



Claude d'Aspremont and Peter J. Hammond

**Cd'A:** *Reinhard Selten, in his contribution to your Festschrift, concluded with some biographical remarks, presumably accurate.*<sup>1</sup> *They mention that in 1947 you received a doctorate in philosophy. What was exactly the topic of your dissertation? Did you use some of it in later work?*

**JCH:** Well, it's certainly true and the topic of my dissertation was the logical structure of philosophical errors.<sup>2</sup> By that I meant that philosophers have some valid insight into some general principle, but then they go much too far in pressing it, which causes serious errors and that's probably their main error. But it is not a very deep observation. And of course I didn't have much opportunity to use this later.

**PJH:** *Except in so far as you became interested in philosophy and philosophical work generally.*

**JCH:** Well I was always interested in logic and the philosophy of mathematics. But I never had the opportunity to study it, although I wrote later a paper and sent it off [to a journal].

**PJH:** *Is that later paper published?*

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<sup>1</sup>See Selten (1992, pp. 430–432).

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<sup>2</sup>See Harsanyi (1947).

**JCH:** Yes, it was published in *Erkenntnis*.<sup>3</sup>

**PJH:** Selten's *biographical remarks* also suggest that, once in Australia, you started to study economics in order to enhance your career opportunities, and that some early papers concerned international trade.<sup>4</sup> Would you like to elaborate?

**JCH** [In Australia] of course they didn't accept my Hungarian degrees. But I could work as a factory worker, and in the evening I took an evening course at the university. As I said, they didn't accept my Hungarian degrees. But they gave me very generous allowances for my previous university work, which was in pharmacy because my father was a pharmacist, and then in philosophy and psychology and sociology, the three subjects of my dissertation. So, I did study economics, but I don't remember ever having written a paper on international trade. I did have to write an essay,<sup>5</sup> which was part of my MA degree in Economics, and it was about the theory of the firm, something about the investment policies of an innovative firm.

**PJH:** *And this was at the University of Queensland?*

**JCH:** No, the University of Sydney. I enrolled at the University of Sydney as soon as we arrived there. We arrived in the last few days of 1950 and then I enrolled when the school started, I think probably in March.<sup>6</sup> I had to do two years of studies although the normal curriculum is four years, but they permitted me to skip the first two years. As a result, I never learned some part[s] of economics. So that was what happened and I got my degree at the end of '53 and I got a university job at the University of Queensland early in '54.<sup>7</sup>

**Cd'A:** *Is it fair to say that you chose to visit Stanford in 1956/57 so that you could work with Ken Arrow, even though by then you were moving on to game theory?*

**JCH:** Yes, that's very true, and I really did it as a result of a mistake, although it was a very favourable one. I read a semi-popular article of Ken which was published in a collected volume on game theory.<sup>8</sup> He made game theory look so interesting that I decided I would like to work with him and I thought that may be his main line of study. But it turned out of course that this was very much a sideline. But it was very good to work with him. I learned a lot from him. Even in game theory, of course.<sup>9</sup>

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<sup>3</sup>Presumably as Harsanyi (1983). This paper, however, makes no mention of his dissertation. Also, to judge from the citations and acknowledgements, it must have been largely written in the years immediately prior to publication.

<sup>4</sup>See Selten (1992, p. 431).

<sup>5</sup>Probably his masters' thesis, in fact—see Harsanyi (1953b). One resulting publication would seem to be Harsanyi (1954).

<sup>6</sup>Of 1951.

<sup>7</sup>From March 1951 to the end of 1953 is three years according to the Australian academic calendar. It seems likely that two years were spent completing the requirements for a bachelor's degree, and a third year was spent doing a master's degree, possibly including a thesis as mentioned above.

<sup>8</sup>Kenneth Arrow's very plausible suggestion is that the relevant paper must be Arrow (1951)—see especially Sect. III (pp. 139–147). Actually, in the whole volume, just one section of Arrow's paper is devoted to game theory! So John Harsanyi presumably means that the article, rather than the volume, is on game theory.

