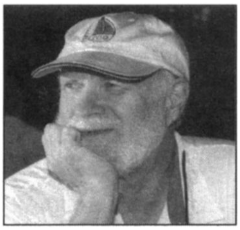
David Gale: Restless Pioneer

Walter Meyer



Walter Meyer (meyer@adelphi.edu) received his Ph.D. from the University of Wisconsin in 1969 and his research has been mainly in geometry and combinatorics. He has written or collaborated on four books, including *For All Practical Purposes* and *Geometry and Its Applications*. For most of his career he has been a professor at Adelphi University (Garden City, NY 11530), but in addition he was a fellow at IBM's Systems Research Institute, he carried out research in geometry related to robotics at the Grumman Corporation, and he was a visiting professor at the U.S. Military Academy. He likes soccer and is trying to learn how to cook, which isn't easy at all.

David Gale made his first mark on mathematics while a graduate student at Princeton just after World War II. He was working in the new fields of game theory and linear programming, and collaborated on new and simpler presentations of duality and the minimax theorem. He is cited by John Nash as being partially responsible for the simplicity of the proof of the theorem for which Nash was awarded the Nobel Prize. Gale continued in the related field of mathematical economics as a professor, but he was always willing to look into something new. For example, he also did research in the geometry of convex sets; and, in combinatorics, the Gale-Shapley matching algorithm established a new and still-vigorous field of research. Another example of Gale's eclectic tastes is the book he wrote on recreational mathematics. He is a member of the National Academy of Sciences and an Emeritus Professor of Mathematics at the University of California at Berkeley. Gale has a strong interest in music, especially traditional and contemporary jazz, which he has been following for seventy years. He also enjoys regular stays in his small Paris apartment.

Gale's Youth

Meyer: Let's start with your boyhood. What impression did grade school make on you?

Gale: I went to a private school in New York City. When I was quite young, seven or eight, I wondered, "What was the exact time around ten past two when the big hand was exactly on top of the little hand?" It wasn't ten past because by that time the little hand had moved ahead, and by the time the big hand caught up, the little hand was again a bit further on—the usual Zeno thing. I didn't think of this as a paradox though; I simply wanted to know when the moment of overtaking occurred. When I finally found out in ninth-grade algebra, it was a revelation. Instead of attacking the problem directly as I had done, you do it indirectly and look at the properties of the possible solution. You know at the moment of overtaking: first, that the big hand has moved twelve times as far as the little hand, and second, that the difference in the distances moved is 10. Put them together and voila!

When I was about 10 or 11, I wondered why the product of two negative numbers was positive. There were people who tried to explain it to me but I never got a good explanation until I got to graduate school. I had also been told that in high school I would learn how to add letters instead of numbers, but I couldn't get anyone in my family to explain that to me. So I was interested in mathematics from an early point in my life. But, as a ten-year-old, I didn't know whether to be a magician or a mathematician.

Meyer: Some of your research was in geometry. Did you become interested in geometry at an early age?

Gale: I liked geometry all through high school. That was really a great revelation. You could prove things. It wasn't just remembering how to factor polynomials; it was a whole new world that opened up at that point.

Meyer: Would you say you were practically-minded, or more given to imagination? Were you studious?

Gale: Not practical—it was the abstract part of mathematics that appealed to me. Studious? I tried to get good grades on exams with as little effort as possible. I was interested in literature and poetry and a lot of other stuff. I still have an interest in poetry. My partner is quite a well-known critic and poet. So I have an everyday connection to poetry.

Meyer: Did your family influence your intellectual development in any particular directions?

Gale: There weren't any scientists in my family. My father was a lawyer, and he encouraged me to go into more academic things. But my parents were happy to have me do whatever I wanted to do. Going to college was probably always likely. My father went to Yale and my mother to Wellesley, so college was traditional in the family.

Meyer: As I recall, you went to Swarthmore as an undergraduate. What was the environment there like?

Gale: We worked pretty hard. I started out as a math major; then the war came along and I switched to physics, and you had to put in the hours to do that. It was a nice school. I'm not sure in retrospect whether I would have been better off going to a bigger college, like Yale or some place like that, because the mathematics wasn't up to date at Swarthmore. Birkhoff and Mac Lane's book had come out, and good places were teaching modern algebra. At Swarthmore, people didn't even know about that. I didn't learn about linear algebra or group theory. Of course we're talking about the early forties. Things weren't as universal as they are now.

