Available online at www.sciencedirect.com
ScienceDirect
Games and Economic Behavior 64 (2008) 355–360
GAMES and Economic Behavior
www.elsevier.com/locate/geb

# Working with Mike ☆

Mike and I worked together for half a century. To date, I've had some twenty-five collaborators—co-authors of joint papers and books; there's no question that Mike is the premier one. He and I wrote three joint papers—the original Bargaining Set paper (21), the one on the Minimax Principle (38), and the Talmud paper (50)—and one joint book (64), about repeated games of incomplete information. These works are among my own most important, and perhaps among Mike's, too. To be sure, each of us also did other things, but these stand out.

Our collaboration started in the late nineteen-fifties, two or three years after I'd come to the math department of the Hebrew University. Mike was working in complex function theory, in which he had done his thesis; I had already made the switch from my thesis topic—knots—to games. One autumn afternoon I spoke at the mathematics colloquium, a weekly gathering of the whole department where a faculty member or guest gives a talk that's supposed to be of general interest to any mathematician, not only a specialist. I decided to speak about the von Neumann–Morgenstern (N-M) "solution," a.k.a. stable set (von Neumann and Morgenstern, 1944). This is a very subtle and beautiful solution notion for coalitional games; at the time, I myself did not fathom its full beauty and subtlety, which became apparent only after subsequent work with Mike. During the question period after the lecture, Mike asked several questions that challenged its appropriateness. As the discussion lengthened, I suggested continuing in private, which we did. I did what I could to explain the N-M notion, but could not satisfy Mike. At last, a little exasperated, I said, well, let's see if you can come up with something better. He said, OK, give me a couple of days. That started a lifetime of friendship and collaboration.

Mike indeed came up with a proposal after a few days, which I promptly "shot down." That is, I constructed a "counterintuitive example": a game in which the proposed definition yields unacceptable results. This process continued for many months—Mike would propose a definition, and I would shoot it down. Finally, as the academic year was drawing to a close, Mike came up with a definition that I could not "shoot down." I didn't like his definition, and told him so; it seemed overly complex and arbitrary, lacking elegance and simplicity. There wasn't even a general existence theorem; it was, indeed, sometimes empty. But, I could not shoot it down.

Shortly thereafter, I left Israel for an extended trip to the United States, and occupied myself with other matters. To my surprise, some time later I received a manuscript from Mike entitled "The Bargaining Set for Cooperative Games"—containing Mike's definition, some worked-out examples, and some additional analysis—by R.J. Aumann and M. Maschler! I wrote to him that

☆ Numbers in parentheses refer to the list of Maschler's publications that is included in this issue of the journal.

0899-8256/$ – see front matter 
doi:10.1016/j.geb.2008.09.001

this was 100% his work, that all I had done was to shoot down previous attempts at a definition, and that there was absolutely no justification for including me as a co-author—all the more so as I really did not like the definition. But Mike was a stubborn guy, he insisted, really kept at me for weeks and months; and finally, out of sheer exhaustion, I capitulated.

That paper has been cited many hundreds of times; it became one of my—and no doubt Mike's—most popular works. Mike's stubbornness really paid off. Moreover, the paper led to a very large literature, it was truly seminal. Later offshoots—one might say descendants—of that original concept were the Maschler–Davis Bargaining set $M_1^i$, for which there *is* an existence theorem (with a beautiful, highly non-trivial proof), and which is altogether more pleasant to work with, as well as the Kernel (24) and Nucleolus (Schmeidler, 1969); taken together, these concepts constitute one of the richest, and yes, most elegant chapters of game theory, with a great many applications yielding beautiful insights. Much of this theory was developed by Mike, alone or in collaboration with game theorists such as Davis, Peleg, Shapley, G. Kalai, Owen, Curiel, Tijs, Granot, Potters, Zhu, and others (22, 24, 26, 28, 31, 33, 39, 41, 42, 43, 46, 50, 58, 60, 61, 65, 67).

