An interview with

MICHAEL J. D. POWELL

Conducted by Philip Davis
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ABSTRACT

Michael Powell discusses his career and research. Powell was born in London and lived in Sussex and Surrey. He had a governess in mathematics when he was very young, and because he enjoyed looking at mathematics books, often just doing the exercises, he was ahead of his classes in mathematics. He became an undergraduate at Cambridge, finishing in three years, two years for part 2 of the Mathematical Tripos and then taking a diploma in numerical analysis and computing in his third year. He joined the Atomic Energy Research Establishment at Harwell, and stayed for seventeen years. At Harwell, he started the Harwell Subroutine Library, one of the first libraries of numerical algorithms, and began his research career. He discusses the origin of the DFP [Davidon-Fletcher-Powell] method and subsequent methods that overtook it. After leaving Harwell, he returned to Cambridge in 1976 as a Professor and continued his research career. He received a Doctor of Science degree in 1979 at Cambridge. He discusses his subsequent work in optimization and approximation, the differences between research at Harwell and Cambridge, and his preferences in conducting research, including his tendency to publish by himself. Powell retired from Cambridge in 1996.

DAVIS: This is an interview with Professor Michael J. D. Powell of the Department of Applied Mathematics and Theoretical Physics in Cambridge. The date is Wednesday, April 6, 2005. The interviewer is Phil Davis and the interview is taking place in Michael Powell's office in the new Mathematics building which is about two years old. Mike, you've been retired for a number of years haven't you?

POWELL: Yes, that's correct. By the way, the full name of the building is the Centre for Mathematical Sciences.

DAVIS: Excellent.

POWELL I have been retired for three and a half years now.

DAVIS Have you been enjoying the retirement?

POWELL: Definitely, the main reason for retiring is to have more time for research.

DAVIS: Do you do any teaching at this point?

POWELL: Definitely not. I gave it up, that was the reason for retiring.

DAVIS: Oh well, you mean it wasn't mandatory in your case

POWELL: No, I retired two years before I had to.

DAVIS: I see. But you do occasional lecturing, invited lectures, things of that sort?

POWELL: I like to give talks at conferences, and recently I visited a technical university in Denmark and gave five lectures there. But Cambridge lecturing is behind me, apart from seminars.

DAVIS: Yes. Well I think we've known each other for perhaps forty years or so –

POWELL: I think so.

DAVIS: Something like that. But I think last time we were together was when you had a birthday party as a matter of fact.

POWELL: There was a conference here to celebrate my sixtieth birthday –

DAVIS: You had not retired at that time.

POWELL: That's correct –

DAVIS: You went on for a few more years.

POWELL: Yes, that was in 1996, I was 65 when I retired.

DAVIS: Let's do a little personal information. Where did you grow up, Mike?

POWELL: I was born in London, and lived in the Home Counties. I've lived in Sussex and Surrey, and have gradually been moving northwards.

DAVIS: And what sort of elementary education did you have?

POWELL: I had a governess in mathematics when I was very young. She was good. I think a lot of my mathematics was self taught because I enjoyed looking at mathematical books, and in fact was quite a long way ahead of most of the classes in school.

DAVIS: This is when you were a kid?

POWELL: Yes.

DAVIS: Do you remember any specific book that you, that impressed you in those years?

POWELL: I can't remember the name of the book; I found books with lots of exercises in them. I hardly ever looked at the text, I merely did the exercises, and I can remember learning trigonometry when I had chicken pox.

DAVIS: Well that's a good way to spend that particular time. This isn't chicken pox experience, but I remember about twenty years ago we had a tremendous blizzard in Providence; it was so high that it covered all the automobiles, and we were blocked in for more than a week before we could get out. I said well what am I going to do, I might as well write a book, you see, and what I did was to sketch out the book that I called "Circular Matrices", and that was the result of a blizzard. Chicken pox in your case resulted in learning trigonometry. As a kid, did you get into any of the general books like (Walter William) Rouse Ball's *Mathematical Recreations and Essays*?

POWELL: Yes, I have a copy of that and I enjoy reading that and studying that.

DAVIS: I also found that very stimulating somehow as a kid, Rouse Ball.

POWELL: I should also mention that when I was at school at Eastbourne, one of my mathematics teachers was Paul Hirst, who was very good. And when I returned to Cambridge, he was professor of education here, which delighted me.