Graduate School at Princeton and Michigan

Meyer: You were a graduate student at Michigan and, then later, Princeton. How would you compare those environments?

Gale: Michigan was traditional in the way it operated. There were some excellent courses; I worked hard, and I learned a lot, and I enjoyed it. The reason I went to Princeton, even though Michigan had a very strong department, was that three of the main professors at Michigan, Samuel Eilenberg, Norman Steenrod, and Warren Ambrose all left in one year and it looked as though the place was falling apart. So I decided I should move to Princeton. I thought I was going to do topology with Steenrod.

Princeton had a different style from Michigan. It took in a very small class. Some of the students at Princeton were self-educated and knew a heck of a lot more than I did. There were no exams and nobody ever checked whether you showed up for class.

You were completely on your own. I wasn't prepared for that. But I got used to it. I got into this linear programming and game theory stuff with A. W. ("Al") Tucker sort of by accident, and that worked out.

Meyer: So how did you come to know Professor Tucker?

Gale: I think he just recruited Harold Kuhn and me—that was in the summer of 1948. He got interested in game theory and linear programming and got some kind of a grant and said, "Would you like to see what this is all about?" So we read parts of von Neumann and Morgenstern's book—that was my first introduction to game theory. Game theory is closely related to linear programming, and one of the things we did that summer was to work out the duality theory of linear programming. We later found that this allowed a simpler, clearer theory for the central minimax theorem for two-person zero-sum games.

Meyer: So Tucker just picked you out, so to speak?

Gale: Yes. Of course at Princeton there were only eight professors on the faculty, so everybody knew everybody else. I hadn't anticipated working with Tucker when I went there. As I said, I thought I'd work with Steenrod.

Meyer: In her book *A Beautiful Mind*, Sylvia Nasar says there was a pecking order at Princeton around the time that you and Nash were there, around 1950, some subjects having more prestige than others. Did you perceive this?

Gale: Yes, there were analysis, geometry, and algebra, and they had the top people in all of those—that's what Princeton was all about. Then when upstart things like game theory and linear programming came along, people said this is just a reworking of old stuff. Yes, I think there was some question of whether this was really Princeton-level mathematics. Maybe there still is that feeling. None of this is in the class of Wiles solving the Fermat problem.

People sometimes say that game theory and linear programming should be taught in other departments, such as engineering, operations research, or economics departments. And it is. I taught linear programming in the operations research department at Berkeley. I also gave a course in it in the mathematics department. My initial appointment here at Berkeley was joint in those two departments. Actually, for a while I was listed in the economics department as well, but none of my salary ever came from economics.

Meyer: When you decided to write your dissertation in game theory, did you think at all about this issue of whether game theory was suitable mathematics? Did you just feel rebellious or confident?

Gale: I felt interested. I said, "Here is a nice problem—maybe I can do it." And that's why I did it. Sure, that pointed me in a direction I hadn't anticipated when I started graduate school.

Meyer: That must have been an interesting experience—to all of a sudden have a new vista.

Gale: Yes, it was quite interesting. I liked the subject too. It was applied mathematics—games of strategy—but the way I attacked it was through the geometry of convex sets, so it was very attractive.

Meyer: I know the proofs in this area are ultimately algebraic, but what was your thought process? Did you think about the problems the way a geometer would, visually?

Gale: Very much so. I have to draw pictures. Yes, the three of us that summer (Tucker, Kuhn, and I) came up with a way of visualizing the von Neumann minimax theorem.

I think in my book[1] I try to describe that—about how you move a convex set until it just touches the original and you have a separating hyperplane that separates it from the negative orthant. That's the kind of mathematical way that my mind works. That was appealing.

John Nash

Meyer: What sort of a person was your fellow graduate student John Nash?