<sup>9</sup>The resulting Ph.D. thesis is Harsanyi (1958). Apart from some details of an existence proof, and also with the exception of Appendix II, which contains an economic application in the form of a

**PJH:** *Are there any significant people you remember having a positive influence on your career at the University of Queensland or later at Stanford, apart from Arrow?*

**JCH:** At the University of Queensland, I was very lucky because there were two or three young economists among my colleagues and they were very friendly and some of them were extremely bright. Two of them got first class, second division, or whatever it's called,<sup>10</sup> which is not the best degree, but a good degree, and we had many interesting discussions. But I don't think professionally I benefited very much because my interests were different, and in Stanford it so happened I learned a lot from a short course on game theory given by Herb Scarf, who was there for a short time.

**PJH:** *Do you remember the names of these people in Queensland? Did they do anything interesting later?*

**JCH:** Well, yes, two names, which I remember most were Austin Holmes and Ron Lane. Both of them were interested, roughly speaking, in public finance, and Austin Holmes later derived a very ingenious scheme of how to make use of banking data in computing monetary circulation and other figures, and he became later a high-ranking official of the Commonwealth Bank, the national bank.<sup>11</sup> But he died very young, and Ron Lane is still alive. He also wrote textbooks, I think, in public finance.<sup>12</sup>

**Cd'A:** *What attracted you to social choice theory and what motivated you to set out your version of utilitarianism based on choice under uncertainty, in what Rawls later called the "original position"?*

**JCH:** Well, it's interesting. It was again a result of a chance observation by two economists. I read an article of Friedman and Savage and they pointed out the importance that von Neumann–Morgenstern utilities have in the study of risk-taking behaviour. But then they had a side remark which says<sup>13</sup>: Sure, this is a cardinal utility concept – but it doesn't mean that it can be used in a traditional way in welfare economics or in ethics. I wrote back a rejoinder in which I pointed out how it can be used. And that was my '53 paper, I think two pages in the *Journal of Political Economy*, where I started with this model in which you assume you have the same

numerical example involving three firms, this was published in a very slightly amended form as Harsanyi (1959).

<sup>10</sup>This should probably be second class, first division—commonly known as a II.i. Usually, in Commonwealth countries following the British system, the first class is undivided and is the best possible degree, so a second class, first division matches the following observation better.

<sup>11</sup>In 1960, this split into the Reserve Bank of Australia for central banking, and the new Commonwealth Bank of Australia for commercial and savings banking.

<sup>12</sup>We were unable to find records of any such textbooks, even with the very kind assistance of Tanya Ziebell of the Economics Library at the University of Queensland, arranged through Dr. Ghanshyam Mehta. Two relevant works, however, are Lane (1975, 1995).

<sup>13</sup>In footnote 11 of Friedman and Savage (1948, p. 283), they criticize the work of Vickrey (1945) and write that "it is entirely unnecessary to identify the quantity that individuals are to be interpreted as maximizing with a quantity that should be given special importance in public policy."

probability of ending up in anybody else's place.<sup>14</sup> And then I continued a similar paper in 1955 in which I discussed what people now call the aggregation model.<sup>15</sup> So that started me on this subject, but of course these are not strictly speaking game theoretic subjects. I started being interested in game theory because I read the major John Nash papers. They were available in the university library in Sydney. And I read, among other things, *Econometrica*, and there were three or four brilliant articles of John Nash.<sup>16</sup> And I was particularly impressed by the fact that, of course, as I learned a few months earlier in my university studies, there is no unique solution to bargaining problems in economics. You have an upper limit and a lower limit, and if they [i.e., the bargaining parties] agree at all, they must find a price between these two limits. But there was no theory whatever about whether it will be closer to the lower limit or the upper limit and what will be decided. Nash implicitly answered these questions and it interested me very much.

**PJH:** *If I may come back to the '53 paper, which as you say was a response to Friedman and Savage. As I recall that footnote on the fact that cardinal utility had no ethical significance, in some sense that was probably a response to earlier work by Lerner and Vickrey. Is that right?*<sup>17</sup>

**JCH:** Could have been, I don't know. It wasn't at least obvious to me and I didn't actually know of Vickrey's work, but when I published this work and went to Stanford, Ken Arrow, of course, pointed out to me that he anticipated some of my ideas.

**PJH:** *But, in Australia, you weren't aware?*

**JCH:** Yes.<sup>18</sup>

**Cd'A:** *So did you have some kind of comment by Savage or Friedman?*

**JCH:** No, I think that I didn't have any comments but I am sure that Friedman and Savage, at least Friedman, didn't really appreciate this idea. He didn't agree with such abstract models.... [Some words lost in the general laughter.] However, I think—I am sure the editor asked them whether they should publish it—and they said, "Of course, yes". Because it still was about their work.

