Peter J. Hammond

Philippe Mongin

Philippe Mongin (**PM**): *The important contributions you made to social choice theory, welfare economics, and social ethics are knit together by some common theoretical ideas, which I would suggest we take up successively in this discussion. I have selected in order, interpersonal comparisons of utility, utilitarianism, consequentialism, welfare economics under uncertainty, and incentive compatibility. Would you agree with this list and this order? What could come first, however, is some kind of intellectual biography. Perhaps you could explain to us how you first became interested in these topics and how they permeated your later academic life.*

Peter Hammond (**PJH**): Following your excellent overall plan, let us start with how I became interested in social choice and welfare, as well as in their application to public economics.

Actually, my interest in economics began in 1964 during the first year of my studies in Cambridge for the Mathematics Tripos. I was at Trinity Hall, where Geoff Harcourt was the Director of Studies for Economics. At a tea party early that October, he asked me when I was going to switch to economics, to which my reply was that I had no interest in doing so. But soon after, the Labour Party won a general election, with Harold Wilson succeeding Alec Douglas-Home as Prime Minister. The UK had a significant balance of trade deficit, and the new government was faced with a sterling crisis that Wilson blamed on the "gnomes of Zurich"—i.e., Swiss currency speculators. Devaluation of the pound was forestalled for three years, but that autumn I could not understand what I was reading in the newspapers about currency speculation and the measures that might alleviate the deficit.

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So I went into Heffer's Paperback Bookshop to look for an economics book I could afford, and bought the one which had the lowest price per page. This happened to be Paul Samuelson's *Economics*, his famous elementary textbook, which I read during the Easter vacation in 1965. At about this time, a college friend who knew of my interest in games such as chess and bridge, as well as cricket and other ball games, recommended that I read a book he had found in the library. This was another classic: von Neumann and Morgenstern's *Theory of Games and Economic Behavior*. Over the coming years I remember occasional forays into some old-fashioned works on mathematical economics such as that by R.G.D. Allen. There was also Ian Little's *Critique of Welfare Economics* which was too philosophical, I would guess, for my untutored taste at that time.

By 1966, I had several college friends studying for the Natural Sciences Tripos who were tiring of spending every afternoon in a laboratory. In those days, completing two years of Natural Sciences was enough for an honours degree provided you did some "diligent study" during your third year. So many of these friends switched to economics, and seemed to be enjoying it. Accordingly, I applied for a government funded scholarship to start studying economics myself in 1967/68, with a view to completing a Ph.D. degree in due course. My application was accepted, subject to not disgracing myself in my final year Maths exams, which I just about managed. Nick Stern, by the way, started along the same career path, though at a higher level of achievement.

After spending the summer vacation of 1967 working for a software startup company, my formal study of economics began that autumn. Cambridge offered me the privilege of being able to attend lectures by distinguished academic economists such as Michael Farrell, who taught not only basic econometrics but also the fundamental efficiency theorems of welfare economics, based on the first of Tjalling Koopman's *Three Essays on the State of Economic Science.* David Champernowne taught statistics. There were also Joan Robinson's lectures on growth and capital accumulation, in the course of which she taught us always to be clear about all our assumptions. Her teaching and writings were the inspiration for my essay on the assumptions of "contemporary neoclassical economic theology" (Hammond 1989).

I also learned much from Christopher Bliss's lectures on general equilibrium theory, and even more from his weekly supervisions. After starting with Keynes' really difficult *General Theory*, Chris had asked me next to read the much cited paper on the theory of second best by Lipsey and Lancaster (1956). He also hinted at feeling some dissatisfaction with it, and this encouraged me to find a counter-example to Lipsey and Lancaster's negative conclusions in the case of, for example, limits to free trade based on quotas rather than tariffs. Chris's dissatisfaction arose, I suspect, because he was aware of Diamond and Mirrlees' (1971) work on optimal taxation, already circulating in preprint form, which showed the desirability of production efficiency even in a second-best world where one has to use distortionary commodity taxes instead of first-best lump-sum redistribution.

The most important influence, however, was Jim Mirrlees. He gave lectures on public economics that included ideas that emerged eventually in his joint two-part paper with Peter Diamond on optimal taxation, as well as other advanced lectures

on optimal growth theory. After Mirrlees moved to Oxford in 1968, that autumn I regularly attended mathematical economics seminars there to which Jim had invited me. Robert Solow, who was visiting Oxford at that time, was one of several prominent participants.[1] With Jim's powers of persuasion over the other economics fellows of Nuffield College, and Christopher Bliss's strong encouragement, I moved there one year later.

Soon after I arrived in Oxford, Jim passed on some conjectures involving an idea of his which helped resolve, in some special cases, the non-existence of an optimal growth path for an infinite horizon. I suspect he already had a clear idea of how to prove them, but I was encouraged to provide proofs anyway. They became incorporated into our joint paper on "Agreeable Plans" (Hammond and Mirrlees 1974). A few months later, Jim presented this to an IEA conference on Economic Growth, held during 1970 in Jerusalem, whose proceedings later appeared in the conference volume (Mirrlees and Stern 1974). Eventually agreeable plans were the subject of a considerable part of my Cambridge Ph.D. thesis on *Consistent Planning and Intertemporal Welfare Economics* (Hammond 1973).

PM: *Concerning our first topic, interpersonal comparisons of utility (ICUs), you stated that social choice theory would not have developed fruitfully (and would have remained a "science of the impossible", as you once wrote) if it had remained at the stage of Arrow's social welfare function (SWF), which excludes collective preferences that rely on ICUs (*Hammond 1987b*). Two papers of yours (*Hammond 1976a, 1979b*) modify the SWF concept to allow for comparisons of welfare levels and axiomatize concepts of equity in the Rawlsian sense. In later discussions, you consider other ways of introducing ICUs, for instance Sen's social welfare functionals, which permit enlarging the scope of ICUs beyond mere level comparisons. Here again, we could make a historical start and begin with the way your ideas on ICUs developed in the 1970s, when social choice theory began reconfiguring Arrow's work.*

PJH: While in Oxford, Jim Mirrlees asked me to look at the typescript version of what became Prasanta Pattanaik's book *Voting and Collective Choice* (Pattanaik 1971), which he had submitted to support his application for a senior research fellowship at Nuffield. It was a pleasure to read. Occasionally I would have a question, only to find it clearly answered in the next paragraph. Prasanta arrived at Nuffield during the academic year 1970/71, and gave a weekly lecture on social choice theory there. The focus was definitely on Arrow's *Social Choice and Individual Values* (Arrow 1951a) which I understood much better after attending Prasanta's lectures. Prasanta, perhaps influenced by meeting Allan Gibbard at Harvard, also introduced me to the idea of strategic voting (Gibbard 1973) and to Yasusuke Murakami's *Logic and Social Choice* (Murakami 1968). He also mentioned a monotonicity condition for sincere voting somewhat like Arrow's positive association. This had some resemblance to the monotonicity condition that Eric Maskin later made famous—see Dasgupta et al. (1979) and Maskin (1999).

[1] Some years after the 25th anniversary of this seminar, Michael Bacharach, Michael Dempster, and John Enos together edited a celebratory online volume.

My two years in Oxford also offered me the opportunity to meet for the first time some other prominent academics such as Amartya Sen and Joe Stiglitz, as well as the political scientist Brian Barry. I was also a friend of Iain McLean, another political scientist, who has since produced some fine work on the early history of social choice theory (MacLean and Hewitt 1994; MacLean and Urken 1995). In 1971, after my own two-year junior research fellowship at Nuffield had ended, I had the privilege of joining Christopher Bliss and Tony Atkinson at the University of Essex, where they had been recruited after Richard Lipsey and other founders of the Economics Department there had moved on. Essex is also where I first met Kevin Roberts, whose exceptional promise was already clear while he was completing his B.A. degree in mathematical economics.

Anyway, in the autumn of 1971 I read Amartya Sen's important book *Collective Choice and Social Welfare* (Sen 1970a). I had already seen his *Econometrica* paper on interpersonally comparable utilities, in which I found a minor mistake, duly acknowledged and corrected shortly thereafter (Sen 1970c, 1972b). I was prompted to start reading the books by Hare (1952, 1963, 1981), the noted Oxford philosopher, and discovering his major thesis that ethics was about universalizable prescriptive statements. And Chaps. 9 and 9* in Sen (1970a) usefully elaborate the key idea of an extended preference ordering over the Cartesian product of ordered pairs (x, i) that combine a social state x with an individual i. The idea of considering such pairs had already been taken up somewhat informally in the second edition of *Social Choice and Individual Values* (Arrow 1951a) (under the name "extended sympathy"), and more formally by Suppes (1966). In the case of two pairs (x, i) and (y, j) where individuals i and j differ, any preference between the two involves an ICU.