Mike was good not only at theory, but also at applying it. Here's that story: I spent the academic year 1960–61, on sabbatical from Jerusalem, with Oskar Morgenstern's outfit—the Econometric Research Program—at Princeton University. In October of '61—as my stay was drawing to a close—there was a conference at Princeton entitled "Recent Advances in Game Theory," sponsored by Morgenstern and Harold Kuhn. All the luminaries of Game Theory came, including Shapley, Shubik, Scarf, Morgenstern and Kuhn themselves, and even Henry Kissinger—later to become Secretary of State of the United States—who was analyzing Cold War games. Mike spent the year '61–62 with Morgenstern, and was given the task of putting together the conference proceedings. In March of '62, in Jerusalem, I received a telegram (does anybody still remember what that was?) from Mike, informing me that the deadline for sending in papers had passed, that all the other participants had sent their papers in, and that if mine was not in within one week, he would go to press without it. I immediately dropped everything else, worked around the clock to get my paper written and typed, and rushed it off to him. Sure enough, the proceedings came out in April, right on time.

Afterwards, it transpired that when Mike sent that telegram, he sent similar telegrams to *all* the other speakers. *Not a single paper had come in yet.* And it worked! The conference proceedings came out on schedule, and became a game-theory classic.

During 1964–65, I was again on sabbatical, this time at Yale. Mike, who was back home, suggested that we sponsor a game-theory workshop in Jerusalem in the summer of 1965. I agreed; perforce, Mike did almost all the preparatory work, raising money, making the reservations, and so on. This workshop was quite different from previous—and for that matter, subsequent—workshops and conferences. There were only 17 or 18 participants, and the workshop was spread out over three weeks. Thus there was only one presentation per day, lasting perhaps an hour or so. *All* the rest of the time was devoted to informal discussions in small groups. We even rented a room in the hotel, with coffee and cake available, where people could talk informally in the evenings whenever they wanted. The participants included Harsanyi, Selten, Shapley, Shubik, Joachim Rosenmuller (who at that time was a young student), and others.

The results were spectacular. Selten's perfect equilibria (Selten, 1965, also Selten, 1975)—which led to the whole enormous refinement literature—as well as Harsanyi's games of incomplete information (Harsanyi, 1967/1968) were initially promulgated at this workshop. It's of course possible that the authors had already thought of these things before coming to Jerusalem

in 1965, but there is no doubt that the discussions at the workshop had an important early formative effect on these developments.

One of the most exciting periods of my life—and probably of Mike's, too—was the late Sixties, when we were working with the US ACDA, the United States Arms Control and Disarmament Agency. This was a US government agency whose job was to conduct nuclear arms control negotiations with the now-defunct Soviet Union. A consulting outfit based in Princeton called Mathematica, whose principals were Oskar Morgenstern and Harold Kuhn, had contracted with the ACDA to bring some game theory to bear on these negotiations. The work began in 1964–65, when a team that included Mike, Harold Kuhn, Frank Anscombe and others examined the game theory of nuclear weapons inspections; the question was, what provisions to write into the treaties to provide reasonable assurance that treaty provisions were being kept. This team wrote a report that became famous in the inspection literature, the star items being Mike's papers on the "Inspector's Non-Zero Sum Game" (25, 32).

In 1965, the emphasis changed from inspection to other aspects of the negotiations, including the effects of repetition; the negotiations were drawn out over many years, creating a repeated-game effect. At that time the team changed; Anscombe and some others left, and on board came a more game-oriented crew: Gerard Debreu, John Harsanyi, Reinhard Selten, Herb Scarf, Jim Mayberry, and the writer of these lines. Maschler and Kuhn stayed. Later, Dick Stearns joined the team. Between 1965 and 1968, we met three or four times a year for several days each time, usually in the Washington area. The agency was represented by Tom Saaty, an American OR specialist of Lebanese origin, very likable, capable, and knowledgeable. These meetings were extremely intense; for sixteen hours a day we would brainstorm with each other, meet with the agency staff, report on what we had done individually since the last meeting. Between meetings, back in Jerusalem, Mike and I—occasionally joined by Dick—would work very intensely, sometimes until three or four in the morning. And, we got results.

One time—it must have been in '67 or '68—we were working in my flat in Jerusalem in the wee hours of the morning. On my previous trip to the States, I had brought back one or two delicious kosher beef salamis, of a kind that was impossible to obtain in Israel. As we were getting a little hungry, I decided to serve sandwiches with my prized salami; it made an immediate hit with Mike. When he had finished one sandwich, I asked if he would like another one. Sure, he said, but don't bother with the bread. He always liked to get to the meat of things.

