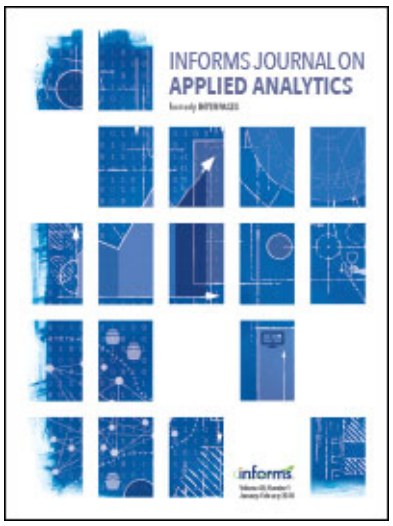

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Letters to the Editor

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LETTERS TO THE EDITOR

To the Editor:

Re: Queueing Theory: Two Nations.

In retrospect, I am glad that you published Professor Byrd's letter on the "inapplicability" of queueing theory. It has served to awaken and reawaken academics to the beauty *and* usefulness of queueing theory and to encourage the flow of applied queueing papers to *Interfaces* and other journals.

Just prior to my recent illness (May, 1980), I worked with two of our best students on a crash project for a division of a major US firm. The problem — evaluation of a new production line technology — hinged on two very nice queueing problems. We had two weeks (which stretched to four because computer computations promised by the firm were late) to complete the work because equipment orders had to be confirmed. We solved the problem and made specific recommendations on equipment, staffing, and operating rules — all of which have been implemented in the new plant.

While on my back I had been working on applied queue problems and on queuelike models of economic markets. I have been working with Isaac Dukhovny, a recent emigre (and queue expert) from the Soviet Union, as well as with several students. Dr. Dukhovny has re-introduced me to the Soviet queueing literature which is very, very good in its analysis while quite weak in its reported applications. Superb theoretical work is also being done in East Germany, particularly at the Mining School in Freiberg. Dukhovny thinks that the drive in theoretical techniques comes about because computers available to nonmilitary research workers are slow and unreliable and software is relatively unsophisticated.

Dr. Dukhovny also introduced me to another part of the literature of queueing theory — and applications in particular — the computer-oriented literature. While the literature is basically American (with great assists from France and Belgium), it is as if there are two Queue Nations — Operations Research and Computer Design. The computer literature, which for all intents and purposes began about 1969 or 1970, is now quite large and is replete with applications and approximations in queueing theory. The Computer Design Nation rarely quotes the Operations Research literature (except for Kleinrock, J. R. Jackson, and Gordon and Newell). They omit Lemoine, R. R. P. Jackson, Morse, Posner and Bernholtz, Kelley, and even Koenigsberg, whenever applicable; by doing so they are forced to re-invent wheels and other old established devices. On the other hand, the work reported on queue networks and on uses of storage is excellent and we in the Operations Research Nation will be forced to re-invent if we ignore what has been accomplished.

Some of the differences between the two nations are demonstrated in Lemoine [1977, 1978] and Kobayashi and Konheim [1977], recent papers on the state of the art in queue networks. Lemoine represents the Operations Research Nation and Kobayashi and Konheim represent the Computer Design Nation. Although these papers appeared at almost the same time, it is remarkable that only a small portion of the citations is common.

There must be some way to create a pact or concordance between the nations. We both have much to gain from sharing. Right now the *OR Abstracts* rarely cover the Computer Nation's publications (e.g., *Acta Informatica, Proceedings of the ACM*) and the IEEE abstracts often omit the OR Nation's publications (both nations generally ignore the Iron Curtain literature). We are in the midst of a period of great and significant developments in methods for exact and approximate solutions of problems which have a wide range of applicability. The more the nations cooperate, the more useful and timely the results.

I suggest that you use your own good offices and *Interfaces* as a center for bringing the two nations together.

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Lemoine, A. J. 1977, "Networks of Queues: A Survey of Equilibrium Analysis," *Management Science* Vol. 24, No. 4, pp. 464–481.

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To the Editor:

Re: "Quickening the Queue in Grocery Stores," by Jones, O'Berski, and Tom, June 1980 *Interfaces*.

Several details of process and content in this paper rather bothered me. First, it appears that the study was performed without a client. Thus, there was no *interface* — why was it reported in *Interfaces*? It seems likely (to me) that a client would have questioned the fundamental assertion that "it is obvious that implementation (of a single-service-line system in grocery stores) is possible." The vast majority of grocery store customers waiting to check out are accompanied by a grocery cart — it is *not* obvious that a single-service line could be implemented within the geometrical constraints of most stores.

