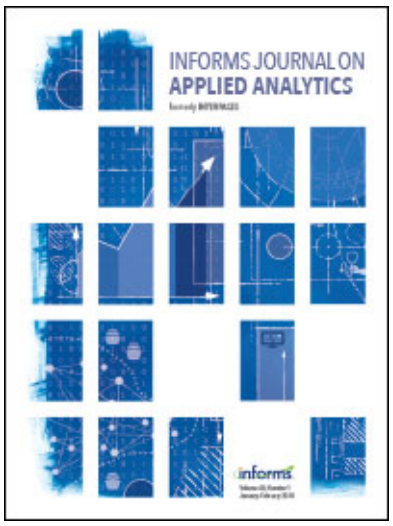


Interfaces

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

Letters to the Editor

To cite this article:
(1979) Letters to the Editor. Interfaces 9(2-part-1):116-124. https://doi.org/10.1287/inte.9.2pt1.116

INTERFACES
Vol. 9, No. 2, Pt. 1, February 1979

0092-2102/79/0902/0116$01.25

LETTERS TO THE EDITOR

Ed. Note: The first three letters are the initial comments elicited by my request for same; more in future issues.

To The Editor:

Denos Gazis' article (*Interfaces*, August 1978) is both enlightening and entertaining. He missed, however, from his suggestions one that would be most effective for producing good papers: *paying* the authors of good papers. Under the present system a competent operations researcher who also happens to be a talented writer prefers to consult with business or to write grant proposals or feasibility studies with an average compensation of about $600 a day plus expenses, instead of spending his or her time writing articles that at best will appear in one of our limited readership periodicals a year and a half later. Writing an article today for an OR/MS magazine is not fun and is not profitable. Even in the academe in recent years I have seen faculty candidates rejected in spite of their long lists of articles. Who can appreciate an article that nobody understands and for which the author got no pay? Free enterprise, man. To repeat part of Townsend's quotation stated in Gazis' article, ''If you are not in business for fun or profit, what the hell are you doing here?''

Nikolaos G. Kondylis
Chairman, Department of Quantitative Analysis, Bentley College

To The Editor:

The article entitled ''Quality Control in OR Literature'' by D. C. Gazis in the August issue of *Interfaces* produced some stimulating conversation with my colleagues where I work and teach.

Even though I think most of the papers presented in the OR publications are pretty good, I also have felt for some time that guidelines should be developed for both authors and referees.

(1) I would reject immediately any paper which attempts to use the journals as an advertising medium for some computer code developed by the author and submitted under the guise of a technical breakthrough. Quite frequently we see articles showing the superior performance of some new code over previous ones. When the author is contacted to obtain listings to verify the comparisons or for future research, the author places a $1,000 price on the code. We end up wasting time recoding it instead of devoting our energies to further advancements.

If an author wants to advertise his code for sale and profit, he should pay for the advertisement like other businesses.

(2) Similarly, I would reject any paper that did not contain (or have available at cost of reproduction and mailing) a completely solved problem. Surely the author has had to solve some real problem to insure that the algorithm works. The selected solved problem should be simple enough so the algorithm is not hidden in complexity and yet still shows the salient features of the proposed method.

(3) It is obvious that as a particular branch of technology matures, the vocabulary becomes more succinct and the size of the participating group decreases. There are those, however, who have chosen a different branch but would like to maintain some peripheral knowledge in other areas. Perhaps prospective authors could spend a little more of their time spelling out some of the acronyms and symbology. For example, does the average practicing OR generalist in industry know when a G/M/M/2 queue will apply to his particular problem. Some journals require a list of symbols either leading or trailing a paper.

(4) At one time I did considerable refereeing for a different journal. I found a clique in referees where "I'll accept your piece of verbal garbage if you'll accept mine" seemed to prevail. Perhaps a broader rotation of referees and a cross-checking of authors and referees would eliminate this problem.

Now that I've gotten that off my chest, let me address Mr. Gazis' suggestions.

The first suggestion, to initiate some formal rating of journals, is basically sound. However, I think that if you ask OR/MS subscribers to evaluate a long list of other journals, you will, by virtue of having a captive audience, have a large percentage accepting OR/MS as it is. So you *may* learn nothing. You may be better off to supply reader reply cards where a reader can rate papers in each issue on a one-for-one basis. This can probably be done for a short trial period to test its effectiveness. Surely some OR specialist can find which referees are letting junk through.

The first part of his second suggestion, which would require the author to submit a list of rejections, would be counterproductive. If a referee sees that a paper has been rejected by some other reputable journal, he may reject it himself out-of-hand. The second part of this suggestion has all of the problems of a massive paper mill of mailed lists. Someone must load the data base and that costs bucks.