By my third year as a Lecturer at Essex, I had finally submitted my Ph.D. thesis (Hammond 1973). Before the examiners Terence (W.M.) Gorman and Geoffrey Heal passed it in 1974, Essex also granted me tenure as a Lecturer, which was possible in those days. During that academic year 1973/74, I was also allowed to teach a master's course in public economics. From the work of Diamond, Mirrlees, Atkinson, Dasgupta, Stiglitz and others, not to mention optimal growth theory, I had got used to writing down a utilitarian Bergson social welfare function to represent the objective of economic policy. Yet Arrow's theorem seemed to have converted politics from Bismarck's "art of the possible" to the "science of the impossible", to use the phrase in Hammond (1987b) that you have kindly recalled. I was also aware of John Rawls' *Theory of Justice*, and even his prior article on "Justice as Fairness" (Rawls 1958, 1971).

Several discussions and even criticisms of Arrow's theorem up to the early 1970s had focused on his condition of independence of irrelevant alternatives (IIA). This condition, which excludes the Borda rule and other similar SWFs based on rank order, is indeed crucially important, but it may not be the root of why avoiding dictatorship seemed so difficult. Chapters 9 and 9* of Sen (1970a) suggest that if one is to arrive at a non-dictatorial solution, in particular a Rawlsian maximin social ordering, or any basic notion of equity, the first thing to do is to add more information in the form of an extended preference ordering. Alternatively, a social welfare functional (SWFL), in the sense also explained in Sen's book, can embody this extra informa-

tion. This information is compatible with a modified form of IIA, something like "independence of irrelevant interpersonal comparisons", to use the title of one of my later papers (Hammond 1991b), or to be more precise, "independence of interpersonal comparisons involving irrelevant alternatives". The Rawlsian maximin and its leximin extension satisfy this new version of IIA, but of course other axioms were needed for it to be fully characterized.

One of these axioms, whose role is to arrive at an equitable rule rather than the inequitable alternative of maximax, came to be known in the literature as "Hammond equity". It is the key postulate of the paper Hammond (1979b) which you kindly mentioned, with its improved characterizations of both Rawlsian maximin and leximin. The idea, however, is essentially already set out in Sen's short book *On Economic Inequality* (Sen 1973), if not in the earlier works of Pigou (1920) and Dalton (1920) claiming that progressive income transfers from rich to poor would increase social welfare.

I was fortunate that some preprint version of my 1976 paper had come to Ken Arrow's attention. The result was that, along with related work by Strasnick (1976) and the more general ideas of D'Aspremont and Gevers (1977), it was cited in an American Economic Association *Papers and Proceedings* issue (Arrow 1977). Arrow was right to recall the logical link to his own discussion of "extended sympathy".

PM: *As you just mentioned, d'Aspremont and Gevers were among the important contributors of these years. Their 1977 article offers a joint characterization of leximin and utilitarianism (D'Aspremont and Gevers* 1977*). To emphasize the duality of these rules is typical of the Belgian school of social choice; this comes out even more clearly in a paper (Deschamps and Gevers* 1978*) which shows that their axioms leave essentially no choice beside leximin and utilitarianism. Perhaps you could briefly assess this alternative treatment of leximin?*

PJH: Thank you indeed for reminding me of this important piece of research. Louis Gevers in particular was a good friend who was lost to us far too early. To be frank, I regarded two papers that Roberts (1980a, b) published in the same issue of the *Review of Economic Studies* as the culmination of the work during the 1970s on ICUs and their role in SWFLs. One of the lessons which emerged was that there are many ways of avoiding Arrow's impossibility theorem once one introduces the extra information required to make ICUs. It is nice to have appealing axioms that reduce the possibilities to a choice between leximin and utilitarianism. How to make this final choice, however, seems to require some deeper principles that, amongst others, I have explored in more recent work.

PM: *Now that we have discussed the rediscovery of ICUs by social choice theorists and your role in it, I would like to ask you about some of their theoretical underpinnings. Obviously, you regard them as being meaningful and feasible, unlike the new welfare economists, and at least in a common interpretation, Arrow himself, but your 1991 survey (*Hammond 1991a*) stresses that their empirical and ethical significance is often unclear. I wonder how this scepticism is compatible both with your early work on equity and your later endorsement of utilitarianism.*

PJH: First, I should probably try to clear up a common misunderstanding. It is that Lionel Robbins wrote that economists should not make interpersonal comparisons of

utility. But what he actually wrote, at least in his later comment (Robbins 1938), was that such comparisons lack "scientific foundations". One interpersonal comparison that I have made in writing is that the few extra euros that a rich person might spend on a better bottle of wine are worth less than the same amount of money that a poor mother might spend on medicine that could save the life of her desperately sick child. Such a comparison is not purely factual; it has significant ethical content precisely because it crosses the divide between facts and values that British philosophers know as Hume's Law. Thus, I agree with Robbins when he writes that ICUs are "more like judgments of value than judgments of verifiable fact".

Sen's framework of SWFLs had played such a key constructive role in the developments of the 1970s. Nevertheless, by the time of my 1991 selective survey of ICUs and their use in social choice and welfare economics, I was becoming increasingly disenchanted with this framework. I think that my disquiet became even clearer when preparing my conference talk (Hammond 1996b) that emerged later as "Consequentialist Decision Theory and Utilitarian Ethics". The part of Sen's framework — which I was also using for at least a decade — that I was questioning concerned his treatment of individuals' utility functions as somehow basic, with comparisons of different individuals' utility functions added on as some sort of superstructure. Yet in the original position arguments used by Vickrey and Harsanyi, a person's utility is really just an estimate of the expected ethical value of becoming that individual upon emerging from what Rawls (1971) later called the "veil of ignorance".

Now, my favourite approach to constructing a utilitarian social objective is one that extends Adam Smith's "impartial spectator", who judges the welfare of single individuals, to an impartial benefactor who makes decisions for society as a whole. In this approach, it turns out that the most useful concept of individual utility is one that builds in social choice in original positions right from the start. Indeed, it seems best to consider "generalized" original positions with different specified hypothetical probabilities of becoming different individuals in society, as well as decision problems where even these hypothetical probabilities can be chosen.

PM: *You just described two theoretical moves, one away from Sen's social welfare functionals, and a move towards utilitarianism. The two moves are of course logically distinct. Is it a biographical accident that you made these two moves simultaneously, or is there a deeper reason?*

PJH: Well, Rawls introduced a different concept of the original position when arguing for his "difference principle", leading to the maximin or leximin SWFL. Of course Harsanyi (1974) famously criticized this as an inappropriate way of making decisions in the original position. Harsanyi maintained that, in the original position, one should maximize expected utility. The work I have done on rational choice reflects me to agree with Harsanyi here, though I do want to use a different concept of utility.

PM: *Another possible line about ICUs is that they are not the only possible interpersonal comparisons to make if one is to assess economic states of affairs. In a 2001 survey of ICUs written with Fleurbaey, you raise the question "interpersonal comparisons of what?" (Fleurbaey and Hammond 2004). Relatedly, Fleurbaey and Maniquet (2011, 2017) have thoroughly explored equity criteria based on the mere*

knowledge of the indifference curves, a direction that, as Fleurbaey and I later found out (in Fleurbaey and Mongin 2005), had already been taken by welfare economists whose work was obfuscated by Arrow's and Sen's successes. Could you possibly comment on this typical example of interpersonal comparisons that are not of utility? And perhaps also say what you think of its prolonged dismissal by social choice theory?

PJH: The *Handbook of Utility Theory* that I co-edited with Salvador Barberà and Christian Seidl needed a chapter on ICUs. Yet I did not want merely to repeat what I had done in 1991. So I was very happy when Marc Fleurbaey agreed to join me as a co-author of the relevant chapter (Fleurbaey and Hammond 2004). The title "Interpersonally Comparable Utility" was carefully chosen with the intention of conveying the message that, rather than seeing ICUs as comparing different components of an independently constructed profile of individual utility functions, it might be better to think of this profile as being constructed right from the start so that it is "interpersonally comparable", with the eventual use of those comparisons in a social ordering very much in mind.

Before assessing the thought provoking contributions of Fleurbaey and Maniquet (2011, 2017), let us first go back to the work on SWFLs during the 1970s. D'Aspremont and Gevers (1977) proved that if a SWFL satisfies the conditions of unrestricted domain, independence of irrelevant alternatives, and Pareto indifference, then it must be welfarist—i.e., in a society of n individuals, one can represent it by a single preference ordering over the n-dimensional Euclidean space of vectors of personal welfare levels. Roberts (1980b) formulated a similar result that uses only the weak Pareto condition, though as I pointed out later (Hammond 1999b), his "weak continuity" assumption needs some slight strengthening to a condition I called "pairwise continuity". Anyway a welfarist SWFL, by definition, can use information only about welfare levels. This forces the ICUs to be independent of other considerations such as fairness. Kaplow and Shavell (2001, 2002, 2003), in their book and associated articles, offer a rather more contentious version of a similar point—namely, that imposing a condition like fairness is likely to violate the Pareto principle if one remains within a welfarist framework.

