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TALES FROM A NONSTANDARD CAREER IN OPERATIONS RESEARCH

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RRR 48-2000, September, 2000

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Abstract. This is a personal account of an unusual career in OR. I am both a researcher and a practitioner. Each of these aspects of me has benefited greatly from the other. In spite of my limited mathematical skills, over the years I have been able to contribute OR theory I am proud of. This paper is a largely collection stories about the ideas I have contributed and their often practical sources. The subject areas covered are sequencing theory, bidding theory, queuing theory, and energy economics. There are also short sections on my roles in practice publications and teaching.

1. Introduction

I have had an unusual career in OR. Most INFORMS members are either academics with limited experience in applying OR or practitioners who seldom contribute new theory. I am both a practitioner and a theorist. Each of these aspects of me has benefited greatly from the other. After completing a PhD in OR at M.I.T., I worked in industry (Shell and Xerox) where I was both a researcher and a practitioner. After about 19 years in industry, six more at a university run Federal research laboratory, Lawrence Berkeley Laboratory (LBL), and a lot of adjunct teaching, I became a "regular" academic at a research university where, among other things, I teach the practice of operations research.

I am a mathematical modeler, but not a great mathematician. However in spite of my limited mathematical skills, over the years I have been able to contribute OR theory I am proud of. This paper is a collection of tales from my nonstandard career. Largely, they are stories about the ideas I have contributed and their sources.

The stories are organized by subject area. Section two deals with sequencing theory, the subject of my doctoral dissertation. It is mostly about learning about what doesn't work in practice. Section three deals with bidding theory. It is an area I stumbled into in the 1960s and have returned to repeatedly, especially after the creators of "high theory" discovered it and started producing models that were misleading in practice. Section four is about a couple of contributions I have made to queuing theory—one when I found an important practical topic that queuing theorists were largely ignoring and the other I found when leading academics said something about queues that didn't sound right. Section five is about energy economics, a field I worked on before and after the energy crisis, but not during it. Section six is about my role in practice publications, and section seven is about my teaching.

2. Scheduling Theory—Learning What Doesn't Work

I went to graduate school at M.I.T. to study operations research. As an undergraduate at Pomona College, I had been math major, but it was a near minimum major with a lot of courses in other fields, especially economics. At Pomona, I had discovered the *Introduction to Operations Research* by Churchman, Ackoff and Arnoff (1957). Its chapter on waiting line models had particularly fascinated me. In addition, I had taken mathematical statistics, some elementary combinatorics, and a couple of other courses that were related to operations research. One was a "game theory" course that had more to do with mathematics that was new to me than with game theory itself. The other course was a linear programming course in the economics department of Claremont Graduate School. From it, I learned to think about optimization problems as scarce resource allocation problems, a point of view that I still find very helpful.

I enrolled in the Sloan School of Management planning to get a master's degree and, perhaps, stay on for a Ph.D. I had little idea what was involved in getting a Ph.D. I did not plan to become a college teacher, the main reason people get Ph.D. degrees. I just wanted to learn a lot about operations research and then get a job using it to help people manage things better. My first year classes were not just in the Sloan School, but all over the Institute. Professor Martin Greenberger approached me about my interest in working on a Ph.D. through the OR Center at M.I.T. M.I.T. had faculty interested in operations research in several different departments and had awarded Ph.D. degrees with operations research dissertation topics in each of them. These faculty had formed the OR Center and had recently decided to sponsor an interdepartmental Ph.D. program. Professor Greenberger's suggestion sounded good, and it worked out well.

During my second year at M.I.T., I completed my master's degree in industrial management from the Sloan School, but I was really working on my Ph.D. I had to take only one or two additional courses. With Professor Greenberger's encouragement, I used my master's thesis as exploratory work for my doctoral dissertation. I had heard John D. C. Little talk about the optimal assignment of priorities in queues. Recent work had solved that problem when there were linear waiting costs (Cox and Smith, 1961). I decided to try to solve the same problem with nonlinear waiting costs. My master's thesis was an exploration of what was known about this problem. It got me my master's degree, but it did not get rave reviews. Its lack of distinction foreshadowed difficulties I was going to have with this dissertation topic. It was too hard. One day, it dawned on me that the problem of optimal assignment of priorities to jobs arriving at a queue was much harder than the problem of optimally scheduling the jobs that were already there without additional arrivals, and I didn't know how to solve that simpler problem. No one else had solved it in general either, so I took this for my topic. It demanded less mathematical talent.

I got a number of new results on scheduling with no arrivals. I was able to find an optimal priority rule for a special nonlinear delay cost case: discounted linear delay costs. I spent a lot of time looking for optimal priority rules for other cases but finally gave up. Eventually, I became convinced that I had discovered the only kind of general nonlinear cost function for which a simple priority rule could be found, and I developed an approximate approach for use with other nonlinear costs.

I also considered some related problems. One was scheduling jobs with uncertain service times. I was able to show that the scheduling could be done optimally using the average service time if costs were linear and a different way if the costs were discounted linear. Another problem involved scheduling jobs when there was no cost if a job met its deadline and a fixed cost if it was late. I developed some insights into the characteristics of the solution in this case and formulated the problem as an integer programming problem. This didn't seem useful, since there was then no way to solve integer programming problems. Another problem involved scheduling jobs using parallel servers. I devised a computational method for scheduling jobs optimally in such a situation for both the linear and discounted linear cost cases. I was also able to show conditions under which it did not pay to interrupt a job and resume it later, perhaps on another server.

These results were sufficient for a dissertation (Rothkopf 1964). My defense went well, and I went to work at Shell Development Company in California. I wrote two papers on the dissertation. Both were accepted promptly (Rothkopf 1965a, 1965b). The work on scheduling on multiple processors and, to a lesser extent, the result for discounted linear costs got some attention from theorists. I didn't bother to write up the results on absolute deadlines. However, a few years later I was chagrined to see the results independently derived in another paper (Lawler and Moore, 1968). Integer programming had progressed, and the results were of more interest than I had thought.

While I was doing research on "scheduling" at M.I.T., I never had any opportunity to observe or deal with a real scheduling problem. At Shell, I looked for operations research projects that would help Shell. I looked for scheduling problems. I didn't find anything for which my dissertation research would be useful. Furthermore, as I thought about real scheduling problems, I decided that the whole approach of my research would not be useful. Most "scheduling problems" are micro problems. It is not reasonable to use anything but the simplest of approaches to these problems. Little is at stake, and the information needed to analyze how to improve the schedules is usually unavailable, difficult to obtain, or highly complex.

I did find a few "big" scheduling problems. The most promising of these involved Shell's fleet of coastal tankers. These tankers carry oil products from gulf coast refineries to east coast terminals. Shell prepared thirty-day schedules for these tankers and updated them every few days. I imagined writing an optimization program to schedule the tankers and then showing the result to the retired tanker captain who did the scheduling. In my imagination, he looked at the computer output, shook his head, and said, "This tanker will not fit under the bridge to that harbor except on a minus tide or with a partial load." In other words, I anticipated trouble knowing all of the relevant technical details and in representing the problem in a useful optimization model. I thought about approaches different from the scheduling research I had done. Another approach to scheduling this fleet that might work better was to create a computer program to help the scheduler. It could make his task easier and, at the same time, give him careful economic evaluations of alternatives, making it easier for him to create better schedules.

More pressing projects arose, and I never got to try this approach. However, a little while later I got a paper on job shop scheduling to referee. It proposed an integer programming formulation that took into account several realistic factors not included in previous work. I wrote a review for the journal saying that it was a competent job of what was currently being published on this topic. However, I added that I had come to doubt the usefulness of the whole approach. I included for possible publication with the paper, were they to publish it, a note suggesting approaches to scheduling problems other than solving optimization models. They published the paper and my note (Gupta 1971, Rothkopf 1971).

I stopped working on scheduling theory, but about ten years later I received an inquiry about my thesis from a German graduate student. He mentioned that he was having difficulty finding priority rules for nonlinear cost cases. His inquiry started me thinking again about the priority problem. As a graduate student, I didn't even think about trying to prove that there were no optimal priority rules other than the ones for the linear and discounted linear cases. Now, however, I realized that such a result would be useful. Steve Smith, a co-worker, and I developed and published such a proof (Smith and Rothkopf, 1984). The implication was, you can be optimal or you can be simple, but not both. For most scheduling problems, simplicity is essential, so approximate solutions are needed. However, the paper did not stem the flood of papers involving complicated scheduling optimizations.

Again, I stopped working on scheduling theory, but a decade later I heard Michael Katehakis talk about a problem in which a server allocates resources to tasks sequentially. He used a scheduling problem as an example; suddenly I was revisiting old territory. The priorities in my thesis and in the 1984 paper were static. Michael raised the possibility of using time-varying priorities; a free server would process the available job with the highest current priority. Were there additional classes of delay cost functions for which dynamic priorities are optimal? I suspected not, but the 1984 proof depended completely upon the assumption that priorities don’t change. I came up with a different approach, and Uri Rothblum's superior mathematical skills turned it into a proof that even allowing dynamic priorities does not add to the classes of cost functions for which priority rules are optimal (Rothblum and Rothkopf, 1994).

I am grateful that my industrial experience steered me clear of low value work on scheduling theory. A 1992 paper in this section of *Operations Research* by three professors who had devoted most of their careers to scheduling theory (Dudek, Panwalker and Smith) contains an amazingly frank lament that they had wasted much of their professional lives developing useless theory. Both fundamental and applied theory can have great value, but for applied theory to be valuable, it must be truly applicable. Paying attention to applications is essential.

3. Competitive Bidding—Practice Crossing the Path of High Theory

Multiplicative Strategies and the Winner's Curse

In 1964, when I was finishing my Ph.D. at M.I.T. in operations research, I wanted to *do* operations research, not teach. I went to work for Shell Development Company's Applied Math Department. Shell soon had me working on a then innovative applied project, using subjective probabilities to help Shell evaluate a new business. Shell was considering building a plant to make synthetic detergent alcohols which no one had ever manufactured commercially before. The project went well. The venture looked attractive, and Shell decided to build the plant. I got involved in other projects including creating a computer program to facilitate similar probabilistic venture analyses. One day, Paul Deisler, head of development for Shell's Industrial Chemicals Division, phoned me. Paul had been the "client" for my project. Paul had a question.

Shell had solicited bids on the construction of the plant. It took chemical construction companies eight weeks to prepare a serious bid. Following Shell policy, three companies had been selected to prepare bids. Six weeks into the eight week bid preparation period, one of the companies notified Shell that it had just won a big contract and would be unable to bid. Shell had to make a fast decision. It could go ahead with the better of the two bids it would receive, or it could delay the whole project by six weeks to let another bidder have a full eight weeks to prepare. Paul knew what the six week delay would cost, but he needed to know the value of a third bid.