Regarding the third suggestion, I think you'll find that it is the university staff and not the graduates that produces most of the papers. With the publish-or-perish approach still used at some schools, I wouldn't want to rate schools or their staffs by the pounds of papers they produce if I'm looking for quality.

The fourth suggestion has some merit. Many journals already require a nominal fee ($10 — $25) with each paper submittal. This is never returned, with a part going to the referees and part to offset publishing costs. When I was refereeing, I felt they should be paid.

The idea of a $100 fine borders on the ludicrous.

Arnold W. Maddox
13346 Amiot Drive, Creve Coeur, Missouri 63141

To The Editor:

Mr. Gazis' article on "Quality Control in OR Literature," *Interfaces*, August 1978, was interesting and enjoyable. However, he failed to mention the Type III and Type IV Errors which do tend to sneak into journals. These errors were alluded to but never fully expounded upon. (These errors are based on the Type III and IV errors discussed by Scanlon of the Urban Institute.)

Type III: Writing about something that does not exist; for example, a balanced Federal budget at full employment and zero inflation.

Type IV: Writing about something no one cares about, i.e., the rain in Spain falling mainly on the plain. (You would care if you live on that soggy plain.)

These two types of errors do occur in many different professional journals, not just MS types. It is important for referees as well as authors to avoid these errors to maintain readability and relevance.

Thanks for your time.

Thomas M. Ruple
5601 Grosvenor Lane, Bethesda, Maryland 20014

To The Editor:

The juxtaposition of the first two papers of the May 1978 issue of *Interfaces* has prompted me to write this letter. The papers I am referring to are F. Paul Wyman's "The Use of Geometric Programming in the Design of an Algerian Water Conveyance System," and Gene Woolsey's fifth column, "Prolegomena on Promoting Practice ..." But before I start with my comments directed mostly at Wyman's paper, let me state unambiguously and unequivocally that I like to (and do) read *Interfaces*, which I can't say of either *Operations Research* or *Management Science*, which have become too abstract and difficult for me to read in the time I have available. (I find myself reading *Interfaces*, and glancing at the titles and sometimes abstracts of *OR* and *MS*.)

First, let me comment on the case of units in the paper. The mixture of English units (i.e., 69 inches) and metric units, (i.e., meters, m^3/s), is inappropriate to say the least. Algeria has always been a metric country, and to propose to the government an optimal pipe diameter of 69 inches must have made them shudder. Even in this country Wyman would have one hell of a time to find someone who makes a 69 inch pipe, because pipes come in discrete diameters and increments and 69 is not one of them; but the roughly equivalent metric diameter of 175 cm. may be one.

Second, I like the idea of a short letter or note at the end of a paper to attest to the fact that the use of OR/MS techniques resulted in an improvement or a saving. However, if that letter comes from someone within the same firm or organization, conflicts of interest and possible cozy personal relations render it doubtful. I would have loved to see a letter by an Algerian official who checked on the original design of Bechtel and found out how the proposed solution could be improved. What is presented is too much in-house gossip of the mutual admiration club.

Third, I was curious when I saw the term geometric programming because I have had difficulties myself explaining to my students the utility of this technique and have always been in search of good applications. I was disappointed then when in reading I found that the study boiled down to finding the optimal solution to the cost equation shown in the appendix where the diameter of the pipe is the only decision variable. Since the turn of the century, at least, it has been known that the cost of a pipe varies roughly proportional to the square of the pipe diameter, and that the cost of energy to overcome friction varies roughly inversely to the fifth power of the diameter. That means that annual costs can be expressed as

$$C_\Lambda = C_p D^2 + C_f D^{-5} \ .$$

Upon taking the first derivative and letting it equal zero one obtains

$$D_{\text{opt}} = \sqrt[7]{\frac{5C}{2C_p^f}} = 1.14 \sqrt[7]{\frac{C}{C_p^f}} \ .$$

In old engineering textbooks this is referred to as the seventh-root rule for the optimal pipe diameter. If one wishes to keep the exponents as variables, the optimal pipe diameter in the notation of the author is:

$$D_{\text{opt}} = \frac{(E_f C_f)}{(E_p C_p)}^{1/(E_f + E_p)} \ .$$

I frankly don't understand the "exponent rule developed from geometric programming," but I concur that the optimal pipe diameter is more sensitive to a percentage error in the friction exponent than the pipe cost exponent, since the former is considerably larger. I disagree with the conclusion of the author that the pipe cost coefficient is more important than the friction coefficient and that more attention should be paid to the former. The opposite is true. C_f is already larger than C_p and has, therefore, a larger influence on the optimal pipe diameter. Moreover, looking at a project period of 50 years, energy costs are bound to increase, deposits in the pipe will reduce the diameter and increase the specific friction, so C_f will definitely increase over time. And thus the optimal pipe diameter is probably larger than the one presented.