Now, social choice theorists have generally invoked an assumption such as an unrestricted domain of finite decision trees, or at least a sufficiently unrestricted domain. This plays a crucial role in those old justifications of welfarism. My own arguments for utilitarianism also put no restrictions on the impartial benefactor's judgements regarding the expected utility ascribed to the different personal consequences faced by each individual in the original position. Fleurbaey and Maniquet (2011, 2017), however, consider restricted domains of social choice problems such as those that involve dividing an aggregate bundle of several different commodities among a set of individuals. They consider social choice procedures which select allocations that maximize a Paretian welfare ordering which, as with Arrow social welfare functions, depends only on the individuals' preference orderings. Unlike Arrow, however, they not only consider a restricted domain of social choice problems, but they also relax independence of (preferences for) irrelevant alternatives. This allows their procedure to generate fair rather than dictatorial allocations. The various notions of fairness that

they consider do depend on interpersonal comparisons of utility levels, of course, and these comparisons can take irrelevant alternatives into account. It is an interesting alternative to welfarist rules like utilitarianism.

Nevertheless, I cannot help wondering how well the kind of procedure they consider would perform in a dynamic or multi-period setting. After all, there can then be continuation sub-problems. In these, dependence on alternatives that have become irrelevant because they are no longer feasible may create difficulties of the kind discussed in my paper on "metastatic" choice (Hammond 1977). The difficulty is illustrated by the sensitivity of the Borda rule, or similar ranking rules, to the feasible set of options to which it is applied. It is also somewhat similar to the difficulties created in decision trees when an individual seeks to maximize a preference ordering which violates the independence axiom and which therefore may be represented by some "non-expected" utility function (Hammond 1988a, b).

Let us return, however, to the vexed question of whether maximizing the sum of individuals' expected utilities is somehow inimical to equity. This is what Diamond (1967a) argued in his famous comment on Harsanyi (1955). Obviously considering only total utility pays no attention to its distribution between different individuals. This neglect would have worried John Rawls even more than Peter Diamond, I suppose. But this valid criticism does not imply that a utilitarian is committed to ignore equity.

Before explaining this, it may be useful to consider some of Amartya Sen's other writings, apart from those that introduced the notion of an SWFL. Notably, he introduced rights into social choice theory (Sen 1970b). He also put forward the concepts of "capabilities and functionings" as what I would regard as important components of an ethically satisfactory notion of individual well-being (Sen 1985). Now, apart from any concern over the proliferation of concepts, there is one other worry, which I have written about in the case of rights (Hammond 1997). This is the thought that rights should matter only to the extent that they benefit the individuals who have them. But then, if rights really are beneficial, should they not be included in an appropriate measure of individual well-being that an impartial benefactor should be using? Pushing this idea far enough suggests that, despite our concern for fairness, we might still want to follow utilitarians at least to the extent of considering only utility, suitably measured, as well as interpersonal comparisons that are restricted to ICUs.

This brings us at last to my main point regarding Fleurbaey and Maniquet, and their new concepts of equity that you mention. Let us first recognize that as one increases the impartial benefactor's "consequence risk aversion" in the relevant original position, in optimal distribution problems that will tend to increase equality among first-best levels of utility. But our ethics may still suggest that there should be more equality than the level of consequence risk aversion on its own implies. In this case it may be more appropriate to emulate the treatment of rights suggested above, and include some measure of equity, reflecting possibly equality of both outcomes and opportunities, among each relevant consequence that affects individual welfare. Essentially the same idea has been discussed by, amongst others, Broome (1990, 1991) and Machina (1989). This line of argument reminds me, however, of Arrow's

own "non-imposition" condition, as stated in his original "A Difficulty" paper, as well as subsequently in the first edition of *Social Choice and Individual Values* (Arrow 1950, 1951a). Overall, I am led to wonder if we should not refuse to give any special treatment to social *desiderata* such as equity or respect for rights unless we deem them to be inherently desirable for individuals. But in this case having more of these *desiderata* should increase any ethically relevant measure of individual "well-being" or utility.[2]

PM: *It is perhaps time to move on to the related questions of utilitarianism. When it comes to this, you offer both formal derivations and a philosophical endorsement. I see two main treatments, one you gave in 1982 and 1983 in connection with the ex ante versus ex post question, to be discussed later, and another from 1992, which I am focusing on now (*Hammond 1982, 1983, 1992*). This latter treatment is essentially the same as Harsanyi's; he is the theorist who perhaps had most influence on your work. You encapsulate an application of Harsanyi's Aggregation Theorem in a decision-theoretic framework of your own, but if this theorem has no ethical relevance, as Sen and others have complained, how would justify your more complex construction? Do you not think that you should defend Harsanyi in the first place?*

PJH: You are surely right in remarking that Harsanyi is the theorist who had most influence on my utilitarian views. Indeed, Amartya Sen once chided me (rather mildly) for my "dangerous fascination with Harsanyi". Quite late in life, Harsanyi did not demur when I suggested that a route to rule utilitarianism, which of course he strongly endorsed, might pass through some concept of "rule consequences", appropriately defined.

But let me now turn to your question regarding Harsanyi's aggregation theorem, and the criticisms of Harsanyi's version of utilitarianism that Sen and Pattanaik, amongst others, have made of it—criticisms, by the way, that Harsanyi seemed unaware of when Claude d'Aspremont and I interviewed him in Caen for *Social Choice and Welfare* (D'Aspremont and Hammond 2001). This is rather surprising unless one recognizes that this could have been an early sign of the illness that eventually took him from us. When I was asked to contribute to the volume of *Essays in Honor of John Harsanyi* that his friend and co-author Reinhard Selten was editing, I chose to offer an alternative proof of the aggregation theorem using the theory of linear programming, and did not provide anything more than the briefest reference to my criticisms of his version of utilitarianism (Hammond 1992).

In discussing Harsanyi's concept of utility, however, let us begin with William Vickrey. In barely one—breathlessly long—sentence on page 329 of Vickrey (1945), he imagines the case where each individual has their own VNM utility function, and the benevolent (or beneficent?) social planner is asked to maximize the expected value of an even chance lottery whose risky states of the world correspond to the individuals in the society. Vickrey, after all, was interested in measuring the marginal utility of

[2]Recently, Fleurbaey and Maniquet (2018, p. 1031) seem to recognize this possibility when they write as follows: "Note that our defense of the social welfare function could even be understood as a defense of the utilitarian approach, for an ecumenical notion of utilitarianism that is flexible about the degree of inequality aversion and the definition of individual utility."

income, or more precisely, the marginal rate of substitution between different individual's incomes. So for him, each individual's VNM utility function depends only on observable "objective" circumstances such as income, educational qualifications, date of birth, marital status, etc. One sentence, however, did not offer Vickrey the scope to explore in any depth the ethical foundation of the individuals' VNM utility functions—though he did take up the subject again in his paper on "social decision rules" (Vickrey 1960). This foundation is what Harsanyi (1953, 1955, 1976, 1977) attempted to provide.

An obvious difficulty is that each individual really has at best a unique cardinal equivalence class of VNM utility functions—equivalence meaning being related by an increasing affine transformation, so that ratios of utility differences represent constant marginal rates of substitution between appropriate probability shifts. As Pattanaik (1968) in particular has pointed out, even if one fixes for each individual one particular VNM utility function within their own cardinal equivalence class, these functions will typically have to be weighted before being added. Mathematically, this is identical to the problem that arises in extending the Savage (1954) and, perhaps more precisely, the Anscombe and Aumann (1963) theory of subjective probability and subjective expected utility (SEU) to the case when there is a separate consequence domain associated with each state of the world. In my FUR 1997 conference paper (Hammond 1999a) on state-independent utility on state-dependent consequence domains, I show that this problem can be resolved if one contemplates decision problems where the agent can choose the probabilities of the different states of the world. Similarly, I think the Harsanyi problem can be resolved by contemplating the choice of generalized original positions with different probabilities of becoming different individuals in the society.

Another weakness of Harsanyi's approach, and of Vickrey's too, I suppose, comes back to my doubt that one should start by postulating a given profile of individual utility functions, rather than deriving them from suitable ethical decision problems as we try to do in Chichilnisky et al. (2018).

PM: *Am I correct in saying that this amounts to vindicating Harsanyi's Impartial Observer Theorem, as against the Aggregation Theorem we just discussed?*

PJH: What I am trying to vindicate is his use of an original position and of a VNM utility function in that position in order to arrive at the ethical decision criterion of what Harsanyi called the "impartial observer", though perhaps one should think instead of a more active "impartial benefactor".

