

## **An Interview with William J. Baumol**

**Alan B. Krueger**

*Alan B. Krueger interviewed William J. Baumol, who is Senior Research Economist and Emeritus Professor of Economics, Princeton University, Princeton, New Jersey, and Professor of Economics, New York University, New York City, New York, at his apartment on the New Jersey shore on August 24, 2000.*

### **Early Years**

KRUEGER: Let me begin by asking you where you were born.

BAUMOL: I was born in New York, in South Bronx, in what later became Fort Apache and in fact spent several years of my life there later.

KRUEGER: Did you go to public schools in New York?

BAUMOL: Yes, I did. All the way through high school, and then went to the College of the City of New York afterwards. So I was in the public school system all the way through. Incidentally, I went to the same high school and college as Colin Powell.

KRUEGER: Oh, is that right?

BAUMOL: And told him that once, to which he responded, “Well, I’ll be damned.”

KRUEGER: When you were at City College from 1938 to 1942, did you study economics?

BAUMOL: Yes. I had been interested in economics actually since high school. I majored in two subjects: economics and art. We had a very mediocre economics department. Many of the other departments, such as philosophy, were excellent. So

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we decided to run parallel classes and act as teachers who take on different parts of the curriculum and lecture to our colleagues. I think we learned more that way than we would have had we had good teachers. I've always suspected that if somehow we could arrange to do that with undergraduates, sort of give them minimal instruction but every incentive and the right opportunities to teach themselves, they would come away knowing much more than they do when they've been through the work with what are called good teachers.

KRUEGER: When you finished City College in 1942, did you first work at the Agriculture Department?

BAUMOL: Yes. I worked with a man named Frederick Waugh, who had first produced the theorem that we know well nowadays that if a commodity changes price frequently, consumers are better off than if it stays at the average of those prices (Waugh, 1944). But I didn't think our work at that point was terribly significant. However, when I got out of the Army [in 1946], I went back to the Department of Agriculture. There I had the most responsible job I have ever had, because our office was in charge of allocation of U.S. grain supplies to all the underfed countries of the world, and all our superiors were busy at meetings all the time. So it turned out that another young man with a bachelor's degree and I were making all the decisions on which country got which shipment of what grain. Which worried us greatly because there were no absolute criteria. We did our best. We were very conscientious, but it was a very funny sort of position in which to find ourselves.

KRUEGER: Allocating food when it was a matter of life and death.

BAUMOL: Exactly right.

KRUEGER: You mentioned that you became interested in economics in high school. When you were in high school, did you take any economics classes?

BAUMOL: They didn't have them at the time. There was a little bit of economics in the social science, history courses. But I did a lot of reading. And a lot of reading particularly of Karl Marx, because both my wife and I grew up in very left wing households. Even though we were both, from very early days, suspicious of things like what Stalin was doing, the fact was that we still believed in things like nondiscrimination, elimination of poverty, etc., etc., as prime social issues. I can claim that we've never deviated from those views.

KRUEGER: When did you decide you wanted to be an economics professor?

BAUMOL: Oh, I think as soon as I started to read some of these books, that became my ambition. So it must have been age 17, something like that.

KRUEGER: And you worked at the Agriculture Department as part of that plan?

BAUMOL: No, that was . . . Remember, we were just coming out of the Depression, and getting any kind of a job was a major triumph. I was considered a little mad by my fellow students because I held out for a job that would pay \$2000 a year—and got it!—while the rest of them sold their services for \$1400 a year or something like that. So we were very happy to get something in the field in which



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I was interested. And to get what then was considered very good pay and very good working conditions.

KRUEGER: When you were in the military, did you use economics at all or was that a break in your work in economics?

BAUMOL: Well, it was a break except that I used the time to study math. I took graduate correspondence courses that were being offered then by the University of Wisconsin, as I remember it. I used to haunt the French bookstores, where I could get mathematics books which in dollar terms were incredibly cheap and I found fascinating reading. So in that sense, it went on, but had nothing to do with my very minor role in the Army.

### **The London School of Economics**

KRUEGER: You went to the London School of Economics in 1947. Someone told me that you were briefly a student at the London School of Economics, but very quickly became a lecturer.

BAUMOL: Well, that's a very funny story; Lionel Robbins used to love to tell it. When I applied to the London School of Economics and they saw that I had attended the College of the City of New York, of which they had never heard, they decided that I probably wasn't qualified. So I was rejected, and I wrote back asking

when I could apply for the following year. And so at a faculty meeting, Lionel Robbins said, “Oh, the poor man wants so badly to come. Let’s let him in for a Master’s.” Then I got there, and Lionel Robbins ran a seminar in which animated discussion was the name of the game. Well, there was no place like the City College for dirty debating as a blood sport. I mean, we were trained as no other group could have been trained. The Oxford Debating Society was composed of amateurs and children by our standards. So just by instinct, whenever I disagreed with something I would wait for the appropriate moment and come in charging with my sword drawn, and they’d never seen anything like this. So quite unjustifiably, given what I knew at the time, within two weeks, I believe it was, I was switched from a Master’s to a Ph.D., and three weeks later I was a member of the faculty. It’s true that they should have admitted me in the first place, but they shouldn’t have promoted me.

KRUEGER: What did you teach when you were there?

BAUMOL: I taught a course in dynamics and a course on the American economy, the one being the price for the other. At that point, I knew very little beyond what I read in the newspapers about the American economy, and had to work very, very hard to prepare those lectures. Whereas the dynamics lectures, which became my first book (Baumol, 1951), was easy stuff and great fun.

KRUEGER: Was that also your dissertation?