Finally, I would plead for using simpler terminology. The "Geometric Programming Model with the zero degree of difficulty" might better be called a trivial problem, and to "run a test to check on the shadow price of the pipe diameter constraint" is equivalent to checking the first derivative of the objective friction. Let's call a spade a spade.

Now, what has all of the above to do with Gene Woolsey's paper. Well, I liked his story about the exchange of views with "the eminent, pin-striped gentleman" who was the dean of an un-named graduate school who blacklisted *Interfaces*. I have the funny feeling, however, that the dean displayed a lot of common sense (which cannot be generalized to all deans), and if you continue to publish papers like the one discussed

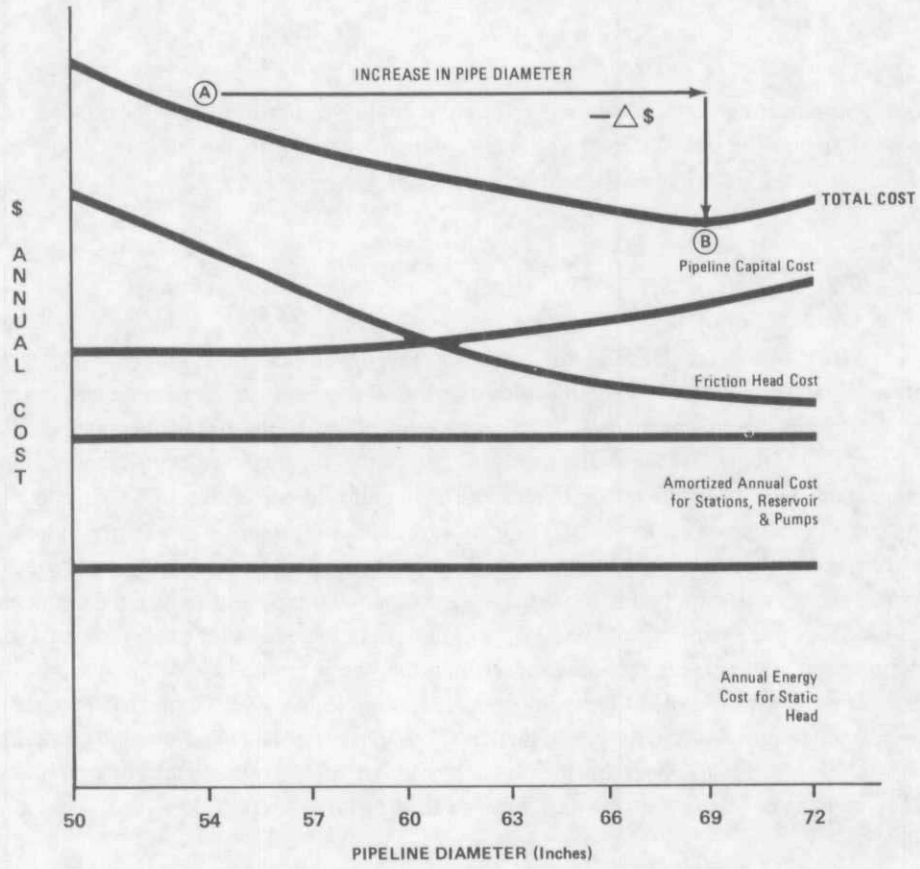
here, I would certainly side with him. And I am sure the ''prominent OR/MS director of a massive U.S. corporation'' will do likewise.

May I, therefore, humbly recommend these principles: (a) go metric all the way; (b) have independent, unbiased, free of conflict of interest testimonial letters; (c) sanitize the papers and eradicate jargon; and (d) have a few knowledgeable people read the papers before they are published.

With luck, perserverance, and quality control you can make *Interfaces* the journal it aims to be and should be.

P.S. I just checked the graph (Figure 2). It is incorrect. The optimal diameter based on the graph would be 66 inches.

FIGURE 2. Concept of Water Conveyance Cost Optimization.



Rolf A. Deininger, School of Public Health, The University of Michigan, Ann Arbor, Michigan 48109

F. Paul Wyman replies:

I read with mixed amusement and interest the letter of Professor Deininger concerning my article in the May, 1978 issue of *Interfaces*. As a former Bechtel employee I will respond to the technical misinterpretations. As a recent arrival to academia I will respond to the editorial implications for *Interfaces* which concern me.