Gale: He was an obnoxious genius. He was very smart, but he really upset people. I had two really close connections with him. The first concerned his independent discovery of the game of Hex. Nash told me one morning at the graduate commons that he had a game that he could prove was a win for the first player, but he said he had no idea how the first player should play to get the win. He described his game on a checkerboard where you are trying to make a connected path from North to South and the other player is trying to make a path from East to West. You can be connected either vertically, horizontally, or on a positive diagonal. Then I said, "Let's make this more elegant—let's turn the cells into hexagons. You'd have the same game, but with a simpler presentation." I got a piece of composition board and made a 13 × 13 Hex board. No one had ever seen one before, but I left it in Fine Hall, and soon everyone was playing Hex. We had a lot of fun. I said, "I think this game has commercial possibilities. Why don't we try to promote it?" Nash agreed. I went around to various game companies in New York and showed it to them. They said they wouldn't do it because there was no luck involved in the game. It was purely a game of skill and wouldn't sell. I think we sent it to Parker Brothers at some point. But Nash was very worried that someone was going to steal his idea and make a lot of money, so he was very restrictive about what could and could not be revealed. This was about 1949 and afterward I saw Nash periodically around Princeton. Parker Brothers did eventually come out with Hex. However, they bought it from the original Danish inventor Piet Hein (an amazing guy who also invented Soma blocks and wrote jingles in newspapers). He had invented Hex without Nash or me knowing about it. Nash called me up in high dudgeon and accused me of ripping him off by selling his game to Parker Brothers. I think I convinced him that I was totally innocent and that Hein was the real inventor. That was just a few months before Nash had his first episode of mental illness. Next thing I heard he had disappeared from MIT and nobody knew where he was.

The second interesting experience I had with Nash was that he came to me one morning to tell me that he had a generalization of the von Neumann minimax theorem, something he called the equilibrium point theorem. This theorem states that in any n-person game there is an n-tuple of strategies giving an outcome that cannot be improved on for any player by that player alone changing his strategy. I said, "Wow, that's a pretty good theorem." When he described the proof, I saw that it could be proved more directly using the Kakutani fixed point theorem (he thanks me for that insight in a footnote of his paper). I said that he should get that in print quickly to establish that he got it first, so we wrote it up and gave it to Solomon Lefschetz, who was a member of the National Academy of Sciences and could get something published by them very quickly. It came out the next month in the *Proceedings of the National Academy of Sciences*, and that's the Nobel theorem.

Meyer: You were elected to the same National Academy of Sciences yourself later. Does that honor carry practical advantages?

[1] *The Theory of Linear Economic Models*, McGraw-Hill, 1950.

Gale: When you get elected to the National Academy, you're likely to get a big pay raise.

Brown University and Research in Mathematical Economics

Meyer: So after your dissertation you went to Brown in 1950. What kind of environment was that?

Gale: Well, we had a strong department with good, interesting, stimulating people. Each semester, we taught three courses, 9 hours a week. I was told to teach a graduate course in partial differential equations, which I didn't know anything about. It was strenuous. Looking back, I'm surprised that I managed to do it.

Meyer: When they hired you, were they particularly interested in you because you were in game theory?

Gale: When I was hired, it was a fairly autocratic department. The chairman was C. R. Adams. He would go down to Princeton and interview people, meet them and see what impression they made. I don't think there was any great desire to introduce game theory at Brown. I think it was just the fact that I made a good impression on him.

Meyer: What were the relations like between the pure and the applied mathematicians?

Gale: Not too cordial when I was where. I think it got much better after a while. Wendell Fleming had a joint appointment in both, and things came together a bit. When I was there, the applied people thought we were these purists living in an ivory tower and stuff. We thought they didn't have any outstanding mathematicians and their work wasn't as good as ours.

Meyer: When did the idea of having two departments, one pure and one applied, get started?

Gale: During the Second World War. Dean Richardson got a big war effort program and brought in famous people. In fact, my last semester at Swarthmore I spent at Brown taking courses from people who later became quite famous—Stefan Bergman and Jacob Tamarkin and people teaching aeronautical kinds of things. William Prager was the main guy. He stayed on at Brown after the war and set up the applied math department. He was a good businessman and a smart guy.

So far as I know, this idea of having a pure department side by side with an applied department did not exist at other universities in the U.S. At Berkeley we have an applied group within the math department who almost might be a separate department because there is not a lot of cross-fertilization. But it's all one department.

Meyer: You probably didn't have any collaborators in the department at Brown.

Gale: That's true, except for my PhD students. Nor in the economics department, really. There were some people in economics who were somewhat interested in what I was doing, but overall I was pretty much isolated. I spent a sabbatical year at RAND in '57–58, and there I was right in the midst of people interested in my research area, people like George Dantzig, Herman Kahn, and Lloyd Shapley. Dantzig is really responsible for me coming to Berkeley. After leaving RAND, he was in the operations research department here. Then he quit in a huff over some issue and went to Stanford.

Meyer: One of the things you worked on was the theory of optimal economic growth. I am wondering if it has any implications that could be turned into policy?