Second, it is claimed that previous studies "have shown that single-service waiting lines are applicable and do in fact reduce the average in-line waiting times in most situations." Is it not true that these studies neglect the time required to get from the head of the line to the check-out counter? Further, the reduction in average waiting time is based on full utilization of servers, which, as the authors point out, is generally accomplished in grocery stores even without a single waiting line system by line switching. Under these conditions, the variance of waiting time is reduced, not the average.

Those are my fundamental concerns, but I also have some nits to pick:

(a) I disagree with the assertion that banks have a captive audience. The annoyed customer would not have to "continually move from one bank to another" unless the banks were doing a lot of experimentation with queue structure — one move (per annoyed customer) would usually suffice.

(b) Random-digit dialing was used in the survey to avoid the bias introduced by unlisted telephone numbers. Yet a response rate of less than 32% was obtained. Wouldn't the self-selection bias swamp the unlisted number bias?

(c) Would a survey of the type performed give any indication of how the Hawthorne Effect would affect customer loyalty?

(d) The single-service queue tends to cause long lines (that is, in terms of the number of people in front of a particular customer) even in banks, as compared to separate lines for each server. Wouldn't the physical length of a single-service line composed of grocery carts tend to cause the customers to perceive a "seemingly interminably long queue"?

Having taken the authors to task on several issues, let me close by acknowledging that the qualitative discussion of shoppers' characteristics and their relationships with stores and store personnel is well taken: *time in queue* is indeed *not* the only criterion involved in the store selection decision. Any analysis that assumed that it was could give totally irrelevant results.

On the other hand, I must disagree with the authors' closing statement: "Realistically, the application of queueing theory principles for grocery store setups may not be warranted at this time." Rather, the principles must be applied appropriately. Models, based on those principles, must not omit factors that may have been negligible in other applications that are non-negligible here. Further, the supersystem in the context of which the system under study is embedded must never be ignored.

Robert G. Chamberlain
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The Authors Reply:

Thank you for the opportunity to reply to Mr. Chamberlain's comments regarding "Quickening the Queue in Grocery Stores." Mr. Chamberlain takes issue with the statement "It is obvious that implementation (of a single service line system in grocery stores) is possible." We believe the issue here may be one of interpretation. Our meaning of this statement is that it is indeed possible to implement a single-service line system in grocery stores, and implementation may mean remodeling and/or rearrangement of the existing store. The question then becomes one, not of possibility, but of feasibility. It was the purpose of the study to determine the feasibility of the implementation.

The studies cited in the article do demonstrate that "single-service waiting lines are applicable and do in fact reduce the average in-line waiting times in most situations." Mr. Chamberlain's suggestion that these studies neglect the time required to get from the head of the line to the check-out counter and that it is variance of waiting time and not the average that is reduced may be valid, in which case his suggestions might be appropriately directed to the author of the cited articles. Objectively measured waiting time is not the major issue. The basic contention of our study rests on an appreciation of the importance of the customers' perception. Subjective reality is more important than objective reality. It is the customers' perception of waiting times in single-service and multiple-service lines that is the center of the study.

Mr. Chamberlain's disagreement with the assertion that banks have a captive audience is once again a question of interpretation of the meaning of our statement. We were comparing the ease of changing grocery stores with the greater difficulty of changing banks. Moving from one bank to another requires a considerable amount of paperwork and time and inconvenience not characteristic of changing grocery stores. Thus, the level of discomfort necessary to cause consumers to change banks is greater than that necessary to cause consumers to change grocery stores.

The validity of the research conclusions is called into question by Mr. Chamberlain because of possible nonresponse bias and the functioning of the Hawthorne effect. Random-digit dialing was used in the survey to generate 300 numbers. These 300 numbers included inoperative, disconnected, and nonexisting numbers. The response rate was thus actually higher than 32 percent. Mr. Chamberlain is aware that survey research, in general, is not known for its high response rate. Given the state of the art with survey research, nonresponse bias remains a threat to the external validity of the findings.

Because the questions in our study were phrased without bias, the possibility of a Hawthorne effect is negligible. Respondents were not provided with any clues as to the "right" response to the questions asked.

Mr. Chamberlain's question, "Wouldn't the physical length of a single-service line composed of grocery carts tend to cause the customers to perceive a 'seemingly indeterminable long queue'?", can best be answered empirically by conducting a field study.

We are in agreement with Mr. Chamberlain's closing statement that "Models, based on those principles, must not omit factors that may have been negligible in other applications that are non-negligible here. Further, the supersystem in the context of which the system under study is embedded must never be ignored." This was the premise under which we conducted our study — that the customer is an important part of the supersystem that cannot be ignored.

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To the Editor:
Re: On-line scheduling [Hodgson, February 1980 issue, p.77].