As preface to the technical issues, I will generally state that my paper was deliberately brief, presenting only the "tip of the iceberg" (as you put it) in order to facilitate corporate review and to adhere to a style of terse presentation. This means that thoughtful reading and reasonable interpretation are required to grasp the full significance of the paper. Therefore a certain amount of "noise" misinterpretation might be expected. However, I question whether even a zealous and sophomoric academic could innocently misconstrue so much without having an axe to grind.

First let us consider the issue of units. All recommendations to the client *were* in metric units, and did consider metric pipeline sizes in Algeria. To suppose that Bechtel would have made recommendations to an Algerian client in English units is so evidently foolish as to even make *me* shudder and to question Deininger's motive for making this point. Perhaps this is indicative of Deininger's lack of familiarity with the actual operations of engineering firms (which apparently is no impediment to the academic inclination of presumption). The work was reported in English units since for *Interfaces* that is "telling it like it is." In other words, international engineers do not necessarily use their client's units for in-house calculations, but rather in units "local" to where the work is being conducted, and these are subsequently translated to customary units in all documents required by contract, including client reports. The issue of units is, to me, a red herring since most engineers (at least at Bechtel) have to move between different systems of units with fluidity.

Second, let us consider the model actually used and its significance. Deininger somehow supposes we used the two-term model presented in the Appendix. That formulation was developed at the time I wrote the paper in late 1977. It applies to cases where Q and N are known (which was not initially true in 1974). As indicated in the paper, the model actually used had seven terms in four design variables. Less explicit was the fact that constraints on Q and D were present plus one signomial condition due to the constraint ($Q + U =$ constant). Through the effort of about 50 pages of design calculations, regression analysis, and careful deductions from hydraulic principles, the exponents and coefficients were developed for this model. Considerable effort was expended to assure conformity with detailed cost estimates (running several hundreds of pages of detail each). When you go to that level of effort to derive a number (and it happens to be 2.46 for example), it is not good engineering practice to blithely round off to the nearest integer for the sake of tractability of the calculus. The model did *not* exhibit integer-valued exponents and therefore was rather difficult by means of the calculus!

While calculus methods would require numerical search for stationary point(s) over a messy vector of nonlinear derivative functions, the GP approach is linear, quick, and clean. By Occam's razor therefore, GP is the more parsimonious tool. Later, the specification of Q reduced the model to five terms in two variables (D and N) with one constraint on D. The model continued to be tight on the D constraint and consistent with the cost estimates. Thus it appeared desirable to let D be unconstrained to lower costs.

Now to lower friction cost by enlarging diameter is a principle so basic to hydraulic engineers that many of them tatoo the seventh-root rule underneath their watch band. But in this project, the impact of larger diameters had not been evaluated by a detailed cost estimate because of an earlier client request to consider only locally available pipe diameters. Since the GP model formulated by that time was found consistent with detailed techniques and much cheaper to invoke, it was a desirable tool for estimating the *value* of relaxing the diameter constraint (i.e., of going out of country). The effect was to resurrect an elementary design issue that had long since been buried, and no longer questioned. The impact of the GP model was to lead to a client decision to go to a foreign supplier despite higher shipping and handling costs, partly because the energy savings would be substantial.

Thirdly, we have the two-term model proposed by Deininger. While the form may be similar to the two-term model in my Appendix, I assure you that we are discussing two completely different animals, primarily due to the level of information embodied in the coefficients and exponents. I realize this distinction may be difficult for certain academics to grasp, so I shall address it in some detail. By ancient Egyptian tradition, it is true that a pipe's circumference varies directly with the square of the diameter. It does *not* follow that pipeline related *costs* (trenching, excavation, access roads, site clearing, steepness of terrain, alignment profile, thickness of pipe, concrete for supports, welding shop, x-ray crew, startup, etc., etc.) vary with the square of the pipe diameter. In fact many such costs are *directly* related to D, but with an exponent slightly greater or less than unity. Obviously, the fifty-year tradition referenced by Deininger doesn't consider such factors since they are site-specific.

