*, 225–243.
Rubinstein, A. (1982). Perfect equilibria in a bargaining model. *Econometrica*, *50*, 97–109.
Samuelson, P. A. (1967). Arrow's mathematical politics. In S. Hook (Ed.), *Human values and economic policy: A symposium* (pp. 41–51). New York: New York University Press.
Satterthwaite, M. A. (1975). Strategy-proofness and Arrow's conditions: Existence and correspondence theorems for voting procedures and social welfare functions. *Journal of Economic Theory*, *10*, 187–217.
Sen, A. K. (1970). *Collective choice and social welfare*. San Francisco: Holden-Day.
Sen, A. K. (1973). *On economic inequality*. Oxford: Clarendon Press.
Sen, A. K. (1974). Informational bases of alternative welfare approaches: Aggregation and income distribution. *Journal of Public Economics*, *3*, 387–403.
Sen, A. K. (1976). Welfare inequalities and Rawlsian axiomatics. *Theory and Decision*, *7*, 243–262.
Sen, A. K. (1977). Non-linear social welfare functions: A reply to Professor Harsanyi. In R. E. Butts & J. Hintikka (Eds.), *Foundational problems in the special sciences* (pp. 297–302). Dordrecht: D. Reidel.
Simula, L. (2010). Optimal nonlinear income tax and nonlinear pricing: Optimality conditions and comparative static properties. *Social Choice and Welfare*, *35*, 199–220.
Stern, N. (1982). Optimum taxation with errors in administration. *Journal of Public Economics*, *17*, 181–211.
Stigler, G. J. (1964). A theory of oligopoly. *Journal of Political Economy*, *72*, 44–61.
Suppes, P. (1966). Some formal models of grading principles. *Synthese*, *6*, 284–306.
Tirole, J., & Guesnerie, R. (1981). Tax reform from the gradient projection viewpoint. *Journal of Public Economics*, *15*, 275–293.
Tulkens, H., & Zamir, S. (1979). Surplus-sharing local games in dynamic exchange economies. *Review of Economic Studies*, *46*, 305–313.
von Neumann, J., & Morgenstern, O. (1944). *Theory of games and economic behavior*. Princeton: Princeton University Press.
Weintraub, E. R. (1979). *Microfoundations: The compatibility of microeconomics and macroeconomics*. Cambridge: Cambridge University Press.
Weymark, J. A. (1978a). On Pareto-improving price changes. *Journal of Economic Theory*, *19*, 338–346.
Weymark, J. A. (1978b). 'Unselfishnness' and prisoner's dilemmas. *Philosophical Studies*, *34*, 417–425.
Weymark, J. A. (1979a). Optimality conditions for public and private goods. *Public Finance Quarterly*, *7*, 338–351.
Weymark, J. A. (1979b). A reconciliation of recent results in optimal taxation theory. *Journal of Public Economics*, *12*, 171–189.
Weymark, J. A. (1981a). Generalized Gini inequality indices. *Mathematical Social Sciences*, *1*, 409–430.

Weymark, J. A. (1981b). Undominated directions of tax reform. *Journal of Public Economics*, *16*, 343–369.
Weymark, J. A. (1986a). Bunching properties of optimal nonlinear income taxes. *Social Choice and Welfare*, *2*, 213–232.
Weymark, J. A. (1986b). A reduced-form optimal nonlinear income tax problem. *Journal of Public Economics*, *30*, 199–217.
Weymark, J. A. (1987). Comparative static properties of optimal nonlinear income taxes. *Econometrica*, *55*, 1165–1185.
Weymark, J. A. (1991). A reconsideration of the Harsanyi-Sen debate on utilitarianism. In J. Elster & J. E. Roemer (Eds.), *Interpersonal comparisons of well-being* (pp. 255–320). Cambridge: Cambridge University Press.
Weymark, J. A. (1999). Decomposable strategy-proof social choice functions. *Japanese Economic Review*, *50*, 343–355.
Weymark, J. A. (2004). Shared consumption: A technological analysis. *Annales d'Économie et de Statistique*, *75–76*, 175–195.
Weymark, J. A. (2005a). Measurement theory and the foundations of utilitarianism. *Social Choice and Welfare*, *25*, 527–555.
Weymark, J. A. (2005b). Shadow prices for a nonconvex public technology in the presence of private constant returns. In U. Schmidt, & S. Traub (Eds.), *Advances in public economics: Utility, choice, and welfare. A festschrift for Christian Seidl*, (pp. 61–71). Springer, Dordrecht.
Weymark, J. A. (2010). Alvorado lá no morro, que beleza. *Exame CEO*, *5*, 52–55.
Wibaut, S. (1989). *Tax reform in disequilibrium economies*. Cambridge: Cambridge University Press.