I was intrigued by the scheduling problem in Thom J. Hodgson's excellent article [February 1980] on the "unsuccessful" case history of on-line scheduling, and I wish he could have included a little more data.

As readers may recall, the transfer line department made four different transmission cases, with a rather high setup time (40 hours for product 4, and 12 hours for switching within the group 1, 2, 3). The department had itself in a real bind, with production runs being cut short because of emergency requirements and the setups eating up more and more of the department's operating time.

The analysts made up a straightforward computer heuristic procedure, which worked well under their supervision, and the focus of the article was on the fact that three months later the department was back with the same problems, since the foreman had not had enough incentive to keep running the computer program.

It would seem to me, however, on the basis of the rather limited data available, that the problem is a little deeper than this. Suppose we start off when the department is in a real bind, with very low inventory and excessive time lost to setups. In order to increase inventory gradually towards the "ideal" level, overtime would have to be worked, or some more workers added temporarily (perhaps transferred in from other departments).

As inventory built up, less productive time would be lost to setups, and the inventory buildup would go on even more rapidly. Eventually, total inventory would equal or exceed the ideal level — perhaps overshooting it considerably — and management would intervene to cut back the department's work force.

Even if inventory buildup could be stopped at exactly the right level, it would still be an unstable equilibrium. Even a modest increase in demand or shortfall in production might start inventory into a nosedive downwards.

I would suggest putting up a big chart on the office wall, like a quality control chart, showing total inventory of the transmission cases by week, with upper and lower control lines, possibly 120% and 80% of the total ideal inventory found by the EMQ rules. (Or 130% and 70%?) When inventory went below the lower control line, management would allow the foreman to work overtime or add workers (and vice versa at the upper limit).

Just something very simple like keeping track of the total inventory might possibly be the crucial element necessary for avoiding panic situations.

Glenn T. Wilson
Management Science Department, Southern Illinois University, Edwardsville, Illinois 62025

To The Editor:

Re: Nahmias, Steven, 1980, "The Use of Management Science to Support a Multimillion Dollar Precedent-Setting Government Contract Litigation," *Interfaces* Vol. 10, No. 1, p. 1.

The article by Mr. Nahmias in the February 1980 edition of *Interfaces* on the use of Management Science to support the Litton claim was interesting but also disappointing. It was interesting to note that statistical methods were being accepted in this previously untried arena. It was disappointing, however, that Mr. Nahmias took a simplistic approach to such a complex situation. I had the opportunity to observe the complexities of shipbuilding while serving on the staff of the Supervisor of Shipbuilding U.S.N., Pascagoula, Mississippi, from May 1976 to August 1978. My duties as an assistant to the contracting officer enabled me to gain first-hand knowledge of Litton's shipbuilding process including cost estimation, scheduling, engineering, progressing and change administration.

Mr. Nahmias' choice of efficiency as a surrogate for the increased contract costs is unfortunate. Efficiency is very difficult to measure and Mr. Nahmias' method is imprecise since the measurement of physical progress is subjective and can be inflated to allow for larger progress payments early in the construction phase. Also, since there are many factors that can influence efficiency, even establishing a correlation between the submarine changes and efficiency does not establish causality.

Mr. Nahmias, however, did not even provide statistical correlation between the changes and efficiency. By arbitrarily dividing the time period into before and after the submarine changes, and not making allowances for changes in efficiency that can occur with time, the bulk of his work was no more than a time series analysis of the data. Efficiency could change for many reasons, such as a yard-wide change in the skill mix over the six-year period, or a general decrease in worker productivity. The skill mix on the various programs, the manning levels on these programs, the total yard manning level, and the total yard skill mix are all interrelated and change with time. The simple two-sample tests that Mr. Nahmias performed cannot explain the complex interactions of these variables over time. More recent studies [Hammon and Graham, 1980] have successfully used regression techniques to build cost estimating relationships for the shipbuilding process that allow for these interactions. These models also incorporate the effects of learning and design changes directly. As such they are much more powerful tools in determining the effects of changes on the costs of a shipbuilding program.

Another critical mistake that Mr. Nahmias made was to use data that was not representative of the situation that was supposed to be his base of comparison. He wanted to determine if submarine manning above the originally planned 2000 head level had an effect on the efficiency of the other shipbuilding programs. However, by dividing the data into periods when submarine labor was less than 2000 heads and greater than 2000 heads, Mr. Nahmias ended up comparing a period when submarine manning averaged less than 1000 heads to a period when it averaged 2450 heads. Thus the period in which Mr. Nahmias was trying to show detrimental effects due to submarine changes was closer to what had been originally planned than was the base data he chose to use.