BAUMOL: No, my dissertation was on welfare economics, on externalities as the theory of government intervention, a generalization of the notion of externalities. And that keeps being reprinted every once in a while (Baumol, 1952a).

KRUEGER: Lionel Robbins was your main advisor while you were there?

BAUMOL: Yes. We became very good friends. He was a wonderful man. He and Jacob Viner, who were close, close friends, were incredible giants of erudition. They had prodigious memories. I’d be sitting in the faculty dining room at lunch, and Robbins and Harold Lasky were sitting there discussing obscure sixteenth-century writers and their still more obscure seventeenth-century critics. And every once in a while, they’d turn to me and say, “Don’t you agree, my dear William?” And I would smile, trying to look intelligent. And I could tell you stories about Viner . . .

But I ended up with the longest and most delightful Ph.D. oral, I think, in the history of the subject. At that time, Ph.D.’s were things that were given only to crazy Americans. I was a member of the faculty, and we had my Ph.D. oral over whiskies and sodas at the Reform Club. It lasted five hours, and then we all looked at our watches, decided our wives must be getting worried, so we all voted, including me, that the thing was over. Then I got home and my wife, Hilda, said, “Did you pass?” I said, “I have no idea.”

## **Princeton and New York University**

KRUEGER: You moved from LSE to Princeton immediately after you finished your Ph.D. Why did you choose to move to Princeton? What were times like then?

BAUMOL: Oh, I moved there because I had a good offer. It seemed a nice place. I met the people and I enjoyed talking to them. It wasn't a matter of my taking a dozen places and ranking them. I really felt that there were about a half-dozen places where I would have been equally happy to go, and Princeton was one of them.

KRUEGER: When you were at Princeton, you mentioned that Viner was someone who had an impact on you. Were there others on the faculty who had a strong influence on you?

BAUMOL: Yes. There was Friedrich Lutz, Lester Chandler, Dick Lester. But then there were also the mathematicians. It turned out that almost by accident I became friendly with a number of the mathematicians, particularly with people who were working on differential equations—on which I claim no special expertise—but they knew nothing about difference equations, and I was able to show them how you jumped from one to the other very easily. So I had the pleasure of, in effect, going on a short speaking tour with Solomon Lefschetz, the great authority on differential equations, and sort of basking in his sunshine.

KRUEGER: Did you work with any other Princeton mathematicians?

BAUMOL: Yes, with Harold Kuhn, to a small extent with Al Tucker and above all with Ralph Gomory. Professor Tucker introduced him to me as “the young man who had solved the integer programming problem,” and after much enjoyable discussion we went on to write an article about the economic interpretation of his analysis. Then, just a few years ago he called me up, after several decades in which we had lost touch, and he told me about a new idea of his on the theory of international trade. One thing led to another, and having enjoyed the collaborative process once, we decided to go at it together once more. The result was a number of articles and a book just published by MIT Press (Gomory and Baumol, 2000). I'm very excited about the book. I think it offers significant new analytic approaches and important conclusions for policy. It shows that where the traded goods are subject to differentiated rates of productivity increase, from one country to another, or production of the traded goods involves scale economies, then there generally are a vast number of equilibria, many of them far from the optimum. So where this is true, the market does not automatically and consistently point things in the right direction. The analysis also shows that, in the significant situations the analysis covers, what is good for one nation may at the same time be very damaging to its trading partner.

KRUEGER: How about students? Were there particular students with whom you worked closely, undergraduates and graduates?

BAUMOL: Yes. Dick Quandt, Burt Malkiel, Gary Becker, Otto Eckstein. And then there were Bill Bowen and Harold Shapiro. For a while it was rumored that to be president of Princeton, you have to have taken one of my courses and written something with me.

KRUEGER: In 1971 you moved part time to NYU and split your time. I'm curious why you decided to do that and how it worked.

BAUMOL: Part of the reason was I felt I was just getting too settled in

Princeton. I ended up teaching there 42 years, which may not be a record, but I'll bet it's close. And always had superb relations there. I think I never once complained about mistreatment of any sort. So that was fine. But I thought a little variety would be nice. Then our children had left home and we have always been urban types, and the opportunity to spend more time in the city was attractive. But it was also attractive to work with a student body that was more heterogeneous than the Princeton student body. I liked the students at Princeton, but they came more out of a similar mold than the students at NYU, many of whom have accents of various sorts, many colors.

KRUEGER: Who have been some of your primary colleagues at NYU?

BAUMOL: Ned Nadiri, Ed Wolff, Jess Benhabib, others.

KRUEGER: Did you have much contact with Wassily Leontief?

BAUMOL: Yes, a fair amount. And I'm happy to say that I gave a paper about his contribution via input/output analysis, which was one of the last at any conference he ever attended (Baumol, 2000b). In my lecture I had great fun teasing him. I said it was a criticism of his work because he did not take sufficient credit for what he had done.

## **The Cost Disease**

KRUEGER: I want to ask you some questions about specific lines of research. I have to confess, I haven't read all 500 of your papers.

BAUMOL: Good God, no.

KRUEGER: But I did read, and very much enjoyed, a nice summary article that Bobby Willig and Betsy Bailey (1992) wrote.

BAUMOL: Oh, yes. They're very kind to me. But they're prejudiced.

KRUEGER: I wanted to begin by asking you about Baumol's cost disease.

BAUMOL: Well, the model is a very simple one and it has many predecessors, as we were careful to emphasize. Basically what it says is that there are sectors that experience rapid productivity growth and other sectors that experience slow productivity growth. That's no surprise. But what's interesting is that the sectors that experience slow productivity growth tend to persist in that. Where we could trace it back, as Bill Bowen did, when we had data for the beginning of the nineteenth century or even earlier, we found that it was the same sectors that required the continuing labor content—in which labor saving innovation was difficult. Well, it follows that relative costs of those sectors will rise in comparison with those of the sectors with rapidly rising productivity. You can formalize that, but the idea is really trivial. But the implication, which I think has not been yet learned, I think is mind-boggling.