My venture analysis work had obtained Shell's cost engineer's probability distribution on the capital cost of the plant. Paul figured that Shell's cost engineer was about as good at estimating costs as the ones at the bidding companies. He had assigned an engineer to do a small Monte Carlo simulation on a desk calculator (This was 1965.) comparing the minimum of two random draws from the probability distribution with the minimum of three draws. The engineer compared the average of the minimum of the two draws with the average of the minimum of three draws and used the difference as an estimate of the extra cost of having only two bids. Paul's asked me, "Are we misusing our estimator's probability distribution?"

I didn't think so, but it occurred to me that it might be possible to do a similar calculation analytically rather than with the approximate and burdensome Monte Carlo simulation. From George Eldridge, a Shell chemist turned statistician, I had learned about the use of extreme value statistics in the study of corrosion pitting. The distributions used in extreme value statistics give formulas for the deepest pit; I thought they would also work for the lowest bid. Although it was too late to help Paul with his decision, I used the Weibull distribution, an extreme value distribution, to calculate formulas for the minimum of n bids and wrote a Shell report, "The Expected Value of an Additional Bidder" (Rothkopf 1965c). The report did analytically what Paul's engineer had done by simulation.

Twice a year, Shell researchers presented new results to Shell Chemical Company's senior engineers. At the next presentation, I explained this report. I thought that it had gone well, but afterwards a senior engineer, Joseph Wilson, took me aside and in the nicest possible way, explained that my approach was wrong. It assumed (as did Paul's simulation) that the bidders would bid the same way no matter how many competitors they had. This, he said, was a false premise. Chemical construction bidders were quite sensitive to the number of competitors they faced. Indeed, a bidder who knew that there were four other bidders might even refuse to prepare a bid. Bidders always asked who else was bidding and, since they could find out anyway (They would need quotes on equipment with the same, often unusual, specifications.), Shell told them.

I was crestfallen but also challenged. I thought some more about the problem. A model that gave the value of an additional bid would have to take into account how bidders would react to the presence of additional competition. It thought I would be unable to get data (beyond what Joe Wilson had told me) on that, so I decided to model bidder reaction theoretically. How should a bidder react? I thought of a bidder's strategy in two parts. First, the bidder would make a cost estimate. This would be its best unbiased estimate of what it would cost to build the plant. This, however, was not its bid. Its bid would be a multiple of that estimate. This multiple would be greater than one to provide for profit if the bidder won. However, if the bidder bid too high, its chance of winning would go down. The best bid would balance profit if it won against the likelihood of its winning. It would depend upon the number of competitors and upon how these competitors bid.

This time I used the Weibull distribution for cost estimates. Initially, I assumed that every bidder drew its cost estimate independently from the same distribution. It then used its multiplier to get its bid. I derived an equation for a bidder's expected profit that depended upon its multiplier and those of its competitors. I used calculus to get an equation for the profit maximizing multiplier for each bidder that depended upon the multipliers of the other bidders. Since all of the bidders were initially (i.e., before making cost estimates) in the same situation and since the multipliers were to be used whatever the estimate turned out to be, I sought a symmetric solution for the best multiplier equations, i.e., one in which each bidder used the same multiplier.

I found one, and it had nice properties. First, it was a Nash equilibrium--a solution in which no competitor can make itself better off by changing its strategy unilaterally. This made it a game theoretic model rather than merely a decision theoretic one. Second, the solution depended upon the estimating accuracy and upon the number of bidders in reasonable ways. The more accurate the estimating, the less room there was for profit. (In the limit, with no uncertainty, there was no profit at all.) This fit with the idea that businesses earn profits for taking risks. It also fit with the industry practice of disclosing all known risks and providing relief for the winning bidder in the event of extreme contingencies. I found a trade press statement by the head engineer of another company claiming to follow such practices.

The behavior of the solution with respect to the number of bidders was interesting. When a bidder went from facing one competitor to facing two, it bid more aggressively. Beyond two competitors, however, increasing competition resulted in a bidder needing to bid less aggressively. At first, this seemed paradoxical. However, a bidder's expected profit if it wins is less than its bid minus its cost estimate. The difference between these two can be thought of as a contingency fund. In other words, the successful bid is equal to the sum of three items: the estimated cost, a contingency fund, and profit. The contingency fund is needed even though the cost estimates are unbiased. They are unbiased with respect to *all* cost estimates. However, a bidder that underestimates costs is more likely to win the auction than one that overestimates them. This selection bias means a bidder's cost estimates on jobs it actually wins will tend to be optimistic. But these are the only ones that matter. The bias is greater with more competitors. Correcting for it increases the optimal bid when there are more competitors.

The expected profit of the winning bidder declines with more competition. The effect of the additional competition more than offsets the effect of the less aggressive bidding strategy. Also, with more bidders the chance of winning goes down. Hence, the expected profit of a bidder declines approximately as the square of the number of bidders. No wonder bidders are sensitive to the number of competitors they face.

I was able to extend the model in two ways. First, I set up equations for situations in which bidders had different average costs. I solved these equations analytically for the case of two bidders. With more than two bidders, numerical solutions are needed. Other Applied Math Department members and I computed extensive tables for cases with more than two bidders

(Rothkopf, Nelson, and Barton, 1966). The other extension was a similar model for high-bid-wins auctions in which the bidders bid to buy something.

After presenting the paper at a national meeting, I got a letter from Ed Capen of ARCO's oil exploration arm asking why I was using so little uncertainty in my examples. I shared Ed's letter with Shell's management since it implied that ARCO was using models for oil tract bidding.

My paper appeared in *Management Science* in 1969. The same issue of *Management Science* had a paper by Bob Wilson of Stanford. These two papers were early game theory treatments of bidding and the first published examples of what is now called a common value bidding model. In a common value model, what is being auctioned has the same value to whoever wins it. The bidders are just uncertain about what that value is. Previously published bidding models had all been what are now called private values models (See Friedman 1956, Vickrey 1961). They assume that each bidder has its own value for the item being auctioned and that the worth of the item to other bidders was not relevant to a bidder except in trying to predict how the competition would bid. In private values models, there is no selection bias.

In 1970, Ed Capen started giving talks on the work he and his colleagues were doing at ARCO, and in 1971 their paper, "Bidding in High Risk Situations, appeared in the *Journal of Petroleum Technology*. It describes simulation models of auctions that ARCO was using to help it decide how much to bid for oil leases. The ARCO model was a common value model. The paper had some striking data on how uncertain oil lease prices were, and it took the phrase "winner's curse" from the oil patch where it was used to describe selection bias and its effects and put it into the academic literature. The paper argued persuasively that the use of private value models that ignored the winner's curse were leading bidders to bid too aggressively and to get low rates of return on their oil exploration investments even though they were discovering lots of oil. The only bidding model that the ARCO paper mentioned favorably was mine. The ARCO paper was extremely influential. I would bet that it is more cited in the economics literature than any other paper from the petroleum engineering literature. The concept of "winner's curse" based upon the common value model is now recognized as important and is widely taught. (In 1997, Bob Wilson told me that at least five Stanford Graduate School of Business courses cover the concept.)

I eventually discovered that my 1969 paper had a flaw. It "proved," incorrectly, that the use of multipliers as bidding strategies was not restrictive. Worse, the result was wrong. While assuming a fixed multiplier for a bidding strategy is sometimes a reasonable descriptive model of bidder behavior and sometimes a good approximation of the optimal strategy, in general, a bidder is better off using different multipliers in different situations. Like my previous error this troubled me. It led to more research and a pair of papers in *Operations Research* in 1980. The first described the flaw and its consequences for interpreting my 1969 paper and other subsequent papers that used multiplicative strategies. (The flaw is a set of inconsistent implicit assumptions about the ratios of estimates and uncertain quantities. My seven page proof of the inconsistency was replaced with a one paragraph proof supplied by a referee.) The second paper analyzed a model that generalized the 1969 model by giving bidders strategies with two parts. One part was a multiple of the bidder's estimate. The other was a fixed amount--a fraction of the common prior estimate of the cost or value before the bidders develop their private estimates. In this model, a bidder's reliance the multiple of the estimate increases as the amount of prior information decreases. In the limit, with no prior information, the best strategies approach the multiplicative strategies in the 1969 paper.

I have used the multiplicative strategy model to study sequential auctions, withdrawable bids, and subsidies for selected bidders. It is relatively easy to solve in some situations such as asymmetry among the bidders. However, I have been frustrated by the lack of use of the models by economists and game theorists. Some game theorists set up models in which strategies are general functions of the estimates. They then derive differential equations for the equilibrium strategies. These differential equations are hard to solve. Often, they can find an analytical solution only in the limiting case in which there is no prior information.

Sequential Auctions

Because my work on bidding was known in Shell Chemical, a manager asked me to examine an analysis by a young engineer. It dealt with bidding to supply a solvent, methyl ethyl ketone (MEK), to government agencies. The manager was uneasy about the analysis, but saw no flaw in it. The engineer used data on past government procurement bids for MEK that occurred several times each year. From this data, he had developed a probability distribution for the unit price in the best competitive bid. He had done so cleverly, correcting the raw data for differences in freight and quantity related costs. With these corrections, the distribution of the best competitive bid was narrow. Using it in a much used decision theory model of how much to bid in a single auction (Friedman, 1956), he determined that offering a slightly lower unit price would increase Shell's expected profit substantially. I saw that the slightly lower bid was substantially more profitable because Shell's chance of winning had gone from about 50% to about 99%.

At that time, the only manufacturers of MEK in the US were Shell and Esso (now Exxon). Both sold the solvent commercially. In the government auctions, occasionally a reseller bid, but usually Shell and Esso were the only bidders. I soon realized that Shell could win 99% of the government MEK procurement auctions only if Esso did not react to Shell's new strategy. This seemed most unlikely. If Shell won several auctions in a row, Esso would probably assume that Shell had cut its price and bid more aggressively. It seemed unlikely that in the long run Shell could win much more than about half of the auctions at anything close to the current price. Thus, Shell put aside the engineer's analysis and kept its approach to MEK bidding.

Even though the bidding literature talked about how to use data on your competitor's prior bids in deciding how much to bid, there were no models of sequential auctions in which a bidder's behavior in one auction affected its competitors' behavior in later ones. This seemed like a problem that needed attention. I didn't have the time to work on it then, but I kept it in mind. In 1973, I left Shell to work at Xerox’s Palo Alto Research Center (PARC). There, I described the problem to Shmuel Oren. We decided to model sequential auctions as a control process. In this model, competitive pricing aggressiveness is the state of the system. When a bidder chooses a bid, the choice has two effects. First, it affects the bidder's expected profit in the current auction. Second, it affects the competitive pricing aggressiveness, which, in turn, affects profit opportunities in future, auctions. The optimal bid balances these two effects. The optimal balance is affected by the discount factor between auctions and by how much a bid affects future competitive bids. We modeled the first of these in the standard way. The second may involve institutional factors such as the ability of competitors to observe bids and outcomes and the extent to which future auctions are easily comparable and involve the same competitors. We modeled it using a "reaction function," an idea from economics that predates game theory.