**PJH:** *Now you also had an early article on welfare economics with variable tastes, and as I recall it seems to have been motivated by a strange review of Arrow's Social Choice and Individual Values written by Schoeffler and published in the American Economic Review.*<sup>19</sup> *That's accurate?*

<sup>14</sup>Harsanyi (1953a).

<sup>15</sup>Harsanyi (1955).

<sup>16</sup>Four, in fact, but only two in *Econometrica*, both of which concerned bargaining and the "Nash solution", whereas the others dealt with non-cooperative "Nash" equilibrium. See Nash (1950a, b, 1951, 1953).

<sup>17</sup>See Lerner (1944) and Vickrey (1945). As pointed out above, Friedman and Savage (1948) do make a point of criticizing Vickrey.

<sup>18</sup>Meaning "Yes (I was not aware)" rather than "Yes (I was aware)", presumably.

<sup>19</sup>See Harsanyi (1953–1954), which (p. 206, footnote 1) has three paragraphs discussing Schoeffler (1952). However, this footnote could well have been added at a late stage of the writing process, perhaps even as a result of the interchange with Ursula Hicks mentioned below.

**JCH:** All I remember is that the editor of the *Review of Economic Studies* was Ursula Hicks, and I had a long correspondence with her because she thought that my ideas that you can compare your utility before your tastes change with the one afterwards.... This is [making] interpersonal comparisons of utility, and that's unorthodox. But I convinced her that it isn't so ... [phrase inaudible]. She published it.

**PJH:** *So like all powerful and novel work, the papers on cardinal welfare, especially the paper in 1955, have drawn criticism. Particularly noticeable, perhaps, were the comment by Peter Diamond in 1967, and the paper in 1968 by Prasanta Pattanaik. We believe our readers would like to know what you think of their criticisms.*<sup>20</sup>

**JCH:** Well, I did read Diamond's criticism and I think I answered it about at the same time [as] when I wrote my article about the maximin principle, in which I criticized Rawls.<sup>21</sup> I didn't feel that Diamond's criticism was valid and I remember I had a very complicated argument based on the idea: Imagine a society in which it is an old custom that they exchange the babies of people randomly so that every baby ends up in a different family, and I don't remember the detailed argument, but I thought that this refuted some of the contentions of Diamond.<sup>22</sup> But I never read Pattanaik's paper so I haven't answered it.<sup>23</sup>

**Cd'A:** *Recently it has been suggested that John Rawls would justify his difference principle by the claim that, in the original position, people would be extremely risk averse and so maximize the minimum possible utility level they could experience. If Rawls were to abandon this hypothesis, and propound a theory based instead on expected utility, would most of your disagreements be resolved?*

**JCH:** No, I don't think so. One disagreement which would remain would be his claim that ethics should be based on giving absolute priority to the interests of the least advantaged social group, which I thought, as I mentioned in my paper the other day,<sup>24</sup> leads to rather extreme conclusions. And so that disagreement would remain. I also would keep on being critical of Rawls's attitude of creating a new absolute priority principle for every particular reason. I don't think this is a sensible thing to do. Again I mentioned this in my paper. And finally, I am very unhappy about his concept of a system, what he calls a "system of democratic equality", in which he essentially argues that people who do good things for society usually don't deserve any moral credit, or certainly not material rewards for it, because their talents are, of course, results of genetic luck. Which is of course true. But I would think that when people use their native talents and develop them further and use them for mutual benefit, that does deserve some social, I mean moral, credit. But I was particularly

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<sup>20</sup>See Diamond (1967) and Pattanaik (1968).

<sup>21</sup>See Harsanyi (1975a or b).

<sup>22</sup>See Harsanyi (1975a, pp. 316–317) or (1976, pp. 69–70)

<sup>23</sup>It seems he never responded to Pattanaik and may genuinely not have noticed the paper when it appeared. Approximately 18 months after the interview, he sent one of us (PJH) a fax which made it clear that he had fundamental disagreements with Pattanaik's paper, but that he preferred not to see them elaborated in the edited version of this interview.