While we are discussing Harsanyi, perhaps I may mention another significant disagreement I have with him, which concerns his view that population should be chosen to maximize average utility per head of population. Both in my Ph.D. thesis and the later *Social Choice and Welfare* paper on consequentialist demographic norms (Hammond 1973, 1988c), I have criticized this criterion as yielding dynamically inconsistent preferences—unless, that is, you keep track of the entire population of humanity all the way back to the origin of *homo sapiens*. Though one wonders if even this is far enough back given recently discovered DNA evidence that our ancestors interbred with Neanderthals, who in turn interbred with Denisovans. Anyway, with an obvious assumption that the unchangeable welfare of long forgotten ancestors should

be irrelevant to contemporary decisions, one returns to classical total utilitarianism. This is even compatible with a version of the Vickrey–Harsanyi original position, where one must reckon with some possibility of never coming into existence. Here, by the way, I like to define zero utility as the level ascribed to any person who never comes into existence. This differs from Chuck Blackorby, Walter Bossert and David Donaldson who, in their book *Population Issues in Social Choice Theory* as well as in several earlier research articles, prefer to use a "critical" utility level (Blackorby et al. 2005).

To sum up, in the end I do not really feel any need to defend Harsanyi. But I am very happy to acknowledge being greatly inspired by his (and Vickrey's) appreciation of the importance of using VNM theory in a fundamental way in order first to construct an interpersonally comparable measure of utility, and then to compare social states based on their expected utilities in suitable even chance lotteries. Also, Harsanyi's (1974) famous criticism of Rawls's view of the decision-maker's extreme risk aversion in the original position remains very much to the point, as well as the subject of Hammond (1975b) on "extreme inequality aversion".

Finally, I found myself wanting to refute the claim in Myerson (1985) that "interpersonal comparisons of utility cannot be given decision theoretic significance". So, in our *Handbook* chapter on "Interpersonally Comparable Utility" (Fleurbaey and Hammond 2004), we refer to Harsanyi (1987), which opens with the example of choosing which friend should receive an opera ticket one could not use.

PM: *If I may linger on the issues here, Harsanyi's utilitarian interpretation of his two theorems runs into two separate difficulties. One is the meaningfulness of the weights in the weighted sum of individual VNM utilities that both theorems derive, and the other is the very choice of VNM utilities to represent individual preferences that are ordinal in nature. Your answer seems to address the former problem, but not the latter, which critics usually regard as being the more worrying. I believe like you that an Anscombe–Aumann reconstruction of the original position may fix the weights—I think each of us has made the point independently—but this takes the VNM representation for granted, so the other problem is still on the table.*

PJH: If I understand correctly, you made the point about fixing the weights in Mongin (2001). By contrast, in our recent working paper (Chichilnisky et al. 2018), Graciela Chichilnisky, Nick Stern and I actually circumvent the state-dependence issue involved in fixing the welfare weights in the original position. We do so by postulating an all-encompassing domain of personal consequences, each of which represents a possible individual life experience, including the possibility of no life at all. This is instead of each individual having their own specific personal consequence domain.

Our formulation allows us to treat ethical decision problems where the number of individuals, as well as their personal characteristics, are risky and affected by the decision being made. Such problems can then include even the hypothetical choice of a partial or biased original position, which implies interpersonal comparisons.

Given the relevant risks, it is natural to use what Kolm (1971, 1994) might call a "fundamental" VNM utility function defined over this all-encompassing domain. An additional equity axiom inspired by Patrick Suppes' famous paper on grading

principles (Suppes 1966) then suggests that, when an impartial benefactor is choosing a world history rather than an original position, it is right to use an unbiased or impartial original position that takes into account the probability of a person coming into existence.

PM: *The decision-theoretic framework involved in our interchange is consequentialism, which we may discuss now. Among moral philosophers, this word applies to those doctrines which define the rightness of an act from the value of its consequences. In your work, especially in your reference article (*Hammond 1988a*), it both applies to an axiomatic reconstruction of decision theory and appears as one salient axiom in this reconstruction. Could I ask you to recapitulate the reconstruction and clarify the semantic connection between your consequentialism and that of the philosophers?*

PJH: Let me precede an answer, if I may, with some more personal background. I have already mentioned attending Joan Robinson's lectures on economic theory that she gave in Cambridge during the 1967/8 academic year. Most likely inspired by an argument from Jan Graaff's book *Theoretical Welfare Economics*, she once pointed out that a difficulty with long-range planning was how to specify the horizon (Graaff 1957). The point is that, however far away the chosen horizon may be, it will eventually be reached, whereupon one will be forced to revise one's original plan. I remember raising my hand and suggesting that recent work on infinite-horizon planning that I had learned about from Mirrlees' lectures showed a possible way round this objection. This interchange does much to explain why *Consistent Planning and Intertemporal Welfare Economics* became the title of my Ph.D. thesis (Hammond 1973), with several of its later chapters devoted to infinite-horizon planning models.

Sometime during the years 1969–71 that I spent at Nuffield College, Graham Pyatt recommended to me the classic paper on myopia and inconsistency by Strotz (1956). Also, at the 1970 World Congress of the Econometric Society in Cambridge, Chuck Blackorby gave a talk in which, according to my possibly imperfect recall, showed that a consumer who maximizes inconsistent preferences at different times will have intertemporal demand functions that violate the Slutsky conditions for consistent preference maximization.[3] That work helped inspire my "Changing Tastes and Coherent Dynamic Choice" (Hammond 1976b), which made a similar point in the framework of choice functions over finite sets that Arrow had used in his 1959 *Economica* article (Arrow 1959). I had also become aware of the concluding part of the second edition of Arrow's book (Arrow 1951a), which set out to justify the collective rationality postulate built into his definition of a SWF. His argument was based on an informal version of the path independence postulate that, in the theory of revealed preference, plays a role in ensuring the integrability of consumer demand functions. Meanwhile, probably while still in Oxford, my supervisor Mirrlees introduced me to the money pump argument, which typically neglects the likely effect of decreasing wealth on an agent's willingness to pay for each successive preferred alternative. Anyway, I was encouraged to look for a better argument in favour of transitivity.

[3] The published paper (Blackorby et al. 1973) uses a different approach to demonstrate inconsistency of demand behaviour.

Then, in 1974/5 at the Australian National University, I suddenly realized that I could offer a different justification for collective rationality, and for transitive preferences more generally. It drew some inspiration from Vickrey's 1964 book *Metastatics and Macroeconomics*, which emphasized that, especially after Debreu's *Theory of Value*, much of economic theory treated the consumer as if making a single lifelong consumption plan subject to just one life-time budget constraint (Vickrey 1964; Debreu 1959). As I now see it, this formulation reflects von Neumann's assertion that a game in extensive form could be fully analysed by considering only the normal form, in which each player is restricted to making a single strategy choice (Von Neumann 1928). In the book with Morgenstern, this normal or reduced form of the game is described as one where each player's only move is to announce in private to an umpire the strategy that the umpire should execute on the player's behalf (Von Neumann and Morgenstern 1944). This general claim that only the normal form matters is valid, I believe, only in a few special cases. One concerns single-person decision problems, and so team decision problems as well. A second concerns the two-person "zero-sum" (or "strictly competitive") games that were really the only ones that von Neumann was able to solve satisfactorily.

Now, my work on changing tastes showed that, except in trivial cases, this reduction to the normal form does not work when an agent has dynamically inconsistent preferences. In fact, it works for all finite decision trees if and only if the decision-maker has preferences for paths through each tree that are consistent and also transitive. Moreover, in the case of a social decision tree, an Arrow SWF allows such a reduction if and only if IIA is satisfied. Inspired by Vickrey (1964), I chose the title "Dynamic Restrictions on Metastatic Choice" for this first paper (Hammond 1977).

So far there had been no attempt to include risk or uncertainty in this approach to decision theory. The next stage comes in a paper that was never published. It bore a title something like "Some Uncomfortable Options in Welfare Economics under Uncertainty". An egalitarian welfare economist like myself, particularly after enjoying the experience of being Tony Atkinson's colleague for several years, might like to see as equal a distribution of income as possible, taking into account various incentive constraints. The paper on "Fully Progressive Taxation" written with Partha Dasgupta helped explore what might be possible (Dasgupta and Hammond 1980). Once one introduced risk and different personal probabilities, however, an ex ante Pareto efficient allocation that started out egalitarian would only be egalitarian ex post in rare circumstances. I was uneasy about this evident dynamic inconsistency between ex ante and ex post.

In 1980 Maurice Salles invited several of us to a conference he had organized with Prasanta Pattanaik to be held in Caen, immediately after the World Congress of the Econometric Society in Aix-en-Provence. Indeed, this meeting led eventually to the founding of *Social Choice and Welfare* as the leading specialist journal. My plan had been to present my "Uncomfortable Options" paper. But while travelling from Aix to Caen, I suddenly realized that the main idea of "Dynamic Restrictions on Metastatic Choice" (Hammond 1977) could be applied to decision trees with chance nodes, where it would imply the independence axiom. That led to a significantly revised conference paper emphasizing that dynamic consistency would require an ex

post approach (Hammond 1983). The idea of calling this "consequentialist" decision theory only came a few years later.