We tried two different reaction functions. In one, bidders react to bid reductions but not to bid increases above the current level. This leads to behavior analogous to the kinked demand curve model of economics. In this model, it is hard to explain how bids arrived at their present price level. However, it is clear that if the auctions are frequent enough and the anticipated reaction is strong enough, the general bid level will remain unchanged. Bidders will not bid more aggressively since to do so will too severely limit their profit in future auctions. Nor will they bid less aggressively, since this will reduce profits in the present auction without leading competitors to bid less aggressively in future ones.

The other reaction function we considered allows decreases in competitiveness by others in response to less aggressive bidding by the bidder. Using it, we generalized the multiplicative strategy model. The effect was to include an additional term in the equations for the equilibrium strategies and for the expected price. This term leads to less aggressive bidding. The discount factor between auctions and the constant of proportionality in the assumed reaction function affect the size of the term. The term and its effect go to zero when the discount factor between auctions is zero. The term is proportional to the size of the competitive reaction. When competitors do not react to what a bidder does, there is no effect. However, when the competitors react strongly and the discount factor between auctions is near one, the effect is dramatic.

This reaction function approach, even when inserted into a one-shot game-theoretic model, is not pure game theory. In particular, it does not examine whether the assumed reaction is the competitors optimal reaction. However, since our paper appeared in 1975 (Oren and Rothkopf) no one has calculated the optimal reaction under any assumption about the structure of the situation. Furthermore, observations of how bidders actually react to deviations from prevailing bid prices are limited since clear deviations are rare. Thus, modeling behavior based upon how bidders assume their competitors will react seems reasonable. However, game theorists have not taken to this "impure" approach. I still hear discussions of repeated auctions that remind me of the model I saw in Shell. They treat the bidders as rational in the context of a single auction, but ignore the effects repetition. I recently wrote about this issue for the *Electricity Journal* since deregulation is bringing about daily electricity auctions (Rothkopf 1999).

Simultaneous Auctions

Although my original work on bidding in Shell had been motivated by a problem in which Shell was a bid taker, Shell's most important bidding concern was offshore oil lease bidding in the Gulf of Mexico. These leases sold for as much as $100 million. I discussed working on Shell's oil lease bidding with the President of Shell Development Company. He said that Shell's Exploration and Production (E&P) organization was secretive about bidding and wouldn't talk about it to anyone in research. He offered to transfer me to its New Orleans headquarters. That was impossible for my family, and I declined the offer.

The secrecy in E&P was not as complete as he said, however. From scientists in the Shell lab that dealt with E&P, I learned that Shell faced a problem in allocating scarce bidding capital in simultaneous lease auctions. Shell's top management limited E&P's "exposure" in simultaneous auctions. Exposure was the amount Shell would spend if all its bids won. E&P had to allocate that exposure to the different tracts.

I discussed the problem with several other operations researchers including Olvi Mangasarian and Bob Stark. In principal, the problem of selecting the set of bids that maximizes expected profit while honoring the exposure constraint is mathematically complex because bids have S-shaped expected return functions. I was hearing, though, that bidders were ignoring this complexity and making marginal adjustments in the amounts of the bids to maximize the expected profit. The 1956 paper by Friedman had suggested this approach, but others had since pointed out that it was not guaranteed to work until the tracts that were to get non-zero bids had been correctly selected. With, say, thirty tracts, there are over a billion ways to select tracts to get non-zero bids. In addition, each tract could have two different bid levels with the same marginal return.

I realized that dynamic programming was capable of calculating an optimal set of bids. Dynamic programming was a numerical procedure, however, that gave little insight into why the bids came out the way they did. This "black box approach" had little appeal. But were the bidders behaving sensibly?

When I thought about the expected return functions, I realized they have maximum marginal expected returns (MMERs). A bidder could rank order the tracts by their MMERs. Starting with the top ranked tract, he could, if he had enough exposure, allocate enough of it to that tract so that the bid was at that MMER. If he had no more exposure available, he would have the optimal solution. If he had more exposure left, he could allocate it to that tract until the marginal expected return fell to the level of the MMER on the next ranked tract. If he ran out of exposure before he reached this level, he would have an optimal solution. Then, if he had enough exposure left, he could allocate enough of it to this second ranked tract so that it received MMER. If he reached this point and then ran out of exposure, he would again have an optimal solution.

And so on. The only chance of suboptimality came if he ran out of exposure while trying to bring the next ranked tract into the solution at its MMER.

With this insight, I calculated tight bounds on the maximum suboptimality from marginal adjustment and proved that a bidder would never bid on two different tracts with the lower of the two bids having the correct marginal rates. In an optimal set of bids, at most one tract would have a bid at the lower level, and I was able to establish conditions under which even this would not happen. Hence, the bidders were behaving sensibly. Eventually, a paper on this work appeared in *Operations Research* (Rothkopf 1977). This story has an epilog. At Rutgers, I met Al Williams who came there after retiring from Mobil. Al said that when this paper came out, Mobil executives became upset. Because its approach was so close to Mobil's they thought that it had leaked. Al, who had read my bidding papers, assured them that I could have figured this out myself.

My next paper on simultaneous bidding came from theory, not practice. A co-worker at PARC, Steve Smith, had worked on a scheduling problem. He thought its mathematics might have implications for bidding. We worked on this together. Steve's mathematics did have implications for a simultaneous bidding problem. Much of the prior work on simultaneous bidding assumed that the different items being sold simultaneously had independent values to the bidders (or that each bidder wanted just one item), but this mathematics suggested some results for a problem in which a bidder would incur a fixed charge if it won any items at all. We wrote a paper on this, motivating it with hypothetical examples of such situations. The paper was quickly accepted. It appeared in 1983, but has apparently not influenced bidders or other research.

In contrast, another paper on bidding in simultaneous auctions has been quite influential. I was exposed to the situation that motivated that paper when I was consulting with US West, the regional telephone company, on bidding in FCC auctions for licenses to use particular parts of the radio spectrum for mobile communications. Initially, I was only to help them decide how to bid. The FCC had issued a notice with proposed rules for the auction. These were close to those for offshore oil auctions, and US West anticipated the final rules would be similar. I did help them with their bidding in the first FCC auction, but the consulting evolved to include advising them on the auction rules. Several companies filed briefs with the FCC (FCC Docket 93-253) accompanied by papers by leading economists and game theorists. The papers suggest alternative rules for the FCC's spectrum auctions. I helped US West understand the implications of these rules.[1] The FCC ultimately selected an imaginative auction design suggested by Paul Milgrom and Bob Wilson. It is a simultaneous progressive auction. A group of licenses is sold simultaneously. There are successive rounds of bids, and nothing is sold until the bids die out on all of the licenses.

One problem with the FCC auction design is that the value of a license to a bidder depends upon the other licenses it wins. Often, winning a license makes licenses for neighboring areas more valuable. However, the auction design did not allow bidders to bid on combinations of licenses. This gives bidders a serious problem and might lead them to bid less than they otherwise would. Two reasons were advanced for not allowing bids on combinations. One of these reasons doesn't apply when combining licenses increases their value, the situation the FCC seemed to face. The other was a mathematical concern. Allowing bids on all combinations of licenses makes the problem of picking the set of nonconflicting bids that maximizes total revenue NP complete.

I was upset that a computational problem, which might arise if bids were allowed on all combinations of licenses, was being used to rule out bids on *some* combinations that seemed economically significant and computationally manageable. With US West's permission, discussed this issue with the FCC. I was able to show them some simple combinations that

[1] To my amusement, five different papers by five sets of leading economists all started out summarizing auction theory and then arguing that existing theory implied there was only one way to hold the auctions;

would not cause computational difficulties. However, by the time I did this, the discussion of the auction design was far along, and the conversations had no immediate result. In addition, I could not identify all of the combinations that could be guaranteed not to lead to computational difficulties. The FCC auctions went ahead and collected billions of dollars. They were viewed as a success, particularly by the people who had designed them. However, I decided to try to answer the question of what combination bids could be allowed without risking computational difficulties.

Ron Harstad, a frequent collaborator, Aleksandar Pekec--a Ph.D. student in Rutgers' Math Department who had taken a couple of RUTCOR seminars from me—and I were able to find several potentially important kinds of bids on combinations that would not cause computational problems. For each of these, we were also able to show that slightly more complex combinations would risk computational problems. The working paper version of our paper was widely circulated and was much cited even before it appeared in *Management Science* (Rothkopf, Pekec, and Harstad 1998). We argued that just because allowing bids on all possible combinations could risk computational difficulties, was no reason to forbid bids on certain kinds of economically significant combinations that do not carry that risk. The FCC has now awarded a contract for software to manage a bidding process involving some combinations. Related computational issues are arising in designing auctions for electricity and e-commerce.

Bill Vickrey's Auction

In 1982, I left PARC to head the Energy Analysis Program LBL. I tried to get funding for research related to bidding. At first, I got a bit, but there was little interest in bidding in the Department of Energy (DOE) and reluctance of other funding sources for work on bidding

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but they had five different proposed ways. After technical conferences and much discussion, it was clear that existing bidding theory had little to say about a situation as complicated as the one the FCC faced.

(including the National Science Foundation, the Department of the Interior, and the Department of Agriculture) to fund work at DOE Labs. Eventually, bidding became a hot topic. PURPA (the Public Utilities Regulatory Act of 1978) had an unexpected impact, and public utilities started holding auctions. PURPA told electric utilities to buy power from two kinds of sources--renewable and cogeneration--at "avoided cost." The utilities were convinced that there was little economic renewable or cogeneration power. They were right about renewable power, but their own rhetoric had fooled them with respect to cogeneration. A lot was available. Had there been only a little, then the utility's marginal cost would be the avoided cost. Since the utility was regulated, its costs were known. Some state public utility commissions held hearings to determine the marginal costs of its utilities to set the rates for power from renewable and cogeneration sources. When the utilities and the commissions realized the large amount of power available from cogeneration, it became clear that this was not marginal power and that the "avoided cost" was not the utility’s marginal cost. Often, more power was available from cogeneration than the utility needed. If only some projects were needed, the avoided cost of an accepted project was the cost of the best project rejected. However, since the cogenerators were unregulated, their costs were not known. The natural solution was for a utility to hold an auction and to accept the lowest priced power.

I worked on a project for DOE with Ed Kahn, an LBL expert in electricity economics and regulation, to compare alternative ways of holding such "PURPA auctions." There were a number of different issues in the design of these auctions including their frequency, and how the utilities should deal with offers of "chunks" of power that did not add up exactly to the quantity that they wanted (See Rothkopf *et al.* 1987 and Kahn *et al.* 1990). An important issue was whether the winning bidders got paid the amount they had bid or a market clearing price set by the amount of the best losing bid. Except for California, the states using PURPA auctions decided to use standard sealed bidding in which the winning bidders gets paid the amount of their bids. California, however, was in the process of opting for "Vickrey auctions" in which the bidders get the amount of the best losing bid.