DAVIS: And as an undergraduate, where did you do your undergraduate work?

POWELL: That was at Cambridge too. I was at Peterhouse then.

DAVIS: I see, and then your higher degree was also at Cambridge?

POWELL: I have a Cambridge higher degree, but originally I was at Cambridge for only three years as an undergraduate, I did Part II of the Mathematical Tripos course in two years, and then in the third year I took the diploma in Numerical Analysis and Computing. And I did not get a higher degree at all then –

DAVIS: Oh, you were without a higher degree –

POWELL: That's not strictly true because after I returned to Cambridge I submitted work for a Doctor of Science degree and was awarded that in 1979.

DAVIS: Did you have an advisor for your thesis or for this particular work that you submitted?

POWELL: No, the procedure for getting a ScD degree was to submit published work, so I simply put a collection of papers into an envelope. My examiners were Jim Wilkinson and Leslie Fox, and I'm quite proud of that.

DAVIS: I see. On the faculty at Cambridge when you were listening to lectures and so on, were there any particular ones that impressed you, particular lecturers?

POWELL: One I remember quite well. I was taught linear algebra by Derek Taunt. This was I think my first lecture course, and one thing I remember about it is that the relation to matrices was not explained until half way through the course. I definitely enjoyed the theoretical courses was more than the applied ones. I was sent to some applied mathematics lectures by George Bachelor, and remember giving up when he taught the theory of the paint brushes. It didn't make sense to me, the way in which it was presented.

DAVIS: Coming back to Taunt and linear algebra, I never liked linear algebra and such, I always thought it was matrix theory, but matrix theory, as I recall it, for a while became unpopular, as such, because people wanted a more general approach that would admit you to Hilbert spaces and Banach spaces and that sort of thing. But I'm galled that the computer has put matrix theory back in business.

POWELL: In fact Hilbert spaces were not taught at all while I was an undergraduate.

DAVIS: Really?

POWELL: I had to pick them up later.

DAVIS: I see, I see. So actually you were interested in mathematics as a kid?

POWELL: Quite definitely. I was quite successful in school in mathematics.

DAVIS: You were good at it, and you stuck with it, and so on.

POWELL: Yes. I didn't enjoy the mathematics very much in the first two years at Cambridge because it covered such a wide range. I much prefer to specialize in mathematics and think in detail about particular parts of mathematics rather than to try to study the subject in a general way.

DAVIS: Did you get into numerical methods rather early?

POWELL: Yes, that was the third year course. Having taken Part 2 in two years, I had to do something in the third year, and the choice was between Part 3 mathematics and numerical analysis with computation. Or I could have read mathematical statistics.

DAVIS: When you took a selection of your papers and shoved them into an envelope, in order to get a higher degree, was it the ScD?

POWELL: Yes, the Doctor of Science.

DAVIS: Do you recall the topics that you shoved into the envelope?

POWELL: Yes. At that time I had worked a lot on optimization and on approximation. I decided that putting all of them in the envelope would be too many, so I didn't include any papers on optimization.

DAVIS: So optimization came fairly early also in your scientific career?

POWELL: After my first degree, I worked at Harwell [Atomic Energy Research Establishment, Harwell, Oxfordshire]. I did calculations with scientists there, writing Autocode and FORTRAN programs, things like that. I tried to improve on current techniques, which was often possible because those techniques were very primitive.

DAVIS: How long were you at Harwell?

POWELL: For seventeen years, 1959 to ’76.

DAVIS: That was a long hitch.

POWELL: Yes.

DAVIS: Who were some of your associates at Harwell?

POWELL: The person who interviewed me and whom I know particularly well is Alan Curtis –

DAVIS: Alan Curtis?

POWELL: Alan Curtis at Harwell, not the National Physical Laboratory one. And Alan and I published some papers together. Then while I was at Harwell, John Reid joined us and Roger Fletcher was there for a time. Iain Duff came before I left. So some very good people went through there.

DAVIS: Harwell is a, recall it to me, Harwell is a military establishment?

POWELL: No, Harwell was part of the Atomic Energy Authority.

DAVIS: The AEC?

POWELL: Harwell concentrated on peaceful applications of atomic energy. The Atomic Weapons Establishment at Aldermaston wasn’t very far away. At Harwell, I began by working closely with physicists. Then one thing I did which I’m very proud of is that I started the Harwell Subroutine Library [now called HSL]. That I think is one of the first libraries of collections of numerical algorithms.