It was in this atmosphere that the theory of Repeated Games with Incomplete Information was born. To illustrate its relevance to the work of the ACDA, suppose that the US and SU (Soviet Union) are considering a treaty that provides for the destruction of a stated number of nuclear bombs on each side. Of course, what concerns the parties is not the number of bombs destroyed, but the number *not* destroyed, the number remaining; but it is much easier to verify that a bomb has been destroyed than that it remains. So we have a game of incomplete information: the payoff is in the number of bombs remaining, which can only be guessed at; thus the players do not know the payoffs, even their own. Harsanyi's theory of games of incomplete information had just been born and was very much in the air; Mike and I decided to apply it to the repeated games context that was inherent in the repeated Arms-Control negotiations between the US and the SU.

The theory created in those years was initially written up in four reports (30, 34, 35, 36); they started a large, rich and mathematically deep literature, to which dozens of people contributed, that continues to develop to this day. For years, it was very difficult to get one's hands on the reports; bootlegged copies were secretly handed from one researcher to the other. Finally, in 1995, the reports were edited and issued in book form (64), with "postscripts" detailing what had

happened in the area since the Sixties. The publication of this book is a saga in itself, to which we return below.

It is difficult to convey the palpable excitement of those years. We felt that we were unravelling secrets of nature, like in the natural sciences. The questions asked were indeed very natural; they were also difficult, and it was very exciting to get a result after weeks and sometimes months of working on it.

Throughout our many decades of joint work and interaction—which extends far beyond the jointly published work—we had many sharp disagreements, which sometimes even degenerated into shouting matches; some of them had conceptual or scientific substance, whereas others were about matters of presentation, including even the minutiae of printing. One disagreement with conceptual substance occurred when we were writing the paper about the minimax principle (38). To resolve the matter and go to publication, we finally hit on the idea of writing, "Some people feel that ... Others disagree, holding that ..." (Section 6). Of course, the "some" referred to one of us, the "others" to the other one. I don't remember now which was which; but I do remember that at the time, it seemed as if Western civilization would stand or fall on this issue.

In the year '80–81, while on sabbatical at Stanford, a preprint of a paper by Barry O'Neill (1982) crossed my desk. The idea that there was something of game-theoretic interest in the 2000-year-old Talmud fascinated me; I sent the paper to my eldest son Shlomo, then studying at a Talmudical academy in Jerusalem. Shlomo wrote back, laconically, "Dad, look at Ketuvot 93a" (a standard form of reference to one of about 5000 folio pages in the Babylonian Talmud). I did look, and found a passage that was indeed related to O'Neill's work, but that was nonetheless extremely puzzling. The Talmud considers three cases of bankruptcy—with debts to three creditors totalling 600 and assets of 100, 200, and 300 respectively—but the payouts that the Talmud decrees do not seem to follow any fixed rule. I could not make sense of it.

After returning to Jerusalem in the fall of '81, Mike and I sat down to try to figure out what is going on in that passage. We put the nine relevant numbers on the blackboard in tabular form (50, Table I) and gazed at them mutely. There seemed no rhyme or reason to them—not equal, not proportional, nothing. We tried the Shapley value of the corresponding coalitional game; this, too, did not work. Finally one of us said, let's try the nucleolus; to which the other responded, come on, that's crazy, the nucleolus is an extremely sophisticated notion of modern mathematical game theory, there's no way that the sages of the Talmud could possibly have thought of it. What do you care, said the first; it will cost us just fifteen minutes of calculation. So we did the calculation, and the nine numbers came out precisely as in the Talmud!

Needless to say, that was only the beginning of the research. As we'd said earlier, the sages of the Talmud could not possibly have known of the nucleolus. Rather, we figured, the nucleolus probably has some general property that corresponds to a principle that *was* within the sages' reach.

Where would one look for such a principle? Well, a natural place is in an axiomatization. At the time, we didn't know of any axiomatization of the nucleolus; but a literature search revealed that several years earlier, the nucleolus had been axiomatized by a Russian mathematical game theorist by the name of Sobolev. The central axiom was *consistency*; roughly, that if you give some of the players the amounts that the nucleolus assigns them, and consider a new game among the remaining players for the remaining money, then the nucleolus of the new game gives the remaining players precisely what it gave to those same players in the old game. I.e., for the nucleolus, it doesn't matter whether the payouts are made in stages or all at once. This principle, as applied to the bankruptcy problem, was indeed within the sages' reach.

It took many more months to unravel the puzzle completely, but consistency did turn out to be the key. The full story is told in (50), which became widely known not only in game theory circles but to the general public as well—especially that with some interest in the Talmud. Largely in the wake of this paper, the Consistency Principle gained considerable notoriety; see, for example, Mike's papers (53, 55, 56, 59).