Gale: The model I worked on uses the price mechanism and reinforces the idea that the market can optimize a social welfare function. You need to assume that there is a social welfare function. That is a function whose inputs are the amounts of money to be invested in various sectors of the economy and the output is the amount of social welfare that results. Coming up with this function is the nonmathematical part—that is where the debate about values occurs. Someone has to give you that social welfare function. Then you can do the mathematics to work out how much to invest in various places in order to optimize that social welfare function.

In the simplest situation, you have one good. You have to decide how much of that good to consume and how much to invest. In the dynamic case, you want to do that over time. Ramsey's model solved that for one good. Then I did it with n different goods and n producers.

Meyer: Do you want to say anything about collaborators you had in mathematical economics?

Gale: With Dantzig, we spoke about problems together and studied each other's work. I saw Kantorovitch occasionally. He liked my book and invited me over to Russia, but we never worked together. With Koopmans—this is back in the early days—it was the same kind of relationship. Kenneth Arrow, Gerard Debreu, and a bunch of these Nobel prize winners[2] were part of my world. And there was John Nash.

Meyer: What is the status of mathematical economics today?

Gale: Economics is still pretty mysterious. George Bush senior was the most popular President we had according to the polls at one point, but he was voted out of office because of a recession whose cause economists disagree about. Similarly, there was no recession through eight years of the Clinton administration, and no one understands that either. Then there is the Great Depression. You know Keynes had a famous book about it, but it is not so clear that his theory is an acceptable scientific theory. Economics is a really difficult subject that needs a better theory.

Meyer: Do you think mathematics will play a useful role in making progress on economic theories?

Gale: Yes, but I don't think we need to invent a new calculus or new branch of mathematics to do it. If it ever gets done. Some people have to have insight and formulate the models. It is a matter of getting the right set of axioms for how people and countries behave. And then seeing if the consequences of those axioms agree with what you see in the outside world. It's very important. If you had a good theory, you would know what policies to put in place, what institutions to use. The distribution of incomes is a terrible problem in this country. What kind of policies should you put in place so it would level out—aside from just taking money away from the rich and handing it to the poor?

Meyer: While you were thinking about linear economic models, optimization, and game theory at the research level, John Kemeny, J. Laurie Snell, and Gerald L. Thompson were writing a book about this for undergraduates, *Introduction to Finite Mathematics*. Did you take an interest in that?

Gale: Yes, I wrote the review of that book for the *Monthly*. Kemeny was a very dynamic and interesting guy who did original things. He was Einstein's assistant when I got to Princeton, but he was also very interested in the foundations of mathematics. Very smart. Quite amazing that a Jewish Hungarian immigrant became president of Dartmouth, this supposedly most lily-white of the Ivy League schools.

I think it's all because of Sputnik, which made a big change in the culture. For example, at Ivy League schools they could have one or two Jewish professors in the

[2]All those mentioned in this reply won Nobel Prizes in economics: Kantorovitch and Koopmans in 1973, Arrow in 1972, Debreu in 1983, and Nash in 1994.

department, but it was almost explicit that there was a quota. And in admissions too. Until Sputnik. Then, all of sudden I think, the walls came down. You just had to hire the best, regardless of who they were. At a certain point, a huge number of good mathematicians were Jewish, many of them educated in New York City, and they became important in American mathematics. I'd like to see if research would confirm my thoughts about this Sputnik effect.

The Gale-Shapley Matching Algorithm

Meyer: How did the work on the Gale-Shapley matching algorithm come about?

Gale: There was an article in the *New Yorker* in 1958 or so, where they interviewed the dean of the graduate school at Yale on the matter of how they ran their admissions office. They had to make offers to students but they had no idea about which offers would be accepted. It seemed to me that you could rationalize this procedure somewhat. You'd get all the colleges to rank the applicants in order and you'd ask all the students to rank the colleges and then let a computer decide who goes where according to these rankings. To begin with, I thought about the case where each college just wants one student. I came up with a definition of stability, which is a key thing you would like to require. A matching is stable if no student, S, prefers some college, C, to the college the algorithm matches him to and where college C prefers S to the student they are actually matched to by the algorithm.

Meyer: But the Gale-Shapley algorithm is often presented as a solution to matching up men and women for marriages.