DAVIS: Does that still exist in some form or fashion?

POWELL: Yes, it’s been overtaken, I think by NAG, and IMSL became stronger as well, and of course people tend to use MATLAB now rather than using subroutines –

DAVIS: But in your Harwell days what sort of machine or programming was available?

POWELL: I first programmed at Cambridge on EDSAC, and there was a Ferranti Mercury machine when I arrived at Harwell in 1959.

DAVIS: This is still a huge thing.

POWELL: Mercury had I think about two thousand words of fast memory and ten thousand words on drum. Then we also had access to an IBM 7030 at Aldermaston.

DAVIS: Coming back to the envelope, you said that Wilkinson was one of the examiners?

POWELL: Yes –

DAVIS: And someone else you mentioned.

POWELL: Leslie Fox –

DAVIS: Leslie Fox, from Oxford. And Wilkinson at that time was at the NPL?

POWELL: Yes –

DAVIS: Did you get to know Leslie and Jim at all?

POWELL: I knew Jim Wilkinson very well, not so much for socializing but certainly as far as work goes, because we met on many occasions and had discussions. I knew Leslie Fox much better on the social side because we were both keen golfers.

DAVIS: I see, so these were the days where Wilkinson was working on, what do you call it, backward error analysis for linear problems?

POWELL: He was working on everything connected with matrix calculations for linear algebra.

DAVIS: Yeah. Let's get to your work on optimization. You've worked on that for many years now have you not?

POWELL: Yes, I first published a paper in 1962 on optimization.

DAVIS: And over the years you've published many papers on this?

POWELL: Yes.

DAVIS: I think that some of the methods that you have worked out, and are known by your name in fact, have become fairly standard. Isn't that the case?

POWELL: Yes, yes, I've been quite fortunate. The first one that's well known is the DFP [Davidon-Fletcher-Powell] method, but that was overtaken by the so-called BFGS [Broyden-Fletcher-Goldfarb-Shanno] method. They're very similar, but the BFGS method does seem to be more efficient. And the reason for it has never really been explained properly.

DAVIS: These came out of the 70s is that so, or later?

POWELL: No, the DFP method was first published by Bill Davidon in 1959[1], and the F and P part, which is Roger Fletcher and myself, was just finding Bill Davidon's report and hardly changing it at all, running more examples, writing it up differently, you could almost describe it as plagiarism, except that the original report may never have seen the light of day. Bill Davidon says he's grateful to us.

DAVIS: Where was he at the time?

POWELL: The report was written at Argonne National Laboratory, but he was a physicist at Haverford College in Pennsylvania.

DAVIS: I used to give courses in numerical methods when I was teaching, and one year I taught optimization and I in fact covered these methods. They have become very standard now. Has that

[1] W.C. Davidon, Variable metric method for minimization, ANL-5990, Atomic Energy Commission Research Development Report, 1959.

been put in, has it been “chippified” you might say, placed in MATLAB or some other package where you just call it in routinely?

POWELL: I don’t think so because one has to make some specifications to use it. The main feature of the method is a way of improving approximations to second derivatives using only first derivative information. If you put that into an actual algorithm you have to worry also about things like line searches. How accurately the line searches are done is at the discretion of the user.

DAVIS: You need the second derivatives for the Jacobian?

POWELL: This is the second derivative matrix for unconstrained optimization so it is symmetric, and the extraordinary thing about the method is that it doesn’t generate a very accurate second derivative matrix, but it does pick up the information that it needs automatically in order to achieve efficiency in optimization calculations.

DAVIS: So what was your strategy from going to the first derivative to the second derivative?

POWELL: The strategy is that the use of some kind of second derivative information is probably a big improvement on steepest descent. You can regard steepest descent as approximating the second derivative matrix by the unit matrix and it’s trying to make headway from that. When Bill Davidon produced his report he did prove that after n iterations you will find the exact minimum of a quadratic function. That’s the big advance over steepest descent, but what happens –

DAVIS: Finite number of steps –

POWELL: It’s a finite number of steps for quadratic functions, but one is applying this method to such large values of n that you never want to get close to doing n iterations. You usually get good convergence in far fewer steps, and that’s why the second derivative matrix doesn’t become good. You need n steps to get accurate second derivative information.