After these perhaps rather lengthy preliminaries, let us now turn to the paper which you asked me about (Hammond 1988a). A paper, by the way, that had been prepared while I was visiting CORE in 1986, to which you very kindly invited me back to give some lectures on this topic in 1994. Anyway, it might actually be better to go a bit further forward to "Consequentialism, Structural Rationality and Game Theory", which appeared in the proceedings of a 1993 conference of the International Economic Association that Ken Arrow co-organized (Hammond 1996a). The later paper may offer a rather clearer statement of consequentialist decision theory, based on three axioms applied to finite decision trees in a specified domain. For simplicity, let us focus first on the case of finite decision trees without risk or uncertainty. Then there is no need to discuss chance nodes at which what Anscombe and Aumann (1963) call a "roulette lottery" is resolved, nor "natural" or "event" nodes at which a "horse lottery" is resolved.

Of these three axioms, the first is that the theory applies on an unrestricted domain—or at least a domain that is sufficiently unrestricted to accommodate the trees that are used in the proof that choice must maximize a (complete and transitive) preference ordering. Indeed, a decision theory whose scope must be limited to a restricted domain of finite decision trees seems evidently insufficient, just as majority rule is in the context of social choice theory.

To explain the second and third axioms, it may be best to introduce a little notation. Given any finite decision tree T and any decision node n of T, let $M(T, n)$ denote the non-empty set of moves that are feasible at n, and let $M^*(T, n)$ denote the set of moves at node n which our decision theory deems normatively acceptable. Since indecision at node n is not an option, as Arrow (1951a) lucidly explained in his thesis, the set $M^*(T, n)$ must be a non-empty subset of $M(T, n)$. We also let $T(n)$ denote the continuation subtree of T that starts at its initial node n. This is the result of "snipping off" $T(n)$ from T, to use the evocative terminology of Machina (1989).

Now, the second axiom is dynamic consistency. Using the notation we have just introduced, this requires that the set $M^*(T, n)$ of acceptable moves at node n in the tree T should be the same as the set $M^*(T(n), n)$ of acceptable moves at the initial node of the continuation subtree $T(n)$. This equality can be made almost tautological by recognizing that any earlier plans of what to do when the decision maker reaches node n are essentially irrelevant by the time n has been reached, so it is $M^*(T(n), n)$ that determines what the agent will actually do at node n. Thus, if we define $M^*(T, n)$ as the set of moves that the agent might actually make at node n, it must equal $M^*(T(n), n)$. Though I find this consistency axiom entirely compelling, in a talk I gave in 2017 to the conference of the European Society for the History of Economic Thought, I set out to replace it by one requiring that the agent should never face any possibility whatsoever of regretting her original decision plan.

The third axiom is intended to express Arrow's claim that an act should be judged by its consequences (Arrow 1951b). It also conforms with Savage's definition of an act as a mapping from states of the world to their consequences (Savage 1954). This "consequentialist" axiom is the one that does by far the most work, so I felt

free to base the name of this approach to normative decision theory on this axiom. Indeed, I would rather call it a "pre-axiom", from which so many axioms in other versions of normative decision theory follow as logical implications. Given any finite decision tree T, one can use backward recursion to construct at each successive decision node n of T: first, the set $F(T,n)$ of all possible consequences that can result ultimately from choosing any move from the feasible set $M(T,n)$; second, the set $F^*(T)$ of all "acceptable" consequences that can result ultimately from choosing any acceptable move from $M^*(T,n)$. Evidently each $F^*(T,n)$ is always a non-empty subset of the corresponding $F(T,n)$, as one can prove by backward induction. What the "consequentialist" axiom requires is the existence of a choice function mapping non-empty finite subsets F of the consequence domain Y into non-empty choice subsets $C(F)$ with the property that, for any finite decision tree T, one has $F^*(T,n) = C(F(T,n))$ at every decision node n. Thus, the agent must behave in any decision tree as if a planned sequence of moves, or "decision strategy", can be chosen if and only if it has acceptable consequences. This "consequentialist" axiom is the application to single-person decision trees of von Neumann's principle that we discussed earlier—namely, the claim that it loses no generality to reduce a game in extensive form to its normal form (Von Neumann 1928).

The main result of this consequentialist approach to normative decision theory is that the three axioms hold if and only if the consequence choice function which maps each non-empty finite feasible set F to the non-empty choice set $C(F) \subseteq F$ is ordinal. That is, there must be a complete and transitive preference ordering defined on the consequence domain Y such that the consequence choice set $F^*(T,n) = C(F(T,n))$ at any decision node n in any finite decision tree T corresponds to choosing consequences to maximize that ordering over the finite feasible set $F(T,n)$.

PM: *This is a very helpful summary. Perhaps you could now extend it to the case of chance nodes, which permits deriving a preference ordering that satisfies the VNM independence condition. As you take this logical derivation also to be a normative justification of this notorious condition, it has led to a lively controversy with Machina.*

PJH: Where Mark Machina and I differ is indeed in the extension of the above consequentialist theory to decision trees with chance nodes. In those trees, the preference ordering is over consequence lotteries. The key axiom that $F^*(T) = C(F(T))$ would actually imply universal indifference if we were to allow random moves at chance nodes to have zero probability, so we do not allow this. Perhaps I may be allowed to mention here two papers (Hammond 1999b, c) I wrote that allow infinitesimal probabilities at chance nodes, building on my contribution to the *Festschrift* for Patrick Suppes, the "scientific philosopher" (Hammond 1994a).

Once we exclude both zero and infinitesimal probabilities, however, the existence of a preference ordering satisfying the independence axiom is necessary and, when combined with continuity with respect to changes in probabilities at chance nodes, sufficient for the preference ordering to be represented by the expected value of each utility function in a non-empty equivalence class. According to my possible misinterpretation of how his work differs from mine, Mark chose to deny what I call dynamic consistency by claiming that continuation subtrees are somehow different from deci-

sion trees. In his book that sets out his theory of resolute choice, Ned McClennen (1990) does something similar. By contrast, I argue that the key consequentialist axiom that behaviour should be explicable by its consequences should apply not only for each entire finite decision tree, but also for each continuation subtree.

Obviously, we can differ over the relative normative appeal of our differing theories. But there are two methodological considerations that worry me. The first is that if we are going to treat continuation subtrees as different from decision trees, why not do the same when considering decision trees without risk or uncertainty? Then one cannot even use my consequentialist argument for the existence of a preference ordering. This certainly troubles me, even if it might not bother Machina or McClennen, since neither hesitates to assume that there is a preference ordering. Second, faced with any decision tree, we would have to start worrying about what had preceded it, including resolutions made in the past. At least we would seem to need a fuller decision theory that includes scope not only for past resolutions but possibly a richer domain of consequences to recognize those that emerge from adhering to or departing from past resolutions.

I am not sure if Mark Machina agrees with my normative theory. But I definitely accept his theory of non-expected utility maximization as a possibly useful but refutable description of what many people—perhaps even a large majority of people—actually choose when faced, for instance, with the lotteries presented in the well-known paradox due to Allais (1953). Also, while non-expected utility may describe accurately what people do, especially in a laboratory, there is still the possibility which Savage discusses in his *Foundations of Statistics* that violating EU theory, at least in the context of the Allais paradox, seems a mistake like paying too much extra for a car that includes an option such as a fitted radio—in the era of Savage (1954).

PM: *I am coming now to another part of your work, which concerns the extension of standard, static welfare economics and social choice theory to the uncertainty case. The many problems this extension raises had been debated in the late 1960s and 1970s by* Diamond (1967a, b), Drèze (1970), Starr (1973) *and others before you tackled them in the 1980s, but you put them more sharply than these early writers, and this was perhaps because you had the comparative advantage of being a social choice theorist as well as a welfare economist. To remind the reader, when uncertainty prevails, the Pareto principle can be applied either to the individuals' ex ante preferences, thus implicitly respecting their subjective probabilities and risk attitudes, or only to their ex post preferences in each given state of the world, thus ignoring these items entirely. In the ex ante approach, the ex ante Pareto principle holds, plus the assumption that individuals satisfy SEU, whereas in the ex post approach, the ex post Pareto principle holds, plus the assumption that the social observer satisfies SEU. In a 1981 paper and two book chapters in 1982 and 1983, you emphasize the conflict between the two approaches (*Hammond 1981, 1982, 1983*)—a conflict I myself investigated much later (*Mongin 1995*), in order to put it into a proper axiomatic framework. You also express a considered preference for the ex post approach. Your ex post solution is actually also utilitarian, and it thus provides you with another line to defend this*

doctrine. I do not think you have later revisited these claims. Would you endorse them today in the same way as you did at the time?

