RICHARD BELLMAN ON THE BIRTH OF
DYNAMIC PROGRAMMING

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What follows concerns events from the summer of 1949, when Richard Bellman first became interested in multistage decision problems, until 1955. Although Bellman died on March 19, 1984, the story will be told in his own words since he left behind an entertaining and informative autobiography, *Eye of the Hurricane* (World Scientific Publishing Company, Singapore, 1984), whose publisher has generously approved extensive excerpting.

During the summer of 1949 Bellman, a tenured associate professor of mathematics at Stanford University with a developing interest in analytic number theory, was consulting for the second summer at the RAND Corporation in Santa Monica. He had received his Ph.D. from Princeton in 1946 at the age of 25, despite various war-related activities during World War II—including being assigned by the Army to the Manhattan Project in Los Alamos. He had already exhibited outstanding ability both in pure mathematics and in solving applied problems arising from the physical world. Assured of a successful conventional academic career, Bellman, during the period under consideration, cast his lot instead with the kind of applied mathematics later to be known as operations research. In those days applied practitioners were regarded as distinctly second-class citizens of the mathematical fraternity. Always one to enjoy controversy, when invited to speak at various university mathematics department seminars, Bellman delighted in justifying his choice of applied over pure mathematics as being motivated by the real world's greater challenges and mathematical demands.

Following are excerpts, taken chronologically from Richard Bellman's autobiography. The page numbers are given after each. The excerpt section titles are mine. These excerpts are far more serious than most of the book, which is full of entertaining anecdotes and outrageous behaviors by an exceptionally *human* being.

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BELLMAN'S INTRODUCTION TO MULTISTAGE DECISION PROCESS PROBLEMS

"I was very eager to go to RAND in the summer of 1949 ... I became friendly with Ed Paxson and asked him what RAND was interested in. He suggested that I work on multistage decision processes. I started following that suggestion" (p. 157).

CHOICE OF THE NAME DYNAMIC PROGRAMMING

"I spent the Fall quarter (of 1950) at RAND. My first task was to find a name for multistage decision processes.

"An interesting question is, 'Where did the name, dynamic programming, come from?' The 1950s were not good years for mathematical research. We had a very interesting gentleman in Washington named Wilson. He was Secretary of Defense, and he actually had a pathological fear and hatred of the word, research. I'm not using the term lightly; I'm using it precisely. His face would suffuse, he would turn red, and he would get violent if people used the term, research, in his presence. You can imagine how he felt, then, about the term, mathematical. The RAND Corporation was employed by the Air Force, and the Air Force had Wilson as its boss, essentially. Hence, I felt I had to do something to shield Wilson and the Air Force from the fact that I was really doing mathematics inside the RAND Corporation. What title, what name, could I choose? In the first place I was interested in planning, in decision making, in thinking. But planning, is not a good word for various reasons. I decided therefore to use the word, 'programming.' I wanted to get across the idea that this was dynamic, this was multistage, this was time-varying—I thought, let's kill two birds with one stone. Let's take a word that has an absolutely precise meaning, namely dynamic, in the classical physical sense. It also has a very interesting property as an adjective, and that is it's impossible to use the word, dynamic, in a pejorative sense. Try thinking of some combination that will possibly give it a pejorative meaning. It's impossible. Thus, I thought dynamic programming was a good name. It was something not even a Congressman could object to. So I used it as an umbrella for my activities" (p. 159).

EARLY ANALYTICAL RESULTS

"The summer of 1951 was old-home-week. Sam Karlin and Hal Shapiro were at RAND.

"Hal Shapiro, Ted Harris, and I worked on an interesting functional equation which arose in learning processes. Although we wrote a long paper on this, we never published our results. Sam Karlin obtained some results concerning this equation which he published in the *Pacific Journal of Mathematics*. I had studied a very interesting functional equation which arose in multi-strike analysis, the gold mining equation. The solution was given most easily not in terms of the unknown function, but in terms of an action or decision. This intrigued me, because I had never seen this phenomenon before.

"Hal Shapiro, Sam Karlin, and I tried very hard to solve the general case. It was like a problem in number theory, the solution seemed close, but outside one's grasp.