In 1961, Columbia University economics professor William Vickrey published a *Journal of Finance* paper on bidding that was well ahead of its time (and which went largely unnoticed for 15 years). It analyzed sealed, "second-price" auctions, now often called Vickrey auctions. In such auctions, the maker of the best bid wins, but the price is set by the best losing bid. (With just one item for sale, the best losing bid is the second best price; hence, the name second-price.) He argued that such auctions would work better than standard sealed bid auctions because the bidders had incentive to bid their true values (or costs, in a supply auction). If they bid more aggressively and this caused them to win, they would wish they hadn't, and if they bid less aggressively and it caused them to lose, they would wish they hadn't. If the bidders bid their true values, then the bidder with the highest value would always win. This made the auctions perfectly efficient. He also developed a game theoretic model of a private values auction and proved a “revenue neutrality theorem,” i.e., that, on average, the revenue of the seller would be the same as in a sealed bid auction. Despite these arguments, Vickrey auctions remained rare.

In California, economists representing Southern California Edison Company had urged Vickrey auctions on the Public Utilities Commission citing these arguments (See California PUC 1986). I tried to imagine how Vickrey auctions would work in a PURPA auction. Would bidders bid their true costs? It was clear that businessmen don't like to reveal their costs. But would they learn to do so because of Vickrey's argument that it was in their best interest? During a discussion of this issue with Ed Kahn and Tom Grahame of DOE, I realized that they wouldn't because it wasn't *really* in their best interests. Businessmen have excellent reasons for not wanting to reveal their costs. They have to negotiate with other people; they would be substantially disadvantaged in these negotiations if they revealed their costs. This was certainly true for many PURPA bidders. If they won, they would have to negotiate for finance, construction, labor, permits, etc. The disadvantage in such subsequent negotiations didn't occur with standard sealed bidding and wasn't considered in Vickrey's analysis.

I realized that this was an important result and wrote a paper on it. To make the paper credible to economists, I decided to include in it a model in which bidders had to give some fraction of the extra amount they were getting above their bids to third parties just because their bids revealed a price at which they were willing to provide the power. Dr. Thomas Teisberg who was consulting with us on this project was faster and better at the mathematics involved in this kind of model than I, and I asked him to work them out. The results were interesting. In our example model, the bidders raised their bids so that, on average, all of the extra revenue captured by third parties would come from the bid taker. When I discussed this with fellow bidding theorist Richard Engelbrecht-Wiggans, he suggested that the result might be general. Following his suggestion, I verified that the arguments of Meyerson (1981), who had proved a more general version of Vickrey's revenue neutrality theorem could be applied to a situation in which third parties captured part of the revealed gain of the winning bidder. Meyerson's model had just two parties, the bidder and the bid taker. Thus, his revenue neutrality theorem was also a payment neutrality theorem for the winning bidder. However, when applied to a situation involving third parties capturing part of the winning bidder's gain, his arguments applied only to the bidders, not to the bid taker. Thus, it was really a payment neutrality theorem. Hence, any gain by a third party had to be paid, on average, by the bid taker.

I also reviewed the arguments in Vickrey's 1961 paper. He had not thought of the effect of second-price auctions on negotiations with third parties, but he had considered and rejected a number of other possible objections. I found all of his arguments except one convincing. That one was the argument that bidders had no reason to fear the bid-taker cheating (by making up a false bid that lowered the payment to the winning bidder) because a trustworthy third party could handle the bids. I decided that trust was still an issue, and that if the bid-taker could anticipate the bids of the parties, it could solicit an insincere bid from a confederate. I also considered and rejected some reasons that Vickrey had not considered. Finally, I decided to add some material to the paper that argued that modeling bidding realistically was important, and that merely solving a game theoretic model might not be sufficient.

I expected trouble with the reaction of economists to this paper, and I decided to call the paper "Why Are Vickrey Auctions Rare?" in order to challenge them to face this issue. After all, if in 1961, Vickrey had found a better way to do auctions and 25 years later almost no one was doing them that way (a fact argued in the paper), then either people were persistently behaving in a suboptimal way (something economists would question) or else there was another explanation. I was right to anticipate trouble. The paper was rejected by the first journal we sent it to. After that, I asked Bob Wilson where he thought we should send the paper. He suggested the well respected and widely read *Journal of Political Economy* (JPE). This journal Bob noted takes rational economic behavior seriously and sometimes doubts mathematical economic theory. The paper was accepted by the JPE and has had a substantial impact. Economists are now usually circumspect in their claims for the virtues of Vickrey auctions. One interesting paper written in reaction to this paper proposed a cryptographic version of the Vickrey auction that would keep the winning bid secret while letting losing bidders have enough information to know that their bids should have lost (Nurmi and Salomaa 1993).

Some economists expressed concern that there was no model to explain exactly how third parties would capture part of the profit of the winning bidder and exactly how much of it, but none has been offered and few seem to doubt the fact of such capture. More practical types, some of whom were not sure they understood the arguments for Vickrey auctions anyway, quickly realized the implications of the information revelation.

On the other hand, there was some concern about our argument about bid taker cheating in Vickrey auctions. Richard Engelbrecht-Wiggans wrote a paper that argued that this was not a reason. He argued that with a commonly known probability of cheating in a Vickrey auction, the bidders would adjust their strategies to reflect that probability and that nothing would change.

Hence, cheating and fear of cheating shouldn't cause the rarity of Vickrey auctions. Richard's argument didn’t convince me. Ron Harstad and I constructed two models of cheating in Vickrey auctions. In these models, there isn't a prespecified probability that the bid taker will cheat in a Vickrey auction. The bid takers decide whether to hold Vickrey or standard auctions and then, if they chose Vickrey auctions, whether to cheat. In one model, the bidders don't know which bid takers are willing to cheat, and they react in a way that makes it unprofitable for bid takers who won't cheat to hold Vickrey auctions. Hence, these honest folks don't hold them. Only those willing to cheat do so, making the probability of cheating one. In the other model, we assumed that bidders were trusting until a bid taker was caught cheating, and that after that they reacted as if the bid taker would always cheat. This model had penalties for getting caught as well as the loss of any future gains from holding Vickrey auctions. It assumed that a bid taker would cheat if it were in its interest to do so. From time to time the bid taker would find it worth the risk to cheat and, eventually, it would get caught, making trusted Vickrey auctions impossible. Our paper with these models appeared in the *Journal of Business* (Rothkopf and Harstad 1995).

In 1996, Bill Vickrey shared the Nobel Prize for economics. The Nobel Prize committee cited three major areas of Vickrey's work. One of them was the Vickrey auction. However, when the press interviewed Vickrey after the announcement of the award, he minimized this contribution and instead concentrated on his pioneering work on congestion pricing. Tragically, he died suddenly a few days after the award announcement.

David Lucking-Riley has been researching stamp auctions. Stamp auctions were one of the few examples I found in the late 1980s of the use of a Vickrey auction. In some stamp auctions, bidders mail in bids which instruct the bid taker to bid up to the amount specified. The rules are that the bidder will pay one bid increment more than the best other bid. The JPE paper mentioned these as one of the few Vickrey auctions. Lucking-Riley (1998) discovered two interesting facts about these auctions. First, their existence is not the result of Vickrey's paper;

the earliest of this kind of stamp auction was held before Bill Vickrey was born. Second, he documented cheating in them.

Enriched Modeling of Auctions

Except for the Vickrey's 1961 paper, economists and game theorists generally ignored bidding until the second half of the 1970s. Then, they got heavily involved. They built game-theoretic models of single, isolated auctions. They used their models to compare different auctions forms--i.e., standard sealed bidding, Vickrey auctions, English auctions, and Dutch auctions--with risk neutral bidders or risk averse bidders. Often, they used elegant mathematics. However, the models were not particularly realistic. Sometimes, they didn't appreciate this and made sweeping statements about auctions that applied only in their unrealistic models and made policy recommendations based upon them. Furthermore, the economic journals that published these statements and recommendations were not concerned about their misleading nature.

As I have mentioned, I am more of a modeler than a solver of equations. That is, I am best at coming up with a useful mathematical representation of a real situation. That was exactly what the economists were not doing. Hence, it seemed to me to be a fruitful area for my expertise. When I came to Rutgers in 1988 and was free to do whatever research I wanted, I concentrated on it. This led me to build models that deal with issues that economists tend to ignore. These include the facts that bids are not always honored, that bids are often only allowed at discrete levels, that bidders sometimes have private information about other bidders, that the participation of bidders in an oral auction is often unobservable to other bidders, that participation in auctions involves costs, and that bid takers, too, can be risk averse.

My JPE paper on Vickrey auctions had one indirect effect. Shortly after it appeared, the journal sent me a bidding paper to review. With one exception that turned out to be fixable, the mathematics in the paper was correct. Furthermore, the paper used game theory to consider a problem that was important, the effects of the auction rules on decisions by bidders to participate.

(Most bidding models assume a given set of bidders. If you pick the best set of auction rules for the bid taker under that assumption ignoring the bidders' ability to decide whether to participate, you may get a misleading answer.) The paper obtained interesting and important results. However, the paper was full of statements about auctions that were wrong in practice. I wrote an unusually prompt and lengthy review that discussed these statements critically. I warned the editor that my opinion on them was nonstandard among economists.

A few months later, the journal sent me a revised manuscript to review and a long letter from the paper's author, Ron Harstad. He had fixed the mathematical error and had removed or fixed some of the other problems. Some of his fixes were fine, but some weren’t, and his letter defended a number of the others as standard in economics. A full discussion of some of the assumptions and comments in the paper would have turned the paper into a critical study of bidding theory, and the letter reported that the editor had indicated that this was beyond the scope of what he would accept. However, Ron stated he wasn't just a game theorist. He did experiments, and he was interested in policy. In addition, he was interested in my criticism of game-theoretic bidding models and would like to collaborate with me, whoever I was, on such a critical study.

This was a rather positive response since Ron, whom I had never met, must have been blind-sided by my nonstandard criticism. After writing a review telling what still needed to be fixed, I called him and accepted his offer. (I needed an economist to collaborate with on such a project to make sure I understood the subtleties of standard economics.) We have now collaborated on three NSF grants and lots of papers including an influential critical essay on bidding theory that appeared in *Management Science* (Rothkopf and Harstad 1994). The main theme of our collaboration has been enriched models of auctions. All but one of my papers on the topics mentioned in this section and several mentioned earlier are collaborations with Ron.

4. On Not Waiting for Queuing Theory

Time Varying Arrival Rates

I have already mentioned that my first attraction to operations research was to a queuing model. At M.I.T., I took a course from Philip Morse who had written the first book dealing with queuing theory (1958). We covered some of the book material in the course, and my interest in waiting line models continued. In another course, I built some simple mathematical models of waiting lines. As I mentioned, my master's thesis examined optimal priority assignment in queues.