PJH: The topic of ex ante versus ex post is yet another to which I was introduced by Jim Mirrlees while he was supervising me in Oxford. He ascribed the idea to Peter Diamond. Indeed there is a highly relevant footnote to Peter's paper that set out a theory of efficient allocations in a model of the stock market (Diamond 1967b). The footnote explained that he had chosen to follow the ex ante approach, while recognizing that the ex post approach could be an interesting alternative.

I am not quite sure what was the immediate impetus, but eventually I took up this suggestion of Peter and Jim in the late 1970s. By the way, the ex post approach could use the individuals' own risk attitudes, provided these were judged to be ethically appropriate. It could also use their subjective probabilities, not indirectly in their subjective expected utilities, but directly through some other aggregation procedure. The paper by Starr (1973) had been interested, inter alia, in exploring cases when differing subjective probabilities would not preclude markets from achieving an ex post efficient allocation. The *Economica* paper that you kindly mention (Hammond 1981) was indeed an attempt to extend some of these results to a more general setting with many periods, etc.

One of the books you mention is *Utilitarianism and Beyond*, edited by Amartya Sen and Bernard Williams, with a number of notable contributions (Sen and Williams 1982). I think I was able to get the title changed from *Beyond Utilitarianism* when I suggested to Amartya that at least one, and eventually several, of the contributors might not want to venture all the way out there.

Anyway, after these works, my interest in the subject remained essentially passive for at least 25 years. But then in December 2008, at a seminar organized by the Paris School of Economics, I gave a talk showing that in a framework like the insurance model of Malinvaud (1972a, 1973), even if not all individuals could agree on the associated probabilities of insurable individual risks, nevertheless ex post Pareto efficiency would still require full risk pooling provided that agents could all agree that these risks were described by what De Finetti (1937) described as exchangeable random variables. These results, I fear, still need to be written up.

Finally, there was a conference in Milan in December 2015 where I had played a small part in the organization, along with Maurizio Motolese and Carsten Nielsen whom I had known as Ph.D. students at Stanford. The theme was "Welfare Evaluation under Subjective Expectations", with some emphasis on overconfidence. My talk there was some sort of survey of social choice theory in such a setting, without finding anything really new to say.

Closely related to these issues is the problem of aggregating subjective probabilities, and this leads me to share the memory of a seminar by Michael Bacharach during my two years in Oxford. This seminar introduced me to the concept of an externally Bayesian procedure for aggregating personal probabilities. A letter to Michael ensued shortly thereafter in which I pointed out that, instead of taking a weighted arithmetic mean of different individuals' probabilities, one should instead take logarithms first and consider a weighted geometric mean. That way you get a

rule for which the operations of aggregation and Bayesian updating commute, which is the property that defines the externally Bayesian criterion.

PM: *I am now venturing into a territory that is less familiar to me, i.e., mechanism design, implementation and incentive compatibility. Would you explain to us how you became interested in this topic and eventually arrived at the reference paper (*Dasgupta et al. 1979*) you coauthored with Dasgupta and Maskin in 1979 on these topics?*

PJH: Again, this part of my work starts with trying to fill in some logical gaps I found while teaching a master's course in public economics at Essex. Now, the efficiency theorems of welfare economics link Pareto efficient allocations to the Walrasian equilibrium outcomes of a competitive market mechanism, provided that one introduces lump-sum wealth redistribution. Paul Samuelson in his *Foundations of Economic Analysis* had recognized that there could be problems in acquiring the information needed to implement satisfactorily such a redistribution scheme (Samuelson 1947). Actually, since the distribution of wealth typically affects aggregate consumer demands and supplies and so market-clearing prices, the redistribution scheme should really specify how each individual's wealth depends on prices, as it will when it is a rule for distributing the profits of firms in a general Arrow–Debreu economy (Arrow and Debreu 1954; Debreu 1959; Grandmont and McFadden 1972). That said, Diamond and Mirrlees' results on optimal taxation applied in a second-best world where there could be distortionary commodity taxes, but any lump-sum wealth redistribution had to take place through uniform lump-sum subsidies (or much less plausibly, taxes) that were the same for everybody, regardless of any individual circumstances. This is the kind of "imperfect" economy that Mirrlees (1997) discussed in his Nobel Prize lecture on the "economics of carrots and sticks".

Indeed, some sort of folk theorem was going around that concerned first-best optimal allocations in the model of income taxation that had been briefly explored in Vickrey (1945) before Mirrlees (1971) gave it a much fuller treatment. Assuming that leisure is a normal good, the unique first-best allocation has all workers consume the same amount, regardless of their skill; workers whose known skills were greater, however, would be expected to work more. Such an allocation seems well described by what became the Marxist slogan: "From each according to their ability; to each according to their need!" (Marx 1875). Yet trying to put this into practice would neglect incentives to reveal one's true skill and lead to this description of working in any of the Communist nations of Eastern Europe prior to 1989: "So long as the bosses pretend to pay us, we will pretend to work." Actually, as I learned from a lecture that my colleague Mark Harrison gave on the occasion of the centenary of the Russian Revolution (Harrison 2017), the Leninist Soviet constitution already suggested "to each according to their work".[4]

During the early 1970s, Leo Hurwicz's Richard T. Ely lecture to the American Economic Association, "The Design of Mechanisms for Resource Allocation", had appeared in print (Hurwicz 1973). As, of course, had Gibbard's (1973) paper on strategyproof voting schemes, though that seemed less relevant to the issues surrounding

[4]See Chap. 5, Sect. 3 of Lenin (1917), as well as Article 12 of the 1936 Constitution of the USSR.

economic allocations which interested me at that time. Hurwicz's paper, despite being largely concerned with impossibility results, like Gibbard's, did offer one positive suggestion: a competitive market mechanism did seem to be strategyproof in a large economy with many consumers.

In 1977 I had agreed to teach during Stanford's summer quarter so that I could attend the summer workshop of the Institute of Mathematical Studies in the Social Sciences, organized by Mordecai Kurz. So unfortunately I missed the conference that led to Jean-Jacques Laffont's volume *Aggregation and Revelation of Preferences* (Laffont 1979). But at the Stanford workshop I could at least present an early version of what became my paper on incentives in large economies that characterized strategyproof mechanisms, including the competitive mechanism (Hammond 1979a). What Guesnerie (1981, 1995), in his parallel work, called the "taxation principle" played a crucial role in this characterization. Smoothness conditions that excluded competitive mechanisms with redistributive lump-sum transfers were provided. In an economy with public goods, similar conditions ensured that the only way to get a strategy-proof mechanism to yield Pareto efficient allocations required the public goods to be financed by what later, in the 1980s, became the hated "poll tax" or "community charge" that Mrs. Thatcher introduced as a way of helping to finance local government expenditure. My paper, however, did point out that such a tax could be difficult to collect. I should record that Paul Champsaur and Guy Laroque did express some objections to my treatment of null sets of agents in the result that relied on smoothness conditions (Champsaur and Laroque 1981, 1982). I hope I may have answered them in the chapter (Hammond 2011) on competitive market mechanisms that Arrow, Sen and Suzumura had asked me to write for the *Handbook of Social Choice and Welfare* they edited.

Around that time, I aspired to follow a precedent that Michael Farrell and Frank Hahn had set as managing editors of the *Review of Economic Studies* when they published symposia on specific research topics — namely, the June 1962 "Symposium on Production Functions and Economic Growth", and the January 1967 "Symposium on Optimal Infinite Programmes". In 1974 Geoffrey Heal continued this tradition with an extra issue devoted to a "Symposium on the Economics of Exhaustible Resources". The subject I chose for what became the April 1979 issue was incentive compatibility, a topic for which there were already a number of good papers in the editorial pipeline.

Meanwhile Eric Maskin had been visiting "our" Cambridge while working on his Harvard Ph.D. thesis in Applied Mathematics, supervised by Kenneth Arrow. Partha Dasgupta and I had been discussing some aspects of Eric's work with him. An attempt (Dasgupta et al. 1979) to synthesize many of the results known at that time, and to place them in the context of social choice theory, seemed something worth including in the symposium. Fortunately, the referees agreed. They let us get away, however, with one rather glaring error.

PM: *The paper covers so much ground and has such a wealth of formal results that a slip in it is excusable. What was this error?*

PJH: We had a correct result, essentially due to Hurwicz (1972), showing that in the case of a two-person exchange economy, any strategy-proof mechanism guaran-

teeing Pareto efficient exchange would have to be dictatorial. But then we claimed that the same would hold with more than two individuals, and referred the reader to Eric's thesis for a result that was related, but failed to offer an adequate proof. Not many years later in the same journal, Mark Satterthwaite and Hugo Sonnenschein came up with a remarkably simple counter-example, which they were kind enough to conceal somewhat in their presentation of interesting new results (Satterthwaite and Sonnenschein 1981). In this counter-example, a third agent's preferences could determine which of the first two agents would be allocated the total endowment in the exchange economy, so neither is a dictator. At least it is an interesting illustration of what difference it makes to add a third player to a game with two players.