<sup>24</sup>See Harsanyi (2008).

surprised ... that he argued that apart from ... having some talents, you need very often some kind of moral qualities which he describes as good character. And they enable you to work harder and to make greater efforts than people without good character could do, and that doesn't give you moral credit. I thought if anything gives you moral credit, it is exactly that you developed a good character and used it for good purposes. I think this [i.e., this Rawlsian system of democratic equality] comes very close at least, if it doesn't [actually] get into hard determinism, which I think would destroy morality, ... and strange that a moral philosopher should use a theory which would really destroy the moral value of our actions. If we can't get any moral credit or discredit for them, this would happen. And of course, his main arguments are based on the implicit assumption that decent people will help him and other people to create a fairer and juster society. But if people might be apparently very fair and just but really have no moral merit in doing so, then this whole thing collapses.

**Cd'A:** *What about primary goods?*

**JCH:** I doubt very much that you can have a sensible index. And I doubt very much that under Rawls' own assumptions such ... an index, which would be an economic index, should have moral significance.

**PJH:** *And you really don't think that these objections to Rawls would be somewhat muted at least if he moved to an expected utility position in the original position?*

**JCH:** Of course. Of course, I find the maximin principle simply absurd as a decision rule. So clearly if he ... somebody ... gives it up ..., that's a benefit. But ...

**PJH:** *But there'd still be serious differences?*

**JCH:** Yes, yes. Really, I think that this is a technical mistake, if it is a mistake, in decision theory. And I much more, much more object to his basic moral views than to decision-theoretical views.

**PJH:** *So, all readers will know that you were awarded the Nobel Prize in 1994, jointly with John Nash and Reinhard Selten, for your contribution to game theory. And that a most important contribution to game theory is your treatment of games with incomplete information played by Bayesian players.<sup>25</sup> These games have very many applications, of course, but we wonder if you could highlight some relation to social choice and welfare.*

**JCH:** Well, if you consider the theory of rule utilitarianism, you have to ask yourself the question: How much social utility would be contributed by a particular moral code which society accepts? And that is hard to predict if you don't know the types of the subject of the moral code. Especially in modern societies, ... even if utilitarianism became the leading philosophy, surely there would be many people who, for religious reasons or for other reasons would use a different moral code. And there would be some people who don't care about any moral code whatever. Now, such people's behaviour would be hard to predict without trying to estimate the probabilities that they are of one type or another, so I think such questions to be useful.

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<sup>25</sup>See J. C. Harsanyi (1967–1968).

**PJH:** *Do I understand that you are in the process of writing a book in which these themes may be addressed?*

**JCH:** Well, I was. I was when the Nobel Prize news came out in October '94. I was writing a volume but I didn't have any opportunity to go back to it up to now. I hope I will be able to.

**Cd'A:** [amid laughter] *A Nobel Prize is very time consuming.*

**JCH:** Let me mention that—though the Nobel Prize committee mentioned this, and this certainly is one of my contributions, the theory of games of incomplete information—I also did, I think what is worthwhile work on Nash's theory and on ... on the generalization of the Shapley value to games with ... non-transferable utility.

**PJH:** *You mean the Nash bargaining solution?*

**JCH:** Bargaining, yes, and let me just mention as a curiosity, that when I read Nash's work, I then tried to generalize it to  $n$  persons, and what came out of it was, of course, the Shapley value. And then I sent it to the *American Economic Review* and got the answer: This looks like an interesting paper, the pity is that this solution concept was developed a few years earlier by Shapley.

**Cd'A:** *Now, if we come back to incomplete information, the so-called "Harsanyi doctrine"—I think Aumann gives the name—requires consistent beliefs (differences between individual probabilities are explained by differences in information). Now, the impartial observer approach to utilitarianism requires a fundamental (or inter-individual) utility (differences between individual utilities are explained by differences in causal variables). Is this just an analogy? Or, if not, and then you think that incomplete information should be introduced, don't you find [that] the "preference revelation" problem would appear? What do you think?*