"I did manage to handle a continuous version, which showed why the original problem was so difficult.

"Hal Shapiro and I took advantage of proximity to do some work on number theory. We greatly enjoyed working together again.

"Meanwhile, of course, I kept up the investigation of dynamic programming processes" (p. 160).

THE MODERN MATHEMATICAL INTELLECTUAL

"I had to make a major decision. Should I return to Stanford or stay at RAND? I had thought about this question in Princeton, but it was not an easy decision to make since there were strong arguments on each side.

"At Stanford, I had a tenured position, good for another thirty-eight years. The retirement age at Stanford was seventy. I also had a good teaching position, with not too much teaching, and a fine house, which I have described above. But these were not the important considerations. At Stanford I had a chance to do analytic number theory, which I had wanted to do since I was sixteen.

"However, I had to face the fact that I could not do what I wanted to do. Possibly the state of mathematics did not allow this. Certainly, my state of knowledge was not up to it.

"I had spent enough time in Los Angeles to know that I would enjoy living there. I also knew that Los Angeles had many fine houses, although it was not until 1968 that I had one that was better than the one up in Stanford.

"I was intrigued by dynamic programming. It was clear to me that there was a good deal of good analysis there. Furthermore, I could see many applications. It was a clear choice. I could either be a traditional intellectual, or a modern intellectual using the results of my research for the problems of contemporary society. This was a dangerous path. Either I could do too much research and too little application, or too little research and too much application. I had confidence that I could do this delicate activity, pie a la mode" (p. 173).

THE PRINCIPLE OF OPTIMALITY AND ITS ASSOCIATED FUNCTIONAL EQUATIONS

"I decided to investigate three areas: dynamic programming, control theory, and time-lag processes.

"My first task in dynamic programming was to put it on a rigorous basis. I found that I was using the same technique over and over again to derive a functional equation. I decided to call this technique "The principle of optimality." Oliver Gross said one day, 'The principle is not rigorous.' I replied, 'Of course not. It's not even precise.' A good principle should guide the intuition.

"Some of the functional equations could be handled easily using classical techniques. Some required a great deal of work. I wish I then had available the projected metric of Garrett Birkhoff. Many years later, Tom Brown and I wrote a short paper pointing out how useful this was.

"Secondly, I turned to the study of the associated functional equations. I was not enthusiastic about doing this. The equations were highly nonlinear and unlike any others that had appeared in analysis. I was delighted when I found that a simple argument could handle most equations.

"While doing this, I started work on control theory. I had seen problems in economics and operations research and it was clear that some effort was required. I had a good team, Irving Glicksberg and Oliver Gross. Both were talented and ingenious mathematicians.

"The tool we used was the calculus of variations. What we found was that very simple problems required great ingenuity. A small change in the problem caused a great change in the solution.

"Clearly, something was wrong. There was an obvious lack of balance. Reluctantly, I was forced to the conclusion that the calculus of variations was not an effective tool for obtaining a solution" (pp. 174–175).

FORMULATION OF THE MARKOV DECISION PROCESS PROBLEM

"I spent a great deal of time and effort on the functional equations of dynamic programming. I was able to solve some equations and to determine the properties of the function and the policy for others. I developed some new theories, Markovian decision processes, and was able to reinterpret an old theory like the calculus of variations, of which I will speak more about below" (p. 178).

DYNAMIC PROGRAMMING AND OPTIMAL CONTROL THEORY

"A number of mathematical models of dynamic programming type were analyzed using the calculus of variations. The treatment was not routine since we suffered either from the presence of constraints or from an excess of linearity. An interesting fact that emerged from this detailed scrutiny was that the way one utilized resources depended critically upon the level of these resources, and the time remaining in the process. Naturally this was surprising only to someone unversed in economics such as myself. But this was my condition, with the result that the observation of this phenomenon came as quite a shock. Again the intriguing thought: A solution is not merely a set of functions of time,

or a set of numbers, but a rule telling the decisionmaker what to do; a policy.

"The mathematical structure of these perplexing analytic problems was quite open. What was remarkable was the level of analytic intricacy of solution introduced by simple constraints. These constraints were an essential part of the background, introduced by immediate economic, engineering, and military considerations.