There is an important lesson that I like to draw from a particular result in our paper, similar to one which Ledyard (1978) demonstrated at about the same time. First let me start with the methodological observation that the literature on incentive compatibility is intended to deal with situations where the participants in the mechanism or game have incomplete information about each other. In my view, this calls for Harsanyi's (1967, 1968a, b) concept of a "Bayesian" equilibrium, rather than the Nash equilibrium concept that Hurwicz, Maskin and others were using rather extensively.[5] Perhaps Nash equilibrium is appropriate in something like a principal–agent setting for the very special case where all the agents happen to have complete information about each other, but the principal does not. Anyway, in general, the outcome of a game of incomplete information will be sensitive to each player's beliefs about the preferences and beliefs of other players, including their beliefs about which strategies the other players will choose. Indeed, such beliefs and the speculative activity which they engender typically play a key role in determining prices in financial markets, as well as the outcome of a first-price sealed bid auction. Yet welfare economists usually focus only on simple allocation mechanisms, without considering at all how the allocation generated by agents' behaviour in the mechanism may depend on their beliefs. Our simple result showed that the only case where this is generally justified is when one is using a strategy-proof mechanism. This may help justify why my subsequent work has largely focused on these.

Roger Guesnerie and I both moved on to consider mechanisms that remained strategy-proof even in the presence of parallel or shadow markets, where agents could conduct hidden deals on the side. Roger introduced the idea of dividing up private goods into two classes: first, goods like potatoes, cigarettes, wine, or drugs that could be exchanged on such shadow markets; second, goods like electricity, houses, and labour supplied to large firms that get noticed by tax authorities (Guesnerie 1981, 1995). The "multilateral" incentive constraints that arise not only imply that pricing schedules have to be linear, which is not too surprising; in addition, lump-sum redistribution which does not depend on publicly observable individual characteristics such as date of birth can be entirely ruled out without any need to assume smooth preferences. In effect, the possibility of shadow trading places additional constraints

[5] For notable examples, see Hurwicz et al. (1994) and Maskin (1999), as well as several contributions to the Hurwicz *Festschrift* (Groves et al. 1987).

on strategyproof mechanisms, as suggested by the title of my later article on "markets as constraints" (Hammond 1987a).

Finally, while on this subject of mechanisms and market design, it may be appropriate to mention Hammond (2017). This paper's subtitle "Twenty-Two Steps to Walrasian Equilibrium" evokes some of the practical difficulties that must be overcome if one is to get competitive markets to work as standard micro-economic theory says they should, even if one is limited only to spot markets with many traders, whose individual influence over prices is negligible.

PM: *I suppose one could argue that implementation theory provides a foundation for public economics. At least, this is the area where public economics and social choice theory have fruitfully met. Decreasing the abstraction level, we could now touch on public economics proper and discuss the contributions you made to it.*

PJH: Many of my ideas on public economics up to the late 1980s are summarized in my "provocative assessment" of theoretical progress in the subject (Hammond 1990). This was a response to the kind invitation of my Oxford friend Peter Sinclair while he was an editor of *Oxford Economic Papers*. But then, after the Berlin Wall fell in 1989, Edmond Malinvaud came to give some Schumann lectures at the European University Institute (EUI), where I spent a leave of absence from Stanford as a "professore di scienze economiche" (plural) for the two academic years 1989/90 and 1990/91. Malinvaud's lectures concerned the then topical subject of suitable programmes for liberalizing the economies of the Eastern European nations that, up to 1989, had been behind the Iron Curtain.[6] These lectures prompted me to think once again about the possible benefits of markets, especially the gains from trade and other forms of economic liberalization. This topic had interested me enough back in 1975 so that, when Murray Kemp invited me down from Canberra to the University of New South Wales in Sydney in order to give a lunch-time seminar, rather presumptuously I chose to speak about "The Gains from Trade in Imperfect Economies". Meanwhile, I became a supervisor to Jaume Sempere as he was studying for his Ph.D. at EUI. After he had had this thesis accepted, we collaborated on three articles discussing how to convert the potential Pareto gains that Kaldor and Hicks had considered in their compensation tests into ethically acceptable actual Pareto gains (Hammond and Sempere 1995, 2006, 2009).

Our first paper, which appeared in the *Economic Journal*, focused on the gains from trade. It built on a *Journal of International Economics* paper by Jean-Michel Grandmont and Daniel McFadden that extended the two standard textbook examples where trade cannot create any pecuniary externalities in the form of adverse terms-of-trade effects (Grandmont and McFadden 1972). These examples are: first, an exchange economy where the status quo is autarky; second, a "small country" whose exports and imports have no effects on world prices. There was also some discussion of the paper by Dixit and Norman (1986). This considered a Diamond–Mirrlees model of optimal taxation where one could ensure a Pareto improvement by freezing

[6] See http://cadmus.eui.eu//handle/1814/23611 for a record dated 1991 of a distinguished lecture that Malinvaud gave to the Robert Schuman Centre for Advanced Studies at the European University Institute. The title was "Macroeconomic Research and European Policy Formation".

consumer prices and then using commodity taxes or subsidies in order to create a gap between consumer and producer prices that would allow the latter to vary in order to clear markets while also providing resources to fund a uniform lump-sum subsidy that would benefit every consumer.

For our second paper in the *Journal of Public Economic Theory*, we turned attention to the additional potential Pareto gains which could be achieved from the free migration of labour. Kemp (1993) had discussed this earlier, but he treated labour as just another traded good. This ignores the non-convexities that make themselves all too evident if one tries to work simultaneously in more than one country. Or, in the example which Malinvaud presented in his *Lectures on Microeconomic Theory* in the days before high-speed trains, if one tries to eat a dinner one evening that is split between Paris and Lyons (Malinvaud 1969). We were able to overcome these non-convexities by the usual device of assuming a continuum of consumers, following the well-known major works (Aumann 1964, 1966; Hildenbrand 1974), as well as papers by Akira Yamazaki and Ali Khan on general equilibrium theory with non-convex consumption sets and dispersed consumer characteristics (Yamazaki 1978, 1981; Khan and Yamazaki 1981).

That second paper was written while I was in California. There I was well aware of Proposition 187 supported by Governor Pete Wilson and passed by Californian voters in 1994.[7] It was intended to deny the access of "illegal" immigrants to publicly provided services such as schools for their children and, in the case of the poor, access to emergency healthcare. That prompted a third paper, published in *Economic Theory*, that allowed for externalities subject to congestion. This does strike me as a legitimate cause for concern about immigration in case, as happens too often, especially in the UK leading up to the Brexit referendum fiasco, the state fails to make adequate adjustments in the provision of public resources that immigrants and their families need. This despite the additional tax revenue that immigration often generates.

Externalities subject to congestion do, of course, make it much harder to ensure Pareto gains from free migration; indeed, in order to encourage people to make efficient decisions regarding where to live and work, we found ourselves analysing residence charges that are uncomfortably close to the Thatcherite poll taxes that had emerged in my paper on incentives in large economies (Hammond 1979a). Even then Jaume and I were only able to ensure Pareto gains by assuming that migration would be limited to exchanges of population which kept fixed any congestion externalities that would have existed in the status quo allocation, in the absence of any reform. Along with David Lodge's amusing novel about an academic exchange of Professors of English Literature between "Rummidge University" in the English Midlands and "Euphoric State" on the edge of the San Francisco Bay, this inspired the "Changing Places" part of the title. It also does much to explain why the paper became the subject of seminar presentations at both U.C. Berkeley and the University of Birmingham.

PM: *Could we now turn to the vexed topic of cost–benefit analysis (CBA), another area you contributed to?*

[7] Immediately after passage, its key provisions were challenged in federal district court. They were declared unconstitutional before having time to take effect.

PJH: My introduction to it occurred while at Nuffield College, where there was a regular Friday afternoon seminar organized by Ian Little and Jim Mirrlees. That seminar concerned issues arising from trying to put into practice the methods advocated in their Manual published by the OECD (Little and Mirrlees 1969, 1974). There were the competing *Guidelines for Project Evaluation* by Partha Dasgupta, Stephen Marglin, and Amartya Sen (UNIDO 1972). Indeed, my first encounter with Amartya may have been when he came to the Nuffield seminar in order to present a talk on their alternative methodology.[8] Also, when I was at the Australian National University (ANU), I found myself gravitating to the Philosophy Department to attend a seminar on CBA that they had organized. This was most likely as an initiative of John Passmore, a great figure in Australian philosophy who in 1974, as I have since discovered, published a book on environmental ethics (Passmore 1974).