If we look at what is happening to the content of expenditure in the economy, we're heading towards 50 percent of GDP going into health care and education. That isn't bad, because if productivity is going up everywhere, it means we can afford more of the expensive stuff as well as more of the less expensive stuff. But it can

cause severe dislocations, as a result of what Wally Oates (1991) called “fiscal illusion,” where people say, “Well, we can’t afford the terrible price of a college education, even though it’s costing fewer labor hours than it did 50 years ago, but it’s costing twice as many automobiles as it did then.” So the danger is that we will deny ourselves health care, that we will deny ourselves education, that we will deny ourselves a lot of the activities or the products whose costs are determined by the cost disease—this will make an enormous difference, politically. You can imagine garbage removal becoming less and less effective. Crime being less and less effectively dealt with because police activity is affected by the cost disease. You can think of all sorts of horrible things that can happen as a result of the fiscal illusion that comes from the rapidly, cumulatively, and compound rising costs of the arts, health care, education, police protection, garbage removal, and you can go on and on. And you see this all around.

KRUEGER: Many of those activities are carried out through the public sector. If they were private sector activities, one would still have the same tendencies. If education were more of a private sector activity—or health care—do you think they would have faced the same fiscal illusion?

BAUMOL: Oh, absolutely. Just look at what happens to insurance companies, where they are accused of gouging because they are paying for health care, automobile repair, for all sorts of things that you cannot mass-produce and standardize, which therefore are secondary providers of things that are affected by the cost disease. And everybody says we can solve the problem—make it illegal.

I’ll tell you another story. I’d given this lecture at the American Philosophical Society. I was very gratified, all these great minds there. The next day Daniel Patrick Moynihan showed up and people said to him, “You must read this great talk.” So a couple of days later I get a call from Washington. It was Moynihan. He said, “You have now explained to me why the Democratic party is called the party of tax and spend, because we are financing all the things that are affected by the cost disease, and the Republicans want to shortchange them. So,” he said, “next time there is a meeting of the Democratic caucus I will go to them and explain this.” Then he said, “Wait a minute: There’s a meeting of the Democratic caucus this afternoon. I’ll go right over there.” Two hours later the phone rang: It was Pat. He reported, “Yes, I met with the Democratic caucus. I explained it to them and they didn’t understand a word.”

KRUEGER: I’m curious as to how you started to work on this issue. Did it begin with the joint work you did with Bill Bowen on orchestras and the arts?

BAUMOL: That’s right. John D. Rockefeller III and August Hecksher of Twentieth Century Fund had decided that it was time for the United States to do something to encourage the arts. (And in fact that’s how the National Endowment for the Arts started.) So they decided they would have a two-pronged operation. One was a panel composed of good, solid business people who could show that the arts were not a Communist homosexual plot. Then they wanted a serious study. They talked to a number of people, and then someone told them that there was this crazy economist at Princeton who was interested in art. Well, it was the wrong art.

I was interested in painting and sculpture. So they called me in, and I told them how I would go about selecting somebody to study it, and what instructions I would give this person. And then the next day they called me and said, "We'd like to give you those instructions." I said, "I'm terribly busy. I can't do it." And they called again, and I said, "Well, I'll do it on one condition. There's a young assistant professor here, in whose work I have great confidence. If he's willing to do it and you'll pay him . . ." And they agreed and Bill Bowen came and took over the whole thing, as you can imagine. It was such a pleasure working with him (Baumol and Bowen, 1966). So we started to work on it, and he laid out all the things that had to be covered, how one should go about covering them. And then we started to get all these statistics about budgets. Then one night, it was 4:00 in the morning, I suddenly woke up and said I know why those costs are going up! I got up, wrote down a few notes, and went to sleep again. That's literally how it happened.

KRUEGER: We were talking, before the tape went on, about health care reform. Was your involvement in the health care task force, or advice that you provided to Mrs. Clinton, based on the cost disease model?

BAUMOL: Oh, yes. There's a funny history, which is an open secret. When they made their proposal, Moynihan was chairman of the [Senate] Finance Committee. And he was very concerned that the bill as written could never get through, so he started to push for some radical reorganization of the bill. The group under Ira Magaziner became convinced that Moynihan was the enemy. And then someone told them that I was a friend of Moynihan and the way to get to him was through me. Their idea was crazy, but that's what happened. So they invited me to come and work with them. And there were several things in the plan that clearly made no sense. First of all, the administrative cost would have been excessive. Second, the notion of rationing what fields you could teach in graduate schools was self-destructive. But most of all, I said, look at what's happened in Europe and Japan. You have virtually every sort of government control: some on doctor's fees, some on cost per visit. In none of the countries have they been able to get costs to rise more slowly than the rate of inflation. You are promising something which will come back to haunt you within the lifetime of this administration. All I want to do is get you not to make promises that are going to undermine what you are trying to do. I'm on your side. I kept saying to Hillary, "I'll go out and speak for you, I really like you, etc., etc." Well, the end of the story is that grudgingly some modifications were made, but not enough. The funniest thing was that at one point they had agreed to reduce the rate of cost saving they promised and called me up and said we have to have a number by tomorrow morning. I said, "How can I possibly do that? All I can do is give you the average of what has happened in other countries with their various schemes." "Oh, no," they insisted, "it has to be based entirely on American data." So I sat down, cooked up some American data that would give the same answer as the European data, and that's what went in.