Gale: Yes, our paper, which came out in the *Monthly*, was entitled "College Admissions and the Stability of Marriage." The marriage interpretation seemed more natural (and more fun) than colleges that admitted only one student, but it turned out to be a bit of a problem with the Office of Naval Research (ONR), which provided support for my research. We sent the paper to ONR so it could be circulated through their channels as one of their reports. One morning I got a call from Washington from one of the ONR science people. He said they had found the result interesting but were a bit worried about the title. What if a supervising admiral should come by and question whether promoting marital stability should be part of ONR's mission. They wondered if perhaps I couldn't think of an alternative title for the ONR version of the work. I saw the point and asked, "How about 'On Allocation of Personnel in Accordance with Mutual Preferences?" They seemed quite satisfied with this.

Meyer: So how did Lloyd Shapley come into the picture?

Gale: For a while I worked by myself trying to prove there was always a stable matching. But I found that if you don't have two sexes—say you had men you wanted to match up in pairs to be roommates—then there might not be a stable matching. I found an example with just four people that illustrated that. So it wasn't at all clear to me that when you had two sexes that there would be a stable matching. So I started running around to all of my friends and colleagues who liked to solve problems. None of them had the answer except Shapley, who wrote back with an algorithm and saying there was always a stable matching in the two-sex case.

But you know, it turns out that we had been partly scooped. By the time we published in 1962, this problem had been posed eight or nine years earlier in the case of assigning graduates of medical schools to residencies in hospitals, and an algorithm had been found to do it. The medical folks had invented this algorithm independently. I don't think they had the concept of stability, and they didn't prove any theorems about their algorithm.

Meyer: I have wondered whether the Gale-Shapley algorithm could be used for matching up mathematicians looking for jobs with departments that needed to hire someone. Has anyone at AMS ever talked to you about using the Gale-Shapley algorithm for this purpose?

Gale: No, but you know that there are hundreds and hundreds of papers and at least three books on the subject of matching. The subject has a lot of life in it. There are so many special circumstances that create variations on the basic matching problem. My most recent paper is on one. The main guy that does this now is Alvin Roth in the Harvard Business School. Even though the AMS does not use this for matching departments with candidates looking for jobs, Roth may know of other societies that do use this.

Certainly the matching of medical students to teaching hospitals is still going on. There was recently a lawsuit in which medical students objected to the algorithm because it tilts toward the hospitals. So then they switched the students from receivers to proposers to give them the advantage. However, some were arguing that the matching should be done by negotiation because that was the best way to treat students appropriately. Roth was right in the middle of this issue.

Meyer: Well maybe I should suggest it to someone at AMS—I'll ask them to call you.

Gale: [Chuckle] All right.

Meyer: Did you and Shapley know each other at Princeton? Your years as graduate students there overlapped.

Gale: Yes, I knew him at Princeton, but we weren't friends in any particular way. He came the year after I did. He was a very smart guy in game theory. He spent a long time at RAND after getting his PhD. I got to know him better in '57–58 when I spent the year out there. He eventually took an academic job at UCLA. He is probably the best game theorist of all time. Definitely should have won the Nobel Prize. Nash certainly deserved the Nobel, but the other two guys he shared it with weren't nearly in the class of Shapley.

Meyer: I suppose there is a lot of politics involved in the Nobel prizes.

Gale: I think so. Because it is a prize in economics, they may try to avoid giving it to mathematicians. Perhaps that is why Dantzig never got one either.

Mathematics and Operations Research at Berkeley

Meyer: So when you came to Berkeley from Brown, how did your intellectual environment change?

Gale: I was chairman at Brown before I came here in 1966. I started that in 1961 or so and was chairman for 4 or 5 years. It is very different at Berkeley. Some things were nicer in Providence. We all knew each other and visited one another a lot. We walked to school. Here you feel the hugeness of the place. You can't walk anywhere—you have to drive.

I started out dividing my time equally between math and operations research. The OR department at Berkeley was a fairly closely knit social group with just eight to ten people. We'd go out to lunch together and it was pretty nice. But over time, the department got more interested in very practical applications. There were some theoretical guys I was hoping the department would hire, but they didn't want to. This is the same kind of difference of opinion as you have in mathematics departments—between pure and applied. Anyhow, in the late 70's, I switched over full time to the mathematics department.

The department had 65 tenured faculty scattered all over. But there were these marvelous people here—wonderful mathematicians.

Meyer: What courses at the undergraduate level did you pay particular attention to?