The whole point is that GP is a much better tool than calculus, since GP readily accommodates the odd-valued exponents that characterize the actual problems of engineering design. To state that "the cost of a pipe varies roughly in proportion to the square of the pipe diameter" is exactly the type of "slap-dash" thinking that is not acceptable once the consultants have left and the real work begins. In regard to friction costs, it is somewhat more valid to use the inverse fifth power, although even here the more accurate 4.87 value from Hazen-Williams is appropriate, again indicating the use of GP. It is essential to consider such factors as pump size, efficiency, amortization, energy prices, number of spares, operating strategy, and local gradients for transient stress, in order to have a friction cost coefficient that will replicate a detailed estimate. The ability to add up all these costs and to accept the total cost curve the way it actually lies is a major improvement of the GP approach over calculus. When Q and N are known, the two-term model will virtually always have nonintegral-valued exponents. So to call a spade a spade, this is *also* a GP problem, not an antique calculus problem. Therefore, I hope that Deininger and others who would like a simple example of GP application will take note. Actually, the engineering literature is now replete with GP *potential* applications, especially in journals like *Design Optimization*.

Finally, the exponent rule derives from solving any two-term GP model in one design variable. It seemed superfluous to include it in the Appendix due to the number of textbooks now explaining GP. However, for clarity I will note the following, where d_1 and d_2 are the two dual variables (one for each term):

$$d_1 + d_2 = 1 \qquad \text{normality condition,} \tag{1}$$

$$E_p d_1 - E_f d_2 = 0 \quad \text{orthogonality.} \tag{2}$$

Without taking any derivatives, we note that the ratio $d_1/d_2 = E_f/E_p$. This means that the ratio of pipeline to friction costs in an optimal cost solution will be E_f/E_p. Now E_f is purely physical, so it is fixed. E_p, however, is project specific and can vary perhaps 50% between extreme cases. Now the real benefit (tacitly implied in my paper) is that you can plot all possible optimal diameters versus the ratio D_f/D_p, and have a family of curves for every value of E_f/E_p. Any engineer should be able to see that this is quite an improvement over the seventh-root rule. Such a set of curves can be calibrated by a company's historic value for C_p, C_f, and E_p, and near-optimal diameters should be immediately obvious for new site-specific conditions with nary a single derivative!

With apologies for the long-winded preceding section, I will try to conclude the next discussion more concisely. Again, as a practioner-turned-academic (*not as a Bechtel employee*), I will discuss my viewpoint on the *Interfaces* editorial issues. Sparked by Deininger's letter, I am reacting to what I see as a general academic condescension towards actual OR/MS applications as published in *Interfaces*. Many academics seem to have the knee-jerk reflex that the poor benighted practitioners are simply too ignorant to have anything of value to teach the academicians. Despite the fact of having never been on the scene and being totally ignorant of the circumstances, the reflex continues its motion through a superficial review and rejection of the methodology as being "nothing new."

I believe that what the practitioners are trying to tell the academicians is that *methodology is basically irrelevant*, that management science is not really a science, and that careful publication of cases of success and failure are now the *best hope we have for enriching the profession*. Therefore, to imply that *Interfaces* editorial standards and review policy should fall into the same decadent refereeing process that has given most of us six feet of bookshelf space filled with useless pulp and paper, is truly a step back to the stone age for the OR/MS profession. In fact, I would like to propose to the editors of the methodological journals (OR, MS, etc.) that *their* refereeing process be modified to require the mandatory blessing of at least one real-world line manager (not a consultant, analyst, nor a staff manager) for *every* article that is published! If this were to be done, we would shortly have the most vital and relevant profession in the country. Of course, the difficulty is to find enough line managers who would serve as referees. However, the *Interfaces* requirement of having an internal CPA verify claimed or estimated cost savings is a giant step in this direction.

This brings us to Deininger's criticism of the "cozy" in-house verification. I will lay down a challenge to Rolf Deininger, that if he believes internal verification to be so cozy and straightforward, that *he* write-up an "impact" story from *his* experiences and then obtain the relevant verification from the internal CPA, and submit it to *Interfaces*.

Now, if a CPA were to publish a financial opinion that is spurious, that CPA can get his union card revoked permanently! Therefore, it will be interesting to see how rapidly Deininger can rise to this challenge. I believe the general readership of *Interfaces* does appreciate the distinction between a blind referee, preoccupied with methodological nitpicking, who has neither livelihood nor reputation at stake, as opposed to the published verification by a CPA.

F. Paul Wyman
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CALL FOR PAPERS

CO79 - A Conference on Combinatorial Optimization will be held at the University of East Anglia, Norwich, England, July 9-12, 1979. Papers are solicited in the areas of: Classical Hard Problems, New Hard Problems, Theory of Heuristics, Combinatorial Complexity Theory, Practical Applications in areas such as Computer Science and Operations Research, and others. Deadline for papers is April 2, 1979. Address correspondence to: Dr. V.J. Rayward-Smith, School of Computing Studies, University of East Anglia, Norwich, NR4 7TJ, England.