KRUEGER: That's politics.

BAUMOL: That's right.

## **The Money Demand Function**

KRUEGER: I wanted to ask you about the Baumol-Tobin money demand model (Baumol, 1952b; Tobin, 1956).

BAUMOL: What the analysis says, basically, is that there are economies of scale in the optimal demand for cash, both on transactions demand and on demand to cover risks. In the case of the latter, it's a matter of the law of large numbers. That is, the larger the volume of transactions against which you're holding cash, the smaller the relative amount of reserve you have to hold. And that goes back to Edgeworth (1888). On transactions costs, the economies of scale arise because if you have a larger volume, you can afford to carry out the transactions less frequently but in larger volume. Essentially, that also says you can save by raising the cash you hold on hand less than proportionately to the volume of transactions. We showed that, in the linear case, it led to a square root relationship. That is, that the optimal amount of cash rises as the square root of the volume of transactions. The theorem itself goes back to inventory theory.

It also turns out that Maurice Allais had come up with it earlier, and Jim and I wrote a little note pointing this out (Baumol and Tobin, 1989).

KRUEGER: Have you thought much about how debit cards and the kind of new financial products that are available, how that alters the situation?

BAUMOL: No, but you've just given me an idea. It's the next thing I'll think about.

## **Contestable Markets, Regulation and Innovation**

KRUEGER: I also wanted to ask about the work you did on contestable markets and industrial organization.

BAUMOL: Well, it actually arose in a very funny way. I was doing some work for the National Science Foundation. Not my own work, but they wanted some advice on financing of journals—whether it was less costly to have independent journals or several journals put out by one publisher, etc. I said, “Oh, that's all very easy. I know all about it. I'll write it up.” I started to write up things and suddenly realized that things I thought were obvious were not obvious and that I didn't know the answers. So this whole issue of multiproduct cost functions, trans-ray convexity, and all these concepts had thereby arisen. Meanwhile, Bobby [Willig] and John Panzar were working on related subjects independently. And Betsy Bailey was working at Bell Labs with Bobby and John, and at NYU with me, and she realized that there was common ground (Baumol, Panzar and Willig, 1982).

KRUEGER: I wanted to ask about the applications of the work. Let me ask you to describe it.

BAUMOL: I think it's about to have its most important effect. Let me talk about that, rather than the past. Because in the past, what it has done is merely offered guidelines saying that where you regulate, what you're trying to do is to get

firms to do what they would if the market really were competitive. But I think where the issue is going to become crucial now is illustrated by the Microsoft case. Incidentally, I have taken no position on the Microsoft case, out of ignorance.

But it is clear that the criteria for market power in industries with heavy and continuing sunk costs, such as those that have to be sunk in R&D, cannot be expected to live up to marginal cost pricing, nonprice discrimination, etc. The standard you have to use is the standard of what they could get away with if entry were totally unimpeded. Entry totally unimpeded, where there are economies of scale, clearly means prices above marginal cost, and no one will enter if the entrant firm has to be prepared to sink \$50 million dollars a year and has no prospect of recouping those \$50 million a year. So entry will occur, and it will drive prices to where the \$50 million can be recouped, but it will not permit more to be charged than that. Entry will occur permitting price discrimination, only if discriminatory prices are the only way of allowing you to recoup those \$50 million. So I think you need new guidelines for even old-line industries with heavy but continuing sunk costs. But in the innovative sectors of the economy, it is absolutely vital, and the model has to be the model of the competitive pressures you get with absolute freedom of entry.

Where contestable markets got into trouble, and appropriately, is that some lawyers used it to say contestability means there can be no monopoly power. We never said that. We say perfect contestability is a theoretical construct which is not more realistic than perfect competition, but more useful as a guide to regulation. Increasing contestability is also a feasible policy to adopt by reducing regulatory barriers to entry, etc., etc., because that will drive you towards efficiency in markets. My argument is that while this was a useful concept in things like railroads, where the rules now in place explicitly are based on contestable markets, or in telephone regulation, where for example, in New Zealand they are explicitly based on contestable market theory, that's not the important place. The important place is the innovative industries where it can make all the difference in the world. Incidentally, the book I'm writing . . .

KRUEGER: One of the six?

BAUMOL: Yes, but the one I think is my great work, is about what the English used to call "Hamlet without the Prince of Denmark." We have all these models of growth theory. Good models—what I say is not meant to criticize them. But they leave out one crucial issue, which is: Why is growth in the free market economies ten times as great as in any other economy in history? And that's not just idle curiosity, because the other countries want to learn what that secret is. My view is that the main thing—there are many things—is the intensity of oligopolistic competition in which the oligopoly's main weapon is not price, but invention, in which it is life and death for them. That competition drives them each to try and prevent the others from getting ahead of them in innovation. But that will only be possible if government doesn't come in and say you have market power because you are not pricing at marginal cost.

KRUEGER: Under the contestability standard, do you think that Microsoft has behaved as if it faced perfect contestability?

BAUMOL: Oh, no. I think they haven't. I think they have tried to make it harder for entry. Without taking my opinion seriously, because I haven't studied the case. What I'd have to study is: Was it their activity that impeded entry, or were market circumstances preventing entry because of network effects, etc.?

KRUEGER: It's a fascinating issue that you raised about capitalist markets and innovation, technological progress, and productivity growth in your 2000 *Quarterly Journal of Economics* article on what we've learned since Marshall (Baumol, 2000a). Bill Bowen told me you were working on a project on collaboration in R&D. I was wondering how you reconcile those two. Because I would have thought that collaboration is harder in a more competitive environment.