**JCH:** Well, I think there is certainly an important formal similarity. Both are based on the idea that any important general theory should try to explain differences between people, if they are relevant to the theory. So one should explain why people have different priors, and the so-called "Harsanyi doctrine" would try to do that. And then the other question is that people obviously have different preferences and this should be explained somehow, and I could explain it in terms of what I called causal variables. But once you do this, I don't think that the preference revelation problem really disappears because even if you have a general theory which is purely formal, ... we of course don't know how specific differences cause specific types of preferences (I mean differences in causal variables called preferences). So this is only a formal theory. But [in] the formal theory, even if we knew the actual rules which connect the causal variables with preferences, it wouldn't follow that we could easily decide what the true preferences of another person are. You still would have, the ... what do you call it, revelation problem. And so I don't think that my causal variables theory, whether it's correct or incorrect, would do much. For instance, suppose that we observed that people educated in particular universities pick up a particular ideology, and this affects their preferences very much. What would we do then?... Even [if] you could predict the person's preferences by knowing that he studied at Princeton, to some degree he might be lying about where he studied just as much as he might lie about his preferences.

**Cd'A:** *And do you think that this poses a problem with respect to the original position and the foundation of morality?*

**JCH:** Well, I think that you can set up a moral code for society without knowing exactly how many people are just pretending to have certain preferences and how many ... [really do have them]. That, of course, does cause a problem.

**PJH:** *Well, it imposes extra constraints on what is feasible, presumably?*

**JCH:** Yes, yes, it certainly would.

**Cd'A:** *Now, you have mentioned your contribution to bargaining, and maybe I would like to return to that, because this is a major part of your general theory of rational behavior, as described in your 1977 book.<sup>26</sup> But it is only applied to individuals deciding on practical issues from a personal viewpoint. Did you ever consider applying it from a moral viewpoint on ethical issues?*

**JCH:** No, I had a feeling that there are some theoretical reasons why one shouldn't do this because bargaining models are meant to be models of real-life bargaining ... and bargaining depends on the relative power positions of the parties. So, a good bargaining model won't predict what the fair solution of the bargaining would be, but what the realistically expected solution would be. And, on the other hand, I think in ethics, you shouldn't start from the assumption that some people are strong, some people are weak, and those who are weak should make major concessions to those who are strong because I really [think] fairness should decide this and fairness shouldn't depend, or at least only marginally, on the question how much power they have. I think the English distinction between right and might comes in. You should distinguish clearly between might and right. And those ... many, many game theorists did interpret even Nash's bargaining solution as a fairness doctrine, but I think this is a misunderstanding.

**Cd'A:** *And even the Shapley value was interpreted like that?*

**JCH:** Yes, surely.

**PJH:** *Now I suppose that the contractarians tend to use bargaining models in clear opposition to your approach. So, you would say that they are using them inappropriately?*

**JCH:** Yes, I wrote a book review of a philosopher who uses ... a bargaining approach.

**PJH:** *Gauthier?*

**JCH:** Gauthier, yes.<sup>27</sup> And I think there are many weaknesses in it, and this is one of them.

**PJH:** *One of your most celebrated arguments in ethics concerns rule utilitarianism, which advocates choosing general rules that maximize expected total utility, rather than acts. So, one way of distinguishing a rule like voting, even at some personal cost, over an act (like abstention)<sup>28</sup> seems to be that a good rule is one*

<sup>26</sup>See Harsanyi (1977a).

<sup>27</sup>See Harsanyi (1987).

<sup>28</sup>(PJH) This is what I had meant to say. The tape shows I actually said "abstinence", following a rough written draft of the questions we planned to ask. In any case, the question was not misunderstood.



*that is beneficial if most people follow it. Is this a correct interpretation? And, since a good rule may also allow exceptions, should it be a requirement for a particular (lazy) person to vote, even if it would be time-consuming?*

**JCH:** Well, I think that first of all, the government can do a good deal to get more votes out than they usually do. And there have been recently some measures adopted to make voting trivially easy, and that ... one could hope ... will increase voting.

**PJH:** *Voting, or registering to vote?*

**JCH:** Registering, maybe registering. But of course in California, for instance, you can always vote without any extra expense. Be an absentee voter if you want to, I mean if you don't want to stand in line.