DAVIS: What is the current state of optimization? Is there any way of summing up the current state of optimization?

POWELL: The simple answer is no, but before we leave the earlier work, let me mention a method, simply being called Powell's method, for minimization without derivatives which is based on properties of conjugate directions of quadratic functions. I published that in 1964 and it has also become very well known. And the method I published in 1965 for least-squares calculations without derivatives is also used often in practice.

DAVIS: One of the things that I remember telling students was that steepest descent could get caught in a very false minimum. How does one overcome this now?

POWELL: I think all of the methods I mentioned could be trapped by local minima. This is another field where there are no very satisfactory answers. The usual advice is to suggest trying a range of starting points and see what happens.

DAVIS: So this is sort of a Monte-Carlo feature to the thing, in events that you start at one place and you get the minimum when you start at somewhere else, and hope the minimum is less.

POWELL: I hadn't investigated such techniques; I just give the standard advice to try other points if you're uncertain of the solution that is being found. Very often in practice, when a method runs slowly users attribute it to local minima and I think quite often they do so wrongly. Instead it is possible that the method they're using simply isn't suitable for the objective function.

DAVIS: Are there any cases in optimization where you do a pure random search, and then pick up the minimum?

POWELL: There's a horrible method called simulated annealing, which the engineers use quite a lot.

DAVIS: How does that operate?

POWELL: It's more or less what you said, you take random steps and if at the end of the random step you find the objective function becomes better of course you accept the step, but also with a small probability you will accept the step if the objective function gets worse. The idea is that because you're doing this you can then perhaps get away from the local minima, and because the probability of accepting a step which is worse is less than one, and the better step is one, in that way you to tend to converge the minimum.

DAVIS: Now that you're retired are you still working on optimization?

POWELL: I work with optimization and approximation, most of my research, until last December, was on a particular technique for minimization without derivatives, and since then I've been looking at some approximation questions.

DAVIS: What sort of question in approximation theory has come up that you're investigating?

POWELL: I've put a great deal of work into use of radial basis functions for multi-variable approximation and my students have worked in this field as well. At present, I'm just looking at a small corner of that. If you use equally spaced function areas on a finite grid, I'm looking at and trying to understand the deterioration of accuracy that occurs at the boundary. You can find out answers easily by numerical calculations, and I'm trying to explain some of these theoretically, and this is only in very simple cases.

DAVIS: Speaking of radial bases, one of my colleagues back at Brown, a young man by the name of Jan Hesthaven, came into my office the other day and asked me for some references on that. So I said that the only thing I could think of at the moment was the work of your student Martin D. Buhmann. He wrote a book on radial basis functions, did he not?

POWELL: That is correct. Another reference that's cited quite a lot is some lectures that I gave at the summer school at Lancaster in about 1991.

DAVIS: Are they available?

POWELL: Yes, they're published by Oxford University Press.

DAVIS: Can you spell that out a little bit, the title and so on.

POWELL: I'll have to go and fetch –

DAVIS: No, no. Okay, there are lectures –

POWELL: They're in a book edited by Will Light published by Oxford University Press in about 1992[2].

DAVIS: You have throughout your career written of course very many papers and a number of books and things of that sort. Some of these are collaborative efforts?

POWELL: Relatively few of my papers are written with other authors, I would think fewer than fifty percent probably. You mentioned Martin Buhmann, I published with him. I have also published quite a lot with Rick Beatson on radial basis function techniques –

DAVIS: How do you find collaboration? Do you like it, do you enjoy it, do you find it stimulating or do you find it irritating in some way?

POWELL: I collaborate far less than other people. It is very helpful sometimes to be able to talk things over with somebody else, but my main interest in mathematics is the problems themselves and in a way I'd quite like to crack them myself.

DAVIS: I see, so would it be appropriate to call you loner?

POWELL: Yes. I've had about 18 research students, and my tendency is not to put my name on papers that they publish when they finish their dissertations.

DAVIS: I think that's proper.

POWELL: I have published with about a third of my research students. Of course I encouraged them all to write things up and I tried to be helpful.

DAVIS: Have you collaborated in papers or books with Arieh Iserles?