At Shell, worked on models involving random fluctuations, but with one exception, I didn't have any opportunity to work on waiting line models. The exception was an application with Shail Parikh of a multi-server model to the problem of allocating tank cars to chemical plants. In the model, the tank cars are the servers and the loads of chemicals needing to be shipped are the arrivals that wait for service. The model showed how many tank cars a chemical plant needed in its fleet in order to keep shipping delays to an acceptable level.

At Xerox, one of my concerns was market models. These models predicted the sales of potential Xerox products. The predictions depended upon product characteristics. Some data for the model came from engineering and planning, but an important part of it came from market research studies of potential customers. These models succeeded because we designed them together with the market research that would supply the data to them. I worked on the design of a model for a proposed high speed duplicator, and I had a number of concerns about the approach of the project. One issue that came up was the customer's reaction to delays in getting his work done. We needed to ask about how the customer valued avoiding such delays. The model's prediction of sales would depend upon the delays imposed on the potential customers by our product and competing ones and customers' disutility for such delays.

There was a gap, however. We could not easily obtain data on delays. We could get data on the job mix and on the speed of the machines for different kind of jobs. I thought a queuing model could convert data on jobs and the speed with which they could be processed into data on the average delay for a job. At a model design meeting, I said so and offered to take responsibility for that aspect of the problem so the team building the model could concentrate on other modeling issues I though were critical.

Although I had not been involved, there had been work at Xerox using waiting line models to help sales people determine how many Xerox machines a customer needed. Indeed, it was not too different in approach from the work that I been involved with at Shell where the servers were tank cars instead of copiers and the jobs were chemical shipments instead of copying jobs. Someone at the design meeting who was aware of this work warned me that standard queuing models would not work because they assume that the jobs arrive at the duplicators at a constant random rate. Such assumptions worked well in some situations, but in copy centers where the duplicators would be used, it wouldn't. The work not only has random fluctuations, it often has large, predictable, systematic ones too. For example, it may be perfectly predictable in some copy centers that little work will arrive in the morning but that lots will come in after lunch. I remembered that Phil Morse's book started to deal with such time varying arrival rates. It didn't give a complete solution, but I knew that this was an important problem and that in the 20 years since he had written the book, there had been thousands of papers on queuing theory. So I said, "Don't worry, I'll take care of it."

When I got back to my office, I looked into the situation. It was startlingly bleak. I called Professor Marcel Neuts, *Management Science*'s area editor for stochastic processes who had handled the review of a paper of mine (Myer, Rothkopf and Smith 1979). He told me that few of those thousands of papers on waiting lines had considered time varying arrival rates. He referred me to a relatively recent paper that had compared several methods for dealing with this phenomenon (Leese and Boyd 1966). The paper reported that the best of the methods was simulation, an imprecise method that was burdensome to use even for one situation. In our market research, we needed to consider many hundreds of situations. Clearly, I was in trouble.

Usually, in PARC it was possible to help with an applied project and then undertake fundamental research that would enable future projects to be done better. This time, however, I needed to find a better way for this project. Fortunately, I needed only an approximate method for solving the problem. The data were very rough anyway. The method would have to give meaningful comparisons of related cases, however, and would have to be computationally reasonable.

Even the simplest queuing model has a messy set of equations that describe how the state of the system, the number of customers in it, varies through time. For each state, a differential equation gives the probability that the system is in it at a given time. This equation depends upon the probability that the system is in neighboring states. When the arrival rate is not changing, the system eventually approaches a "steady state" in which these probabilities do not change. This reduces the differential equations to simple algebraic ones. There are still an infinite number of equations (one for each possible number of customers in the system), but they are solvable. However, when the arrival rate is changing, the system does not get to a steady state, and there is an infinite set of interrelated differential equations. For the simplest model, the solution of these equations involves an infinite sum of Bessel Functions, which are, themselves, defined in terms of infinite sums. Hence, the solution was too messy for useful computation.

I realized, however, that I didn't need to know the complete distribution of the number in the system at any time. All I really needed was the average number. From that, I could compute the average delay. The infinite set of differential equations could be combined to get equations for the average number in the system and for the variance of the number in the system. Clarke (1956) had done this. However, these equations depend upon the probability that the system is empty. I thought that if I knew the average number in the system and the variability about that average, I could estimate closely the probability that the system was empty. There is a well known probability distribution, the negative binomial, defined on the nonnegative integers (i.e., 0, 1, 2, 3,...) that can be specified in terms of its average and variance. I set up a calculation scheme using it.

The scheme took the average and variance of the number of jobs in the system and used the negative binomial distribution to approximate the probability that the system was empty. With this probability, the differential equations could be used to estimate the rate of change in the average and variance. Using these estimates and a small time step, the scheme then calculates new estimates of the average and variance of the number of jobs. The scheme repeats this process, tracking the path through time of the average and variance of the number in the system. I programmed it and computed some test cases. I checked it by calculating an extremely close approximation of the exact answer for a few cases by truncating the infinite set of differential equations by assuming that there was a (high) maximum number of jobs that could ever be in the waiting line and tracking the state equations for each possible number. My approximation approach worked well. In particular, when I assumed that the arrival rate had a regularly repeated fluctuation, the calculation of the average and variance of the number of jobs in the system soon settled down itself to a repeated pattern.

At this point, a PARC co-worker, Shmuel Oren, wandered by and asked what I was doing. He asked how I knew that my calculation scheme would converge. I pointed out that it was converging but conceded that I couldn't guarantee that it always would. The next morning, he had a proof that the method would always converge and that it would always converge to the same thing no matter where I started it. The market research project got delayed, so we had time for testing on a wide variety of problems. The approach tested well. We computed tables for average delays with different average levels of utilization, different degrees of systematic fluctuations, and different numbers of servers. I used tables in the market research study when it resumed.

Shmuel and I wrote a paper describing our work and arguing that time varying queues were an understudied phenomenon. We presented the paper to Stanford’s Operations Research

Department. Joseph Keller, a distinguished applied mathematician from Stanford's Math Department, was there. Joe told us that in mathematical physics our approach is called a "closure approximation." He gave us a fascinating lecture on the history of the use of closure approximations in mathematical physics. We thanked him and asked for a reference to cite in our paper, but the substance of Joe's half hour lecture had never been written. We added a paragraph to the paper summarizing what Joe told us and credited "J. Keller, private communication." After some small fixes, the paper appeared in *Management Science* in 1979.

The paper got some attention for a few years. A few follow on papers used our approach to consider other queuing models or, in one case, to improve our approximation by using a four parameter probability distribution. I promoted the work within Xerox. Xerox’s management science service group had a simulation model to approximate the average job delay in a copy center in a variety of complicated situations. The simulation model was expensive to use and got little use. Bob Johnson, one of the creators of the computer program, and I inserted our tables plus some other approximations to enable the program to calculate average delays without simulations. We checked a few cases, and the combined approximations closely approximated lengthy simulation runs. After that, no one used simulation runs, and the model with the approximations got lots of use. We published a paper describing this experience (Rothkopf and Johnson 1982).

Combining Queues

One day while I was working at LBL, I attended a seminar at Stanford. For it, Gerry Lieberman, the Operations Research Department chair, had assembled a panel of faculty members. Gerry, who recently had returned to Stanford from working for the National Science Foundation, charged the panel roughly as follows: "Pretend I'm the head of the NSF. Explain to me why I should give money to operations research rather than to, say, chemistry or physics. What are some of the important ideas that have come out of operations research?"

The panel discussed several ideas. One was that combining queues was good. No one on the panel disagreed, but the issue was raised of why we often see parallel, uncombined queues. The answer proposed was that people are slow to adopt new ideas. The only dissent was from an audience member, a recent graduate from the department, who suggested that combined queues looked long and that customers might be put off since *they* didn't understand the benefits of combining queues. In any event, the only reasons mentioned for not combining queues involved stupidity.

I didn't participate in the discussion, but I was not convinced. I recalled a conversation with San Francisco State professor Paul Rech, a former coworker at Shell. He had studied queues in a bank and noticed that tellers worked harder when they had their own queue than when the queues were combined. When the queues are combined, a teller isn't keeping anyone in particular waiting; however, when she has her own queue there are particular people and they are standing in front of her and looking at her. I started to think about the problem and realized that there were a lot of potential reasons for not combining queues. Furthermore, often the standard reasons for combining queues do not apply. I thought there would be interest in a paper explaining why combining queues might not be a good idea. I invited Paul to work with me on it. It proved to be easy to write.

The standard argument for combining queues depends upon the comparison of the average delay in two different queuing models. In one model, there are a number of servers, each with its own separate, independent waiting line. In the other, the same number of servers handles the same stream of arriving jobs at the same rate, but there is one combined waiting line for all of them. In such a comparison, the combined line wins easily. Its jobs have a lower average wait, and the variability of waiting time is reduced too. This comparison is in the chapter on queuing in many introductory texts on operations research. Typically, part a. of the problem has the student calculate a customer's average wait with separate queues, and part b. do the same with combined queues. Some older texts had a part c. asking for reflection on the assumptions behind the comparison, but most recent texts did not.

One assumption that is not always met is that each job takes exactly the same time in either system. If tellers work more slowly, as Paul had observed, with a combined queue, then the comparison is not appropriate. If the time to get from the combined queue to the server adds to the service time, then it is not appropriate. If service times can be overlapped (as when one customer unloads a shopping cart while another's order is being rung up), it is not appropriate. Even a small increase in the average service time in a congested system greatly increases the average wait.

Another assumption often not met is that separate queues are independent. They may be in some mechanical systems, but usually not in waiting lines involving people. People tend to join the shorter line. Furthermore, they switch lines, especially when a server is idle. ("No waiting in aisle 7.") The only reason work gets processed faster in the combined system than in *independent* separate systems operating at the same rate of speed is that sometimes, in the separate systems, a job is waiting for one server while another one is idle. This doesn't happen with a combined queue. However, if customers don't wait for a server when another is free, then this source of advantage disappears.

There are a number of other potential reasons for separate queues. Separate queues allow for specialization of service without having to train all of the servers to perform all possible tasks. ("Money orders at counters 5 and 6 only.") Separate queues allow management to use customers' desire to avoid delay to motivate other behaviors. ("Cash only in the express line.") Separate lines allow customers to chose their server. This power of choice may be a benefit itself, and if the customer can estimate the relative speed of the different servers for different tasks, it may speed things up as well.

A previously documented exception to the faster average job times for combined queues that the Stanford panel had missed was that when shorter jobs can be given better service at the expense of longer jobs, it decreases the average delay of all jobs.