**Cd'A:** *In Belgium, voting is compulsory.*

**PJH:** *And in Australia.*

**JCH:** Oh, is that so? So, I think it's a good idea to encourage people to vote. Though, of course there are counter arguments because one might say that those who wouldn't vote otherwise, ... they don't vote because they are absolutely uninformed about the issues. Maybe to get what they are saying is the "asinine vote" is not a good idea.<sup>29</sup> But in any case, in principle, of course, one should vote. And if the law requires, at least if the social customs require, then one should vote. But of course this is subject to the general rule that if you have some minor duties, if complying with them would cause you hardship, then you are basically exempt.

**PJH:** *Now, I asked the question about voting because I remember early papers on rule utilitarianism where this is an example you had used.<sup>30</sup> Now it seems you prefer to discuss promise keeping. Is that right?*

**JCH:** Yes. Yes. Well I used it [i.e., the voting example] because it was easy to connect with a game-theoretical equilibrium point. And it has interesting properties. But I'm not sure that for ethical purposes, this is the most informative example.

**PJH:** *Right, but it does capture the essence?*

**JCH:** Yes, it does.

**PJH:** *So, finally, what would Harsanyi the game theorist say about resolving the prisoner's dilemma by having each prisoner follow the utility increasing rule of not confessing?*

**JCH:** Well, ... I would assume that normally a prisoner in that situation has a moral duty to confess if he's guilty. But on the other hand, it's a common experience that our self-interest points one way and our moral duties point the other way. And, of course, depending on the situation, it might be a sort of pardonable sin not to admit that they are guilty, though in many cases they should, because otherwise some other person might be found guilty. So, it's just the typical case that self-interest is one way and moral duty goes the other way.

**PJH:** *Now, that reply, I think, is taking the prisoner's dilemma literally.*

**JCH:** Yes.

<sup>29</sup>Here the recording is too indistinct for us to be sure that "asinine vote" is John's exact phrase, but it is our best guess and certainly conveys rather forcefully what we believe he meant.

<sup>30</sup>See Harsanyi (1977b), especially Sect. 7 (pp. 38–41). The previous section of this paper discusses promise keeping, in fact.

**PJH:** *But as you are well aware, so many works use it as an example of conflict between cooperative and non-cooperative behaviour. So, if you thought of one of these more general situations, is the answer similar?*

**JCH:** Well, no.... Usually,... if there is any moral duty involved, of course, it is to cooperate. And the only problem is that cooperation might be unstable in such situations. As ... was very clear during the cold war, there were some pacifists who argued that the United States should disarm if necessary unilaterally, which I could never agree to. But, on the other hand, clearly ... we were more lucky than wise to avoid a catastrophe. And, of course, it was very good that we could avoid it. But, ... Rapoport always argued that we should unilaterally disarm. And ... [on] the whole, he was very hostile to the very concept of Nash equilibrium because he thought that would justify the cold war situation.<sup>31</sup> But,... clearly your moral duty may be to disarm if it's possible safely, possibly only by mutual agreement, but it can be only crazy to disarm unilaterally, given the other side being what is was. So ...

**PJH:** *So you should think carefully whether the other side is really going to follow the rule?*

**JCH:** Exactly, and really the problem is mutual trust—not [whether] one should trust trustworthy people, but to trust people who are obviously untrustworthy is crazy; it's not moral.

**PJH:** *So, is there anything else we should have asked you?*

**JCH:** I don't know. You are kind enough to spend a lot of time with me.

**Acknowledgments** The following is an edited and slightly abridged version of an interview which John Harsanyi kindly agreed should take place on June 19, 1996. This was during the Conference on “Justice, Political Liberalism and Utilitarianism in Honour of John Harsanyi and John Rawls” at the University of Caen. In particular, the footnotes, references, and text in square brackets have been added by the interviewers, and some hesitations or repetitions removed. Our warm thanks to Maurice Salles for arranging the interview room (as well as the conference), to Lucien Bézier for producing a sound recording, to Tina Corsi and John Weymark for transcribing that recording, and to Kenneth Arrow for some advice concerning the final editing process. Finally, our thanks also to Dr. Ghanshyam Mehta and to Tanya Ziebell for bibliographic research in the Economics Library and the Archives section [UQA S 135 Staff files (1911–)] of the University of Queensland, and especially for passing on information concerning the dates and titles of the two theses—Harsanyi (1947, 1953b).

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<sup>31</sup>This refers to an old debate that Harsanyi had with Anatol Rapoport. See Harsanyi (1977a, pp. 276–8) and the work cited there—especially Rapoport (1966).

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