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[2] M. J. D. Powell, The theory of radial basis function approximation in 1990. In W.A. Light, editor, *Advances in Numerical Analysis, Volume 2: Wavelets, Subdivision Algorithms and Radial Basis Functions*, pages 105–210, Oxford University Press, 1992

POWELL: I've only written one paper with Arieh, that was one on differential equations. I made a suggestion that he found very helpful and he insisted that I be a co-author. He certainly did the bulk of the work.

DAVIS: Well, in your role as loner then you would say that your interaction with other members of the department here is rather less?

POWELL: Yes, it's quite small; retirement suits me very well because it's easier to be a loner. I just enjoy mathematics intensely. I think if you do research in mathematics you have a responsibility to write papers on your discoveries, but its mathematics itself that I prefer.

DAVIS: So this being a loner, would extend to the people that happen to be in the Newton Institute just across here?

POWELL: At the Newton Institute most of the people there are visitors who come for up to six months.

DAVIS: So that's the way it works?

POWELL: Yes. There have been some people I've known who visited there but I haven't in fact collaborated with them at the time. Occasionally I've had visitors who've come maybe for two or three years to Cambridge with the intention of working with me, but often I find this rather painful.

DAVIS: You don't encourage it, or you encourage it? Encourage their coming?

POWELL: In such cases, I've tried to find a suitable project for them to do if they ask for one, but I certainly don't want to talk to people and say how are you getting on more often than maybe once a month, something like that. My preference is to find a nice piece of mathematics or an idea for a new algorithm and to get stuck in it, and I like my visitors to do that too.

DAVIS: Some years ago when I spent a few days here at Cambridge I did some taping with you which I never used because you were furious at the support given by the Government to various mathematical projects, this came out very clearly in the interview, and I thought that at the time that this might be embarrassing to you, you were not retired, at that time so I never did anything with the tape. Do you remember that interview?

POWELL: I don't remember the interview –

DAVIS: You've been on national committees and so on?

POWELL: Not very much.

DAVIS: You avoid committee work?

POWELL: I prefer to avoid it, yes.

DAVIS: But, presumably, now your influence is felt in higher circles?

POWELL: I think I've helped my research students quite a lot, and so there has been some second hand influence.

DAVIS: But not in terms of policy?

POWELL: I think I've been rather unsuccessful. When I came to Cambridge as a professor in 1976, I had an obligation to try to get numerical analysis well established, but my efforts to appoint a lecturer were futile for more than ten years. So numerical analysis has always been done by a very small number of people at Cambridge compared with other subjects in mathematics. If I had been good at getting large groups formed, exerting influence, then numerical analysis would have been much stronger in terms of staff.

DAVIS: How large is it currently?

POWELL: There are myself and Arieh Iserles who filled the lectureship that was approved eventually. He has now become a professor, and when I retired another lectureship was created. So there is one retired professor, one professor, one lecturer, and we also have two very good people who have offices here but work elsewhere. They come in quite often, and otherwise there are occasional visitors on sabbatical and research students.

DAVIS: So you were satisfied with the size and the strength of the group?

POWELL: I feel under an obligation to form a group of what’s been typically a reasonable size. As far as my personal work goes, as I said, I don’t really want lots of people about; I want the opportunity to do research.

DAVIS: Well this is very important too, as a psychological feature because different people work in different ways, and of course you must know this. How do you get ideas?

POWELL: As far as proving theorems goes, and a lot of them come from numerical calculations, all the work I’m doing at present, trying to research all this convergence, is absolutely crystal clear from numerical results. The difficulty of finding theoretical proofs is frustrating. Mainly, I get ideas just by thinking about questions, and my work is heavily slanted towards developing algorithms. If I try an algorithm and it doesn’t behave in the way I expect then there’s a basis of an idea, and I try to explain it.

DAVIS: If you observe some phenomenon by computation that you are subsequently at a loss to prove theoretically, would you go into print with your hunch as to what’s going on?

POWELL: Definitely, and there are heaps of examples in which I have proposed algorithms without having good theoretical justification for them. The most successful algorithms have been like this, such as the DFP method and the conjugate direction method without derivatives. I don’t think people who develop algorithms should wait to provide theoretical support. If they get good results in practice, that’s the important thing.