This last result can have startling effects on the conclusions drawn from the data. Mr. Nahmias in his paper starts from the premise that the rise in the manning levels and the priority placed on the submarine program resulted in an increase in the proportion of apprentices and a decrease in the normal efficiency on the surface ships. Based on the results in the previous paragraph, one could argue that the low efficiency on the commercial programs was normal and that the higher efficiency in the earlier period was a result of greatly reduced manning on the submarine programs. Pursuing this a little farther leads to the scenario that the normal situation is for the submarine work to be the priority work requiring a higher ratio of skilled personnel. When submarine manning dropped to the low levels in the period prior to 1965, skilled submarine craftsmen were used on the commercial and other Navy work so that they would not be lost. This is a logical management decision. Thus one could conclude that the commercial and other Navy programs actually benefitted from the low submarine manning prior to 1965.

Thus although the paper is interesting in its application, it does not justify the position taken. The results would have been much more meaningful if Mr. Nahmias had done a more thorough statistical analysis for cause and effect rather than a simple time series analysis.

Reference

Hammon, Colin and Graham, David R., 1980, *Estimation and Analysis of Navy Shipbuilding Program Distribution Costs*, Ninth DOD/FAI Acquistion Research Symposium, p. 9–7.

J. W. Hargrove, Jr., LCDR, SC, USN
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The Author Responds:

I would like to respond to Mr. Hargrove's comments. His remarks show that he has misinterpreted the role of this writer in the litigation process and furthermore he has little understanding of statistical methods and the conclusions that can be drawn from such analyses.

This writer was retained by Litton as an expert in statistics, not in the industrial engineering aspects of shipyard operations. The manner in which efficiency was (and still is) measured in the shipyard is established procedure. Its verification and relationship to contract costs was the subject of the testimony of industrial engineers affiliated with Ingalls and other shipyard personnel. The actual data used in the analysis was compiled by Ingalls personnel and given to this writer as described.

The goal of the first study was to show in the simplest manner possible that average efficiency declined on the commercial ship program from time A (4/63-8/65) to time B (12/66-12/69). A simple and straightforward way, which could be understood by a layman, to demonstrate this inescapable conclusion is the two-sample t test that was used (which is not "a time series analysis of the data," whatever that means).

That other measures of shipyard operation show similar changes over the same period *does* establish statistical correlations (which could undoubtedly be established in other ways as well). However, Mr. Hargrove apparently does not understand that no matter how such correlations might be established, they would *not necessarily* imply causality. Certainly efficiency could change for many reasons. However, the other testimony presented in conjunction with our results did establish the necessary causal links as the outcome of the hearing attests.

Finally, this paper takes no position. It merely presents an objective statistical analysis of data presented to the writer and the relevance of that analysis to the claim.

Steven Nahmias
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To the Editor:

Re: Doll, Loren C., "Quick and Dirty Vehicle Routing Procedure," *Interfaces* February, 1980, pp. 84-85.

After reading this article, I am getting more and more convinced that many of us try to reinvent the wheel. The procedure described therein is a simplistic version of the one presented by Billy E. Gillett and Leland R. Miller in their article "A Heuristic Algorithm for the Vehicle-Dispatch Problem," *Operations Research* 1974, pp. 340-349. That algorithm is known as the "sweep" algorithm. The observation that good routes do not cross themselves or other routes was mentioned by Christofides and Eilon in their article "An Algorithm for the Vehicle-Dispatching Problem," *Operational Research Quarterly* 1969, pp. 309–318. Had Mr. Doll done his homework properly, he would also have found comparisons between the various heuristic algorithms.

Although I often share Mr. Doll's view that the marginal value of ultrasophisticated models is questionable for solving practical problems, proper credit should be given to those who developed those models when one uses their insights.

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CALL FOR PAPERS: MATHEMATICAL PROGRAMMING WITH DATA PERTURBATIONS. The Institute for Management Science and Engineering and the Department of Operations Research, School of Engineering and Applied Science, The George Washington University, will sponsor the Third Symposium on Mathematical Programming with Data Perturbations at the University's Marvin Conference Center on 21 and 22 May, 1981. This symposium is designed to bring together practitioners who use mathematical programming optimization models, and who have to deal with questions of sensitivity analysis, with academic and other research workers who are developing tools applicable to these problems.

Contributed papers in mathematical programming are solicited in the following areas:

(1) Sensitivity and stability analysis results and their applications.
(2) Solution methods for problems involving implicitly defined problem functions.
(3) Solution methods for problems involving deterministic or stochastic parameter changes.
(4) Solution approximation techniques and error analysis.

"Clinical" presentations are also solicited that describe problems in sensitivity or stability analysis encountered in applications. These will help to bring applied problems to the attention of research workers who may have, or be able to develop, effective techniques for solving them.

Further information is available from the symposium organizer:

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