We sent paper on this to the Forum section of *Operations Research*. At about the same time, Richard Larson of M.I.T., who in addition to being a professor was the president of a company that does consulting on managing queues, also wrote about combining queues. He argued that combining queues was a good idea, not for the traditional reasons, but because doing so guarantees "fairness." By fairness he meant that with a combined queue, a job that arrived first always got served first. He was offended by situations in which he got somewhere ahead of someone who was nonetheless served ahead of him. We ended up citing each other's papers and having them published together (Rothkopf and Rech 1987, Larson 1987). For a while after the paper came out, I was (inappropriately) regarded as an expert on the psychology of queuing.

5. Energy Economics-Being Out of Phase

The Elasticity of the Demand for Energy

At Shell, I worked in the energy industry. Over the years, I learned something about its engineering economics. The oil industry had been growing smoothly for decades. Each year, Shell Oil Co. prepared a ten year energy supply-demand balance for the U.S. Shell's planners projected demand for gasoline, heating oil, diesel fuel, natural gas, and electricity without reference to price. Then, they sought the cheapest sources of supplies to meet this demand. U.S. petroleum reserves were decreasing. The 1969 projection met the demand for gasoline in the later part of the 10 year balance with "unconventional" raw materials such as oil from oil shale. However, by the time the 1970 balance was prepared, Shell's planners realized that technology was proceeding too slowly for unconventional raw material to be available in significant quantities before 1980. Therefore, they “closed" the energy balance in the later years with oil imports. At the time, the U.S. was operating under an oil import quota system, imposed originally by President Eisenhower, that was supposed to limit oil imports to 15% of consumption. The projected balance required lifting the quota.

Shell Oil Co. is a U.S. subsidiary of the Royal Dutch Shell group. The Shell Oil Co. planners took their ten year energy balance to the Shell planners in London. These planners had been struggling with a world energy balance. Although there was plenty of oil in Saudi Arabia, they had begun to doubt the Saudi's willingness to sell the oil at a high rate. This caused a problem closing their ten year world energy balance. They had closed it with coal exports from the U.S. The planners realized that it didn't make sense for the U.S. simultaneously to import oil and export coal. They decided that next year's projections should consider the possibility that price increases would reduce energy demand. In late 1970, Paul Rech, who had transferred from the Applied Math Department to Shell Oil's planning organization, called me. He asked if we could calculate a statistical estimate of the price elasticity of demand for energy in the U.S. The planning organization wanted the estimate in three months. I told him I'd call him back.

The more I thought about such a statistical study, the more I doubted it would get meaningful results. The price of energy in the U.S. had been remarkably constant since World War II. Since the price hadn't changed much, it would be impossible to get a good estimate of the effect of price changes by analyzing historical economic data. An estimate based upon pre-World-War-II data, would be of doubtful relevance to future projections. Some foreign countries had different energy prices from those in the U.S., but they also differed in lots of other ways. Hence, I would not believe a projection that said if U.S. energy prices rose U.S. energy use would become like the energy use in countries with higher energy prices. Eventually, I realized that the only way I would trust any statistical calculation of energy demand elasticity was if it agreed with a good "engineering" estimate. This implied, however, that we didn't need to do the statistical calculation. We just needed to do the good engineering estimate. I called Paul, and explained this. He agreed to support the engineering estimate instead.

Shail Parikh, a fellow member of Shell's Emeryville lab, and I undertook the task. We started with Shell Oil's energy end use breakdown. We then took each end use area--e.g., transportation--and tried to estimate how energy end use would change if the price of energy doubled. For example, for transportation we realized that people might drive less and that they might buy cars that got better gasoline mileage. For each end use sub area, we used what ever data or analyses we could. For example, there were studies of the price elasticity for air travel that estimated that a one percent increase in airfares would result in a one percent decrease in air travel. We knew that fuel costs were about 15% of air transportation expense. Hence, if these costs were passed on to travelers, there would be about 13.5% less travel.

Shail and I split up the end use areas and reviewed each other's work. One area was residential heating energy. Better insulation is one way of dealing with higher home heating costs. I used the basic physics of heat loss through insulated surfaces and basic economics to derive a simple rule for the optimum thickness of insulation. On insulated surfaces, it increases as the square root of the cost of energy; heat loss through optimally insulated surfaces decreases as that square root. This implies a demand elasticity for heat that would be dissipated through insulated surfaces of minus point five. This result is independent of the interest rate used to trade off insulation cost against heating expense.

In some areas, we had no studies or models and had to use judgment. For example, lacking any data, we estimated that doubling heating costs would result in a one degree reduction of indoor temperatures. Our report showed the sources of the elasticity we had found in such a way that Shell management could easily substitute their judgment for ours. The overall elasticity we found came from many different sources, not just one or two subjective judgments. We tried to be quite conservative in our judgmental inputs to convince Shell management that there was indeed energy demand elasticity. We knew that some managers were skeptical and that the demand extrapolation procedure they had used assumed that there was none. Nonetheless, we found a significant overall elasticity of demand. Our report, delivered in early 1971, was well received. I left Shell in early 1973, and I didn't work on energy during the "energy crisis" when many others moved into the field.

When energy prices rose dramatically in October 1973, Shell had in hand better thinking than was available from leading economists. In early 1974, I went to a meeting of the American Association for the Advancement of Science at which a leading economist was supposed to talk on the elasticity of demand for energy. On the day of the session, he was testifying before Congress about energy prices and had his co-author give the paper. It was based upon statistical calculations that used international data for individual fuels. The paper ignored fuel substitution effects, which are large compared to overall energy elasticity, and was totally unconvincing. Without the statistical jargon, the paper could be summarized as: "We have observed that when the price of coal goes up, people switch from coal to oil and that when the price of oil goes up people switch from oil to coal. Therefore, if both price of both go up, people will do without both."

The energy crisis gave us a chance to see how good our projections were. We had done a reasonable job of thinking of the major ways that people would save energy, but we did miss a few (e.g., the 55 mph speed limit). On the other hand, our subjective estimates, while leading to a larger elasticity than Shell management was presuming, were still on the conservative side.

This work on energy elasticity was private. However, in the late 1970s Shail Parikh was working for the Energy Modeling Forum at Stanford. Shell was co-operating with the Forum and agreed to release the study (with appropriate disclaimers). We wrote a contemporary preface, and published it as a Stanford report and in *Energy Economics* (Parikh and Rothkopf 1977, 1980). By this time, the approach had gained respectability and was known as an end use study.

In 1982, I was getting ready to leave PARC. I wanted a job near San Francisco if possible. The University of California's LBL was looking for a head of their Energy Analysis Program. I applied and was invited to give a seminar. LBL preferred a seminar on my old work on energy to one on my recent work, so I presented this study, then twelve years old. It was generally well received, but there was one soft spoken elderly gentleman who asked me a question. I answered it incorrectly. After I had answered a few other questions, he asked me to reconsider my answer. I saw my mistake and corrected it. Afterwards, I found out that he was Charles Hitch, economist, former president of the University of California, former president of Resources for the Future, former under secretary of defense for systems analysis, former president of the ORSA, and a member of the search committee. I got the job.

World Models

In October 1971, Shell decided to close their Emeryville research lab. A few of the scientists lost their jobs, but most were offered jobs in one of Shell's Houston labs. My case was different. I was offered a two to four year assignment in a high level staff position in Shell's Group Planning organization in London. It meant uprooting my family, at least temporarily, from the Bay Area, but it also provided an opportunity to see Europe, and it was exciting professionally. At the end of 1971, we moved to London.

Jay Forrester of M.I.T. had just published *World Dynamics* (1971). It proposed using his system dynamics methodology for making ultra long range global forecasts. The variables in his model were things like total world population, total world food production, and total world pollution level. He ran the model for the two hundred years from 1900 to 2100. It seemed to say that the world economy and population was going to collapse from lack of resources and, if that somehow didn't happen, from excess pollution. Forrester was careful to say that his model was just how things *might* be. He recommended further studies using his approach.

Soon after I arrived in London, my boss, Cornelius Kuiken, gave me a copy of the book and asked me to look into it. He pointed out that Shell was in the resources business and that our product caused pollution. If Forrester's approach was useful, we ought to use it, and if it was wrong, we ought to know why. He told me that I could draw upon all of Shell’s expertise to get data for the model.

I became convinced that the Forrester model was unlikely to produce reliable forecasts. Ultra long range forecasts are inherently difficult, and there was nothing in the methodology to indicate that the problems had been overcome. The problem was not Forrester's system dynamics approach's use of the differential equations but the fit between reality and the equations. (Of course Forrester didn't claim that his equations reflected reality, only that they might be made to.) Some of the input was going to be guesses--ones upon which the answers would depend heavily. Some of the variables were so vaguely defined that it would be hard even to ask experts what they should be. For example, one key variable in the model was pollution. It was a single global aggregate. It was increased by economic activity and, if it got too high, it increased the death rate. It decayed over time, but if it got too high, its decay rate decreased. (This led to the positive feedback that caused the model's pollution crises.) Those are its only important roles in the model. What does it mean to ask how the world pollution level changed between 1900 and 1970? How, in this context, can we average meaningfully carbon monoxide in the Sahara desert with sewage in the Rhine River and smog in the Los Angeles basin?

What the model did not allow for was even more significant. It had no prices, but it did have some feedback loops that prevented crises. For example, if food per capita decreased, there was a part of the model that increased the allocation of resources to food production. Hence, the model did not indicate that world starvation was a major problem. However, the model did not have any feedback loop that allocated resources to reducing pollution. Even if pollution got so bad that it greatly increased the death rate, nothing extra was done to control it. Nor did the model have any way to increase natural resources. It defined natural resources as *nonrenewable*. If they got used up early in the 130 year forecast period, then their shortage caused an economic crisis in the later years.

At the beginning of the 19th century, Malthus predicted that the world would do badly economically because population was growing exponentially but resources such as land were fixed. This was not a useful prediction. Over time many things we do not think of as resources become resources. We can invest in finding them, creating them, or using them more efficiently. We tend to do so when they become scarce and costly. The Forrester model was mathematical Malthus. It attempted a forecast of 128 years. If, in 1972, we thought about the situation 128 years earlier in 1844 and tried to list the world's then known natural resources, we would not list nuclear power or even petroleum. The first oil well was drilled in 1859. Part of the problem is time scale. For a ten year economic forecast, it might be reasonable to approximate the world's technology as fixed. However, for a 100 year forecast, it would be silly.

Scientists in Shell's Amsterdam Laboratory were also working on Forrester's model. They had implemented it on the computer and reproduced his results. I worked with them. We made two changes in the model. First, we inserted a feedback loop for pollution. If pollution greatly increased death rates, the loop caused expenditures for pollution control that lowered pollution. Testing indicated that almost any reasonable parameters for this feedback loop would eliminate the model's pollution crisis mode. The other change to Forrester's model we made redefined resources and put in an activity to create them and a feedback loop that increased this activity when resources were scarce. A critical input to our revised model was the effectiveness of this activity. We analyzed the model's sensitivity to this input. Depending upon it, we could get any result from Forrester's resource crisis to unfettered economic growth.