In June of 1982, my son Shlomo—the one who had first called attention to the apparently strange Talmudic passage—was killed in action while doing military reserve duty in "Operation Peace for Galilee." Mike was distraught. As soon as he heard the news, he rushed over to my house and sat on the stairs, unable to talk. During the "shiv'a"—the seven traditional days of mourning—he must have visited at least half a dozen times.

At some time in the mid-Eighties, we were approached by MIT Press to bring the old ACDA reports up to date and publish them in book form. We readily agreed to this proposal, and it came to fruition with the 1995 publication of "Repeated Games of Incomplete Information" (64), which won the Lanchester prize for the best OR book of that year.

Why did the production of this book take almost ten years, though all the research was already in place and indeed had been written up even before we began? Perhaps the major reason is that Mike had become a Tex afficionado shortly before, and insisted that the typesetting be done under his direct supervision, at the math department of the Hebrew University. I tried to tell him that we are mathematicians, and to some extent writers, but certainly not typesetters; the typesetting should be left to the publisher, who would do it for nothing, no doubt better than we possibly could. But when Mike had set his mind on something, there was no moving him. He insisted, and I capitulated. A typist was hired, and over the course of almost ten years, we spent some fifty thousand dollars of research money to pay her for the typesetting, not to speak of hours spent on endless discussions of the minutiae of Tex and of printing. Above, I mentioned some advantages of Mike's stubbornness; but this particular project of his does seem crazy, even in retrospect.

In a lighter vein is the following story. By the late Eighties, I still had not learned to work with computers. But when a favorable deal became available, I decided to invest in a "small" (ten kg) computer for use at home. I liked it, so when several months later, Mike proposed that we spend some research money to buy computers for use in the office, I readily agreed. And then Mike told the following (politically incorrect and chauvinistic) story: Computers are like women in three ways: (i) You tell them to do one thing, and they do something else; (ii) you can't manage with them, and you can't manage without them; and (iii) after you have one at home for a few months, you want one in the office, too.

And while on the subject of stories, Hanna—Mike's widow—relates the following: In addition to his work in complex variables, game theory, and experiments, Mike was a marvellous teacher at all levels. Indeed, he wrote several textbooks in general math for seventh and eighth grades (in addition to High School and University texts in game theory), which were, for a long time, *the* texts generally used in Israeli schools. One September day, Hanna was visiting a bookstore in downtown Jerusalem, and heard one young girl say to another, perhaps you have at home a used Maschler in good condition? Whereupon Hanna intervened and said, I do, but I'm not selling.

Mike's outstanding characteristic was his total honesty and straightforwardness. If he did not understand something, he would tell you right out; if he disagreed with you, he would tell you right out—and even insist, to an unreasonable degree, as mentioned above. If he refereed a paper and had a question or remark, he would write straight to the author, without any attempt to hide his identity. His stubbornness was, I think, associated with this extreme honesty.

Another outstanding characteristic was his generosity, which was also extreme, and which is also mentioned above. A minor chance remark regarding a paper would be enough to make him offer you joint authorship; and he was always extremely scrupulous in assigning credit.

Altogether, working with Mike for fifty years was exciting, fun, and a true privilege. I think we made some real progress, and am sorry it has come to an end.

## References

Harsanyi, J.C., 1967/1968. Games with incomplete information played by 'Bayesian' players, I, II, III. Manage. Sci. 14, 159–182, 320–334, 486–502.

O'Neill, Barry, 1982. A problem of rights arbitration in the Talmud. Math. Soc. Sci. 2, 345–371.

Schmeidler, D., 1969. The nucleolus of a characteristic function game. SIAM J. Appl. Mathematics 17, 1163–1170.

Selten, R.C., 1965. Spieltheoretische Behandlung eines Oligopolmodells mit Nachfragetragheit. Z. Ges. Staatswissen. 121, 304–324, 667–689.

Selten, R.C., 1975. Reexamination of the perfectness concept for equilibrium points in extensive games. Int. J. Game Theory 4, 25–55.

von Neumann, J., Morgenstern, O., 1944. Theory of Games and Economic Behavior. Princeton University Press, Princeton.

Robert J. Aumann
*E-mail address:* raumann@math.huji.ac.il

10 September 2008

Available online 19 September 2008