DAVIS: Well this is very important, philosophically as a matter of fact, because it hits the question of what is proof, what is the function of proof, when is it necessary, when it is not necessary, when is it useful, and so on. Recently, there was a conference on this topic, called by the Royal Society, which you may be aware of. They had a number of speakers, twenty or so speakers, and they’re about to publish something. But I found that the thrust of what was going on was rather theoretical and not modulated in any way by real world experience. Still, you know, still chewing on Russell and Whitehead, that sort of thing.

POWELL: I haven't really looked at the theory of proof at all. Let me recall that I started my career at Harwell where I had to help scientists with their calculations, and so the important thing was to provide good computer software that gave results. And it wasn't in the interest of Harwell very much for me to try to establish theoretically that these subroutines behave as they should. So, I don't delay publication while waiting for proof. I am a sufficiently good mathematician I think to know whether or not the convergence of an algorithm's been proved. I'm very happy to go ahead and say that theoretical questions are still open. I've got a very simple notion of the theory of proof, and I certainly don't indulge in deep philosophical questions about it.

DAVIS: To change the subject just a little bit. You spent many years at Harwell, and now many years in the university environment. How would you contrast the two?

POWELL: Unfortunately Harwell has changed. The main contrast that I've experienced is there are far better opportunities for research at Harwell, and far better support from the administration. I had to give an after dinner speech a few years after I left Harwell, and I contrasted then Harwell with Cambridge. At Harwell, I said I was doing research and people said they understood this; it was appreciated. After I moved to Cambridge, I said I'm with Cambridge University, I'm there as a professor, and people said well what do you do during the vacation? The idea that university people take research seriously seems to be lost on the general populace. This is one difference I noticed, people took research at Harwell far more seriously than people take research at universities, and I'm talking about people in general rather than knowledgeable people.

DAVIS: I suppose that people in general think the point of a university professor is to teach, give lectures and so on –

POWELL: Exactly –

DAVIS: And research doesn't cross their minds.

POWELL: I think that is a point of view, but I moved to Cambridge deliberately because I wanted to do research in a different environment, and the original government support for research at Harwell was becoming less.

DAVIS: But despite this disparity, you've managed very well apparently in Cambridge.

POWELL: The frequency of papers that I've written at Cambridge is about half the number at Harwell, although just talking about numbers of papers, of course, doesn't mean very much. The other strong contrast between Harwell and Cambridge is that at Harwell the first priority of the administration is to help the scientific staff to do their work, but I would never say that about the administration at universities.

DAVIS: How might they help more?

POWELL: I think by taking the point of view that the reason they're employed as administrators is because of the teaching and research of academics. I think that administrators don't try their hardest to keep those activities functioning as well as possible, but there is too much red tape, which diverts their efforts in other directions.

DAVIS: You were mentioning your association with J.C.P. Miller many years ago; could you say a word about that?

POWELL: He helped me more than anyone else when, as an undergraduate at Cambridge, I did the diploma in numerical analysis and automatic computation. His view of the subject was inspiring, and he gave very careful attention to the dissertation that I wrote on automatic quadrature.

DAVIS: J.C.P. Miller was of an older generation. You would not call him a man of the electronic digital computing generation because his experiences were prior to that.

POWELL: Yes. In fact I could tell you a story about it. He used to use a Brunsviga computer, and I heard it going before I went into his office. He said I should know what calculation he was doing. I said you were dividing by three, multiplying by .3333, but he replied that he was working out two thirds, and that I should have heard the bell.

DAVIS: I don't recall that I ever met Miller. It's possible that I did; he might have come to the Bureau of Standards in Washington, where I was working at that time. But he and two other collaborators put out a Handbook of Tables, a table of tables, mathematical functions that were then available. In the early days of the digital computers, it was rather a Bible.

POWELL: Yes, I remember him talking about this. I think he also spoke about the accuracy of those tables because of course occasionally the last digit is pretty close to an adjacent digit if the number ends in .50001 or something like that. I guess I remember him discussing that because he was very proud of his tables.

DAVIS: Yes. What sort of a chap was he, personality wise?

POWELL: He was quiet; he worked very hard. He was here for a long time without being a fellow of a college, but then he was elected at Wolfson. He was always very enthusiastic about numerical analysis, including hand computation.

DAVIS: Do you know anything of his training as a mathematician? His teachers and that sort of thing, I'm going back a bit now.

POWELL: I don't, no I don't. I don't even know if he had a Ph.D. or not –

DAVIS: Probably not, yeah, probably not. Well, let's get off onto something else. As far as your personal work is concerned what are the important questions of the future that you are proposing to look into?