BAUMOL: No. And this is one of the things that amazed me. Here I'm speaking not empirically but anecdotally, through lots and lots and lots of firms that I've worked with. What happens is that most of the firms involved in high tech participate in what I call technology-exchange consortia. I have a letter from IBM, which I have published with their permission—and with their editing, by the way—telling that they have an arrangement with every major competitor in every major computer component with an agreement that for the next  $x$  years the competitor will have access to every patent that IBM takes out, and IBM will have access to the competitor's patents. Now, very interesting, the arrangements are made not over current patents, but future patents. What happens is that, say, IBM sends a delegation to Toshiba or vice versa. At the meeting IBM lists all the things it's got on the drawing boards. Toshiba lists everything it's got on the drawing boards. Then the people from Toshiba say, "Well, it's clear you're ahead of us. It's clear your patents are worth \$30 million more than ours." And the people from IBM laugh and say, "That's ridiculous. It's worth at minimum, conservatively, \$80 million dollars more." And they haggle over that. They arrive at a price. Now, the point is that it is an added incentive for each of them to have a lot to put on the table. It doesn't slow down the race. I put large segments of the contracts in the book. The contracts are so detailed, they tell you how many people IBM will train, who will pay for their lunches, how many days . . . I mean, it's just incredible.

What happens is if IBM is faster than Toshiba in improving, say, its screen, it'll have a two-year advantage, even by training Toshiba people, before Toshiba can adapt its production equipment, training. And in such fields, two years is everything. So you have a double advantage. Number one, you still have the incentive to run as fast as you can in order to stand still, in Lewis Carroll's famous description. At the same time, this speeds up the dissemination of each new technique so you don't have a bunch of firms stuck with obsolete techniques for five years, as you would through what I call hostile technology transfer. Friendly technology transfer reduces the five years to two years. That accelerates growth.

Furthermore—now this is the really nice part—is that, say, there are five firms that engage in this consortium, although it's always done bilaterally. I know of no case where they all get together. But, say, there are five firms, and a sixth firm fails

to get in, or worse yet, one tries to cheat. If it is excluded from the consortium, it isn't like a price consortium. The other four can still go on exchanging their information, getting the competitive advantage of help from four sets of R&D establishments. The other one has only its own technology to live on, and the penalty for being excluded is tremendous.

So it's a competitive advantage, it hastens dissemination, and it doesn't slow down the need, the pressure, to produce your own. What we have come up with is the market mechanism turning into a giant innovation-stimulating machine, with not only production of the innovations and improvement of the innovation, but dissemination of the innovations all being forced by market pressures.

KRUEGER: Many of the blockbuster papers that you've had—the blockbuster ideas—started in some respects about very small questions. The arts is not a big part of the U.S. economy, yet it leads to an insight about costs and productivity which tells us a tremendous amount about the U.S. economy and other economies. Likewise, contestable markets started out with economics journals, or professional journals, which is not, again, a big issue in itself. Is that the way your work typically proceeds?

BAUMOL: I'm always looking for the theory. And sometimes the theory turns out to have wide application, sometimes nothing much. So when I teach at the university, I'm looking for theoretical ideas. And when I do consulting, I'm always looking at theoretical ideas. Steve Goldfeld used to be very amused when we'd go out consulting together and we'd have lunch with the CEO. And he'd see me grilling him, and Steve would know what theoretical model I was trying to test out. He would wink at me and see he'd caught me out again. But that's basically the answer. It's not that I'm particularly interested in the small items, but small or large, I'm always trying to get at the abstraction that comes from it.

Sometimes I'm lucky; the abstraction turns out to be useful. And sometimes I'm very lucky, and I turn out to be totally wrong. Because when I turn out to be totally wrong, that's when the best ideas come out. Because if my intuition was right, it's almost always going to be fairly simple and straightforward. When my intuition turns out to be wrong, then there is something less obvious to explain. Of course, I'm always depressed when I turn out to be wrong, but for a short while only.

KRUEGER: Actually, you seem to be one of the few economists who can admit when you're wrong.

BAUMOL: It's sort of fun.

KRUEGER: I found it quite striking that the reply you wrote to Brad De Long's comment on convergence began with a "mea culpa" (Baumol and Wolff, 1988).

BAUMOL: Well, but it's true, and there was nothing shameful about it. Shameful would have been hanging on by my fingernails and obfuscating, because I'm sure I would have been found out. But the temptation is there.

KRUEGER: The last few years have seen a resurgence of productivity growth. It seems also to have occurred in many of the service industries, perhaps because of new information and communications technology. Do you see that as a steady trend in the future?

BAUMOL: My guess is that in the long run we're going to see something not quite up to what we've seen the past few years, but something well above the years of the productivity slowdown. I think the pressures are there, for the reasons I've just indicated. And those pressures are enormous. But at the same time, we do have fluctuations, and people tend to think that because we've slowed down for five years we're going to hell immediately, and the reverse when we have a great explosion.

KRUEGER: What do you say to people who say productivity growth isn't so slow in the service sector. Maybe it's not growing so slowly; it's just that we're not measuring it properly.

BAUMOL: Oh, I'm sure that's right. But there are service sectors where you can measure productivity, as in orchestra performance—cost per performance, etc.—you can measure that. And it's indeed growing slowly.