The work on the Forrester model took on much greater importance when, in 1972, Meadows, *et al*. published *Limits to Growth*. The Club of Rome, a group of prestigious European industrialists, sponsored this book. It elaborated the Forrester model, got similar results, and, unlike Forrester's book, it claimed to give reliable forecasts. It argued that the only way to avoid global disaster was limiting economic growth. Also unlike Forrester's book, it was widely publicized and read. U.S. cabinet members recommended it. In the Netherlands, one of Shell's two home countries, it topped the best seller list. To some, it appeared to be The Truth direct from science, M.I.T., and the computer. Forrester's book included the computer program that he had used and all of its input data. *Limits to Growth* included only a general description of the kinds of relationships that were included, making it hard for its critics to be specific. However, while what was in the model was not clear, it was clear that there were no feedback loops for controlling pollution or increasing resources.

The staff at the Amsterdam lab wrote a paper in Dutch on our work pointing out its relevance to the model in *Limits to Growth* and proposed submitting it to a Dutch engineering weekly. An English translation of the proposed article was sent to my boss and me in London. My boss noted that I was not listed as one of the authors. Since the publication was to be in Dutch in an obscure place, I didn't ask to be made an author. Instead, we suggested that my contribution be acknowledged. This was a mistake. When the article appeared in the Dutch engineering weekly, the prestigious journal *Nature* got permission to print an English translation (Oerlemans, Tellings, and deVries, 1972). This became the definitive critique of *Limits to Growth*.

I was not only critical of the particular models in Forrester's book and in *Limits to Growth*. I was critical of the whole enterprise. I didn't and still don't think it possible to make reliable 130 year forecasts of this kind. I ended up writing a short piece for the magazine *New Scientist* that argued this point (Rothkopf 1973). Later, when the management science community was putting together a volume on world models, I contributed a short paper, "Limits to Models" (Rothkopf 1976). However, several projects were started to create reliable systems dynamics models of this kind. Shell had agreed to cooperate with one of these projects, and I was asked to create an energy submodel. I agreed reluctantly to do what I thought possible.

I limited the time scale to 50 years and made price an explicit variable. The model included several different sources of energy including coal, petroleum, and nuclear and some demand elasticity. At the time, nuclear power was growing rapidly and expected to continue to grow, although there were questions about it. When I got a model working, the first sensitivity analysis I ran was to eliminating nuclear power. The general tenor of the model was optimistic. On its 50 year horizon, it did not appear that we were running out of energy. I wrote a report on the model for Shell to give to the people it had agreed to cooperate with and a paper for publication (Rothkopf 1973). In both, I was careful to indicate the necessarily subjective and speculative nature of long range forecasts. One thing was bothersome. Many of my coworkers in Shell Group Planning were convinced that Saudi Arabia did not want to and would not sell the oil it had at anything near the rate necessary to avoid an energy crisis of some kind. Two of Shell's four planning scenarios were crisis scenarios. Because of this, I added a footnote to the first page of the paper warning, "This does not mean that there could not be temporary problems of energy supply in the medium term due to incorrect anticipation or monopolistic constraint. This model deals exclusively with very long term developments and necessarily smoothes out shorter fluctuations; from studies of other, shorter term considerations some of my colleagues at Shell in fact anticipate energy supply problems during the next 10 or 15 years." The paper appeared a few months before the energy crisis began. The price of oil then stayed high until it collapsed about twelve and half years later. My co-workers in Group Planning were savvy.

Energy Conservation

When the energy crisis hit in 1973, there were two widely accepted opinions about what would happen. One was that the world was running out of energy and that economic activity would suffer (as, indeed, it did at first). This, the "freezing in the dark" alternative, was shared by those who believed the message in *The Limits to Growth* whether or not they believed the methodology. The other widely accepted view was that higher prices for energy would produce more energy supply. The first view required and the second view was consistent with the widely held opinion that economic activity was directly tied to energy use. A third view, that the energy crisis could be alleviated by using energy more efficiently, was not widely accepted. Some saw it as a fantasy concocted by radicals opposed to nuclear power.

Scientists started working on energy efficiency. Lee Schipper, a graduate student in astrophysics at Berkeley, compared energy use in different countries. His 1976 paper in *Science* gave the results of carefully comparing Swedish and American energy use (Schipper and

Lichtenberg). Swedes had as high a standard of living and a harsher climate than did Americans, but they used much less energy per capita. This comparison helped make energy conservation a respectable third opinion to freezing in the dark and higher prices increasing supply.

Most economists were comfortable with the higher-prices-will-produce-more-supply point of view, and they did not have too much difficulty with the idea that conservation might lower demand without lowering living standards. However, most of them expected the market to produce the right amount of conservation. The energy crisis involved national security issues, not just economic ones. It led to a number of U.S. government programs on both the supply side and the demand side. In 1980, Ronald Reagan was elected president. He wanted to let the market decide, and his administration set about dismantling nonnuclear government energy programs.

In late 1982, I became the head of LBL's Energy Analysis Program (and, thus, Lee Schipper's boss). The program was largely funded from the remains of the Department of Energy's conservation program. This remainder was under constant pressure from the Reagan administration. Some of my concerns at the Energy Analysis Program were managerial, but I had an interest in the economic rationale for government programs in energy conservation.

Some people view the free market is an end in itself--a moral imperative. They may even subscribe to the libertarian slogan, "Taxation is theft." Most people, however, who favor the free market claim to do so on pragmatic grounds--it works. If the free market works, then government programs can only make things worse. Many people who favor free markets on pragmatic grounds applied this logic to government programs to improve conservation. But the free market doesn't always work (Kuttner 1996 is as good a discussion of this as I have seen.). In a number of important ways related to conservation, it demonstrably was not working.

When I started dealing with Washington on energy policy issues, I expected there to be a difference between conservatives and liberals, and there was. However, my experiences pointed to an even greater difference that I had not anticipated--that between pragmatists and "know-nothing" ideologues. Because there was good evidence that certain markets for conservation weren't working, I had no trouble working with pragmatic conservatives, but the "know-nothing" ideologues wouldn't trust evidence, however strong, and often wouldn't even look at it. I didn't have occasion to interact with anti-free-market ideologues, but I suspect that the frustration would have been as great. (Some people in LBL approached any situation with a strong presumption in favor of conservation programs, but, perhaps because they were scientists, they generally could be persuaded by sufficiently strong evidence.)

Many economists earn their living explaining the advantages of the free market, and they start with a strong presumption that it works. Some economists understood quickly, but for others, every step was a struggle. I had extensive discussion with Tony Fisher, an energy and resource economist on the Berkeley campus. He helped me understand the kinds of arguments about market failures that conservative economists accepted as justifying interference in markets. We wrote a paper about market failures related to energy efficiency. We considered a wide variety of problems with markets for energy and the evidence on them. We also considered what economists would accept as the best--i.e., most efficient-- remedies for these failures and other, secondary remedies. There were landlord/tenant problems and information and transaction cost failures, like the ones on appliance energy efficiency. There were pollution related externalities, national security related externalities and the effects of regulated natural monopolies. When we got through, even though we had not set out to justify government conservation programs, the list of appropriate remedies we ended up with resembled a government conservation program (Fisher and Rothkopf 1989). The paper was well received by conservationists, but I'm not aware of it directly influencing others.

6. Practice Publication

Simulating Wave Heights

One project at Shell was developing a tool for simulating an off-shore pipe laying barge operations. Shell contracted for pipelines connecting its oil and gas producing platforms in the

Gulf of Mexico to each other and to shore. This was expensive, and Shell considered building its own pipe laying barge.

Shell Pipeline Co. asked for help designing such a barge. They could estimate the cost of barge designs, but they wanted help estimating the time each design would take to lay pipe. Pipe laying speed depends on barge design and wave heights in a complicated way. Pipe can be loaded onto the barge at a one rate if there is room, and the barge can lay pipe at a smaller rate. These rates depend on the barge design and the wave heights. As the wave heights increase, the loading operation has to be interrupted, but the barge can still lay pipe if it has pipe on board. If the weather deteriorates further, the barge can no longer lay pipe, but it can hold onto the end of the pipe already laid and wait for lower waves. Finally, if the weather deteriorates even more, the barge has to attach a float to the end of the pipe and head for port. When the weather improved sufficiently, it can return, retrieve the pipeline and resume laying. Certain tasks such as starting and ending the pipeline require continuous periods of low enough wave heights. If these tasks are interrupted by high waves, they have to be restarted.

Clearly, the time needed to lay a given pipe with a given barge design depends not just on the proportion of time the waves are at given heights but also on the average duration of periods of low wave heights. Shell had contracted with a meteorological consulting service for data of this kind (much of it came from "hindcasts"). Our job was to write a computer program to use this data and data on barge capabilities to estimate the time needed to lay a given pipeline.

We created a simulation model to do this. It sampled the weather data at random and then calculated the duration of the job. It repeated this enough times to estimate the probability distribution of job length. For this simulation, we needed to represent the consultant's data and sample from it. The data was stated in terms of "sea states"--e.g., 0 to 2 foot waves, 2 to 4 foot waves, 4 to 8 foot waves, etc. For each sea state, we had the proportion of time the weather would be in that state, and for each state except the first, the average time from when the sea went to a lower state until it returned. We used these data to fit a simple tridiagonal Markov model that allowed the wave heights to change at random to adjacent sea states. For example, if the current state was 2 to 4 feet, the next state could be 0 to 2 feet, the same, or 2 to 4 feet, but it couldn't change to any other. With a short enough time period, this makes sense and the consultants' data is always sufficient to define the state transition probabilities.

Shell used the simulation to design a barge for the Gulf of Mexico but decided not to build it. However, they got the companies that do the pipe laying for them to quote the cost on a per day basis as well as for the entire job. This allowed Shell to assume the weather risk. For the first job quoted this way, the simulation indicated that Shell had a 99% chance of a lower cost with the daily rate. It took this rate and saved over a third of the $1.3 million fixed price cost of the job.

I wrote a paper on this work with John K. McCarron of Shell Pipeline Co. and Stan Fromoviz of Applied Math Department. At that time, *Management Science* had two separate series appearing as alternate issues: Series A for theory and Series B for application. What we had done was not particularly original, but the application seemed important[2], so I submitted it to Series B. The area editor wanted to publish the paper but with the material on the application removed. This startled me. I argued, but was unable to change his mind. He was a theorist who handled the journal's papers dealing with stochastic processes for both theory and application. He liked the little bit of theory in the paper but saw little value in the application. I eventually eliminated most of the material on the application, and the paper appeared in 1974.

Later, at a national meeting talk Martin Starr, editor-in-chief of *Management Science* said that he wanted application papers but that they were hard to find. After the talk, I told him that he didn't know what was going on in the trenches. I told him about the wave height simulation paper. He asked me to send him documentation. I did. About six months later, he and John D.

[2] Shell was using it for simulating other wave height related operations in the Gulf of Mexico and, with different weather data, to simulate North Sea oil operations.