"A problem of LaSalle's which caught my attention at the same time was the 'bang-bang' control problem. This was a question of restoring a system to equilibrium as quickly as possible subject to constraints in the restoring force. This was a problem closely related to stability theory.

"As a result of a detailed study of dozens of variational problems of the foregoing type, and filling hundreds of pages with equations and calculations, it became quite clear that there would never be any elegant, uniform way of solving problems of this genre in analytic terms. Each individual problem was an exercise in ingenuity, much like plane geometry. Change one small feature, and the structure of the solution was strongly altered. There was no stability!

"Consequently, if one really wanted to obtain numerical solutions to variational problems in an effective fashion, we needed some other tools. I was reluctant to become over-involved, since all along I had no desire to work seriously in the calculus of variations. A course in the subject in college had given me simultaneously a rather low opinion of its intrinsic interest and a healthy respect for its intricacies. It appeared to be filled with complicated existence and uniqueness theorems with self-imposed restrictions, none pointing in any particular direction. This is pertinent to a comment made by Felix Klein, the great German mathematician, concerning a certain type of mathematician. When this individual discovers that he can jump across a stream, he returns to the other side, ties a chair to his leg, and sees if he can still jump across the stream. Some may enjoy this sport; others, like myself, may feel that it is more fun to see if you can jump across bigger streams, or at least different ones.

"Despite my personal feelings, the challenge remained. How did one obtain the numerical solutions of optimization problems? Were there reliable methods? As pointed out above, I did not wish to grapple with this thorny question, and I had certainly not contemplated the application of dynamic programming to control processes of deterministic types. Originally, I had developed the theory as a tool for stochastic decision processes. However, the thought was finally forced upon me that the desired solution in a control process was a policy: 'Do thus-and-thus if you find yourself in this portion of state space with this amount of time left.' Conversely, once it was realized that the concept of policy was fundamental in control theory, the mathematicization of the basic engineering concept of 'feedback control,' then the emphasis upon a state variable formulation became natural. We see then a very interesting interaction between dynamic programming and control theory. This reinforces the point that when working in the field of

analysis it is exceedingly helpful to have some underlying physical processes clearly in mind.

"What is worth noting about the foregoing development is that I should have seen the application of dynamic programming to control theory several years before. I should have, but I didn't. It is very well to start a lecture by saying, 'Clearly, a control process can be regarded as a multistage decision process in which...,' but it is a bit misleading. Scientific developments can always be made logical and rational with sufficient hindsight. It is amazing, however, how clouded the crystal ball looks beforehand. We all wear such intellectual blinders and make such inexplicable blunders that it is amazing that any progress is made at all.

"All this contributes to the misleading nature of conventional history, whether it be analysis of a scientific discovery or of a political movement. We are always looking at the situation from the wrong side, when events have already been frozen in time. Since we know what happened, it is not too difficult to present convincing arguments to justify a particular course of events. None of these analyses must be taken too seriously, no more than Monday morning quarterbacking.

"I strongly recommend the interesting study of these and related matters by Jacques Hadamard, the great French mathematician, in his book *The Psychology of Invention in the Mathematical Field* (Dover Publications, New York, 1945: paperback)" (pp. 180–182).

A SYSTEMATIC METHODOLOGICAL APPROACH TO MATHEMATICS

"As pointed out above, as of 1954 or so I had stumbled into some important types of problems and had been pushed, willy-nilly, into answering some significant kinds of questions. I could handle deterministic control processes to some extent and stochastic decision process in economics and operations research as well. Where next? At this point, I began to think in a logical fashion, using a systematic methodological approach. The point about the suitable philosophy preparing one for the fortunate accident should be kept in mind.

"There are several ways in which a mathematician can proceed to extend his research efforts, particularly one who is deeply interested in problems arising from the physical world. He can, on one hand, examine the equations he has been working with and modify them in a variety of ways. Or he can ask questions that have not been asked before concerning the nature of the solution of the original equations. This is basically a very difficult way to carry out research. It is very easy to change the form of an equation in a large number of ways. The great majority of the new equations are not meaningful, and, in consequence, lead to both difficult and unimportant problems. Similarly, there are many questions that are difficult to answer, but hardly worth asking. The well-trained mathematician does not measure the value of a problem solely by its intractability. The challenge is there, but even very small boys do not accept all dares.