There's a note I got from a young man who works at the Bank of England, Nick Oulton, which points out an important but paradoxical result I overlooked. Suppose it is true that all services grow in productivity, but at a fraction of the rate in manufacturing. Also, that the services supply only intermediate goods, to oversimplify it—the Oulton theorem works when it's a mixture of goods, but it's not as simple. Then, curiously, the larger the share of the labor force that goes into the slow productivity growth services, the faster productivity in the economy will grow. So if the slow-growing sectors are producing *final products*, the larger their labor force, the slower the economy's average productivity will grow. But if it's slow growth in *intermediate goods*, then its result is the opposite. The reason, when you think about it, is simple. The productivity in the two sectors is then additive, because one is serving as an input for the other. Say that the final product is growing at 10 percent a year, and the productivity in the intermediate good grows at 2 percent a year. As more and more of the labor force goes out of the fast-growing sector into the slow-growing sector, the more the labor force is getting the benefit of both the 2 percent and the 10 percent, as opposed to getting only the 10 percent if it stayed only in the fast-growing sector. So, since Oulton argues that most of the growth of the service sector has been in intermediate goods, then what would appear from the cost disease to be a drag on the economic growth is, in fact, a contributor to economic growth. He's absolutely right.

KRUEGER: Do you think that's been the case, that there's been a growth in services as an input into manufacturing?

BAUMOL: Oh, surely that's what computers are doing, assuming that we're finally really seeing the effects of computers on productivity. A lot of it has got to be in business services, in intermediate goods. So it's not a trivial component of the services.

## **Intellectual Approaches and History of Thought**

KRUEGER: When you think about your enormous corpus of work, what themes do you think run through it?

BAUMOL: Well, I've always been oriented towards applied theory. That is, I go back to the theme I started with my left wing orientation. I'm interested in economics not as pure abstraction, but how it can help to eliminate poverty, how it can help to deal with all the problems we know and recognize. My belief, which I think most of us share, is that without systematic analysis that is backed, if possible, by data, either statistical or historical or something else, we end up often doing things which sound rational but which in fact we live to regret. So I think if you look at almost everything I've written, I almost always follow it up, having written it as an abstraction, by asking, "Well, what can you do better with it that you couldn't do without it?"

KRUEGER: When you look over the field of economics now, which branches do you think are the most promising in terms of achieving those goals, in terms of making society work better or making people better off?

BAUMOL: There I'll take the Fifth Amendment. Number one, because you never can tell which is going to turn up producing a great new insight. And second, because I really believe passionately that there is no right method, that there is no better branch. The stories that macroeconomics is in terrible trouble, or microeconomics has reached a dead end—I don't believe any of them. I think every once in a while you reach a hiatus where for several years nothing very interesting will come out of the field, then it bursts forth again. But I really don't believe there is one particular arena that is going to be the most valuable.

KRUEGER: One arena where you've worked, and relatively few economists work, is in the history of economic thought. I'm interested in what attracts you to the history of economic thought.

BAUMOL: Thorstein Veblen (1918 [1957]) said that the motivation for academic research is idle curiosity. And I think that's the answer for my activity there. Jacob Viner, that very wise man, used to say that even though he has dedicated much of his life to history of economic thought, he didn't think it was a subject that needed to be taught to everyone, unlike economic history. In fact, fields that are constantly coming up with new ideas, such as physics, pay very little attention to their own history. It's only in fields where progress is slow and difficult that you spend much time worrying about predecessors. I do the work on history of thought because some of my friends from way back were very much interested in it, partly because of my early concerns with the writings of Karl Marx and Engels. But there's no really special or rational explanation.

## **Introductory Economics**

KRUEGER: I wanted to ask you about your textbook. You and Alan Blinder co-authored one of the most successful, durable, and readable introductory economics textbooks (Baumol and Blinder, 1988). How did the two of you come to work on the book?

BAUMOL: I had served as an advisor to Harcourt Brace. They were anxious to

get a high-powered textbook. And so we recruited Jim Tobin, Harry Johnson, Walter Heller—I've forgotten who else—but four people who were clearly deservedly well-known and of high quality.

And they were quite excited about it, but did nothing. So it became a little embarrassing. Harcourt Brace said to me look, you'd better step in and I said, "Well, clearly all four of these people are friends of mine and I love them all, but I think nothing is ever going to move while they're on the project. I think first we have to get them to agree on a friendly divorce, and then we have to find someone else." The publisher said, "Well, we hear there's a good young man at your university named Alan Blinder. Wouldn't he do?" I said, "Gee, what a wonderful idea! Why didn't I think of it?" That's how it happened. I can tell you, there's something wrong with both of us, because we have never quarreled or said a harsh word from the first edition until the present. We've said harsh words to the publisher, but not to one another.

KRUEGER: Did you, when you were at Princeton and then NYU, teach introductory economics?

BAUMOL: Yes. I've never taught it year after year after year, but I have taught it many times.

KRUEGER: I wanted to ask you a question that you posed in your paper in the *Quarterly Journal of Economics* (Baumol, 2000a). As you noted, there has been a great deal of progress since Marshall. Yet, other than macroeconomics, the introductory textbooks don't seem to reflect the progress that's taken place in economics. Why do you think that's the case?

BAUMOL: I think part of the story is that competition forces us to make sure we have the standard story in there. Part of it is that the new contributions are harder to explain at an elementary level. And part of it is that we authors haven't taken enough initiative. The latest edition of our book contains a chapter in which I try to introduce students to my model on why capitalist economies grow so rapidly. And I argue that should be not in the suburbia of microeconomics, but should be part of the heart of the story. Because anyone who is not an economist and is asked what's special about our economy, it isn't that marginal productivities are proportionate to wages. It's that we grow so fast. And that's what we leave out. So I tried to put that in a separate chapter, and I think we'll know soon from the survey with which one starts preparing an edition, whether anybody has used that chapter or not. I'll be rather curious.