C. Little asked me to chair the TIMS Publications Committee. After chairing the committee for two years, I was elected TIMS' vice president for publications.

Publication Policy and Editing

Along with meetings, publishing academic journals was the main "business" of TIMS. As Publications Committee chair, I set about learning that business. I started with smaller issues such as should we charge a submission fee for papers and use the funds to pay reviewers. Some of the push for this proposal came from those who believed that all papers were either good or bad, that the authors of bad ones knew they were writing bad papers, and that a submission fee would keep them from bothering our reviewers with them. My own view was that many papers we reviewed were in a gray area and that the review process was somewhat random. Sending papers to different reviewers might lead to different decisions on some of them. Usually, we are not trying to decide correctness, but importance; such judgments are quite subjective. Certainly, the authors of most papers submitted to us thought their papers were important. In any event, I headed off the proposal by showing that we couldn't charge enough to "buy" faster reviews and that there would be substantial bookkeeping and clerical problems—especially if we made exceptions for special cases such as overseas submissions.

Struggling with a series of single topic publications that had started as special issues of *Management Science* and then been spun off as the *TIMS/North Holland Series in the Management Sciences* taught me a lot about the economics of journal publishing (before electronic publishing). TIMS' arrangement with North Holland gave it a small commission on all sales in return for doing the editorial work (with volunteer labor). North Holland controlled the pricing. For North Holland, high prices and selling to only a few libraries was the most profitable course. Bob Machol, the editor of the series, was chagrinned by the low circulation and wanted TIMS to advertise the series. This, however, made no sense for TIMS since North Holland owned the bottom line and would not pay for advertising. Eventually, Bob resigned over this issue. TIMS decided to end the series rather than to replace him. Too much effort was spent producing volumes that were not being widely circulated, and the series was suffering. I learned that there are three things about a journal that all tend to go well or badly together: its circulation, its cost per copy, and the quality of the papers it attracts.

I also handled papers as an associate editor of *Operations Research* in the Business Applications area and served a term on the ORSA Council. Unlike TIMS, ORSA's flagship journal was bundled with membership. Early volumes of *Operations Research* had dealt with practical problems and applied models. However, by volume 30 it had become a journal written by researchers for researchers. Given that it was bundled and that about half the members were practitioners, a consensus arose that the journal should have an area on applications. I was asked to edit it. After reflecting on my experience with *Management Science*'s by then defunct applications series and on what Gene Woolsey had accomplished in transforming *Interfaces* from a newsletter into a journal about real practice, I accepted on several conditions. I insisted that the section be limited to papers about actual practice, that it have its own editors, that each of these editors have practical experience and no responsibilities for a theory section, and that it have its own page allocation that could not be reallocated for theory papers. I edited the Practice Section of *Operations Research* for a decade, leaving to be editor of *Interfaces*. Getting practice papers is difficult, but once it got started, the section put one or two good papers on real OR practice into each issue of *Operations Research*.

There are several reasons why getting practice publications is hard. Academics are paid to publish, but they seldom have the time and skills for practice. Practitioners, on the other hand, are seldom paid to publish. In addition, they must usually get permission from their clients who often have reasons not to give it. I have joked that there are two potential reasons to not give permission: the project failed and you don't want to look silly or it succeeded and you don't want your competitors to know. However, there are often good reasons for clients to allow publication. See my *Interfaces* editorial "On Not Keeping Secrets" (1999).

Unfortunately, our journals often train authors to leave out material on practice. The pipe laying paper was not an isolated example. When I started the OR Practice section, I wanted it to start fast. I asked the other area editors for submissions involving real practice that could be reassigned. I got one. It had been accepted subject to revisions, revised, and was in rereview. The paper, which dealt with a queuing model for operator staffing, barely mentioned the application. I decided I wanted it but that it would have to discuss the application more fully. I feared that the author would be put off by a request for another revision. I called him, introduced myself, got his permission to have the paper transferred to the Practice Section, and then gingerly brought up the matter of further revision. When he understood what I wanted, he said it would be no trouble at all. He would just restore what he had removed in the previous revision. He did, and his was the first paper in the new practice section (Sze 1984).

I believe that the survival of operations research as a profession absolutely requires theory and practice to interact. If we do not have that interaction (and many forces work against it) we will end up as two separate, unsuccessful groups: "applied" mathematicians whose math is not applied and consultants with nothing new to offer. Publications about practice are the key to that interaction. They redirect both theory and practice by reporting what works and what doesn't. They help justify useful theory and improve practice. They help make practical work academically respectability. Getting practice publications may be difficult, but the day or two per week I devoted for the past fifteen years to seeking and editing them was well spent.

7. Teaching OR—Math for Business Students and Business for Math Students

Math for Business Students

Soon after I started working at Shell, I was approached about the possibility of teaching an OR course for the University of California extension. Many people in industry had not had been exposed to OR courses and wanted to learn OR. (I also participated in OR training within Shell.) After teaching for the UC extension, I taught undergraduate courses on OR in Cal State, Hayward's math department.

At first, teaching was challenging. As a graduate student, I had a fellowship and had not taught. Initially, I followed closely texts someone else had selected. Teaching interested me and was useful for my career in Shell. In Shell, I worked in only one or two mathematical areas of OR. However, I wanted to be a generalist since I could be called upon to deal with a problem that required other methods. Teaching kept me fresh and improved my skills in these other areas.

My part time teaching was intermittent, depending on the teaching needs of San Francisco area universities and on the time demands of my job and family. While there were exceptions (notably, teaching OR in the medical information sciences program at UC San Francisco), most of my teaching was to graduate business students. In the last years before I became a full time academic, I regularly taught MBA students at the UC, Berkeley. By then, I brought something valuable to the teaching of quantitative methods to business students. I knew which methods worked in practice and which didn't. I also knew what might cause a theoretically good method to fail. I was able to use this knowledge to select better material to teach. For example, I had used the critical path method effectively. However, I never saw successfully uses of PERT, with its probabilities for job durations, even though PERT often got equal treatment in text books. I also made a point of emphasizing the things that could make a method fail. This succeeded with part-time students. Students would come up to me after a lecture and say, "Now I know why my company's project failed." I am not sure how many full time students got something useful from it. I sometimes feared they were thinking, "If it doesn't work, why is he teaching it to us?"

A challenge of teaching Berkeley MBA students was their widely varying quantitative backgrounds. It was common to have students with a technical master's degree or doctorate in a class containing students who had been English majors. Partly because I often worked with technical people and partly because I have an intuitive rather than systematic way of talking about technical matters, it was easy for me to lose the nontechnical people in the class. They would sit quietly feeling stupid and not ask me to explain. To combat this, I explained the problem to the first class meeting and got volunteers for a "poets committee" responsible for stopping me and asking questions as soon as they stopped following what was going on. This usually worked well. I have heard the engineers sigh with relief when my poet stopped me after I had omitted a crucial step in an explanation.[3]

Too much of the quantitative education of business students deals with manipulation of numbers and not enough with thinking about what the numbers mean. The latter is what the students need proficiency in.[4] My first semester teaching the bright[5] business under graduates at Rutgers' School of Business, I had an upsetting experience. After a homework assignment that involved using queuing formulas, the class asked me to go over the problems. I reviewed the first one and asked if they understood it. They said they did. Since the next problem involved the same formula, I suggest we skip it. The class objected, so I went over it. Then, I asked why they had needed it reviewed. With one voice, they said, "What about that other number?" Unlike the text's other problems, this one had an irrelevant number. The students were depending upon the

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[3] One semester, a bright young woman who had been an English major and was intimidated by quantitative material chaired my poets committee. She did an excellent job, worked incredibly hard on the class, and astonished me by getting the highest score in the class. Impressed, I offered to write her a job recommendation. A few years later, she contacted me and asked for a recommendation. She had decided to go back to school to become a high school math teacher. She lacked the standard math background. I wrote her an extraordinary letter.

[4] One year, I got a beginning class of Berkeley MBA students to think about numbers in the following way: An introductory lecture that reviewed their prior statistical training had a discussion of simple averages. I told them that the previous year the average starting salary for a Stanford MBA was less that the average starting salary of Stanford's graduates with bachelor's degrees in economics and asked them to interpret this. After some predictable concerned discussion, I asked them if their interpretations would change if I told them that John Elway, Stanford's star quarterback, had just graduated with an economics major.

[5] They need calculus, statistics, and a B average their first two years to get into the business program.

absence of irrelevant data and using the number of numbers in the problem in select a formula. This bothered me. The world the students will face is filled with irrelevant numbers. They will have to struggle to find numbers that mean, even approximately, what formulas call for. I created problems for them containing irrelevant data. The data they needed were imbedded in narratives. They found the problems difficult even though they involved only the simple formula—$L = \lambda W$ and I warned them about the irrelevant data. They also had trouble realizing that a manager's statement in a problem that she "served 400 customers per day" gave the arrival rate, not the service rate.

Teaching Business to Math Students

RUTOR's PhD program is quite rigorous mathematically. Many of its students are good mathematicians. I teach a RUTCOR class that deals with using math to solve real problems. Often, some of my students are better mathematicians than I. That doesn't mean, however, that they know how to use math to solve real problems. I tell the class that while other RUTCOR classes teach them how to solve equations, this class deals with which equations to solve. Some of them are startled and haven't really thought about where equations come from.

The class is diverse. After initial readings about the nature of OR and of models, they do modeling exercises—mostly adapted from the modeling course taught at Stanford by Dick Smallwood and Pete Morris. My favorite of these involves a team of two athletes taking turns with a bicycle in a race in which their team's time is the time of the second athlete to complete 25 laps. The students are to advise a team on several aspects of race strategy. There are two particular questions, each of which is easy to answer with a simple model. However, the two simple models are mathematically inconsistent.[6] Some of the best mathematicians have the most

[6] One question involves the fraction of the time each athlete should be on the bicycle. The other involves strategy for the final lap. The first is well answered assuming one athlete should always be on the bicycle.

trouble with this exercise, but it drives home the importance of the difference between the model and what is being modeled. The class also involves several speakers from industry describing real projects, some decision theory, and some basic engineering economics. (One can get a PhD from RUTCOR and not know what a present value is.)

The class ends with a team project for a client with a real problem. At first, the projects were within Rutgers. Some involved improving the campus bus system, the improving the campus mail system, improving the assignment of faculty to courses, and helping a faculty committee understand the interaction of student lateness to class and the bus system.[7] Recently, projects have come from industry. The project should give the students an opportunity to think about ways of dealing with a client in order to formulate a useful model. I hope the class is preparing graduates entering industry to practice more effectively and those entering academia to do more valuable research and to teach better.

However, a reasonable final lap strategy will usually have the athlete on the bicycle leave it on the ground for his following teammate's use.

[7] This isn't a new strategy for OR education. I was in a class Phil Morse turned loose on the M.I.T. library.

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