POWELL: I think perhaps I have too many questions, which are outstanding from the past –

DAVIS: Well could you mention perhaps one or two of them.

POWELL: One thing, I mentioned simulated annealing, the technique that engineers use too often for optimization calculations. One thing I would like to do very much is to make better techniques easily available. I think that for a lot of professional people in optimization their work hasn't been very successful from the point of view of applications because actually using the techniques is not easy. Of course, it was very bad news indeed from the point of view of getting sophisticated techniques accepted that computers have become so fast that one can get away with primitive methods. I regret very much the success of primitive methods, and I would like to provide more powerful methods that are just as easy to use. This is why I thought a lot recently about optimization without derivatives. There are just a huge number of questions I'd like to look at, but I do have to be selective of course.

DAVIS: When you are confronted with a minimization problem that comes from another field, perhaps technology or physics or chemistry or something like that, they are often of high dimension. Is not that the case? What you're looking for is a minimum of the function of many variables.

POWELL: Yes, the dimensions that are of interest vary enormously. There are difficult problems with fewer than ten variables, and relatively easy problems sometimes with millions of variables, just huge differences in scale. The techniques I've developed tend to be for smaller numbers of variables and I think my software is hardly ever applied for more than about two or three hundred variables.

DAVIS: When this software gets into commercial packages, such a MATLAB and Maple or whatever, do they say now use this for many variables, use this for fewer variables, or is there an automatic transfer by the algorithm itself?

POWELL: I don't know about MATLAB and Maple; there's a project call NEOS that offers –

DAVIS: N-E-O-S?

POWELL: N-E-O-S, yes. This is a valuable facility from Argonne and Northwestern University. There is wide choice of packages available, you submit a problem, and then you'll be given a menu of optimization techniques that may be suitable, and then you pick one.

DAVIS: You pick one. The computer does not automatically pick one itself?

POWELL: I think it might if you ask it to.

DAVIS: Moving away from your specialty, and your field, as it were, to perhaps the whole field in numerical methods, computational methods. What do you see as the important problems of the future?

POWELL: I don’t think about that very much. There are certainly specialized questions I want to look at myself, and I prefer to put my effort into those directions. Also, I have taken a very dim view of important fields, in quotations, because they encourage research effort to polarize towards particular subjects, especially when they attract special funding. I would much prefer researchers to think about mathematics without such tendencies. Some fields are very much neglected I think, optimization without derivatives being one, because most people in optimization assume that first and second derivatives are available.

DAVIS: So, since you focus quite narrowly on the special problems, I mean, you have no feelings about the future course of mathematics in general?

POWELL: I haven’t really considered it. I think if all mathematicians would do what they can do best and try to make valuable contributions then that would help the subject enormously.

DAVIS: The reason I ask this question is that I have been reading articles, which claim stagnation in certain theories. I’ve even heard this claim for physics.

POWELL: Yes, no comment.

DAVIS: Physics has stagnated since the days of quantum theory and so on, whether this is true or not. I have no idea, but the claims have been put forth.

POWELL: I prefer to avoid fields where many other people are working already. If ones area of research is relatively uncrowded, then ones contributions are more likely to be valuable.

DAVIS: I was recently asked this question by a young innocent student, a girl, bright, 13 or 14, and she asked me how you can tell when a problem is solved. Of course she was thinking in terms of mathematics that she reads in grade school and so on. And I said to myself this is a very wonderful question, and I decided I’m going to write a little paper on this topic. How do we know when the problem is solved? How would you answer that question?

POWELL: I think I would hedge and say define the problem. Usually I’m fairly clear whether or not I have proved the theorem, but I wouldn’t like to expose my beliefs on when proofs are complete to people who are experts in logic. When a problem is solved is often a point of view.

DAVIS: Not if you've defined it in some way. You can ask when this particular small area is saturated. Can you move away from it?

POWELL: There are many reasons for changing to another field of research, including interests elsewhere, but I would not expect saturation to be among them.

DAVIS: Never, of course that was my answer to the young girl, but, of course, this is not a very satisfactory answer for a young person. I had to modify it, but most of the mathematicians I've asked this question answered the same way.

We have gotten very philosophical. Perhaps it is a good time to end this interview. Thank you very much for your time.

POWELL: Thank you

[Tape ends]