Gale: When I came here to Berkeley I decided that I would teach all of the upper division courses, to educate myself. I'm most comfortable teaching modern algebra, number theory, and differential geometry. I like to keep educating myself. You could do this at Brown too, but they didn't have the same variety of courses. There was no discrete math at Brown when I was there, but I taught it here at Berkeley several times. For another example, here they have undergraduate topology. But I taught statistics one year at Brown.

Meyer: In the last few decades—since Sputnik—there have been a lot of new ideas about undergraduate teaching. NSF has poured money into undergraduate teaching experiments. Did this have any influence at Brown, or did Brown follow its own internal evolution?

Gale: I left Brown at 1965, and up to that point, I didn't notice any effect of NSF money. Some time later, I did see an effect—for example the push for reforming calculus. NSF has been a great source for getting money for grants, but I don't know how effective it has been in terms of getting students to learn math better. I've never been on the inside on that, although I wrote an opinion piece in the *Notices of the AMS* on Calculus, its Past, Present, and Future.

Applied Mathematics Today

Meyer: Do you think the relations between pure and applied mathematics have changed much during your career?

Gale: I would not say so. Well, some things are different. Computer science, which you would have to say is applied, has made a difference. For example, the study of complexity is very pure, coming close to foundations, logic, and that sort of thing. So the waters got muddied a bit.

When you get new branches of the mathematical sciences coming up, varied and proliferating ideas, then the lines between pure and applied may blur. Information theory, which came along at the same time as linear programming, had such an effect.

Meyer: What is the state of game theory today? If a young person wanted to specialize in game theory, where would they go? Princeton? Cornell?

Gale: Not to Princeton, nor Cornell either. They might go to Hebrew University, which has had work in this area pretty much since the beginning of the subject. I don't know how it got started there, whether it was one particular person. Robert Aumann is the central figure I know of there.

For a while, Princeton was the only university in the United States that was doing game theory. When Tucker became chair, he didn't take students any longer, so game theory activity stopped there. I don't think there is much going on in mathematics departments in the U.S. There is some work in economics departments and business schools.

Recreational Mathematics

Meyer: In your varied career, you have been a pure mathematician,[3] but you took an interest in applied things, in economics and operations research. You also had an interest in recreational mathematics. Would you have predicted this about yourself when you were a graduate student—predicted the diversity of your activities?

Gale: I guess I would not have predicted it. But if someone had suggested some of these activities to me I would have said, "Well that would appeal to me." We all read Martin Gardner—he was a model, right? I got into recreational mathematics in 1991 because the editor of the *Mathematical Intelligencer*, Sheldon Axler, wanted me to take over the problem section of the magazine. I said I didn't want to do that, but I said I would be willing to do a column similar to Gardner's columns. So they let me do it, and I did it for six years. Some of those columns have been collected in my 1998 Springer book *The Automatic Ant.*

Meyer: One of the things I notice about those columns is that you had a tendency to make comments about the nature of mathematics and the nature of science. And those allusions to poetry. Evidently this is an expression of the nature of your mind.

Gale: Yes, I did like philosophizing. Everyone loves to express their opinions, and they allowed me to do it, so why not! I think my last column was called "What is Mathematics, After All, " or something like that. I gave examples of reasoning that did not involve symbols or actual proofs, but reached sound conclusions anyhow. From examples like those, I came to the conclusion that you shouldn't ask for a rigorous answer to what mathematics is. It is so hard to answer that question, and it wouldn't be very helpful if you did give an answer.

Careers and Advice

Meyer: Do you have any advice for a young person starting out in mathematics today?

Gale: If you are interested in something, try to get into the best school you can and gobble it up.

Meyer: Did you think about any other possibilities besides mathematics?

Gale: During the Second World War, I switched my major to physics because of the need for people to do war-related work. When I got my Bachelor's degree, I went to work at the Radiation Laboratory at MIT. But before long, I came back to my first love, mathematics. I didn't weigh the issue of jobs much in my choice of what to study. But I knew there were people who made a living as math professors and I thought that looked like a good life. And it has been.

Meyer: This might be a good place to end our interview. Thank you very much, Professor Gale.

Note. This interview took place in April, 2004.

[3] Including for example, work on higher dimensional convex polytopes such as "Neighborly and Cyclic Polytopes" in *Convexity, Proceedings of the Symposia in Pure Mathematics*, Vol. 7, American Mathematical Society, 1963.