KRUEGER: One thing I wanted to ask, Alan Blinder told me that one of the few areas where you are not particularly talented is when it comes to spelling. I find that very hard to reconcile with the fact that I've seen your work editing a paper—you once edited one of my papers for a book that you co-edited with Bill Becker—and I don't think I've had a paper edited so well. So it seemed to me a curious combination of skills.

BAUMOL: That's very easy to explain. My mother had an incredibly logical mind and sort of trained me to think through things. When my daughter was

hunting desperately for a doll she had misplaced, and we had to go catch a train, everyone else was tearing up the place, I would pull a Sherlock Holmes and sit down and say, “If they hadn’t found it yet, knowing what she is likely to do, where would she have put it?” And then go right to the spot where it was. It’s a trick I have pulled many times. Now, spelling is nothing like that. It’s purely mnemonic, whereas grammar has a structure, has a logic. So I almost never make a grammatical mistake and almost always, given the opportunity, will make a spelling mistake, though I am gradually learning now under the tutelage of the spell check.

## Consulting Activities

KRUEGER: In addition to your extraordinarily high academic output, you’ve also worked on several important antitrust cases and other consulting cases. Which were, in your opinion, some of the more important cases that you worked on?

BAUMOL: Some of the most important cases were the ones dealing with regulation rather than antitrust. I think the evidence of their importance was that litigation, where we prevailed, virtually came to an end. I believe strongly that many of the old regulatory rules, notably in transportation, not only were sources of huge direct inefficiency, but established an enormous rent-seeking activity which took the time of excessively paid minds in the lawyers, the witnesses, the management of the firms.

KRUEGER: In what capacity did you work on the regulation? Was it with the Federal Trade Commission?

BAUMOL: No, I would usually work with the railroads or the telephone company. I was happiest when the chief economic witness on the other side was somebody very good, because what would happen is that both of us would be very careful to acknowledge what the other person was saying that made sense. So the result would often be that the regulatory agency, having heard 90 percent of the story identical from both sides, had no choice but to come away with those rules. That’s actually what happened in many cases.

KRUEGER: Just to be concrete, the industries were . . .

BAUMOL: Railroads, mostly. Telecommunications. Electricity, where we’d been less successful. The chaos we have now in electricity pricing is something that could have, I think, been prevented. Those were the main industries.

KRUEGER: The *JEP* had a symposium [in the Spring 1999 issue] on the role an economist has as an expert witness. One of the papers, which was a skeptic’s perspective, was by Michael Mandel (Mandel, 1999), who writes for *Business Week*. And one of the things Mandel claimed is that he finds it difficult when he talks to economists on the phone to sort out what their potential conflicts of interest might be and what their involvement is in cases. So the question I have for you is do you think there’s a worry that academic economists spend too much of their time in consulting activities and not enough in research activities? Also, what about potential conflicts of interest that might arise?

BAUMOL: Yes. Both problems arise. I've seen a number of people who've let themselves be swallowed up by their consulting activities, which after all are highly lucrative—and fun—and end up distracting them from their academic research. I may have spent too much time at it. I've never let it distract me from my academic research because, as I said before, I used it as a laboratory. Quite systematically, not just occasionally. Some other people have done that too, but not everyone. So that's the answer to the first question.

And the answer to the second question—I have come across a few cases of blatant dishonesty, but by and large, I've been impressed with the integrity of the economics witnesses. But there's a different danger. I often wonder how come I always work with clients with whose ideas I agree? Have I convinced myself? Now, there are two answers to that, which may or may not be the right answers. One of the vice presidents of one of the firms with whom I've done a lot of consulting told me, "You know, I've checked the record, and every case where you've testified for us you first made us change our behavior the way you wanted to." Now, that's an exaggeration, but that's what he said. That's part of the story. But I can claim I've been very successful in dealing with juries. My tactic is a very simple one. I know my job is to convince the jury of my integrity, and that I'm not a hired gun, and that's not easy. I do that by carefully criticizing my client in front of the jury whenever I disagree with my client. Whenever there is a document that puts my client in a bad light, I insist on introducing it to the jury. When I think that my client should be constrained from doing something, I say it, and I tell the lawyers, if you want a witness who doesn't do that, that's fine. We'll still be friends, but that's the way I work. Now, the funny thing is it also turns out to be a good forensic device. The case where—this is now a legend among lawyers—where the chief juror interviewed by the newspaper said about me, "That kind of guy wouldn't lie to nobody." But I did it to satisfy my ego . . . and then discovered that it really works.

KRUEGER: Your work has had enormous influence on public policy. I presume you have had many opportunities to work directly in government. You also were on the New Jersey Council of Economic Advisors and chairman for several years. Is there a reason why you didn't go back to Washington?

BAUMOL: I was never asked. I mean, I was asked many times to testify, to come advise the Secretary of Commerce or someone, and I always went.

KRUEGER: But you were never invited to be a member of the CEA?

BAUMOL: No.

KRUEGER: Is that something that you regret?

BAUMOL: Not terribly. I think I would have liked it, but it's not something I wake up and feel that it is the tragedy of my life. I've tried to contribute wherever I could, and I hope I have made a difference in directions that were desirable. How much more I could have contributed had I gone down, I don't know.

## Theater, Art and Other Hobbies

KRUEGER: An article in the *New York Times* gave you much of the credit for the idea of the half-price theater tickets shop in Times Square. How did that come about?