My writing on the subject of CBA began with a contribution to the then annual AUTE (Association of University Teachers of Economics) conference, when it was held in the spring of 1978 at the University of Warwick. The title of the paper is "Cost–Benefit Analysis as a Planning Procedure" (Hammond 1980).[9] The idea was that, properly conducted, a CBA test should be a tool for identifying projects or policy changes that would improve the allocation of resources. This is somewhat along the lines of Malinvaud's "Decentralized Procedures for Planning" that appeared in a 1967 conference volume that he co-edited with Michael Bacharach (Malinvaud 1967). Similar ideas arise in the MDP planning procedure, named after Malinvaud (1971, 1972b) as well as Drèze and de la Vallée Poussin (1971). Another source of inspiration for me was a seminar that I heard Peter Warr present while I was at the ANU (Warr 1977).

A key issue that arises when doing CBA is the choice of shadow prices at which to evaluate the inputs to and outputs from the project being analysed. The UNIDO manual largely advocates using consumers' demand prices. Its approach focuses ideally on how the project, together with any "balancing policies" such as financing the project that are needed to allow the economy as a whole to adjust to it, affect different consumers' well-being (Hammond 1986). If the project is small, one can use their aggregate willingness to pay, with suitable welfare weights for different individuals, to see if the benefits outweigh the costs. Doing this thoroughly, however, even for a project as small as deepening one village well by the odd metre, is likely to be very burdensome. Partha Dasgupta, in his published discussion of my paper at the AUTE conference, was energetic in defending the UNIDO approach, and in noticing some gaps in my background knowledge.

By contrast, the OECD manual by Little and Mirrlees largely favoured using producers' supply prices. In the prominent case of goods that are traded internationally by a small country facing competitive world markets, these could be the prices at which those goods cross the national border. Many non-traded goods, on the other hand, could be priced by looking at input and output relationships in the national

[8] See also Sen (1972a).

[9] See https://web.stanford.edu/~hammond/CostBenefit1978.pdf for a version where the scanned pages appear in numerical order.

economy. Some decades later, while preparing my paper "Reassessing the Diamond–Mirrlees Efficiency Theorem" for the conference whose proceedings appeared in what became the Mirrlees *Festschrift* that Gareth Myles and I co-edited, I realized how this approach based on supply prices could be justified if it was able to identify projects that would really enhance the economy's production possibilities (Hammond 2000). Then, provided that the project is accompanied by other suitable policy measures for distributing the benefits of increased production efficiency to consumers, it could generate Pareto gains. This proviso, of course, often fails to be met, as is illustrated by what has been happening over the last decades to poorer workers who have become the increasingly desperate victims of globalization.

Looking back almost 40 years, using the demand prices that the UNIDO approach emphasizes always works in principle provided that one restricts attention to genuinely small projects, if there are any, where shadow prices are fixed. Yet using demand prices seems unreasonable in requiring not only a finely detailed disaggregated analysis to determine the precise distribution of individual gains and losses, but also a full specification of what "balancing policy" gets used to re-equilibrate the national economy by, for instance, providing adequate finance for the project. On the other hand, whereas the OECD approach based on supply prices surely requires calculations that should be much easier in practice, it is really only ethically justified if one has faith that institutions which supplement the project can ensure that its gains and losses will be appropriately distributed between different individual consumers.

Finally, there is also the associated topic of welfare measurement. Some pieces I wrote during the 1980s on approximate measures (Hammond 1984, 1988d) reflected the fact that I was then rather confused. One benefit of knowing Erwin Diewert, however, was that Wolfgang Eichhorn invited me to a conference in Karlsruhe (Eichhorn 1994). By then I had finally grown to understand and appreciate the power and intuitive appeal of money metric utility functions, which are related to but more useful than Hicks's notions of compensating and equivalent variation that appear in the better textbooks.

I was also familiar with Dale Jorgenson's work on measuring social welfare that had featured in his Presidential Address to the Econometric Society and other articles (Jorgenson 1990, 1997a, b). This relied on concepts like equally or optimally distributed income equivalents, as well as hypotheses about demand behaviour close to allocations that would be reached with equal or optimal income distributions. Yet in our very unequal societies, it is hard to know what this behaviour would be. So in Hammond (1994b), following ideas due to Feldstein (1974) and Rosen (1976), I proposed a uniform money metric measure of social welfare. Its construction depends only at what demand behavior would be near allocations where all individuals have equal increases to their status quo income, which seems much more practical. Also, to support our host Wolfgang Eichhorn in what in the end was clearly his successful effort to secure the generous funding that the conference deserved, he asked us to include some discussion of issues surrounding externalities. In my case I found this relatively easy to do, though it did raise some associated issues about how to generalize the Slutsky conditions to deal with marginal willingness to pay for public goods. These issues, as far as I am aware, still remain unresolved some 25 years later.

PM: *Before we are coming to a close and I thank you for a wonderful treat of ideas and recollections, I would like to put to you a loose, but perhaps important question. Your career began, and some of your achievements indeed took place, in what was still the heyday of economic theory. In the post-war years, neo-classical economics had finally crystallized into elegant theorems on equilibrium and optimality, and entirely new theories of rational action with a strong axiomatic tone had just entered stage—game theory, decision theory, social choice theory, and their dependencies. New formal tools like convex analysis, combinatorics, linear programming or optimization theory, had emerged or become better understood. All this encouraged the mentors and senior colleagues you mentioned in this interview to expect further quick progress as well as perhaps a final unification—the expression in the singular "economic theory" testifies to this hope. You are the heir to this intellectual tradition, which your life-long work has enriched. But already before the turn of the century, the economists' academic interests shifted towards experiments, applications and new disciplinary connections, such as cognitive psychology and behavioural sciences. Today's young theorists have a hard time competing with ambitious millennials who produce more and more empirical research with shallow theoretical foundations. Do you agree with this sombre diagnosis? Do you think there is a future in the profession for the economic theory we discussed in this interview?*

PJH: Now you are asking me to do something even more dangerous than predicting future economic events, which is to predict the progress of economic thought, especially in the area of normative economic theory which has been the subject of this interview. Nevertheless, I am ready to admit that I tend to agree with your diagnosis, though in the end I may disagree with the assessment that it is sombre.

Probably I should start by confessing to some rather extreme views regarding economic methodology, though they are extreme in two opposite directions, which may be as uncomfortable as it would be for one person to try to balance a see-saw by occupying both ends simultaneously. At one end is descriptive or positive economics, which is concerned with how individuals make decisions, and how economic institutions perform, as well as who gets allocated what in an economic system. Here I am an extremist to the extent of refusing to accept, at least in the absence of convincing supporting evidence, the standard neoclassical hypothesis that economic agents behave rationally.

At the other extreme is theory that is purely normative or prescriptive, setting out principles that should guide economic policy, or decision-making more generally. Here I go to the other extreme in saying that the decision-maker should choose policies and actions that they can defend because they can be expected to have good consequences. As I claimed in an invited address to last year's meeting of the European Society for the History of Economic Thought whose theme was "rationality", one should avoid any unnecessary possibility of creating regrettable consequences. This, by the way, is intended to recognize that some regret may be necessary because of our inability to have a complete model of all the consequences that may result from those decisions that we must make right now. It turns out that planning to avoid regrettable consequences is equivalent to dynamic consistency. There is some relationship here to Gilboa and Schmeidler's view that rationality requires avoiding

regret over the axioms one chooses to satisfy; I have much more concern, however, for the practical consequences of economic decisions than for the logical "consequences" (i.e., implications) of alternative axiom systems (Gilboa and Schmeidler 2001; Gilboa 2010, 2015). Anyway, this kind of rationality based on avoiding regrettable consequences leads rather directly to subjective expected utility as a decision criterion, with some modifications that I am still exploring in an attempt to recognize that any model we use to guide our decision-making involves bounds that prevent our noticing possibilities whose neglect we may later regret.

When discussing the future of our discipline, we should admit the possibility of diminishing returns to some kinds of further research. Certainly young researchers trying to establish themselves in academic economics may be well advised to focus on better descriptive models. So I welcome progress in behavioural economics that attempts to improve our understanding of how real people form their tastes and beliefs, and of how they make decisions, including in laboratory experiments. Of course, the search for a coherent theoretical framework is important, as is looking for psychologically realistic ways of advising people how to make better decisions. Indeed, one reason that my move to Warwick in 2007 has proved personally beneficial is that it created more scope for me to explore some of these topics.

I would like to end, however, with what may be a salutary lesson. One reaction to the financial meltdown in 2007–8 was that economists had neglected to construct appropriate models that build in some understanding of what really happens in messy real financial markets.[10] The hope seemed to be that such models could help us predict financial markets better, and perhaps head off the next crisis. Yet to me, thinking as a normative economist, this may be fruitless. Instead, rather than trying to model the existing mess better, we should at least start thinking about how to regulate and even re-design financial markets so that their behaviour becomes much easier to understand. Then, perhaps, we can hope to control them well enough to ward off unnecessary threats of disaster.

Finally, let me conclude this last response by expressing my profound thanks to you for the wonderfully well-informed and probing questions that you have asked me. Attempting to answer them has been a most welcome intellectual challenge.

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[10] See especially the papers in *Nature* by Bouchaud (2008) as well as Farmer and Foley (2009).

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