"The trick that one learns over time, a basic part of mathematical methodology, is to sidestep the equation and focus instead on the structure of the underlying physical process. One learns to submit oneself to a catechism: 'When I set up these equations originally, I made certain assumptions. How realistic were these assumptions? What state variables, and what effects did I ignore?...'

"To obtain, in this fashion, a more interesting and more useful theory of control processes, we observe that the use of the calculus of variations in control theory presupposes, albeit tacitly, that we have cause and effect under control, that we know both the objective and the duration of the control process. As a matter of fact, also implicit is the assumption that one knows what to observe and that the state variables can be measured with arbitrary accuracy.

"In the real world, none of these assumptions are uniformly valid. Often people want to know why mathematics and computers cannot be used to handle the meaningful problems of society, as opposed, let us say, to the moon boondoggle and high energy–high cost physics. The answer lies in the fact that we don't know how to describe the complex systems of society involving people, we don't understand cause and effect, which is to say the consequences of decisions, and we don't even know how to make our objectives reasonably precise. None of the requirements of classical science are met. Gradually, a new methodology for dealing with these 'fuzzy' problems is being developed, but the path is not easy.

"Upon first gazing upon the complexities of the real world, there is a certain temptation to return to number theory. Number theory, however, does not seem to be rewarding enough for continual effort. The problems are too difficult and the victories too few. Taking up the challenge of complexity, I felt that the appropriate thing to do was to start with deterministic control processes and to modify them stage by stage to obtain theories which could be used to deal with basic uncertainties in a more sophisticated fashion.

"To this end, we can begin by introducing well-behaved uncertainty of the type extensively treated by the classical theory of probability. This leads to the modern theory of stochastic control processes where uncertainty is represented by random variables with known probability distributions, and where the objective is to maximize expected values. This gives rise to an elegant theory with a good deal of attractive analysis. It is a new part of pure mathematics.

"The Riccati equation plays an essential role. I had observed this but had published little on it, saving it for Norman for a Ph.D. thesis. Unfortunately, he decided not to go ahead. In the meantime, others, like Rudy Kalman, had observed this and published various results.

"In order to make any progress, it is necessary to think of approximate techniques, and above all, of numerical algorithms. Finally, having devoted a great deal of time and effort, mostly fruitless, to the analysis of many varieties of simple models, I was prepared to face up to the challenge of using dynamic programming as an effective tool for obtaining numerical answers to numerical questions. A considerable part of the motivation in this direction at that time was the continuing development of the digital computer. Before it was freely available, it was not very interesting to conjure up hypothetical algorithms. Once there, it was challenging to utilize this Sorcerer's Apprentice.

"Once again, what became a major endeavor of mine, the computational solution of complex functional equations, was entered into quite diffidently. I had never been interested in numerical analysis up to that point. Like most mathematicians of my generation, I had been brought up to scorn this utilitarian activity. Numerical solution was considered the last resort of an incompetent mathematician. The opposite, of course, is true. Once working in the area, it is very quickly realized that far more ability and sophistication is required to obtain a numerical solution than to establish the usual existence and uniqueness theorems. It is far more difficult to obtain an effective algorithm than one that stops with a demonstration of validity. A final goal of any scientific theory must be the derivation of numbers. Theories stand or fall, ultimately, upon numbers. Thus I became interested in computers, not as electronic toys but rather because of what they signified mathematically and scientifically. This interest led in many unexpected directions, as I will indicate subsequently. This is a significant part of the story of scientific methodology. It is usually, if not always, impossible to predict where a theoretical investigation will end once started. But what one can be certain of is that the investigation of a meaningful scientific area will lead to meaningful mathematics. Inevitably, as soon as one pursues the basic theme of obtaining numerical answers to numerical questions, one will be led to all kinds of interesting and significant problems in pure mathematics" (pp. 182–185).