BAUMOL: Bill Bowen and I had finished working on our book. At that point, we were not unpopular in the theater community, and they were very concerned by the rules against scalping—the rules that, in effect, made it very difficult if a show had been a great hit, to raise the prices afterwards, having sold some tickets earlier, or to lower prices if it were not, etc. These were under the New York state legislature’s rules. So the Theater League, the organization of the theater owners and producers, asked Bill and me to go and meet with the attorney general, a man much publicized at the time—his name was Louie Lefkowitz. We went and met with him, had great fun talking to him for a couple of hours. We said, “Look, the object isn’t to worry about low prices for the seats occupied by corporation executives and other fat cats. You want to make sure that poor students can get in and maybe if we can facilitate the provision of low-price tickets to those groups, that you would be flexible about the other rules.” So that’s how the idea was born, and we went back and discussed it with the theater owners and producers. And they founded the Theater Development Fund, of which I am now a member of the board, which runs the half-price ticket booths. There were many others who liked the idea and pushed for it, but its immediate origin was an attempt to weaken price controls on ticket prices.

KRUEGER: By allowing price discrimination.

BAUMOL: That’s right.

KRUEGER: Another thing I wanted to ask you, involving the arts. I’ve heard from sources within the AEA hierarchy that when you were president of the AEA you suggested putting artwork or a colorful picture on the program for the AEA meetings, the ASSA meetings. And that, in fact, the first cover to have artwork was your work.

BAUMOL: That’s correct. But it actually has a slightly longer history, and that is that we used to have, already several years before that, art exhibits by economists at the annual meetings. Harry Johnson, Abba Lerner, and I and several other people would bring sculptures and paintings, and they used to have a special room, usually next to where the publishers were exhibiting their wares. So we had a regular art exhibit.

At meetings, it was really very funny. Harry and I would sit on opposite sides of the table, each with little pieces of wood, sit there carving chess sets or whatever. We used to bring pieces of wood to each other, with a little one-upmanship. I thought I had won one time when I brought him a piece of Monticello, because Julian Boyd, who was then on the faculty, was the head of the work on the Jefferson papers, and they were working on Monticello and replaced some of the beams. And Julian carefully brought me back some of the wood. So I thought, “There, I’ve got you, Harry.” And next time Harry came back with a piece of wood from King’s College Chapel. So I lost that one.

KRUEGER: What did you make out of the piece of wood from King's College?

BAUMOL: Actually, I've still got it. I haven't carved it yet.

KRUEGER: I wanted to ask you about your interest in artwork, and your hobbies more generally. Your interest in art goes back at least to college, and probably much earlier.

BAUMOL: Yes, earlier than that.

KRUEGER: I've seen some of the work. Could you describe what type of art you do?

BAUMOL: Mostly it is three media. One is wood sculpture, one is oil painting, and the latest is computer painting.

KRUEGER: Could you describe what computer painting means?

BAUMOL: Yes, I can show you. There are programs in which you can make the drawings, you can create paintings that have the effects of water colors, oils, whatever you want. And they are totally what you have done, because it is your hand that is drawing them. It is your hand that is painting them. And it has many virtues. Number one, I hope you will publish the address of my website, because by calling up the website you can get a gallery of about 40 of my computer paintings, which you can download, and they are suitable for framing. [The website is (<http://www.econ.nyu.edu/user/baumolw>).]

But aside from that, it has the advantage that, for example, you can try two variants. You don't know whether you want a blue or a purple spot at some place. You try it both ways, show them both on the screen, keep the one you like. You can combine two paintings. You can do all sorts of wonderful things, and yet keep the painting as it was.

KRUEGER: I'd be very interested to see. It's like you can also run it through spell check. You've taught a class in art? I know at Princeton you taught a course in woodcarving.

BAUMOL: That's right. For about 20 years.

KRUEGER: And computer art, did you just learn on your own?

BAUMOL: Yes, entirely on my own. I don't know if there's anyone who does the sort of thing I do. It's basically oil painting done with the computer.

KRUEGER: You should submit one for the AEA cover.

BAUMOL: That's a thought.

KRUEGER: Are there other hobbies?

BAUMOL: I've collected watches and even learned to take them apart and sometimes successfully put them together again. Other than that my main hobby is reading history, which I prefer to fiction because the plots are so much more improbable and much more daring.

## **The Secret of Personal Productivity**

KRUEGER: Will, as one of the world's leading experts on productivity, I want to ask what's the secret to your productivity? Over such a long time span, over so many different media: research, public service, artistic output.

BAUMOL: Well, partly, I've had such stimulating friends. I mean, I've talked to you and already one idea came up in the course of the discussion. So the answer is I listen to what my friends say. Aside from that, I have been very fortunate that I write very quickly and very rarely have to rewrite. So that, of course, is an enormous help.

KRUEGER: You also must get ideas very quickly.

BAUMOL: No, it's not that I get them quickly. For example, I do 20 minutes of swimming three times a week. Well, when I'm doing the swimming, it's in a pool doing laps. Nothing is more boring than that. So I'm moving along there and thinking what should go into the last chapter of my book. I often tell students—graduate students—look, start thinking of your thesis topic in your first year, because you must get a topic you really love . . . if you hate it, it's going to show. So you've got to have something you love and that is feasible. That takes a long time. Just get so that whenever there's nothing else doing, you mull it over. And then at the end of two years when you're really getting ready to write, you're all ready to go. Because the writing you can do, very often, very rapidly. It's true. I wrote my thesis in six weeks, but again, I had thought about it for years beforehand. It was a matter of writing down all the things I had been thinking about at odd moments and debating with friends and so on.

KRUEGER: I think we should try to clone that secret. Thanks very much for chatting with me.

■ *The interview was transcribed by Jayne Bialkowski, and Melissa Clark provided assistance with references. It was edited by Alan Krueger and Timothy Taylor. Readers interested in a more in-depth description of William Baumol's work are recommended to read Bailey and Willig (1992), a subset of his collected works edited by Elizabeth Bailey (1976), and a collection of his cost disease articles edited by Ruth Towse (1997).*

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